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PROCEEDINGS

OF

THE ROYAL SOCIETY

OF

EDINBURGH.

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ERRATUM.

Index, vol. vi. p. 608, Professor Tait's Paper, line 4 from bottom, second column, *for* Parts *read* Roots.

Monday, 3d December, 1866.

Sir David Brewster, the President, delivered the following Opening Address:—

Gentlemen,—Of all the agencies which are at work in the material universe, the light of the sun is doubtless the most remarkable, whether we study it in its sanitary, its scientific, or its æsthetical relations In the language of metaphor, it is the very life-blood of nature, without which everything material would fade and perish; the fountain of all our knowledge of the external universe; and the historiographer of the visible creation, recording and transmitting to future ages all that is beautiful and sublime in organic and inorganic nature, and stamping on perennial tablets the hallowed scenes of domestic life, the ever-varying phases of social intercourse and the more exciting scenes of bloodshed and war, which Christians still struggle to reconcile with the obligations of their faith.

The influence of light on physical life is a subject of which we know little; but the science of light, which has been studied for nearly two centuries by the brightest intellects in the civilised world, consists of a body of facts and laws of the most extraordinary kind—rich in knowledge, popular and profound, and affording to educated students simple and lucid explanations of that boundless and brilliant array of phenomena which light creates, and manifests, and developes. While it has given to astronomy and navigation their telescopes and instruments of discovery, and to the botanist, the naturalist, and the physiologist, their microscopes, simple, compound, and polarising,—it has shown to the student of nature how the juices of plants and animals, and the integuments and filaments of organic life, elicit from the pure sunbeam its prismatic elements—clothing fruit and flower with their gorgeous attire, bathing every aspect of nature in the rich hues of spring and autumn, giving to the sky its azure, and to the clouds their gold.

In treating of the influence of light as a sanitary agent, we enter upon a subject almost entirely new; but admitting the existence of the influence itself, as partially established by analogy and observation, and asserting the vast importance of the subject in its social aspects, we venture to say that science furnishes us with principles and methods by which the light of day may be thrown into apartments which a sunbeam has never reached, and where the poisons and the malaria of darkness have been undermining sound constitutions, and carrying thousands prematurely to the grave.

The influence of light upon vegetable life has been long and successfully studied by the chemist and the botanist. The researches of Priestley, Ingenhousz, Sennebier, and Decandolle, and the more recent ones of Carradori, Payen, Macaire, Draper, Gratiolet, Gardner, Daubeny, and Hunt, have placed it beyond a doubt that sunlight exerts the most marked influence upon the respiration, the absorption, and the exhalation of plants, and, consequently, on their general and local nutrition. Draper and Gardner determined the influence of the differently coloured rays in the germination of plants and in the decomposition of carbonic acid, and they found that the yellow rays were particularly active in producing the green colour of the leaves. Mr Hunt has shown that the germination of seeds is more rapid under the influence of the chemical rays than in the dark or even under white light—a result confirmed by the observations, on a large scale, of the Messrs Lawson and Sons. The curious fact of plants bending towards the light, as if to catch its influence, must have been frequently observed. Mr Hunt found that this influence, which is strikingly shown by the potato, is due to the action of the *yellow rays*, and, what is very remarkable, that in *red light* the plant bends from it.

If light, then, is so essential to the life of plants, we may reasonably suppose that it is necessary, though to a less extent, to the development and growth of animals; and many observations have been recently made in confirmation of this important fact. Dr Edwards found that all the eggs of frogs exposed to light were developed in succession, but that none of those kept in the dark were well developed. Employing the tadpoles of the *Rana obstetricans*, he found that the action of light developes the different parts of the body in that just proportion which characterises the adult —the type of the species.

In papers communicated to the Imperial Academy of Sciences, M. Moleschoff of Heidelberg has shown that the quantity of carbonic acid exhaled by frogs (the *Rana esculenta*) is much greater in light that in darkness;* but the most important researches on the subject were communicated to the same Society by M. Beclard in 1858. From a series of experiments, continued for four years in the Laboratory of the Faculty of Medicine, he obtained the following results:—†

1. That the nutrition and development of animals that breathe by the skin experience remarkable modifications under the influence of the differently-coloured rays of the spectrum. The eggs of the fly (*Musca carnaria*), placed in six bell glasses, *violet*, *blue*, *red*, *yellow*, *transparent*, and *green*, produced worms which, at the end of four or five days, were very differently developed. Those developed in the *violet* glass were triple in size and length to those in the *green* glass, the influence of the other colours diminishing in the above order.

2. That the same weight of frogs produced more than twice the quantity of carbonic acid, in respiration, in the *green* than in the *red* glass. When the same frogs were skinned, the carbonic acid was greater in the *red* than in the *green* rays. No difference was observed with birds, and the small mammalia, such as mice, owing to their being covered with hair and feathers.

3. That the cutaneous exhalation of the vapour of water is *one-half* or *one-third* less in the dark than in ordinary daylight, or in *violet* light, in which the exhalation is the same.

Few and imperfect as these observations are, they entitle us to inquire into the influence of light upon the human frame, physical and mental, in health and disease, in developing the perfect form of the adult, and in preserving it from premature decay. In the absence of researches which might have been made in our hospitals, prisons, and madhouses, we must grope our way among general speculations and insulated facts, and we have no doubt that the direct influence of light over the phenomena of life will not be found limited to the lower races of the animal world.

Man, in his most perfect type, is doubtless to be found in the temperate regions of the globe, where the solar influences of light, heat, and actinism, are so nicely balanced. Under the scorching

* *Comptes Rendus*, 1855, tom. lv. pp. 363, 456, 643.
† *Ib.* 1858, tom. xlvi. p. 441.

heat of the tropics, he cannot call into exercise his highest powers. The calorific rays are all-powerful there, and lassitude of body and immaturity of mind are their necessary results; while in the darkness of the polar regions, the distinctive characters of our species almost disappear in the absence of those genial influences which are so powerful in the organic world.

It is well known to all who seek for health in a southern climate, that an ample share of light is considered necessary for its recovery. In the hotels of France and Italy, the apartments with a south exposure are earnestly sought for, and, under the advice of his physican, the patient strives to fix himself in these bright localities. The salutary effect, however, thus ascribed to light is supposed to arise from the greater warmth of the solar rays; but this can hardly be the case in mild climates, or indeed in any climate where a fixed artificial temperature can be easily maintained. Something, too, is due to the cheering effect of light upon an invalid; but this influence is not excluded from apartments so situated that from a western or even northern window we may enjoy the finest scenery illuminated by the full blaze of a meridian sun.

While our distinguished countryman, Sir James Wylie, late physician to the Emperor of Russia, resided in St Petersburg, he studied the effects of light as a curative agent. In the hospitals of that city there were apartments without light, and upon comparing the number of patients who left these apartments cured, he found that they were only *one-fourth* the number of those who went out cured from properly lighted rooms. In this case the curative agency could not reasonably be ascribed either to the superior warmth or ventilation of the well-lighted rooms, because in all such hospitals the introduction of fresh air is a special object of attention, and the heating of wards without windows is not difficult to accomplish.

But though the records of our great hospitals may not assist us in our present inquiry, yet facts sufficiently authentic may be gathered from various quarters. In the years of cholera, when this frightful disease nearly decimated the population of some of the principal cities in the world, it was invariably found that the deaths were more numerous in narrow streets and northern exposures, where the salutary beams of light and actinism had seldom shed their beneficial influence. The resistless epidemic found an easy prey among a people whose physical organisation had not been matured under those benign influences of solar radiation which shed health and happiness over our fertile plains, our open valleys, and those mountain sides and elevated plateaus where man breathes in the brighter regions of the atmosphere.

Could we investigate the history of dungeon life of those noble martyrs whom ecclesiastical or political tyranny have immured in darkness, or of those felons whom law and justice have driven from society, we should find many examples of the terrible effects which have been engendered by the exclusion of those influences which are necessary for the nutrition and development of the lower animals.

Dr Edwards applies to man the principles which he deduced from his experiments on animals; and he maintains that, "in climates in which nudity is not incompatible with health, the exposure of the whole surface of the body to light will be very favourable to the regular conformation of the body." In support of this opinion he quotes a statement by Humboldt, that among the people called Chaymas, who live under the Equator, both men and women are very muscular and have fleshy and rounded forms, and that he had not seen a single individual with a natural deformity." "I can say the same," he adds, "of many thousands of Caribs, Muyscas, and Mexican and Peruvian Indians, whom I have observed during five years. Deformities and deviations are exceedingly rare in certain races of men, especially those which have the skin strongly coloured."

If light thus developes in certain races the perfect type of the adult who has grown under its influence, we can hardly avoid the conclusion drawn by Dr Edwards, "that the want of sufficient light must constitute one of the external causes which produce those deviations in form which are observed in children affected with scrofula,—an opinion supported by the fact that this disease is most prevalent in poor children living in confined and dark streets."

Following out the same principle, Dr Edwards infers, "that when these deformities do not appear incurable, exposure to the sun is one of the means tending to restore a good conformation." "It is true," he adds, "that the light which falls upon our clothes acts only by its heat, but the exposed parts receive the peculiar influence of the light." "Among these parts," he continues, "we must certainly regard the eyes as not merely designed for the perception of colour and form. Their exquisite sensibility to light fits them peculiarly for transmitting it throughout the system, and we know that even a moderate light on the retina produces in several diseases a general exacerbation of symptoms."

If the light of day, then, contributes to the development of the human form, and lends its aid to art and nature in the cure of disease, it becomes a personal and national duty to construct our dwelling-houses, our schools, our workshops, our factories, our churches, our villages, towns, and cities upon such principles and in such styles of architecture as will allow the life-giving element to have the fullest and the freest entrance, and to chase from every crypt, and cell, and corner the elements of uncleanness and corruption which have a vested interest in darkness.

Although we have not visited the prisons and lazarettos of foreign countries, to describe the dungeons and caverns in which the victims of despotism and crime are perishing without light and air, yet we have seen enough in our own country—in private houses, in the most magnificent of our castles, and in the most gorgeous of our palaces—to establish the fact that there is hardly a house in town or country without dark apartments which it is in the power of science to illuminate. In most of the principal cities of Europe, and in many of the finest towns of Italy, where external nature wears her brightest attire, there are streets and lanes in which the houses on one side are so near those on the other, that hundreds of thousands of human beings are neither supplied with light nor air, and carry on their trades in almost total darkness. Providence—more beneficent than man—has provided the means of lighting up to a certain extent the workman's home, by the expanding power of the pupil of his eye, and by an increasing sensibility of his retina; but the very exercise of such powers is painful, and every attempt to see when seeing is an effort, or to read and work with a straining eye and an erring hand, is injurious to the organ of vision, and sooner or later must impair its powers. Thus, deprived of the light of day, thousands are compelled to carry on their trades principally by artificial light—by the consumption of tallow, oil, or gas, thus inhaling from morning till midnight the offensive odours and polluted effluvia which are more or less the products of artificial illumination.

It is in vain to expect that such evils, shortening and rendering miserable the life of man, can be removed by legislation or arbitrary power. In various great cities attempts are making to replace their densely congregated streets and dwellings by structures at once ornamental and salutary; and Europe is now admiring that great renovation in a neighbouring metropolis, by which hundreds of streets and thousands of dwellings, once the seat of poverty and crime, are replaced by architectural combinations the most beautiful, and by hotels and palaces which vie with the finest edifices of Greek or of Roman art.

These great improvements, however, are necessarily local and partial, and centuries must pass away before we can expect those revolutions in our domestic and city architecture under which the masses of the people will find a cheerful and well-lighted home. We must, therefore, attack the evil as it exists, and call upon science to give us such a remedy as she can supply.

There can be no doubt that science does possess such a remedy —a remedy, too, easily understood and easily applied. Like all other remedies, however, it has its limits, but within these limits the principles and methods which science dictates are unquestionable and efficacious.

With rare exceptions, apartments are lighted with vertical windows, which admit the light of the sun, either directly or when reflected from the sky, or from adjacent surfaces. In narrow streets and lanes, the light which enters the window comes chiefly from a portion of the sky, and the value of this portion as an illuminating agent depends on its magnitude or area, and on its varying distance from the sun in his daily path; but whether the area be large or small, bright or obscure, it is the only source of light which the window can command; and the problem which science pretends to solve is to throw into the dark apartment as much light as possible—all the light, indeed, which is visible from the window, excepting that which is necessarily lost in the process.

In lanes or closes shut up at both ends by lofty houses, or by projecting buildings, it is sometimes only from a small portion of the sky towards the zenith that the light can be obtained, and in this case part of it might be advantageously thrown into a window by expensive reflectors; but in streets and lanes open at both ends, where a narrow strip of the sky is visible from one point of the horizon to the opposite point, no aid whatever can be obtained from reflectors, and in such cases the method we are about to describe is peculiarly effective.

If in a very narrow street or lane, we look out of a window with the eye in the same plane as the outer face of the wall in which the window is placed, we shall see the whole of the sky by which the apartment can be illuminated. If we now withdraw the eye inwards, we shall gradually lose sight of the sky till it wholly disappears, which may take place when the eye is only *six* or *eight* inches from its first position. In such a case the apartment is illuminated only by the light reflected from the opposite wall, or the sides of the stones which form the window; because, if the glass of the window is *six* or *eight* inches within the wall, as it generally is, not a ray of light can fall upon it.

If we now remove our window and substitute another in which all the panes of glass are roughly ground on the outside, and flush with the outer wall, the light from the whole of the visible sky and from the remotest parts of the opposite wall, will be introduced into the apartment, reflected from the innumerable faces or facets which the rough grinding of the glass has produced. The whole window will appear as if the sky were beyond it, and from every point of this luminous surface light will radiate into all parts of the room. In order to explain the superior effect of roughly ground glass, let us suppose that the ordinary window is replaced with a single sheet of the best glass inserted flush with the outer wall. The whole of the light from the visible portions of the sky will fall upon its surface, but at such an obliquity that four or five sixths of it will be reflected outwards, and the other two or one sixth, which is transmitted, will fall on the floor or on the shutters, and be of no value.

In aid of this method of distributing light, the opposite sides of the street or lane should be kept white-washed with lime, and for the same reason the ceilings and walls of the apartment should be as white as possible, and all the furniture of the lightest colours.

Having seen such effects produced by imperfect means, we feel as if we had introduced our poor workman or needle-woman from a dungeon into a summer-house, where the aged can read their Bible,—where the inmates can see each other, and carry on their work in facility and comfort. By pushing out the window we have increased by a few cubic feet the quantity of air to be breathed, and we have enabled the housemaid to look into dark corners where there had hitherto nestled all the elements of corruption. To these inmates the winter twilight has been shortened, the sun has risen sooner and set later, and the midnight lamp is no longer lighted when all nature is smiling with the blessed influences of day.

But it is not merely to the poor man's home that these processes are applicable. In all great towns, where neither houses nor palaces can be insulated, there are almost in every edifice dark and gloomy crypts thirsting for light; and in the city of London there are places of business where the light of day never enters, and where the precious light which the sky sends down between chimney tops is allowed to fall useless on the ground. On visiting a friend whose duty confined him to his desk during the official part of the day, we found him with bleared eyes struggling against the feeble light which the opposite wall threw into his window. We counselled him to extend a blind of fine white muslin on the outside of his window and flush with the wall. The experiment was soon made. The light of the sky above was caught by the fibres of the linen, and thrown straight upon his writing table, as if it had been reflected from an equal surface of ground glass. We may mention another case equally illustrative of our process. A party visiting the mausoleum of a Scottish nobleman wished to see the gilded receptacles of the dead which occupied its interior. There was only one small window through which the light entered, but it did not fall upon the objects to be examined. Upon stretching a muslin handkerchief from its four corners, it threw such a quantity of light into the crypt as to display fully its contents.

But while our method of illuminating dark apartments is a great utilitarian agent, it is also an æsthetical power of some value, enabling the architect to give the full effect of his design to the external façade of his building, without exhibiting to the public eye the vulgar arrangements which are required in its interior. Our National Picture Gallery on the Mound, from the beautiful designs of Mr Playfair, is lighted from above; but there are certain small apartments on the west side of it which cannot be thus lighted, and these being very useful, the architect was obliged to light them by little windows in the western façade. These windows are dark and unseemly gashes in the wall, about two feet high and one foot broad; and being unfortunately placed near the fine Ionic portico,—the principal feature in the building,—they greatly interfere with the symmefry and beauty of the western façade. Had there been no science in Edinburgh to give counsel on this occasion, the architect should have left his little apartments under the patronage of gas or oil; but science had a complete remedy for the evil, and in the hope that it may yet be applied, we offer it to those who have the charge of this noble building. Take sheets of thick plate glass the exact size of the present windows, and of such a colour, that when one side is roughly ground it will have the same colour as the freestone. When the openings are filled with these plates, having the ground side outwards, the dark gashes will disappear, the apartments will be better lighted, and the building will assume its true architectural character. The plates of glass thus inserted among the stones may, when viewed at a short distance, show their true outline, but this could not have happened if, during the building of the wall, some of the stones had been left out and replaced by plates of glass of the same size. This method of illumination will enable architects to light the interior of their buildings by invisible windows, and thus give to its exterior façade the full æsthetical effect of their design.

If it is important to obtain a proper illumination of our apartments when the sun is above the horizon, it is not less so when he has left us to a short-lived twilight, or consigned us to the tender mercies of the moon. In the one case it is chiefly in ill-constructed dwelling-houses, and in large towns and cities, where a dense population, crowded into a limited area, occupy streets and lanes in almost total darkness, that science is called upon for her aid; but in the other, we demand from her the best system of artificial illumination, under which we must spend nearly one-third of our lives, whether they are passed in the cottage or in the palace, in the open village, or the crowded city.

When we pass from the flickering flame of a wood fire to rods of pine wood charged with turpentine—from the cylinder of tallow to the vase filled with oil—from wax lights to the flame of gas, and from the latter to the electric light,—we see the vast and rapid stride which art and science have taken in the illumination of our houses and streets. We have now obtained sufficient sources of light; we require only to use it safely, economically, and salubriously. The method which we mean not only to recommend, but to press upon the public attention, unites the three qualities which are essential in house illumination; but till our legislators and architects, and the leaders of public opinion, shall be more alive to the importance of scientific truths, in their practical phase, we can have no hope of its general adoption. True knowledge, however, advances with time. Vulgar prejudices are gradually worn down, and in less than a century, whether we have the electric light or not, we shall have our artificial suns shedding their beneficial rays under the guidance of science.

The present method of lighting our houses by burning the lights within its apartments, is attended with many evils. The intolerable increase of temperature in well lighted rooms, whether they are occupied with small or large parties—the rapid consumption of the oxygen which our respiratory system requires to be undiminished—the offensive smell of the unconsumed gas—the stench of the oleaginous products of combustion—the damage done to gilded furniture and picture frames,—the positive injury inflicted upon the eye by the action of a number of scattered lights upon the retina,—and the risks of fire and explosion,—are strong objections to the system of internal illumination. About half a century ago I proposed to illuminate our houses by burning the gas externally, or placing it within the walls of the house, or in any other way by which the products of combustion should not vitiate the air of the apartment. The plan was received with a smile. It had not even the honour of being ridiculed. It was too Quixotic to endanger existing interests, or tread upon vested rights. Owing to the extended use of gas, however, its evils became more generally felt, especially when large assemblages of persons were for many hours exposed to them; but no attempt was made to alter the existing system till 1839, when a Committee of the House of Commons was appointed to inquire into the best method of lighting the house. Many eminent individuals were examined; and in consequence of the report of the Committee, the new system was adopted of lighting from without, or, in which the air breathed by the members is entirely separated from the air which supplies the burners. A similar change has been made in lighting many other public halls; but the new system, in its most general aspect, has been carried out in one of the principal apartments in Buckingham Palace, where the light radiates from the roof, as if from the sky above, without any of the sources of light being visible. This method, of course, can be adopted only in halls, or apartments with an external roof. In all other cases difficulties must be encountered in houses already built; but we have no doubt that the ingenuity of the engineer and architect will overcome them, whether the method is to be accommodated to old buildings, or applied in its most perfect state to houses erected on purpose to receive it. But, however great may be these difficulties, it is fortunate that, whether we are to have the advantage of the electric light, or of a purer form of carburetted hydrogen gas, the mode of distributing it will be, generally speaking, the same, and we therefore need not hesitate to introduce the new method on the ground that it may be superseded by another.

Having so recently escaped from the inhumanity of a tax which prohibited the light and air of heaven from entering our dwellings, we trust that the governments of Europe will freely throw these precious influences into the dark abodes of their overcrowded cities, and that wealthy and philanthropic individuals will promote the same object in more limited spheres, and set the example of lighting, heating, and ventilating, according to the principles of science. Dr Arnott has already taught us how to heat our apartments with coal fires, without breathing either the gases or the dust which they diffuse. Why should we delay to light them also without breathing a noxious gas, and overlaying the organs of respiration with the nameless poisons generated in the combustion of the animal and vegetable substances employed in the furnishing of our apartments.

I cannot conclude these observations without referring to the use which may be made of them in our own city, notorious for the number of its dark and narrow lanes, and for the thousands of unlighted and unventilated dwellings which they contain. The devoted men who venture daily into these abodes of malaria and uncleanness, can alone describe to us the cimmerian darkness and the tainted atmosphere in which their pallid occupants live, and move, and have their being. They alone can paint the harrowing scenes which disease and destitution present to them in these joyless homes. To what extent evils like these can be remedied it is a sacred duty to inquire. To what extent they will be remedied by the large and expensive sanitary measures now contemplated, we do not venture to predict; but it is very obvious, that the upper and lower ends of the offensive lanes, which are to be intersected by the new streets, can derive little benefit from them in respect of ventilation, and none whatever in giving additional light to the houses which remain.

The only effectual mode of ventilating and lighting a dark and crowded apartment is, to strike out a large opening in the wall for the fresh admission of air, and to construct the window which is to close it so as to give the most copious entrance to the light of the sky. A process so cheap, so easily executed, and so obviously effectual, ought to be the very first step in any measure of sanitary reform; and it is clearly one which, if not effected by the philanthropy of the public, ought to be enjoined by Act of Parliament upon the house proprietors individually, or upon the citizens at large.

In giving expression to these opinions I did not expect to have them instantly, and in every point, confirmed by one of those devoted men to whom I have referred as the visitors of the dwellings of the poor. "The closer," says this anonymous philanthropist, in an article published within these few days,* "we look into the state of the homes of the poor in this city, the greater seems the extent of misery and degradation to be grappled with, and the more are we convinced that any sanitary scheme which does not contemplate the internal improvement of the dwelling-houses as one of its chief objects, will never go to the root of the

* "Edinburgh Evening Courant," Nov. 21, 1866.

evil. The opening of new streets will not alone be sufficient; the houses that remain must be so altered that they will admit light and fresh air, which, in many cases, are at present almost entirely excluded. We have visited dwellings in the Grassmarket and West Port, situated in airy localities, but which, by simply being out of repair and not having sufficient ventilation, are really uninhabitable. We cannot insist too strongly on the absolute necessity there is for the authorities directing their attention to this point. Where proprietors receive a sufficient return, they must be compelled to do their duty in this respect; and in cases where they can on good ground plead inability, the work must be done for them. The labour is so formidable that it will task the whole energies of the civic authorities, and yet leave much to be done by private benevolence."

In proceeding to give you a brief notice of the Fellows of the Society who have died during the present year, I regret to say that their number is greater than usual, and that we have to lament the loss of some of our most distinguished members.

David Craigie, an eminent Physician, was born in Leith on the 6th June 1793. He entered the University of Edinburgh in 1809, where he was distinguished by his classical acquirements and by his successful study of several of the Eastern languages, especially Hebrew and Persian. In 1816 he took the degree of M.D. in the University—the subject of his thesis being *De Vita Animali*. In 1818 he became a Member, and in 1819 one of the Presidents of the Royal Medical Society. Soon after this he gave two courses of lectures on Anatomy. Between 1820 and 1827 he assisted Dr Andrew Duncan in the editorship of the "The Edinburgh Medical and Surgical Journal," which Dr Duncan had founded in 1805. In 1827 Dr Christison joined Dr Craigie in editing this important journal, and this joint editorship continued till 1832, when Dr Christison was appointed to the Chair of Materia Medica, and from that time till 1855 Dr Craigie was its sole proprietor and editor. In 1832 Dr Craigie was elected a Fellow of the Royal College of Physicians, and in the following year a Fellow of the Royal Society, to whose Transactions he does not seem to have contributed. In 1833 he succeeded Dr James Gregory as physician to the Royal Infirmary; and while he lectured on Practical Medicine, he gave a course of Clinical Medicine, and published for several years Clinical Reports, which contain much valuable information.

For a number of years from 1846 Dr Craigie was prevented by ill health from carrying on the active duties of his profession, but he had so far recovered in 1858 that he was appointed one of the Examiners of the College of Physicians, a duty which on several occasions he performed for the University of St Andrews, who gave Degrees to candidates who had not studied at the United College.

In December 1861 he was elected President of the Royal College of Physicians, and he was able to preside at the dinner given on the 12th October 1863 to the Social Science Congress, at which Prince Alfred, Lord Brougham, and other eminent individuals were present. His health now began to give way; but though he regained it after a journey to Devonshire, symptoms of a serious disease appeared, and he died on the 17th May 1866, when he had nearly completed his seventy-third year.

Dr Craigie's principal works are his "Elements of General and Pathological Anatomy," and his "Elements of the Practice of Medicine," both of which have been much esteemed by the medical profession. The first of these was published in 1828, and was republished in 1848 in a second and enlarged edition. The second appeared in 1836, and displayed great erudition and professional knowledge, but owing to the high price at which it was published it had a comparatively limited circulation.

Sir John Hepburn Stuart Forbes was born at Dean House, Edinburgh, on the 25th September 1804. He was the second son of Sir William Forbes, who was a Fellow of this Society from 1804 till his death, and who was frequently a Member of Council. Sir William was an early friend of Sir Walter Scott, who in the fourth canto of Marmion, inscribed to his brother-in-law, Mr Skene, thus justly describes him :—

> And thou and I and dear loved R—(Rae),
> And one whose name I may not say,
> For not Mimosa's tender tree
> Shrinks sooner from the touch than he.

Sir William, the grandfather of Sir John, and the author of the life of Dr Beattie (who became in 1783 an original Member of the Royal Society), was in the same poem thus truly characterised :—

Scarce had lamented Forbes paid
The tribute to his Minstrel's* shade;
The tale of friendship scarce was told
Ere the narrator's heart was cold;
Far may we search before we find
A heart so manly and so kind.

* * * *

If mortal charity dare claim
The Almighty's attributed name,
Inscribe above his mouldering clay—
"The widow's shield, the orphan's stay."†

After a complete course of study at the University of Edinburgh, Sir John passed as Advocate in 1826, and he would have pursued the profession of the law, but for the death of his elder brother in 1826, and of his father in 1828, when he succeeded to the baronetcy. In 1833 he was elected a Member of this Society, and in 1834 he married Lady Harriet Kerr, the third daughter of the sixth Marquis of Lothian. Soon after this event he left Edinburgh, where his father and grandfather had resided, and lived almost wholly at Fettercairn House, where he occupied himself nearly exclusively in the management of his estates, and in the discharge of those numerous duties of patriotism and charity, which are so cheerfully and nobly performed by our Scottish landholders. Sir John was for many years an active member of the Highland and Agricultural Society of Scotland, and, for some time acted as its Treasurer. He was particularly interested in the advancement of agriculture as a science as well as an art, and he exerted himself in vain against the miserable parsimony of the Government to obtain a better endowment for the Agricultural Chair in our University.

In 1857 Sir John was appointed Convener of Kincardineshire, the duties of which he discharged with exemplary diligence, devoting himself to the business of the county with a zeal and self-sacrifice which the public did not fail to appreciate. At all hours and in all weathers he was found at his post, even while labouring under attacks of what proved to be a mortal disease.

* Dr Beattie's principal poem.

† In a note upon this passage Sir Walter says:—"Sir William Forbes of Pitsligo, Bart., unequalled perhaps in the individual affection entertained for him by his friends, as well as in the general respect and esteem of Scotland at large." See *Marmion*, canto iv. note ii.

Sir John acquired at the University of Edinburgh a taste for scientific pursuits. He was particularly attached to geology and meteorology, and he was one of the founders of the Meteorological Society of Scotland, which under his vice-presidency and the presidency of the Marquis of Tweeddale was doing valuable work for that important branch of scientific research.

After an illness of some months' duration, Sir John died in London on the 28th May 1866, in the sixty-second year of his age, and was succeeded in the baronetcy by his nephew, Mr. William Forbes. He left an only daughter, who was married in 1858 to the Hon. C. B. Trefusis, M.P., now Lord Clinton.

The Rev. James Macfarlane was the son of the Rev. John Macfarlane, Bridgton, Glasgow, and was born at Waterbeck, Dumfriesshire, on the 27th April 1808. After receiving his elementary education at the High School of Glasgow, he entered the University there, and distinguished himself both as a classical scholar and as a student of Divinity. While at College he obtained the degree of Master of Arts, and subsequently that of Doctor of Divinity, and in 1865 he was appointed Moderator of the General Assembly of the Church of Scotland. He was licensed by the Presbytery of Glasgow in 1830, and ordained in 1831 as pastor of the East Church in Stirling. In 1832 he was translated to St Bernard's Church in Edinburgh, and in 1840 he was presented to the living of Duddingston, where he continued to discharge his duties as a parish minister faithfully and efficiently till his death, which took place on the 6th of February 1866, in the 58th year of his age.

James Duncan, M.D., an eminent physician, was born at Perth on the 2d November 1810, and was the only son of Mr Duncan, of the well-known firm of Duncan, Flockhart, and Co. After receiving his early education at Perth, and at the High School of Edinburgh, he entered the University, and took the degree of M.D. in 1834. In the same year, he became a Fellow of the Royal College of Surgeons in London, and in 1835 a Fellow of the Royal College of Surgeons in Edinburgh. He spent about two years on the Continent, and prosecuted the study of anatomy and surgery at the medical schools of France, Germany, Austria, and Italy. On his return to Scotland, he acted as house-surgeon to our eminent countryman Mr Liston, in the Infirmary of Edinburgh, and in the hospital of University College, London. In 1837 Dr Duncan settled in Edinburgh as a medical practitioner, where he rose to great eminence, and enjoyed the largest general practice. He was surgeon to the Royal Infirmary for many years, and in that capacity delivered lectures on clinical surgery. He was elected a Fellow of this Society in 1858; but though he was the author of many valuable papers in the medical journals, he did not contribute to our Transactions.

Dr Duncan was in the habit of spending a few weeks in the country every autumn, as a relaxation from the duties of his extensive practice. In July 1866 he went to the Continent with his wife and family, committing to his son the charge of his patients in Edinburgh. During a few days' residence in Paris, when the cholera prevailed, he seems to have imbibed the germs of that fatal disease. On the 12th of August he was attacked with diarrhœa, which so completely yielded to its usual treatment that he went next day to Orleans. From Orleans he went to Tours, where, on the 15th, he was attacked with cholera, which proved fatal on the 16th August, in the 56th year of his age.

John Cay, of North Charlton, in the county of Northumberland, was born in Edinburgh on the 31st August 1790, and was the eldest son of Mr Robert Hodshon Kay, of North Charlton, Judge Admiral of Scotland. He was educated at the High School and University of Edinburgh, and was admitted into the Faculty of Advocates in 1812. In 1822 he was appointed Sheriff of the county of Linlithgow, an office which he held for forty-three years, and the duties of which, as a sound lawyer and a judicious judge, he discharged with great credit to himself and great satisfaction to the public. Mr Cay took a great interest in the literary and scientific institutions of Edinburgh. He was elected a Fellow of the Royal Society in 1821, and contributed to its Transactions a short notice of a bronze spear-head found on his property in Northumberland. He was also a Member of the Royal Scottish Society of Arts, and was one of the annual Presidents of that important institution. Mr Cay was the author of a work on "The Reform Act," and of two small pamphlets—one an "Analysis of the

Burgh Begistration Act," and the other entitled, "Outlines of the Procedure at Elections for Members of Parliament." When Mr Cay was residing at Burntisland in August 1865, he accidentally fell, and received a severe fracture of the thigh-bone. His naturally robust constitution at first gave hopes of his recovery; but these hopes were disappointed, and after much suffering, born with great Christian resignation and patience, he died in December 1865, in the 75th year of his age.

John Archibald Campbell, Clerk to the Signet, and Sheriff-Clerk of Mid-Lothian, was born in the year 1788, and was the son of John Campbell, Esq. of The Citadel, Leith. In 1813, he was admitted into the Society of Writers to the Signet. From that year to 1816, he was joint Crown Agent; and from 1843 to 1859, when he retired into private life, he was Sheriff-Clerk of Mid-Lothian. Mr Campbell was the founder or joint-founder of some of the most thriving institutions in Edinburgh, and he took an active part in those municipal changes in which the public were so deeply interested.

Mr Campbell took a great interest in archæological researches. He was a Member of the Antiquarian Society, and of the Royal Society of Arts, and he was elected a Fellow of this Society in 1837. He died in Edinburgh on the 7th September 1866, in the 78th year of his age.

James Ivory, one of the Lords of Session, was born at Dundee on the 29th February 1792, and was the son of an engraver of great mechanical skill, and superior literary attainments. He was educated at the Dundee Academy, and acquired there a taste for mathematics, which he afterwards cultivated, and found an agreeable relaxation during the rest of his life. His devotion to mathematics was no doubt encouraged by his uncle, James Ivory, K.H., who was one of the most eminent of our Scottish mathematicians. Mr Ivory was educated at the University of Edinburgh, where he was distinguished by his literary as well as his mathematical acquirements. In 1816 he was called to the Scottish Bar, where he soon rose to great eminence in his profession. In 1830, Lord Jeffrey appointed him one of his depute-advocates, and in 1832 he was appointed Sheriff of Caithness. In the following year he was transferred to the Sheriffdom of Bute, and in 1839, he was appointed Solicitor-General for Scotland. In the following year he was appointed one of the Lords of Session, and in 1849 one of the Judges in the Court of Justiciary. In the same year he was elected a Fellow of this Society. In these various legal offices which Lord Ivory filled his judgments were distinguished by their great soundness, as well as by the elegant diction in which they were expressed.

Before he was called to the Bar, Lord Ivory published a treatise on the Form of Process, and in 1827, the first volume of his edition of Erskine's Institutes, the second volume of which appeared some years afterwards. In consequence of ill health, Lord Ivory resigned his seat on the bench in 1862. Since that time, his health gradually declined, and he was carried off by a stroke of paralysis on the 17th October 1866, in the 74th year of his age.

Lord Ivory was married, in 1817, to the daughter of Mr Lawrie, Deputy-Gazette Writer for Scotland. His eldest son, Mr Thomas Ivory, is counsel to the Commissioners of Woods and Forests; and his third son, Mr William Ivory, Sheriff of Inverness-shire.

John Frederick Louis Hausmann, a distinguished geologist and mineralogist, was born at Hanover on the 22d February 1782. After completing his studies at Brunswick and Gottingen, he was appointed in 1803 to a situation in the administration of the Mines at Brunswick, and in 1805 he made a geological tour through Norway and Sweden. In 1809 he was appointed by the government Inspector General of the Mines and Saltworks of Westphalia, but he soon resigned this office in order to devote himself to the study of Mineralogy. In 1811 he was appointed Professor of Mineralogy and Geology in the University of Gottingen, and while he filled this important chair, he devoted himself to the examination of the Harz Mountains, and made many geological excursions in Sweden, Holland, France, Spain, and England. He was councillor of state in Hanover, an active member of the Royal Society of Gottingen, and a Corresponding Member of the Academy of Sciences, in the Imperial Institute of France.

Professor Hausmann was the author of various Articles and

Memoirs in the German Journals, and in the Transactions of the Royal Society of Gottingen, and of several separate works on Chemistry, Geology, and the Mines of Germany; but his chief productions are his "Travels in various parts of Scandinavia," published in five volumes between 1811 and 1818, and his "Manual of Mineralogy," published in 1813, and reprinted in 1828 and 1847.

Henry Darwin Rogers, LL.D., F.R.S., and F.G.S., a distinguished American Geologist, Professor of Geology in the University of Glasgow, and one of the Foreign Members of this Society, was born in Philadelphia on the 1st of August 1808, and was the third of four brothers, all of whom have been devoted to the cultivation and teaching of Physical Science, their father Dr Patrick Kerr Rogers, having himself been Professor of Natural Philosophy and Chemistry in the College of "William and Mary" in Virginia. At the early age of twenty-two he was elected Professor of Natural History and Physics in Dickinson College, Carlisle, but being desirous of improvement, he resigned his chair, and spent above a year in England, where he studied Analytical Chemistry in the laboratory of Dr Edward Turner, and had the privilege of accompanying Sir Henry De la Beche in his geological excursions.

On his return to Philadelphia he was appointed to the chair of Geology in the University of that city, and while discharging its duties, which he did for many years, he carried on many important scientific researches. Along with Professor A. D. Bache, he made an extensive series of Experiments on the Ashes of Coal, and in conjunction with his brother W. B. Rogers, he made experiments on "The Laws of the Elementary Voltaic Battery."

The first fruits of his geological labours was his Report of the Survey of the state of New Jersey, which he published in 1835 with a large geological map. While engaged in that survey he was employed by the legislature to make a survey of the great state of Pennsylvania, and to this herculean task he devoted many years of his life. It commenced in 1836, and with the aid of a staff of able coadjutors, it was completed in 1855, and published in three volumes, under the title of "The Geology of Pennsylvania, a Government Survey, with a General Survey of the Geology of the United States, Essay on the Coal

Formation and its Fossils, and a description of the Coal Fields of North America and Great Britain." This great work, which was printed and published in Edinburgh, appeared in 1858, and is illustrated with an immense number of maps, geological sections, sketches of scenery, and fossils, executed by Messrs W. and A. K. Johnston.*

It was during the progress of this great survey that Professor Rogers and his brother already mentioned, who was in charge of the survey of the state of Virginia produced their remarkable Memoir "On the Physical structure of the Appalachian Chain, as exemplifying the laws which have regulated the Elevation of Mountain Chains, &c." This Memoir excited a deep interest when read at the meeting of the association of geologists and naturalists which was held at Boston in 1842, and it has been published in its memoirs for that year. Professor Rogers was one of the founders of the association which was expanded into the Association for the Advancement of Science.

After having given to the world the results of his geological researches, he took up his residence at Boston, devoting himself to his favourite studies, and occasionally delivering courses of lectures in the Lowell Institute, which he did with a readiness and felicity of diction rarely exhibited by the cultivators of physical science.

While in Edinburgh, engaged in the publication of his great work, a vacancy took place in the Regius Professorship of Natural History in the University of Glasgow, and through the influence, we believe, of the Duke of Argyll, he was appointed to that chair in 1857. The lectures which he delivered in this new position were greatly admired, and he might have looked forward to many years of rest from the toils of his early life; but in the severe exposures which he endured in his geological surveys, his constitution was so enfeebled, and in the composition and superintendence of his great work he had so overworked his mind, that his colleagues observed for two or three years a gradual failure of health, which terminated fatally at Glasgow on the 29th of May 1866, in the 58th year of his age.

Alan Stevenson, an eminent engineer, was born in Edinburgh,

* About the same time Professor Rogers published an elaborate paper "On the Origin of the Appalachian Coal Strata, Bituminous and Anthracite."

in 1807, and was the eldest son of Mr Robert Stevenson, engineer to the Scottish Lighthouse Board. He was educated at the High School and University of Edinburgh, where he obtained the Felloes' Prize as an advanced student of Natural Philosophy. In 1826 he took the degree of M.A., and in 1840 the University of Glasgow conferred on him the degree of LL.B.

Although he had himself desired to study for the Church, yet he was afterwards led to adopt the profession of a civil engineer, for which he was so highly qualified by his previous education. For many years he was Clerk of Works to the Scottish Lighthouse Board, and on the death of his father in 1842 he succeeded him as their engineer in chief.

During the comparatively short time that his health enabled him to discharge its duties, he erected several Lighthouses on the Scottish coast, the most important of which is that at Skerryvore, a magnificent work, which is not surpassed by any similar structure in the world. It was began in 1838, and completed in 1844. The Eddystone Lighthouse, by the celebrated Smeaton, and that on the Bell Rock, by Mr Robert Stevenson, lose their grandeur when placed in comparison with the gigantic tower at Skerryvore. An interesting account of this great work was published by Mr Stevenson in 1848, under the title of "Account of the Skerryvore Lighthouse, with notes on the Illumination of Lighthouses."

In 1850 he published a very valuable work, entitled "A Rudimentary Treatise on the History, Construction, and Illumination of Lighthouses."

To his great accomplishments as a civil engineer, Mr Stevenson added classical acquirements of no ordinary kind. He had mastered the languages of Greece and Rome, and acquired a critical knowledge of Italian and Spanish.

In 1852, when a severe nervous affection had unfitted him for the duties of his office, he beguiled his hours of suffering by translating into English verse the ten hymns of Synesius, Bishop of Cyrene. These translations, with some pieces of his own, were printed in the present year for private circulation, and have been much admired.

Mr Stevenson was elected a Fellow of this Society in 1838. He was a Member of the Institution of Civil Engineers, and he had the honour of receiving complimentary medals from the

Emperor of Russia, and from the Kings of Prussia and Holland, as acknowledged merits as a lighthouse engineer. When his health prevented him from attending the meetings of this Society, he gave in his resignation, but the Council declined to accept it, and continued to him all the privileges of a Fellow.

The long illness of Mr Stevenson, produced no doubt by the severity of his exposures at Skerryvore, and the responsibilities and anxieties attaching to so great and difficult a work, was borne with exemplary patience and resignation, and he died at Portobello, as a Christian should die, on the 23d December 1865, in the 58th year of his age.

Robert Kaye Greville, a distinguished botanist, was born at Bishop Auckland, on the 13th December 1794, and was the son of the Rev. Robert Greville, Rector of Wyaston in Derbyshire. While he was receiving his education under his father's roof, he spent his leisure hours in the study of plants; and so rapid was his progress, that before he was nineteen he had made careful coloured drawings of between one and two hundred native plants. Being intended for the medical profession, he went through the usual curriculum of four years in London and Edinburgh; but circumstances having rendered him independent, he resolved to devote himself to his favourite study.

In 1816 he married a daughter of Sir John Eden, Bart. of Windleston, and in the same year he came to Edinburgh, where he studied human and comparative anatomy under Dr Barclay.

In 1816 he joined the Wernerian Society, to which he communicated a series of papers on Algæ, which are published in the Transactions of the Society. In 1823 he published the first volume of his great work, entitled "Scottish Cryptogamic Flora," which he completed in six volumes, in 1828. His "Flora Edinensis," containing a description of the plants in the neighbourhood of Edinburgh, appeared in 1828; and in 1830 his "Algæ Britannicæ," a description of the marine and other inarticulated plants in the British Islands, belonging to the Order Algæ. In conjunction with Dr Hooker he published the "Icones Filicum," a splendid work, containing drawings and descriptions of ferns, either very rare or which had been incorrectly figured.

After he had completed these great works, Dr Greville made a special study of the Diatomaceæ, of which he formed a most valuable collection, all the specimens being put upon slides and finely labelled. He wrote upwards of twelve papers descriptive of these minute organisms. For these papers, and also for his contributions to Cryptogamic Botany, the Council of this Society awarded to him, in 1862, the medal founded by his friend, Dr Patrick Neill.

In the brief space to which these biographical notices are necessarily limited, it would be impossible to enumerate even the large number of important papers which Dr Greville published in the Wernerian Transactions, and in the various scientific periodicals of the day, and to give an intelligible account of the important facts which they contain. We must notice, however, his collections of natural history, which are extremely valuable, and not less important to science than the works in which they are described.

His fine collection of Phanerogamic plants, and of Ferns, Mosses, Fungi, and Algæ, increased by subsequent purchases, and consisting of about 50,000 species, constitutes our University Herbarium in the Botanic Garden. His collection of Insects, purchased also by the University, is now deposited in the Museum of Science and Art. His great collection of Diatoms has been purchased by the British Museum; and his collection of land and fresh water Mollusca has been purchased for the Museum of Science and Art in Edinburgh.

The value of Dr Greville's works has been greatly enhanced by the beauty and accuracy of his drawings. His pencil was that of an accomplished artist; and at one time he took up landscape painting as a profession, and contributed several paintings to the Exhibitions of the Scottish Academy.

The works of Dr Greville were as highly appreciated in foreign countries as they were in his own. In 1821 he was elected a Fellow of this Society. In 1825 he was chosen a Member of the Royal Irish Academy, and in 1826 the University of Glasgow conferred upon him the degree of Doctor of Laws. He was also a Fellow of the Linnean Society, and either a corresponding or an honorary member of a great number of Natural History Societies in Europe and America.

In his latter years Dr Greville lived happily at his beautiful villa of Ormelie, near Edinburgh, where, with some exceptions, his health was generally good. Towards the end of last May, however, he imprudently fell asleep on the damp grass, and brought upon himself an attack of inflammation of the lungs, which, in the course of a week, terminated fatally on the 4th of June 1866, in the 72d year of his age.

Dr Greville was a man of rare accomplishments, and of no ordinary virtues. His studies were not confined to science, nor his ambition limited to the honours which it merits, and the fame which it brings. His large heart embraced every measure of philanthropy, whether national or local,—whether originating in vicious legislation, in the necessary inequalities of social life, or bearing upon the victims of ignorance, intemperance, and crime. Nor did he take a less interest in those higher questions which disquiet the inner and nobler life of man, stretching beyond that bourne from which no traveller returns, and affecting interests which time cannot measure nor space define. He had pondered in the lesser world over mysteries which he failed to comprehend,—over marvels of life which startled science and rebuked reason; and he submitted with the same reverence to those deeper mysteries which human instruments had equally revealed. Faith, and science falsely so called, had to him, and has, I hope, to many of us, two opposite horizons—the one where the sun rises, and the other where he sets. In the auroral and meridian light of the one he studied, and lived, and died; and in the murky twilight and midnight darkness of the other, he wept over the fallen stars of science,—the Sappers and Miners of our Faith.

Charles Maclaren, an eminent geologist, was born in Ormiston, East Lothian, on the 7th October 1782, and was the only son of his father, who was a small farmer. His uncle by the mother's side was a smith in the household of George III., and his nephew would have been brought up to the same profession, had it not proved incompatible with the delicacy of his constitution. After acquiring a good education at different parochial schools, and acquiring at home a knowledge of modern languages and of some branches of science, he spent some years as clerk and book-keeper to several Edinburgh firms.

In the year 1816 Mr Maclaren, in conjunction with Mr William Ritchie, established the *Scotsman*, a newspaper which, though persecuted in its early life, has acquired a greater popularity, and reached a higher circulation than any of our Scottish journals. The first number was published on the 25th January 1817, and, speaking generally, Mr Maclaren had the honour of being its Editor till the year 1847, when he resigned it into the hands of its present able conductor. Thus freed from the labours and anxieties of a public journalist, Mr Maclaren was able to devote his whole time to his favourite studies. He made numerous geological excursions to different parts of Scotland, acquiring that accurate knowledge of the science which he displayed in his later works.

Having taken a great interest in the speculations respecting the Plain of Troy as described in the Iliad, he published in 1822 a dissertation on its topography; and in 1847 he visited the locality itself, and acquired that thorough knowledge of the subject, which he communicated to the public in 1863, under the title of "The Plain of Troy Described, and the identity of the Ilium of Homer, with the new Ilium of Strabo proved, by comparing the poet's narrative with its present Topography." The views contained in this able and interesting work have been fully adopted by the most distinguished scholars in this country and on the continent.

Among his numerous geological writings which have appeared in the *Scotsman* and in other journals, the most important is his "Geology of Fife and the Lothians," which appeared in 1839. It contains a description of the wide valley stretching from the Ochils on the North to the Lammermuirs on the South, and fully established Mr Maclaren's reputation as a geologist.*

Among the more prominent points in Mr Maclaren's character, was his remarkable sagacity, and his faith in the power and progress of science, a quality not often possessed by its cultivators. His sagacity was strikingly displayed in predicting the great destiny of the American Republic, a sentiment which he drew from his knowledge of the politics and statistics of that great and prosperous nation. The article on America, in the "Encyclopædia Britannica," was from his pen, and also the article on Troy, in which he describes the interesting results to which we have already referred.

His sagacity was no less displayed, and his faith in science too,

* A recent edition, with corrections and additions by the Author, has just been published.

when, so early as 1824, he predicted the great success of railway travelling, and spoke of locomotion at the rate of twenty-four miles an hour, or quicker than a race-horse, as a very probable event. Science, however, has outstripped the tardy estimate of our author, and the still tardier one of George Stephenson. A flight of sixty miles an hour with a cargo heavier then a ship of war, which many of us have made, would have shaken the nerves even of these sanguine speculators. When we learn that a distinguished man of science announced in the "Edinburgh Review" the impossibility of navigating the Atlantic by steam, we need not wonder at the slow-coach convictions of the uneducated world.

Mr Maclaren was elected a Fellow of this Society in 1837, and of the Geological Society of London in 1846. He was an active member of the British Association, and for the two last years he was President of the Geological Society of Edinburgh.

After resigning the editorship of the *Scotsman*, he retired to his villa of Moreland Cottage, near Edinburgh, where he spent the rest of his life. His health was generally good, and his mental powers unimpared, when on the morning of the 27th August he experienced a stroke of paralysis, which carried him off without suffering on the 10th of September 1866, in the 84th year of his age.

William Whewell, D.D., F.R.S., a distinguished writer on almost every branch of science, was born at Lancaster on the 24th of May 1794. He was the son of a house carpenter, whose little library supplied the first books which excited the literary ambition of his son. From the Grammar School of Lancaster he went to Hevenham, in order to be qualified for holding an Exhibition at Trinity College, Cambridge. After gaining this Exhibition of £50 a-year, he commenced residence at Cambridge, and distinguished himself so much that in his first year he gained a scholarship and a Foundation Sizarship. In the mathematical tripos of 1816 he graduated as second wrangler. In 1817 he was elected Fellow of Trinity, and soon after this he began to lecture on mathematics as assistant tutor, at the salary of £75 per annum. He was elected a Fellow of the Royal Society of London in 1820, and he enriched their Transactions from 1833 to 1840 with *twelve* papers, entitled

"Researches on the Tides." In 1837, after the publication of the ninth series, the Royal Society conferred upon him the Royal Medal, in recognition of the great value of his Researches.*

In 1821 he published his "Syllabus of an Elementary Treatise on Mechanics," and in 1823 his "Treatise on Dynamics." In 1823 he became Tutor of Trinity, an office which he held till 1839.

In 1828 he was appointed Professor of Mineralogy; and if we desired an example of the unrivalled power with which he mastered a subject with which his previous studies had but little connection, it will be found in the admirable report "On the Progress and Present State of Mineralogy," which he communicated to the British Association in 1832.† In 1833 Dr Whewell published his Bridgewater Treatise, entitled "Astronomy and General Physics, considered in reference to Natural Theology."

In 1837 he published his principal work, entitled the "History of the Inductive Sciences, from the Earliest to the Present Times." In appreciating the value of this noble record of physical truth, and admiring the capacity and vast acquirements of its author, we must at the same time regret the numerous deficiencies in the work, and its doubtful decisions in the controversies of science. As a necessary sequel to this work he published, in 1841, "The Philosophy of the Inductive Sciences," a work of able and ingenious speculation, which the public did not so gratefully receive.

In 1837 he preached before the University four sermons on the foundation of morals, and having been thus led to the study of this branch of knowledge, he accepted, in 1838, the Chair of "Moral Philosophy and Casuistry," as he called it. While he occupied this chair he composed "Lectures on the History of Moral Philosophy in England," "Lectures on Systematic Morality," and "Elements of Morality and Polity," which appeared in 1845. His attention being thus directed to the subject of international law, he published a condensed translation of Grotius' "De Jure Belli et Pacis;" and regarding this subject, as we now find it to be, of national importance, he left to the University an ample legacy

* See Phil. Trans. 1833, p. 147; 1834, p. 15; 1835, p. 83; 1836, pp. 1, 131, 129; 1837, pp. 79, 227; 1838, p. 231; 1839, pp. 151, 163: 1840, pp. 161, 255.

† Report in 1832, pp. 322–365.

to found a Chair of International Law, with scholarships for the most distinguished students.

In 1841 he married Miss Marshall, and a few months afterwards he was, on the resignation of Dr Wordsworth, made Master of Trinity. The domestic happiness which he enjoyed in this new position was marred by a long and painful illness of Mrs Whewell, and his grief on this occasion is said to have been so profound that, in order to occupy his mind in this season of affliction, he composed his work of "The Plurality of Worlds," which was published in 1853. A more probable opinion is, that the book was written to quiet certain doubts which had arisen in the mind of his wife respecting the doctrine of redemption, on the ground of the comparative insignificance of our planet, and of the race in whose behalf the Deity interposed. But whatever was the motive or state of mind under which this singular work was composed, we can hardly admit that a mind so richly endowed, and so strong in its religious conviction, could have believed in the assertions that the nebulæ are "dots and lumps of light;" that "the distant stars are sparks struck off from the potter's wheel in the formation of the universe;" that Jupiter is "a mere sphere of water with a few cinders in its centre;" that the ninety asteroids are "bits of a planet that had failed in the making;" and that the nineteen satellites of the solar system "are pieces of vapour neatly wound into balls."

But, however sternly posterity may denounce the paradox, "that our earth is the oasis in the desert of the solar system," "the really largest planetary body" it contains, and "the only world in the universe," they will forgive its author on account of the noble sentiments, the lofty aspirations, and the suggestions almost divine, which mark his closing chapter on the future of the universe. In the bright noon of his intellectual life, Dr Whewell had hazarded the sentiment, "that science was its own reward," requiring neither patronage for its extension, nor liberality to its cultivators; but when a high position rewarded his own acquirements and stimulated his own labours, he turned with sympathy to his less fortunate fellow-workmen, and looked for their reward to "a universal and perpetual peace," in which the advancement of human knowledge should be the great object of every social and national combination. "Were the nations of the earth," says he, "to employ for the promotion of human knowledge a small fraction only of the means, the wealth, the ingenuity, the energy, the combination which they have employed in every age for the destruction of human life and human means of enjoyment, we might soon find that what we hitherto knew is little compared with what man has the power of knowing." Would that these words were placed in letters of gold above the portal of every college in the empire, and pondered by every student to whom the name of statesman, and patriot, and Christian, is dear.

Though happy in his domestic relations, they were to Dr Whewell the cause of the deepest sorrow. The ill health of Mrs Whewell had for several years been a source of great anxiety and care, and in December 1855 he mourned her loss in a volume of elegiac verses, printed for private circulation. In 1858 he married Lady Affleck, the widow of Sir Archibald Affleck, and after eight years of uninterrupted happiness, he had again, in 1865, to resign himself to another domestic bereavement.

After some months of sorrow and seclusion, Dr Whewell was so far able to resume his studies, as to write the article on "Comte and Positivism," which appeared in "Macmillan's Magazine" for March 1866; and he prepared for "Frazer's Magazine" a paper "On Grote's Plato," which was destined to be the last of his works.

On the 24th February 1865, when taking a ride, he met his own carriage with ladies who were his guests, and turned to follow them home. His horse became excited and ran off, and losing all control over it, owing to a weakness in his left arm from a previous fall from horseback, he fell upon his horse's neck and then to the ground upon his head. A concussion of the brain, which had previously been in a shrunk state, terminated fatally on the 6th March 1866, in the 72d year of his age.

The great talents and acquirements of Dr Whewell were as highly appreciated in foreign countries as in his own. His Syllabus of Mechanics, and his "Mechanical Euclid," were translated into German. He was a corresponding member, in the section of Philosophy, of the Academy of Moral and Political Sciences in the Imperial Institute of France, and an honorary or corresponding member of various academies in Europe and America.

Alexander Macduff, Esq., was born at Springfield Cottage, near Perth, on the 5th December 1816, and was the son of Alexander Macduff of Bonhard. He received the rudiments of his education at the Grammar School of Perth, and also at the Perth Academy, where he obtained the first prize for general proficiency during the session 1831–2. He entered the University of Edinburgh in 1832, and, after attending the usual course of instruction, he began the study of law in 1835. He passed as writer to the Signet in 1839, and continued in the profession in partnership with Mr Welsh of Collin till 1845, when he retired to Perthshire, where he lived upon his estate from 1847 to 1860, discharging the various important duties which our landed proprietors are called upon to perform. In 1859 he acted as Secretary to the Royal Commission on Roads and Bridges in Scotland. In 1860 he took up his residence in Edinburgh, and was appointed Vice-Convener and Secretary of the Church Endowment Scheme. In 1865 he was elected Secretary to the Highland and Agricultural Society of Scotland, and he had hardly entered upon the duties of his office when he was attacked with pleurisy, and died at Edinburgh on the 21st March 1866, in the 50th year of his age.

William Bonar was born in Edinburgh on the 3d January 1798, and was the third son of Andrew Bonar of Kimmerghame and Warriston, banker in Edinburgh. After receiving his classical education at the High School of Edinburgh, he went through the usual course of study at the University.

In 1817 he was admitted a partner of the bank of Messrs Ramsay, Bonar, & Co., and he continued to discharge its duties till it was merged in the Bank of Scotland.

Mr Bonar was one of a body of excellent men in Edinburgh who employ their wealth and their leisure in promoting and managing the many religious and charitable institutions by which Edinburgh is distinguished. He was a man of fervent piety, and along with his brother, Mr Andrew Bonar, now the only survivor of four brothers, he employed a missionary to look after the ignorant and neglected poor. In the same good cause, Mr Bonar wrote several religious tracts during his residence in Edinburgh.

Having purchased Warriston from his brother, Mr James Bonar, he lived there with his family, till the interference of railways with its amenity drove him to Babbicombe, in Devonshire, where he continued those benevolent labours which he had so zealously prosecuted during his residence in Edinburgh.

Mr Bonar was elected a Fellow of this Society in 1822. He took a great interest in matters of science, but specially in the great inventions which were in his time bearing such noble fruit.

Mr Bonar died at Malvern, where he had gone for the recovery of his health, on the 9th of November 1866, at the age of 68.

James Stevenson was born at Paisley on the 28th April 1786, and was the son of Mr Stevenson, a silk-gauze manufacturer in that town. He was educated at the Grammar School there, and afterwards went to Glasgow, where he remained for thirty years in business as a merchant and cotton-spinner. In 1844 he removed to South Shields, where he became senior partner in the Jarrow Chemical Works, the importance and extent of which were greatly increased by his energy and talents for business.

At South Shields he took a deep interest in the welfare of the working classes, founding excellent schools for their children, and reducing, as far as practicable, the work usually done on the Sabbath. He established the "North and South Shields Gazette," a newspaper by which the public interests of these two large towns were greatly advanced. Mr Stevenson was likewise the most active promoter of a company for supplying South Shields with water, which previously had been chiefly derived from surface wells. He was an elder in the Free Church, and an active and liberal friend of the Presbyterian Church in England.

In 1854 he retired from active business, and spent the rest of his life in Edinburgh. He was elected a Fellow of this Society in 1865, and he died in his house, in Randolph Crescent, on the 13th June 1866, in the 81st year of his age.

William Thomas Brande, an eminent chemist, was born in 1786, and was the grandson of a physician who came from Hanover with one of the Georges. After being educated at Westminster School, he went to complete his education in Hanover, but, on account of the disturbed state of Germany, he returned to England in 1803, and entered St George's Hospital, attending the lectures and dissecting room of that institution. At this time he communicated several papers to Nicholson's Journal: one *On Benzoin* in 1805;* one on *The Theory of Respiration* in 1805;† and another *On the Enamel of the Teeth* in 1806.‡ In 1806 he communicated to the Royal Society a paper *On Guaiacum*, which was published in their Transactions. In 1808 he examined the calculi in the Hunterian Museum, and gave lectures on Chemistry in Dr Hooper's in Cork Street. At this time he became a member of the New Medical School in Windmill Street, and began his career as a Teacher and Demonstrator in Chemistry, publishing many important papers in the "Philosophical Journals," and in the "Transactions of the Royal Society." In 1809 he was elected a Fellow of the Royal Society. In 1813 he received the Copley Medal for his communications to the Society; and on the death of Dr Wollaston, in the same year, he succeeded him as Principal Secretary to that body, an office which he held till 1826. In 1812 he was elected Professor of Chemistry and Materia Medica to the Apothecaries' Company, of which he became Master in 1851. On Sir Humphry Davy's recommendation, he was elected Professor of Chemistry to the Royal Institution; and he occupied that position for many years, in conjunction with Dr Faraday. He was elected an Ordinary Fellow of this Society in 1815, but made no contributions to our Transactions. In 1816 he began, in conjunction with Dr Faraday, to publish "The Journal of Science and the Arts, edited at the Royal Institution," which was continued in sixteen volumes till 1825, when he was appointed Superintendent of the Die department in the Mint. In 1836 he was appointed Fellow, and in 1846 Examiner in the London University. In 1853 he received the honorary degree of D.C.L. from the University of Oxford. In 1823 he published his his "Manual of Pharmacy," and in 1842 his "Dictionary of Art and Science."

He took an active interest in the establishment of the University of London, and was one of its Senate. Mr Brande was particularly distinguished as an expositor of science, as is testified by the success of his "Manual of Chemistry," which has passed through six editions, and has been translated into the leading foreign

* Vol. x. p. 89. † Vol. xi. p. 79. ‡ Vol. xiii. p. 214.

languages. He was one of the eminent galaxy of chemists that has rendered the Royal Institution so illustrious. The successor of Davy and colleague of Faraday could not have failed to have had a marked influence on the progress of chemistry; and though he has not left behind him many original memoirs, he has certainly done much to diffuse a knowledge of that science, and to render its study attractive by the clearness of his exposition of its leading truths.

The following statement respecting the Members of the Society was read by the Chairman:—

I. Honorary Fellows—		
Royal Personages,	2	
British Subjects,	19	
Foreign,	35	
Total Honorary Fellows,		56
II. Non-Resident Member under Old Laws,		1
III. Ordinary Fellows—		
Ordinary Fellows at November 1865,		273
New Fellows, 1865–66—Sir James E. Alexander, Adam Black, David Chalmers, W. D. Clark, Thomas Constable, David Douglas, Dr James Dunsmure, William Euing, James Falshaw, Joseph M. Joseph, Dr A. Keiller, Alexander Macduff, John M'Culloch, John Macnair, Professor David Masson, Dr Arthur Mitchell, Dr Charles Morehead, Right Rev. Bishop Morrell, Thomas Nelson, Dr John Alexander Smith, Professor Spence, Dr Thomas Grainger Stewart, Dr Fraser Thomson, John K. Watson, Dr Patrick Heron Watson,		25
		298
Deduct Deceased—William Bonar, William Thomas Brande, John Archibald Campbell, John Cay, Dr David Craigie, Dr James Duncan, Sir John H. S. Forbes, Dr R. Kaye Greville, Hon. Lord Ivory, Alexander Macduff, Rev. Dr Macfarlane, Charles Maclaren, Alan Stevenson, James Stevenson,	14	
Resigned—Alex. Christie, William Handyside, Dr J. P. Macartney, Dr Pagan,	4	
Cancelled for Non-payment of Admission Fee—W. D. Clark, Dr Joseph,	2	
Cancelled for Non-payment of Annual Subscriptions—James Hannay, Dr M'Kinlay,	2	
Total Deductions,		22
Total Number of Ordinary Fellows at November 1866,		276

The following Donations to the Library were announced:—

Philosophical Transactions of the Royal Society of London. Vol. CLVI. Part 1. 4to.—*From the Society.*

Proceedings of the Royal Society of London. Nos. 83–87. 8vo.—*From the Society.*

Transactions of the Zoological Society of London. Vol. V. Part 5. 4to.—*From the Society.*

Proceedings of the Zoological Society of London. 1865. Parts 1 . 8vo.—*From the Society.*

Report of the Council of the Zoological Society of London. 1866. 8vo.—*From the Society.*

Quarterly Journal of the Geological Society. Vol. XXII. Parts 2–4. London, 1866. 8vo.—*From the Society.*

Journal of the Chemical Society. Nos. 41–47. London, 1866. 8vo.—*From the Society.*

Journal of the Statistical Society of London. Vol. XXIX. Parts 2 and 3. 8vo.—*From the Society.*

Journal of the Linnean Society. Botany, Vol. IX., Nos. 37, 38; Zoology, Vol. IX., Nos. 33 and 34. London, 1866. 8vo.—*From the Society.*

Proceedings of the Meteorological Society. Vol. III. Nos. 23–26. London, 1866. 8vo.—*From the Society.*

Journal of the Royal Geographical Society. Vol. XXXV. London, 1865.—*From the Society.*

Proceedings of the Royal Geographical Society. Vol. X. Nos. 3–6. London, 1866. 8vo.—*From the Society.*

Journal of the Royal Horticultural Society of London. Vol. I. Part 3. 1866. 8vo.—*From the Society.*

Proceedings of the Royal Horticultural Society of London. Vol. I. (New Series). Parts 4 and 5. 1866. 8vo.—*From the Society.*

Proceedings of the Society of Antiquaries of London. Vol. II. No. 7. 1864. 8vo.—*From the Society.*

Catalogue of a Collection of Printed Broadsides in the possession of the Society of Antiquaries of London. Compiled by Robert Lemon, Esq. London, 1866. 8vo.—*From the Society.*

Proceedings of the Royal Medical and Chirurgical Society of

London. Vol. V. Nos. 4 and 5. 1866. 8vo.—*From the Society.*

Transactions of the Royal Medical and Chirurgical Society of London. Vol. XL. 8vo.—*From the Society.*

Journal of the Royal Asiatic Society of Great Britain and Ireland. Vol. II. Part 1. New Series. London, 1866. 8vo.—*From the Society.*

Journal of the Asiatic Society of Bengal. History: Part 1, No. 4, 1865; Part 1, No. 1, 1866. Physical Science: Part 2, No. 4, 1865; Part 2, No. 1, 1866. Calcutta, 1866. 8vo. —*From the Society.*

Proceedings of the Asiatic Society of Bengal, with Index for 1865. Nos. 1–3, for 1866. Calcutta. 8vo.—*From the Society.*

Memoirs of the Literary and Philosophical Society of Manchester. Third Series. Vol. II. 8vo.—*From the Society.*

Proceedings of the Literary and Philosophical Society of Manchester. Vols. III. and IV. 1864–65. 8vo —*From the Society.*

Journal of the Royal Geological Society of Ireland. Vol. 1. Part 2. Dublin, 1866. 8vo.—*From the Society.*

The American Journal of Science and Arts. Vol. XLI. No. 123; Vol. XLII. No. 124. New Haven, 1866. 8vo.—*From the Editors.*

The Canadian Journal of Industry, Science, and Art. New Series. No. LXII. Toronto, 1866. 8vo.—*From the Canadian Institute.*

Proceedings of the Academy of Natural Sciences of Philadelphia. Nos. 1–5. 1865. 8vo.—*From the Academy.*

Proceedings of the American Philosophical Society. Vol. X. No. 75. Philadelphia, 1866. 8vo.—*From the Society.*

Catalogue of the Library of the American Philosophical Society. Part 2. Philadelphia, 1866. 8vo.—*From the Society.*

Journal of the North-China Branch of the Royal Asiatic Society. New Series. No. 2. 1865. Shanghai. 8vo.—*From the Society.*

Illustrated Catalogue of the Museum of Comparative Zoology at Harvard College. No. 2. Cambridge, Mass. 4to.—*From the College.*

Extracts of Papers by Isaac Lea, LL.D., from the Proceedings of the Academy of Natural Sciences of Philadelphia. 8vo.—*From the Author.*

Catalogue of the Library of the Royal United Service Institution. London, 1865. 8vo.—*From the Institution.*

Astronomical and Meteorological Observations made at the Radcliffe Observatory, Oxford, in the year 1863. Vol. XXIII. Oxford, 1866. 8vo.—*From the Observatory.*

Geological Survey of Canada; Report of Progress from its commencement till 1863, with Atlas. Montreal, 1865. 8vo.—*From the Survey.*

Memoirs of the Geological Survey of India. Vol. III. Nos. 6–9; Vol. IV. Part 3. 4to. Vol. V. Part 1. 8vo.—*From the Survey.*

Annual Report of the Geological Survey of India for 1864–65. Calcutta. 8vo.—*From the Survey.*

Catalogue of the Organic Remains belonging to the Echinodermata in the Museum of the Geological Survey of India. Calcutta, 1865. 8vo.—*From the Survey.*

Journal of the Scottish Meteorological Society. Nos. 10–12. Edinburgh, 1866. Large 8vo.—*From the Society.*

Second Report upon the River Waimakariri, and the lower plains of Canterbury, New Zealand. By W. T. Doyne, C.E. Christchurch, 1865 Fol.—*From the Author.*

Documents of the United States Sanitary Commission. Vols. I. and II. New York, 1866. 8vo.—*From the Commission.*

United States Sanitary Commission Bulletin for 1863–65. New York, 1866. 8vo.—*From the Commission.*

Ages of United States Volunteer Soldiery. New York, 1866. 8vo.—*From the United States Sanitary Commission.*

Annual Report of the Board of Regents of the Smithsonian Institution. 1864. Washington, 1865. 8vo.—*From the Institution.*

Annual Report of the Trustees of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass.; together with the Report of the Directors. 1864–65. Boston, 1866. 8vo.—*From the Trustees.*

Bulletin of the Museum of Comparative Zoology, Cambridge, Mass., U.S. 8vo.—*From the Trustees.*

Proceedings of the Boston Society of Natural History for 1865-66. 8vo.—*From the Society.*

Condition and Doings of the Boston Society of Natural History. 1865. 8vo.—*From the Society.*

Proceedings of the American Academy of Arts and Sciences. Vol. VII. Boston, 1866. 8vo.—*From the Academy.*

Astronomical and Meteorological Observations, made at the United States Naval Observatory during the year 1863. Washington, 1865. 4to.—*From the Observatory.*

Reports on the Extent and Nature of the Materials available for the preparation of a Medical and Surgical History of the Rebellion. Philadelphia, 1865. 4to.—*From the United States Government.*

Memoirs of the Royal Astronomical Society. Vol. XXXIV. London, 1866. 4to.—*From the Society.*

Transactions of the Royal Irish Academy. Vol. XXIV. Antiquities, Parts 5–7; Polite Literature, Part 3; Science, Part 5. Dublin, 1866. 4to.—*From the Academy.*

Astronomical, Magnetical, and Meteorological Observations, made at the Royal Observatory, Greenwich, in the year 1864. London, 1866. 4to.—*From the Observatory.*

Verification and Extension of La Caille's Arc of Meridian at the Cape of Good Hope. By Sir Thomas Maclear. Vols I. and II. 4to.—*From the Royal Observatory, Greenwich.*

Report of the Professor of Astronomy in the University of Glasgow. 1866. 8vo.—*From the University.*

Annual Report for 1864–65 of the Leeds Philosophical and Literary Society. 8vo.—*From the Society.*

Catalogue of the Library of the Leeds Philosophical and Literary Society. 8vo.—*From the Society.*

Report of the Proceedings of the Geological and Polytechnic Society of the West-Riding of Yorkshire, 1864–65. Leeds, 1866. 8vo.—*From the Society.*

Proceedings of the London Mathematical Society, Nos. 1–7. 8vo. *From the Society.*

Nugæ Mathematicæ, by J. J. Sylvester, LL.D. 1866. 8vo.—*From the Author.*

Results of a series of Experiments on the Strength of Colonial Woods. By James Melville Balfour, C.E. 1865. 8vo.—*From the Author.*

On the Affinities of Platysonus and allied Genera. By John Young, M.D. 1866. 8vo.—*From the Author.*

Science and Christian Thought. By John Duns, D.D. London, 1866. 8vo.—*From the Author.*

Quarterly Return of Births, Deaths, and Marriages registered in the Divisions, Counties, and Districts of Scotland, Nos. 45–47. Edinburgh, 1866. 8vo.—*From the Registrar-General.*

Monthly Returns of Births, Deaths, and Marriages registered in the Eight Principal Towns of Scotland. March to November, 1866. Edinburgh, 1866. 8vo.—*From the Registrar-General.*

Annual of the National Academy of Sciences for 1865. Cambridge, U.S. 8vo.—*From the Academy.*

The St Andrews University Calendar for 1866–67. Edinburgh, 1866. 8vo.—*From the University.*

Modern Theory of Chemical Types, &c. By Charles M. Wetherill, Ph.D., M.D. 8vo.—*From the Author.*

A Word on the Origin of Life. No. 4. By James D. Dana. 8vo. —*From the Author.*

Supplementary Report on the Mortality Experience of the Scottish Equitable Assurance Society. By William Robertson, M.D. Edinburgh. 8vo.—*From the Author.*

Geology and Modern Thought, an Address to the Edinburgh Geological Society. By David Page, F.R.S.E. 8vo.—*From the Society.*

Epidemic Epizootic Fever Cattle Plague. By Draper Mackinder, M.D. London, 1866. 8vo.—*From the Author.*

On Finding the most Economical Rates of Expansion in Steam Engines. By W. J. Macquorn Rankine, LL.D. 4to.—*From the Author.*

The Distribution and Migrations of North American Birds. By Spencer F. Baird. 8vo.—*From the Author.*

Areas of the Drainage of Scottish Rivers and their Principal Tributaries. MS. Plans.—*From James Leslie, C.E.*

Catalogue of the Specimens of Meteoric Stones and Irons in the Museum of the Geological Survey, Calcutta. By T. Oldham. 8vo.—*From the Author.*

On a Balanced Rudder for Screw Steamers. By W. J. Macquorn Rankine, LL.D. 4to.—*From the Author.*

Memorie della Reale Accademia della Scienze di Torina. Serie II. Tomo XXI. Torino, 1865. 4to.—*From the Academy.*

Novorum Actorum Academiæ Cæsareæ Leopoldino-Carolinæ Germanicæ Naturæ Curiosorum. Vol. XXXII. Part 1. Dresdæ, 1865. 4to.—*From the Academy.*

Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin, 1864. 4to.—*From the Academy.*

Monatsberichte der Königlichen Preuss. Akademie der Wissenschaften zu Berlin, 1865. Jan.–Juli, 1866. 8vo.—*From the Academy.*

Nieuwe Verhandelingen van het Bataafsch Genootschap der proefondervindelijke Wijsbegeerte te Rotterdam. Deel XII. Stuk 2, 3. Rotterdam, 1865. 4to.—*From the Batavian Society of Natural Philosophy.*

Memorie dell' Accademia delle Scienze dell' Istitute di Bologna. Serie II. Tomo IV. Fasc. 2–4. Tomo V. Fasc. 1, 2. Bologna, 1865–66. 4to.—*From the Academy.*

Rendiconto delle Sessioni dell' Accademia delle Scienze dell' Istituto di Bologna, 1864–65. 8vo.—*From the Academy.*

Recueil des Voyages et des Mémoires publié par la Société de Géographie. Tome VII. Part 2. Paris, 1864. 4to.—*From the Society.*

Mémoires de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. Tome XXXV. Bruxelles, 1865. 4to. —*From the Academy.*

Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. 2nd Serie. Tome XX., XXI., XXII. Nos. 6–11. Bruxelles, 1865. *From the Academy.*

Mémoires Couronnés et autres Mémoires publiés par l'Académie Royale de Belgique. Tome XVIII. Bruxelles, 1866. 4to.—*From the Academy.*

Abhandlungen herausgegeben von der Senckenbergischen naturforschenden Gesellschaft. Band V. pt. 3, 4. Frankfort, 1865. 4to.—*From the Society.*

Amtlicher Bericht über die neun und dreissigste Versammlung Deutscher Naturforscher und Ärzte in Giessen im Sept. 1864, herausgegeben von den Geschäftsführern Wernher und Leuckart. Giessen, 1865. 4to.—*From the Authors.*

Mémoires de l'Académie Impériale des Sciences de St Pétersbourg. VII^{e}. Serie. Tome IX. X., N°. 1–2. St Petersburg, 1865–66. 4to.—*From the Academy.*

Bulletin de l'Académie Impériale des Sciences de St Pétersbourg Tome IX. St Petersburg. 4to.—*From the Academy.*

Neue Denkschriften der Allgemeinen Schweizerischen Gesellschaft für die gesammten Naturwissenschaften. Nouveaux Mémoires de la Société Helvétique des Sciences Naturelles. Band XXI. Zürich, 1865. 4to.—*From the Society.*

Geschichte der Schweizerischen Naturforschenden Gesellschaft zur Erinnerung an den Stiflungstag den 6 Octob. 1815. Zurich, 1865. 4to.—*From the Society.*

Verhandlungen der Kaiserlich-Koniglichen Zoologisch-Botanischen Gesellschaft in Wien. Band. XV. Wien, 1865. 8vo.—*From the Society.*

Bulletin de la Société de Géographie. Paris. Tome IX. Avril-Nov. 1866. 8vo.—*From the Society.*

Schriften der Universität zu Kiel aus dem Jahre 1665. Band XII. Kiel, 1866. 4to.—*From the University.*

Atti dell' Imp. Reg. Istituto Veneto di Scienze, Lettere ed Arti dal Novembre 1864 all' Ottobre 1865. Tomo X. F. 11, pt. 1–4. Venezia. 8vo.—*From the Institute.*

Memorie del Reale Istituto Lombardo di Scienze e Lettere—Classe di Lettere e Scienze Morali e Politiche, Vol. X. 1 della serie III. Fasc. 2. Matematiche e Naturali, Vol. X., 1 della serie III. Fasc. 2.—4to. Rendiconti Matematiche e Naturali, Vol. II. Fasc. 3–8. Moralie Politiche, Vol. II. Fasc. 3–7. 8vo.—Milano, 1865.—*From the Institute.*

Bulletin de la Société Vaudoise des Sciences Naturelles. Vol. IX. No. 54. Lausanne, 1866. 8vo.—*From the Society.*

Annales des Mines, Paris. Tome VIII., Liv. 5–6.; Tome IX., Liv. 1–2. 8vo.—*From the Ecole des Mines, Paris.*

Sciences Mathématiques et Physiques chez les Belges au commencement du XIX^{e}. siécle. Par Ad. Quetelet. Bruxelles, 1866. 8vo.—*From the Author.*

Almanaque Nautico para 1867, Calculado de orden de S. M. en el Observatorio de Marina de la Ciudad de S. Fernando. Cadiz, 1865. 8vo.—*From the Observatory.*

Bulletin de la Société Polymathique au Morbihan 1866. Vannes, 1866. 8vo.—*From the Society.*

Memoria sobse el Eclipse de Sol de 18 de Julio de 1860. Par D. Francisco de Paula Marquez. Madrid, 1861. 8vo.—*From the Author.*

Astronomische Beobachtungen auf der Königlichen Universitats-Sternwart zu Königsberg. Von Dr Eduard Luther. Königsberg, 1862. Fol.—*From the Author.*

Jahrbuch der Kaiserlich-Königlichen Geologischen Reichsanstalt. Band XV. N°. 4. Band XVI. N°. 1. Wien, 8vo.—*From the Archivar of the Reichsanstalt.*

Denkschriften der Kaiserlichen Akademie der Wissenschaften. Mathematisch-Naturwissenschaftliche Classe, Band XXIV., XXV. Philosophisch-Historische Classe, Band XIV. Wien, 1865. 8vo.—*From the Academy.*

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften. Phil. Hist. Classe, Band XLIX., L., LI. Heft 1–3.; B. LII. H. 1–4. Math.-Nat. Cl., Band LI. Heft 3–5.; B. LII. H. 1–5. B. LIII. H. 1–5.; B. LIV. H. 1. Register Math. Nat. Cl., Band V. Phil. Hist. Cl., Band V. Almanach, 1865. Wien, 8vo.—*From the Academy.*

Sitzungsberichte der Königl. bayer. Akademie der Wissenschaften zu München, 1865, Heft 3, 4 ; 1866, Heft 1–3. München, 8vo.—*From the Academy.*

Mittheilungen der Naturforschenden Gesellschaft in Bern, aus dem Jahre, 1865. Nr. 580–602. Bern, 1866. 8vo.—*From the Society.*

Übersicht der Thütigkeit der Nicolai-Hauptsternwarte wahrend der ersten 25 Jahres ihres Bestehens, zusammengestellt von Otto Struve. St Petersburg, 1865. 8vo.—*From the Author.*

Jahresbericht am 19 Mai 1865, den Comité der Nicolai-Hauptsternwarte. Von dem älteren astronomen W. Döllen. St Petersburg, 1865. 8vo.—*From the Author.*

De Sarcine (Sarcina ventriculi, Goodsir) onderzoek naar de plantaardige natuur, dem ligchaamsbrouw en de ontwikkelingswetten van dit organisme. Door Dr W. F. R. Suringar. Leeuwarden, 1865. 4to.—*From the Author.*

Jahresbericht über die Forstchritte der Chemie und Verwandter. Theile Anderer Wissenschaften von C. Bohn, Th. Engelbach, und H. Will; fur 1865. Heft 1. Giessen, 1866. 8vo.—*From the Editors.*

Libro Duodecimo della Politica del Commendatore Salvatore Fenicia da Ruvo. Napoli, 1866. 8vo.—*From the Author.*

Annuaire de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique, 1866. Cinquantième Anniversaire de la Reconstitution de l'Académie, 1816–1866. Bruxelles, 1866. 8vo.—*From the Academy.*

Annuaire de l'Observatoire Royale de Bruxelles, par A. Quetelet. 1866. 12mo.—*From the Observatory.*

Archives Neérlandaises des Sciences Exactes et Naturelles, publiées par la Société Hollandaise des Sciences a Harlem. Tome I. Liv. 1–4. La Haye, 1866. 8vo.—*From the Society.*

Atti della R. Accademia delle Scienze di Torino. Vol. I. Disp. 1, 2. Torino, 1866. 8vo.—*From the Academy.*

Simplicii Commentarius in IV. Libros Aristotelis de Cælo, ex recensione Sim, Karstenii, mandato Regiæ Academiæ disciplinarum. Nederlandicæ editus, 1865. 4to.—*From the Editor.*

Verslagen en Mededeelingen der Koninklijke Akademie van Wetenschappen Afdeeling Letterkunde, negende Deel-Natuurkunde, Tweede Reeks, Eerste Deel. Amsterdam, 1866. 8vo. —*From the Academy.*

Processen-verbaal van de Gewone vergaderingen der Koninklijke Akademie van Wetenschappen, Afdeeling Natuurkunde. 8vo. —*From the Academy.*

Catalogus van de Boekerij der Koninklijke Akademie van Wetenschappen gevestigd ze Amsterdam. Tweeden Deels, Eerste Stuk. Amsterdam, 1866. 8vo.—*From the Academy.*

Jaarboek van de Koninklijke Akademie van Wetenschappen, gevestigd ze Amsterdam. Voor, 1865.—*From the Academy.*

Traité des Projections des Cartes Géographiques, représentation Plane de la Sphère et du Sphéroïde, par A. Germain. Paris, 8vo.—*From the Author.*

The following Publications have been presented by the Depôt de la Marine, Paris:—

Annales Hydrographiques, 2e, 3e, Trimestre, Paris, 1865. 8vo.
Pilote du Golfe Saint-Laurent, 3e Partie. Paris, 1865. 8vo.
Instructions Nautiques sur La Côte Est de la Malaisie. Paris, 1865. 8vo.
Supplément aux Instructions sur la Mer de Chine, 2e partie. Paris, 1865. 8vo.
Supplément au Routier de l'Australie. Paris, 1865. 8vo.
Routier de l'Ile Aurigny. Paris, 1865. 8vo.
Renseignements sur quelques Mouillages de la Côte d'Islande et de Norvege. Paris, 1865. 8vo.
Recherches sur les Chronomètres et les Instruments Nautiques VIII.e cahier. Paris, 1865. 8vo.
Le Pilote de la Nouvelle Zélande, 1re, 2e partie. Paris, 1865. Fol.
Pilote de la Mer Noire. Paris, 1865. Fol.

Recherches sur les Climats de l'Epoque Actuelle, et des Epoques Anciennes particulièrement, au point de vue des Phénomènes Glaciaires de la Période Diluvienne. Par M. Sartorius de Waltershausen. 8vo.—*From the Author.*
Actes de la Société Helvétique des Sciences Naturelles, réunie a Genève. 1865. 8vo.—*From the Society.*
Biographie Nationale publiée par l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. 1re partie. Bruxelles, 1866. 8vo.—*From the Society.*
Observations des Phénomènes périodiques pendant l'Année 1863. 4to.—*From the Brussels Academy.*
Sur les travaux d'ensemble de l'Académie Royale, et sur les rapports avec les Sociétés Savantes Étrangères, &c. Par M. Ad. Quetelet. 8vo.—*From the Brussels Academy.*
Les Vieux. Par M. Ad. Mathieu. Bruxelles, 1866. 8vo.—*From the Brussels Academy.*

Bericht über eine Sammelreise durch England, Schottland, Irland, und die Schweiz in den Sommermonaten des Jahres, 1865. 8vo.—*From the Nat. Hist. Soc. Vienna.*

Zoologische Miscellen, Nos. 4, 5, 6. 8vo.—*From the Nat. Hist. Soc. Vienna.*

Eine dringende Mahnungan Freande der Physik, Mechanik, und Astronomie. Von Lambert V. West. Wien, 1866. 8vo.—*From the Author.*

Dissertatio qua ad audiendam orationem pro loco in facultate rite obtinendo die VI. Octobris hora XI. in auditorio maximo habendam invitat Eduardus Luther, Ph.D. 4to.—*From the Author.*

La Sarcine de l'Estomac. Par M. W. F. R. Suringer. 8vo.—*From the Author.*

Bollettino Meteorologico dell' Oservatorio Astronomico dell' Universita di Torino, 1866. 4to.—*From the University.*

Transactions of the Geological Society of Glasgow. Vol. II. Part 2. 8vo.—*From the Society.*

Transactions of the Historic Society of Lancashire and Cheshire. New Series. Vol. V. Liverpool, 1865. 8vo.—*From the Society.*

Sketch of the Geology of Fife and the Lothians. By Charles Maclaren, F.R.S.E. Second Edition. Edinburgh, 1866. 8vo. —*From Mrs Maclaren.*

Useful Information for Engineers. Third Series. By William Fairbairn, C.E., LL.D. London, 1866. 8vo.—*From the Author.*

A Short Practical Grammar of the Tibetan Language, with special reference to the Spoken Dialects. By H. A. Jaeschke, Moravian Missionary. 1865.—*From Dr Cleghorn.*

The British Association, considered with reference to its History, Plan, and Results, and to the approaching Meeting at Dundee. Being an Address delivered at the opening of the Winter Session of the United College of St Salvator and St Leonard, in the University of St Andrews, on Thursday, November 1, 1866. By James D. Forbes, D.C.L., LL.D., F.R.S., F.G.S. 8vo.—*From the Author.*

Pinetum Britannicum. Parts 16–19. Folio.—*From Charles Lawson, Esq.*

Researches on Solar Physics. By Warren de la Rue, Esq., Balfour Stewart, Esq., Benjamin Loewy, Esq. London, 1866. 4to.—*From the Authors.*

Register zu den Bänden I.–XIV. der Denkschriften der Philosophisch-Historischen Classe der Kaiserlichen Akademie der Wissenschaften. N° 1. Wien, 1866. 4to.—*From the Royal Academy of Vienna.*

Almanach der Kaiserlichen Akademie der Wissenschaften, 1866. Wien. 8vo.—*From the Royal Academy of Vienna.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VI. 1866–67. No. 72.

Monday, 17th December 1866.

The Hon. LORD NEAVES, Vice-President, in the Chair.

The following Communications were read:—

I. Influence of Marriage on the Death-Rates of Men and Women in Scotland. By James Stark, M.D., &c.

The object of this paper was to show the different death-rates which prevailed among the married and unmarried men and women in Scotland.

It was pointed out that the mortality among the unmarried men was, at all ages, very much higher than among the married; that, in fact, the mortality of the unmarried was thrice as high at the age 20 to 25 years; and only slowly approximated to that of the married man at 85 years of age.

The difference in the death-rates of the married and unmarried men was rendered more striking by calculating their mean age at death, when it was found that from 20 years to the close of life the mean age at death of the married man was 59½ years, that of the unmarried man only 40 years—giving a difference in favour of the married man of 19½ years of life. When the mean age at death was calculated from the 25th year, it was found to give a mean age of $60\frac{2}{10}$ths years to the married, but only $47\frac{7}{10}$ths years to the unmarried, or a difference of 12½ years in favour of married life.

It was pointed out that this appeared to be a special provision of nature to protect the father of a family, so that he might provide for his offspring, and superintend their rearing; but that it was also capable of being so far explained by natural causes. The married man was in one sense a selected life; because the weak, the diseased, the intemperate, and the licentious, did not marry; so that all this unhealthy class of persons remained bachelors, and it was a known fact that their mortality was high.

It was then pointed out that summarising the tables—that is, comparing the total deaths at all ages with the total number living, led to a conclusion the very opposite of the truth, inasmuch as it seemed to prove that unmarried men had the healthiest lives. for it showed that in every 100,000 unmarried men only 1723 died annually, in a like number of married men 2338 died; and the natural conclusion from such a fact would be, that the death-rate of the unmarried was much lower than that of the married man.

It was explained how this paradoxical result was produced; and the English Registrar-General's tables of the mortality of men following different occupations was referred to, showing the fallacy of summarising the facts in these tables, at the same time pointing out how the true comparative healthiness of each occupation could be ascertained.

The death-rates of the married and unmarried women were then explained. It was shown that at each quinquennial age, from 15 to 30 years, the death-rates of the married women slightly exceeded that of the unmarried, being greatest at the junior age, but approximating and becoming identical at 30. From 30 to 40 years the mortality of the married women was slightly lower than that of the unmarried, but from 40 to 45 it was very slightly higher. From 45 years to the close of life, however, the death-rates of the married women were lower than those of the unmarried. The difference, however, was comparatively trifling at every age, as compared with the mortality of the married and unmarried men.

It was explained that these variations in the death-rates of the married and unmarried women were explicable. The higher mortality among the married under 30 years was caused by the additional dangers which attended the birth of the first child, and just in proportion to the number of mothers who bore their first child was the mortality at each quinquennial period of life. The slightly higher mortality, again, in the married between 40 and 45 years of age, at which time the change of life occurs among the women of Scotland, was attributed to their constitutions having been somewhat weakened by this childbearing, lactation, and the extra labours and fatigues attending the rearing of their children.

2. On the Physiological Action of the Calabar Bean (*Physostigma venenosum*, Balf.) By Thomas R. Fraser, M.D.

Although many able observers have, within the last few years, investigated the physiological action of the Calabar bean, a singular amount of discordance has characterised the results arrived at. An intention, in the first place, of adding proofs in confirmation of the views advanced in an investigation published in 1862, has induced the author to reconsider the subject. The result has been that the actions of this substance have been found to be much more complicated than was at first supposed, and this may be understood when it is stated that it acts on the spinal cord and on the motor nerves, and also on the sympathetic system, and that, in this manner, it directly influences nearly all the vital functions. It is not proposed, at this time, to examine what must be regarded as the most curious action of Physostigma, that, namely, on the pupil, which results from its topical application to the eyeball or to the mucous membranes which are anatomically connected with it. The theories of this action, though some of them have been advanced by men of such eminence as Donders, Graefe, Rosenthal and Bowman, are extremely unsatisfactory. It is hoped that a distinct conception of the general physiological effects will suggest a correct explanation of the myositic action, and there can be no doubt it will assist in arriving at this conclusion.

In the following experiments, an extract prepared by acting on the finely pulverised kernel with boiling alcohol has been used. This preparation contains a considerable proportion of fatty matter, which prevents its perfect solution in water; and, as the division into separate doses of a mere watery suspension would lead to many inaccuracies, it was found necessary to weigh the requisite quantity of extract, separately, for each experiment. This extract is hygroscopic, which further required that it should be dried and kept *in vacuo* over sulphuric acid. The greatest number of the experiments were made with the common frog (*Rana temporaria*), birds, and various mammals. It was found that fatal results were produced with the smallest quantity on birds, and that the largest doses, in proportion to weight, were required by amphibia. A dose of one-sixteenth of a grain proved rapidly fatal to a pigeon, whereas three grains have been recovered from by a frog—a quantity sufficient to produce death in a dog of average size.

The immediate causes of death in birds and mammalia were found to be syncope and asphyxia, or a combination of the two; and a marked connection was observed between the dose and its rate of absorption, and the preponderance of one or other of these effects. In frogs, the cardiac and respiratory functions were rapidly affected, but complete destruction of all the vital functions of the animal never occurred for many hours. This slowness in the progress of the effects from one system to another, constituted the peculiar advantage of employing this animal in the research, as, thereby, it was possible distinctly to determine the sequence of the phenomena. In animals of a higher type, the implication of one system so rapidly influences the others, that symptoms follow each other with such speed as to increase very greatly the difficulties of a clear apprehension.

The following are the conclusions of the investigation.

A. *Action through the Blood.*

1. Physostigma proved fatal to every animal hitherto examined, with the single exception of the Esĕrĕ moth. Death is most rapidly caused in birds and mammals by the injection of the poison into the circulation, or when it is brought in contact with a wounded surface. It follows nearly as quickly when Calabar bean is introduced into a serous cavity, and much less rapidly when introduced by the mucous membrane of the digestive system. In rabbits, death has been caused by its application to the Schneiderian, auditory, and conjunctival mucous membranes. The skin of frogs resists its effects for a long time, but, if applied for a sufficient period and with proper precautions, distinct evidence of its

absorption may be obtained, though death has never been caused by such application.

2. The contact of the extract of Calabar bean with the gastric juice of a dog for twenty-four hours, at a temperature a little above 95° F., did not, in the slightest degree, modify its energy.

3. A large dose, injected into the abdominal cavity of a frog, affects nearly simultaneously the heart and spinal cord,* and very rapidly destroys the vitality of both organs. With such a dose, the motor nerves are unaffected, and retain their conductivity for at least thirty hours. Evidence of the activity of the afferent nerves may be obtained so long as the retained vitality of the spinal cord permits of its diastaltic function being examined.

4. An average dose, in the first place, impairs the function of the spinal cord and diminishes the rate of the cardiac contractions and of the respiratory movements, and, soon after, these latter cease. In periods varying from one and a-half to four hours afterwards, *the motor nerves are paralysed*, this paralysis first implicating the endorgans of these nerves, and afterwards the nerve trunks. From this it must not be inferred that the nerve is paralysed by a centripetal progression of the poison, the only fact which was demonstrated being that a direct ratio existed between subdivision of nerve substance and facility of contact of poison, on the one hand, and, on the other, rapidity of paralysing effect. Indeed, division of the nerve trunk, previous to the administration of Calabar bean, delayed the paralysis of its endorgans. The afferent nerves retain their activity so long, at least, as the functions of the spinal cord are not lost, and this generally happens about the same time as the motor paralysis.

5. When a small but still fatal dose of Calabar bean is administered to a frog, the effects are the same as those in the previous conclusion, until they arrive at the stage of paralysis of the motor nerves, and, after this, an interval of several hours may elapse before the functions of the spinal cord are completely suspended. During this interval the *tactile* sensibility of the afferent nerves is increased; so that, if the ischiadic artery and vein of one limb were tied before the exhibition of the poison, an ordinary excitant, such

* The effects on the spinal cord were determined by frequent measurements of the reflex activity by means of the Mètronome.

as sulphuric acid, will show everywhere a marked diminution, as measured by the metronome, in the diastaltic activity; while a slight touch of the skin in the poisoned region, which before the administration of the poison caused no effect, will now produce faint twitches of the limb whose vessels are tied.

6. *A frog may have its cardiac contractions reduced from seventy to eight or ten per minute, its respiratory movements completely stopped, and the endorgans of its motor nerves paralysed, by a still smaller dose, and afterwards completely recover.* This has occurred when two grains were injected into the abdominal cavity of a frog weighing seven hundred and thirty grains.

7. In frogs, the voluntary muscles are unaffected by the poison, and may continue to respond to galvanic stimulation during three or four days after its administration. The contrast and independence in the effects of Calabar bean on the motor nerves and on the muscles may be well shown by ligaturing the ischiadic vessels of one limb before injecting the poison. If, when strong stimulation causes no reflex movement, the two gastrocnemii muscles with their attached nerves are so placed that an interrupted current, from one Daniell's cell and Du Bois Reymond's induction apparatus, may be transmitted simultaneously, either through both muscles, or both nerve-trunks, it will be found, in the case of the muscles, that when the secondary coil is slowly advanced, contractions will occur with the same current in both muscles, or with a weaker current in the case of the poisoned than of the non-poisoned muscle, this varying with the length of time which has elapsed since the limb was deprived of blood; when the current is transmitted through both nerves, contractions will be simultaneously produced, or with a weaker current in the non-poisoned, or contractions will occur in the non-poisoned muscle only, this varying with the length of time which has elapsed since the exhibition of the poison.

8. In mammals and in birds the voluntary muscles are affected in a very remarkable manner. At an early stage of the poisoning faint twitches occur, which gradually extend over the body, and, at the same time, increase in vigour, so as to interfere with the respiratory movements. Shortly before death, they again become mere successive twitches, often requiring the hand to be placed

over the part in order to distinguish their existence. After death, if a muscular surface be exposed, these twitches will be observed to rarely involve the whole of one muscle, but at different times different muscular fasciculi; and they may persist for more than thirty minutes after death. They are caused by a direct effect of Calabar bean on the muscular substance. This is shown by their occurring in muscles after paralysis of the motor nerves, by their persisting in a muscle cut out of the body, and by their non-occurrence in parts which have been separated by ligature from the circulation.

9. The heart's action is rapidly slowered and then stopped, in birds and mammals, by a large dose. In dogs it may diminish to one-half in three minutes and cease in ten. A large dose injected into the abdominal cavity of a frog causes rapid and complete paralysis. A smaller dose causes either a gradual cessation and then a renewal at a diminished rate, or a gradual slowering from sixty or seventy to four or six beats per minute, followed by a gradual acceleration to a diminished rate varying from eight to twenty per minute. At this stage, and for many hours afterwards, the only signs of vitality are the diminished cardiac action and the power of the voluntary muscles to respond to galvanic and other stimulation. In the frog, where alone this diminution without stoppage, succeeded by partial acceleration, has been observed, the heart may continue so to contract for three, and for even five days, provided the temperature of the apartment be as low as 50° F. After stoppage, galvanic stimulation may cause a renewal of its rythmical contraction; but this is usually lost, and unrythmical and partial contractions can be only excited. The heart ceases to contract in diastole with all its chambers full.

10. The pneumogastric nerves retain their inhibitory power on the heart during the whole time from the diminution to the partial recovery of its action. Soon after this, however, they are paralysed; and this occurs at nearly the same time as the affection of the motor nerves.

11. Division of the pneumogastric nerves, or destruction of the medulla oblongata and medulla spinalis, does not protect the heart from the action of Physostigma.

12. The lymphatic hearts of frogs poisoned by Calabar bean soon cease to contract.

13. A large dose paralyses the cervical sympathetic nerves in rabbits before the death of the animal. A smaller fatal dose diminishes, without destroying, their activity.

14. Before the stoppage of the heart, proofs may be obtained of the vitality of its sympathetic ganglia, and, as striped muscle is not affected by Calabar bean conveyed by the blood, we are obliged to infer that the cardiac sympathetic system may be destroyed by a large dose, and its activity lessened by a smaller one.

15. The animal temperature, both external and internal, has been invariably observed to rise in rabbits and dogs, but only slightly.

16. The condition of the capillary circulation was examined in the web of the frog. Soon after the exhibition of the poison, the smaller arteries and veins contracted slightly; after a short interval, this contraction was succeeded by a rapid and permanent dilatation, in which the calibre of the vessels was considerably above their maximum previous to the poisoning. This dilatation of the capillaries appears to occur universally over the body, as is shown by a peculiar blue coloration of the voluntary muscles and of the heart, a similar coloration of the serous and fibro-serous tissues, and a congestion of the blood-vessels in the conjunctiva and iris. These changes occur, also, in a less marked manner, in birds and mammals.

17. The general results of experiments in which the arterial and venous tensions were examined were, that almost immediately after the administration of Calabar bean the arterial tension rose slightly, attained its maximum when the number of cardiac contractions had diminished to at least one-half, and then rapidly fell; and that the venous tension rose less quickly, attained its maximum when the arterial tension had diminished considerably, and, in its turn, fell, though more gradually than that of the arterial system. The number of the cardiac contractions, when the venous tension had attained its maximum, was about one-third of the average before the poisoning; the respirations were rather less frequent than before, and the temperature had risen a few tenths of a degree.

18. Physostigma causes extreme diffusion in the pigment cells of the frog's skin, and so a very marked change occurs in the colour of the animal during the progress of the symptoms.

19. The peristaltic action of the intestines is usually destroyed at death; it may, however, continue a short time afterwards.

20. The pupil contracts in all cases of rapid poisoning in mammalia and in birds. When the dose is small, in place of contraction, dilatation is sometimes observed; but this is extremely rare. In frogs the pupil has been generally observed to contract, but no connection could be determined between the dose and this symptom.

21. Calabar bean acts as an excitant of the secretory system, increasing the action of the alimentary mucous, of the lachrymal, and of the salivary glands.

22. The symptoms of poisoning are not materially altered, in the frog, by the removal of the brain or by division of the cervical portion of the spinal cord.

23. Artificial respiration does not prevent death in mammals after the exhibition of a poisonous dose. This is a necessary result of the effects of Physostigma on both the cerebro-spinal and sympathetic systems.

24. Congestion of internal organs occasionally occurs, but this is by no means an invariable consequence of a fatal dose.

25. The blood is dark after death, but becomes arterialised on exposure to the air; it usually clots loosely and imperfectly; and, when examined with the spectroscope, the bands of scarlet crurine are found unchanged. A microscopic examination demonstrates, in the rabbit and dog, an invariable change in the coloured corpuscles, which have their outlines distinctly crenated. This change is not observed in the blood of birds or amphibia. The white corpuscles remain unaltered.

B. *Topical Effects.*

1. When applied to the surface of the brain of a frog, no effect was produced; but when the poison was brought in contact with the spinal cord, a few twitches occurred in the extremities, followed by paralysis of the portion of cord acted on.

2. When Calabar bean is applied to a mixed nerve, in a concentrated form and with proper precautions to prevent absorption, the afferent nerve-fibres are first paralysed, and afterwards the efferent. This is readily demonstrated by acting on the sciatic nerve of the frog and, after some time, administering a small dose of strychnia.

3. Topical application destroys the contractility of striped and of unstriped muscular fibre. The heart's action is stopped by repeated application to the pericardium or to its external surface. If a sufficient quantity be injected into one of its chambers, paralysis nearly immediately follows.

4. The effects of the application of Calabar bean to the eyeball are a somewhat painful sensation of tension in the ciliary region, contraction of the pupil, with immobility in extreme contraction, myopia and astigmatism, congestion of the conjunctival vessels, and twitches of the orbicularis palpebrarum muscle.

3. On some Phenomena of Indistinct Vision. By Edward Sang, Esq.

If an exceedingly small luminous object be placed in front of the eye, but out of the range of distinct vision, it appears to be a roundish bright disc traversed by various markings, and most of these markings are found to be permanent for the same eye, but different for the two eyes. The subject of the present paper is a modification of this appearance.

Instead of a luminous point let us take a very narrow luminous line, such as a small slit in the window-shutter, or the reflection of the light of a fire from a polished metallic rod, and let us put this out of focus by means of a pair of convex spectacles. On regarding the luminous line, without any attempt to adjust the eye to distinct vision, we perceive a long luminous band, the breadth of which varies with the distance of the line. When the luminous line is brought into focus, the apparent breadth becomes zero; it increases as the line is moved beyond or within the distance for distinct vision.

If, having removed the shining line to a considerable distance, so as to obtain a pretty broad band, we take a thin ring, such as that used for keys, in the hand, and pass it close to the eye so as to intercept part of the light, we shall perceive that the bright band is traversed for its whole length by a dark line, and that this line moves across it in the direction of the motion of the ring.

On removing the ring to a little distance, the black line produced by it is seen to be curved in the same direction with the curvature of the ring; and an attentive examination shows that its curvature is much less than that belonging to the projection of the ring to the same distance. As the curved wire is farther removed, the appearance of the dark line becomes that of an elliptic arc taken from near the end of its shorter axis; and when the ring is taken to the distance proper for distinct vision, the dark band becomes circular, becomes, in fact, the true image of the ring itself.

If we continue to remove the ring to a greater distance from the eye, the curvature of the dark band is increased, and presents the appearance of a portion of an ellipse taken from near the end of the major axis. Its motion across the luminous band is evidently greater than that due to the movement of the ring.

Let us now bring the luminous slit within the distance for distinct vision, or, better, let us remove the optical focus to beyond the luminous slit, by wearing a pair of deep concave glasses, and we shall find that the motion of the dark line is opposite in direction to that of the ring, and that the curvature is also turned the other way.

It was this reversion which drew my attention to the phenomena; and, not recollecting of having seen it noticed, I thought it worth while to bring it before the Royal Society.

4. Note on Determinants of the Third Order. By Professor Tait.

Hamilton long ago showed that if we have

$$\begin{aligned} \alpha &= ix + jy + kz, \\ \beta &= ix_1 + jy_1 + kz_1, \\ \gamma &= ix_2 + jy_2 + kz_2, \end{aligned}$$

then

$$S \, . \, \alpha\beta\gamma = - \begin{vmatrix} x & y & z \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}$$

This opens up an exceedingly simple quaternion path to the proof of various properties of determinants of the third order. Several of those I subjoin have long been known, some, however, appear to be new, and our methods indicate how to obtain an unlimited number of new ones; but *all* are proved much more simply than by the usual processes.

(1.) We have at once, by quaternions,

$$S \,.\, (\alpha+\beta)(\beta+\gamma)(\gamma+\alpha) = 2\, S \,.\, \alpha\beta\gamma \,.$$

In its algebraic form this is

$$\begin{vmatrix} x+x_1 & y+y_1 & z+z_1 \\ x_1+x_2 & y_1+y_2 & z_1+z_2 \\ x_2+x & y_2+y & z_2+z \end{vmatrix} = 2 \begin{vmatrix} x & y & z \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}$$

(2.) Again

$$S \,.\, V\alpha\beta\, V\beta\gamma\, V\gamma\alpha = -(S \,.\, \alpha\beta\gamma)^2,$$

or

$$\begin{vmatrix} yz_1-zy_1 & zx_1-xz_1 & xy_1-yx_1 \\ y_1z_2-z_1y_2 & z_1x_2-x_1z_2 & x_1y_2-y_1x_2 \\ y_2z-z_2y & z_2x-x_2z & x_2y-y_2x \end{vmatrix} = \begin{vmatrix} x & y & z \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}^2 .$$

This is the well-known property of the *Reciprocal*, or determinant formed from the minors of the original determinant.

(3.) Since we have

$$V\alpha\beta = i \begin{vmatrix} y & z \\ y_1 & z_1 \end{vmatrix} + j \begin{vmatrix} z & x \\ z_1 & x_1 \end{vmatrix} + k \begin{vmatrix} x & y \\ x_1 & y_1 \end{vmatrix}$$

(whose application to (2) is obvious), we may also write

$$V\alpha\beta = \begin{vmatrix} i & & k \\ x & y & z \\ x_1 & y_1 & z_1 \end{vmatrix} .$$

(4.) But, if we put

$$\Delta = \begin{vmatrix} x & y & z \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}$$

we have

$$V\alpha\beta = \left(i\frac{d}{dx_2} + j\frac{d}{dy_2} + k\frac{d}{dz_2} \right)\Delta$$

which is easily seen to agree with (3). From this many well-known theorems may be deduced at once.

(5.) By (1) and (2) we have

$$S \,.\, (V\alpha\beta + V\beta\gamma)\ (V\beta\gamma + V\gamma\alpha)\ (V\gamma\alpha + V\alpha\beta)$$
$$= 2\, S \,.\, V\alpha\beta\, V\beta\gamma\, V\gamma\alpha = -2\,(S \,.\, \alpha\beta\gamma)^2.$$

(6.) Similarly, if we put

$$\delta = V(V\alpha\beta + V\beta\gamma\ \overline{V\beta\gamma + V\gamma\alpha}),\ \epsilon = V(\overline{V\beta\gamma + V\gamma\alpha}\ \overline{V\gamma\alpha + V\alpha\beta}),$$
$$\zeta = V\,(\overline{V\gamma\alpha + V\alpha\beta}\ \overline{V\alpha\beta + V\beta\gamma}),$$

we have

$$S \,.\, \delta\epsilon\zeta = -\ (S \,.\, \overline{V\alpha\beta + V\beta\gamma}\ \overline{V\beta\gamma + V\gamma\alpha}\ \overline{V\gamma\alpha + V\alpha\beta})^2$$
$$= -\ 4\,(S \,.\, \alpha\beta\gamma)^4.$$

Such theorems may be multiplied indefinitely; but we have already reached a stage in which the algebraic form is very unwieldy.

(7.) The common rule for the multiplication of two determinants is, of course, easily found by this process in the form

$$S \,.\, \alpha\beta\gamma\ S \,.\, \alpha_1\beta_1\gamma_1 = \begin{vmatrix} S\alpha\alpha_1 & S\alpha\beta_1 & S\alpha\gamma_1 \\ S\beta\alpha_1 & S\beta\beta_1 & S\beta\gamma_1 \\ S\gamma\alpha_1 & S\gamma\beta_1 & S\gamma\gamma_1 \end{vmatrix}.$$

The following Gentlemen were admitted Fellows of the Society:—

T. B. Johnston, Esq.
George F. Barbour, Esq., of Bonskeid.
David Davidson, Esq.
Peter Waddell, Esq.
George Harvey, Esq.
George Stirling Home Drummond, Esq. of Ardoch.
Professor Fuller, Aberdeen.

The following Donations to the Library were announced:—

Transactions of the Royal Scottish Society of Arts. Vol. VII. Part 2. Edinburgh, 1866. 8vo.—*From the Society.*

Thirty-Ninth Annual Report of the Royal Scottish Academy of Painting, Sculpture, and Architecture. Edinburgh, 1866. 8vo.—*From the Academy.*

Journal of the Statistical Society of London. Vol. XXIX. Part 4. London, 1866. 8vo.—*From the Society.*

The American Journal of Science and Arts. Second Series. No. 126. New Haven, 1866. 8vo.—*From the Editors.*

Conferences Agricoles faites au champ d'experiences de Vincennes dans la saison de 1864. Par M. Georges Ville. Première—Sixième Conférence. Paris, 1865–66. 8vo.—*From the Author.*

Jahresbericht über die Fortschritte der Chemie und verwandter Theile anderen Wissenschaften. Unter Mitwirkung von C. Bohn und Th. Engelbach herausgegeben von Heinrich Will; für 1865. Zweites Heft. Giessen 1866. 8vo.—*From the Editors.*

Monatsbericht der Königlichen Preussischen Akademie der Wissenschaften zu Berlin. August 1866. 8vo.—*From the Academy.*

Annales des Mines. 6ᵉ Série. Tome IX. 2ᵉ, 3ᵉ Livraison de 1866. Paris, 1866. 8vo.—*From the Commission of Mines.*

Résumé Météorologique de l'Année 1865, pour Genève et le Grand Saint-Bernard. Par E. Plantamour. Genève, 1866. 8vo.—*From the Author.*

Mémoires de la Société de Physique et d'Histoire Naturelle de Genève. Tome XVIII. 2 partie. 4to.—*From the Society.*

Expériences faites a Genève avec le pendule a Réversion. Par E. Plantamour. Genève, 1866. 4to.—*From the Author.*

Abhandlungen der Königlichen Gesellschaft der Wissenschaften zu Göttingen. XII. Band. Göttingen, 1866. 4to.—*From the Society.*

Almanaque Nautico para el año 1868, calculado de órden de S. M. en el observatorio de Marina de la Ciudad de S. Fernando. Cadiz, 1866. 8vo.—*From the Observatory.*

Monday, 7th January 1867.

THE HON. LORD NEAVES, Vice-President, in the Chair.

Dr HUGHES BENNETT, at the request of the Council, performed several experiments with a view of showing how, in modern times, physiology was successfully investigated by means of newly invented instruments. He noticed especially the subject of animal electricity, stating, that a specimen of the *Melapterurus beninensis* which had been forwarded to him from Old Calabar in Africa, had been preserved for eighteen months in one of the hot-houses of the Botanic Garden of this city. Unfortunately, it died only that afternoon. He regretted that the arrangements he had made for showing the electrical currents in muscles and nerves had failed, in consequence of the injury which had been inflicted on two of the delicate galvanometers, recently invented by Sir W. Thomson, in their transit from the University to the Society's Rooms. He proceeded, however, to show the influence of continued and interrupted streams of electricity, on muscles and nerves. The law of contraction in muscles, as determined by Pfluger, was clearly demonstrated, and the variations in the influence of the continued stream, according to its amount and direction, were fully exhibited. The phenomena of electrotonus and the excitability of nerves were next illustrated by experiments. He then explained and performed the experiment of Helmholz, demonstrating how the time which the nervous influence occupied in traversing a given distance of nerve might be determined with exactitude, and noticed the more recent experiments of Schelzke, and De Jaeger, as to the rapidity with which volition and sensation were produced. Lastly, he alluded to the action of recently invented Sphygmographs, Cardiographs, and the Kymographion, dwelling more particularly on the Sphygmosphone. This instrument enabled the time and rapidity of the pulse and movements of the heart to be accurately determined by the ear, in consequence of an arrangement which caused each pulsation to break a continued electrical circuit, that in its turn communicated a movement to levers which struck bells. The two halves of this Sphygmosphone might be removed to any distance, so that if connected with a telegraphic wire, the rhythm of the pulse of an individual in London might be made audible in Edinburgh.

Monday, 21st January 1867.

Dr CHRISTISON, Vice-President, in the Chair.

The following Communications were read:—

1. On the Colours of the Soap-Bubble. By Sir David Brewster, K.H., F.R.S.

The colours of the soap-bubble have been the subject of frequent observation since the time of Boyle, Hook, and Newton, and they have been invariably ascribed "not to any colour in the medium itself in which they are formed, or on whose surfaces they appear, but solely to its greater or less thickness." The author of this paper had been led to doubt the correctness of this opinion, and while repeating the beautiful experiments of Professor Plateau "On the equilibrium of liquid films," he was led to discover the true cause of these colours, whether they are observed on the soap-bubble or on plane, convex, and concave films stretched across the mouths of closed or open vessels.

The paper, which is illustrated with numerous coloured drawings, is divided into five parts.

1. On the phenomena of colour in a vertical plane film.

2. On the production of revolving systems of coloured rings on the soap film.

3. On the form and movements of the bands and rings on convex and concave films.

4. On the phenomena produced by different solutions.

5. On the origin and development of the colours on the soap-bubble.

In these sections the author has shown that the colouring matter of the soap-bubble is secreted from the soap solution when reduced to the state of a film;—that it rises to the highest point of the film in colourless portions, in the form of a tadpole, which pass into molecules of every possible order of colour, and then take their proper place in the coloured bands;—that these bands move over the surface of the film under the influence of gravity, and may be blown into fragments or into molecules of all colours, or even recombined with the film; that they may be blown into two systems of coloured rings, the one revolving from right to left, and the other from left to right; and that under the influence of the centrifugal force, these molecules are carried into their place in Newton's scale—those of the first orders going to the centre of the rings, and followed by those of higher orders that happen to be in the film when it is blown upon through a tube in the direction of a diameter.

"It is impossible," the author adds, "to convey in language an adequate idea of the molecular movements, and the brilliant chromatic phenomena exhibited on the soap films, and it is equally impossible for art to delineate them. The visible secretion of a colourless fluid from a film less than the twelve thousandth of an inch in thickness,—its separation into portions of every possible colour,—the quick passage of these portions into bands of the different orders in Newton's scale,—their ever varying forms and hues when the bands either break up spontaneously, or are forcibly broken up,—their conversion into revolving systems of coloured rings under the influence of a centrifugal force,—their various motions when the film is at rest, and protected from aerial currents,—their recombination into a colourless fluid when driven to the centre or margin of concave and convex films, and their reabsorption by the film by means of mechanical diffusion, are facts constituting a system of visible molecular actions, of which we have no example, and nothing even approaching to it in Physics."

2. On the *Musculus Sternalis*. By William Turner, M.B., Demonstrator of Anatomy.

In this paper the author described the results of his observations on the presence of the musculus sternalis in upwards of six hundred bodies dissected in the anatomical rooms of the University of Edinburgh. He had found it in nineteen individuals, *i.e.*, in about 3 per cent. of the bodies examined. It occurred nearly equally in the two sexes. It bore no relation to the general muscularity of the individual. In eleven subjects the muscle was single, in eight double, making together twenty-seven specimens of the muscle. The variations which it exhibited in its attachments, size, and shape, were then described. In no case were its fibres continuous with those of the rectus abdominis, or were tendinous intersections found in it, but it mostly arose either from the flattened tendon of the external oblique muscle of the abdomen, or from the cartilages of the lower true ribs, and in many instances it was continuous at its upper end with the sternal tendon of one or both sterno-mastoids, whilst in others it was inserted into the aponeurosis covering the pectoralis major. It was always superficial to the great pectoral muscle. Of the single specimens, four occurred on the right side, two on the left; whilst in the remaining five it arose on one side of the middle line, and was inserted either altogether or in part on the opposite side. It formed an excellent illustration of the truth of the general statement, that occasional and rudimentary structures are especially liable to variations in arrangement.

A sketch of the history of the muscle, from the first observation by Cabrolius in 1604, was then given, and the various opinions as to its morphology were discussed. In opposition to the view usually entertained by anatomists, the author contended that it was not an upward extension of the rectus abdominis, such as is so frequently seen in the mammalia, so that the name rectus sternalis, or sternalis brutorum, usually applied to it, is not appropriate. For it was not continuous with the rectus, and was placed superficial to the pectoralis, whilst the anterior end of the mammalian rectus is always continuous with its abdominal part, and, moreover, concealed by the pectoral muscle; and further, another muscle has occasionally been seen in man which, differing in its position from the sternalis, lying under cover of the great pectoral muscle next the ribs, is undoubtedly to be regarded as homologous with the anterior end of the mammalian rectus.

The sternalis muscle, from many of its relations, seems to be most closely allied to the *panniculus carnosus*, or great skin muscle of the quadruped, and may perhaps be regarded as an additional rudiment of that muscle, occasionally present in man, though it must be admitted that the human platysma (which is generally acknowledged to represent the panniculus) lies on a plane superficial to the fibres of the sternalis in those individuals in whom they coexist.

3. On Compensation Pendulums of two Pieces. By Edward Sang, Esq.

Three or four years ago, wishing to make some experiments on time-keepers, and desiring for that purpose to have a compensation pendulum fitted to my clock, I turned my attention to pendulums composed of two pieces, having different rates of expansion.

Such compensation pendulums are now pretty common; the idea which has led to their construction being this,—that if the pendulum rod be made of material having a very small rate of expansion, and if the bob or weight be of a substance much more expansive, then the upward expansion of the one may be made to compensate for the downward expansion of the other, and so the going of the clock may remain unaffected by changes of temperature.

On designing the arrangement, and on proceeding to make the calculation, according to the received expansions of deal and lead, I was much surprised to find the solution of the resulting equation to be impossible; and the surprise was the greater, because I had understood that of clocks constructed with such pendulums, the compensation had been verified by actual observations. There is not sufficient disparity between the expansibilities of deal and lead.

Fig. 1.

A very slight glance at the subject is sufficient to show, that if it be impossible to construct such a pendulum with a deal rod of great tenuity, and with an enclosing cylinder of lead also of inappreciable diameter, it must be impossible to make one with the parts having appreciable diameters; since the lateral expansion of the parts tends to augment the length of the corresponding simple pendulum.

Let us then suppose an exceedingly thin rod AB, of some slightly expansive substance, suspended by the end A, and having on the lower end B a small projection or seat, on which the end of a cylinder BC of heavy and expansive metal rests. Then neglecting the weight of the inner rod, and the radius of the cylinder, and putting AB $=b$, AC $=c$, and l for the length of the corresponding simple pendulum, we have

$$\frac{3}{2}l=\frac{b^2+bc+c^2}{b+c} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

Let now β be the rate of expansion of the supporting rod AB, γ that of the cylinder BC, we have for a change of temperature t.

$$b'=b(1+\beta t), \quad c'=c(1+\gamma t).$$

and therefore, in order that there be no change in the value of b, we must have

$$\frac{b^2+bc+c^2}{b+c}=\frac{3}{2}l=\frac{b'^2+b'c'+c'^2}{b'+c'}=\frac{(3b^2+3bc)\beta+(2c^2-bc-b^2)\gamma}{2b\beta-b\gamma+c\gamma}$$

whence the equation

$$\beta\,.\,b^3+(4\beta-2\gamma)b^2c+(\beta+\gamma)bc^2+\gamma c^3=0, \quad . \quad . \quad . \quad (2.)$$

by help of which the ratio of b to c may be found. Now, unless γ exceed the double of β, all the terms of this equation have the same sign, and therefore there can be no positive root; so that, on this account, there can be no such pendulum as we are thinking of, unless the one rate of expansion be more than double of the other. In order to discover the least possible disparity of expansion, let us put $b=cx$, $\gamma=n\beta$, and our equation becomes

$$x^3+(4-2n)x^2+(1+n)x+n=0, \quad . \quad . \quad . \quad . \quad (3.)$$

When our solution just begins to be possible, this equation must have two equal roots, and therefore must have a divisor common to it and its derivative

$$3x^2+(8-4n)x^2+(1+n)=0, \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

On eliminating x from the equations (3) and (4), we obtain an equation of the fifth degree; of which $n=2$ is one solution, and on dividing there results the biquadratic

$$12n^4-80n^3+123n^2-60n+4=0, \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

which has two possible roots, viz.,

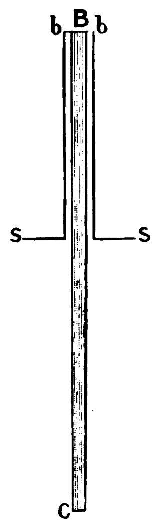
$$n=0\,.\,07873\ \ 06113\ \ 75,$$

and $$n=4\,.\,71418\ \ 14416\ \ 13.$$

The first of these can have no application to our present problem and the second, almost exactly $4\frac{5}{7}$, exceeds the ratio of the expansions of any two available solid substances. Thus we conclude that pendulums constructed of two pieces after the manner shown in figure 1, cannot be compensated.

The equation of condition (3) being of an odd degree, must have always one root possible. It is true that this is merely a numerical possibility, which may or may not be represented by a mechanical arrangement; yet it may be worth while to examine into it. Now, when n is positive, the equation (3), must have one of its roots negative, and thus the point B, corresponding to that root, must be the above point of suspension.

Fig. 2.



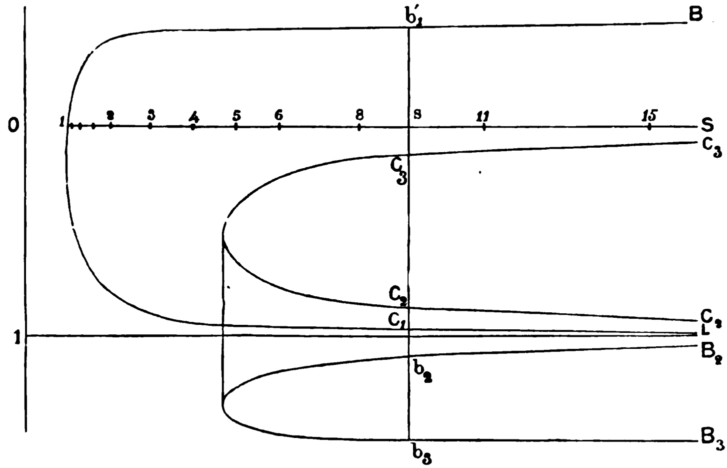
Let us suppose, then, a tube *abba* of glass or some substance having a very slight expansion, to proceed upwards from the axis of motion SS, and from the upper end of the tube let a heavy rod, BC, of more expansive material depend, and the arrangement will represent, mechanically, the negative value of x obtained from the solution of the equation (3).

On assuming for n a number of successive values, and thence computing the corresponding roots of the equation (3), as also the values of a, b, and c, necessary to give a pendulum vibrating isochronously with a simple pendulum whose length is unit, and on representing the results geometrically, we obtain the diagram given in figure 3.

In this figure, the distances measured along the horizontal line OS indicate the ratio of the expansibilities of the two substances, OI standing for that of the suspender; Ol or SL is the length of the corresponding simple pendulum; and the curved lines marked B_1 B_2 B_3; C_1 C_2 C_3 define the values of b and c, corresponding to the three roots of the equation.

For example, if the ratios of the rates of expansion were 9.2, let us take the point s at that distance along OS, and draw through it a vertical line, crossing the six curves at the points b_1 c_3 c_2, c_1 b_2 b_3; then sb_1 c_1 shows the proportions for a pendulum constructed in the manner shown in figure 2. sb_2 c_2 shows that which is intended in the actual execution of clocks, while sb_3 c_3 is another possible arrangement of the same kind, and it is worthy of notice, that the

Fig. 8.



first of these three is always possible, although it be not desirable in practice.

The following Gentlemen were admitted Fellows of the Society:—

Andrew Graham, M.D., R.N.
William Turnbull, Esq.
A. H. Bryce, LL.D.
Francis Deas, LL.B.
Arthur Gamgee, M.D.
Sheriff Hallard.

The following Donations to the Society were announced:—

Proceedings of the Royal Society of London. Vol. XV. No. 88. 8vo.—*From the Society.*

Journal of the Royal Dublin Society. No. 35. 8vo.—*From the Society.*

Journal of the Royal Horticultural Society of London. Vol. I. Part 4. 8vo.—*From the Society.*

Proceedings of the same. Vol. I. (New Series). No. 6. 8vo.—*From the Society.*

Transactions of the Botanical Society of Edinburgh. Vol. VIII. Part 3. 8vo.—*From the Society.*

Journal of the Chemical Society. No. 49. London, 1867. 8vo.—*From the Society.*

Monthly Return of the Births, Deaths, and Marriages registered in the Eight principal Towns of Scotland. December 1866. 8vo.—*From the Registrar-General.*

An Essay to show that Petroleum may be used with advantage in Manufacturing Operations, for the purpose of heating Steam Boilers and generating Steam. By D. Bodde, Notary Public at Batavia. 8vo.—*From the Author.*

Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. No. 12. Bruxelles, 1866. 8vo.—*From the Academy.*

Monatsberichte der Königlichen Preussischen Akademie der Wissenschaften zu Berlin, Sept.—Oct., 1866. 8vo.—*From the Academy.*

Die Menschlichen Parasiten und die von Ihnen Herrührenden Krankheiten. Ein Hand und Lehrbuch für Naturforscher und Aerzte. Von Rudolf Leuckart. Zweiter Band. I. Lieferung. Leipzig, 1867. 8vo.—*From the Editor.*

Comparisons of the Standards of Length of England, France, Belgium, Prussia, Russia, India, and Australia, made at the Ordnance Survey Office, Southampton. London, 1866. 4to.—*From the Survey.*

Sur la Vrille des Cucurbitacées. Par M. Ad. Chatin.—*From the Author.*

Monday, 4th February 1867.

Sir DAVID BREWSTER, President, in the Chair.

The following Communications were read:—

1. On the Tertiary Volcanic Rocks of the British Islands. By Archibald Geikie, Esq., F.R.S.

This paper was in continuation of the series of memoirs on the volcanic rocks of Scotland previously read by the author before the Society,* and contained the first portion of the results of a survey

* See Proceedings, iv. 309, 458, 582, and Transactions, vol. xxii. 633.

of the western region, extending from the south of Antrim to the north of Syke. The districts more especially dwelt upon were the islands of Mull, Eigg, and Staffa. After alluding to the writings of previous geologists upon these tracts, more particularly to the discovery by the Duke of Argyll of tertiary leaves under basalt at Ardtun Head, in Mull, the author remarked that up to this time the great mass of volcanic rocks in the Western Islands has been usually regarded as of Oolitic age—an opinion in which he himself had shared. His object in the present communication was to show that as regards Mull and the adjoining islets this opinion was erroneous, that the enormous volcanic accumulations of these islands belonged in reality to the Miocene period, and that, in all likelihood, the long chain of basaltic masses, extending from the north of Ireland along the west coast of Scotland to the Faroe Islands, and beyond these to Iceland, was all erupted during the same wide interval in the Tertiary periods.

The nature of the volcanic products was first sketched. It was shown that the two great classes of recent lavas—the basaltic and the trachytic—were well represented among the Western Islands, and that the basaltic series was on the whole the older, since it was found to pass under massive sheets of pale grey and blue clay-stones, clinkstones, and porphyries belonging to the trachytic group. In addition to these lava-form rocks, masses of coarse volcanic agglomerate occurred, along with beds of tuff and peperino.

The manner in which these various volcanic rocks occur in Mull and Eigg was next described. It was shown that the leaf-beds of Ardtun, which are known by their fossil contents to be of Miocene age, lie near the bottom of the whole volcanic series, and that above them comes a series of trap-beds between 3000 and 4000 feet in thickness. Throughout this enormous mass of bedded igneous rock layers of ash, often abounding in chalk-flints, are interstratified, and in one part of the cliffs of Inimore of Carsaig a bed of flints twenty-five feet thick lies between the dolerites. Thin lenticular seams or nests of coal likewise occur, but these only occupy small pond-like hollows of the original surface of the trap beds, and are overlaid directly with trap. They are sometimes excellent in quality, and occasionally three feet in thickness; but they rapidly die out in every direction. There is thus no probability that the tertiary coal of the Western Islands will ever come to be of commercial importance.

Proofs of the long continuance of volcanic action among these islands are afforded by the great thickness of the successive sheets of igneous matter, which in one mountain alone—Ben More—reach a depth of 3185 feet without revealing either the actual bottom or top of the series. Another and striking piece of evidence on this subject is given by the well-known Scuir of Eigg. That island consists of nearly horizontal sheets of dolerite, like those of Mull, resting unconformably upon oolitic rocks. After their eruption, they must have been long exposed to the wasting agencies of the atmosphere. A valley was cut out of them, and its bottom was watered by a river, which brought down coarse shingle and sand from the distant Cambrian mountains of the north-west. These changes must have demanded a lengthened lapse of time, yet they took place during an interval in the volcanic history of the island. The igneous forces which had been long dormant broke out anew, and poured several successive *coulées* of vitreous lava down the river-bed. In this way the channel of the stream came to be sealed up. But the same powers of waste which had scooped out that channel continued their operation. The hills which had bounded the valley crumbled away, and the lava-currents that filled the river-bed being much harder than the surrounding rocks, were enabled in great measure to resist the degradation. Hence the singular result now appears that the former hills have been levelled down into slopes and valleys, while the ancient valley occupies the highest ground in the neighbourhood, and its lava-current stands up as the well-known precipitous ridge of the Scuir of Eigg. The gravel and drift-wood of the old river are still to be seen under the rock of the Scuir.

The author then proceeded to point out the possible connection between these tertiary volcanic rocks and the metamorphism of different parts of the West Highlands. He showed that in Mull, under Ben More, the volcanic rocks themselves give signs of having been subjected to a process of metamorphism, and that they are associated there with masses of syenite, like those of Raasay and Skye. Macculloch pointed out that the syenite of the two latter islands was later than the secondary rocks of that district;

pure; it is only faintly yellow when it has not been exposed to heat, but a very slight warmth colours it brown.

The acicular crystals dissolve readily and completely in ether and in alcohol; these solutions also rapidly decompose, chloride of ammonium being deposited, and the viscous substance above mentioned remaining in solution.

It would be premature to speculate on the nature of these substances without quantitative analyses; I hope before long to be able to lay these before the Society.

3. On the Figures of Equilibrium of Liquid Films. By Sir David Brewster, K.H., F.R.S.

In repeating some of the experiments of Professor Plateau, described in seven interesting memoirs published in "The Transactions of the Belgian Academy," and in prosecuting his own experiments on the colours of the soap-bubble, the author of this paper observed several new phenomena which may have escaped the notice of the Belgian philosopher.

Professor Plateau has described and drawn the beautiful systems of soap-films, obtained by lifting from a soap solution a cube made of wires about one and a half inch long. This system is a polyhedron, composed of twelve similar films stretching from the wires, and united to a plane quadrangular film in the centre. When this vertical film was blown upon, M. Von Rees observed that it was reduced to a line, and then reproduced in a horizontal position, from which it could be blown again into a vertical position.

If we suppose the quadrangular film removed, and all the twelve films radiating from the centre of the cube, Professor Plateau found that such a system could not be kept in equilibrium, unless there was something solid in the central point, such as the end of a wire or a drop of fluid.

In repeating these experiments the author found that, after converting the horizontal into the vertical quadrangular film, and continuing the blowing, he produced the radial system of films, which in an instant returned to the system with a vertical film, and then into the system with the horizontal film.

M. Von Rees had found that, by immersing the wire cube with the normal polyhedron a few millimetres in the soap solution, the film formed on its lower face imprisoned the air in the quadrangular pyramid above it, and that this air rose to the centre of the cube, and replaced the quadrangular plane with a hollow cube with curved faces.

In this beautiful experiment the hollow cube is invariable in size, being necessarily equal in its contents to one-fourth part of the wire cube. The author of the present paper discovered a method of inserting a hollow cube of any magnitude in the centre of the polyhedron. This was done by blowing a bubble of the requisite size, and introducing it within the wire cube. He succeeded also by this means in inserting a second hollow cube beside the first, the side common to both being plane when the two cubes were equal, convex when the one was less, and concave when it was greater than the other. In such a system, which is in perfect equilibrium, the number of films is *nineteen.* He found also that two hollow solid figures could, by the same means, be inserted in the other systems of films which Professor Plateau had discovered in a wire tetrahedron, or a quadrangular pyramid, or a regular octohedron, or a rectangular prism, or in a system obtained from two rectangular planes fixed at right angles to each other.

This last and interesting system consists of four curved films extending from each vertical wire, and connected with an elliptical film in the common section of the rectangles. The major axis of this film is four times greater than its minor axis, and it is placed in the angle, which is a little greater than 90°, but sometimes also in the other angle.

By making this system of wires movable, so that the rectangular planes can pass from 90° to 180°, the author obtained some singular results. As the angle increased from 90°, the minor axis of the elliptical film increased, till when it approached to 180° it was nearly circular, appropriating gradually the fluid of the four curved films attached to the wires.

By again diminishing this angle the almost circular film became more and more elliptical, till it reached its normal state at 90°, giving back to the curved films the fluid which formed them. If the angle of the rectangular plane which contain the elliptical film is diminished, the film will grow more elliptical, and at 45°

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The acicular crystals dissolve readily and completely in ether and in alcohol; these solutions also rapidly decompose, chloride of ammonium being deposited, and the viscous substance above mentioned remaining in solution.

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By again diminishing this angle the almost circular film became more and more elliptical, till it reached its normal state at 90°, giving back to the curved films the fluid which formed them. If the angle of the rectangular plane which contain the elliptical film is diminished, the film will grow more elliptical, and at 45°

will become a straight line, giving up its fluid to the other four films. At this instant the whole system changes, the oval film being reproduced in the angle of 135°!

Remarkable as this phenomenon is, there is one still more remarkable, which requires the testimony of the eye to make it credible. If when the rectangles are inclined 90° we blow upon the elliptical film, a bubble of such a size as to replace the system of films with a hollow curvilineal cube, and wait till it bursts, *the system of liquid films which it expelled will reappear, as if it had left its ghost behind it to recover the elements which the bubble had appropriated!*

By uniting the upper and lower ends of all the wires in this system, and also by uniting the wires at various points in their length, the author obtained a number of beautiful and complex systems of films, which require numerous diagrams to make them intelligible.

After treating of the equilibrium of liquid films, as seen in the union of spherical bubbles and other hollow solids, the author considers the formation of plane, convex, and concave films upon the mouths of open and closed vessels of different shapes, and their deposition on the same vessels from bubbles; and he describes various remarkable movements of the films, upwards and downwards, when they are formed upon conical vessels open at both ends.

4. On a Method of ascertaining the Specific Gravity of Water which holds a minute quantity of Foreign Matter in Solution; or, on the Specific Gravity of Impure Water. By Edward Sang, Esq.

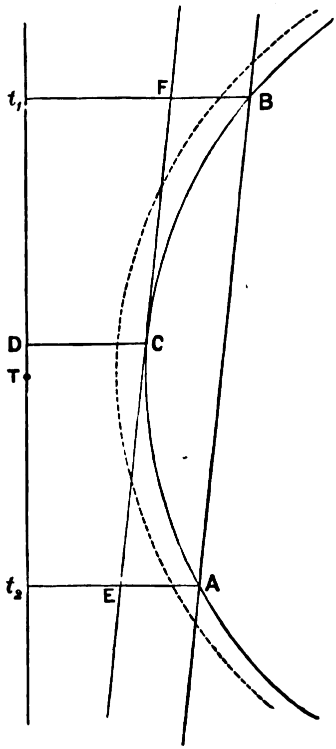
The determination of the degree of impurity of spring or well water is every day becoming of more and more importance. In order to discover how much foreign matter is contained in a sample of water, the chemist is under the necessity of drying up a quantity of it, and of weighing the residue. This operation is tedious, and gives a trust-worthy result only when the evaporation has been carried on in closed vessels from which atmospheric impurities have been carefully excluded; and, therefore, any assistance which can be obtained from mechanical measurements must be acceptable as affording a test for confirmation of the results.

If we attempt to find the specific gravity of any potable water by means of the balance, we encounter very great obstacles; the extreme delicacy of the weighing, and the uncertainty introduced by changes of temperature during the process, render the direct method almost valueless; since both the expansion of the water and that of the glass vessel in which it is contained would need to be allowed for. But, by taking advantage of the peculiarity of water in regard to changes of temperature, we can obtain an extremely sensitive indicator of changes in its composition.

Let us prepare a glass ball, adjusted in its specific gravity so that it will just float in pure water between the temperature of melting ice and 5 degrees of the centigrade thermometer (between 32° and 42° of Fahrenheit's), and placing a delicately graduated thermometer along with it in a jar of cold water, let the whole be allowed slowly to become warmer. The effect of an increase of temperature at first is to produce a contraction in the water, and at the same time a slight expansion in the glass of the ball; and the result is that, when the temperature becomes such that the two specific gravities are alike, the ball begins to float upwards. By placing the ball of the thermometer at some distance from the bottom of the vessel, we can note the temperature at the instant when the ball passes it; let this temperature be t_1.

As the water continues to grow warmer its density augments, but more and more slowly, until it reach its maximum; after this the water begins to expand, and eventually the glass ball, which had risen to the top, begins to descend. If we note the temperature t_2, at which the ball again passes the thermometer bulb, we shall have, in the difference $t_2 - t_1$ between the two temperatures, an argument by help of which to compute the relative densities of the water and the glass.

Since the expansion of water on either side of its state of maximum density is proportional to the square of the change of temperature from that point, if we were to measure, on the line of abscissæ, equal distances to represent the degrees of temperature, and were to lay off ordinates to represent the corresponding volumes of a given quantity of water, the line thus produced would be a parabola. Having traced the curve, let ordinates t_1 A and t_2 B be drawn corresponding to the observed temperatures t_1, t_2, and let the points A and B at which these cut the curve, be joined by a straight line A B. This line is not parallel to the line of abscissæ, but is inclined so as to show the rate of expansion of the glass ball, and a line drawn parallel to it to touch the curve will do so, not on the ordinate which shows the maximum density of the water, but on the ordinate mid-way between t_1 and t_2; which ordinate D C would indicate the temperature at which the water would seem to have the greatest density when inclosed in a thermometer tube made of the same kind of glass. The distance E A, or F B between the tangent and the chord is, according to the well-known properties of the parabola, proportional to the square of the intercepted abscissa Dt_2, or to that of its double t_1 t_2.



Hence the square of the observed interval t_1 t_2 between the two temperatures at which the ball passes the thermometer bulb, when multiplied by some constant number to be determined by means of the known formula for the expansion of water, will give the difference between the glass and the water, when each is of the temperature half-way between t_1 and t_2.

If now we mix in the water some soluble salt, so as to change slightly its specific gravity, without perceptibly altering the law of its expansion, we shall thereby augment the interval between the temperatures at which the ball passes the thermometer; and the difference between the squares of the interval in the one case and in the other will be proportional to the change in the specific gravity of the water, which again, when the amount of impurity is minute, may be held as being proportional to the amount of that impurity.

The determination, by this method, of the quantity of heavy impurity in potable water may be freed from all dependence on a knowledge of the co-efficients of the expansibility of water, by comparing the observations made on distilled water with those made on water containing a known quantity of lime or other saline matter. And this, indeed, is the only useful method of procedure; for in the apparatus exhibited there are many glass balls, and, to distinguish these from each other, they are made of glass of various colours; and it is found that the means of the temperatures at which these pass the thermometer are different for the different colours. These means are also influenced by the conformation of the balls; thus those which are thin, and which, consequently, have heavy drops, are more acted on by the expansion of the internal air, than those are which have thick sides, and, consequently, small drops.

By using a glass ball adjusted to float in pure water just at its maximum relative density, we obtain the greatest delicacy in the indications. I found that such a ball, when placed in the water supplied to the New Town of Edinburgh, gave an interval of 17·2 degrees on Fahrenheit's thermometer. And the same method may be extended to the case of balls which float on water of much higher temperatures, only it then becomes necessary to use a thermometer of proportionally greater delicacy in its graduations. And it may be remarked that, for the purpose of securing certainty in the indications, it is expedient to inclose the jar containing the water to be experimented on in a large vessel also containing water, and to agitate the water contained in it so as to keep the fluid in the internal vessel as nearly as possible at the same temperature from top to bottom. By this arrangement we almost entirely avoid the formation of internal currents, which would tend to give uncertainty to the observations.

5. On the Oxidation of Phenyl Alcohol, and a Mechanical Arrangement adapted to illustrate Structure in the Non-saturated Hydrocarbons. By J. Dewar, Esq. Communicated by Professor Playfair.

The syntheses and oxidation analyses of organic compounds have so confirmed each other in many cases, that chemists are enabled to judge of the structure of a compound from the oxidation products. Many chemists have used the oxidation method in special cases, the bodies operated upon belonging principally to the fatty series; but until Fittig and Beilstein published their memoirs on the aromatic compounds, it was never applied to the systematic study of a hydrocarbon and its derivatives. But although the syntheses and oxidation analyses of the derivatives of benzol confirmed each other, still the structure of the original nucleus (benzol) remained unexplained. Kekulé's original and elegant speculations on the structure of benzol and its derivatives induced me to try the effect of oxidising agents on benzol, with the view of eliciting whether the carbon atoms would separate in the way theory pointed out. The carbon atoms in benzol may be supposed to be arranged in a closed chain, where the carbon affinities are bound two and one alternately. Now, if we examine the formula graphically, it is evident there are three symmetrical groupings, C_2H_2 (acetylene), in benzol. We would, therefore, expect the carbons to separate in twos, and produce the corresponding oxidised product, $C_2H_2O_4$, oxalic acid. I attempted the oxidation of benzol with permanganate of potash, but no decomposition took place in a sealed tube at 150° C. I then had recourse to phenyl alcohol. If a solution of permanganate of potash is added to phenyl alcohol, dissolved in water, the decomposition is immediate, the solution becomes alkaline, and peroxide of manganese separates as a bulky precipitate. Equivalent quantities of the substances, 1 pt. phenyl alcohol to 3·5 pts. of permanganate of potash were the proportions used. If the liquid is filtered and acidulated with acetic acid, on the addition of acetate of lime, a white precipitate of oxalate of lime separates. I have analysed the lime salt, and also examined the physical properties of the acid. So that phenyl alcohol, when treated with one-third of the quantity of permanganate of potash necessary for complete oxidation, gives oxalic acid. If the oxidising agent had been in excess, we could not have considered the action specific; but as Berthelot has shown that acetylene can be condensed into benzol, and that acetylene, when oxidised, gives oxalic acid, we must consider the production of oxalic acid as depending on the more intimate union of the two carbon atoms.

By the regulated oxidation of phenyl alcohol several substances must be produced. If we compare benzol and naphthalin, both members of a homologous series of hydrocarbons where the common difference is C_4H_2, we would expect them to undergo a similar decomposition on oxidation. And as naphthalin, when oxidised, gives oxalic and phthalic acids, thus

$$C_8H_6 . C_2H_2 + O_8 = C_8H_6O_4 + C_2H_2O_4,$$

so we would expect benzol to give oxalic and fumaric or an isomer, thus

$$C_4H_4 . C_2H_2 + O_8 = C_4H_4O_4 + C_2H_2O_4.$$

But if the resisting nucleus in benzol be C_4H_2, we would expect the formation of an acid, $C_4H_2O_4$ mellitic. So far as my experiments have gone, they prove the formation of two other acids along with oxalic, when two molecules of permanganate of potash to one of phenyl alcohol were used. When the alkaline liquid, after filtration from the manganic oxide, is strongly acidulated with hydrochloric acid, shaken up with ether, and the ethereal extract evaporated on the water-bath, a yellow resinous acid, giving a purple colour, with ferric chloride, is left. If this substance is heated, it chars, giving a beautiful sublimate in the form of long needles. The crystalline acid has a styptic taste, and gives a strong reddish violet reaction with ferric chloride, but no reaction with ferrous sulphate or lime water. The acid, therefore, differs from pyrogallic. Whether this crystalline acid is a decomposition product of the resinous acid, or the same substance separated from an impurity, further experiments must decide. When the acid liquid left, after separating the ether, is evaporated to dryness, treated with alcohol to dissolve the oxalic acid, freed from alcohol by evaporation, and the lime salt precipitated in presence of acetic acid, the percentage of lime, in the precipitate, was 4 per cent. below the theoretical quantity required for oxalate of lime. Analysis gave 34·1 per cent. of lime instead of 38·3 per cent.; the salt blackening on ignition. There must therefore be an acid along with oxalic whose lime salt is insoluble in acetic acid, and, judging from the percentage of lime, probably fumaric or mellitic. Further experiments on a larger scale must be made before the exact composition of the two acids can be affirmed.

The application of permanganate potash as an oxidising agent in organic chemistry promises interesting results. Many hydrocarbons are directly attacked, such as the oil from caoutchouc, some of the substances in Young's paraffin oil, &c., while others, having the same composition, resist. It may prove of great value in investigating the isomeric hydrocarbons.

In connection with this subject, I bring before the Society a simple mechanical arrangement adapted to illustrate structure in the non-saturated hydrocarbons. This little device is the mechanical representative of Dr C. Brown's well-known graphic notation. A series of narrow thin bars of brass of equal length are taken, and every two of the bars clamped in the centre by a nut, so as to admit of free motion the one on the other. Such a combination represents a single carbon atom with its four places of attachment. In order to make the combination look like an atom, a thin round disc of blackened brass can be placed under the central nut. At the ends of the arms are holes to connect one carbon atom with another by means of a nut. The filling up of the places of attachment may be effected by slipping on the arms round discs of brass having a groove attached, and placing the symbol of the chemical element on the round projection. A carbon atom would then look like the following diagram (see p. 85).

As it is only intended to express the number of places of attachment along with the arrangement, when a given number of carbon atoms are combined in different ways, it is better to dispense with the symbols, remembering that every free arm represents a place of attachment. When a number of carbon atoms are joined together, all the joints and arms being moveable, it is easy to show saturation in a closed or open chain, and the many arrangements of the atoms corresponding to the same formula. Although the

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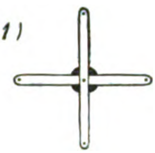
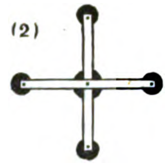
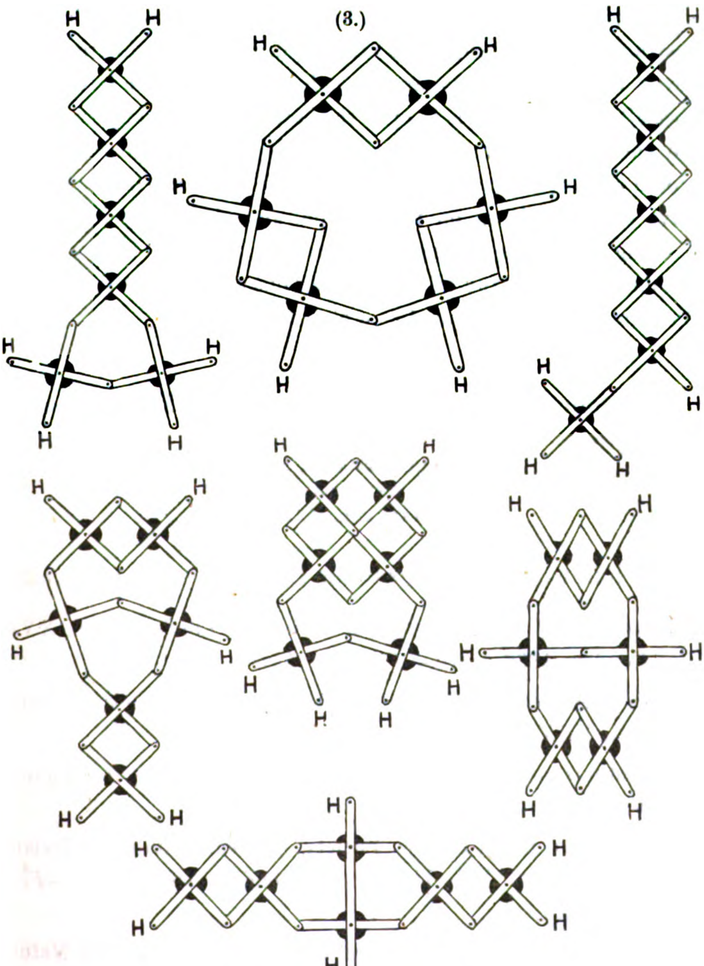


(2)



(3.)



Formulæ of Benzol.

bars are of equal length they are not intended to represent equal forces. We have no unit for comparing the values of the unit affinities in different atoms, and they may be incommensurable. In filling up the places of attachment we saturate the fixing powers of the individual atoms, taking no account of the influence the different elements and radicals have in affecting the energy of the whole compound. Without entering into any detail on the structure of the hydrocarbons, to show the ease with which the instrument can represent the same hydrocarbon in different ways, seven formulæ of benzol are appended. To show the latent affinities, all we have to do is to open the bars connecting any two atoms. To show the six latent affinities in benzol, as represented in a closed circuit, we have merely to separate the bars a little, when we have six free places on the outside, and six in the inside. By opening the whole arrangement we get fourteen free affinities. Naphthalin and many other hydrocarbons give elegant symmetrical diagrams when represented as a closed circuit.

The following Gentlemen were elected Members of the Society:—

Thomas R. Fraser, M.D.
Thomas Annandale, F.R.C.S.
D. R. Haldane, M.D., F.R.C.P.E.

The following Donations to the Library were announced:—

Transactions of the Pathological Society of London. Vol. XVII. London, 1866. 8vo.—*From the Society.*

Monthly Notices of the Royal Astronomical Society, London. Vol. XXVII. No. 2. 8vo.—*From the Society.*

Transactions and Proceedings of the Royal Society of Victoria. Vol. VII. 8vo.—*From the Society.*

Journal of the Royal Asiatic Society of Great Britain and Ireland. New Series. Vol. II. Part 2. London, 1866. 8vo.—*From the Society.*

Journal of the Asiatic Society of Bengal. Edited by the Natural History Secretary. Part 2, No. 2. Calcutta, 1866. 8vo.—*From the Society.*

Journal of the Asiatic Society of Bengal. Edited by the Philological Secretary. Part 1. No. 2. Calcutta, 1866. 8vo.—*From the Society.*

Sitzungsberichte der Königl.-bayer Akademie der Wissenschaften zu Munchen. Tome I. Heft 2, Tome II. Heft 1. Munchen, 1866. 8vo.—*From the Academy.*

Bulletin de la Société des Sciences Naturelles de Neuchatel. Tome VII. Neuchatel, 1866. 8vo.—*From the Society.*

Das Testament des Grossen Kurfürsten, von Joh. Gust. Droysen. Leipzig, 1866. 8vo.—*From the Royal Saxon Academy.*

Tables of Heights in North-west Provinces and Bengal, determined by the Great Trigonometrical Survey of India, by Spirit-levelling Operations, to May 1865. Roorkee, 1866. 8vo.—*From the Survey.*

Berichte über die Verhandlungen der Königlich Sächsischen Gesellschaft der Wissenschaften zu Leipzig. Heft 1, 2, 3. 1866. 8vo.—*From the Society.*

Sul Monto Ondosa del Mare e su le Correnti di esso Specialmente su Quelle Littorali, pel Comm. Alessandro Cialdi. Rome, 1866. 8vo.—*From the Author.*

Les Ports-Canaux. Article Extrait de l'Ouvrage sur le Mouvement des Ondes sur les Courants Littoraux par le Comm. Alexandre Cialdi. Rome, 1866. 8vo.—*From the Author.*

Bulletin de la Société de Géographie, December 1866. Paris, 1866. 8vo.—*From the Society.*

Flora Batava, Afbeelding en beschrijving van Nederlandsche Gewassen. Door Wijlen Jan Kops, vervolgd door Jhr. F. A. Hartsen. No. 196–199. Amsterdam. 4to.—*From the Authors.*

Journal of the Linnean Society. Vol. IX. Zoology, No. 35. London, 1867. 8vo.—*From the Society.*

Journal of the Chemical Society. Series II. Vol. IV. No. 48. London, 1866. 8vo.—*From the Society.*

Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. No. 1. 1867. Bruxelles, 1867. 8vo.—*From the Society.*

Cartes Géologique et Hydrologique de la Ville de Paris. Par M. Delesse. 8vo.—*From the Author.*

Recherches sur l'Origine des Roches. Par M. Delesse. Paris, 1865. 8vo.—*From the Author.*

Recherches sur l'Eau dans l'interieur de la Terre. Par M. Delesse. 8vo.—*From the Author.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VI. 1866–67. No. 73.

Monday, 18*th February* 1867.

SIR DAVID BREWSTER, President, in the Chair.

The following Communications were read:—

1. On an Application of Mathematics to Chemistry. By Dr A. Crum Brown.

In this paper chemical substances are represented as "operands," and chemical processes as "operators"—thus, $\phi \cdot x$ is the substance produced by acting on the substance x by the process ϕ. When ϕ, χ, ψ, &c., act independently on x, this is indicated by the notation $\left.\begin{array}{c}\phi\\ \chi\\ \psi\\ \&c.\end{array}\right| \cdot x$. When they act one on another to produce a complex operator which acts on x, the notation $\phi \cdot \chi \cdot \psi \cdot x$ is used. It is shown that in the latter case the commutative law does not apply to operators.

The meaning of the negative index applied to an operator is defined thus: $\phi \cdot (\phi^{-1} \cdot x) = x$; and it is shown that $\phi^{-1} \cdot x$ may have more than one value. $\left.\begin{array}{c}\phi\\ \phi\end{array}\right| \cdot x$ is written $\phi^{2)} \cdot x$, and generally $\left.\begin{array}{c}\phi^{m)}\\ \phi^{n)}\end{array}\right| \cdot x = \phi^{m+n)} \cdot x$ where m and n are both positive or both negative $\left(\left.\begin{array}{c}\phi\\ \phi\end{array}{}_{-1}\right| \cdot x \text{ not being necessarily} = x\right)$. $\phi \cdot \phi \cdot x$ is written $\phi^2 \cdot x$, and generally $\phi^n \cdot \phi \cdot x = \phi^{n+1} \cdot x$.

The general series $x, \phi \cdot x, \phi^2 \cdot x, \phi^3 \cdot x \cdots\cdots \phi^n \cdot x$ is shown to include the case of homologous series, and the essential difference between this series and x, $\phi \cdot x$, $\phi^{2)} \cdot x$, $\phi^{3} \cdot x \cdots \cdots \phi^{n)} \cdot x$ is pointed out.

2. Notice of an Easy Method of Preparing Nitroprussides. By Dr Lyon Playfair, F.R.S.

Since I described the method of making nitroprussides by the action of nitric acid on yellow prusside of potassium, Hadow (Chem. Soc. Journal, 1866) has given a more productive process, by treating potassic ferridcyanide with an equivalent quantity of mercuric chloride, of sodic nitrite, and of acetic acid. Hadow contends that N_2O_3 substitutes Cy_2 in this reaction, instead of NO as supposed by me. I am still of opinion that the formula of nitroprussides is $FeCy_5 NOM_2$. For if an equivalent quantity of an acid be added to a solution of potassic ferridcyanide, and a stream of NO be passed through this solution, at a temperature of between 70° C and 80° C, nitroprusside is slowly but abundantly formed, with the evolution of hydrocyanic acid.

$$FeCy_6K_2H + NO = FeCy_5NOK_2 + HCy.$$

In these experiments every care was taken to remove all traces of a higher oxides of nitrogen. But this process is not productive, and is only interesting as showing that NO can substitute Cy.

After various experiments, I found the following simple process to yield excellent results:—Equal weights of potassic ferridcyanide and of crystallised tartaric acid are dissolved separately in the smallest quantity of cold water. One-half of the solution of tartaric acid is then mixed with the solution of ferridcyanide and briskly stirred, the clear liquid being decanted from the precipitated cream of tartar. A standard solution of sodic nitrite* having been determined beforehand, a quantity, corresponding to one equivalent for each equivalent of the potassic ferridcyanide used, is now divided into two parts. One-half of this solution is added to the cold mixture as above described, and immediately after the remaining half

* I prefer for this purpose sodic nitrite, made by fusing lead with sodic nitrate, lixiviating with water, and then adding H_2SO_4 cautiously, till the lead is precipitated.

of the solution of tartaric acid, and a solution of one equivalent of chloride of sodium. These cause a further precipitation of potassic bitartrate. A slight effervescence now ensues; and from the insoluble precipitate giving points of escape for the nitrous acid, a good deal would be lost. The clear solution is, therefore, rapidly decanted from the precipitate, and the latter is thrown on a filter to drain. The mixture is now heated up as quickly as possible to the boiling point, and is briskly boiled.* Portions of it, taken out at intervals upon bibulous paper, must be tested for nitrite with a solution of starch and potassium iodide. When the nitrite appears to be vanishing, the remaining half of the nitrite solution must be added by degrees, so as always to keep nitrite in solution; and it must remain distinctly acid with the original tartaric acid added. In about an hour the solution, which had become at first coffee-coloured, has acquired a fine clear red; but it must be boiled for some time longer. The reasons for the precautions in the manner of adding the nitrite, and preventing any large excess either of it or of acid, are the following:—Red prusside of potassium heated with sodic nitrite, becomes yellow prusside of sodium and potassium, which would remain unconverted. Again, if the tartaric acid has decomposed all the nitrite, and remains in the boiling solution, it decomposes the ferridcyanide, and forms Prussian blue. But if the above process be followed out, after one or two hours' boiling, the whole of the ferridcyanide is converted into nitroprusside. The progress is readily ascertained by placing from time to time a drop of the solution on bibulous paper, and touching it with a solution of the ferrous sulphate. The last traces of ferridcyanide are, however, the most difficult to obliterate. The solution is now evaporated and crystallized. Two abundant crops of nearly pure sodic nitroprusside may thus be obtained. The third crop is generally mixed with potassic chloride, and sodic and potassic tartrate, and has to be picked out from them.

Unless the precautions which have been described have been strictly attended to, a small quantity of sodic ferrocyanide will be found in the mother liquor.

* If the solution, on mixture, instead of being boiled, as recommended, be kept cool, a large quantity of nitroprusside is still formed, and a considerable amount of oxamide is deposited.

By the process now described sodic nitroprusside can be made in any quantity, and without the loss of any of the Cyanogen products. The reaction involved in the above process is extremely simple. The first portion of tartaric acid removes an atom of potassium, and substitutes it by hydrogen. The second portion of acid acts on the sodic nitrite, liberating nitrous acid:—

$$2FeCy_6K_2H + 2NO_2H = 2H_2O + 2FeCy_5NOK_2 + Cy_2.$$

The sodic chloride and tartrate exchange their sodium for the potassium of the nitroprusside, partly on addition, and more largely on crystallization.

3. Note on the Reality of the Roots of the Symbolical Cubic which expresses the Properties of a Self-conjugate Linear and Vector Function. By Professor Tait.

Hamilton has shown that if

$$\phi\rho = \Sigma\alpha S\beta\rho + A\rho,$$

where α and β are given vectors, and A a given scalar, we have

$$(\phi^3 - m_2\phi^2 + m_1\phi - m)\rho = 0,$$

where m, m_1, m_2 are scalars depending only on ϕ.

When the function ϕ *is its own conjugate*, *i.e.*, when

$$S\rho\phi\sigma = S\sigma\phi\rho,$$

ρ and σ being any vectors whatever, the vectors for which

$$(\phi - g)\rho = 0, \text{ or } \phi\rho \parallel \rho, \text{ or } V\rho\phi\rho = 0,$$

form in general a real and definite rectangular system. This, of course, may in particular cases degrade into one definite vector, and *any* pair of others perpendicular to it; and cases may occur in which the equation is satisfied for *every* vector.

Suppose the roots of $m_g = m + m_1 g + m_2 g^2 + g^3 = 0$ to be real and different, then

$$\left.\begin{aligned}\phi\rho_1 &= g_1\rho_1\\ \phi\rho_2 &= g_2\rho_2\\ \phi\rho_3 &= g_3\rho_3\end{aligned}\right\} \text{where } \rho_1, \rho_2, \rho_3 \text{ are definite vectors.}$$

Hence $g_1 g_2 S\rho_1\rho_2 = S\phi\rho_1\phi\rho_2$

$$= S\rho_1\phi^2\rho_2, \text{ or } = S\rho_2\phi^2\rho_1,$$

because ϕ is its own conjugate.

But
$$\phi^2\rho_2 = g_2^2\rho_2,$$
$$\phi^2\rho_1 = g_1^2\rho_1,$$

and therefore

$$g_1 g_2 S\rho_1\rho_2 = g_2^2 S\rho_1\rho_2 = g_1^2 S\rho_1\rho_2;$$

which, as g_1 and g_2 are by hypothesis different, requires

$$S\rho_1\rho_2 = 0.$$

Similarly $S\rho_2\rho_3 = 0, \quad S\rho_3\rho_1 = 0.$

If two roots be equal, as g_2, g_3, we still have, by the above proof, $S\rho_1\rho_2 = 0$, and $S\rho_1\rho_3 = 0$. But there is nothing farther to determine ρ_2 and ρ_3, which are therefore *any* vectors perpendicular to ρ_1.

If all three roots be equal, *every* real vector satisfies the equation $(\phi - g)\rho = 0$.

Next, as to the *reality* of the three roots when the function is self-conjugate.

Suppose $g_2 + h_2\sqrt{-1}$ to be a root, and let $\rho_2 + \sigma_2\sqrt{-1}$ be the corresponding value of ρ, where g_2 and h_2 are real numbers, ρ_2 and σ_2 real vectors, and $\sqrt{-1}$ the old imaginary of algebra.

Then $\phi(\rho_2 + \sigma_2\sqrt{-1}) = (g_2 + h_2\sqrt{-1})(\rho_2 + \sigma_2\sqrt{-1}),$

and this divides itself, as in algebra, into the two equations

$$\phi\rho_2 = g_2\rho_2 - h_2\sigma_2,$$
$$\phi\sigma_2 = h_2\rho_2 + g_2\sigma_2.$$

Operating on these by $S\sigma_2$, $S\rho_2$ respectively, and subtracting the results, remembering our condition as to the nature of ϕ

$$S\sigma_2\phi\rho_2 = S\rho_2\phi\sigma_2,$$

we have $h_2(\sigma_2^2 + \rho_2^2) = 0.$

But, as σ_2 and ρ_2 are both real vectors, the sum of their squares cannot vanish. Hence h_2 vanishes, and with it the impossible part of the root.

4. On Vortex Atoms. By Professor Sir William Thomson.

After noticing Helmholtz's admirable discovery of the law of vortex motion in a perfect liquid, that is, in a fluid perfectly destitute of viscosity (or fluid friction), the author said that this discovery inevitably suggests the idea that Helmholtz's rings are the only true atoms. For the only pretext seeming to justify the monstrous assumption of infinitely strong and infinitely rigid pieces of matter, the existence of which is asserted as a probable hypothesis by some of the greatest modern chemists in their rashly-worded introductory statements, is that urged by Lucretius and adopted by Newton; that it seems necessary to account for the unalterable distinguishing qualities of different kinds of matter. But Helmholtz has proved an absolutely unalterable quality in the motion of any portion of a perfect liquid, in which the peculiar motion which he calls "wirbel-bewegung" has been once created. Thus, any portion of a perfect liquid which has "wirbel-bewegung" has one recommendation of Lucretius' atoms—infinitely perennial specific quality. To generate or to destroy "wirbel-bewegung" in a perfect fluid can only be an act of creative power. Lucretius' atom does not explain any of the properties of matter without attributing them to the atom itself. Thus the "clash of atoms," as it has been well called, has been invoked by his modern followers to account for the elasticity of gases. Every other property of matter has similarly required an assumption of specific forces pertaining to the atom. It is as easy (and as improbable—not more so) to assume whatever specific forces may be required in any portion of matter which possesses the "wirbel-bewegung," as in a solid indivisible piece of matter, and hence the Lucretius atom has no *prima facie* advantage over the Helmholtz atom. A magnificent display of smoke-rings, which he recently had the pleasure of witnessing in Professor Tait's lecture-room, diminished by one the number of assumptions required to explain the properties of matter, on the hypothesis that all bodies are composed of vortex atoms in a perfect homogeneous liquid. Two smoke-rings were frequently seen to bound obliquely from one another, shaking violently from the effects of the shock. The result was very similar to that observable in two large india-rubber rings striking one another in the air. The elasticity of each smoke-ring seemed no further from perfection than might be expected in a solid india-rubber ring of the same shape from what we know of the viscosity of india-rubber. Of course this kinetic elasticity of form is perfect elasticity for vortex rings in a perfect liquid. It is at least as good a beginning as the "clash of atoms" to account for the elasticity of gases. Probably the beautiful investigations of D. Bernouilli, Herapath, Joule, Krönig, Clausius, and Maxwell, on the various thermodynamic properties of gases, may have all the positive assumptions they have been obliged to make as to mutual forces between two atoms and kinetic energy acquired by individual atoms or molecules, satisfied by vortex rings, without requiring any other property in the matter whose motion composes them than inertia and incompressible occupation of space. A full mathematical investigation of the mutual action between two vortex rings of any given magnitudes and velocities, passing one another in any two lines, so directed that they never come nearer one another than a large multiple of the diameter of either, is a perfectly solvable mathematical problem; and the novelty of the circumstances contemplated presents difficulties of an exciting character. Its solution will become the foundation of the proposed new kinetic theory of gases. The possibility of founding a theory of elastic solids and liquids on the dynamics of more closely-packed vortex atoms may be reasonably anticipated. It may be remarked, in connection with this anticipation, that the mere title of Rankine's paper on "Molecular Vortices," communicated to the Royal Society of Edinburgh in 1849 and 1850, was a most suggestive step in physical theory.

Diagrams and wire models were shown to the Society, to illustrate knotted or knitted vortex atoms, the endless variety of which is infinitely more than sufficient to explain the varieties and allotropies of known simple bodies and their mutual affinities. It is to be remarked that two ring atoms linked together, or one knotted in any manner with its ends meeting, constitute a system which, however it may be altered in shape, can never deviate from its own peculiarity of multiple continuity, it being impossible for the matter in any line of vortex motion to go through the line of any other matter in such motion or any other part of its own line. In fact, a closed line of vortex core is literally indivisible by any action resulting from vortex motion.

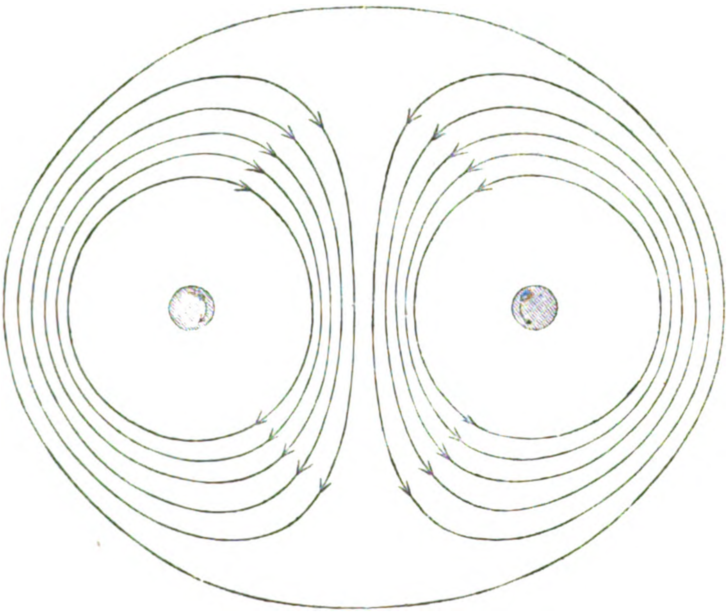
The author called attention to a very important property of the vortex atom, with reference to the now celebrated spectrum analysis practically established by the discoveries and labours of Kirchof and Bunsen. The dynamical theory of this subject, which Professor Stokes had taught to the author of the present paper before September 1852, and which he has taught in his lectures in the University of Glasgow from that time forward, required that the ultimate constitution of simple bodies should have one or more fundamental periods of vibration, as has a stringed instrument of one or more strings, or an elastic solid consisting of one or more tuning forks rigidly connected. To assume such a property in the Lucretius atom, is at once to give it that very flexibility and elasticity, for the explanation of which, as exhibited in aggregate bodies, the atomic constitution was originally assumed. If, then, the hypothesis of atoms and vacuum imagined by Lucretius and his followers to be necessary to account for the flexibility and compressibility of tangible solids and fluids, were really necessary, it would be necessary that the molecule of sodium, for instance, should be not an atom, but a group of atoms with void space between them. Such a molecule could not be strong and durable, and thus it loses the one recommendation, which has given it the degree of acceptance it has had among philosophers; but, as the experiments shown to the Society illustrate, the vortex atom has perfectly definite fundamental modes of vibration, depending solely on that motion, the existence of which constitutes it. The discovery of these fundamental modes forms an intensely interesting problem of pure mathematics. Even for a simple Helmholtz ring, the analytical difficulties which it presents are of a very formidable character, but certainly far from insuperable in the present state of mathematical science. The author of the present communication had not attempted, hitherto, to work it out except for an infinitely long, straight, cylindrical vortex. For this case he is working out solutions corresponding to every possible description of infinitesimal vibration, and intended to include them in a mathematical paper, which he hoped soon to be able to communicate to the Royal Society. One very simple result which he could now state is the following. Let such a vortex be given with its section differing from exact circular figure by an infinitesmal harmonic deviation of order i. This *form* will travel as waves round the axis of the cylinder in the same direction as the vortex rotation, with an angular velocity equal to $\frac{i-1}{i}$ of the angular velocity of this rotation. Hence, as the number of crests in a whole circumference is equal to i, for a harmonic deviation of order i, there are $i-1$ periods of vibration, in the period of revolution of the vortex. For the case $i=1$ there is no vibration, and the solution expresses merely an infinitesimally displaced vortex with its circular form unchanged. The case $i=2$ corresponds to elliptic deformation of the circular section; and for it the period of vibration is simply, therefore, the period of revolution. These results are, of course, applicable to the Helmholtz ring, when the diameter of the approximately circular section is small in comparison with the diameter of the ring, as it is in the smoke-rings exhibited to the Society. The lowest fundamental modes of the two kinds of transverse vibrations of a ring, such as the vibrations that were seen in the experiments, must be much graver than the elliptic vibration of section. It is probable that the vibrations which constitute the incandescence of sodium vapour are analogous to those which the smoke-rings had exhibited; and it is therefore probable that the period of the vortex rotations of the atoms of sodium vapour are much less than $\frac{1}{525}$ of the millionth of the millionth of a second, this being approximately the period of vibration of the yellow sodium light. Further, inasmuch as this light consists of two sets of vibrations co-existent in slightly different periods, equal approximately to the time just stated, and of as nearly as can be perceived equal intensities; the sodium atom must have two fundamental modes of vibration, having those for their respective periods, and being about equally excitable by such forces as the atom experiences in the incandescent vapour. This last condition renders it probable that the two fundamental modes concerned are approximately similar (and not merely different orders of different series chancing to concur very nearly in their periods of vibration). In an approximately circular and uniform disc of elastic solid the fundamental modes of transverse vibration, with nodal division into quadrants, fulfils both the conditions. In an approximately circular and uniform ring of elastic solid these conditions are fulfilled for the flexural vibrations in its plane, and also in its transverse vibrations perpendicular to its own plane. But the circular vortex ring, if created with one part somewhat thicker than another, would not remain so, but would experience longitudinal vibrations round its own circumference, and could not possibly have two fundamental modes of vibration similar in character and approximately equal in period. The same assertion may, it is probable,* be practically extended to any atom consisting of a single vortex ring, however involved, as illustrated by those of the models shown to the Society, which consisted of only a single wire, knotted in various ways. It seems, therefore, probable that the sodium atom may not consist of a single vortex line; but it may very probably consist of two approximately equal vortex rings passing through one another, like two links of a chain. It is, however, quite certain that a vapour consisting of such atoms, with proper volumes and angular velocities in the two rings of each atom, would act precisely as incandescent sodium vapour acts; that is to say, would fulfil the "spectrum test" for sodium.

The possible effect of change of temperature on the fundamental modes cannot be pronounced upon without mathematical investigation not hitherto executed; and therefore we cannot say that the dynamical explanation now suggested is mathematically demonstrated so far as to include the very approximate identity of the periods of the vibrating particles of the incandescent vapour with those of their corresponding fundamental modes, at the lower temperature, at which the vapour exhibits its remarkable absorbing power for the sodium light.

A very remarkable discovery made by Helmholtz regarding the

* [*Note, April* 25, 1867.—The author has recently seen reason for believing that the sodium characteristic might be realised by a certain configuration of a single line of vortex core to be described in the mathematical paper which he intends to communicate to the Society.]

simple vortex ring is, that it always moves, relatively to the distant parts of the fluid, in a direction perpendicular to its plane towards the side towards which the rotatory motion carries the inner parts of the ring. The determination of the velocity of this motion, even approximately for rings of which the sectional radius is small in comparison with the radius of the circular axis, has presented mathematical difficulties which have not yet been overcome.



In the smoke-rings which have been actually observed, it seems to be always something smaller than the velocity of the fluid along the straight axis through the centre of the ring; for the observer standing beside the line of motion of the ring, sees, as its plane passes through the position of his eye, a convex*

* The diagram represents precisely the convex outline referred to, and the lines of motion of the interior fluid carried along by the vortex, for the case of a double vortex consisting of two infinitely long, parallel straight vortices of equal rotations in opposite directions. The curves have been drawn by

outline of an atmosphere of smoke in front of the ring. This convex outline indicates the bounding surface between the quantity of smoke which is carried forward with the ring in its motion, and the surrounding air which yields to let it pass. It is not so easy to distinguish the corresponding convex outline behind the ring, because a confused trail of smoke is generally left in the rear. In a perfect fluid the bounding surface of the portion carried forward would necessarily be quite symmetrical on the anterior and posterior sides of the middle plane of the ring. The motion of the surrounding fluid must be precisely the same as it would be if the space within this surface were occupied by a smooth solid; but in reality the air within it is in a state of rapid motion, circulating round the circular axis of the ring with increasing velocity on the circuits nearer and nearer to the ring itself. The circumstances of the actual motion may be imagined thus:—Let a solid column of india-rubber, of circular section, with a diameter small in proportion to its length, be bent into a circle, and its two ends properly spliced together, so that it may keep the circular shape when left to itself; let the aperture of the ring be closed by an infinitely thin film; let an impulsive pressure be applied all over this film, of intensity so distributed as to produce the definite motion of the fluid, specified as follows, and instantly thereafter let the film be all liquefied. This motion is, in accordance with one of Helmholtz's laws, to be along those curves which would be the lines of force, if, in place of the india-rubber circle, were substituted a ring electro-magnet,* and the velocities at different points are to be in proportion to the intensities of the magnetic forces in the corresponding points of the magnetic field. The motion, as has been long known, will fulfil this definition, and will continue fulfilling it, if the initiating velocities at every point of the film perpendicular to its own

Mr D. M'Farlane, from calculations which he has performed by means of the equation of the system of curves, which is

$$\frac{y^2}{a^2}=\frac{2x}{a}\cdot\frac{N+1}{N-1}-\left(1+\frac{x^2}{a^2}\right),\ \text{where}\ \log_e N=\frac{x+b}{a},$$

The proof will be given in the mathematical paper which the author intends to communicate in a short time to the Royal Society of Edinburgh.

* That is to say, a circular conductor with a current of electricity maintained circulating through it.

plane be in proportion to the intensities of the magnetic force in the corresponding points of the magnetic field. Let, now, the ring be moved perpendicular to its own plane in the direction *with* the motion of the fluid, through the middle of the ring, with a velocity very small in comparison with that of the fluid at the centre of the ring. A large approximately globular portion of the fluid will be carried forward with the ring. Let the velocity of the ring be increased; the volume of fluid carried forward will be diminished in every diameter, but most in the axial or fore-and-aft diameter, and its shape will thus become sensibly oblate. By increasing the velocity of the ring forward more and more this oblateness will increase, until, instead of being wholly convex it will be concave, before and behind, round the two ends of the axis. If the forward velocity of the ring be increased until it is just equal to the velocity of the fluid, through the centre of the ring, the axial section of the outline of the portion of fluid carried forward will become a lemniscate. If the ring be carried still faster forward, the portion of it carried with the india-rubber ring will be itself annular; and, relatively to the ring, the motion of the fluid will be backwards through the centre. In all cases, the figure of the portion of fluid carried forward, and the lines of motion, will be symmetrical, both relatively to the axis and relatively to the two sides of the equatorial plane. Any one of the states of motion thus described might of course be produced either in the order described, or by first giving a velocity to the ring, and then setting the fluid in motion by aid of an instantaneous film, or by applying the two initiative actions simultaneously. The whole amount of the impulse required, or, as we may call it, the effective momentum of the motion, or simply the momentum of the motion, is the sum of the integral values of the impulses on the ring and on the film required to produce one or other of the two components of the whole motion. Now it is obvious that, as the diameter of the ring is very small in comparison with the diameter of the circular axis, the impulse on the ring must be very small in comparison with the impulse on the film, unless the velocity given to the ring is much greater than that given to the central parts of the film. Hence, unless the velocity given to the ring is so very great as to reduce the volume of the fluid carried forward with it to something not incomparably greater than the volume of the solid ring itself, the momenta of the several configurations of motions we have been considering will exceed by but insensible quantities the momentum when the ring is fixed. The value of this momentum is easily found by a proper application of Green's formulæ. Thus the actual momentum of the portion of fluid carried forward (being the same as that of a solid of the same density moving with the same velocity), together with an equivalent for the inertia of the fluid yielding to let it pass, is approximately the same in all these cases, and is equal to a Green's integral expressing the whole initial impulse on the film. The equality of the effective momentum for different velocities of the ring is easily verified without analysis for velocities not so great as to cause sensible deviations from spherical figure in the portion of fluid carried forward. Thus, in every case, the length of the axis of the portion of the fluid carried forward is determined by finding the point in the axis of the ring at which the velocity is equal to the velocity of the ring. At great distances from the plane of the ring this velocity varies, as does the magnetic force of an infinitesimal magnet on a point in its axis, inversely as the cube of the distance from the centre. Hence the cube of the radius of the approximately globular portion carried forward is in simple proportion to the velocity of the ring, and therefore its momentum is constant for different velocities of the ring. To this must be added, as was proved by Poisson, a quantity equal to half its own amount, as an equivalent for the inertia of the external fluid; and the sum is the whole effective momentum of the motion. Hence we see not only that the whole effective momentum is independent of the velocity of the ring, but that its amount is the same as the magnetic moment in the corresponding ring electro-magnet. The same result is, of course, obtained by the Green's integral referred to above.

The synthetical method just explained is not confined to the case of a single circular ring specially referred to, but is equally applicable to a number of rings of any form, detached from one another, or linked through one another, in any way, or to a single line knotted to any degree and quality of "multiple continuity," and joined continuously, so as to have no end. In every possible such case, the motion of the fluid at every point, whether of the vortex core or of the fluid filling all space round it, is perfectly determined by Helmholtz's formulæ, when the shape of the core is given. And the synthetic investigation now explained proves that the effective momentum of the whole fluid motion agrees, in magnitude and direction, with the magnetic moment of the corresponding electro-magnet. Hence, still considering, for simplicity, only an infinitely thin line of core, let this line be projected on each of three planes at right angles to one another. The areas of the plane circuits thus obtained (to be reckoned, according to De Morgan's rule, when automic, as they will generally be), are the components of momentum perpendicular to these three planes. The verification of this result will be a good exercise on "multiple continuity." The author is not yet sufficiently acquainted with Riemann's remarkable researches on this branch of analytical geometry, to know whether or not all the kinds of "multiple continuity" now suggested are included in his classification and nomenclature.

That part of the synthetical investigation in which a thin solid wire ring is supposed to be moving in any direction through a fluid with the free vortex motion previously excited in it, requires the diameter of the wire at every point to be infinitely small in comparison with the radius of curvature of its axis and with the distance of the nearest of any other part of the circuit from that point of the wire. But when the effective moment of the whole fluid motion has been found for a vortex with infinitely thin core, we may suppose any number of such vortices, however, near one another to be excited simultaneously: and the whole effective momentum, in magnitude and direction, will be the resultant of the momenta of the different component vortices each estimated separately. Hence we have the remarkable proposition that the effective momentum of any possible motion in an infinite incompressible fluid agrees in direction and magnitude with the magnetic moment of the corresponding electro-magnet in Helmholtz's theory. The author hopes to give the mathematical formulæ expressing and proving this statement in the more detailed paper, which he hopes soon to be able to lay before the Royal Society.

The question early occurs to any one either observing the phenomena of smoke-rings or investigating the theory—What conditions determine the size of the ring in any case? Helmholtz's investigation proves that the angular vortex velocity of the core varies directly as its length, or inversely as its sectional area. Hence the strength of the electric current in the electro-magnet, corresponding to an infinitely thin vortex core, remains constant, however much its length may be altered in the course of the transformations which it experiences by the motion of the fluid. Hence it is obvious that the larger the diameter of the ring, for the same volume and strength of vortex motions in an ordinary Helmholtz ring, the greater is the whole kinetic energy of the fluid, and the greater is the momentum; and we therefore see that the dimensions of a Helmholtz ring are determinate when the volume and strength of the vortex motion are given, and besides, either the kinetic energy or the momentum of the whole fluid motion due to it. Hence if, after any number of collisions or influences, a Helmholtz ring escapes to a great distance from others and is then free, or nearly free, from vibrations, its diameter will have been increased or diminished according as it has taken energy from, or given energy to, the others. A full theory of the swelling of vortex atoms by elevation of temperature is to be worked out from this principle.

Professor Tait's plan of exhibiting smoke rings is as follows:—A large rectangular box, open at one side, has a circular hole of six or eight inches diameter cut in the opposite side. A common rough packing-box of two feet cube or thereabout will answer the purpose very well. The open side of the box is closed by a stout towel or piece of cloth, or by a sheet of India-rubber stretched across it. A blow on this flexible side causes a circular vortex ring to shoot out from the hole on the other side. The vortex rings thus generated are visible, if the box is filled with smoke. One of the most convenient ways of doing this is to use two retorts with their necks thrust into holes made for the purpose in one of the sides of the box. A small quantity of muriatic acid is put into one of these retorts, and of strong liquid ammonia into the other. By a spirit-lamp applied from time to time to one or other of these retorts, a thick cloud of sal-ammoniac is readily maintained in the inside of the box. A curious and interesting experiment may be made with two boxes thus arranged, and placed either side by side, close to one another, or facing one another so as to project smoke-rings meeting from opposite directions; or in various relative positions, so as to give smoke-rings proceeding in paths inclined to one another at any angle, and passing one another at various distances. An interesting variation of the experiment may be made by using clear air without smoke in one of the boxes. The invisible vortex rings projected from it render their existence startlingly sensible, when they come near any of the smoke rings proceeding from the other box.

The following Gentleman was admitted a Fellow of the Society:—

JOHN M. M'CANDLISH, Esq., W.S.,

The following Donations to the Society were announced:—

Proceedings of the Royal Society of London. Vol. XV. No. 89. 8vo.—*From the Society.*

Proceedings of the Natural History Society of Dublin. Vol. IV. Part 3. 8vo.—*From the Society.*

Quarterly Journal of the Geological Society, London. Vol. XXIII. Part 1. 8vo.—*From the Society.*

Journal of the Chemical Society, London. Vol. V. No. 1. 8vo.—*From the Society.*

Ninth Detailed Annual Report of the Registrar-General of Births, Deaths, and Marriages in Scotland. Edinburgh, 1867. 8vo. —*From the Registrar-General.*

An Elementary Treatise on Quartz and Opal. By George Wm. Traill, F.G.S.E. Edinburgh, 1867. 8vo.—*From the Author.*

Bulletin de la Société de Géographie. Paris, Janvier, 1867. 8vo. —*From the Society.*

Die Urzeit der Erde, gedicht von Franz v. Kobell. München, 1856. 8vo.—*From the Author.*

Akademische Denkreden von Dr Carl Fr. Ph. v. Martius. Leipzig, 1866. 8vo.—*From the Author.*

Bulletin de la Société Impériale des Naturalistes de Moscou. No. 3, 1865; No. 2, 1866. 8vo.—*From the Society.*

Berichte über die Verhandlungen der Königlich Sachsischen Gesellschaft der Wissenschaften zu Leipzig. Mathematisch—Physische Classe No. 1–3. 1866. 8vo.—*From the Society.*

Abhandlungen der Philosophisch - Philologischen Classe der Königlich Bayerischen Akademie der Wissenschaften. Band XXXIX. B. XLII. Historischen Classe. B. XXXVIII. München, 1866. 4to.—*From the Academy.*

Abhandlungen herausgegeben von der Senckenbergischen Naturforschenden Gesellschaft. Band VI. No. 1, 2. Frankfurt, 1866. 4to.—*From the Society.*

Jahrbuch der Kaiserlich-Königlichen Geologischen Reichsanstalt. Band XVI. No. 2, 3. Wien, 1866. 8vo.—*From the Society.*

Elektrische Untersuchungen, Siebente Abhandlung. Uber die Thermoelektrischen Eigenschaften des Bergkrystalles. Von W. G. Hankel. Leipzig, 1866. 8vo.—*From the Natural History Society, Leipzig.*

Bestimmung des Längenunterschiedes zwischen den Sternwarten zu Gotha und Leipzig. Von P. A. Hansen. Leipzig, 1866. 8vo.—*From the Natural History Society, Leipzig.*

Monday, 4th March 1867.

Sir DAVID BREWSTER, President, in the Chair.

The following Communications were read :—

1. On the Arctic Shell Clay of Elie and Errol, viewed in connection with our other Glacial and more Recent Deposits. By the Rev. Thomas Brown, F.R.S.E.

Referring to the papers formerly read before the Society in 1863 and 1864, the author first noticed the Section at Errol, its stratigraphical position and fossils, more especially the skeleton of a seal which had been found in the clay. He then described more in detail the different deposits as they occur in the neighbourhood of Elie. Since the shore Section was described in 1863 two new Sections had been examined, and the position of the different beds more fully ascertained. One was in the railway cutting extending from Elie Bridge to the Railway Station, in which, when first laid open, the series of deposits was remarkably well displayed. Beginning a little to the east of the station, the following beds were seen in the descending order :—

1. Blown sand with intercalated beds of shell-bearing peat, one of which was six feet in thickness. This upper portion of the series is largely developed.

2. A peculiar bed of peat destitute of shells, and apparently a continuation of that at the shore,—the submerged forest.

3. A portion of the high-level stratified gravels and sands of the district.

At this point there is a break in the series. The portion above is unconformable to that beneath.

4. The arctic shell clay—a continuation of the same deposit seen on the shore about one-third of a mile distant.

5. The boulder-clay in two stages, with large boulders enclosed.

6. The basement bed of the boulder-clay full of shivers, and resting immediately on the rock.

These different deposits are seen in regular succession from east to west.

The other Section lies to the westward of Kincraig, and runs nearly at right angles to the former, passing from the shore inland up the course of the Cockle Mill Burn. It shows the following beds:—

1. Blown sand rising into dunes close to the shore.

2. The so called raised beach consisting of sand, shingle, and shells in layers, the shells being the common species of the shore, and very much in the same state as those now on the beach. This deposit is powerfully displayed, rising at some points to twenty feet above high-water mark.

3. Further inland, and apparently underlying No. 2, is a series of sands and clays in contorted layers. Marine shells occur at various points. One of the layers of clay was pointed out containing numerous specimens of *Scrobicularia piperata* in their natural position, with the siphonal end uppermost. As this bed is from twelve to fourteen feet above high-water mark, attention was called to the evidence thus furnished for a rise in the bed of the Firth, and its remarkable coincidence with the evidence furnished by the same deposits at Stirling and Portobello.

4. The submerged forest of Largo Bay as it is found near the mouth of the stream, between tide-marks.

The additional species of shells were described as determined by Dr O. Tozell, and the still more complete evidence thus furnished of extreme arctic cold.

2. On the Motions and Colours upon Films of Alcohol, Volatile Oils, and other Fluids. By Sir David Brewster, K.H., F.R.S., &c.

In a paper "On the Phenomena of thin Plates exposed to Polarised Light," published in the Philosophical Transactions for 1841, the author observed certain motions and colours upon some of the volatile and fixed oils, the cause of which he did not attempt to discover. Their apparent similarity to the molecular movements and colours, described in a preceding paper, induced him to resume the subject.

When a drop of alcohol is placed upon an aperture the fifth of an inch in diameter or less, a concave lens will be formed upon it. As the alcohol evaporates, a very small plane film will appear in the centre, and will gradually increase till it fills the aperture. If held in a vertical or even inclined position, and examined by transmitted light, a current of fluid, without colour, will be seen issuing from the margin of the film, moving quickly to different parts of its circumference, sometimes dividing itself into two currents dancing opposite one another, and then extending into secondary currents in constant motion. Similar currents are produced upon various alcoholic solutions and a large number (seventy to eighty) of volatile oils, &c.

If we now examine the film by reflected light, the principle and secondary currents will be seen as before, but accompanied with systems of coloured rings of great beauty, shifting their place on the film, sometimes in rotation, expanding and contracting quickly, and changing their form and colour.

In small films there is often only one system of rings contracting and expanding with a constant variation of the central tint. In general, however, there are two, three, or several systems—each system being produced by a secondary current giving motion to the colouring matter on the surface of the film. In some cases the motions and colour disappear, the film becomes colourless, and tints issue from its margin as on the soap-bubble; but, in general, the film bursts before this takes place. The colourless currents and the colours into which they expand are supposed by the author to have the same origin as those upon the soap-bubble. The paper is illustrated with drawings of the currents and of the systems of rings.

3. Note on the Action of Nitric Oxide, Nitrous Acid, and Nitrites on Hæmoglobin. By Arthur Gamgee, M.D., F.R.S.E., F.C.S.

When engaged, during the course of last summer, in the examination of the action of various medicinal and poisonous agents on blood, I observed the remarkable change in the colour of the blood of animals poisoned with nitrite of amyl. In every case in which I induced death by this agent the blood presented a dirty chocolate coloration, instead of the characteristic colour of normal blood. When examined by means of the spectroscope, this blood was found to present optical characters very different from those of ordinary blood, for the two well-marked absorption bands of 0 – hæmoglobin, or (as the readers of Professor Stokes' paper will know them better as) of scarlet cruorine, were found to be extremely faint, and only visible when comparatively thick layers of fluid were examined; whilst an additional and very faint absorption band in the red, occupying the same position as that of acid hæmatine, was seen. I at first thought that the acids, generated by the oxidation of the nitrite, had acted upon the blood-colouring matter in the same manner as weak acids usually do, effecting a partial conversion into hæmatin, so that whilst the bands of hæmoglobin were yet visible, the one characteristic of hæmatin had been super-added. The total incorrectness of this view was proved on a more careful examination being made. On adding a few drops of ammonia to the blood-solution in which nitrite of amyl had effected the peculiar change alluded to, its colour again became red, and whilst the band in the red instantly and totally disappeared, the two absorption bands situated between D and E became much more distinct. Moreover, a third very faint absorption band was found occupying the junction of the orange and yellow, and just below Frauenhofer's line D. On now treating the blood solutions with such deoxidising agents as protosalts of iron and tin and sulphide of ammonium, a spectrum, differing in no respect from that of reduced hæmoglobin, was obtained, and the solution, when agitated with air, presented the most beautiful spectrum of oxy-hæmoglobin, *i.e.*, the two bands α and β reassumed all the distinctness and beauty which they presented in the unaltered blood solution.

Nitrite of ethyl was found to effect exactly the same alteration as nitrite of amyl, whilst, as might be expected, vinic and amylic alcohols, when added in small quantities to blood, were without any effect. When solutions of nitrite of sodium or of potassium were added to blood, instantly the same change was noticed as was produced by amylic and vinic nitrites. On very faintly acidulating blood with acetic acid, and subsequently adding a small quantity of a solution of nitrite of sodium, the same change occurred.

In this case the change must have been due to the nascent nitrous acid, or to the nitric oxide secondarily resulting from it. In order to endeavour to determine whether the body produced under the influence of nitrites was really due to the action of nitrous acid, or whether, on the contrary, it was formed through the agency of nitric oxide, I performed the following experiment. Perfectly pure nitric oxide was passed into blood, exactly the same arrangements being employed as were used by Hadow in his experiments on the constitution of nitro-prussides; instead, however, of generating the nitric oxide by the action of nitric oxide on copper, the gas was obtained by boiling a solution of ferric sulphate, which had been saturated with NΘ, in a stream of carbonic acid. After the action of nitric oxide had gone on for some seconds, the line in the red made its appearance, and the two hæmoglobin bands became very indistinct, whilst the colour of the blood became brown; the same changes were thus induced as were found to occur when nitrites act upon blood. On the addition of ammonia the band in the red disappeared, and the spectrum was marked by the band in the orange, to which allusion has already been made.

Although this observation appeared to point to nitric oxide as the agent in effecting the change, it was perfectly possible that, in reality, it was due to nitrous acid, for it appeared likely that this acid might be formed in the blood by the nitric oxide seizing that portion of the oxygen of the blood-colouring matter, which is capable of removal by reducing agents, or capable of expulsion by carbonic oxide. It would indeed appear from my experiments that nitrous acid is produced in the reaction, as when by means of a long funnel a mixture of potassic iodide, starch, and acetic acid is conducted to the bottom of a blood solution, and through which nitric oxide is passing, a blue colour is produced.

In order to separate, if possible, the substance formed under the influence of NΘ in a crystalline form, the blood of a dog was allowed to coagulate, and after complete separation of the serum the clot was squeezed in linen. The red fluid thus expressed was mixed with one-and-a-half times its volume of water, and then solution of nitrite of sodium added, until the characteristic spectrum was obtained. The fluid was then mixed with one-fourth of its volume of 86 per cent. alcohol, when it became filled with microscopic needles and prisms identical with those of 0 – hæmoglobin. They merely differed from the latter in having a dirty brown colour. They were collected on filter paper, washed with ice-cold water, pressed, and afterwards mixed with a considerable quantity of water. They did not, however, entirely redissolve, a small quantity of a white albuminoid substance remaining undissolved. The solution of the crystals presented the same spectrum as the blood from which they had been precipitated.

At the present stage of my researches it would be premature to express an opinion on the nature of the substance produced, when hæmoglobin is acted upon by nitrites and nitrous acid. That it holds a very close relation, indeed, to O – hæmoglobin, with which it appears to be isomorphous, is obvious, whilst it is most improbable that it has any analogy to the bodies formed when blood-colouring matter is treated with carbonic oxide, or when subjected to the prolonged action of nitric oxide, *in the presence of ammonia.* When CO acts upon blood, or, as Hermann has recently shown, when NΘ is made for a long time to pass through blood made alkaline with ammonia, there are formed compounds in which one volume of CO or one volume of NΘ have respectively replaced one volume of oxygen in the colouring matter—compounds which present exactly the same spectrum as ordinary blood, but which differ from it in being totally irreducible. That N_2O_3 should form a compound of this character seems improbable, and that NO should form two altogether distinct compounds with blood-colouring matter, is even more improbable. It is, however, much more likely that the action of nitrites and nitrous acid is mainly a reducing action, and that the body obtained by me is merely a less oxygenised form of blood-colouring matter.

4. Antiquities, Scenery, &c, of Cambodia, illustrated by a series of Photographs, shown by the Oxyhydrogen Light. By J. Thomson, Esq., F.R.G.S. Communicated by Professor Sir J. Y. Simpson, Bart.

The following Gentlemen were elected Fellows of the Society:—

James Donaldson, LL.D.
James Richardson, Esq.

The following Donations to the Library were announced:—

Transactions of the Linnean Society of London. Vol. XXV. Part 3. 4to.—*From the Society.*

List of the Linnean Society of London for 1866. 8vo.—*From the Society.*

Report of Proceedings of International Horticultural Exhibition and Botanical Congress, held in London from 22d to 31st May 1866. 8vo.—*From the Acting Committee.*

Madras Journal of Literature and Science for October 1866. 8vo. —*From the Society.*

Monday, 18th March 1867.

Sir DAVID BREWSTER, President, in the Chair.

The following Communications were read:—

1. On the Application of Relative, or Proportional, Equality to International Organisation. By Professor Lorimer.

It is often regarded almost as a matter of admission, on the part of the cultivators of social and political philosophy, that their subjects defy scientific treatment; and that, when they talk of tracing out laws of social wellbeing or progress, they use words which indicate, at most, a very faint analogy between the methods which they affect to follow, and those really employed in the physical sciences. The reproach to which they are exposed, on this ground, would never have arisen, or would not have been merited, had they habituated themselves and others to regard their subject, as a science, in the ordinary sense of an inquiry into nature, and not as a series of random observations, in which the contingent and the necessary, the permanent and the accidental, were hopelessly and inextricably mixed up. Political methodology, viewed as a branch of applied logic, has risen, in the hands of some of its recent cultivators, almost to the dignity of a separate science. By eliminating impossible schemes, and thus circumscribing the sphere of political effort, it has already given evidence of its practical value. Within the state, forms of government, after which the vulgar still aspire as the ideal forms of society, have been shown by its means to be permanently irreconcilable with order, and if with order, then with liberty, which is possible only through order, and so ultimately with civilisation itself.

But all that can be attempted on the present occasion, is to point out two principles, the realisation of which can be shown to be impossible, and which have, nevertheless, been sought to be realised in conjunction with every scheme of international organisation which has yet been propounded. These principles are :—

1. *Finality.*—In national politics this principle has been exhibited in those arbitrary, and, in some states, impassable lines between classes, which science has long ago condemned, and which practical men are everywhere engaged in obliterating. In schemes of international organisation, it has sought to manifest itself in the establishment of final and permanent international relations, or in the maintenance of what is technically called a *status quo.*

2. *Absolute equality of rights and obligations.*—In internal politics this principle is the basis of the form of government called Democracy. In international politics it has exhibited itself in the custom of assigning equal votes to all the members of the family of nations, not absolutely excluded from the European council board, however widely they might differ in real power and importance.

As regarded the first of these principles, the absurdity of attempting to stereotype the map of Europe will be readily admitted. With reference to the second, there is much difference of opinion, both as to the possibility and the justice of recognising, or declaring absolute political equality, whether within the state or in the wider commonwealth of nations. Now this diversity of opinion, great as it is, and terrible as have been, and may yet be its effects, is traceable to a defect in the popular mind, on which Aristotle, with his usual perspicacity, put his finger more than two thousand years ago. "The vulgar," he says, "do not distinguish," and in modern Europe, for nearly a century, they have lost sight of a distinction which Aristotle did them the farther favour to point out.

The distinction is that between *absolute and relative* or *proportional equality.*

The two are, in truth, neither more nor less than two different manifestations of the principle of justice. They differ not in themselves, but in the manner of their application, and in the subject matter with which they deal. Following, and giving definiteness, as usual, to Plato's conception of what, in its origin, was probably the teaching of Socrates, Aristotle gave to these two forms of applied justice the names of *diorthotic* and *dianemetic justice* (δίκαιον διορθωτικόν and δίκαιον διανεμητικόν), names which the schoolmen and jurists rendered by *justicia correctiva* or *commutativa*, and *justicia distributiva.* The object of diorthotic or corrective justice Aristotle explained to be, to give to each a perfectly fair, unbiassed, and, *in that sense, equal* opportunity of vindicating whatever might be due to him, whether the amount was greater or smaller than that which was due to his neighbour. This was what we call equality before the law; and justice demanded that equality, in this sense, should be *absolute.* There was to be no distinction whatever of rich or poor, wise or foolish, male or female, old or young. The object of dianemetic justice, on the other hand, was to ascertain *how much* was due to each, and to *rank* them accordingly. Here was still equality, *perfect* equality, but equality which was no longer *absolute*, but *relative.* It was *proportioned* to the facts which the claimants respectively established with reference to the matter in dispute. So far all is clear. There can be no doubt that this was what Aristotle meant, and as little doubt that he was right. As to the application of the doctrine there is great confusion in the text, and even Sir Alexander Grant has not made much of it. What it seems to indicate is, that diorthotic justice, or absolute equality, is applicable to private—and dianemetic justice, or relative equality, to public questions, *and both exclusively.* But this could scarcely have been Aristotle's meaning. Both principles come into play in every department of jurisprudence, and are called into action in the decision of every case, from the most insignificant question of private right to the most momentous questions of national or international policy. And the method of their action is this: The first principle—absolute equality—governs the conduct of the suit, or of the investigation, whatever form it may take, whether it be conducted for judicial, legislative, or social purposes. The second, relative equality, governs the decision of the cause, whether that decision be pronounced in a small-debt court, in a national legislature, in a congress of nations, or in a club of gossips. As an illustration of the mutual action of these principles in private law, take the familiar case of the distribution of a bankrupt estate. One man has invested L.5 in the concern, and another L.50. *As suitors*, the law puts them on a footing of *absolute* equality. No preference is given to Jew or to Gentile, to noble or to simple, to white skin or to black skin. So far they are dealt with diorthotically. But then the dianemetic, or distributive principle, comes into play;—and suppose the estate to yield a shilling in the pound, one man gets five shillings, and the other gets fifty. The *distribution* has reference to the *objects* of the suit, not to the suitors, and is wholly dianemetic. But so far is the dianemetic principle from acting alone, that it is in virtue of the diorthotic principle that it assigns fifty to a man who may possibly be a millionaire and a scoundrel, and five shillings to a man who may be a pauper and a saint! And just in the same way the presence of *both* principles is indispensable to the decision of questions of a public nature. There, too, justice demands that the dianemetic principle shall act diorthotically, and the diorthotic principle dianemetically. Suppose that the suffrage is claimed by a particular class of persons whose right to it has hitherto been ignored or denied. What justice entitles them to ask the State to do is, not to create *new* rights in their favour, but to recognise rights which they allege exist in their persons already. Here, too, they claim equality before the law—they demand a fair (*i.e.*, a diorthotic) hearing. But their plea is that they are entitled to the suffrage on some *ground*, as they call it,—property, education, a hearth and a chimney, perhaps a fire, if they have it, or simple humanity. Whatever the ground may be, they demand that it should be diorthotically inquired into. But they do not dispute, except by mistake, that it must be dianemetically recognised. Even if they ask the State to proceed on the *assumption* that they all possess the ground of recognition equally, they do not ask the State to *make* it equal to them all; in other words, they do not ask it to give them means to teach them, or to make them human beings, for that would be to ask not recognition, not equality before the law, but revolution, or the abolition of the law. It was Rousseau's doctrine that all men *ought* to be equal, and not Hobbes's doctrine that all men *are* equal, and ought, consequently, to be *recognised* as such, which brought about the revolutions of last century, and which threaten us still.

Keeping this distinction in view, then, it is obvious that, if justice is to be done between nations, there must, as in all other cases in which it is called into play, be proportion between the claimant and the thing granted to him (ὅις καὶ ἐν ὅις), in order that those who are unequal may not have equal things (μὴ ἴσοι οὐκ ἴσα ἴξο υσι) or the reverse. But since the Peace of Westphalia, and more especially since the American and French revolutions, the tendency in international, as in national politics, has been to ignore the proportional or dianemetic principle altogether, to give exclusive sway to the diorthotic principle, and to deal with all states either as internationally equal, or else as internationally non-existent.

As regards the future, then, the question, on which the possibility or impossibility of international organisation seems to turn is this,—Can we shadow forth a European or cosmopolitan constitution, self-sustaining and self-vindicating, which shall make provision for legitimate progress and righteous development, and for inevitable retrogression, whilst it takes cognizance of existing diversities of power? To anything approaching to a confederation, in the stricter sense of a single composite state, there is, I think, the objection which exists to all confederations, and of which we have just seen the consequences so terribly exhibited, first in America and then in Germany. In a confederation there are always two forces at work,—a centrifugal force and a centripetal force: the tendency of the first of which is to pull it to pieces, and the tendency of the second of which is to centralise it till it becomes a homogeneous State. A perfect and permanent balance between these forces I believe to be a practical impossibility; and for this reason I regard all confederations as transitional forms of government. I concur, therefore, in the latest opinion of Kant, whose great mind was much occupied with this great subject before it experienced the eclipse which darkened his last days—an opinion in which he was partially anticipated by Grotius—to the effect that it is to the creation, not of a confederation, but of a Permanent Congress of Nations, or International Parliament, that we must direct our endeavours. Such a Congress, I think, would obviate the errors I have indicated, and might possibly satisfy the great desideratum of a self-vindicating international legislature, if it were constituted in accordance with something approaching the following scheme:—1*st*, That its meetings should be annual, taking place in the autumn, between the sessions of the various national assemblies; and that the places of meeting should be Belgium and Switzerland alternately, or perhaps one of the Swiss Cantons—say Geneva—set apart as neutral European ground. 2*d*, That each State should be represented by two deputies, both of whom should be present at the meetings of the Congress, but one of whom only should be entitled to speak and to vote. 3*d*, That each State be entitled to vote in proportion to its real power and importance, *for the time being.* 4*th*, That in order to fix this proportion, it be the *first* business of each Congress to ascertain the relative importance of each State on the basis,—(*a*) of population; (*b*) of exports and imports; (*c*) of free revenue. 5*th*, That each State be entitled to propose and push to a vote any question of international politics in which it might be interested. 6*th*, That each State be bound to supply a contingent of men, or money, proportioned to the number of votes assigned to it, for the purpose of enforcing the decrees of the Congress, *by arms if necessary.* 7*th*, That the representatives of any State which should make war without the sanction of the Congress be excluded from its next meeting; and that the conduct of such State be judged of in their absence, on a written statement and oral hearing of counsel, by the representatives of the other States. 8*th*, That all purely national questions be excluded from the deliberations of the Congress; but that the Congress itself should determine whether any question brought before it were or were not of this kind. 9*th*, That civil wars, as opposed to rebellions, be within the jurisdiction of the Congress; the Congress itself being entitled to judge what internal commotions possess the character of civil wars. 10*th*, That all questions brought by individual States before the Congress be submitted to it by the representatives of such States,—first *scripto*, and then *viva voce.* 11*th*, That a Judicial Tribunal be constituted, to the decision of which it should be competent for the Congress to remit any matter which it conceived to admit, demand, or to admit of, judicial determination. 12*th*, That a final appeal should lie from this tribunal to the Congress itself, in a manner analogous to that in which the judgments of our supreme courts may be carried to the House of Lords. 13*th*, That the judges of this court be appointed by the Congress, each State voting in proportion to its real weight, ascertained as above. 14*th*, That the presidents, both of the Congress itself and of the judicial tribunal, be appointed or re-elected at each meeting of the Congress, but that the ordinary judges of the tribunal should hold their offices *ad vitam aut culpam.* 15*th*, That the presidents and judges, being officers of the Congress, be paid by the Congress; but that the representatives receive no remuneration, except such as should be granted them by their respective States.

16*th*, That the expenses of the Congress be defrayed by an international tax, to be fixed by the Congress; that the said tax be proportioned to the number of votes enjoyed for the previous year by each State, and be levied by the several States on their own inhabitants.

Many provisions of a more special kind would, of course, suggest themselves were the scheme to assume a practical shape; but the preceding will sufficiently indicate its general character. A body which should take cognizance of the real power and importance of its various members would have a very much better chance of being accepted in lieu of the verdict of battle than those of a body of which all the members voted equally. The chances would then be many that individual States would gain no more by fighting than by voting; and to assume that, in such circumstances, they would in general prefer to vote, is surely to credit them with no very wonderful measure either of humanity or of wisdom.

2. On the Sophists of the Fifth Century, B.C. By Professor Blackie.

The object of this paper was to controvert the views of Mr Grote as stated in his history of Greece and in his work on Plato. Professor Blackie, while admitting that the Sophists might have been somewhat hardly dealt with by certain extreme writers in modern times, and recognising gladly the view stated by Meiners and Hegel that their teaching was an important and necessary step in the intellectual development of Greece, nevertheless maintained that the current character of the Sophists, as handed down to us from ancient times, was in the main correct; and that all that could be said in defence of their teaching amounted to no more than a slight palliation of the charges brought against them by Plato and Aristophanes, not to an acquittal. The portrait of the Sophists left us by these two great writers was substantially correct; and, if any man considered their testimony unworthy of credit, it was amply confirmed in all points by Aristotle, and Xenophon, and Isocrates in passages of their works, which had been ably analysed by Mr Cope of Trinity College, Cambridge, but which Mr Grote had either overlooked altogether, or looked at with eyes of prejudice and obliquity. Professor Blackie concluded his paper with the following five points of the Sophistical doctrine, as he gathered it from Plato:—

1. General information and alert intelligence without a philosophical basis, or a scientific method of verification. 2. The art of public speaking, considered merely as an instrument of moving masses of ignorant men with a view to political advancement, and not necessarily connected either with noble motives, earnest purpose, or business habits. 3. The exercise of a dexterous logic, which aimed at the ingenious, the striking, and the plausible, rather than the true, the solid, and the judicious. 4. A theory of metaphysics, which, by confounding knowledge with sensation, and subordinating the general to the particular, made wisdom consist rather in the urgent use of present opportunity than in the moulding of materials according to an intellectual principle. 5. A theory of morals, which, by basing right on convention, not on nature, deprived our sensual and animal instincts of the strong control of reason, and substituted for the eternal instinct of justice in the human heart the arbitrary enactments of positive law, whose ultimate sanction is the intelligent selfishness of the individual.

3. An Account of some peculiarly-shaped Mineral Concretions found near Carlisle. By Henry Barnes, M.D. Communicated by Sir David Brewster.

I have very great pleasure in presenting to the Society's museum a few very peculiarly-shaped stones or mineral concretions found near Carlisle, similar in external characters to those fairy stones found in Elwand Water, near Melrose, and described at a meeting of this Society, held 5th February 1866, by Sir D. Brewster. These stones or nodules were found about twenty years ago embedded in marl in a deep cutting at Wreay, five miles south of Carlisle, during the formation of the Lancaster and Carlisle Railway. They were found in great numbers by the workmen, but only in this particular cutting, and excited a great deal of curiosity at the time, but were soon laid aside and forgotten; and it was not until the account of those fairy stones described by Sir D. Brewster appeared, that my esteemed relative, Dr T. Barnes, F.R.S.E., remembered that he had them in his possession, and asked me to present them to the Society.

They are all of a light colour, presenting a smooth surface, and inodorous when dry. When breathed upon they emit an argillaceous odour. They vary in weight from two to seven ounces.

Table of Weights.

6 oz. 6 drs.	3 oz.
6 oz. 3 drs.	2 oz. 2 drs.
3 oz. 3 drs.	1 oz. 7 drs.

On section they are seen to be perfectly homogeneous, presenting the same uniform structure throughout, showing that the nodules on the external surface are produced by some external agency acting on the surface of the stone. They are probably formed in the same way as the fairy stones, viz., by the dropping of water containing the matter of which they are composed; but must be of great age, as the cutting in which they were found is about one hundred yards from the bed of a river. The following are the geological characters of this part of Cumberland in which they occurred. It is situated on the New Red Sandstone, but at Coathill and Newbiggin gypsum is found lying in red argillaceous marl between two strata of sandstone, and at Wreay is the commencement of the whinstone dyke which extends from that place to Renwick.

4. Note on Professor Tait's "Quaternion Path" to Determinants of the Third Order. By the Rev. Hugh Martin, M.A. Communicated by Professor Kelland.

I have read with much interest Professor Tait's "Note on Determinants of the Third Order" in the *Proceedings* of this Session (pp. 59–61), and admire the method of discovering new properties of Determinants. I am not sure, however, that the properties, when discovered, are more difficult of proof by Determinant methods, and I venture to submit the following as simple and elementary:—

The *first* property, namely,

$$\begin{vmatrix} x+x_1 & y+y_1 & z+z_1 \\ x_1+x_2 & y_1+y_2 & z_1+z_2 \\ x_2+x & y_2+y & z_2+z \end{vmatrix} = 2 \begin{vmatrix} x & y & z \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}$$

is true under greater generality, and the Determinant proof is the same as for the special case.

Let

$$\left|\begin{array}{ccc} a_{1,1}+a_{1,2}+\ldots+a_{1,n-2}+a_{1,n-1}, & \ldots, & a_{n,1}+a_{n,2}+\ldots+a_{n,n-2}+a_{n,n-1} \\ a_{1,2}+a_{1,3}+\ldots+a_{1,n-1}+a_{1,n}, & \ldots, & a_{n,2}+a_{n,3}+\ldots+a_{n,n-1}+a_{n,n} \\ a_{1,3}+a_{1,4}+\ldots+a_{1,n}+a_{1,1}, & \ldots, & a_{n,3}+a_{n,4}+\ldots+a_{n,n}+a_{n,1} \\ \ldots\ldots\ldots, & \ldots, & \ldots\ldots\ldots \\ \ldots\ldots\ldots, & \ldots, & \ldots\ldots\ldots \\ \ldots\ldots\ldots, & \ldots, & \ldots\ldots\ldots \\ a_{1,n}+a_{1,1}+\ldots+a_{1,n-3}+a_{1,n-2}, & \ldots, & a_{n,n}+a_{n,1}+\ldots+a_{n,n-3}+a_{n,n-2} \end{array}\right| = \nabla$$

Add all the rows to form a new first row, namely,

$$(n-1).\overline{a_{1,1}+\ldots+a_{1,n}},\ \ldots,\ (n-1).\overline{a_{n,1}+\ldots+a_{n,n}}:$$

take the factor $(n-1)$ outside the determinant: subtract the resulting new first row from each of the others:

then add to it the sum of the others: and we have

$$\nabla = (n-1)\begin{vmatrix} a_{1,n} & a_{2,n} & \cdots & a_{n,n} \\ -a_{1,1} & -a_{2,1} & \cdots & -a_{n,1} \\ -a_{1,2} & -a_{2,2} & \cdots & -a_{n,2} \\ \cdots & \cdots & \cdots & \cdots \\ \cdots & \cdots & \cdots & \cdots \\ -a_{1,n-1}, & -a_{2,n-1} & \cdots & -a_{n,n-1} \end{vmatrix}$$

The negative signs may be removed and compensated for by the factor $(-1)^{n-1}$ placed outside the Determinant, and the first row may be made the last by multiplying again by $(-1)^{n-1}$, which counteracts the former multiplication. Hence

$$\nabla = (n-1)\begin{vmatrix} a_{1,1} & a_{2,1} & \cdots & a_{n,1} \\ a_{1,2} & a_{2,2} & \cdots & a_{n,2} \\ \cdots & \cdots & \cdots & \cdots \\ a_{1,n} & a_{2,n} & \cdots & a_{n,n} \end{vmatrix} \qquad \text{Q. E. D.}$$

When $n = 3$, this is the quaternion theorem in question.

The *second* formula is the well-known property of the reciprocal, and a simple proof of it will be given below.

The *third* and *fourth* are immediate translations of Quaternion expressions into Determinant forms. And I suppose there is no analytical difficulty in enlarging the number of Sir William R. Hamilton's symbols, *i*, *j*, *k* (though it would introduce the conception of ideal space of more dimensions than three), and thus extending the reach of the "Quaternion path" to Determinants of any order.

The *fifth* formula, in its algebraical expression, would be

$$\begin{vmatrix} \begin{vmatrix} y & z \\ y_1 & z_1 \end{vmatrix} + \begin{vmatrix} y_1 & z_1 \\ y_2 & z_2 \end{vmatrix}, & \begin{vmatrix} z & x \\ z_1 & x_1 \end{vmatrix} + \begin{vmatrix} z_1 & x_1 \\ z_2 & x_2 \end{vmatrix}, & \&c. \\ \begin{vmatrix} y_1 & z_1 \\ y_2 & z_2 \end{vmatrix} + \begin{vmatrix} y_2 & z_2 \\ y & z \end{vmatrix}, & \&c., & \&c. \\ \begin{vmatrix} y_2 & z_2 \\ y & z \end{vmatrix} + \begin{vmatrix} y & z \\ y_1 & z_1 \end{vmatrix}, & \&c., & \&c. \end{vmatrix} = -2\begin{vmatrix} x & y & z \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}^2$$

Now, using capitals for minors we have, by the first property,

$$\begin{vmatrix} X_2+X & Y_2+Y & Z_2+Z \\ X+X_1 & Y+Y_1 & Z+Z_1 \\ X_1+X_2 & Y_1+Y_2 & Z+Z_2 \end{vmatrix} = 2\begin{vmatrix} X & Y & Z \\ X_2 & Y_2 & Z_2 \\ X_1 & Y_1 & Z_1 \end{vmatrix}$$

$$= -2\begin{vmatrix} X & Y & Z \\ X_1 & Y_1 & Z_1 \\ X_2 & Y_2 & Z_2 \end{vmatrix} = -2\begin{vmatrix} x & y & z \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}^2 \quad \text{Q. E. D.}$$

This, of course, may be generalised like the *first* formula; and the result is

$$\begin{vmatrix} A_{1,n} & +A_{1,1}+A_{1,2}+ & . & +A_{1,n-2}, \&c., \&c. \\ A_{1,1} & +A_{1,2}+A_{1,3}+ & . & +A_{1,n-1}, \&c., \&c. \\ A_{1,2} & +A_{1,3}+A_{1,4}+ & . & +A_{1,1}, \&c., \&c. \\ \cdots & \cdots & \cdots & \cdots \\ A_{1,n-1} & +A_{1,n}+A_{1,1}+ & . & +A_{1,n-3}, \&c., \&c. \end{vmatrix}$$

$$= -(n-1)\begin{vmatrix} a_{1,1} & . & . & . & a_{1,n} \\ . & . & . & . & . \\ . & . & . & . & . \\ a_{1,n} & . & . & . & a_{n,n} \end{vmatrix}^{n-1}$$

The *sixth* formula is got by a reduplication of the proof of the *fifth*, whether the method used be Quaternion or Determinant.

As to the reciprocal, the following seems quite elementary:—

Let

$$\begin{vmatrix} yz_1-zy_1 & zx_1-xz_1 & xy_1-yx_1 \\ y_1z_2-z_1y_2 & z_1x_2-x_2z_1 & x_1y_2-y_1x_2 \\ y_2z-z_2y & z_2x-xz_2 & x_2y-y_2x \end{vmatrix} = \nabla$$

Multiply the first row by x_2, the second by x, the third by x_1; add all the rows for a new first row; divide the second row by x, and the third by x_1.

$$\begin{vmatrix} \begin{vmatrix} x & y & z \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix} & 0 & 0 \\ X & Y & Z \\ X_1 & Y_1 & Z_1 \end{vmatrix} = x_2\nabla$$

Hence

$$\begin{vmatrix} x & y & z \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix} \begin{vmatrix} Y & Z \\ Y_1 & Z_1 \end{vmatrix} = x_2 \nabla$$

But it is at once seen that

$$\begin{vmatrix} Y & Z \\ Y_1 & Z_1 \end{vmatrix} = x_2 \begin{vmatrix} x & y & z \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}$$

Hence

$$\nabla = \begin{vmatrix} x & y & z \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}^2 \qquad \text{Q. E. D.}$$

The following Gentleman was elected a Fellow of the Society :—

JAMES H. B. HALLEN, Esq.

The following Donations to the Library were announced:—

Transactions of the Highland and Agricultural Society of Scotland. Vol. I. No. 2. Aberdeen, 1867. 8vo.—*From the Society.*

Transactions of the Royal Society of Literature, London. Vol. VIII. Part 3. 8vo.—*From the Society.*

Journal of the Scottish Meteorological Society for January 1867. Edinburgh, 8vo.—*From the Society.*

Journal of the Chemical Society of London for March 1867. 8vo. —*From the Society.*

Proceedings of the Meteorological Society of London. Vol. III. No. 27. 8vo.—*From the Society.*

Proceedings of the Royal Horticultural Society of London. Vol. I. New Series, No. 7. 8vo.—*From the Society.*

Monthly Notices of the Royal Astronomical Society, London. Vol. XXVII. No. 4. 8vo.—*From the Society.*

Proceedings of the Royal Geographical Society, London. Vol. XI. No. 1. 8vo.—*From the Society.*

Proceedings of the Royal Society, London. Vol. XV. No. 90. 8vo.—*From the Society.*

Proceedings of the Royal Medical and Chirurgical Society of London. Vol. V. No. 6. 8vo.—*From the Society.*

Monthly Return of Births, Deaths, and Marriages Registered in the eight Principal Towns of Scotland, January 1867. 8vo.—*From the Registrar-General.*

Quarterly Return of Births, Deaths, and Marriages Registered in the Divisions, Counties, and Districts of Scotland. No. 48. 8vo.—*From the Registrar-General.*

American Journal of Science and Arts. Second Series. No. 127. New Haven, 1867. 8vo.—*From the Editors.*

Notes on the Great Ganges Canal. By T. Login, Esq., C.E. Roorkee, 1867. 8vo.—*From the Author.*

Roads, Railways, and Canals for India. By T. Login, Esq., C.E. Roorkee, 1866. 8vo.—*From the Author.*

Historical Sketch and Laws of the Royal College of Physicians of Edinburgh, from its Institution to December 1865. Edinburgh, 1867. 4to.—*From the College.*

Bulletin de l'Académie Royale des Sciences, des Lettres et des Beaux-Arts de Belgique. Tome XXIII. No. 2. Bruxelles, 1867. 8vo.—*From the Academy.*

Monatsbericht der Königlich Preussischen Akadémie der Wissenschaften zu Berlin, November 1866. Berlin, 1867. 8vo.—*From the Academy.*

Sveriges Geologiska Undersökning, pa offentlig bekostnad utförd under ledning af A. Erdmann. Liv. 17–21 (with maps). Stockholm, 1866.—*From the Geological Commission of Sweden.*

Nova Acta Regiæ Societatis Scientiarum Upsaliensis. Vol. VI. Fascs. 1. Upsala, 1866. 4to.—*From the Society.*

Monday, 1*st April* 1867.

Sir DAVID BREWSTER, President, in the Chair.

The following Communications were read:—

1. On the Analysis of Compound Ethers. By W. Dittmar.

In many cases the best guarantee of the purity of a chemical substance is the method by which it has been prepared. If the processes are well devised and carefully conducted, they constitute in themselves a qualitative analysis on a large scale, the exactitude and reliability of which go far beyond what could be obtained by ordinary testing. This, however, does not apply to many organic preparations, and is eminently inapplicable in the case of compound ethers. If in the preparation of such an ether, we start with a pure alcohol and a pure acid, and carefully distil, wash, and redistil the ether produced, the product ultimately obtained may contain considerable quantities of alcohol, free acid, and water. The purity of a compound ether can be established only by direct experiment. Free acid is easily tested for, and even quantitatively determined by the well-known acidimetric processes; the presence or absence of water may be proved by an elementary analysis, but an admixture of alcohol is not easily established. An elementary analysis would, in a majority of cases, show very little, because the difference between the percentages of carbon in the ether and alcohol respectively, is usually too small to admit of basing upon a small fraction of that difference anything like an exact calculation. Thus, for instance, acetate of ethyl contains 54·54 per cent. of carbon, and alcohol 52·18 %—the difference is 2·36; a decrease of 0·1 % in the carbon would therefore correspond to an admixture of about 4 per cent. of alcohol.

Many years ago F. Mohr proposed a method for quantitatively testing the officinal "æther aceticus," which consisted in heating the ether in a closed vessel, with an excess of standard alkali, and determining the amount of alkali saturated. The method was in 1863 extended and improved upon by Berthelot, who seems to have employed it extensively in his researches. The standard alkali used by Berthelot was caustic baryta, which he heated with the ether under examination in a glass tube. On attempting some time ago to employ Berthelot's method for testing, what I had good reason to look upon as very pure formiate of ethyl, I was surprised by obtaining perfectly inexplicable results; and on subjecting his process to a critical examination, I found that the discrepancies in my results arose from the action of the baryta solution on glass. When baryta water was heated to 120° C. in sealed-up tubes, and kept at that temperature for about twenty-four hours, the glass was found to be strongly attacked, and the titre of the baryta solution considerably altered. How Berthelot managed to obtain exact results is more than I can explain. Seeing that baryta was not a reliable reagent for the titration of ethers, I tried a solution of carbonate of sodium, hoping that it would not appreciably act on hard glass, but without success.

A solution of the carbonate, when heated in a sealed-up tube, took up enough of alkali from the glass for its titre being increased by about $\frac{1}{10}$th. At last I found that, of all the available alkaline solutions, caustic ammonia was the only one which could be safely heated in sealed-up glass tubes. In order to see now if ammonia also was capable of completely decomposing compound ethers, a series of experiments was instituted, in which measured quantities of standard ammonia (containing about 17 grms. NH_3 per litre) were heated with acid acetate of ethyl, containing a known quantity of acetic acid. Such mixtures were procured by heating to 130°—140° C. &c., in sealed-up tubes, weighed quantities of glacial acetic acid, with a large excess of alcohol, a direct experiment having shown that, after a few hours heating, 60 °/o of the acid employed was etherified. The amount of ammonia saturated by the acid, after it had passed into the state of ether, was compared with that which the same quantity required when mixed directly with the alkali. Of the results obtained in this manner some came surprisingly near the truth, others differed by a per cent., and some even more; this I can only explain by the fact well known to chemists who are familiar with titrimetric analysis, that in the saturation of a weak acid with ammonia the point of neutrality is not by any means well marked. It may be, also, that in some cases the ethers had not been completely decomposed. Although the conditions under which this method would be sure to give exact results might, perhaps, be established by an extended investigation, I preferred to go back to using baryta-water, as recommended by Berthelot, substituting only for the sealed glass tubes a copper digestor, soldered with sterling silver, and provided with a conical copper stopper, which could be securely fastened by means of a screw. The stopper kept perfectly tight, even under a pressure of two atmospheres, as was shown by charging the digestor with water, weighing the arrangement, keeping it at 120° C. for about four hours, and weighing again. There was not any loss of weight observable by a balance turning with two centigrammes.

The liquid obtained by heating in the digestor a mixture of acetate of ethyl and an excess of baryta-water, did not contain any appreciable trace of copper. After these facts had been established, a number of experiments were made similar to those carried out for testing the ammonia method. The amount of baryta taken up by a weighed quantity of partially ætherified acetic acid was found to be about $\frac{1}{80}$th to $\frac{1}{100}$th less that what the same quantity of acid would have saturated when directly mixed with the standard alkali. This must be looked upon as a pretty satisfactory approximation in experiments of this kind. The method would, no doubt, give much more exact results if the excess of baryta was not titrated directly, but first precipitated by carbonic acid, and then alkalimetrically determined. The undecidedness of the end-reaction observed in titrating a weak organic acid with an alkali would thus be completely eliminated. I am now occupied with experiments in this direction.

It was only after the greater part of the facts communicated in this notice had been established that I first saw Mr Wanklyn's paper "On the Titration of Ethers" in the Chemical Society's Journal. Mr Wanklyn uses alcoholic potash for decomposing the ethers, and obtains very satisfactory results. But he extended his experiments to such substances only which can be safely heated in an open flask, and which are easily decomposed by a short digestion with alcoholic potash. When alcoholic potash is heated to 100° C. in sealed glass tubes for about twenty-four hours, the glass is so strongly attacked that there is a regular layer of silica formed.

I cannot conclude without acknowledging the great assistance which my friend Mr Cranston gave me in carrying out my experiments.

2. On a Derivative of Meconic Acid. By Messrs James Dewar and William Dittmar.

The meconic acid group of substances has always been regarded as peculiarly interesting. Situated, as it is, on the ill-defined limits of the fatty and aromatic compounds, and exhibiting interesting isomeric relations with other organic substances, the structure and derivatives of the whole group afford ample scope for theoretical speculation. Meconic acid has always been considered a tribasic acid, which, by the successive elimination of two molecules of carbonic anhydride, yields first a dibasic and then a monobasic acid. But chemists now know that the basicity of an acid may be less than its atomicity, and that even the former cannot be deduced from the composition of one or two isolated salts, but only from a comprehensive view of all the salts and ethers of the acid. As now a general review of the salts and ethers of meconic acid seems to point to an acid with an atomicity greater than its basicity, and as any additional proof of this constitution would help to explain many abnormal decompositions in the series, the authors have investigated the action of the general reagents on these compounds that enable us to distinguish between the basic and alcoholic hydroxyls. In this paper we communicate to the Society some of the results obtained in the course of our experiments, as a recent paper, by Baron von Crofft, on the action of nascent hydrogen on the members of this group, shows that other chemists are working in the same direction. The introduction of hydriodic acid and pentachloride of phosphorus as general reagents in organic chemistry has been productive of the happiest results, through the easy separation of the water residue, HO. By their use we have obtained a very complete knowledge of the relation of acids, alcohols, and hydrocarbons, and we now await the discovery of some reagent that will separate the ammonia residues with as great ease, to obtain a more precise knowledge of the structure of the organic alkaloids.

When meconic acid is treated with hydriodic acid of constant boiling point at 150° C., carbonic anhydride is evolved, and large quantities of free iodine liberated. No definite compound could be separated from the products of this decomposition. As the first action of the hydriodic acid was to form comenic acid, and as many actions succeed with concentrated hydriodic acid that are unmanageable with the dilute, we attempted the reduction by the action of a saturated solution of hydriodic acid on comenic acid. As Berthelot has lately shown that the action of concentrated hydriodic acid, at high temperatures, on mono- and bi-basic acids of the fatty series, regenerates the hydrocarbon from which they were derived, the formation of a hydrocarbon in this reaction was not impossible.

But the effect of hydriodic acid on this group is totally different from its action on the fatty series. If comenic acid and concentrated hydriodic acid are heated to upwards of 200° C., large quantities of free iodine separate out, mixed with a considerable quantity of free carbon. Hydriodic acid, therefore, is inapplicable as a reducing agent on these compounds.

Seeing that no definite compound could be obtained by the agency of hydriodic acid, the action of pentachloride of phosphorus did not look very promising. Under the influence of this reagent, however, the acid sustained a perfectly regular decomposition, and did not break up in the way one would have anticipated from its general instability in presence of powerful reagents. If meconic acid is treated with three molecules of pentachloride of phosphorus, hydrochloric acid is slowly given off in the cold; and if the retort containing the substance be heated to 100° C., the reaction takes place with great rapidity, torrents of hydrochloric acid are evolved, and oxychloride of phosphorus (PCl_3O) distilled over. In order to separate the oxychloride of phosphorus, the whole was heated for some hours to 130° C. in a current of dry carbonic anhydride. Terchloride of meconyl remained as an intensely red, non-volatile, semisolid substance, fuming in air, and decomposible by water, with the formation of an intensely dark-brown liquid. Several organic analyses of this substance were made, but as it was found impossible to free it from all traces of phosphorus, perfectly agreeing results could not be obtained.

	Found.	The formula $C_7HO_4Cl_3$ requires
Carbon, ...	31·5	32·9
Hydrogen, ...	0·7	0·4
Chlorine, ...	44·5	41·6

As analysis did not give a satisfactory proof of the carbon group remaining entire in this reaction, a direct experiment was made with the view of estimating the amount of carbonic anhydride evolved. For this purpose a special apparatus was devised, by means of which the amount of any permanent gas produced during the reaction could be estimated. To effect this, the retort containing the substance was connected with a tubulated receiver, to condense the oxychloride of phosphorus, and this joined to a small gasometer containing a saturated solution of chloride of sodium.

During the action we did not observe any gas collecting in the receiver. After the reaction the gases in the retort and condenser were swept into the gasometer by a current of dry hydrogen. Any hydrochloric acid gas was immediately absorbed by the water, and the residual gases were then made to pass through a series of U tubes filled with soda-lime, in order to estimate the carbonic anhydride by weight. Only a trace of carbonic anhydride could be detected; nothing like a molecule was evolved.

When the terchloride of meconyl was treated with water, hydrochloric acid was produced with much frothing, and the whole dissolved to an intensely brown liquid. When this action took place in the apparatus formerly described, large quantities of carbonic anhydride could easily be detected. Many attempts were made to produce definite salts of the acid formed in this reaction, but without success. When the acid liquid was thrown upon a dialyser, the whole of the hydrochloric acid diffused out; a dark brown solution of the acid remained. This acid solution, when boiled with strong nitric acid, in presence of nitrate of silver, produced chloride of silver, and is, therefore, a chlorinated compound. Although the acid is a lower derivative of meconic acid, the presence of chlorine shows that the three atoms of chlorine replacing the water-residues are not retained with equal degrees of force. The dark brown solution was completely decolourised by the action of sodium amalgam.

When comenic acid was treated with pentachloride of phosphorus, a chloride more intractable than that formerly described was obtained.

As pyromeconic acid contains the simplest form of the characteristic nucleus of the whole group, a knowledge of the structure of this substance would throw great light on the constitution of the meconic acid series. If a careful examination be made of the general properties of pyromeconic acid, we can hardly characterise it as a real acid. No doubt salts can be formed, but with great difficulty; and the fact of it not combining with the alkalies or alcohol radicals, shows that it bears a great analogy to pyrogallic acid. If we compare the formulæ of pyromeconic and pyrogallic acids, they would appear to be members of a homologous series; and as we know by synthesis that pyrogallic acid is trihydroxyl-benzol, the pyromeconic acid may be looked upon as bearing the same relation to a hydrocarbon, C_8H_4, below benzol. From our general knowledge of this class of compounds we would expect the formation, by the action of pentachloride of phosphorus, of a chloride of a hydrocarbon. We investigated this reaction on the small quantity of pyromeconic acid we had at our disposal. When pyromeconic acid, in presence of oxychloride of phosphorus, was heated in a sealed tube to 200° C. with an excess of pentachloride of phosphorus, and the result of the action treated with water, a brown oil separated out, which, when distilled, yielded a colourless oil that boiled above 200° C. This substance did not fume in the air, and remained in water without dissolving for days. The analysis gave somewhat anomalous results, the percentages of its constituents agreeing very exactly with the formula $C_{10}H_5Cl_9O_2$. This highly chlorinated substance seems to approach nearly to the chloride of a hydrocarbon.

Unfortunately, pyromeconic acid is a substance which cannot be easily investigated, as it is extremely difficult to procure in any quantity. In the course of our experiments we obtained only one per cent. of pyromeconic from meconic acid, so that until a better process for preparing this substance is discovered, it must remain an open question whether pyromeconic acid is a lower homologue of pyrogallic acid.

3. On the Action of Ammonia on Dichloracetone. By Dr A. Crum Brown.

This paper contains some further details of the properties and composition of the substance mentioned in a former note (*Proceedings*, vi. 75).

The substance is produced by the action of dry ammonia gas on dichloracetone. A low temperature favours its formation. It forms colourless needles, generally radiating from central points. It is extremely unstable, and can only be preserved in an atmosphere of dry ammonia. In *vacuo* over sulphuric acid, even at a temperature of 0° C., it decomposes into ammonia, dichloracetone, and a viscous substance, which has a smell recalling that of acetamide. The same decomposition takes place in dry air. Hydrochloric acid converts it into dichloracetone and chloride of ammonium. It may be fused in an atmosphere of dry ammonia, and sublimes readily at temperatures far below its fusing point. The point of fusion was found to be about 45° C. When exposed to a slightly higher temperature it boils with partial decomposition, leaving a slight trace of chloride of ammonium. Owing to the readiness with which it decomposes, great difficulties were experienced in its analysis, and the results have hitherto been somewhat discordant. They lead to the formula, $C_3H_{10}Cl_2N_2O$, or $C_3H_4Cl_2O + 2NH_3$, and it is possible that it may be the representative in the acetic series of the hydrochlorate of flavine.

The author is at present engaged in the further examination of this substance and the products of its decomposition.

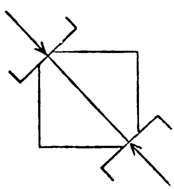
4. On Centres, Faisceaux, and Envelopes of Homology. By Rev. Hugh Martin, M.A. Communicated by Professor Kelland.

5. On the Effect of Pressure upon Rock Salt and Iceland Spar. By M. Reusch, Professor of Natural Philosophy in the University of Tubingen. Communicated by Sir David Brewster.

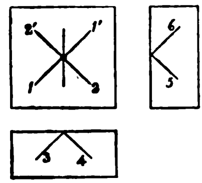
SIR DAVID BREWSTER,
à Edinburgh.

MONSIEUR,—Les semaines passées j'âi trouvé quelques faits dans le sel gemme et le spath d'Islande, que je prends la liberté de vous communiquer, parceque je m'imagine, qu'il n'y a personne au monde, qui saurait mieux apprécier la valeur des petites choses qui se sont offertes à moi.

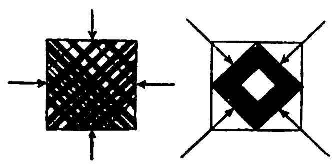
Dans le sel gemme il y a un clivage ou passage multiple, omis à tort par les minéralogistes, qui jouit des propriétès singulières

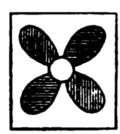


et qui jetera peut être quelque lumière sur le phénomène, de la "polarisation lamellaire." Ce passage suit les faces du *granatoëdre* ou dodecaëdre régulier. En pressant un morceau carré, dont deux arètes opposées sont enlevées par la lime, on obtient par une pression modique un passage brillant suivant la diagonale. Mais on peut avoir les six passages dodecaëdriques tous à la fois en posant un poinçon conique sur le milieu d'une plaque, et en donnant dessus un petit coup d'un marteau leger. Vous trouverez au point frappé une étoile à huit bras, dont quatre tiennent au clivage ordinaire, 1 et 2 au clivage dodecaëdrique ; et en regardant par les faces latérales vous trouverez les autres passages 3, 4, 5, 6. Ce petit coup a de plus déterminé suivant les six plans dodecaëdriques, qui passent par le point frappé, des *condensations permanentes*, cause de phénomènes de polarisation bien connus par vous (" Treatise on Optics," page 281, 282). Les six directions, suivant lesquelles les condensations se font avec la plus grande facilité paraissent être parallèles aux grandes diagonales des rhombes du dodecaëdre.



En pressant des pièces convenables sur les faces du cube ou mieux suivant quatre faces limées suivant le granatoëdre, on obtient dans la lumière polarisée des phénomènes qui rapellent, quoique imparfaitement, ceux de la polarisation, lamellaire, car jamais on ne réussira à réaliser un état de compression uniforme, indispensable pour avoir les effets de l'alun. La meilleure expérience, que j'aie pu trouver est la suivante : on perce un trou de quelques millimètres dans une plaque carrée, en tournant le foret (a métal ) doucement entre les doigts. Avec la plaque sensible de gÿpse on a une très jolie fleur, due aux condensations prédominantes suivant les diagonales.





Sur les faces octaëdrique du spath fluor, attaquées par le poinçon, j'ai plusieurs fois observé des clivages normaux aux cotés du triangle isocèle, qui correspondent aux surfaces du granatoëdre. Sur l'alun, dont je n'avais que de très mauvais échantillons, je n'ai encore rien vu de distinct, mais je ne manquerai pas de poursuivre mes recherches. En attendant je suis porter à soupçonner, que la " polarisation lamellaire" pourrait être due aux condensations permanentes et uniformes suivant les passages dodecaëdriques, les pressions étant dues ou à des forces extérieures, ou à l'act de la cristallisation même. De cette manière je ne pense pas être en désaccord avec les vues, que vous vous êtes formées sur la véritable cause de ces phénomènes.

Quant au spath d'Islande je prends la liberté de vous rappeler vos observations sur cette substance (Treat. on Optics, pp. 254–256, et pp. 349–351); de plus je vous prierai de jeter un coup d'oeil sur un mémoire de Fr. Pfaff à Erlangen, dans Poggendorff's Annalen, band 107, p. 336, et band 108, p. 599. En rapprochant ces deux choses on est poussé vers ce résultat singulier, qu'il doit être possible, *de faire naitre des " twin-films,"* dans ce cristal par une pression suffisante. Or c'est ce que j'ai fait par douzaines de fois dans le dernier temps. On prend un prisme de spath, rhombique ou rhomboidique, on y lime des surfaces planes parallèles et normales aux arètes longues du prisme, on y colle des petits cartons et l'on met le prisme dans une presse à machoires parallèles. En pressant doucement vous aurez un à trois passages, correspondant au rhomboëdre obtus, et ces passages ne sont pas ordinairement des clivages, mais de véritables " twin films." Ce fait singulier m'a rappelé l'albite, le labrador, qui sont si terriblement maclès (twin films) et pourrait donner quelques indication sur la cause de la formation de differents twin-cristaux dans la nature. Quant aux twin-films du spath d'Islande je suis persuadé, qu'il sont tous dûs à une pression extérieure soit dans le lieu de leur naissance, soit dans les mains de celui qui maltraite les cristaux en les clivant.

Voilà Monsieur! ce que j'ai voulu vous communiquer. Ce sera sans doute peu de chose dans vos yeux, qui ont tant vu; mais dépuis long temps j'ai suivi vos traces,* et étudié vos travaux, et toujours, j'ai songè à vous prèsenter quelque chose, qui fût digne de Vous. Puissé-je avoir réussi!—Agréez Monsieur l'assurance de ma plas haute considération, votre très humble serv.

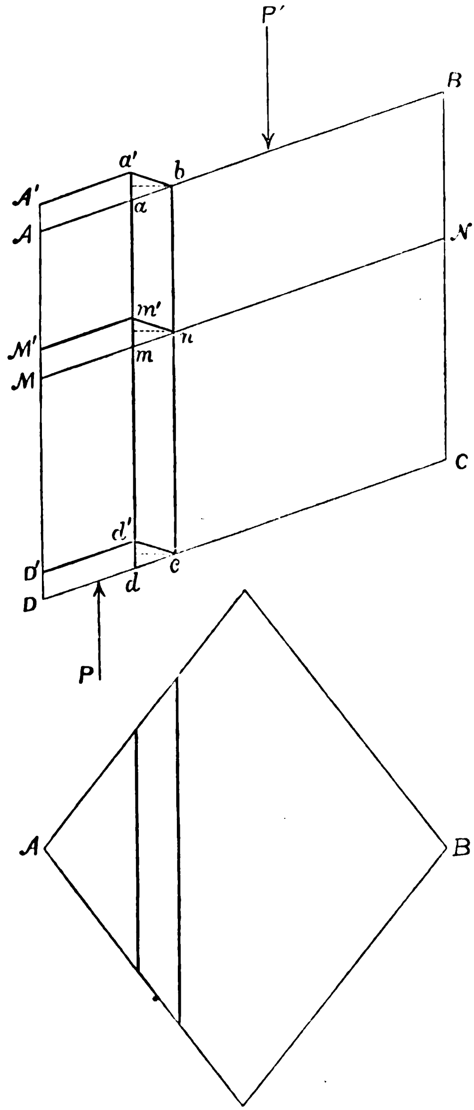
Reusch,
Prof. der Physik an der Universität.

Tübingen (Würtemberg),
18 *März* 1867.

* Poggendorff's Annalen, *Ueber das Schillern gewisser Krÿstalle*, band 116, p. 392; band 118, p. 256; band 120, p. 95. *Ueber den Agat*, band 123, p. 49. *Ueber den Hÿdrophan*, band 124, p. 431.

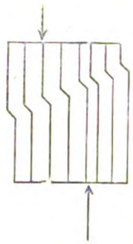
TÜBINGEN, 27 *März* 1867.

MONSIEUR,—Je m'empresse à repondre aux observations justes, dont vous avez daigné ma prèmière communication.



Quant à la production des "twin films" dans le spath d'Islande, je tache de l'expliquer de la manière suivante: soit ABCD la section principale d'un rhomdoëdre de spath, qui est soumis à des pressions parallèles PP′ non directement opposées; il y aura dans une certaine partie *abcd* une tendance de glissement, qui finira par amener toutes les molécules *ab*, *mn*, *dc*, dans les nouvelles positions d'équilibre *a′b*, *m′n*, *d′c*, de sorte qu'une série de molecules, situées d'abord sur la ligne M*mn*N, se trouve transportée sur la ligne brisée M′*m′n*N. En rêalitê les parties *a′b*, *d′c*, sont souvent tellement bien planes, qu'elles donnent des images mésurables au goniomètre.

Si vous pressez un petit morceau de bois suivant les fibres, vous avez un phénomène analogue, parceque jamais la pression ne sera également répartie sur les faces pressées.



Or, je dis que la lame *a′bcd′* est hémitrope, car en la rétournant autour d'une normale de 180° elle viendra se replacer dans la position primitive *abcd* (Miller, *Treat. on Crist.*, chap. viii. 230.) La manière, dont les minéralogistes expliquent les "twin crystals" n'est qu'une fiction géométrique, qui malheureusement n'a aucun rapport avec la structure intime des cristaux. Le sel gemme et le spath sont peut-être appelés á donner quelque lumière sur la merveilleuse charpente des cristaux.

Ce n'est pas à tort que le mot *passages* Vous a choqué. En français je n'avais pas d'autre mot à ma disposition; en allemand je les appelle "Gleitfläche" (*sliding plane*), car cette nouvelle espèce de clivage est réellement très différente des clivages ordinaires. Il est vrai, qu'une pression suffisante et poussée jusque à la séparation des molécules, peut produire des clivages miroitants dans le sel gemme et le spath; mais ce qui se passe dans les "sliding planes" *avant la rupture*, me parait être tout autre chose que ce qui s'opère dans un clivage ordinaire. Je soupçonne, que suivant ces plans, qui sans doute existent dans tous les cristaux, les molécules sont singuliérement disposées, non à se séparer, mais à prendre dans des directions déterminées des nouvelles positions d'équilibre sous l'influence de pressions extérieures, et cela sans solution de continuité. Voilà un problème qu'il s'agit d'approfondir et auquel je crois a peine avoir touché.

La preuve de ce que les lames, dues à la pression, ne sont pas des clivages, mais bien des véritables twin-lames, est très simple.

1°. En observant dans une telle lame l'image réfléchie d'une ligne horizontale, la reflexion s'opérant dans un plan vertical, normal à l'arête obtuse du rhomboëdre, vous verrez la même bifurcation de la ligne observée, que dans une twin-lame naturelle. De plus vous aurez les quatre images d'une flamme, dont deux ordinairement sont colorées (votre Treat. on Opt. p. 350, 351.)

2°. Vous taillez des plans normaux à l'axe dans une pièce, qui par la pression a gagnè une twin-lame, ou vous pressez la pièce après la coupe (expérience de Fr. Pfaff), et vous verrez entre les tourmalines les mêmes phénomènes, que dans un morceau taillè, possédant une lame naturelle (votre Optics, p. 254.)

J'espére Monsieur! que cette exposition sera propre à dissiper quelques doutes et objections, que ma première lettre pourrait avoir laissées dans votre esprit.

Il-y-a dèja quelques semaines, que j'ai communiqué mes observations aux Messieurs Dove et G.Rose à Berlin, pour étre présentées par eux à l'academie. Il parait que ces savans distingués mettent beaucoup de temps à examiner sérieusement mes résultats; car je suis encore sans reponse, mais bien sûr d'en avoir avec le temps. C'est pourquoi l'intéret, avec lequel le Nestor des Phÿsicians a bien voulu recevoir ma communication, m'a été d'une satisfaction toute particulière. Monsieur, je vous remercie beaucoup de votre bienveillance et je vous prie, de faire tout ce que vous jugerez à propos pour publier les petites choses, que j'ai mises dans vos mains. Les deux "brief abstracts" hautement intéressants que vous avez eu la bontè de m'envoyer, m'ont fait un grand plaisir; je ne manquerai pas de répéter vos expériences, Les memoires mêmes se trouveront sans doute dans les Transactions d'Edinburgh.—Agreéz Monsieur, l'assurance, de mon plus haut éstime, votre très devoué,

REUSCH.

Sir DAVID BREWSTER, Edinburgh.

The following Gentleman was admitted a Fellow of the Society:—

HENRY DIRCKS, Esq., C.E., F.C.S.

The following Donations to the Society were announced:—

Journal of the Asiatic Society of Bengal. Part 2. 1866. Special No. Ethnology. Calcutta, 1866. 8vo.—*From the Society.*

Proceedings of the Royal Irish Academy. Vol. IX. Part 4. Dublin, 1867. 8vo.—*From the Academy.*

Transactions of the Royal Irish Academy. Vol. XXIV. Parts 7, 8. (Science.) Dublin, 1866-67. 4to.—*From the Academy.*

Verhandlungen der Vereins für Naturkunde zu Presburg, VIII., IX. Jahrgang. 8vo.—*From Professor E. Mack, Presburg.*

Bulletin de la Société de Geographie. Fevrier, 1867. Paris, 1867. 8vo.—*From the Society.*

Abhandlungen der Koniglichen Akademie der Wissenschaften zu Berlin. 1865. Berlin, 1866. 4to.—*From the Academy.*

Ueber die Verschiedenheit in der Schädelbildung des Gorilla, Chimpansé, und Orang-Outang, vorzüglich nach Geschlecht und Alter, nebst einer Bemerkung über die Darwinsche Theorie (with plates), von Dr Th. L. Bischoff. Munchen, 1867. 4to. —*From the Royal Academy, Munich.*

Monday, 15th April 1867.

SIR DAVID BREWSTER, President, in the Chair.

The following Communications were read :—

1. On the Nature and Character of European Winter Storms, with the best means of giving Warning of their Approach. By R. Russell, Esq.

The object of this paper was to show the true character of storms, illustrated by those of 2d, 3d, and 4th December 1863. It was pointed out that the mode usually adopted of laying down the isobarometric lines was calculated to present an erroneous view of their figure or form. The laying down representative lines in round numbers of 5 millemetres in each, or in two-tenths of an inch of barometric readings, lead deceptively to the conclusion that the areas of least pressure were circular or elliptical.

It was then shown that the lines of equal pressures were ribbed into the *latitudinal* line of minimum barometer, which was usually found running north and south, sometimes nearly straight, but often curved, with its convex side towards the east. The minimum line of barometer was easily fixed by consulting the self-registering barometers. The minimum line of barometer was shown to have been on the meridian of London at 8 A.M. of the 3d December, and apparently nearly straight from Algiers to the Orkney Islands. The barometer along the line of minimum barometer gradually descended from upwards of 30 inches in Algiers to 29 inches a little to the south of London. From London to the Shetland Islands the minimum barometer was isobarometric in its character —the pressure varying little over a distance of 600 miles. The isobarometric lines were shown to run parallel to each other over Spain, the Mediterranean, and south of France, and joined to the minimum line at an obtuse angle. Where the difference of pressure on the latitudes from the middle of France to the south of England was considerable, they were ribbed in from both sides at acute angles. Where the minimum line assumed the character of an isobarometric line, the isobarometric lines on the east were nearly straight and parallel to each other. On the west, they were much of the same character, especially about a hundred miles west of the minimum line. It was shown that curved lines could not be put in over France on the 3d December storm, according to the barometrical readings as given in the Paris Bulletins.

The isobarometric lines were shown to have been similar in their character on the morning of the 2d; that, however, they were flatly ribbed into the minimum line of barometer over France and England, and corresponded with the diagrams of the self-registering barometers at Oxford, Kew, and Greenwich. The effects of the storm of the 3d, then out into the Atlantic, were shown on the lines in contrast with those of the latter date.

It was shown that the winds on the *west side* of the minimum barometer were from the west and north-west—the only exception to this being over the north of France and south of England, where south-west winds were observed at those stations where the isobarometric lines were kneed in towards the minimum line of barometer. At those places they mostly blew right across the isobarometric lines. At other places to the west the winds blew obliquely or directly across these lines towards the minimum barometer.

Along the line of minimum barometer, where it was almost isobarometric, the winds either lulled or fell down to a calm on its passage from west to east, both on the 2d and 3d December. On the morning of the 2d, calms and light winds prevailed for several hours from the north of Scotland to the south of England. The winds to the east were shown to have blown from easterly quarters obliquely or directly across the isobarometric lines towards the calms and line of low barometer—the winds to the west, across the isobarometric lines, also towards the calms and line of low barometer.

The winds on the west were comparatively cold and dry, raising the barometer, while on the east they were relatively moist and warm, and attended with rains and falling barometer.

Over the south and easterly parts of France the winds were from southerly quarters up to the line of low barometer, which apparently ran nearly north and south at 8 A.M. on the 3d betwixt Rochefort and Bordeaux. The winds in the south-east of the line were then light, but blowing across the isobarometric lines, but in the north strong, where the difference in the pressure on the latitudes became great.

The storm of the 2d December travelled rapidly from west to east from the north of Scotland to the south of Europe—the barometers all rising on the passage of the line of low barometer, very sharply as far north as Utrecht, Munich, and Vienna; as well as at Geneva, Marseilles, and Rome. It reached Rome and Ancona at 8 A.M. of 3d, with wind and rain. The self-registering barometers at Prague and Cracow showed that the rise was only slight at both these places.

During the 2d the barometers continued to fall slowly over Norway and Sweden. On the morning of the 3d, however, after the barometers had again fallen rapidly after midnight over the south of Europe, the rise of the barometer was general over the whole of Europe.

The line of low barometer was over Liverpool at 6 A.M. of 3d; Oxford at 6.45; Nottingham at 7.45; Greenwich at 8.12; Brussels at 11; Utrecht at noon; Geneva at 4.20; St Bernard, Marseilles, and Munich at 6; and Prague at 8. At 9 P.M. the barometers from Haparanda, at the head of the Gulf of Bothnia, to about 50 miles east of Prague, attained their minimum. It passed Vienna at 10, Buda at midnight, and Cracow at 1 A.M. of the 4th. On this occasion the line of low barometer was largely curved on its passage over the Alps, having only reached Florence at 9 A.M. of the 4th, and Rome at 2 P.M. The effect of the Alps in sometimes delaying the RISE of the barometer in Italy, but not the FALL, was formerly pointed out by the author to Signor Matteucci—the cause of the Alps and mountains of Norway in sometimes delaying the rise of the barometer, is alluded to as an interesting fact for discussion.

The storms of 2d and 3d December both travelled rapidly from west to east over Europe. They were shown to have had no such erratic course as some had been led to suppose. That of the 3d, in which the rise of the barômeter was general over Europe, was shown, by the self-registering barometers and observations, taken at different hours, to have travelled with great rapidity. Over Norway and Sweden it was upwards of fifty miles an hour, and about the same from Greenwich to Cracow. The rate of motion was less in the lower latitudes, being 45 miles an hour from Paris to Munich, 37 from Rochefort to Geneva, 34 from Bordeaux to Marseilles, and only 30 miles an hour from Florence to Rome.

The winds blew obliquely or directly towards the low barometer when it was to the westward, and in the opposite direction when it was to the east. The bending of the isobarometric line in its passage across the Alps produced a hurricane from the north at Rome after 2 P.M. of the 4th, in consequence of the westerly cold current being pressed down towards the south by the higher barometers then existing in the north.

The easterly progression of storms is due to the westerly current that generally prevails, as an upper or under current in extra-tropical latitudes. Henry and Herschel consider this a counterpart or return of the trades of the tropics. The author is not satisfied with this explanation, but in the meantime cannot give a better.

The high west winds at Liverpool and all places to the south, on the morning of the 2d December, was caused by a descent of the upper westerly current, and they blew with great force for four or five hours at the earth's surface. There was a calm behind them and a calm in front. The self-registering anemometers at Liverpool and Utrecht, which are not far from being on the same latitude, showed that the strong winds from the west lasted about the same time at both places. It was merely a repetition of the same phenomena along the same latitude.

On the 3d the westerly wind prevailed more generally on the surface of the earth, modified into north and north-west winds, as the pressure required to be equalised. The winds over any great space do not blow to a centre, but to the calms and light winds along the line of minimum barometer. It was shown that a minimum point or space of barometric pressure existed, on the line of low barometer, over the south of Scotland and north of England on the 3d December. This caused a centripetal action of the winds as it passed from west to east. They blew to the low barometer from the N.E. over the Firth of Forth, and gradually veered round to N. and N.W. as the low barometer passed on toward the east. The winds to the south, on the contrary, veered from S.E. round by S. and S.W., till the arrival of the cold north-westerly current.

The minimum barometer is propagated, and cannot strictly be said to travel. It is merely the western edge of the south-westerly current, which prevails as a middle current, probably not more than 15,000 feet from the earth's surface. It is this south-westerly current that brings the moisture and comparatively high temperature which is found *east* of the line of low barometer. The easterly winds, which are of no great depth, ascend into this south-westerly current, not over a given elongated space, as Espy supposed, but over the whole area where they blow and the barometers are falling. The existence of south-west winds on St Bernard's, 8200 feet above the level of the sea, while easterly winds prevail over the plains of Europe during the falling barometers in the December storms, is appealed to as decisive of the views which have been long held by the Author on this point.

The latitudinal line of minimum barometer represents that space where the air from the surface of the earth to the top of the atmosphere is warmest on any latitude, and consequently lightest. The air is abnormally heated by the condensation of vapour in its upper beds, which has the effect of propelling the air from the south-west above, and from easterly quarters below. The heated air of the upper surface of the south-west middle current is swept off towards the east, and causes the high pressure in Eastern Europe, such as prevailed there on the morning of the 2d December, when the barometer was low in Western Europe. The same principle that regulates the sea-breeze on our coasts in summer, regulates the motions of the winds in storms.

The south-westerly current that blows above in the high latitude is usually felt at the surface in France and the Mediterranean eight or ten hours before the passage of the minimum barometer. It blows obliquely across the isobarometric lines, and, in contradistinction to Espy's views, obliquely *from* the line of minimum barometer. When the difference of pressure is great betwixt the isobarometric lines, the wind blows violently from the south, at first changing to the south-west, on the passage of the low barometer, and then to the west and north-west, becoming a part of the general cold and dry current which raises the barometer.

The cold and dry current prevailing from the west, causing the rise of the barometer, imparts form to the line of minimum barometer. This line is of great length, and frequently nearly straight, and for obvious reasons, when bent, it can only be largely bent with its convex side towards the east. It is often greatly curved in the slow-moving storms of autumn, when north-easterly rainy winds are largely developed on the north-eastern part of storms.

In regard to storm-warning, it was contended that, as storms all issued out of the Atlantic, a day and night watch at Valentia, on the west coast of Ireland, was indispensable to give warning of such rapidly-moving storms as those of December 1863. That storms all travelled from west to east was established eighty years ago, through the labours of the Meteorological Society of the Palatinate, and had been completely verified by the experience of Signor Matteucci, the distinguished director of the Meteorological Department in Italy. Every country in Europe must watch the development of storms as they pass to the east on the latitudes on which they are situated. It was shown that it is highly probable that great breakings up in our weather may be telegraphed from Newfoundland.

2. On the Vapour Lines in the Spectrum. By Sir David Brewster, K.H., F.R.S.

In the year 1842 I discovered that the luminous and brilliant lines in the spectrum of certain flames, since called *vapour lines*, corresponded to certain dark lines in the solar spectrum.

This observation was made for the first time by the spectrum produced by the deflagration of nitre, and I afterwards found that this was a property belonging to every such flame.*

This result was obtained from experiments made at St Andrews in 1842, on nearly 180 substances, deflagrated in a platina cup by a mixture of oxygen and coal gas. A notice of these experiments was read at the meeting of the British Association at Manchester in 1842. The journal containing them was laid before the Physical Section, and one or two of the more interesting results were published in the reports of the Association for that year. The object of these experiments was merely to discover new facts for future investigation, and hence the place of the bright lines was simply estimated by the eye. Other pursuits, of a less laborious kind, prevented me from determining the exact places of these lines in those flames where their number and position were most remarkable, but this will now be better done by some of those numerous observers who are working so successfully with the spectroscope.

The following specimens of these observations will show their limited character, and how far they may be useful and suggestive to future observers :—

1. In the *Chlorides of Tin* and *Lead*. The whole spectrum was covered with splendid lines, the double line D being conspicuous.

2. In the *Ammonio-Chloride of Platinum*. The colour of the flame *yellow*, and fine lines chiefly in the *red* and *green* spaces.

3. *Acetate of Strontites*. Colour of the flame yellow. Fine red and orange lines near D, and one fine *blue* line, and an extreme red.

4. *Acetate of Lime*. Flame *brown*. Many fine lines, and a large green and red one on each side of D.

5. *Chromate of Ammonia*. Fine *green* and *red* bands near D.

6. *Lithoxanthate of Ammonia*. Fine lines throughout the whole spectrum, with a fine *bright* blue band.

7. *Superacetate of Lime*. A fine *blue* line in the extreme violet; and an *orange* and *green* band equidistant from D.

8. *Tartrate of Potash* and *Antimony*. Flame brilliant *blue*. The extreme *red* finely insulated.

* Report of the British Association for 1842, p. 15.

9. *Cryolite.* Flame *yellow*, but green at top. Bright lines in the green, and a very remarkable and bright one in the yellow close to D. There are lines also in the *red*, and the *yellow* light is unusually copious.

10. *Muriate of Barytes.* Flame *green.* The lines splendid. *Two* in the *blue*, *two* in the *green*, *four* in the *yellowish green*, and *two* in the *red.*

3. On the Radiant Spectrum. By Sir David Brewster, K.H., F.R.S.

I have given the name of *Radiant Spectrum* to a phenomenon which I discovered in 1814, and which I described to this Society in the early part of that year.

It will be understood from fig. 1, which represents the brilliant radiation which surrounds a very small image of the sun, when it is formed either by reflection or refraction, or otherwise.

If we now form a spectrum of this radiant image, either by a prism or by diffraction, we shall have the *radiant spectrum* shown in fig 2, where MN is the spectrum of the small circular image S,

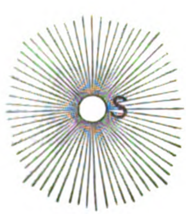


Fig. 1.

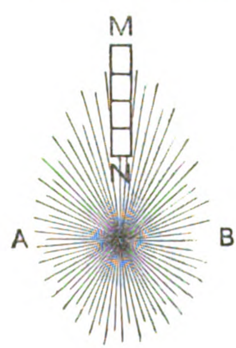


Fig. 2.

and AB the spectrum of the radiation, the centre of which is beyond the violet, and nearly in the place where the intensity of the chemical or invisible rays is a *maximum.*

In order to analyse this compound radiation, let the image of the sun S, fig. 1, be taken from homogeneous *red* light R, fig. 3, and refracted by the prism, we shall have its radiation *ab* at a little distance from the bright portion R, as in fig. 3. In homogeneous *yellow* light Y, fig. 4, the radiation *ab* will be at a greater distance from Y than in the *red* light. In homogeneous *violet* light V, fig. 5, the radiation *ab* will be at a greater distance from V than in the *yellow* light.

If we now refract laterally these homogeneous radiant spectra, fig. 3 will be changed into fig. 6, fig. 4 into fig. 7, and fig. 5 into

Fig. 6.

Fig. 3.

Fig. 7.

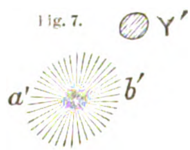


Fig. 4.

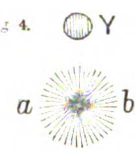


Fig. 8.

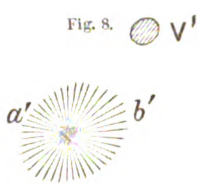


Fig. 5.

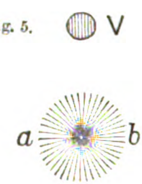


Fig. 9.

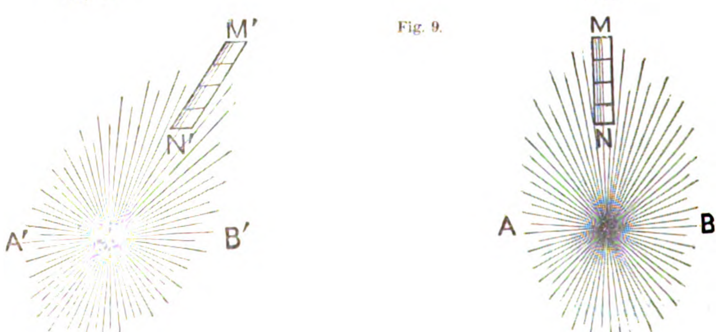


fig. 8, thus proving that the radiant portion of the spectra consists of rays more refrangible than the portion RY and V from which it is derived, and that the difference between the refractive indices of these portions increases with the refrangibility of the rays at RY and V.

The compound spectrum MN, AB, fig. 2, is therefore composed of all these separate spectra, and if we refract it laterally, as shown in fig. 9, we produce the oblique radiant spectrum M′N′A′B′, thus proving that the radiant image consists of rays more refrangible than the homogeneous light from which it is derived.

In a rude experiment with a prism of flint glass, whose mean index of refraction was 1·596, the index of the extreme violet was 1·610, and that of the centre of the radiant image 1·640.

In the preceding experiments the radiation is produced by the action, on the retina, of the small and bright image of the sun; but the same results are obtained, and more distinctly exhibited, by placing a surface of finely-ground glass either on the front of the prism, or behind it, and near the eye.

The existence of a radiant image beyond the violet end of the spectrum, as in fig. 2, is a fact difficult to explain. I have had an opportunity of describing, or showing it to several distinguished philosophers—to the Marquis Laplace and M. Biot in the autumn of 1814, and more recently to others, by some of whom the experiments have been repeated, but no explanation of them has been suggested, excepting the untenable one that the separation of the radiant image from the ordinary spectrum might be the result of parallax.

A better theory, and one of great interest, if true, may be sought in the phenomena of fluorescence, discovered in sulphate of quinine by Sir John Herschel, and in fluor spar and other substances by myself, and in the beautiful explanation of them by Professor Stokes. In this theory the invisible radiation of the chemical rays is rendered visible by being scattered by granular surfaces, just as the invisible chemical rays in the ordinary spectrum are rendered visible by being reflected and scattered by the particles of fluorescent bodies.

4. On the Vibration of a Uniform Straight Spring. By Edward Sang, Esq.

In this paper the solution of the well-known and hitherto refractory problem, "to investigate the law of motion of a uniform elastic plate," is presented as an example of the application of the theory of primaries to physical research. This problem, on account of its importance in the theory of acoustics, has attracted the attention of the most celebrated mathematicians.

If we suppose a physical elastic line, flexible only in one plane, to have a number of masses fixed at intervals along it—if we investigate the law of the motions of these masses—and if, thereafter, we imagine the masses to be indefinitely subdivided and their parts distributed along the intervals, we shall arrive at the law of vibration of an elastic plate. Now, when we examine the general features of the vibration of such a discrete series, we discover that every motion of which it is susceptible may be regarded as the combination of simple movements, the number of which is equal to the number of the connecting ties; these simple movements being such that, if one of them existed alone, all the parts of the series would come simultaneously into their mean positions. The demonstration of this theorem is identic with that which was given in a paper on linear vibration read before the Society during the session 1856–57, and therefore omitted from the present paper. From this theorem it follows, that the trembling of a continuous spring must be composed of an infinity of simple vibrations, each of which is performed in its own peculiar periodic time. Hence our attention must be given to the characters of a simple vibration.

In order that a number of bodies may perform simultaneous oscillations, it is necessary that their tendencies to return to their mean positions be proportional to their distances from these positions, and to their masses jointly. Hence, if a number of equal masses be arranged uniformly along a thin elastic uniform bar, the form which that bar assumes when making a simple vibration must be such that the pressures necessary to retain it at rest in that shape are proportional to the distances from the mean position.

That is, the pressure at A must be proportional to the ordinate A*a*, the pressure at B to the ordinate B*b*, and so on. Now, if the ordinates A*a*, B*b*, C*c*, D*d*, &c., were in arithmetical progression, there would be no flexure at any of the points; the flexure is, in fact, proportional to the second difference of the ordinates; for example, the flexure at D is proportional to the second difference

of the three ordinates C*c*, D*d*, E*e*, or to C*c* − 2D*d* + E*e*. Again, when a straight bar is slightly bent by pressures applied at three points, the flexures at equidistant intermediate points are in arithmetical progression; that is to say, if at the point D no pressure be applied, the flexure at D must be an arithmetical mean between those at C and E. When pressure is applied at D, that pressure must be proportional to the second difference of the flexures at C and E, and consequently to the fourth difference of the ordinates at B, C, D, E, and F.

Hence the characteristic property of the form assumed by such a discrete series when making a simple vibration is, that the length of any ordinate, as *d*D, must be proportional to the fourth difference of the five ordinates of which it is the middle one.

In passing from the discrete series of masses to the concrete bar, the differences of the various orders are replaced by the derivatives of the same orders, and hence the general character of the curve assumed by an elastic bar when making a simple vibration is this, "that the ordinate at any point of the curve is proportional to its fourth derivative, the abscissa being the primary;" and this statement contains the complete solution of the problem.

Putting x for the abscissa, and y for the ordinate, of the curve, the above proposition may be written

$${}_{4x}y = ky\,;$$

it indicates the relation between the function and one of its derivatives, thus bringing the problem under the dominion of the calculus of primaries; the object of that calculus being to discover the relation of the primary variable to the function, when the relation subsisting between that function and its derivative is known. It also shows that the function belongs to the class of recurring functions of the fourth order; and that, therefore, the equation

$$\phi x = y = A\boxed{0}\frac{x}{l} + B\boxed{1}\frac{x}{l} + C\boxed{2}\frac{x}{l} + D\boxed{3}\frac{x}{l}$$

represents the form of the curve in every possible case of simple vibration; A, B, C, D being coefficients, having fixed ratios to each other, and l being a length depending on the relation of the linear unit to the dimensions of the system.

It only remains to apply this general formula to particular cases. The end of an elastic bar may be held firmly as in a vice, it may be entirely free, and it may be touched, but without any angular tension; and these three conditions may be combined at the two ends of the bar, thus making in all six distinct cases, as under, viz.—

Case 1. When the elastic bar is held firmly by one end, the other end being free.

Case 2. When both ends are free.

Case 3. When both ends are held fast.

Case 4. When one end is free, the other touched.

Case 5. When one end is held, the other touched.

Case 6. When both ends are touched.

Now, when the end of the bar is held fast, both the ordinate and its derivative must be zero for that end; when the end is free, the second and third derivatives must be zeroes there; and lastly, when the end is touched without angular tension, the ordinate and its second derivative must each be zero; and therefore the peculiarities of the equations applicable to the various cases are easily found.

Case I.

In the first and most familiar case, when the elastic bar is held firmly by one end, it is easily shown that if X be the entire length, the zero being placed at the fixed end, we must have A = 0, B = 0, while the coefficients C and D must satisfy the two conditions

$$C\boxed{0}\frac{X}{l} + D\boxed{1}\frac{X}{l} = 0\,; \qquad C\boxed{3}\frac{X}{l} + D\boxed{0}\frac{X}{l} = 0\,;$$

which give

$$\boxed{0}\frac{X}{l}\cdot\boxed{0}\frac{X}{l} = \boxed{1}\frac{X}{l}\cdot\boxed{3}\frac{X}{l}\,;$$

from which equation the ratio $\frac{X}{l}$ has to be determined.

Denoting this ratio by ρ_1, we observe that ρ_1 has an infinite number of values corresponding to the infinite number of simple vibrations of which the system is susceptible, and that the times of these vibrations are as the squares of the different values of l thence resulting. This general equation of condition may also be put into the form

$$\boxed{0}\rho_1^2 - \boxed{2}\rho_1^2 = -1\,;$$

and the equation of the curve becomes

$$y_1 = \boxed{1}\rho_1\cdot\boxed{2}\frac{x}{l} - \boxed{0}\rho_1\cdot\boxed{3}\frac{x}{l}\,.$$

Case II.

When both ends of the bar are free.

In this case the equation of condition becomes

$$\boxed{2}\rho_2\cdot\boxed{2}\rho_2 = \boxed{1}\rho_2\cdot\boxed{3}\rho_2\,,$$

or

$$\boxed{0}\rho_2^2 - \boxed{2}\rho_2^2 = +1\,,$$

while that of the curve is

$$y_2 = \boxed{2}\rho_2\cdot\boxed{0}\frac{x}{l} - \boxed{1}\rho_2\cdot\boxed{1}\frac{x}{l}\,.$$

Case III.

When both ends are held fast.

The equation of condition in this case is found to be identic with that of the preceding case, and hence we have this very remarkable law, that the sounds emitted by an elastic bar when suspended so as to vibrate freely, are identic in pitch with those given out by a similar bar of the same length held firmly at each end. The equation of the curve in this case is

$$y_3 = \boxed{2}\rho_2 \cdot \boxed{2}\frac{x}{l} - \boxed{1}\rho_2 \cdot \boxed{3}\frac{x}{l} .$$

Cases IV. and V.

are shown to be included in the two preceding; the touched extremity corresponding to the middles of the lengths in those cases when the node of the curve happens to be there.

Case VI.

In this case, when the ends are held, but without angular tension, the equation of condition is

$$\boxed{1}\frac{X}{l} - \boxed{3}\frac{X}{l} = 0 ,$$

but the difference between these two quaternary functions is the sine of $\frac{X}{l}$, and so the equation takes the familiar form

$$\sin\frac{X}{l} = 0 ,$$

and so $\frac{X}{l}$ has an infinity of values forming the arithmetical progression π, 2π, 3π, 4π, &c.; and the equation to the curve becomes

$$y_6 = \boxed{1}\frac{x}{l} - \boxed{3}\frac{x}{l} = \sin\frac{x}{l} ,$$

so that in vibrating, the bar takes the form of the curve of sines, and the nodes divide the whole length equally. In this case the rapidities of the different vibrations are as the squares of the series of natural numbers.

The determination of the numerical values of the coefficients in the other cases must be obtained by the solution of the transcendental equations

$$\boxed{0}\rho^2 - \boxed{2}\rho^2 = -1, \text{ and } \boxed{0}\rho^2 - \boxed{2}\rho^2 = +1 ,$$

and this solution is obtained in the following manner.

By putting θx to represent the complex function $\boxed{0}x^2 - \boxed{2}x^2$, and taking its successive derivatives, we obtain, for the fourth derivative, the result

$${}_{4x}\theta x = -4\,\theta x ,$$

that is to say, the fourth derivative is quadruple of the function with the sign changed. This brings us to a new class of functions in reality of the eighth order, and bearing to the quaternary functions the same kind of relation which the circular functions, *cosine* and *sine*, bear to the catenarian ones.

If we write instead of x, $z\sqrt{\frac{1}{2}}$ we have

$$\boxed{0}x^2 - \boxed{2}x^2 = 1 - \frac{z^4}{\dots 4} + \frac{z^8}{\dots 8} - \text{etc.},$$

in which the signs are alternately + and − ; denoting this function by the character $\Diamond_0 z$, and its derivatives by $-\Diamond_3 z$, $-\Diamond_2 z$, $-\Diamond_1 z$, we have four functions, viz.,

$$\Diamond_0 z = 1 - \frac{z^4}{..4} + \frac{z^8}{\dots 8} - \text{etc.},$$

$$\Diamond_1 z = \frac{z}{1} - \frac{z^5}{..5} + \frac{z^9}{\dots 9} - \text{etc.},$$

$$\Diamond_2 z = \frac{z^2}{1.2} - \frac{z^6}{\dots 6} + \frac{z^{10}}{\dots 10} - \text{etc.},$$

$$\Diamond_3 z = \frac{z^3}{1.2.3} - \frac{z^7}{\dots 7} + \frac{z^{11}}{\dots . 11} - \text{etc.},$$

which are almost the counterparts of the quaternary functions, and of which the values may be computed with great ease. And it is noteworthy that by putting $z = v\sqrt{\frac{1}{2}}$, these functions reproduce the quaternary ones.

The determination of the values of the coefficients in the equations of the curve, is thus reduced to the numerical solution of the two equations

$$\Diamond_0 z = -1, \text{ and } \Diamond_0 z = +1,$$

and situated between these there is this, viz.,

$$\Diamond_0 z = 0,$$

the roots of which are the odd multiples of $\pi\sqrt{\frac{1}{2}}$.

The results of the calculation, as applied to the case of an elastic rod held firmly by one end, were stated to be, that the fundamental or slowest vibration, and the next one, have their periodic times in the ratio of 6·2557 to 1, or very nearly as 25 to 4. And that the node in the second vibration is at the distance ·78345 parts of the whole length from the fixed end. These results were confirmed by experiments made in the manner first shown by Professor Wheatstone.

The following Gentlemen were admitted Fellows of the Society:—

CHARLES GAINER, M.A., M.D., F.L.S.
WILLIAM KEDDIE, Esq.

The following Donations to the Library were announced:—

Proceedings of the Royal Society, London. Vol. XV. No. 91. 8vo. —*From the Society.*

Proceedings of the Meteorological Society, London. Vol. III. Nos. 28, 29. 8vo.—*From the Society.*

Journal of the Linnean Society, London. Vol. IX. [Botany] No. 39. 8vo.—*From the Society.*

Journal of the Statistical Society of London. Vol. XXX. Part 1. 8vo.—*From the Society.*

Journal of the Chemical Society, London. April 1867. 8vo.—*From the Society.*

Twelfth Annual Report of the Registrar-General on the Births, Deaths, and Marriages registered in Scotland during the year 1866; and Second Annual Report on Vaccination (with Supplement). Edinburgh, 1867. 8vo.—*From the Registrar-General.*

Monthly Returns of the Births, Deaths, and Marriages registered in the Eight Principal Towns of Scotland. For February and March 1867. 8vo.—*From the Registrar-General.*

Mémoires de la Société des Sciences Physiques et Naturelles de Bourdeaux. Tome I.–IV. 8vo.—*From the Society.*

Bulletin de la Société de Géographie. Mars, 1867. 8vo.—*From the Society.*

Nachrichten von der Königlichen Gesellschaft der Wissenschaften und der Georg-Augusts-Universität aus dem Jahre 1866. Gottingen, 1866. 8vo.—*From the Society.*

Jahrbuch der Kaiserlich-Königlichen Geologischen Reichsanstalt. October, November, December 1866. 4to.—*From the Geological Society of Vienna.*

Report of the Committee on Safety-Signals. Presented to the General Railroad Convention, held at New York, October 24, 1866. 8vo.—*From the Committee.*

Monatsbericht der Königlich Preussischen Akademie der Wissenschaften zu Berlin. December 1866. Berlin, 1867. 8vo.—*From the Academy.*

Monday, 29th April 1867.

Sir DAVID BREWSTER, President, in the Chair.

The following Communications were read:—

1. Description of a Double and Triple Holophote, and of a Method of Introducing the Electric and other Lights By Sir David Brewster, K.H., F.R.S.

2. On the Effect of Reduction of Temperature on the Coagulation of the Blood. By John Davy, M.D., F.R.SS. Lond. and Edin.

In an excellent little work recently published on elementary physiology,* its author, when treating of the blood and its coagulation, remarks that a low temperature retards its coagulation; and that "blood kept at the freezing-point of water will not coagulate at all." He adds: "Blood thus kept fluid will, however, coagulate when its temperature is raised; and blood has been thus cooled and warmed till near coagulation for three successive times without losing its coagulability."

That the lowering the temperature of the blood retards its coagulation is now unquestionable; but the statement that it does not coagulate at all at the freezing-point of water, is new to me. Not aware that it has been the subject of experimental inquiry, I thought it right to make some trials to endeavour to determine

* Lessons on Elementary Physiology. By Thomas H. Huxley, LL.D., F.R.S. 1866.

whether the blood can, indeed, be retained in its liquid form in the manner stated.

The trials I have instituted have been of two kinds—one by receiving blood in a cooled vessel, cooled by snow, and kept in snow; the other, by receiving it in a vessel of low temperature in a freezing mixture of salt and snow, and, when frozen, transferring it to snow, with the intent to see whether, after thawing (the freezing-point of blood being lower than that of water) it would continue liquid or would coagulate.

The results of both trials—and they were more than once made—were negative. The blood used was that of the sheep, procured at the slaughter-house; the quantity (the subject of the experiment) half a cubic inch; and the vessels into which it was received, tubes of thin glass about half an inch in diameter.

So small a quantity of blood was employed on account of the greater facility of lowering its temperature, whether to the freezing-point of water, or to the lower degree requisite for the freezing of the blood itself.

As regards the first mode of trial, in each instance, the blood in snow, at a temperature of 32° or 33° F., was found coagulated in about a quarter of an hour. The coagulum was somewhat softer than had the coagulation been more rapid without any interference—*i. e.*, in about two minutes.

As regards the second mode of trial, the coagulum formed after the thawing of the frozen blood was also softer than common; and the more so, it seemed, the longer the blood had been kept in the frozen state previous to liquefaction, as if the fibrin had thus been rendered less contractile.

The parts of the quotation given, in which it is said that blood kept fluid at the freezing-point of water will coagulate when its temperature is raised, and may be "cooled and warmed till near coagulation for three or four successive times," I do not, I must confess, well comprehend.

That blood may be rapidly frozen in small quantity, and rapidly thawed, and this more than once, I ascertained many years ago;* and just now, using sheep's blood, I have had the results con-

* Researches, Physiological and Anatomical, ii. p. 77.

firmed. In the space of one hour, a cubic inch of blood was frozen and liquefied four different times. The vessel in which it was first frozen was a thin drinking cup about two inches in diameter. For the sake of rapid effect, it had been kept some time previously in a freezing mixture; and the blood, as soon as liquefied, was poured into a tube similarly cooled in the same mixture, to be again frozen,—and so on in succession.

These results, I need hardly remark, are little more than a confirmation of those obtained by Hewson nearly a century ago.*

3. Note on a former Paper on the Theory of Double Refraction. By A. R. Catton, M.A., Fellow of St John's College, Cambridge.

4. A Preliminary Notice of the Akazga Ordeal of West Africa, and of its Active Principle. By Thomas R. Fraser, M.D.

This ordeal poison is referred to in the works of Du Chaillu† and Winwood Reade;‡ and several of its toxic properties have been described by MM. Pecholier et Saintpierre.§ A few specimens were sent to this country in 1864 by the Rev. A. Bushnell of Baraka, and these were very kindly given to the author by Mr Thomson of Glasgow; and a further supply came from the same quarter in 1865. These gentlemen, and Dr Nassau of Bonita, supplied valuable and interesting information regarding its employment.

The poison is known in Africa as Akazga, Boundou (or M'Boundou), Ikaja, and Quai; Akazga being probably derived from *nkazga*, which signifies pain or hurt. It is used as an ordeal for the detection of real and superstitious crimes on the West Coast of Africa, in a large district which extends north and south of the equator, and many miles inland, and also in the adjacent island of Corisco.

* The Works of William Hewson, F.R.S. Edited by George Sullivan, F.R.S., p. 17.

† Explorations and Adventures in Equatorial Africa, 1861.

‡ Savage Africa, 1862.

§ Comptes Rendus, 1866, p. 809.

It is believed that several thousand persons are annually subjected to this method of trial, and that the fatal cases are about 50 per cent.

The Akazga arrived in bundles, which consisted of long, slender, and crooked stems, having their roots generally attached to them, but sometimes their leaf-bearing branches only, and containing also a very few complete plants, with roots, stem, and branches. The plant is usually about six feet in length; but some specimens were only four, and others as long as eight feet. The bark is yellowish orange, and in some parts light red, and it is frequently covered with a gray efflorescence. It adheres firmly to the stem, but may be readily detached, after exposure to a gentle heat for some days. Its internal surface is light brown. The space between the bark and the wood was found, in a few pieces, to be occupied by a large number of minute sparkling crystals; but it has not yet been determined whether these consist of a vegetable or mineral substance. The leaves are opposite and oval-accuminate in form; the apex frequently consisting of a linear prolongation more than an inch in length. From its general characters, the plant is supposed to belong to the Loganiaceæ, but the materials are insufficient to identify it.

By boiling the powdered bark with alcohol of 85 per cent., and distilling and evaporating the tincture, a brown shining extract is procured, weighing from 12 to 15 per cent. of the bark employed. It has a bitter, non-persistent taste, and, when treated with concentrated nitric acid, produces a brownish-yellow colour, which is not materially affected by heat, nor by solution of protochloride of tin. It is obvious that the active principle of Akazga is contained in this extract; and to separate it the following method has been adopted, after several attempts with various processes:—The extract is treated with a very dilute solution of tartaric acid, which removes 77 per cent., and filtered. The clear, yellowish-brown acid solution is shaken with successive portions of ether, so long as any colour is removed; and by this means also a small quantity of an aromatic oil is separated from it. After decantation, a solution of carbonate of sodium is added to the liquor, so long as it causes a nearly colourless, flocculent precipitate. It is again shaken with ether, which is decanted, and agitated with three successive portions of distilled water, and finally received in a bottle containing a dilute solution of tartaric acid, and shaken with it. As soon as the etherial solution is brought in contact with the acid, it becomes opalescent, but again assumes its normal appearance on agitation. This change is of some value, as indicating the frequency with which the alkaline solution should be treated with ether, as the milkiness, on contact with tartaric acid, is not produced when the former is exhausted. On reaching this stage the tartaric solution is exposed to a gentle heat—to free it completely from ether—filtered, and again treated with carbonate of sodium, by means of which a bulky, colourless, and flocculent precipitate is obtained. This is collected in a filter, washed, and dried, by exposure to a gentle heat for a short time, and then by the action of sulphuric acid *in vacuo*.

By this means a colourless, amorphous substance is obtained, which is the active principle of the Akazga poison, and which possesses the general properties of a vegetable alkaloid. About 10 grains may be separated from 500 grains of the powdered stem-bark, or 2 per cent. *Akazgia* is proposed as its name; and it is hoped that when the plant is described, if it has been previously unknown to science, Akazga will be adopted as its specific name, and thus the usual connection of nomenclature between the vegetable alkaloid and its source will be maintained.

Akazgia is soluble in about 60 parts of cold absolute alcohol; in about 16 parts of spirit, of 85 per cent.; in about 120 parts of anhydrous sulphuric ether; and in 13,000 parts of distilled water, at a temperature of 60° F. It is freely soluble in chloroform, in bisulphide of carbon, in benzole, and in sulphuric ether of specific gravity 0·735. It crystallises with difficulty, but it may be obtained in the form of minute prisms, by the slow evaporation of a solution in rectified spirit. An analysis of its platinum-salt, and a determination of its combining proportion with dry hydrochloric acid, yielded 290 in the former, and 293 in the latter, as the equivalent of Akazgia. When heated it becomes yellow, then melts, and gives off fumes of a pungent, disagreeable odour, and finally becomes charred, but leaves almost no residue if the heat be continued for a sufficient time. Its solutions have an alkaline reaction, and neutralise acids; and the salts are freely soluble in water, and have a very bitter, non-persistent taste. Concentrated nitric, hydrochloric, and sulphuric acids change its colour to brown, but these in a diluted state, as well as many of the organic acids, form pale, yellowish solutions with Akazgia. It is precipitated from these solutions by hydrate, carbonate, and bicarbonate of sodium, and of potassium; by iodide, sulphocyanate, ferrocyanide, and chromate of potassium; by phosphate of sodium, protochloride of tin, trichloride of gold, dichloride of platinum, potassio-mercuric-iodide, carbazotic acid, tincture of galls, solution of iodine, and various other substances, *but these precipitates are never crystalline.* Corrosive sublimate causes an amorphous white precipitate, which is dissolved by heat, and reappears in a non-crystalline form when the solution has cooled. Chlorine produces an amorphous, colourless precipitate, which does not disappear on the addition of ammonia. With concentrated sulphuric acid, and peroxide of manganese, bichromate of potassium, or any other of the usual oxidizing agents, the same succession of colours is produced, from blue to brown, which results from a similar treatment of strychnia.

The alcoholic extract of Akazga possess physiological properties very similar to those of nux vomica; and comparative experiments were detailed, to show that the active principle, Akazgia, has exactly the same actions as the extract, and a proportional activity to it.

There are several instances in which a Natural Order produces several very similar active principles. In the Loganiaceæ itself, strychnia, brucia, and igasuria already exist, and these are nearly identical in their physiological actions. In chemical properties, brucia and igasuria have much in common, and they are both readily distinguishable, in this respect, from strychnia. Akazgia conveniently completes this group, as its chemical properties are nearly allied to those of strychnia, whilst its connection with all the numbers is maintained by the similarity of its physiological actions.

5 On a Lower Limit to the Power exerted in the Function of Parturition. By J. Matthews Duncan, M.D.

In this paper the author's object was to show what propelling power was required and produced to push the fœtus through the passages into the world in the easiest deliveries. The fact chiefly founded on, as affording a basis for the necessary calculations, was the occasional birth of the ovum entire, that is, the membranes containing the fœtus and liquor amnii being entire after passing through the maternal passages. In such cases the strength of the amniotic membrane was greater than that of the parturient or expulsive power. The expulsive power was in them never so strong as to break the bag of membranes; and the tensile strength of the membrane being ascertained by experiment, the propelling power could be calculated from it.

In like manner, in labours in which the expulsive efforts merely ruptured the bag in the last pains, or did not increase in power after the rupture, the tensile strength of the membranes afforded, along with other data, the means of estimating with considerable certainty and exactness the effective force which completed the labour.

One hundred experiments were performed upon the fœtal membranes in many different cases, in order to acquire data for the calculations above referred to. In conducting them, Dr Duncan had the assistance of Professor Tait, who also pointed out the method of making the computations from the data acquired in them.

After experimentally ascertaining the tensile strength of the membranes, the power of the labour at the time of the rupture of the bag was found, and it was held that the lumen of the passage opened up by the advancing ovum was circular, and of 4½ inches in diameter, and that the bulge of the unsupported membrane was hemispherical. The force required to rupture the weakest amnion showed that the power of the labour was at least 4·08 lbs., that for the strongest 37·58 lbs.; and the average power indicated by the experiments on the amnion was 16·73 lbs.

It was interesting to point out that although the patient and a skilled observer might not remark any increased strength of labour as it advanced, yet, in consequence of the diminished dimensions of the uterine cavity, its muscular action, though the same, had greater effect or power.

Dr J. Poppel had made similar experiments, which anticipated some of the results of this paper. But his method of experimenting was open to the grave objection, that he always supposed the membrane to burst when in a hemispherical form—an error whose tendency is to make the strength of the membrane too little, and which may account for the difference between his results and those given in this paper.

Poppel experimented on the membranes of seven cases in which they burst "with the birth." In these cases, according to Poppel's method of calculating, and changing his results only so far as to make them comparable to those of this paper, the power terminating labour varied from about 6 to about 27 lbs.

The interesting result is thus arrived at, that in some of the easiest cases of labour the mere weight of the child, if duly applied, gives a force sufficient to effect delivery.

The average strength of the amnion found by Poppel, keeping in view an aperture of 2·5 inches in radius, was about 19 lbs.; by Dr Duncan, about 17 lbs.

Dr Duncan proceeded to show how an estimate could be formed of the force exerted in the most difficult labours spontaneously terminated. Joulin had stated it as about 100 lbs., while Dr Duncan believed it did not exceed 80 lbs.

Dr Duncan then observed that he knew of no method of ascertaining the powers of ordinary labours. But he described, though he did not recommend the use of, means by which it could be ascertained in any case. The essential part of this was the discovery of the pressure to which the pool of liquor amnii retained in utero after the rupture of the membranes was subjected as the labour progressed.

6. Some Mathematical Researches. By H. Fox Talbot, Esq.

7. On the Small Oscillations of Heterogeneous Interpenetrating Systems of Particles, which Act upon each other according to Laws dependent only on their Mutual Distaries. By A. R. Catton, M.A., Fellow of St John's College, Cambridge.

8. Note on a celebrated Geometrical Problem. By Professor Tait.

The following problem, originally proposed by Fermat to Torricelli, *To find the point the sum of whose distances from three given points is the least possible*, seems to have given considerable trouble to the older mathematicians, and even in modern times (see *Gregory's Examples*, p. 126) to have been solved in a very tedious manner. Simpler solutions have since been given (*e.g.*, *Cambridge and Dublin Mathematical Journal*, viii. p. 92), but none, to my knowledge, so direct as that indicated by Quaternions. The object of this note is to show the simplicity of the quaternion method.

If α, β be the vectors of two of the given points, the origin being the third, and if ρ be the vector of the required point, we must have (by the conditions of the problem)

$$T\rho + T(\alpha-\rho) + T(\beta-\rho) \text{ a minimum.}$$

Hence $$S[U\rho - U(\alpha-\rho) - U(\beta-\rho)]\,d\rho = 0,$$

for all values of $Ud\rho$. Hence the versor sum in square brackets must vanish identically. The immediate interpretation is, that *lines parallel to* ρ, $\rho-\alpha$, $\rho-\beta$, *form an equilateral triangle*. The required point is therefore in the same plane as the three given points; and their distances, two and two, subtend equal angles at it, which is the well-known solution.

Equally simple is the quaternion solution of the same problem if more points than three be given. Let their vectors, to any origin, be α, β, γ, &c., and let ρ be the vector of the sought point. We have

$$\Sigma \,.\, T(\alpha-\rho) = \text{minimum},$$

from which, as above,

$$\Sigma U(\alpha-\rho) = 0. \quad . \quad . \quad . \quad . \qquad (1.)$$

Hence, *if unit forces act at the required point, in the lines joining it with the given points, these forces are in equilibrium.* Or, in another form, *a closed equilateral gauche polygon may be drawn whose sides are parallel to the lines joining the sought points with the given ones.* This opens up some very curious geometrical speculations, which I have not time to pursue.

That there is but one point whose vector satisfies equation (1) may easily be proved by quaternions, but even more easily by the following reasoning. Consider the system of unit-forces, just mentioned, at any *two* points, one of which satisfies the problem. It is obvious that, if these forces be referred to the line joining the two points, each will be less inclined to it at one than at the other; so that, as at one they produce equilibrium, at the other they must have a finite component in the direction of this line.

The quaternion investigation at once suggests the following kinematical solution of the problem. Suppose an inextensible string to be passed through a small movable ring, then through small rings at two of the fixed points, then again through the movable ring, and so on—one end of the string being fixed to the movable ring when the number of given points is odd, and to the first fixed ring when the number is even. When the string is drawn tight, *i.e.*, when the sum of the lengths joining each fixed ring to the movable one is a minimum, the movable ring will evidently be in the position of the required point. Also, since the tension of the cord is the same throughout, the movable ring is kept in equilibrium by a set of equal forces in the directions of the lines joining it with the given points, which is the condition above found.

This kinematical process, equally with the quaternion one, whose form directly suggests it, gives easily the solution of the more general problem,—To find a point such that *m* times its distance from A, together with *n* times its distance from B, &c., may be a minimum.

9. On Vortex Motion. By Professor Sir William Thomson .

10. Note on the Radiant Spectrum. By Professor Tait.

This was a preliminary notice of a set of experiments, then in progress under very unfavourable circumstances, on the rare occasions when a ray of sunlight was procurable. The production of the phenomenon was traced to the peculiar texture of the membrane covering the cornea. The dependence of the whole on parallax was confirmed by the appearance of the radiant spectrum in precisely the same position with reference to the bright spectrum, when it was produced artificially by interposing a plate of slightly ground glass in the course of the light, so long as this plate was sufficiently near the eye to produce the phenomenon at all. A farther confirmation was given by the fact that, if the eye be near the prism, the appearance is the same on whichever side of the prism the ground glass be placed. The effects of achromatic prisms, of absorption by coloured glasses, and of the use of various kinds of homogeneous light taken from a pure spectrum, were also described.

11. Note upon a Method of varying Weights by Minute Quantities. By J. A. Broun, F.R.S.

A notice of a Gravimeter, proposed by me, appeared in the Proceedings of the Royal Society of Edinburgh early in 1861. The instrument, with various modifications, was forwarded to me in India about three years thereafter, and was found to have several imperfections which could have been corrected only by my own supervision of the work as it proceeded. His Highness the Rajah of Travancore has been good enough to sanction a sum of money for the construction of a second instrument, with all the precautions experience has suggested.

There are two methods by which the observations may be made. One, by which there is a constant angle of torsion of a single wire giving a variable angular movement to the weight suspended by two wires (depending on the force of gravity at the place of observation). The other, by which the weight suspended is varied, and the angles of torsion of the single wire and movement of the weight are constant. This second method I had at first rejected, as experience had shown me the difficulty of changing the weights without affecting the stability of the instrument. I desire now to note that I devised, about a year ago, and communicated to different men of science, a method of varying the weight, suspended with the greatest delicacy, and without jar to the instrument. This method consists in suspending from the weight a metallic wire, which enters a piece of barometer tube fixed below the instrument: by means of a screw entering a cistern below the tube, mercury (or another fluid, can be forced into the tube so as to immerse the metallic wire in the fluid; the wire being properly chosen as to fineness, or as to its specific gravity compared with that of the fluid, and the height of the fluid being read to a thousandth of an inch as in the barometer, the weight suspended can be made, by turning the cistern-screw, to vary gently and gradually, by as minute a quantity as we please; while the eye is occupied with the coincidences of the telescope wire and its images, which indicate the constancy of the angles of torsion of the single and double wires.

Although the difference of the specific gravities is considerable, I propose to employ iron for the wire and mercury for the fluid.

It has occurred to me that this method of varying a weight might be of use in other branches of research.

12. On the Diurnal Variation of the Magnetic Declination near the Magnetic Equator, and in both Hemispheres. By John Allan Broun, F.R.S., late Director of the Observatory of His Highness the Maharajah of Travancore, G.C.S.I., at Trevandrum.

The following Gentlemen were elected Fellows of the Society:—

J. F. M'Lennan, Esq., Advocate.
Rev. W. Lindsay Alexander, D.D.

The following Donations to the Library were announced:—

National Manuscripts, from William the Conqueror to Queen Anne, selected under the direction of the Master of the Rolls, and photozincographed by command of H.M. Queen Victoria, by Colonel Sir Henry James, R.E. Parts 1 and 2. Southampton, 1865. Fol.—*From the Ordnance Survey.*

The Life, Times, and Scientific Labours of the Second Marquis of Worcester. By Henry Dircks, C.E. London, 1865. 8vo.—*From the Author.*

Worcesteriana: a Collection of Literary Authorities, affording Historical, Biographical, and other Notices relating to Edward Somerset, Sixth Earl and Second Marquis of Worcester. By Henry Dircks, C.E. London, 1866. 8vo.—*From the Author.*

Three Centuries of Perpetual Motion. By Henry Dircks, C.E. London, 1861. 8vo.—*From the Author.*

Life of Samuel Hartlib, Account of his Publications, and Reprint of an Invention of Engines of Motion. By Henry Dircks, C.E. London, 1865. 8vo.—*From the Author.*

The Ghost, as produced in the Spectre Drama, popularly illustrating the Marvellous Optical Illusions obtained by the Apparatus called the Dircksian Phantasmagoria. By Henry Dircks, C.E. London, 1864. 8vo.—*From the Author.*

Contribution towards a History of Electro-Metallurgy, establishing the Origin of the Art. By Henry Dircks. London, 1863. 8vo.—*From the Author.*

American Journal of Science and Arts. No. 128 New Haven, 1867. 8vo.—*From the Editors.*

Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. No. 3. Bruxelles, 1867. 8vo.—*From the Academy.*

Journal of the Chemical Society. Nos. 52, 53, 54. London, 1867. 8vo.—*From the Society.*

Comptes Rendus for 1866–67, etc.—*From the Academy of Sciences, Paris.*

Journal of the Royal Society of Arts, London, for 1866–67. 8vo. —*From the Society.*

Monthly Notices of the Royal Astronomical Society, London, for 1866–67. 8vo.—*From the Society.*

Proceedings of the Society of Antiquaries of London. Vol. III. Nos. 1 and 2. London, 1865. 8vo.—*From the Society.*

Archæologia, or Miscellaneous Tracts relating to Antiquity. Vol. XL. London, 1866. 4to.—*From the Society of Antiquaries of London.*

Memoirs of the Geological Survey of India. Vol. V. Parts 2 and 3. Calcutta, 1866. 8vo.—*From the Survey.*

Palæontologia Indica. Vol. II. Parts 10–13. Calcutta, 1866. 4to.—*From the Geological Survey of India.*

Annual Report of the Geological Survey of India, and of the Museum of Geology, Calcutta, for 1865–66. 8vo.—*From the Survey.*

Catalogue of the Meteorites in the Museum of the Geological Survey of India. Calcutta, 1866. 8vo.—*From the Survey.*

Catalogue of the Organic Remains belonging to the Cephalopoda, in the Museum of the Geological Survey of India. Calcutta, 1866. 8vo.—*From the Survey.*

Astronomical and Meteorological Observations made at the United States Naval Observatory during 1864. Washington, 1866. 4to.—*From the Observatory.*

Journal of the Asiatic Society of Bengal. Part I. No. 3, 1866. Part II. No. 3, 1866. Calcutta. 8vo.—*From the Society.*

Proceedings of the Royal Society, London. No. 92. 8vo.—*From the Society.*

Proceedings of the Royal Geographical Society, London. Vol. IX. No. 2. 8vo.—*From the Society.*

Proceedings of the Meteorological Society, London. Vol. III. No. 30. 8vo.—*From the Society.*

Proceedings of the Royal Medical and Chirurgical Society of London. Vol. V. No. 17. 8vo.—*From the Society.*

Report of the Proceedings of the Geological and Polytechnic Society of the West Riding of Yorkshire for 1865–66. Leeds, 1867. 8vo.—*From the Society.*

Report of the Professor of Astronomy in the University of Glasgow for 1867. Glasgow, 1867. 8vo.—*From the Observatory.*

November Meteors of 1866, as observed at the United States Naval Observatory, Washington. 8vo—*From the Observatory.*

Quarterly Return of the Births, Deaths, and Marriages registered in the Divisions, Counties, and Districts of Scotland. March 1867. Edinburgh, 1867. 8vo.—*From the Registrar-General.*

Monthly Return of the same for April 1867. Edinburgh, 1867. 8vo.—*From the Registrar-General.*

Die Fortschritte der Physik im Jahre, 1864; dargestellt von der Physikalischen Gesellschaft zu Berlin. Jahrgang, XX. Abtheilung, 1–2. Berlin, 1866–67. 8vo.—*From the Physical Society of Berlin.*

Monatsbericht der Königlich Preussischen Akademie der Wissenschaften zu Berlin. Jan.–Feb. 1867. Berlin, 1667. 8vo. —*From the Academy.*

Sitzungsberichte der Königl. bayer. Akademie der Wissenschaften zu München. Band. II. Heft 2, 3, 4. München, 1866. 8vo. —*From the Academy.*

Annales des Mines. Tome X. Liv. 4–5. Paris, 1866. 8vo.—*From the Ecole des Mines.*

Bulletin de la Société de Géographie. Avril 1867. Paris, 1867. 8vo.—*From the Society.*

Atti dell' Accademia Gioenia di Scienze Naturali. Tomo XX. Catania, 1865. 4to.—*From the Academy.*

Atti dell' Imp. Reg. Istituto Veneto di Scienze, Lettere ed Arti. Tomo XI. Dispensa 5–10; Tom. XII. Dispensa 1–3. Venezia. 8vo.—*From the Institute.*

Biografia di Carlo Gemmellara. Par Salvator Brancaleone. Catania, 1866. 8vo.—*From the Author.*

Della Natura del Colera Asiatico. Memoria del Dott. Filippo Pacini. Firenze, 1866. 8vo.—*From the Author.*

Bulletin de la Société Protectrice des Animaux. Mars 1867. Paris, 1867. 8vo.—*From the Society.*

Novorum Actorum Academiæ Cæsareæ Leopoldino-Carolinæ Germanicæ Naturæ Curiosorum. Vol. XXXII. Part 2. Dresdæ, 1867. 4to.—*From the Academy.*

Oversigt over det Kongelige danske Videnskabernes Selskabs Forhandlinger og dets Medlemmers Arbeider i Aaret 1865. Nr. 1, 2, 3, 4; 1866, 1, 2, 3, 4, 5, 6; 1867, 1, 2, 3. Kjobenhavn. 8vo.—*From the Academy of Sciences, Copenhagen.*

Memoires de la Société Impériale des Sciences Naturelles de Cherbourg. Tome XI., XII. 8vo.—*From the Society.*

Verhandlungen der Kaiserlich-Königlichen Zoologisch-botanischen Gesellschaft in Wien. Band XVI. Wien, 1866. 8vo.—*From the Royal Society of Vienna.*

Contribuzione pella Fauna dei Molluschi Dalmati, per Spiridione Brusina. Vienna, 1866. 8vo.—*From the same.*

Nachträge zur Flora von Nieder-Oesterreich von Dr August Neilreich. Vienna, 1866. 8vo.—*From the same.*

Bulletin de l'Académie Royal des Sciences, des Lettres, et des Beaux-Arts de Belgique. No. 4. Bruxelles, 1867. 8vo.—*From the Academy.*

Preisschriften gekrönt und Herausgegeben von der Fürstlich Jablonowskïschen Gesellschaft zu Leipzig. 1867. 8vo.—*From the Royal Saxon Academy.*

Bulletin de la Société Vaudoise des Sciences Naturelles. Vol. IX. No. 55, 56. Lausanne, 1866. 8vo.—*From the Society.*

Die Kosmischen Abkühlungen ein Meteorologisches Prinzip von Dr Franz Octav Sofka. Mèn, 1863. 8vo.—*From the Author.*

Pinetum Britannicum. Parts XXII.-XXIV. Fol.—*From Charles Lawson, Esq.*

Quarterly Journal of the Geological Society. No. 90. London, 1867. 8vo.—*From the Society.*

Canadian Journal of Industry, Science, and Art. No. 63. Toronto, 1867. 8vo.—*From the Canadian Institute.*

Journal of the Royal Institution of Cornwall, with the Forty-ninth Annual Report. No. 7. 8vo.—*From the Institution.*

Proceedings of the Royal Asiatic Society of Bengal, from May to December 1866. January 1867. Calcutta. 8vo.—*From the Society.*

On the Scientific Premonitions of the Ancients. No. 1. Greek Geology. By John Young, M.D., Professor of Nat. Hist., Glasgow. 8vo.—*From the Author.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VI. | 1867–68. | No. 74.

Eighty-Fifth Session.

Monday, 25th November 1867.

Professor PLAYFAIR, C.B., Vice-President, in the Chair.

The following Council were elected:—

President.

Principal Sir DAVID BREWSTER, K.H., LL.D., D.C.L.

Honorary Vice-President, having filled the Office of President.

His Grace the DUKE of ARGYLL.

Vice-Presidents.

Principal Forbes.
Professor C. Innes.
Prof. Lyon Playfair, C.B.
D. Milne Home, Esq.
Dr Christison.
Professor Kelland.

General Secretary—Dr John Hutton Balfour.

Secretaries to the Ordinary Meetings.

Dr George James Allman.
Professor Tait.

Treasurer—David Smith, Esq.

Curator of Library and Museum—Dr Douglas Maclagan.

Councillors.

Dr A. Crum Brown.
Dr Burt.
Dr Matthews Duncan.
Professor Turner.
Dr John Muir.
Rev. Thomas Brown.
James Sanderson, Esq.
Hon. Lord Neaves.
R. W. Thomson, Esq., C.E.
George Robertson, Esq., C.E.
Professor Piazzi Smyth.
Patrick Dudgeon, Esq. of Cargen.

Monday, 2d December 1867.

Opening Address by Sir David Brewster, the President. Communicated by Professor Kelland.

Gentlemen,—If it is the primary object of our scientific institutions to advance the interests of science, to promote its diffusion, and to extend its influence, it is not the least of its secondary functions to grapple with intellectual error, and to expose those various forms of superstition and spiritual agency which are now exercising such a perilous influence over half-educated minds.

In mediæval times, when positive knowledge had hardly assumed a definite form, and when the little which did exist was confined to particular classes of society, minds of activity and power naturally threw themselves into depths which they could not sound, or among quicksands from which they could not escape, and thus sought, in wild speculation, for the excitement and notoriety which they could not find in patient research. But in an enlightened age, when real knowledge has made such extraordinary advances, and when the open fields of literature and science invite into their broad domains every variety of genius, and offer a rich harvest of truths to the patient reaper, it is difficult to discover how men of character and high attainments should have surrendered themselves to opinions not less visionary than the legends and prodigies of the ancient mythology.

From such a high level the stream of error has a quick and easy descent, permeating the more influential circles, but driven back by the shrewd intelligence of the pious and educated poor. It is doubtless among the middle and upper classes of society that this credulity and love of the marvellous is most conspicuous. It is luxuriant among the gay and the idle, who have been reared on the rank pastures of our fictitious literature, and who have no faith in those material forces and those cosmical laws which are in daily operation around them. But whatever be its cause, its only cure is a system of secular and scientific education.

When the scholar has learnt to read, to write, and to count, he has obtained only the tools of instruction. To acquire a knowledge of the works of God and of man, of the miracles of nature and of art, is the first step in the civilisation of the people. Without such information the highest as well as the humblest of our race is unfit for a place in the social scale. He may have learned to read his Bible, and may have read it. He may have committed to memory every sentence of the decalogue; and he may have packed into the storehouse of his brain all the wisdom of Solomon, and all the divine precepts of One greater than Solomon, while he is ignorant of everything above him, around him, and within him.

To live upon a world so wonderfully made, without desiring to know its form, its structure, and its purpose;—to eat the ambrosia of its gardens, and drink the nectar of its vineyards, without inquiring where, and how, and why they grow;—to toil for its gold and its silver, and to appropriate its coal and its iron, without studying their nature and their origin;—to tremble under its earthquakes, and stand appalled before its volcanoes, in ignorance of their origin and of their power;—to see and to handle the fossils of animal and vegetable life without asking where, and how, and why they perished;—to neglect such pursuits as these would indicate a mind destitute of the intellectual faculty, and unworthy of the life and reason with which it has been endowed. It is only the irreligious man that can blindly gaze on the grandeur and beauty of material nature without seeking to understand its phenomena and laws.—It is only the ignorant man that can depreciate the value of that true knowledge which is within the grasp of his divine reason;—and it is only the presumptuous man that can prefer those speculative inquiries before which the strongest intellect quails, and the weakest triumphs. "In wisdom hast Thou made them all," can be the language only of the wise;—and it is to the wise only that "the heavens can declare the glory of God," and that "the firmament can show forth His handiwork."

Under the influence of views like these I have, during the last thirty years, in various works and on various grounds, pleaded for a national system of secular and scientific education. Like other germs of truth, it excited the ridicule of ignorance, and alarmed the timidity of a political and religious fanaticism; but as the poorest seed, even when cast among thorns, sometimes springs into life, the cause of secular and scientific education has been gradually entrenching itself in the aspirations of the teacher and the taught, and though not yet comprehended by our statesmen, it must sooner or later receive the sanction of Parliament.

The British Association, who have done more for science in a quarter of a century than all the Governments of England have done since the revival of learning, have taken up the subject of scientific education, and have issued an elaborate report which cannot fail to command the approbation of the public. The rejection of the School Bill, when twice submitted to the House of Commons, warns us that a tribunal so constituted is ill fitted to decide the great questions in which art and science, morality and religion, are so deeply concerned; but in the great "Leap into the Dark," mysterious though it be, and deep as may be the abyss into which it has plunged us, we may yet have fallen upon a solid platform, from which orators, of a new class and loftier aspirations, may plead the cause of neglected science, of taxed and persecuted inventors, and of those higher and holier interests which statesmen have failed to advance or comprehend.

In calling your attention to this subject, it may be useful to mention that the Report of the British Association was drawn up principally by Fellows of the Royal Society of London, and that they count upon the assistance of other institutions in carrying out the work which they have begun. That science itself will be advanced by teaching it in our schools, and more fully in our universities, is a proposition too obvious to be questioned. A startling fact, a striking experiment, and even a popular toy, has often given an impulse to geuius; and some of the greatest of our discoverers, even Newton and Leibnitz, spent their early life in flying kites, making sun-dials, and constructing simple machines. The smattering of science in the school will acquire solidity in the university, and will re-appear in the workshop with valuable applications. It was the chemical teaching of Dr Black that made James Watt the greatest inventor of his age; and it was the rush of electricity through a mile of wire that gave the electric telegraph to the world.

Extensive as is the scheme of scientific instruction and scientific training proposed by the British Association, we would venture to suggest that some knowledge, however small, of the fine and useful arts should be introduced into our schools and universities. If we cannot teach the arts of the architect, the painter, the sculptor, and the manufacturer, we can at least exhibit and explain the most interesting of their works, and thus cultivate in some degree the public taste. There is hardly a neighbourhood where machinery for almost every art, and where the works of the ancient masters, may not be shown at stated periods to the young; while by the facilities of locomotion our galleries of art and our museums of industry may be visited by the humblest of our schools. But even under the village roof-tree we may cultivate the taste, and add to the happiness of its inmates. We may decorate their walls by permanent photographs of the finest specimens of painting and sculpture, and through the stereoscope they may gaze, as if on solid marble, upon the Apollo Belvidere, the Laocöon, and the Venus de Medicis, or upon the modern productions of Canova, Thorwaldsen, and Chantry. Even in the plain furnishings and simple utensils of the cottage we may refine the tastes and elevate the sentiments of the hardworking children of toil. The beautiful in art and nature, equally the gift of the great Giver, may be enjoyed by the humblest of our race. The cup of cold water will taste the sweeter, and the goblet of wine the richer, when the eye rests with pleasure on their lovely forms. The village Lavinia will be "adorned the most" when she has exchanged the apparel of the ball-room for the simple drapery of a less luxurious age. Nor will the cottage family be less joyous when, in their plot of flower-garden, they revel in the harmonies of colour, or when the mantel-piece or walls of their dwelling exhibit to them the choicest forms of art, or those scenes of the picturesque or the sublime with which modern science can so cheaply supply them.

The pleasures of the eye and the ear are the cheapest and sweetest of our luxuries, and when they shall be equally appreciated by the classes whom no common sympathy had previously blended, or whom the usages of a barbarous age had too widely severed, society will be welded by more enduring bonds, and new buttresses added to the social fabric. The artisan or the labourer who devotes his leisure hour to the observation of nature or the admiration of art, who gathers for his family the curious plant or the travelled pebble, or who presents to them the elegant flower vase or the graceful statue, is not likely to seek for excitement in village revels, in political clubs, or in dishonest combinations. His moral nature will rise with his material tastes; and while his less instructed neighbours will look up to him as a model for imitation, his more educated superiors will appreciate his acquirements as a companion or a friend.

It is only in those studies where the eye becomes our teacher, that we can unite in one common pursuit the dissevered classes of society. It is in our galleries of art,—in the rich museums of our cities,—in our botanical, horticultural, and zoological gardens,—or in our crystal palaces, where art and science are rivals, that the children of wealth and of toil can assemble in the common admiration of all that is beautiful and sublime. It is among the remains of ancient and the achievements of modern art, and amid the beauties which we daily appreciate, and the lovely forms of organic life which are ever before us, that we can rise to a purer morality and a higher civilisation.

But it is to the middle, and even to the upper classes, and through them to the nation, that scientific teaching will offer its richest benefits. The functionaries who administer our affairs are in number legion. Without science, without that elevation of character which positive knowledge confers, we can readily understand how the greatest interests of the state are mismanaged,—how interests equally great are neglected,—and how the public wealth is recklessly squandered. Incompetent subordinates assume the importance, and discharge the duties of their chiefs; and while the deep problems of practical science receive a wrong solution, the feelings and interests of inventors and discoverers are utterly disregarded.

In this great country, wealthy by its commerce, its agriculture, and its manufactures, and in no other country in the world, even the poorest, our numerous scientific boards are conducted by men who are too honest to have any pretensions to science, and whose aggregate wisdom, if it exists at all, is enshrined in the cerebellum of a secretary or a clerk. The parsimony of the Exchequer grudges a salary for the services which science only can supply, and consigns to unpaid and irresponsible hands the great interests in which life and property are at stake.

Nor will scientific education be of less value to the lawgiver and to the statesman—to the men who preside over the life and death of the nation,—who make its laws and direct its battles.* To them the British Association has applied in vain for the national recognition of science. Its parliamentary committee, presided over by Lord Wrottesley (a patriotic nobleman, now removed to a brighter sphere), and containing members of both Houses of Parliament, have pleaded in vain for a board of science, to extend its influence and to introduce it into our schools. They have proclaimed to successive governments "that science has not its due weight and importance in the councils of the nation;"—that "a feeling pervades the community at large that our country's welfare, and even safety, depends on its due encouragement and fostering;"—and adopting the words of our great political economist, Mr Mill,†—"that no limit can be set to the importance, even in a purely productive and material point of view, of mere thought,"—and "that every extension of knowledge of the powers of nature is fruitful of application to the purposes of nature and life."

But there is another class of public servants to whom scientific education must be of great importance. Peaceful as science is in its theoretical as well as its practical aspect, it has often to wage war against pirates; and with its meagre exchequer to struggle against the hoarded pelf of unprincipled capitalists, or the combined resources of needy speculators. The discoverer or the inventor is thus driven into a court of law, and our judges and juries have to decide in the most perplexing suits where profound science can be their only guide. To decide against a pirate who has stolen the intellectual property of his neighbour, and can plead only a mistake in the specification of his patent, is a trivial error, even if the decision is unjust; but it is a deeper injustice, and one not to be forgiven, when an inventor is deprived of a property which he had provided for his family, and when the verdict rests either upon the ignorance of the judge, or upon the erroneous appreciation of scientific testimony.

* The Earl of Harrowby, as one of the Parliamentary Committee of the British Association, stated "that those who administer the affairs of the country ought, at least, to know enough of science to appreciate its value, and to be acquainted with its wants and bearings on the interests of society."

† Political Economy, vol. i. p. 52.

But there are still higher interests for which a scientific education is essential,—interests intensely national, and affecting the well-being of every member of the community. In expounding the results of the Great Exhibition of 1851, our eminent colleague, Dr Lyon Playfair, as one of the commissioners, did not scruple to tell the Government "that all European nations, except England, have recognised the fact *that industry must in future be supported, not by a competition of local advantages, but by a competition of intellect* . . . that each foreign metropolis rejoices in an industrial university;—that the result of the Exhibition was one that England may well be startled at,—and that British manufacturers themselves were convinced that most of the foreign countries were rapidly approaching, and sometimes excelling, us in those very manufactures which were our own by hereditary and traditional right." This voice of wisdom and of warning was listened to only by that noble Prince, who, but for petty jealousies and rival interests, would have established that great college of industry, with affiliated schools, which he had announced to his friends.

The same mortifying lesson, thus neglected by the Government, was taught us by the French Exhibition of 1855. The British jurors were so surprised at the superiority of France and other nations in many of the scientific and useful arts, that it became the subject of frequent discussion; and at a meeting called for the purpose, the jurors unanimously declared "that it was only by great exertion, under the most favourable circumstances, that the hitherto almost uncontested superiority of Great Britain in the mechanical and chemical arts could be maintained."*

This second voice of warning, like the first, found no echo

* This meeting was attended by Sir David Brewster, who was called to the chair, by Sir Charles Manby, Secretary to the Institution of Civil Engineers, who was requested to act as secretary, and by Professor Graham, Master of the Mint, Professor Owen, Professor Wheatstone, Professor Cockerell, Mr Fairbairn, Mr George Rennie, Mr De La Rue, Mr Warren De La Rue, Dr Hoffmann, and Mr Crampton, civil engineer. Several jurors, Sir Charles Barry, and others who were unable to attend, warmly approved of the object of the meeting.

Resolutions of a more specific nature were moved by Professor Owen, Professor Wheatstone, and Professor Cockerell, and it was resolved to represent to the British Government the convictions of the jurors.

among our statesmen. Foreign nations were gradually out-running us in the most important of our arts, and the French International Exhibition of 1867 exhibited even to some British statesmen the infatuation of their policy. Dr Playfair and the British jurors again sounded the note of alarm. Lord Granville and Lord Taunton have given public expression to the same judgment, and we may therefore hope that, under a wider constituency and a wiser legislature, the rights of science may be vindicated; and that scientific instruction, beginning in our schools, may be extended to every department of art which contributes to the wellbeing of the citizen, and to the wealth of the empire.

If these views are approximately correct,—if our statesmen, our lawgivers, and our official functionaries are unacquainted with science even in its popular aspects, what must be the intellectual condition of the residue of social life in the upper and middle ranks of the community! All of us can testify, if we had courage, that there is not one person in a hundred who believes, or who understands if he believes, that he lives upon a ball of earth moving daily about its axis, and annually around the sun; and that these two motions are so adjusted as to give us that variety of seasons on which our daily happiness so essentially depends. How few know anything more of our nocturnal lamp, than that the moon has horns in one week and is without them in another. How few know anything more of the telescope and microscope, than that they look into one end of a tube, and see something nearer and larger at the other. How few know anything more of the barometer and thermometer, than that the one is placed inside and the other outside of the house, and that the one tells us something about rain, and the other something about cold. How few know anything more of their inner and outer being, than that the one requires to be fed, and the other to be clothed.

To such persons scientific instruction must be the greatest of gifts; and if we cannot wrest it from the State, it becomes our duty to accomplish by private enterprise what in other countries has been done by the nation.* The efforts of individuals must, of

* The following valuable observations by a competent judge, Dr R. Angus Smith, F.R.S., justify many of our suggestions:—

"Within the last thirty or forty years the violent attempts to teach the course, be feeble in their nature, and limited in their results; but many of the noblest schemes of philanthropy have sprung from one willing heart, and been achieved by one brawny arm. In the field of education there may be many labourers, and there is hardly an educated person who may not throw some seeds into its soil, and glean in return a portion of the harvest which it yields. Each of you must feel some interest in a parochial or other school, in which the youth receive no instruction respecting the material universe around them, and the productions of nature and of art which are daily within their reach. At a very moderate expense you can furnish a telescope and microscope, a barometer and thermometer, a stereoscope, a magic-lantern, a magnet, a burning-glass, and a small electrical machine such as that little globe of revolving glass with which Faraday began his illustrious career; and with this apparatus, during an hour or two in each week, phenomena could be displayed and illustrated, which would be a source of instruction and amusement, relieving the monotony of the school, and giving the teacher a new power over the minds and affections of his pupils. Important as this step would be, it is but a small part of what might be done in conveying scientific instruction.

people here, by schools, mechanics' institutes, and lectures, given or promoted by benevolent persons, have wearied the souls of all those who have co-operated or even looked on with interest. In Germany, without any commotion, calmly and pleasantly, the youths have been trained in schools and colleges without number, and so thoroughly, that they are able to supply foremen and managers to their own manufacturers' establishments, and to send a supply also to foreign countries. In other words, whilst we have failed with our most violent efforts, and with much noise, to teach our own people, they have succeeded not only to teach their own citizens, but to assist in educating the rest of the world. In this matter of education the governments have been able to mould the nation's mind, and to alter the habits of a leading portion of it in a few years. A careful education would probably show its influence in less than *ten* years. . . . . *We require education in the fundamental principles of physical science;* the moral principles, as found in literature, are not alone sufficient either for the higher cultivation of the mind or every pursuit of the useful arts. This applies to the rich and not merely to the poor."—*Proceedings of Manchester Phil. Society*, Nov. 12, 1867.

In reference to the provincial *lectures*, which, Dr Smith says, " have wearied the souls of their promoters," there can be no doubt that, if properly organised by a committee in our University seats, such as the Committee for Local Examinations, a valuable course of scientific and literary instruction might be provided for every class of the community.

In most of our provincial towns there is a museum of natural history and antiquities, which would be a valuable auxiliary in teaching natural science in the neighbouring schools. But even where no such collection exists, a small museum might be established in the humblest of them. Within their own narrow sphere objects of natural history might be obtained, and many a private collection in the district would surrender a tithe of its specimens for the public use. Our Industrial Museum, too, might distribute a portion of its overflowing collections, and even the British Museum might contribute some of its innumerable duplicates, and bring into use its accumulated and unproductive treasures. Itinerant museums, like the itinerant libraries, might be chartered in the same cause, and might sell or exchange the duplicates which are found in different localities.

By these means our school museums might obtain specimens of the more important rocks which form the carpentry of the globe,—of the metallic ores, and the metals themselves, which are in daily use,—of the more precious minerals which are employed for the purposes of art or ornament,—and thus give to the youthful student some knowledge of the world on which he resides, and of the elements of civilisation which it embosoms.

In the departments of Zoology and Botany we cannot expect to collect specimens for our schools, but our travelling menageries, and the museums and botanic gardens of our principal towns, would supply, to a great extent, the means of instruction. But even when these are beyond our reach, photography, and the stereoscope which gives relief to its pictures, might be advantageously employed. The photographic process will give us accurate representations of those objects, both of nature and of art, which it would be desirable to describe and to explain in the instruction of youth. In the department of zoology, the picture might be often taken from the living animal, standing before the camera in vigorous life and transcendent beauty; or when this cannot be done, from the fine specimens of zoological forms which adorn our metropolitan and provincial museums. With equal accuracy might be represented the osteology and integuments of animals,—the framework which protects life, and to which life gives activity and power.

The trees and plants too of distant zones will show themselves in true relief,—the banyan clinging with its hundred roots to the ground,—the bread-fruit tree, with its beneficent burden, or the deadly upas, preparing its poison for the arrow of the savage or the poniard of the assassin. With no less interest will the school-boy gaze on the structures of the inorganic world,—the minerals which have lain in the earth beneath his feet,—the crystals which chemistry has conjured into being, displaying to him their geometric forms, infinite in variety, and interesting from their rarity and value. Painted by the very light which they reflect, he will see the Koh-i-noor and other diamonds, and the huge rubies and sapphires and emeralds which have adorned the chaplet of beauty, and sparkled in the diadems of kings. The gigantic productions of the earth will appeal to him with equal power—the colossal granites which have travelled in chariots of ice, the precipices of ancient lava, the doric colonnades of basalt, and the fossil giants of the primeval world, which trod the earth during its preparation for man, and have been embalmed in stone—to instruct and to humble him.

In acquiring a knowledge of physical geography, of the grander aspects of nature, their representations in relief will be peculiarly instructive. The mountain range, whether scarred with peaks, or undulating in outline;—the volcano ejecting its burning missiles;—the fixed or the floating iceberg;—the glacier and its moraines;—and even the colossal wave, with its foaming crest, will be portrayed in all the grandeur of nature.

The works of human hands, too, will stand before the scholar in their pristine condition, or their ruined grandeur;—the monuments by which sovereigns and nations perpetuate their names;—the pyramids, with their mysterious legacy to science;—the gorgeous palaces of kings;—the garish temples of superstition;—and the bastions and strongholds of war, will be seen as if the observer were placed at their base, and warmed by the very sun which shines upon their walls.

Although few of our village youth may become sculptors, yet the sight of ancient statues, in actual relief, and in their real apparent magnitude, cannot fail to instruct and to refine them. To gaze upon the masterpieces of ancient art, standing in the very halls which they occupy, or to contemplate the *chefs d'œuvres* of modern or living artists, with the sculptor himself standing by their side, must excite an interest of no ordinary kind.

The works of the architect, the engineer, and the mechanist, may also be exhibited in full relief at our schools;—the gigantic aqueducts of ancient and modern times;—the viaducts and bridges which span our valleys and our rivers;—and the living machinery in our factories and workshops, which toil daily for our benefit, and supply the commerce of the world.

With such means in our power, cheaply obtained, and easily applied, a large portion of scientific instruction may be instilled into the youth of our schools,—familiarising them with the works of their Maker, and preparing them for the reception of that higher revelation with which the truths of science cannot fail to harmonise. The knowledge thus imparted will not be confined to the schoolroom. It will elevate the amusements of the holiday and the leisure hour. It will pass into the cottage, amusing and enlightening its inmates. It will find its way into the workshop, giving skill to the workman, and value to his work. It will insinuate itself into the servants' hall, and even into the boudoir and the drawing-room, returning an usurious interest upon the liberality which introduced it into the school. Thus, diffused among our now popular constituencies, and appreciated by those above them, the truths of science may rise into the regions of legislation, wresting from the still reluctant statesman a measure of secular, scientific, and compulsory education, by which the benighted and criminal population around us may be taught to fear God and honour the King.

In conformity with the usual practice of the Society, the following brief notices are given of deceased Members, arranged alphabetically. In calling your attention to the present state of our Society, I regret to state that our losses during the past year have been severe, not only in their number, but in the talent and reputation of our deceased colleagues.

Sir Archibald Alison, Bart., was the younger son of the Rev. Archibald Alison, LL.B., a Fellow of this Society, who was for many years senior clergyman of the Episcopal Chapel in the Cowgate, Edinburgh. His son Archibald was born at Kenley, in Shropshire, in December 1792. In 1800, his father removed to Edinburgh, where his two sons received their education, and studied at the University, when its more important chairs were filled by Dugald Stewart, Playfair, and Leslie. Having chosen the law as his profession, he was called to the Bar in 1814. In the same year he made a tour on the Continent, and, in conjunction with his friend Patrick Fraser Tytler, who accompanied him, he published an interesting account of it, entitled "Travels in France during the years 1814–15, comprising a residence at Paris during the stay of the Allied Sovereigns, and at Aix at the period of the landing of Bonaparte." In 1822 he was made an advocate-depute, an office which he filled with great credit till the downfall of the Wellington Administration in 1830; and, as the result of his experience in that office, in 1831 he published his work, in two vols., "On the Principles and Practice of the Criminal Law of Scotland." On the return of the Duke to power in 1834, Mr Alison was appointed to the Sheriffdom of Lanarkshire, an office which he held to the end of his life. Although his special duties as Sheriff occupied much of his time, he yet found leisure to prosecute his literary studies. During his visit to the Continent, when indignant Europe had sent the first Napoleon into exile, Sir Archibald conceived the idea of writing "The History of Europe, from the commencement of the French Revolution to the Restoration of the Bourbons in 1815." Before he quitted Edinburgh he had published the first two volumes of this able and popular work, and the third and fourth were nearly ready for the press when he removed to Glasgow, where he completed the work in fourteen volumes. In 1852 he began to publish a continuation of it in six volumes, under the title of the "The History of Europe, from the Fall of Napoleon in 1815 to the Accession of Louis Napoleon in 1852." In 1848 he published his "Military Life of the Duke of Marlborough," and in 1859 his "Lives of Lord Castlereagh and Lord Londonderry." Sir Archibald is also the author of a "Treatise on the Principles of Population," which appeared in 1840, and of a great number of interesting articles on various literary and political subjects in *Blackwood's*

Magazine, a selection from which was published in three volumes. In 1852 he received the honour of a Baronetcy from the Earl of Derby during his short term of office.

Although of a robust constitution, Sir Archibald was attacked with a severe affection of the throat in the beginning of May 1867, and on the 23d of the same month he died at Possil House, near Glasgow, in the seventy-fifth year of his age. Sir Archibald was married in 1825 to the youngest daughter of Colonel Patrick Tytler, the brother of Lord Woodhouselee, who still survives him; and he has left a daughter, and two sons, who have both distinguished themselves in the army.

Alexander Dallas Bache, LL.D., an eminent natural philosopher and engineer, was a descendant of the celebrated Franklin. He graduated at the Military Academy at West Point in 1825, and held such a high rank in his class that he was immediately appointed Professor of Engineering in that institution, but he held the chair only for a year. After serving as an officer in the corps of Engineers, for three years, he was appointed, in 1827, to the chair of Natural Philosophy and Chemistry in the University of Pennsylvania, the duties of which he discharged with distinguished ability. In 1833, he was elected superintendent of the Girard College, which had been established in Philadelphia under the princely bequest of Stephen Girard. In 1843, on the death of Professor Karsler, he was appointed superintendent of the United States Survey, a position in which, for a period of twenty-three years, he exhibited high scientific acquirements and unrivalled administrative talents. His annual reports to Congress form a series of valuable contributions to science, and have been much admired in every part of Europe. While he was president of Girard College, between 1836 and 1841, he was directed by the Board of Trustees to report upon the state of education in Europe. With this view he spent a year abroad, accompanied by his wife, and published in 1839 the result of his inquiries. On this occasion he visited Scotland, and I had the great pleasure of a visit from this distinguished philosopher.

When the Congress established, on the 3d March 1863, their National Academy of Science, he was unanimously elected their first President for six years. He discharged the duties of this office till the time of his death.*

Dr Bache was a corresponding member of the French Academy of Sciences, and of various other learned societies.

He died at Washington in 1867, and left a widow, who had been the companion of his labours.

James Black, M.D., an eminent physician, was born in 1787. He began the study of medicine in the University of Edinburgh in 1806, and became a licentiate of the Royal College of Surgeons in 1808. In 1809 he was appointed an assistant surgeon in the Royal Navy, and in the following year was appointed surgeon to the Raven sloop-of-war, in which he served on the coast of Spain during the Peninsular war.

After practising his profession for a short time in Newton-Stewart, he went to Bolton, in Lancashire, where he remained till 1839, when he removed to Manchester for the education of his family. In 1840 he was appointed physician to the Union Hospital, and lecturer on forensic medicine in the Medical School.

As a member of the Philosophical Society, of which he was chosen President in 1859, he contributed to its "Transactions" many papers on Antiquities and Geology, and enriched its Museum with numerous specimens of the rocks and fossils of South Lancashire. In 1853 he was elected president of the Provincial Medical Association, organised by the late Sir Charles Hastings of Worcester; and he was one of the original members of the British Association which met at York in 1831. In 1848 he returned to Bolton, where he remained for eight years, carrying on with activity the scientific pursuits of his early life. In 1856 he returned to Edinburgh, where he was elected a Fellow of this Society in 1857, and read several papers at its meetings. Dr Black was a Fellow of the Geological Societies of London and Paris, a member of the Medico-Chirurgical Society of Edinburgh, of the Social Science Association, and of the Historic Society of Lancashire; and he contributed several papers to their "Proceedings." Dr Black was also an active

* The only separate work which he published beside his volume on Education, was an edition of Brewster's Optics, with copious notes and a large appendix.

contributor to different medical and scientific journals; and it is wonderful how, amid the occupations of an extensive practice, he could have found leisure for such a variety of pursuits. His various writings, which are too numerous for insertion here, appeared as separate articles, in the "Proceedings" of the societies to which he belonged, in the medical and scientific journals of London and Edinburgh, and in the newspapers of the localities in which his lot was cast. Dr Black was a devoted philanthropist, and a defender of Scripture against science, falsely so called. He died at Edinburgh on the 30th April 1867, in the eightieth year of his age, leaving a widow and two sons to lament his loss.

Alexander Bryson, an eminent naturalist, was born at Edinburgh on the 12th October 1816, and was the eldest son of Robert Bryson, senior partner in the celebrated firm of clock and watchmakers. After receiving his preliminary education at the High School, he was apprenticed to a watch and clock maker in Musselburgh, and subsequently went to London to acquire a higher knowledge of his profession. On his return to Edinburgh, after a year's absence, he was received into partnership with his father and brother; and was then able to pursue his scientific tastes, by attending the classes of Chemistry and Natural Philosophy in the University, and the lectures delivered in the School of Arts. Mr Bryson was elected a Fellow of this Society in 1858, and contributed to its "Transactions" the biographies of Sir Thomas Makdougal Brisbane and Dr John Fleming, and other papers, on the Preservation of Footprints on the Sand, and on the Boring of the Pholadidæ. He was elected, in 1860, President of the Royal Society of Arts, to which he contributed several papers, for one of which, on the Method of detecting the Presence of Icebergs in Darkness, he received the Hepburn Prize. He was elected President of the Royal Physical Society in 1863, and contributed a large number of papers to its "Proceedings." Mr Bryson was likewise a member of the Society of Scottish Antiquaries, the Botanical Society, the Geological Societies of London and Edinburgh, and the British Association, at whose meetings he read various papers, chiefly on geological subjects.

Mr Bryson died at Hawkhill on the 7th December 1866, in the fifty-first year of his age.

VICTOR COUSIN, a distinguished philosopher, was born in Paris on the 28th November 1792, and was the son of a watchmaker. He was educated at the Charlemagne Lyceum, and he was so distinguished a pupil, that at the competitions in all the French Lyceums he carried off the principal prize. Cousin had at first resolved to follow music as a profession, but having attended the lectures of Royer Collard on the History of Modern Philosophy, they made such a deep impression upon him, that he devoted himself to the study of literature and philosophy. In 1812 he was appointed assistant teacher of Greek in the École Normale for training professors, and he afterwards became a professor in the same institution. On the return of Napoleon from Elba he enrolled himself among the Royalist Volunteers; but on the Restoration of the Bourbons he resumed his studies, and though only twenty-three years of age, he was appointed the successor of Royer Collard at the Sorbonne. In this new position he assailed the opinions of the Doctrinaires, and expounded those of Reid and Dugald Stewart, which Royer Collard had first made known in France. A visit to Germany, however, unsettled his opinions, and on his return to France he expounded the views of Kant, Hegel, Fichte, and Schelling. Under the Restoration the boldness of his metaphysics exposed him to the royal displeasure. Suspended from his professorship, he became tutor to the present Duke of Montebello, and while he held that position he prepared his editions of Proclus and Descartes, and his translation of Plato. During a visit to Germany in 1825, he was arrested at Dresden, on the supposition of his belonging to the Carbonari, and being transferred to Berlin, he lay in prison for six months. On his return to Paris he opposed the Villèle ministry along with Guizot and Villemain, but he was restored to his professorship under the more liberal ministry of Martignach.

Under the Orleans dynasty Cousin obtained high preferment. Before the close of 1830 he was made Councillor of State, Member of the Royal Council of Public Instruction, Professor in the Sorbonne, Director of the École Normale, and Member of the French Academy. In 1832 he was made a Peer of France, and a Member of the Academy of Moral and Political Sciences. During the short ministry of Thiers in 1840, he filled the office of Minister of Public Instruction.

After the Revolution of February 1848, Cousin ceased to take a part in political affairs. At the instance of General Cavagnac, however, he published a popular edition of his *Profession de foi d'un vicaire Savoyard*, and of his *Justice et Charité*, in which he attacked the socialism of the day. In order to please the clerical party, he republished several of his former lectures in a volume, entitled *Du Vrai, Du Beau, et Du Bien.* During the last twenty years his chair in the Sorbonne had been filled by a substitute, and in 1852 he was, along with Guizot and Villemain, placed on the retired list, with the rank of Honorary Professor.

Between 1818 and 1865 Cousin produced many valuable works. In 1842 he edited the *Pensées* of Pascal, in which the original text was restored. In 1853 he began his historical biographies of the most remarkable women of the 18th century, of Madame de Longueville, Madame de Sablé, Madame de Chevreuse, Madame de Hautefort, and Jacqueline Pascal. In 1863 he published his *General History of Philosophy from the most remote times down to the* 18*th Century*. His works include his *Philosophical Fragments*, in 2 vols., his editions of Plato, Proclus, and Descartes, his *Metaphysics of Aristotle*, his *Scholastic Philosophy*, his *Lectures on the Philosophy of Kant*, his *Lectures on the History of Scottish Philosophy*, including an account of Hutcheson, Reid, Dugald Stewart, Beattie, and Ferguson, and his *Literary Fragments*. He contributed also many papers to the *Memoirs of the Academy of Moral and Political Sciences*, and to the *Journal des Savants*, and the *Revue des Deux Mondes*.

Owing to his failing health, Cousin had for several years spent the winter at Cannes. When he went there in December 1866, the state of his health was by no means alarming; but he was taken ill in the beginning of the present year, and he died on the 15th of January, in the seventy-fifth year of his age.

Henry Home Drummond, Esq. of Blair Drummond, was born in 1783, and was the grandson of the celebrated Lord Kames. He was educated at the High School of Edinburgh, where he distinguished himself as a classical scholar, and he afterwards studied at Oxford, where he took the degree of LL B. Having been educated for the bar, he was admitted a member of the Faculty of Advocates in 1808. In 1812 he was appointed one of the depute-advocates, and during his continuance in that office he was entrusted with the management of a large part of the criminal business of Scotland, at a time when, in the State trials of from 1807 to 1821, courage as well as skill was required in the public prosecutor. Having discharged with distinguished ability the duties thus imposed upon him, he resigned his appointment in 1821, and relinquished his general practice at the Bar.

In 1821 Mr Drummond entered Parliament as member for the county of Stirling, and he continued to discharge his parliamentary duties till 1852, when he resigned in favour of his friend and neighbour, Mr Stirling of Keir. During that long period of public service he gave valuable aid in passing the various statutes relating to Scotland; but the country is under special obligations to him for the Turnpike and Statute Labour Acts, the Salmon Fishing Act of 19 Geo. IV., and especially the Act for the Recovery of Small Debts, which confers the most signal benefits upon the poor.

As a country gentleman, a landlord, and an ardent and skilful improver of his estates, his name will be long remembered in his county. Although it is to Lord Kames that we owe the happy idea of floating away to the Forth the moss which covers the rich alluvial soil beneath the Blair Drummond Moss, it was his grandson who completed this great enterprise, and converted into a fertile field this wide and useless expanse.

Mr Home Drummond, who had long been in feeble health, died in September 1867. He was married to Miss C. Moray of Abercairney, and has left behind him two sons and a daughter—Mr G. Stirling Home Drummond of Ardoch, who succeeds to the paternal estates; Mr C. Home Drummond Moray of Abercairney; and Her Grace The Dowager-Duchess of Athole.

Michael Faraday, a distinguished chemist, was born at Newington Butts, Surrey, on the 22d September 1791. His father was a smith, and at the age of fourteen he was apprenticed for seven years to a bookbinder. In 1812 he became a journeyman bookbinder, and in that year he attended the course of lectures of Sir H. Davy, in the Royal Institution. Having sent to Sir Humphry the notes which he had taken of his lectures, and requested his assistance in obtaining some position which would allow him to pursue his scientific tastes, Sir Humphry kindly attended to his request, and procured for him the place of chemical assistant in the laboratory of the Royal Institution. Faraday entered upon this office in March 1813, and at Sir Humphry's request he accompanied him as his amanuensis on his tour to the Continent in 1814. Upon their return, in 1815, Faraday resumed his place at the Royal Institution, and began that course of experimental research which led him to such brilliant discoveries.

In May 1821 he was appointed superintendent of the house and laboratory of the Institution. In 1823 he was elected a corresponding member of the French Academy of Sciences. On the death of Dalton, in 1844, he was elected one of the eight Foreign Associates of that distinguished body; and since the death of Humboldt, he has been the eldest member in that class of the Academy. In 1824 he was elected a fellow of the Royal Society of London, and for his brilliant discoveries, recorded in their Transactions, he received successively the Copley, Rumford, and Royal Medals, being, with one exception, the only person who has received all the medals which the Royal Society can bestow. In 1827 he published his work on Chemical Manipulation, which reached a second edition in 1836, and a third in 1842. In 1829 he was appointed Lecturer on Chemistry to the Royal Military Academy at Woolwich. In 1832, when the British Association met at Oxford, he received the degree of D.C.L. In 1833 he was appointed Fullerian Professor of Chemistry in the Royal Institution. In 1835 Lord Melbourne granted him a pension of L.300, in recognition of his great discoveries. In 1836 he was appointed scientific adviser to the Trinity House, in reference to the lighthouses under their management, and in the same year he was nominated one of the senate of the University of London. In 1842 he was made Chevalier of the Prussian Order "*Pour le Merite*," founded by Frederick the Great, and recently extended to men of science and learning, who are recommended by the Royal Berlin Academy of Sciences. In 1855 he received the decoration of Commander of the Legion of Honour, and he obtained similar decorations from other sovereigns.

In 1855, he published his "Observations on Mental Education," a lecture delivered before Prince Albert, in which he advocates the teaching of natural science in our schools and colleges.

In 1858 Her Majesty allotted him a residence at Hampton Court, where he died on the 25th August 1867, in the 76th year of his age.

We cannot close this brief sketch of the life of Faraday without a notice, however imperfect, of several of the brilliant discoveries which have placed him at the head of the experimental philosophers of every age.

His first experiment, made in 1816, was an analysis of a specimen of caustic lime, which was published, with observations, by Sir H. Davy. In 1817 and 1818, he discovered that the velocity of gases in passing through a narrow tube, depended not only on their density, but on their own nature; and these experiments were followed, at different dates, by a variety of what may be called his minor discoveries, when compared with those on electricity and magnetism. The most important of these relate to the combustion of the diamond;—to the sounds produced by a bar of heated iron laid upon a mass of metal, the experiment of Mr A. Trevelyan, described in our Transactions;—to the evaporation of mercury at low temperatures;—to the optical illusions produced in the *phenokistoscope* of Plateau;—to the acoustic figures of Chladni;—to the remarkable phenomenon of *regelation*, so successfully pursued by Professor Tyndall;—and to the mysterious movements of turning tables, which he so ingeniously explained. In 1820 he described two new compounds of chlorine and carbon. In 1823 he began his fine experiments on the condensation of gases into liquids; and, by refrigerating mixtures, he obtained liquids more dense than water, from a variety of gases; but in 1844, by combining a mechanical pressure of fifty atmospheres with cold of $-166°$ of Fahrenheit, he obtained in a liquid state olefiant gas, fluosilicic acid, phosphorated hydrogen, and arsenical hydrogen. With this double power he converted the first gases which he liquefied into solid crystals, but those last mentioned he was not able to solidify. In the compression of oil gas, in 1825, he discovered a new compound, the bicarburet of hydrogen, from which Dr Hoffman has extracted another substance of great value in the production of colour. In 1857, Faraday published his curious experiment, in which a gold leaf, placed upon a plate of glass, becomes transparent and colourless at a high temperature, and recovers its green colour by transmitted light, under pressure. In 1820, he discovered the valuable properties of alloys of rhodium and steel, and of silver and steel for cutting instruments. In 1827, he published his work on the "Manufacture of Glass for Optical Purposes," and in his Memoir on the same subject, in 1829, he has given a minute account of all the processes which he employed.

These various researches, numerous and brilliant as they are, might have been the occupation of a busy life, but they form only an introduction, as it were, to the grand discoveries in electricity and magnetism with which his name is more specially associated.

With the exception of his papers on the Electricity of the Gymnotus, and the Electricity Developed by the Friction of Globules of Water upon Solid Bodies, his discoveries in electricity and magnetism may be arranged in three groups,—those that relate to electro-chemistry;—those that relate to electro-dynamical and electro-statical induction;—and those that relate to the action of magnetism and dynamic electricity, upon light and all the other bodies of nature.*

The discoveries in these three groups, which are too numerous and profound to be noticed in a brief sketch of his life, he began to publish in the "Philosophical Transactions" for 1831. They were continued in successive volumes of that work, and were afterwards republished in four octavo volumes, which appeared in 1839, 1844, 1855, and 1859.

As an original investigator of scientific truth, Faraday had few, if any, equals; and as a lecturer and expounder of the discoveries of others he was without a rival. With a judgment thus sound and thus patiently exercised, he had no difficulty in answering the question "What is truth?" among the complex laws of the material world, and he had none in answering the question as put by the Roman Governor. Like Newton, the greatest of his predecessors, he was a humble Christian, with the simplicity of apostolic times. Among the grand truths which he had studied and made known, he had found none out of harmony with his

* See "Notice sur Michael Faraday sa vie et ses Travaux, par Professeur De La Rive, Geneve, 1867," a pamphlet containing an interesting account of Faraday's discoveries. A translation of it is published in the "Philosophical Magazine," vol. xxxiv. p. 409.

faith; and from the very depths of science he has proclaimed to the Sciolists, that there is a wisdom which is not "Foolishness with God."

John Goodsir, a celebrated anatomist, was born at Anstruther in 1814, and was the eldest son of Dr Goodsir, surgeon in that town. After passing through the curriculum of arts at the University of St Andrews, he was apprenticed to Mr Nasmyth, the celebrated dentist in Edinburgh. While he was going through the usual course of medical instruction at the University, he also pursued his anatomical studies under Dr Knox, and the study of mineralogy under Professor Jameson. After obtaining license from the College of Surgeons, he returned to Anstruther, to practise medicine with his father, and in this favourable locality he found leisure to study the various forms of animal life which are so numerous in the Firth of Forth. In 1839 he published in the "Edinburgh Medical and Surgical Journal" an elaborate memoir on the development and structure of the human teeth, and he was soon after this appointed Conservator of the Museum of the College of Surgeons. While he held this office, but particularly in 1842 and 1843, he delivered a series of lectures on various branches of anatomy and physiology. In 1840 he became a member of the Wernerian Society, and contributed many papers to its Memoirs. In 1842 he was elected a Member of this Society, and enriched our Transactions and Proceedings with many valuable papers. When Professor Monro required an assistant, Mr Goodsir was appointed Demonstrator of Anatomy in the University; and when the chair was vacant in 1846, he was appointed to it by the Town Council. In this position, so favourable for research, he prepared an extensive series of specimens illustrative of the anatomy of the animal kingdom, which now constitute the most valuable part of the University Anatomical Museum. As a teacher he enjoyed a high reputation, and he had the gratification of lecturing in the winter session to between 300 and 400 pupils. Under such engrossing studies, with which his whole time was occupied, Mr Goodsir's health gave way, and he was obliged, in 1855, to seek for its restoration in a visit to the Continent. Though still in feeble health on his return to Scotland, he continued to prosecute with ardour his anatomical studies, and in 1856 he published a series of memoirs on the structure of the skeleton, which added greatly to his reputation. Although Mr Goodsir continued to discharge the duties of his class, and carry on his original researches, his health was gradually giving way. He had been afflicted for many years with a general paralysis, which carried him off on the 6th March 1867. In order to do honour to his memory, his friends and pupils have resolved to establish in the University a Fellowship in Anatomy and Physiology bearing his honoured name.

ROBERT EDWARD SCORESBY-JACKSON was born at Whitby in 1833, and was the nephew of our celebrated colleague the late Dr William Scoresby. He prosecuted his medical studies in London, where he became a licentiate of the Apothecaries' Company in 1855. In 1857 he came to Edinburgh, and took the degree of M.D. He then visited the Continent, and on his return to Edinburgh, where he settled as a physician, he became a Fellow of the Royal College of Surgeons in 1859, a Fellow of this Society in 1861, and a Fellow of the Royal College of Physicians in 1862. In 1860 he wrote a Life of his uncle. In 1862 he published his *Medical Climatology*, and in 1863 he read an interesting paper to this Society on the Influence of Weather upon Disease and Mortality, a subject to which he had devoted much attention, and another *on the Temperature of certain Hot Springs in the Pyrenees.* As chairman of the medical department of the Meteorological Society, he conducted, under its auspices, some important inquiries into climate, and he published several valuable papers in its "Proceedings." On the appointment of Dr Douglas Maclagan to the chair of Medical Jurisprudence in the University, Dr Jackson succeeded him as lecturer in Materia Medica and Therapeutics in the extra-Academical School of Edinburgh; and the last work which he published was entitled "A Note-Book of Materia Medica, Pharmacology, and Therapeutics." Dr Jackson was in 1865 elected one of the physicians of the Royal Infirmary. Besides the papers we have mentioned, he contributed several articles to the "Edinburgh Medical Journal." Dr Jackson was an active member of the Edinburgh Medical Missionary Society, and an earnest student of divine truth. He was attacked with typhus fever caught in the Infirmary, and died on the 1st February 1867, in the thirty-fourth year of his age. Dr Jackson was married to the only daughter of Sir William Johnston of Kirkhill, and has left a widow and two daughters to lament his loss.

Thomas Richardson, an eminent chemist, was born at Newcastle on the 8th October 1816, and was educated in that city. At a comparatively early age he went to the University of Glasgow, where he prosecuted his chemical studies in the laboratory of the late Dr Thomson. From Glasgow he went to Germany, where he pursued his favourite science at the University of Giessen, in the celebrated laboratory of organic chemistry, then under the direction of Baron Justus Liebig. After taking the degree of Ph.D. he accompanied Dr Thomson to Paris, where he completed his studies under the late celebrated Pelouze, whose recent death has deprived the Academy of Sciences of one of its most distinguished members.

On his return to Newcastle Dr Richardson devoted himself chiefly to the application of chemistry to manufactures, and the results of his researches have been published in an important work, entitled "*Chemical Technology;* or, Chemistry in its Application to Arts and Manufactures." This work was commenced as a translation of Knapp's *Technology*, by Drs Ronald and Richardson in 1847. The first two parts of a second edition greatly enlarged, and forming quite a new work, by the same authors, was published in 1865. Parts third and fourth, by Dr Richardson and Dr Watt, appeared in the years 1863–1866. This valuable work is still far from being completed. Dr Richardson also took an important part in the preparation of the work upon "The Industries of the Tyne, the Wear, and the Tees."

During the last two years of his life Dr Richardson was engaged at Kirklees, near Wigan, in a series of elaborate experiments on the coal of that district, with the view of obtaining for it a place on the Admiralty list for naval purposes.

In 1853, on the death of our late colleague, Professor Johnston Dr Richardson was appointed Reader of Chemistry in the University of Durham. On this occasion the degree of M.A. was conferred upon him, and he continued for fourteen years to discharge the duties of the chair with great credit to himself, and satisfaction to the University.

When he was engaged at Wigan with his experiments on coal, he was attacked by paralysis on the 10th of July 1867, and expired in a few hours. Dr Richardson was elected a Fellow of this Society in 1866, and he was a Fellow of the Royal Society of London, a Member of the Royal Irish Academy, and an Associate of the Institution of Civil Engineers.

THE EARL OF ROSSE, WILLIAM PARSONS, a distinguished astronomer, was the eldest son of Lawrence, the second Earl, and was born in the city of York on the 17th June 1800. In 1818 Lord Oxmantown entered Trinity College, Dublin, and in the following year he passed into residence in Magdalene College, Oxford, where he took the degree of M.A. in 1822, when he was first-class in mathematics. After quitting Oxford he directed his attention to the construction of reflecting telescopes, and while he was occupied with his earliest experiments, I had the pleasure of frequent communication with him. His first scheme was to remove as much as possible the spherical aberration of spherical surfaces, by dividing the speculum into several rings, which were so placed as to have a common focus. The difficulty of adjusting these rings, and keeping them in their place, was so great that he abandoned the attempt, and set himself to the construction of telescopes with specula of a large size. In the execution of such instruments a thorough knowledge of chemistry and mechanics was required, and the difficulties which he encountered at every step of the process were surmounted with a success greater than he could have anticipated.

With the exception of Sir W. Herschel's large speculum of four feet, the largest telescopes hitherto constructed were those of 13½, 15, and 21 inches in diameter, by Mr Ramage of Aberdeen, and those of 18 inches, by Sir John Herschel. So early as 1822 Lord Oxmantown began to construct large specula, and he had ground and polished them of 15, 24, and 36 inches in diameter before he began the gigantic speculum of six feet, with a focal length of 56 feet. This speculum was cast on the 13th April 1842, ground in 1843, polished in 1844, and ready for trial in February 1845. When it was placed in a tube made of deal staves an inch thick, and hooped with strong iron clamp rings, it was established between two lofty castellated piers 60 feet high, and raised to different altitudes by a strong chain cable attached to the top of the tube.

With this noble instrument, which cost L.20,000, Lord Rosse made a series of important observations on nebulæ and clusters of stars, which have been published in the Transactions of the Royal Society of London, who evinced the high value which they put upon them by giving him the Royal Medal. Many of the principal scientific institutions in Europe hastened to do him honour. In 1849 he was elected President of the Royal Society of London. The University of Cambridge conferred upon him the degree of LL.D. The Imperial Academy of St Petersburg elected him an honorary member; and he was a member of the Royal Irish Academy, and the Astronomical and Geological Societies of London. He was made a Knight of St Patrick by the Queen, and he received from Napoleon III. the decoration of the Legion of Honour. Lord Rosse was also a trustee of the British Museum, and in 1862 he was elected Vice-Chancellor of the University of Dublin.

In 1821 Lord Rosse was elected Member of Parliament for King's County, and he continued to represent it till the end of the first reformed Parliament. After he succeeded his father in 1841, he sat in the Upper House as a representative Peer. After a protracted illness, Lord Rosse died at Monkstown, near Dublin, on the 31st of October 1867, and is succeeded by his eldest son, Lord Oxmantown, who inherits the taste and abilities of his father.

James Smith of Jordanhill, an eminent geologist, was born in Glasgow on the 15th August 1782, and was the son of Archibald Smith, a West Indian merchant resident in that city. After studying at the Grammar School and University of Glasgow, he became a partner in the West India house of Leitch and Smith; but he never took an active part in business, devoting his leisure entirely to literary and scientific pursuits. His love of yachting was one of the most prominent features of his life. He performed his first cruise to the Isle of Skye in 1806, in a small vessel of twelve tons, with Professor Milne and Dr Ure, and he was one of the earliest Commodores in the Royal Northern Yacht Club. He had thus many opportunities of carrying on in different localities his scientific inquiries, and his writings show how successfully he availed himself of them. After the peace in 1814, Mr Smith visited France and Italy, where he resided for some time, occupied chiefly with the study of their works of art. About the year 1830 he began to take an active interest in the management of the Andersonian Institution, of which he was President, and he was the founder of its Natural History Museum, and presented to it its valuable collection of Scottish coins. His earliest scientific paper was a notice printed in the "Transactions of the Antiquarian Society" of an undescribed vitrified fort in the Burnt Isles of the Kyles of Bute. In dredging on the Frith of Clyde, and examining the superficial deposits of the existing shells, he found that a large proportion of those which are not in the Clyde are still to be found in the Arctic Seas; and he was thus led to announce to the Geological Society in 1836, that the climate of this country was once colder than at present. From 1839 to 1846 Mr Smith resided successively at Madeira, Gibraltar, Lisbon, and Malta, on account of the health of some of his family, and in each of these localities he found the materials for the most interesting of his writings.

During his residence at Malta he carried on those remarkable inquiries by which he has attained a high reputation as a Scripture critic and theologian. The work which contains them was published in 1848, under the title of "The Voyage and Shipwreck of St Paul, with Dissertations on the Life and Writings of St Luke, and the Ships and Navigation of the Ancients." His views on the shipwreck of St Paul have been admired by the most eminent theologians of every country; and his essay on the Life of St Luke and the sources of his writings, has been highly praised, even by those who do not adopt all the views of its author. In the last edition of this work, in 1866, Mr Smith has expressed his gratification on finding that his views have received a remarkable confirmation by the publication of Dr Cureton's translation of the newly discovered Syriac version of St Matthew.

Mr Smith was a member of many of our scientific institutions. He was elected a Fellow of the Royal Society of London in 1830, and of this Society in 1822. He was also a Fellow of the Wernerian Society of Edinburgh, and of the Geological Society of London

Mr Smith enjoyed good health till the spring of 1866, when he had an attack of paralysis, which did not affect his mind; but a recent attack, at the close of the year, had a fatal termination, and he died on the 17th January 1867, in the eighty-fifth year of his age.

Mr Smith was married, in 1809, to the granddaughter of Dr Wilson, Professor of Astronomy in Glasgow, and he left nine children, of whom only three survive, viz., Archibald Smith, Esq., F.R.S., a distinguished mathematician and barrister; the wife of Walter Buchanan, Esq., M.P.; and the wife of the late William Hamilton, Esq. of Minard.

Sir James South, an eminent astronomer, was born in London in 1785. He was the son of Mr South, a dispensing druggist in Southwark. His son, however, chose a higher branch of the medical profession, and became a member of the Royal College of Surgeons. Although he practised medicine for some years in Southwark, the greater part of his time was spent in the study of practical astronomy.

In his observatory in Blackman Street, Southwark, Mr South had a very fine five feet telescope, mounted equatorially, and with it, and a seven feet telescope equatorially mounted, he and Mr Herschel carried on, in the years 1821, 1822, and 1823, a series of most valuable observations on 3000 double and triple stars, measuring their apparent distances and positions, and comparing them with those of other astronomers. The large volume containing these remarkable observations was communicated to the Royal Society of London on the 15th January 1824, and is published as a part of their Transactions for that year. For the part which he took in this great work Sir James received the Copley Medal from the Society.

In the vicinity of one of the great thoroughfares of the metropolis astronomical instruments require particular caution against tremors, and therefore Mr South removed to Camden Hill, Kensington, where he erected a fine observatory, and furnished it with a magnificent equatorial instrument, executed by Troughton, but owing to some imperfection in its construction that was never remedied, Sir James was never able to make with it a continued series of observations. As one of the finest instruments of the day, it was visited and occasionally used by many distinguished foreigners, who were always hospitably received at Camden Hill.

Sir James was one of the founders of the Royal Astronomical Society, and was once its President. In 1830, when the Duke of Wellington was at the head of the Government, he received the honour of Knighthood, and a pension of L.300 a-year was afterwards conferred upon him in consideration of his services to astronomy. Sir James was a Corresponding Member of the Imperial Academy of Sciences at St Petersburg, of the Royal Belgic Academy of Sciences, an Honorary Member of the Royal Irish Academy, and a Fellow of the Linnean Society. In his later years he was afflicted with severe deafness, and spent much of his time in the use of the microscope. He died at Camden Hill in October 1867, in the eighty-second year of his age.

John Stewart, of Nateby Hall, a gentleman versed in various branches of natural science, was born at Parkhouse, Stranraer, on the 10th January 1813, and was the son of Lieutenant Leveson Douglas Stewart, R.N. He was educated at the Academy and at the University of Edinburgh. He began life as a merchant, but he soon quitted his profession, his ample fortune enabling him to devote his time to different scientific pursuits, but especially to geology and various departments of natural history, in which he made very large and valuable collections. He devoted much of his time to the microscope, and was one of the first to execute instantaneous photographs.

Mr Stewart was a member of the Royal Physical Society of Edinburgh, and of the Botanical Society. He died on the 17th March 1867.

The following statement respecting the Members of the Society was read by the Chairman :—

I. Honorary Fellows—

Royal Personage,	1	
British Subjects,	18	
Foreign,	33	
Total Honorary Fellows,		52

II. Non-Resident Member under Old Laws,			1
III. Ordinary Fellows—			
Ordinary Fellows at November 1866,			276
New Fellows, 1866–67—Rev. Dr L. Alexander, Thomas Annandale, George F. Barbour, Dr A. H. Bryce, David Davidson, Francis Deas, Henry Dircks, Dr James Donaldson, G. S. Home Drummond, Dr T. R. Fraser, Professor Fuller, Dr Charles Gainer, Dr A. Gamgee, Dr A. Graham, Dr D. R. Haldane, Sheriff Hallard, J. H. B. Hallen, Sir George Harvey, T. B. Johnston, William Keddie, John M. M'Candlish, Jas. Richardson, William Turnbull, Peter Waddell,			24
			300
Deduct Deceased—Sir Archibald Alison, Dr James Black, Alexander Bryson, H. Home Drummond, Professor Goodsir, Dr R. E. Scoresby-Jackson, J. G. Kinnear, Professor Richardson, James Smith, Sir James South, John Stewart,	11		
Resigned—Rev. James S. Hodson, D.D. Oxon., Rev. L. Shafto Orde,	2		
Total Deductions,			13
Total Number of Ordinary Fellows at November 1867,			287

It was announced from the Chair that the Council had awarded the Keith Prize, for the biennial period ending April 1867, to Professor C. Piazzi Smyth, for his paper on Recent Measures at the Great Pyramid, published in the Transactions.

The following Gentlemen were elected Fellows of the Society:—

I. *Foreign Honorary Fellows*:—

1. Professor BENJAMIN PEIRCE, Director of the United States' Survey, Harvard University, Cambridge, Massachusetts.
2. CHARLES LE COMTE DE REMUSAT, Member de l'Institut de France, Lafitte, Haute Garonne, France.
3. FRIEDRICH WÖHLER, Göttingen.

II. *British Honorary Fellows* :—

1. James Prescott Joule, LL.D., Thorncliff, Old Trafford, Manchester.
2. Charles Wheatstone, D.C.L., Professor of Experimental Philosophy, King's College, London.

III. *Ordinary Fellow* :—

Robert Daun, M.D., Deputy Inspector-General of Hospitals (re-elected, having resigned the Fellowship in 1845).

The following Donations to the Society were announced :—

Balfour (John Hutton). Obituary Notice of Professor John Goodsir. Edinburgh, 1867. 8vo.—*From the Author.*

Bert (Dr Paul). Note sur un cas de Greffe Animale. 8vo.—*From the Author.*

——— Recherches Expérimentales pour servir a l'histoire de la Vitalité propre des Tissus Animaux. Paris, 1866. 4to.—*From the Author.*

——— Recherches sur les Mouvements de la Sensitive (*Mimosa pudica*, L.). Paris, 1867. 8vo.—*From the Author.*

——— Notes d'Anatomie et de Physiologie Comparées. Paris, 1867. 8vo.—*From the Author.*

——— Sur un Monstre double Autositaire de la Famille des Monosomiens. Paris, 1863. 8vo.—*From the Author.*

Brusina (Spiridione). Contribuzione pella Fauna dei Molluschi Dalmati. Vienna, 1866. 8vo.—*From the Author.*

Catalogue. Index to the Catalogue of Books in the Bates Hall of the Public Library of the City of Boston. First Supplement. Boston, 1866. 8vo.—*From the Library.*

Coleman (Rev. Lyman), D.D. The Great Crevasse of the Jordan and of the Red Sea. 8vo.—*From the Author.*

Delesse (M.), et Lapparent (M. de). Revue de Géologie pour les Années 1864 et 1865. Paris, 1866. 8vo.—*From the Author.*

Dircks (Henry), C.E., F.C.S. The Polytechnic College. A proposed Institution for aiding depressed Talent to complete Works in progress connected with Science, Literature, or Arts. London, 1867. 8vo.—*From the Author.*

Dircks (Henry), C.E., F.C.S. Inventors and Inventions. London, 1867. 8vo.—*From the Author.*

Hinrichs (Gustave). Atomechanik oder die Chemie eine Mechanik der Panatome. Iowa-City, Etats Unis, 1867. 4to.—*From the Author.*

Journal. American Journal of Science and Arts. Second Series. Nos. 129, 130, 131. 8vo.—*From the Editors.*

Lapparent, *vide* Delesse.

Lawson (Charles). Pinetum Britannicum. Parts XXV., XXVI. Elephant fol.—*From the Author.*

Lea (Isaac), LL.D. Check List of the Shells of North America (Unionidæ). 8vo.—*From the Author.*

——— Tables of the Rectification of Mr T. A. Conrad's "Synopsis of the Family of Naïades of North America." Philadelphia, 1866.—*From the Author.*

——— Observations on the Genus Unio, together with descriptions of new species in the family Unionidæ, and descriptions of new species of the Melanidæ, Limneidæ, Paludinæ, and Helicidæ, with 24 Plates. Vol. XI. 4to.—*From the Author.*

Lesley (J. P.). Notes on a Map intended to illustrate five types of Earth-surface in the United States, between Cincinnati and the Atlantic Seaboard. Philadelphia, 1866. 4to.—*From Dr J. Young.*

Manuscripts. Facsimiles of National Manuscripts of Scotland, selected under the direction of the Right Hon. Sir William Gibson-Craig, Bart., Lord Clerk-Register of Scotland, and Photozincographed, by command of Her Majesty Queen Victoria, by Colonel Sir Henry James, R.E., Director of the Ordnance Survey. Part I. 1867. Folio.—*From the Secretary of State for War.*

Martius (Dr Carl Friedrich Phil. von). Beiträge zur Ethnographie und Sprachenkunde Amerika's zumal Brasiliens. 2 vols. Leipzig, 1867. 8vo.—*From the Author.*

Météorologie de la Belgique Comparée a celle du Globe, par Ad. Quetelet. Bruxellés, 1867. 8vo.—*From the Royal Observatory, Brussels.*

Meteorology. Results of twenty-five years' Meteorological Observations for Hobart Town, together with a two years' Register of the Principal Atmospheric Meteors and Aurora Australis. By Francis Abbott, F.R.A.S. Hobart Town, 1866. 4to.—*From the Author.*

Maestri (Pierre). Rapport soumis a la Junte Organisatrice sur le programme de la VI[me] Session du Congrés International de Statistique. Florence, 1867. 8vo.—*From the Author.*

Mitra (M. L.). The Ultimate Structure of Voluntary Muscular Tissue, and the Mode of Termination of Motor Nerves. Edinburgh, 1867. 8vo.—*From the Author.*

Modderman (W.). De Wettelijke Bewijsleer in Strafzaken. Utrecht, 1867. 8vo.—*From the Author.*

Mueller (Ferdinandus), Ph.D., M.D. Fragmenta Phytographiæ Australiæ. Vol. III. and V. Melbourne, 1862–66. 8vo.—*From the Author.*

Murchison (Sir Roderick Impey), Bart., K.C.B. Siluria; A History of the Oldest Rocks in the British Isles and other countries. Fourth Edition. London, 1867. 8vo.—*From the Author.*

Neilreich (Dr August). Nachträge zur Flora von Neider-Oesterreich. Wien, 1866. 8vo.—*From the Zoologico-Botanical Society of Vienna.*

Plantamour (E.). Des Anomalies de la Température Observées a Genève pendant les quarante Années 1826–65. 4to.—*From the Author.*

Quetelet (Ad.). Des Lois Mathématiques concernant les Étoiles Filantes. 8vo.—*From the Author.*

Quetelet (Ern.). Mémoire sur la Température de l'Air à Bruxelles, 1867. 4to.—*From the Author.*

Rankine (W. J. Macquorn). Mechanics (applied). Edinburgh, 1857. 4to.—*From the Author.*

Rive (Professeur A. de la). Notice sur Michel Faraday sa vie et Travaux. Genève, 1867. 8vo.—*From the Author.*

Rizzoli (Francesco). Nuovo processo operatorio per la cura di una vasta apertura Uretro-Cisto-Vaginale. Bologna, 1867. 8vo. —*From the Author.*

Robertson (George). Recent Marine, Hydraulic, and Sanitary Engineering in Scotland. Edinburgh, 1867. 8vo.—*From the Author.*

Rutherford (Wm.), M.D. A Physiological Demonstration, given in the Physiological Laboratory of the University. 8vo.—*From the Author.*

Ryan (Matthew). The Celebrated Theory of Parallels. Second Edition, with Supplement Appendix. Washington, 1866. 8vo.—*From the Author.*

Thomson (J. T.), F.R.G.S. Sequel to some Glimpses into Life in the Far East. London, 1865. 8vo.—*From the Author.*

Tillman (S. D.), A.M. A new Chemical Nomenclature. Albany, 1866. 8vo.—*From the Author.*

Turnbull (William P.). Birds of East Lothian and a portion of the adjoining counties. Glasgow, 1867. 8vo.—*From the Author.*

Transactions and Proceedings of Societies, Academies, and Universities.

Berlin.—Monatsbericht der Königliche Preussischen Akademie der Wissenschaften zu Berlin. March—July 1867. 8vo.—*From the Academy.*

Berne.—Beitraege zur Geologischen Karte der Schweiz herausgegeben von der Geologischen Commission der Schweizerischen Naturforschenden-Gesellschaft auf rosten der Eidgenossenschaft. Lieferung, 3–5. 4to.—*From the Natural History Society of Berne.*

Boston.—Condition and Doings of the Society of Natural History. 1866. 8vo.—*From the Society.*

Memoirs of the Society of Natural History. Vol. I. Parts 1 and 2. 4to.—*From the Society.*

Proceedings of the Society of Natural History. Vol. X., pages 289 to 418; Vol. XI., pages 1 to 96. 8vo.—*From the Society.*

Bourdeaux.—Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux. Tome IV., V. 8vo.—*From the Society.*

Brussels.—Tables Générales et Analytiques du recueil des Bulletins de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. Tome I.–XX. 8vo.—*From the Academy.*

Bulletins de l'Académie Royale des Sciences, des Lettres et des Beaux-Arts de Belgique. Tome XXII., XXIII. 8vo.—*From the Academy.*

Biographie Nationale publiée par l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. Tome I. Partie 2. 8vo.—*From the Academy.*

Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. Tome XXIII. No. 4–6; Tome XXIV. No. 8, 9, et 10. 8vo.—*From the Academy.*

Mémoires de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. Tome XXXVI. 4to.—*From the Academy.*

Annuaire de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. 1867. 12mo.—*From the Academy.*

Annuaire de l'Observatoire Royale de Bruxelles, par A. Quetelet. 1867. 12mo.—*From the Observatory.*

Annales de l'Observatoire Royale de Bruxelles publiées aux frais de l'Etat, par le directeur A. Quetelet. Tome XVII. 4to.—*From the Observatory.*

Cadiz.—Almanaque Nautico para 1869, calculado de orden de S. M. en el Observatorio de Marina de la Ciudad de San Fernando. Cadiz, 1867. 8vo.—*From the Observatory.*

Calcutta.—Journal of the Asiatic Society of Bengal. New Series, Nos. 136, 138, 139. 8vo.—*From the Society.*

Proceedings of the Asiatic Society of Bengal. 1867. Nos. 2–7. 8vo.—*From the Society.*

Connecticut.—Transactions of the Connecticut Academy of Arts and Sciences. Vol. I. Part 1. 8vo.—*From the Academy.*

Cornwall.—Journal of the Royal Institution of Cornwall, with the 49th Annual Report. No. 7. 8vo.—*From the Institution.*

Dublin.—Journal of the Royal Geological Society of Ireland. Vol. I. Part 3. 8vo.—*From the Society.*

Journal of the Royal Dublin Society. No. 36. 8vo.—*From the Society.*

Edinburgh.—Journal of the Scottish Meteorological Society. New Series. No. 15. 8vo.—*From the Society.*

Geneva.—Mémoires de la Société de Physique et d'Histoire Naturelle de Genève. Tome XIX. Partie 1. 4to.—*From the Society.*

Haarlem. — Archives Néerlandaises des Sciences Exactes, et Naturelles, publiées par la Société Hollandaise des Sciences à Haarlem. Tome I. Liv. 5; Tome II. Liv. 1, 2. 8vo.—*From the Society.*

Naturkundige Verhandelingen van de Hollandsche Maatschappij der Wetenschappen te Haarlem. Deel 24, 25. 4to.—*From the Natural History Society.*

Harvard University.—Catalogue of the Officers and Students of Harvard University, for 1866-67. 8vo.—*From the University.*

Catalogus Universitatis Harvardianæ, 1866. 8vo.—*From the University.*

Reports of the President and Treasurer of Harvard College, 1865-66. Cambridge, Mass., 1866. 8vo.—*From the College.*

Report of the Trustees of the Museum of Comparative Zoology at Harvard College in Cambridge, Mass., for 1866. 8vo.—*From the College.*

Kiel.—Schriften der Universitat zu Kiel, aus dem Jahre, 1866. Band XIII. 4to.—*From the University.*

Königsberg.—Astronomische Beobachtungen auf der Königlichen Universitäts-Sternwart zu Königsberg, 1865. Fol.—*From the University.*

Schriften der Königlichen Physicalisch-Oekonomischen Gesellschaft zu Königsberg. 1865, Abth. 1, 2; 1866, Abth. 1, 2. 4to.—*From the Society.*

Lausanne.—Bulletin de la Société Vaudoise des Sciences Naturelles. Vol. IX. No. 55-57. 8vo.—*From the Society.*

Leeds.—Annual Report of the Philosophical and Literary Society, 1866-67. 8vo.—*From the Society.*

Leipzig.—Preisschriften gekrönt und herausgegeben von der Fürstlich Jablonowskischen Gesellschaft zu Leipzig. No. XII., 1867. 8vo.—*From the Royal Saxon Academy.*

Liverpool.—Transactions of the Historic Society of Lancashire and Cheshire. Vol. VI. 8vo.—*From the Society.*

London.—Journal of the Chemical Society. Vol. V. (New Series.) Nos. 54–59. 8vo.—*From the Society.*

Journal of the Royal Geographical Society. Vol. XXXVI. 8vo.—*From the Society.*

Proceedings of the Royal Geographical Society. Vol. XI. Nos. 3, 4, 5. 8vo.—*From the Society.*

Philosophical Transactions of the Royal Society of London. Vol. CLVI. Part 2; Vol. CLVII. Part 1. 4to.—*From the Society.*

Proceedings of the Royal Society of London. Vol. XV., No. 93; Vol. XVI. Nos. 94, 95. 8vo.—*From the Society.*

List of the Royal Society of London. Nov. 1866. 4to.—*From the Society.*

Proceedings of the Royal Institution of Great Britain. Vol. V. Parts 1, 2. 8vo.—*From the Society.*

Proceedings of the Meteorological Society. Vol. III. Nos. 31, 32. 8vo.—*From the Society.*

Journal of the Linnean Society. Vol. IX. Nos. 36, 37 (Zoology); Vol. IX. No. 40; Vol. X. No. 41 (Botany). 8vo.—*From the Society.*

Proceedings of the Linnean Society for 1866–67. 8vo.—*From the Society.*

General Index to the first 25 Vols. of the Transactions of the Linnean Society. London, 1867. 4to.—*From the Society.*

Proceedings of the Royal Medical and Chirurgical Society of London. Vol. V. No. 8. 8vo.—*From the Society.*

Journal of the Statistical Society of London. Vol. XXX. Parts 2, 3. 8vo.—*From the Society.*

Quarterly Journal of the Geological Society. Vol. XXIII. Parts 2, 3, 4. 8vo.—*From the Society.*

List of the Geological Society of London, November 1867. 8vo.—*From the Society.*

Transactions of the Zoological Society of London. Vol. VI. Parts 1, 2, 3. 4to.—*From the Society.*

Proceedings of the Zoological Society of London for 1866. 8vo.—*From the Society.*

Proceedings of the Royal Horticultural Society. Vol. I. (New Series.) No. 8. 8vo.—*From the Society.*

Report of the Committee of the Harveian Medical Society, for the Prevention of Venereal Diseases. London, 1867. 8vo.—*From the Society.*

Lund.—Lunds Universitets Ars-skrift Mathematik och Naturvetenskap. Rätts och Statsvetenskap. Philosophi Språkvetenskap och Historia. 1865. 4to.—*From the University.*

Lyons.—Mémoires de l'Académie Impériale des Sciences, Belles-Lettres, et Arts de Lyon. Classe des Lettres. Tome XII. Classe des Sciences. Tome XIV., XV. 8vo.—*From the Academy.*

Annales de la Société d'Agriculture, d'Histoire Naturelle, et des Arts utiles de Lyon. Tome IX., X. 8vo.—*From the Society.*

Massachusetts.—Proceedings of the Essex Institute at Salem. Vol. IV., Vol. V. Nos. 1 and 2. 8vo.—*From the Institute.*

Melbourne.—Transactions and Proceedings of the Royal Society of Victoria. Vol. VIII. Part I. 8vo.—*From the Society.*

Milan.—Reale Istituto Lombardo di Scienze e Lettere—Classe di Scienze Matematiche e Naturali. Vol. II. Fasc. 9, 10; Vol. III. Fasc. 1–9. Classe Morali e Politiche. Vol. II. Fasc. 8–10; Vol. III. Fasc. 1–10.—8vo. Classe Matematiche e Naturali. Vol. X. Fasc. 3. Cl. Morali e Politiche. Vol. X. Fasc. 3. 4to.—*From the Society.*

Solenui Adunanze del Reale Istituto Lombardo di Scienze e Lettere. Von Guilio Carcano. 1866. 8vo.—*From the Institute.*

Annuario del Reale Istituto Lombardo di Scienze e Lettere. 1866. 12mo.—*From the Institute.*

Moscow.—Bulletin de la Société Impériale des Naturalistes de Moscou. Nos. 3 et 4, 1866; No. 1, 1867. 8vo.—*From the Society.*

Munich.—Sitzungsberichte der Königl. Bayer. Akademie der Wissenschaften zu München. Tome I. Heft 1–4; Tome II. Heft 1. 8vo.—*From the Academy.*

Abhandlungen der Historischen Classe der Königlich Bayerischen Akademie der Wissenschaften. Band IX. Abth. 3.

Mathematisch-Physikalischen Classe. Band X. Abth. 1. 4to.—*From the Academy.*

Naples.—Rendiconto delle Tornate e dei Lavori dell' Academia di Scienze Morali e Politiche, Anno sesto quaderni di Luglio e Agosto, 1867. 8vo.—*From the Academy.*

Ohio.—Report (Twentieth Annual) of the Ohio State Board of Agriculture for 1865. Columbus, 1866. 8vo.—*From the Board.*

Oxford.—Astronomical and Meteorological Observations made at the Radcliffe Observatory, Oxford, in the year 1864. Vol. XXIV. 8vo.—*From the Observatory.*

Palermo.—Giornale di Scienze Naturali ed Economiche pubblicato per cura del Consiglio di perfezionamento annesso al R. Instituto Tecnico di Palermo. Vol. II. Fasc. 2, 3, 4. 4to.—*From the Institute.*

Paris.—Annales des Mines. Tome X., XI. 8vo.—*From the Ecole de Mines.*

Nouvelles Archives du Muséum d'Histoire Naturelle. Tome I. Fasc. 1–4; Tome II. Fasc. 1–4; Tome III. Fasc. 1, 2. 4to.—*From the Natural History Museum, Paris.*

Bulletin de la Société de Géographie, Mai–Oct. 1867. 8vo.—*From the Society.*

Philadelphia.—Journal of the Academy of Natural Sciences of Philadelphia. New Series. Vol. VI. Part 1. 4to.—*From the Academy.*

Proceedings of the Academy of Natural Sciences of Philadelphia. Vol. I., 1841–43; Vol. III., 1846–47; Vol. IV., 1848–49; 1862, Nos. 1–6; 1866, Nos. 1–5. 8vo. —*From the Academy.*

Proceedings of the American Academy of Arts and Sciences. Vol. VII., pages 97 to 184. 8vo.—*From the Academy.*

Proceedings of the American Philosophical Society. Vol. X. No. 76. 8vo.—*From the Society.*

St Petersburg.—Mémoires de l'Académie des Sciences de St Petersbourg. vii^{e}. série. Tome X. Nos. 3–16; Tome XI. Nos. 1–8. 4to.—*From the Academy.*

Bulletin de l'Académie Impériale des Sciences de St Petersbourg. Tome X. Nos. 1–4; Tome XI. Nos. 1–4; Tome XII. No. 1. 4to.—*From the Academy.*

Compte-Rendu Annuel adressé a S. Esc. M. de Reutern, par le Directeur de l'Observatoire Physique Central A. T. Kupffer, 1864. St Petersburg, 1865. 4to.—*From the Russian Government.*

Annales de l'Observatoire Physique Central de Russie, 1863, (with Supplement) 1864. St Petersburg, 1865–66. 4to.—*From the Russian Government.*

Toronto.—Canadian Journal of Industry, Science, and Art. New Series, Nos. 63–65. 8vo.—*From the Canadian Institute.*

Turin.—Atti della R. Accademia delle Scienze di Torino. Vol. I. Disp. 3–7; Vol. II. Disp. 1–3. 8vo.—*From the Academy.*

Memorie della Reale Accademia delle Scienze di Torino. Serie seconda. Tomo 22. 4to.—*From the Academy.*

Utrecht.—Aanteekeningen van het Verhandelde in de Sectie-Vergaderingen van het Provinciaal Utrechtsch Genootschap van Kunsten en Wetenschappen ter gelegenheid van de Algemeene Vergadering, 1866. 8vo.—*From the Society.*

Meteorologische Waarnemingen in Aederland en Zijne Bezittingen en Afwijkingen van Temperatuur en Barometerstand of vele Plaatsen in Europa utigegeven door het Koninklijk Nederlandsch Meteorologisch Instituut, 1864. Utrecht, 1865. 4to.—*From the Institute.*

Nederlandsch Meteorologisch Jaarboek, 1864, 1865, 1866. Utrecht, 1866. 4to.—*From the Meteorological Institute of Utrecht.*

Verslag van het Verhandelde in Algemeene Vergadering van het Provinciaal Utrechtsch Genootschap van Kunsten en Wetenschappen gehonden den 16 October 1866. 8vo.—*From the Society.*

Vienna.—Jahrbücher der Kaiserlich-Königlichen Central-Anstalt für Meteorologie und Erdmagnetismus, von Carl Jelinek und Carl Fritsch. Band I., 1864. 4to.—*From the Society.*

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften. Philosophisch-Historische Classe. Band LIII. Heft 1–3. Mathematisch Classe. Band LIV. Heft 1–4. 8vo.—*From the Academy.*

Washington.—Memoirs of the National Academy of Sciences. Vol. I. 4to.—*From the Academy.*

Astronomical Observations made at the U.S. Naval Observatory during 1851 and 1852. Washington, 1867. 4to. —*From the Observatory.*

Miscellaneous Collections of the Smithsonian Institution. Vols. VI. and VII. 8vo.—*From the Institution.*

Report of the Board of Regents of the Smithsonian Institution, Washington, for 1865. 8vo.—*From the Institution.*

Report of the Secretary of War, with accompanying paper. Washington, 1866. 8vo.—*From the American Government.*

Report on Epidemic Cholera. Washington, 1867. 4to.—*From the War Office, U.S.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VI. 1867–68. No. 75.

Monday, 16*th December* 1867.

Professor LYON PLAYFAIR, C.B., Vice-President, in the Chair.

The following Communications were read:—

1. On Polyzomal Curves, otherwise the Curves $\sqrt{U} + \sqrt{V} + \text{\&c.} = 0$.

By Professor Cayley. Communicated by Professor Tait.

If U, V, &c., are rational and integral functions (*) $(x, y, z)^r$, all of the same degree r, in regard to the co-ordinates (x, y, z), then $\sqrt{U} + \sqrt{V} +$ &c. is a polyzome, and the curve $\sqrt{U} + \sqrt{V} +$ &c. $= 0$ a polyzomal curve. Each of the curves $\sqrt{U} = 0$, $\sqrt{V} = 0$, &c. (or say the curves $U = 0$, $V = 0$, &c.), is on account of its relation of circumscription to the curve $\sqrt{U} + \sqrt{V} +$ &c. $= 0$, considered as a girdle thereto (ζωμα), and we have thence the term "zome" and the derived expressions "polyzome," "zomal," &c. If the number of the zomes $\sqrt{U}$, $\sqrt{V}$, &c. be $= \nu$, then we have a ν-zome, and corresponding thereto a ν-zomal curve; the curves $U = 0$, $V = 0$, &c., are the zomal curves or zomals thereof. The cases $\nu = 1$, $\nu = 2$, are not, for their own sake, worthy of consideration; it is in general assumed that ν is $= 3$ at least. It is sometimes convenient to write the general equation in the form $\sqrt{lU} +$ &c. $= 0$, where l, &c., are constants. The memoir contains researches in regard to the general ν-zomal curve; the branches thereof, the order of the curve, its singularities, class, &c.; also in regard to the ν-zomal curve $\sqrt{l(\Theta + L\Phi)} + \&c. = 0$, where the zomal curves $\Theta + L\Phi = 0$, all pass through the points of intersection of the same two curves $\Theta = 0$, $\Phi = 0$ of the orders r and $r-s$ respectively; included herein we have the theory of the depression of order as arising from the ideal factor or factors of a branch or branches. A general theorem is given of "the decomposition of a tetrazomal curve," viz., taking the equation to be $\sqrt{lU} + \sqrt{mV} + \sqrt{nW} + \sqrt{pT} = 0$; then if U, V, W, T are in involution, that is, connected by an identical equation $\mathrm{a}U + \mathrm{b}V + \mathrm{c}W + \mathrm{d}T = 0$, and if l, m, n, p, satisfy the condition $\frac{l}{\mathrm{a}} + \frac{m}{\mathrm{b}} + \frac{n}{\mathrm{c}} + \frac{p}{\mathrm{d}} = 0$, the tetrazomal curve breaks up into two trizomal curves, each expressible by means of any three of the four functions U, V, W, T; for example in the form $\sqrt{l'U} + \sqrt{m'V} + \sqrt{p'T} = 0$. If, in this theorem, we take $p = 0$, then the original curve is the trizomal $\sqrt{lU} + \sqrt{mV} + \sqrt{nW} = 0$, T is any function $= \frac{1}{\mathrm{d}}(\mathrm{a}U + \mathrm{b}V + \mathrm{c}W)$, where, considering l, m, n as given, a, b, c are quantities subject only to the condition $\frac{l}{\mathrm{a}} + \frac{m}{\mathrm{b}} + \frac{n}{\mathrm{c}} = 0$, and we have the theorem of "the variable zomal of a trizomal curve," viz., the equation of the trizomal $\sqrt{lU} + \sqrt{mV} + \sqrt{nW} = 0$, may be expressed by means of any two of the three functions U, V, W, and of a function T determined as above, for example in the form $\sqrt{l'U} + \sqrt{m'V} + \sqrt{p'T} = 0$; whence also it may be expressed in terms of three new functions T, determined as above. This theorem, which occupies a prominent position in the whole theory, was suggested to me by Mr Casey's theorem, presently referred to, for the construction of a bicircular quartic as the envelope of a variable circle.

In the ν-zomal curve $\sqrt{l(\Theta + L\Phi)} + \&c. = 0$, if $\Theta = 0$ be a conic, $\Phi = 0$ a line, the zomals $\Theta + L\Phi = 0$, &c., are conics passing through the same two points $\Theta = 0$, $\Phi = 0$, and there is no real loss of generality in taking these to be the circular points at infinity—that is, in taking the conics to be circles. Doing this, and using a special notation $\mathbf{A}^{\circ} = 0$ for the equation of a circle having its centre at a given point A, and similarly $\mathbf{A} = 0$ for the equation of an evanescent circle, or say of the point A, we have the ν-zomal curve $\sqrt{l\mathsf{A}^0}$ + &c. = 0, and the more special form $\sqrt{l\mathsf{A}}$ + &c. = 0. As regards the last mentioned curve, $\sqrt{l\mathsf{A}}$ + &c. = 0, the point A, to which the equation $\mathsf{A} = 0$ belongs, is a focus of the curve, viz., in the case $\nu = 3$, it is an ordinary focus, and in the case $\nu > 3$, it is a special kind of focus, which, if the term were required, might be called a foco-focus; the memoir contains an explanation of the general theory of the foci of plane curves. For $\nu = 3$, the equation $\sqrt{l\mathsf{A}} + \sqrt{m\mathsf{B}} + \sqrt{n\mathsf{C}} = 0$ is really equivalent to the apparently more general form $\sqrt{l\mathsf{A}^0} + \sqrt{m\mathsf{B}^0} + \sqrt{n\mathsf{C}^0} = 0$. In fact, this last is in general a bicircular quartic, and, in regard to it, the before-mentioned theorem of the variable zomal becomes Mr Casey's theorem, that "the bicircular quartic (and, as a particular case thereof, the circular cubic) is the envelope of a variable circle, having its centre on a given conic, and cutting at right angles a given circle." This theorem is a sufficient basis for the complete theory of the trizomal curve $\sqrt{l\mathsf{A}^0} + \sqrt{m\mathsf{B}^0} + \sqrt{n\mathsf{C}^0} = 0$; and it is thereby very easily seen that the curve $\sqrt{l\mathsf{A}^0} + \sqrt{m\mathsf{B}^0} + \sqrt{n\mathsf{C}^0} = 0$ can be represented by an equation $\sqrt{l'\mathsf{A}'} + \sqrt{m'\mathsf{B}'} + \sqrt{n'\mathsf{C}'} = 0$. But for $\nu > 3$, this is not so, and the curve $\sqrt{l\mathsf{A}}$ + &c. = 0 is only a particular form of the curve $\sqrt{l\mathsf{A}^0}$ + &c. = 0; and the discussion of this general form is scarcely more difficult than that of the special form $\sqrt{l\mathsf{A}}$ + &c. = 0, included therein. The investigations in relation to the theory of foci, and in particular to that of the foci of the circular cubic and bicircular quartic, precede in the memoir the theories of the trizomal curve $\sqrt{l\mathsf{A}^0} + \sqrt{m\mathsf{B}^0} + \sqrt{n\mathsf{C}^0} = 0$, and of the tetrazomal curve $\sqrt{\ \mathsf{A}^0} + \sqrt{m\mathsf{B}^0} + \sqrt{n\mathsf{C}^0} + \sqrt{p\mathsf{D}^0} = 0$, to which the concluding portions relate. I have, accordingly, divided the memoir into four parts, viz., these are Part I., On Polyzomal Curves in general; Part II., Subsidiary Investigations; Part III., On the Theory of Foci; and Part IV., On the Trizomal and Tetrazomal Curves, where the zomals are circles. There is, however, some necessary intermixture of the theories treated of, and the arrangement will appear more in detail from the headings of the several articles. The paragraphs are numbered continuously through the memoir. There are four Annexes, relating to questions which it seemed to me more convenient to treat of thus separately.

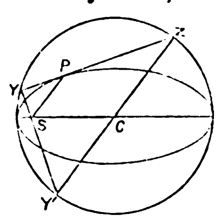
It is right that I should explain the very great extent to which, in the composition of the present memoir, I am indebted to Mr Casey's researches. His paper "On the Equations and Properties (1.) of the System of Circles touching three circles in a plane; (2.) of the System of Spheres touching four spheres in space; (3.) of the System of Circles touching three circles on a sphere; (4.) on the System of Conics inscribed in a conic, and touching three inscribed conics in a plane," was read to the Royal Irish Academy, April 9, 1866, and is published in their "Proceedings." The fundamental theorem for the equation of the pairs of circles touching three given circles was, previous to the publication of the paper, mentioned to me by Dr Salmon, and I communicated it to Professor Cremona, suggesting to him the problem solved in his letter of March 3, 1866, as mentioned in my paper, "Investigations in connection with Casey's Equation," "Quart. Math. Journal," t. viii. 1867, pp. 334–341, and as also appears, Annex No. IV. of the present memoir.

In connection with this theorem, I communicated to Mr Casey, in March or April 1867, the theorem No. 164 of the present memoir, that for any three given circles, centres A, B, C, the equation $BC\sqrt{\mathsf{A}^0} + CA\sqrt{\mathsf{B}^0} + AB\sqrt{\mathsf{C}^0}$ belongs to a Cartesian. Mr Casey, in a letter to me dated 30th April 1867, informed me of his own mode of viewing the question as follows:—"The general equation of the second order $(a, b, c, f, g, h)(\alpha, \beta, \gamma)^2$, where α, β, γ are circles, is a bicircular quartic. If we take the equation $(a, b, c, f, g, h)(\lambda, \mu, \nu)^2$ in tangential co-ordinates (that is, when λ, μ, ν are perpendiculars let fall from the centres of α, β, γ on any line), it denotes a conic; denoting this conic by F, and the circle which cuts α, β, γ orthogonally by J, I proved that, if a variable circle moves with its centre on F, and if it cuts J orthogonally, its envelope will be the bicircular quartic whose equation is that written down above;" and among other consequences, he mentions that the foci of F are the double foci of the quartic, and the points in which J cuts F single foci of the quartic, and also the theorem which I had sent him as to the Cartesian, and he refers to his paper on bicircular quartics as then nearly finished. An abstract of the paper as read before the Royal Irish Academy, 10th February 1867, and published in their "Proceedings," pp. 44, 45, contains the theorems mentioned in the letter of 30th April, and some other theorems. It is not necessary that I should particularly explain in what manner the present memoir has been, in the course of writing it, added to or altered, in consequence of the information which I have thus had of Mr Casey's researches; it is enough to say that I have freely availed myself of such information, and that there is no question as to Mr Casey's priority in anything which there may be in common in his memoir on Bicircular Quartics and in the present memoir.

2. Note on the Hodograph. By Professor Tait.

The object of the present Note is to show, by a few examples (of which, however, the last is the only one of any importance), how easily the geometrical ideas supplied by Hamilton's beautiful invention of the Hodograph enable us to dispense with analytical processes in the establishment of some of the fundamental propositions connected with the motion of a single particle, besides many others which are merely curious; and also how they help us to understand the full bearing of some of the analytical methods. Some of the simplest of such geometrical investigations are given in "Tait and Steele's Dynamics of a Particle," and will not be reproduced here; though a few of the results will be assumed,—as, for instance, that when the acceleration is directed to a fixed point, and varies inversely as the square of the distance from it, the hodograph is a circle, and the path a conic section, of which the point is a focus.

1. If the figure represent an ellipse and its auxiliary circle, it is known that the circle may be considered as the hodograph corresponding to planetary motion in the ellipse, but turned through a right angle. In fact, if YPZ be a tangent to the ellipse at P, SY′ is proportional to the velocity at P, and perpendicular to it in direction. The actual velocity bears to SY′ the ratio of μ to ha, in the usual notation.



Hence the tangent at Y′ is perpendicular to SP (the direction of acceleration), and thus we have an immediate proof that SP is parallel to Y′CZ. But by this means we also get at once, and without analysis, the two well-known and peculiar first integrals, in the form

$$\dot{x} = -\frac{\mu}{h}\frac{y}{r}, \quad \dot{y} = \frac{\mu}{h}\left(\frac{x}{r} + e\right),$$

which cannot be directly deduced from the equations of acceleration

$$\ddot{x} = -\frac{\mu x}{r^3}, \quad \ddot{y} = -\frac{\mu y}{r^3}.$$

[The equation of the orbit is, of course,

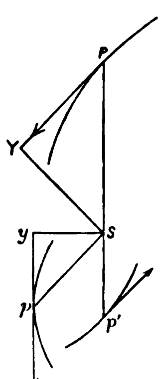
$$h = x\dot{y} - y\dot{x} = \frac{\mu}{h}\left(r + ex\right),$$

from which we see that

$$h^2 = \mu a(1 - e^2)\ ].$$

2. The only central orbits whose hodographs also are described as central orbits, are those in which the acceleration varies directly as the distance from the centre.

Let S be the centre, P any point in the path, p the corresponding point in the hodograph, p' that in the hodograph of the hodograph. Then Sp' is parallel to the tangent at p, which again is parallel to SP. Hence PSp' is a straight line. Also, since p belongs (by hypothesis) to a central orbit, the tangent at p' is parallel to Sp, *i. e.*, to the tangent at P. Hence the locus of p' is similar to that of P, and therefore Sp' is *proportional to* SP. But Sp' represents the acceleration at P. Hence the proposition.



3. If Π be the acceleration in a central orbit, Π′ that required for the description of the hodograph as a central orbit; h, h', the moments of momentum, and r, r', the radii vectores in the two orbits,

$$\Pi\Pi' = \frac{h'^2}{h^2} rr'.$$

In the figure above let SY $= \varpi$ and Sy $= \varpi'$ be the perpendiculars from S on the tangents at P and p, ρ and ρ' the radii of curvature at P and p, then

$$\frac{r}{\varpi} = \frac{r'}{\varpi'} .$$

Also the velocity at p is

$$\Pi = \frac{h\rho'}{r^2} .$$

But, since we have $$\Pi = r' . \frac{r'}{\rho} . \frac{r}{\varpi}$$

(as we see by expressing it in terms of the angular velocity of Sp), if Sp' be called r'', we have

$$\Pi' = r'' . \frac{r''}{\rho'} . \frac{r'}{\varpi'} .$$

Hence, as $$\varpi r' = h , \varpi' r'' = h' ,$$

$$\Pi\Pi' = \frac{h}{r^2} . \frac{r' r''^2}{\varpi'} = \frac{h}{r^2} \frac{r' h'^2}{\varpi'^3} = \frac{h'^2}{h^2} r r' .$$

Or, more simply, if v be the velocity in the orbit, we have, by expressing the centrifugal force in terms of the normal component of the acceleration,

$$\frac{v^2}{\rho} = \Pi \frac{\varpi}{r} .$$

Hence $$\frac{h^2}{\rho} = \Pi \frac{\varpi^3}{r} .$$

[This is the well-known formula

$$\Pi = \frac{h^2}{\varpi^3} \frac{d\varpi}{dr} .]$$

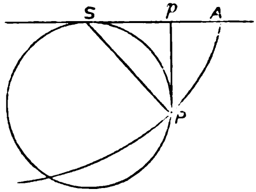
Thus $$\Pi\Pi' = \frac{h^2 r}{\varpi^3 \rho} . \frac{h'^2 r}{\varpi'^3 \rho'} = \frac{h'^2}{h^2} r r' ,$$

because from $$r\varpi' = r'\varpi = h$$

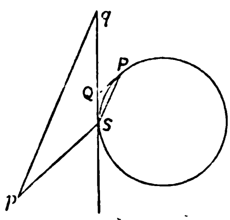
we have at once $$r^2 r'^2 = \varpi\varpi' \rho\rho' .$$

4. Again, if the hodograph be a circle described with uniform angular velocity about a point in its circumference, the path is the cycloidal brachistochrone.

For, if AP be the cycloid described by the point P of the circle SP rolling uniformly on the line AS, the velocity at P is proportional to SP, and the direction of motion is perpendicular to SP. Hence the hodograph (turned through a right angle in its own plane) may be represented by the circle SP, described with uniform angular velocity about the point S. That the motion is due to constant acceleration perpendicular to AS is obvious from the fact that, if Pp be drawn perpendicular to AS, $SP^2 \propto Pp$.

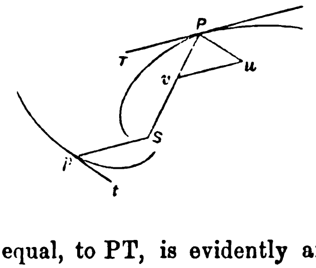


5. If the orbit be central, and be a circle described about a point in its circumference, the hodograph is a parabola described about the focus with angular velocity proportional to the radius vector.



For, if S be the centre of force, P the point in its circular orbit, p the corresponding point of the hodograph: qp, the tangent to the hodograph at p, must be parallel to SP; and, therefore, if SQq be the tangent at S, the triangle pSq (being similar to PSQ) is isosceles. Thus the locus of p is a parabola. Also the angular velocity of Sp, being the same as that of PQ, is double that of SP, and is, therefore, inversely as SP^2. But the length of Sp is inversely as the perpendicular from S upon PQ, *i.e.*, inversely as SP^2.

6. A point describes a logarithmic spiral with uniform angular velocity about the pole—find the acceleration.



Since the angular velocity of SP and the inclination of this line to the tangent are each constant, the linear velocity of P is as SP. Take a length PT, equal to e SP, to represent it. Then the hodograph, the locus of p, where Sp is parallel, and equal, to PT, is evidently another logarithmic spiral similar to the former, and described with the same uniform angular velocity. Hence pt, the acceleration required, is equal to e Sp, and makes with Sp an angle equal to SPT. Hence, if Pu be drawn parallel and equal to pt, and uv parallel to PT, the whole acceleration Pu may be resolved into Pv and vu; and Pvu is an isosceles triangle, whose base angles are each equal to the angle of the spiral. Hence Pv and vu bear constant ratios to Pu, or to SP or PT.

The acceleration, therefore, is composed of a central attractive part proportional to the distance, and a tangential retarding part proportional to the velocity.

And, if the resolved part of P's motion parallel to any line in the plane of the spiral be considered, it is obvious that in it also the acceleration will consist of two parts—one directed towards a point in the line (the projection of the pole of the spiral), and proportional to the distance from it, the other proportional to the velocity, but retarding the motion.

Hence a particle which, unresisted, would have a simple harmonic motion, has, when subject to resistance proportional to its velocity, a motion represented by the resolved part of the spiral motion just described.

If α be the angle of the spiral, ω the angular velocity of SP, we have evidently

$$\mathrm{PT} \,.\, \sin\alpha = \mathrm{SP} \,.\, \omega.$$

Hence

$$\mathrm{P}v = \mathrm{P}u = pt = \frac{\mathrm{PT}^2}{\mathrm{SP}} = \frac{\omega}{\sin\alpha}\mathrm{PT} = \frac{\omega^2}{\sin^2\alpha}\mathrm{SP} = n^2 \,.\, \mathrm{SP} \text{ (suppose)}$$

$$\text{and } vu = 2\mathrm{P}v \,.\, \cos\alpha = \frac{2\omega\cos\alpha}{\sin\alpha}\mathrm{PT} = k \,.\, \mathrm{PT} \text{ (suppose.)}$$

Thus the central force at unit distance is $n^2 = \dfrac{\omega^2}{\sin^2\alpha}$, and the coefficient of resistance is $k = \dfrac{2\omega\cos\alpha}{\sin\alpha}$.

The time of oscillation is evidently $\dfrac{2\pi}{\omega}$; but, if there had been no resistance, the properties of simple harmonic motion show that it would have been $\dfrac{2\pi}{n}$; so that it is increased by the resistance in the ratio cosec α : 1, or $n : \sqrt{n^2 - \dfrac{k^2}{4}}$.

The rate of diminution of SP is evidently

$$PT. \cos \alpha = \frac{\omega \cos \alpha}{\sin \alpha} SP = \frac{k}{2} SP;$$

that is, SP diminishes in geometrical progression as time increases, the rate being $\frac{k}{2}$ per unit of time per unit of length. By an ordinary result of arithmetic (compound interest payable every instant) the diminution of log. SP in unit of time is $\frac{k}{2}$.

This process of solution is only applicable to resisted harmonic vibrations when n is greater than $\frac{k}{2}$. When n is not greater than $\frac{k}{2}$ the auxiliary curve can no longer be a logarithmic spiral, for the moving particle never describes more than a finite angle about the pole. A curve, derived from an equilateral hyperbola, by a process somewhat resembling that by which the logarithmic spiral is deduced from a circle, must be introduced; and then the geometrical method ceases to be simpler than the analytical one, so that it is useless to pursue the investigation farther, at least from this point of view.

3. On the Antiquity of Intellectual Man: from a Practical and Astronomical point of view. By Professor C. Piazzi Smyth.

The author of this paper confined himself entirely to the use of *contemporary* data; and amongst these, the only kinds found available for the very earliest ages, were buildings of one kind or another.

Examining these again, after their first approximate arrangement in chronological relative order by well-known archæologists, it was discovered that there resided a peculiar astronomical opposition between the chief structures in the valley of the Euphrates and those in the valley of the Nile.

The final contest for antiquity amongst all the archæological known remains of the whole earth seemed to lie entirely with the

Euphrates and Nile series of buildings; the palm being assigned at last to the Great Pyramid beginning of the latter.

This particular monument, being then still further examined for the manner of its origination and appearance, was found to be in direct violation of what has often been laid down elsewhere to be the rule or natural law of all purely human work. And as such interference with a natural law, is in so far one of the ordinary definitions of a miracle, a yet more stringent examination of the monument was made; and resulted in the discovery, partly by some of the author's friends, of a continued and consistent series of geometrical and physical data, infinitely above the knowledge of all mankind in the days when the Great Pyramid was constructed; and therefore in so far strengthening the conclusion for a miracle, already otherwise obtained.

First, therefore, beyond compare, of all known human architectural remains, in point of both time, moral purity, mechanical excellence, and philosophical knowledge; and dating itself astronomically from no further back than 2170 B.C., the author thought that the continued study of this Great Pyramid building was likely to shed a most unexpected amount of light on the doctrine of miracles in general; as well as on what was the manner of appearance of the first traces of a high order of human civilisation in the present post-diluvian history of the world.

Monday, 6th January 1868.

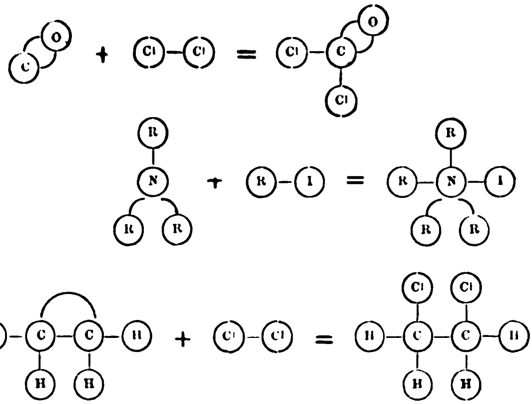
Dr CHRISTISON, Vice-President, in the Chair.

The Chairman, in presenting the Keith Medal to Professor Piazzi Smyth, read the following Minute of Council relative to the award of the prize:—"The Committee, having examined the papers presented to the Society within the last biennial period, resolve to recommend the Council to award the Keith prize to the communication from Professor Piazzi Smyth, entitled 'Notes of Recent Measurements at the Great Pyramid;' and the Committee desire to record their opinion that the energy, self-sacrifice, and skill displayed by Professor Smyth in that vast series of measurements, the details of which are appended to the second volume of his 'Life and Work at the Great Pyramid,' fully entitle him to the highest testimony which the Council has in its power to confer." The Chairman then handed the Keith Medal to Professor Smyth, remarking that the Council offered no opinion on the important theories he had enunciated, but recognised the valuable character of the measurements he had made.

The following Communications were read:—

1. On the Changes produced by direct Chemical Addition on the Physiological Action of certain Poisons. By Drs A. Crum Brown and Thomas R. Fraser.

In this paper the authors communicate the first results of an attempt to discover the relation which must exist between the chemical constitution and the physiological action of a substance. As the chemical constitution of the majority of physiologically active substances is unknown, they investigate the subject by examining the physiological action of a substance before and after the performance upon it of a definite chemical operation, introducing a known change into its constitution. The question is thus reduced to a problem in what may be called a chemico-physical calculus of finite variations. Thus, if the chemical constitution be represented by C, the physiological action, P, is some unknown function of C, say fC. In order to find f a change is produced on C, by which it becomes $C + \Delta C$, and the corresponding change of physiological action from fC to $fC + \Delta f \cdot C$ is investigated. We here know ΔC, fC, and $\Delta f \cdot C$; and by finding their relations for a sufficiently large number of values of C (even where these are unknown), and, by varying ΔC, the function f may be determinded. The change of constitution represented by ΔC must be a simple and unambiguous one. There are two kinds of operation to choose between —replacement and addition; and the authors select as the first subject of inquiry the effect of addition—that is, such chemical change as increases the active atomicity of atoms or radicals. The following may be given as simple examples of such operations:—



Their reason for this selection is, that, as far as it is possible to judge from previously known facts, replacement does not produce nearly so great a change of physiological action as addition does. Thus, comparing the action of carbonic oxide and carbonic acid, hydrocyanic acid and methylamine, arsenious and kakodylic acids, strychnia and brucia, and the salts of the ammonium bases derived from them, it may be seen that addition, in many cases at least, diminishes or removes physiological activity. This comparison leads to a suspicion that physiogical activity is in some way connected with *chemical condensation*, by which term the authors mean susceptibility of addition, whether the addition takes place by the increase of the atomicity of an atom, or of a group of atoms. This suspicion receives some confirmation from the fact, that such of the stable combinations of pentatomic arsenic and antimony, as have been examined physiologically, are stated to be inert, while all the soluble compounds of triatomic arsenic and antimony are active; similarly, the aromatic bodies are, as a rule, more active than the corresponding fatty bodies. The occurrence, however, of such poisons as alcohol, oxalic acid, and corrosive sublimate among saturated substances, and of comparatively inert condensed compounds, such as benzoic acid and salicine, shows that condensation is not the only condition of physiological activity.

The statements of Stahlschmidt and Schroff, in reference to the action of the salts of methyl strychnia, induced the authors to turn their attention, in the first place, to the effect of the addition of iodide of methyl to the natural alkaloids. As the iodides of the complex ammoniums thus produced are, in most cases, sparingly soluble in water, they have also examined the action of the corresponding sulphates.

The poisonous alkaloids thus examined, and included in this paper, are strychnia, brucia, thebaia, codeia, morphia, and nicotia. The authors give details of the processes followed in obtaining the iodide and the sulphate of the methyl-derivatives of these bases, and they describe their physiological effects.

Twelve grains of iodide of methyl-strychnium,* subcutaneously administered, produced no effect on a rabbit, weighing three pounds. Fifteen grains were recovered from, after symptoms; and twenty grains was a fatal dose. When exhibited by the stomach, twenty grains of this compound did not cause any symptom; while the same rabbit was rapidly killed by one-tenth of a grain of strychnia, given in exactly the same way. Twenty grains of iodide of methyl-strychnium contain about fourteen grains of strychnia.

The sulphate of methyl-strychnium, being a much more soluble salt than the iodide, was found to have a much smaller poisonous dose. One grain was fatal to a rabbit, by subcutaneous exhibition. Eight-tenths of a grain were recovered from, while five-tenths did not cause any symptom. The rabbit that recovered after the administration of eight-tenths of a grain of sulphate of methyl-strychnium, died shortly after one-twentieth of a grain of strychnia was injected under the skin.

Both the iodide and the sulphate of methyl-strychnium produced symptoms altogether different from those of strychnia. There were no convulsions, nor was there the slightest exaggeration of the reflex function; the symptoms were those of paralysis, and death was produced by the asphyxia that this occasioned. The authors further investigated this action by localised poisoning in frogs; and they have demonstrated that iodide and sulphate of methyl-strychnium paralyse the peripheral terminations (end-organs) of the motor nerves, and, therefore, possess exactly the same action as curare (wourali) has.

Brucia and thebaia act in the same way as strychnia, and it was

* The action of iodide of ethyl-strychnium was also examined, and found to be the same as that of iodide of methyl-strychnium.

found that iodide and sulphate of methyl-brucium and methyl-thebaium have the same action as the analogous strychnia compounds. The fatal dose of iodide of methyl-brucium was found to be very much the same as that of the corresponding compound of strychnia; a larger dose was, however, necessary to produce death with sulphate of methyl-brucium, than with sulphate of methyl-strychnium. Iodide of methyl-thebaium, being more soluble in warm water, has a smaller fatal dose than the iodides of methyl-strychnium and methyl-brucium. Six grains produced no effect when injected under the skin of a rabbit; eight grains caused symptoms, which were recovered from; and death occurred eleven minutes after the injection of ten grains. Eight grains of iodide of methyl-thebaium contained about five grains and a half of thebaia, and, for a rabbit, the fatal dose of this alkaloid is one-fifth of a grain.

Among the opium alkaloids, codeia ranks next to thebaia in activity. It was found by the authors, that six grains of iodide of methyl-codeium dissolved in warm warer, and injected under the skin of a rabbit, caused no effect. Ten grains, however, was an almost fatal dose, and this contains about twelve times as much codeia as would kill a rabbit. It was also found that the fatal dose of sulphate of methyl-codeium is not very different from that of the iodide. Neither of these compounds possess the usual convulsant action of codeia; and as this alkaloid has but a feeble soporific action, it was difficult to determine how far this was modified by the direct chemical addition of methyl compounds. The author also found that iodide and sulphate of methyl-codeium paralyse the motor nerve end-organs, an effect that is not produced by codeia itself.

Iodide of methyl-morphium is a very insoluble substance. The largest dose that could, therefore, be administered subcutaneously to a rabbit was twenty grains, and this large quantity produced no effect. Eight grains of morphia were, some days afterwards, exhibited in the same way to this rabbit; the result was a decided soporific effect, followed by epileptiform convulsions and death.

No effect was produced when iodide of methyl-morphium was administered to rabbits by the stomach, even in so large a dose as thirty grains.

Recognising the possible fallacies connected with experiments with such a substance on rabbits, the authors determined to observe the effect on man. One of themselves, accordingly, took, on one occasion, half a grain of iodide of methyl-morphium as a powder, and on another, one grain (containing about three-fourths of a grain of morphia), but on neither occasion was there observed the slightest soporific or other effect.

Four grains of sulphate of methyl-morphium produced decided narcotism on a rabbit, but no convulsive effect. Indeed, with this dose, and with various others that were given, paralysis appeared, and the authors have demonstrated that this symptom is due to an effect on the motor nerve end-organs.

Iodide of methyl-nicotium was obtained in the form of crystals extremely soluble in cold water. When given to rabbits by subcutaneous injection, a dose of five grains was perfectly inert; one of fifteen grains produced serious symptoms, followed by recovery: and a dose of twenty grains was fatal. The symptoms were principally distinguished from those of nicotia by the absence of convulsions; but no paralytic action on motor nerve end-organs was caused.

The authors have also investigated the action of iodide of methyl, and they obtained no evidence in support of the extremely improbable hypothesis, that some of the changes produced in the action of the substances they had examined might have been due to the addition of the physiological action of the methyl compounds. They conclude by discussing the possible causes of these modifications, by pointing out some of the practical applications of their results, and by promising to examine how far iodide of methyl may prove an antidote to the poisonous effects of these vegetable alkaloids, whose fatal dose it increases.

2. On the Burning Mirrors of Archimedes, and on the Concentration of Light produced by Reflectors. By John Scott, Esq. Communicated by Professor Kelland.

That the Roman ships were destroyed by burning glasses invented by Archimedes, is mentioned as a fact by most of the ancient writers, especially those who treat on mechanics, and their statements have been repeated by succeeding authors, without any doubts having been suggested until comparatively recent times. The earliest authorities on the subject are Diodorus Siculus, Lucian, Galen, Dion Cassius, and Pappus. It is much to be regretted that a work by the last named author on the Siege of Syracuse is now lost; but Zonares and Tzetzes, writers of the 12th century, in whose time it was extant, give quotations from it. That of the latter, translated pretty literally, runs thus:—" When Marcellus had placed the ships a bow shot off, the old man (Archimedes) constructed a sort of hexagonal mirror. He placed at proper distances from the mirror other smaller mirrors of the same kind, which were moved by means of their hinges and certain plates of metal. He placed it amid the rays of the sun at noon, both in summer and winter. The rays being reflected by this, a frightful fiery kindling was excited on the ships, and it reduced them to ashes, from the distance of a bow shot. Thus the old man baffled Marcellus, by means of his inventions."

At a later period, mirrors similar to that of Archimedes, appear to have engaged the attention of Baron Napier of Merchiston, and other mathematicians; but strange enough, the famous naturalist, Buffon, was the first to establish the practicability, and therefore the probability, of the achievement. He employed a combination of plane reflectors, consisting of ordinary looking-glasses, eight inches by six, attached to a single frame. With forty of these glasses he set on fire tarred beech at a distance of 66 feet. A plank smeared with tar and brimstone, was ignited at 126 feet, by 98 glasses. A combination of 128 glasses, with a clear sun, inflamed very suddenly a plank of tarred fir at 150 feet. In addition to these experiments made at Paris, about the beginning of April, others were made in summer, by which wood was kindled at 200 and 210 feet, and silver and other metals were melted at distances varying from 25 to 40 feet.

So much for the positive side of the question, let us now briefly consider the negative side.

Descartes and others treated the whole affair of the burning mirrors as a fiction, on the gratuitous assumption that the specula employed were single parabolic reflectors. A different and more valid reason for doubt has arisen from the circumstance, that

Polybius, Livy, and Plutarch, are silent respecting the burning mirrors, although they describe some of the other contrivances employed by Archimedes against the assailants. In reply to this objection, it has been stated, as an admitted axiom in estimating historical evidence, that the silence of one author is not sufficient to disprove an event of which another, even subsequent in point of time, has given a plain and consistent narrative. Besides, similar omissions by ancient historians are not unfrequent; and in this instance, it is the less surprising, since the invention had perished with the fall of Syracuse, and the death of the illustrious inventor, who, in the peculiar circumstances, must have endeavoured to keep from his contemporaries the secret of its construction.

Returning to Buffon's experiments, Peyrard appends to his translation of the works of Archimedes a memoir of his own, pointing out the defects of Buffon's combination of reflectors, viz.:—That the number of assistants required to adjust so many mirrors were apt to confuse one another—the operation itself was a tedious one, taking about half an hour for one hundred and sixty-eight mirrors—and, after all, the superposition of the light was imperfect. Again, after a short interval, a readjustment would be rendered necessary by the angular deviation of the rays, arising from the varying position of the sun. Peyrard suggests "that each mirror should be furnished with a telescope, and a somewhat complex apparatus, to render the adjustment more speedy and accurate, and by this means to diminish the number of assistants required. This remedy is admitted to be only partial. It is proposed, in what follows, to obviate the forementioned defects, by producing a solar reflecting machine, capable of being directed by one eye, guided by a single hand, and coinciding in its construction with the description in the fragment preserved by Tzetzes from Pappus.

It is stated that Archimedes contrived a hexagonal mirror, and placed at proper distances from it other smaller ones of the same kind. This peculiarity has no parallel in that of Buffon. Articles 11, 12, and 13, in the memoir, show how burning mirrors can be constructed, corresponding to the above description, free from the defects of Buffon, and capable, moreover, of darting the consuming rays instantaneously in any given direction, thereby affording a strong presumption that the accounts respecting the Archimedian Mirror are in the main authentic.

3. On the Great Pyramid of Gizeh, and Professor C. P. Smyth's Views concerning it. By A. D. Wackerbarth, A.M., of Upsala. Communicated by Professor Kelland.

The author gives a detailed statement of the theories of Professor Smyth, as given in the Transactions of this Society, Vol. XXIII. Part III. He then, after heartily commending the zeal and diligence of the Professor, brings forward objections to some of his views. 1. As to the metron or unit of linear measure. Mr Wackerbarth objects that this measure was utterly unknown to the ancient Egyptians—appearing in no Egyptian document or monument whatever, nor in any ancient writer who describes the condition of the Egyptians. Mr Wackerbarth collects from the materials most accessible the following table of weights and measures in use amongst the ancient Egyptians (corrected for the recent measure of the cubit of Karnak, made by Sir Henry James).

Long Measure.									English Inches.
1. ⲧⲏⲃ (Digitus),									0·73925
2	1. Κόνδυλος,								1·4785
4	2	1. ϣⲟⲡ (Palmus),							2·957
5	2½	1¼	1. ϭⲛⲁⲧϩ (Vola),						3·69625
6	3	1½	1⅕	1. Kubdeh,					4·4355
8	4	2	1⅗	1⅓	1. Διχάς,				5·914
11	5½	2¾	2⅕	1⅚	1⅜	1. Fitr,			8·13175
13	6½	3¼	2⅗	2⅙	1⅝	1 2/11	1. ⲉⲣⲧⲱ (Σπιθαμή),		9·61925
28	14	7	5⅗	4⅔	3½	2 6/11	2 2/13	1. ⲙⲙⲁϩⲓ (cubit)	20·699

Itinerary Measure.

1. ϩⲟⲟⲩⲧⲛ (Στάδιον),			606·875 ft. Eng., or 623 ft. Swedish.
60	1. ⲛⲟϩ ⲛ̀ⲡⲱϣ (Σχοῖνος),		6·9 English miles, or 1 Norwegian mile = 1/16 degree of equator.
150	2½	1. ⲁⲟⲩⲣ	17·2 English miles, or 2½ Norweg. miles = ¼ degree.

Land Measure.

1. Ἄρουρα = $(100 \text{ cubits})^2$ = 10,000 square cubits = 0·683 English acres = 0·557 Sw. Tunnaland.

Wine Measure.

1. Ἀρυστὴρ = about ½ pint English, or about $\frac{1}{16}$ Swedish kanna.

Weight.

1. ⲉⲙⲛⲁ (Mina) = 8304 grains = 1·1863 lb. av., or 1·278 Swedish Skålpund.

2. The author then, finding no such measure as the metron, goes on to say:—

"It is really very unpleasant to reject a beautiful and ingeniously carried out theory which one wishes to believe, but I cannot, however, arrive at any other conclusion than that Professor Smyth's very interesting account of an ancient Egyptian standard of length, exactly equal to a ten-millionth of the earth's axis of rotation, and the entire system of weights and measures that he has deduced from this imaginary unit, requiring accurate knowledge of the earth's dimensions, figure, and density, several centuries before the age of Abraham, and the supposed connection of the English measures with these, are things purely mythical. On the other hand, that the pyramid and its contents were really the standards of the ancient Egyptian kingdom, is, or seems to me an opinion not destitute of probability; but I cannot think that those standards were constructed with any reference to the dimensions of the earth, but that they were arbitrarily chosen quantities, intended to represent on a tolerably large scale the size of those portions of the human body which their names indicate: *e.g.*, ⲙⲁϩⲓ (*cubitus*), the arm from the elbow to the point of the middle finger; ϣⲟⲡ *palmus*), the breadth of the hand, &c., &c.

"In fine, Professor Smyth's method, namely, that of multiplying or dividing by quantities, for the introduction of which it would be hard to give any satisfactory reason,—as, for example, the number 366, with which the base was divided, and which is neither the exact number of days in a sidereal year, nor anything else, and again, the very uncertain number, 5,672 (the earth's density), —appears to me in the highest degree dangerous. How deceitful results thus obtained can be, I shall now, in conclusion of this discussion, attempt to illustrate by a few examples; and before doing this, I beg to state that, entertaining the highest respect for Professor Smyth, I have not the least intention of making light of his work, though this method of reasoning, being analogous to the *reductio ad absurdum*, will unavoidably at times have something of that appearance.

"*Ex.* 1. It is a historical fact, that the present English weights and measures are not of any very great antiquity. At the time of the Norman conquest the yard was about 39·6 of the present English inches, a little longer than the French mètre; and the foot, accordingly, 13·2 modern inches, a little greater than the Paris foot. In the year 1101, King Henry I. determined the yard by the length of his own arm, and that is the determination which the present yard is intended to represent; and that yard, moreover, has never been, even in modern times, defined by any fraction of any of the earth's dimensions, but by its proportion to the length of the seconds' pendulum at 51½° latitude, and of this yard the English foot is the third part. Now, a degree of the equator is just 365,260·524 ft. Divide a thousandth part of that number by the length of the sidereal year in solar days, 365·256358, and we have 1·0000114; that is to say, if we take a thousandth of a degree of the equator, and divide it by the number of days in a sidereal year, we have an English foot as nearly as a powerful microscope can determine it. *And yet it is certain that this is purely accidental.*

"*Ex.* 2. If I take 10,000 times e, the base of the hyperbolic logarithms, and multiply it into the quantity, which in the lunar theory is called g,—that is, the ratio of the difference between the moon's and its ascending node's mean motions to the moon's mean motion,—and divide the earth's polar radius by the product, the result is the length of the pyramid's side. But are we to suppose that the Egyptians forty centuries ago were acquainted with the lunar theory, the earth's compression, and the use of logarithms, and, moreover, took this clumsy method of perpetuating their knowledge? Is it not far more probable that the architect simply determined that the length of the side should be 444 cubits, and accordingly measured out that distance?

"*Ex.* 3. Again, if instead of taking Colonel Vyse's measure of the base, we accept for that quantity the mean of half a dozen of the most trustworthy travellers' statements,—for instance, Vyse's, the French Academicians', Caviglia's, Wilkinson's, Lane's, and Davison's,—we have for the length of the side in English feet a number which expresses in millimétres the mean height of the barometer at Upsala. But will any one maintain that the dimensions of the pyramid were intended by its builders to have any reference whatever to that interesting constant?

"*Ex.* 4. If we multiply together one-tenth of the side of the pyramid's base, the length of the line joining the middle of the side and the apex (that is, the height of each one of the four isosceles triangles that compose the pyramid), and the modulus for the common logarithms, the result is 3420, the constant of lunar parallax, for which, in Burg's tables, the value 3420″·96 is given.

"*Ex.* 5. Lastly, if the side of the base (763·81) be divided by the hyperbolic logarithm of π (the circumference to diameter), and that quotient again by the ratio of the force of gravity at London to the force of gravity at the pyramid (lat. = 30°), (1·00188), the result is 666!"

4. On the best Arrangement for producing a Pure Spectrum on a Screen. By J. Clerk Maxwell, M.A., F.R.SS. L. & E.

In experiments on the spectrum, it is usual to employ a slit through which the light is admitted, a prism to analyse the light, and one or more lenses to bring the rays of each distinct kind to a distinct focus on the screen. The most perfect arrangement is that adopted by M. Kirchhoff, in which two achromatic lenses are used, one before and the other after the passage of the light through the prism, so that every pencil consists of parallel rays while passing through the prism.

But when the observer has not achromatic lenses at his command, or when, as in the case of the highly refrangible rays, or the rays of heat, he is restricted in the use of materials, it may still be useful to be able to place the lenses and prism in such a way as to bring the rays of all colours to their foci at approximately the same distance from the prism.

We shall first examine the effect of the prism in changing the convergency or divergency of the pencils passing through it, and then that of the lens, so that by combining the prism and the lens we may cause their defects to disappear.

When a pencil of light is refracted through a plane surface, its convergency or divergency is less in that medium which has the greatest refractive index; and this change is greater as the angle of incidence is greater, and also as the index of refraction is greater.

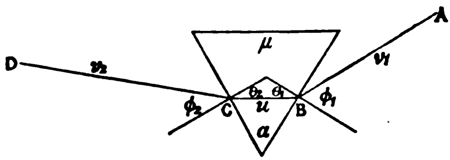
When the pencil passes through a prism its convergency or divergency will be diminished as it enters, and will be increased when it emerges, and the emergent pencil will be more or less convergent or divergent than the incident one, according as the angle of emergence is greater or less than that of incidence. This effect will increase with the difference of these angles and with the refractive index.

When the angle of incidence is equal to that of emergence the convergence of the pencil is unaltered, but since the more refrangible rays have the greatest angle of emergence, their convergency or divergency will be greater than that of the less refrangible rays.

This effect will be increased by making the angle of incidence less, and that of emergence greater; and it will be diminished by increasing the angle of incidence, that is, by turning the prism round its edge towards the slit. If the angle of the prism is not too great, the convergency or divergency of all the pencils may be made the same (approximately) by turning the prism in this way.

This correction, however, diminishes the separation of the colours. It is inapplicable to a prism of large angle, and it takes no account of the chromatic aberration of the lens. By altering the arrangement, the lens may be made to correct the prism. The effect of a convex lens is to increase the convergency, and to diminish the divergency, of every pencil; but the change is greatest on the most refrangible rays. The prism, except when its base is very much turned towards the slit, makes the highly refrangible rays more convergent or more divergent than the less refrangible rays, according as they were convergent or divergent originally. If the rays pass through the prism before they reach the lens, the pencils will be divergent at incidence, and the more refrangible will be most divergent at emergence. If they then fall on the lens, they will be more converged than the rest; so that by a proper arrangement all may be brought to their foci at approximately the same distance. If the violet rays come to their focus first, we must turn the base of the prism more towards the light, and *vice versa.*

We proceed to the numerical calculation of the proper arrangement.



To find the variation of position of the focus of light passed through a prism as dependent on the nature of the light.

Let μ be the index of refraction of the prism, a its angle, ϕ_1 and ϕ_2 the angles of incidence and emergence, θ_1 and θ_2 the angles of the ray within the prism with the normals to the first and second surfaces, δ the difference of these angles—then by geometry

$$\theta_1 + \theta_2 = a \qquad \theta_1 - \theta_2 = \delta$$

and by the law of refraction,

$$\sin\phi_1 = \mu\sin\theta_1 \qquad \sin\phi_2 = \mu\sin\theta_2$$

ϕ_1 is constant, being the angle of incidence for all kinds of light, but the other angles vary with μ, so that

$$\frac{d\theta_1}{d\mu} = -\frac{\sin\theta_1}{\mu\cos\theta_1} \qquad \frac{d\theta_2}{d\mu} = \frac{\sin\theta_1}{\mu\cos\theta_1} \qquad \frac{d\phi_2}{d\mu} = \frac{\sin a}{\cos\theta_1\cos\phi_2}$$

The last expression gives the dispersion, or breadth of the spectrum, and shows that it increases as the base of the prism is turned from the light.

As the slit is parallel to the edge of the prism, we have only to consider the primary foci of the pencils when we wish to render the image distinct.

Let v_1 be the distance of the focus of incident light from the prism, v_2 that of the emergent light, and u that within the prism, all measured to the right, then by the ordinary formula,

$$\frac{v_1}{\cos^2\phi_1} = \frac{u}{\mu\cos^2\theta_1} \qquad \frac{u}{\mu\cos^2\theta_2} = \frac{v_2}{\cos^2\phi_2}$$

$$v_1\cos^2\theta_1\cos^2\phi_2 = v_2\cos^2\theta_2\cos^2\phi_1 .$$

Taking the differential coefficient of the logarithms of these quantities,

$$\frac{1}{v_1}\frac{dv_1}{d\mu} - \frac{2\sin\theta_1}{\cos\theta_1}\frac{d\theta_1}{d\mu} - \frac{2\sin\phi_2}{\cos\phi_2}\frac{d\phi_2}{d\mu} = \frac{1}{v_2}\frac{dv_2}{d\mu} - \frac{2\sin\theta_2}{\cos\theta_2}\frac{d\theta_2}{d\mu}$$

$$\frac{1}{v_1}\frac{dv_1}{d\mu} - \frac{1}{v_2}\frac{dv_2}{d\mu} = \frac{2\sin\varphi_2\sin a}{\cos\theta_1\cos^2\varphi_2} - \frac{2\sin^2\theta_1}{\mu\cos^2\theta_1} - \frac{2\sin\theta_1\sin\theta_2}{\mu\cos\theta_1\cos\theta_2}$$

Substituting for these angles their values in terms of a and δ we find—

$$\frac{1}{v_1}\frac{dv_1}{d\mu} - \frac{1}{v_2}\frac{dv_2}{d\mu}$$

$$= \frac{4\sin a}{(\cos a + \cos\delta)^2} \cdot \frac{(\mu^2 - 1)\sin a - \sin\delta\,(1 + \mu^2\cos a - \delta)}{1 - \frac{1}{2}\mu^2\left(1 - \cos(a - \delta)\right)}$$

The quantity on the right of this equation is always positive, unless the value of δ exceeds that given by the equation

$$(\mu^2 - 1)\sin a = \sin\delta\left(1 + \mu^2\cos(a - \delta)\right)$$

If $\mu = 1{\cdot}5$ and $a = 60°$, then $\delta = 22°\ 52'$, that is, the ray within the prism makes an angle of $11°\ 26'$ with the base, which corresponds to an angle of incidence $82°\ 50'$, or the incident ray is inclined $7°\ 10'$ to the face of the prism.

If two lenses are used, of the same material with the prism, we may correct the defects of the prism without turning it so far from its position of least deviation.

Let a be the distance of the slit from the prism, and b that of the screen from the prism. Let f_1 be the focal length of the lens between the prism and slit, and f_2 that of the lens between the prism and the screen, then the condition of a flat image is

$$\frac{1}{v_1}\frac{dv_1}{d\mu} - \frac{1}{v_2}\frac{dv_2}{d\mu} = \frac{1}{\mu-1}\left(\frac{v_1}{f_1} + \frac{v_2}{f_2}\right)$$

Let us first find the conditions of a flat spectrum $\delta = o$. When $\delta = o$, $v_1 = v_2$, and we obtain the conditions

$$\frac{1}{a}-\frac{1}{f_1}=\frac{1}{f_2}-\frac{1}{b}=\left(\frac{1}{a}+\frac{1}{b}\right)\frac{(1+\cos a)^2\left(1-\frac{1}{2}\mu^2(1-\cos a)\right)}{4(\mu-1)^2(\mu+1)\sin^2 a}=\left(\frac{1}{a}+\frac{1}{b}\right)c$$

from which f_1 and f_2 may be found in terms of known quantities.

$$\frac{1}{f}=\frac{1}{a}(1-c)-\frac{1}{b}c$$

$$\frac{1}{f_2}=\frac{1}{b}(1+c)+\frac{1}{a}c$$

When a, the angle of the prism, is 60°, then

$$c=\frac{3(4-\mu^2)}{16(\mu-1)^2(\mu+1)}$$

If we now make $\mu=1{\cdot}5$ we find $c={\cdot}5025$, and

$$\frac{1}{f_1}=\frac{{\cdot}4975}{a}-\frac{{\cdot}5025}{b}$$

$$\frac{1}{f_2}=\frac{1{\cdot}5025}{b}+\frac{{\cdot}5025}{a}$$

If $a=\frac{19}{21}b$, the first lens may be dispensed with, and the second lens will correct the defects of the prism.

If a is greater in proportion to b, a concave lens must be placed in front of the prism. The most convenient arrangement will be that in which the prism is placed in the position of least deviation, and the lens placed between the prism and the screen, while the distance from the slit to the prism is to that between the prism and the screen as $1-c$ is to c. For quartz, in which $\mu=1{\cdot}584$ for the ordinary ray, $\frac{1}{c}=2{\cdot}53$, so that the best arrangement is $a=1{\cdot}53\,b$, or the lens should be placed on the side next the screen, and the distance from the slit which admits the light to the prism should be about one and a half times the distance from the prism to the screen.

The following Gentlemen were admitted Fellows of the Society:—

Rev. David Aitken, D.D.
Robert M. Ferguson, Ph.D.

Monday, 20*th January* 1868.

Professor KELLAND, Vice-President in the Chair.

The following Communications were read:—

1. Pyramidal Structures in Egypt and elsewhere; and the Objects of their Erection. By Sir J. Y. Simpson, Bart.

After considering the many proposed derivations of the word Pyramid, it was pointed out that the origin of the name suggested by the distinguished Egyptologist, Mr Birch, from two Coptic words, "*pouro*," "the king," and "*emahau*" or "*maha*," "tomb," —the two in combination signifying "the king's tomb,"—was probably correct. "*Men*," in Coptic, signifies "monument," "memorial;" and "*pouro-men*," or "king's monument" may possibly also be the original form of the word. Various authors, as Pope, Pownall, Daniel Wilson, Burton, had long applied the term pyramid to the larger forms of conical and round sepulchral mounds, cairns, or barrows—such as are found in Ireland, Brittany, Orkney, &c., and in numerous districts of the New World as well as the Old; and which are all characterised by containing in their interior, chambers or cells, constructed usually of large stones, and with megalithic galleries leading into them. In these chambers (small in relation to the hills of stone or earth in which they were imbedded) were found the remains of sepulture, with stone weapons, ornaments, &c. The galleries and chambers were roofed, sometimes with flags laid quite flat, or placed abutting against each other; and occasionally with large stones arranged over the internal cells in the form of a horizontal arch or dome. In his travels to Madeira and the Mediterranean (1840), Sir W. Wilde details in interesting terms his visit to the pyramids of Egypt; and in describing the roof of the interior chambers of one of the pyramids at Sakkara,* he remarks on the analogy of its construc-

* Sir J. Gardner Wilkinson thinks that the pyramids of Sakkara are probably older than the other groups of these structures, as those of Gizeh or the Great Pyramid.—See Rawlinson's *Herodotus*, vol. ii. chapter viii.

tion to the great barrow of Dowth in Ireland; and again, when writing—in his work on the Beauties of the Boyne (1849)—an account of the great old barrows of Dowth, New Grange, &c., placed on its banks above Drogheda, he describes at some length the last of these mounds (New Grange), stating that it "consists" of an enormous cairn or "hill of small stones, calculated at 180,000 tons weight, occupying the summit of one of the natural undulating slopes which enclose the valley of the Boyne upon the north. It is said to cover nearly two acres, and is 400 paces in circumference, and now about 80 feet higher than the adjoining natural surface. Various excavations (he adds) made into its sides and upon its summit, at different times, in order to supply materials for building and road-making, have assisted to lessen its original height, and also to destroy the beauty of its outline." Like the other analogous mounds and pyramids placed there and elsewhere, New Grange has a long megalithic gallery, of above 60 feet in length, leading inward into three dome-shaped chambers or crypts. After describing minutely, and with a master-hand, the construction of these interior parts, and the carvings of circles, spirals, &c., upon the enormous stones of which they are built, Sir William Wilde goes on to observe:—"We believe with most modern investigators into such subjects, that it was a tomb, or great sepulchral Pyramid, similar in every respect to those now standing by the banks of the Nile, from Dashour to Gizeh, each consisting of a great central chamber containing one or more sarcophagi, entered by a long stone-covered passage. The external aperture was concealed, and the whole covered with a great mound of stones or earth in a conical form. The early Egyptians, and the Mexicans also, possessing greater art and better tools than the primitive Irish, carved, smoothed, and cemented their great pyramids; *but the type and purpose is all the same.* . . . How far anterior to the Christian era its date should be placed, would be a matter of speculation; it may be of an age coeval, or even anterior, to its brethren on the Nile." Other pyramidal barrows at Maeshowe, Gavr Inis, &c., were referred to and illustrated; showing that a gigantic sepulchral cairn was in its mass an unbuilt pyramid, or, in other words, that a pyramid was a built cairn.

Sepulchral Character, &c., of the Egyptian Pyramids.

All authors, from the father of history downwards, have generally agreed in considering the pyramids of Egypt as magnificent and regal sepulchres; and the sarcophagi, &c., of the dead have been found in them when first opened for the purposes of plunder or curiosity. The pyramidal sepulchral mounds on the banks of the Boyne were opened and rifled in the ninth century by the invading Dane, as told in different old Irish annals; and the Great Pyramid of Gizeh, &c., is said to have been broken into and harried in the same century by the Arabian Caliph, El Mamoon. This, the largest and most gigantic of the many pyramids of Egypt, had been calculated by Major Forlong, Asso. Inst. C. Engrs., as a structure which in the East would cost about L.1,000,000. Over India and the East generally, enormous sums had often been expended on royal sepulchres. The Taj Mahal of Agra, built by the Emperor Shah Jahan for his favourite queen, cost perhaps double or triple this money; and yet it formed only a portion of an intended larger mausoleum which he expected to rear for himself. The Great Pyramid contains in its interior, and directly over the King's Chamber, five entresols or "chambers of construction," as they have been termed, intended apparently to take off the enormous weight of masonry from the cross stones forming the roof of the King's Chamber itself. These entresols are chambers, small and unpolished, and never intended to be opened. But in the uppermost of them, opened by Colonel Howard Vyse, a most interesting discovery was made about thirty years ago. The surfaces of some of the stones were found painted over in red ochre or paint with rudish hieroglyphics—being quarry marks written on the stones 4000 years ago, and hence, perhaps, forming the oldest preserved writing in the world. Among these accidental hieroglyphics Mr Birch discovered two royal ovals, viz., Shufu (the Cheops of Herodotus) and Nu Shufu—"a brother" (writes Professor Smyth) "of Shufu, also a king and a co-regent with him." Most, if not all, of the other pyramids are believed to have been erected by individual kings during their individual or separate reigns. If these hieroglyphics proved that two kings were connected with the building of the Great Pyramid, that circumstance would perhaps account for its size and the duplicity and position of its sepulchral chambers.* The Great Pyramid is now totally deprived of the external polished limestone coating which covered it at the time of Herodotus' visit, some twenty-two centuries ago; and "now (writes Mr Smyth) is so injured as to be in the eyes of some passing travellers little better than a heap of stones." But all the internal built core of the magnificent structure remains, and contains in its interior (beside a rock chamber below) two higher built chambers or crypts above—the so-called King's Chamber and Queen's Chamber—with galleries and apartments leading to them. The walls of these galleries and upper chambers are built with granite and limestone masonry of a highly-finished character. The pyramid standing next the Great Pyramid, and nearly of equal size, is said by Herodotus to have been raised by the brother of Cheops. The other pyramids at Gizeh are usually regarded as later in date. But the exact era of the reign or reigns of their builders has not as yet been determined, in consequence of the break made in Egyptian chronology by the invasion of the Shepherd Kings.

In their mode of building, the pyramids of Gizeh, &c., are all similar. "There is nothing" (observes Professor Smyth) "in the stone-upon-stone composition of the Great Pyramid which speaks of the mere building problem to be solved there, as being of a different character or requiring inventions by man of any absolutely higher order, than elsewhere." But the Great Pyramid has been imagined to contain some hidden symbols and meanings. For "it is the manner of the Pyramid" (according to Professor Smyth) "not to wear its most vital truths in prominent outside positions."

Alleged Metrological Object of the Great Pyramid.

By several authorities the largest† of the group of pyramids

* Mr Birch, however—and it is impossible to cite a higher authority in such a question—holds the cartouches of Shufu and Nu Shufu to refer only to one personage—namely, the Cheops of Herodotus; and, believing with Mr Wilde and Professor Lepsius, that the pyramids were as royal sepulchres built and methodically extended and enlarged as the reigns of their intended occupants lengthened out, he ascribes the unusual size of the Great Pyramid to the unusual length—as testified by Manetho, &c.—of the reign of Cheops; the erection of a sepulchral chamber in its built portion being, perhaps, in consequence of some ascertained deficiency in the rock chamber or gallery below.

† The Mexican Pyramid of Cholula has a base of more than 1420 feet, and is hence about twice the length of the basis of the Great Pyramid of Gizeh. See Prescott's *Conquest of Mexico*, book iii. chap. i., and book v. chap. iv.

at Gizeh, the "Great Pyramid," as it is often termed, has been maintained—and particularly of late by Gabb, Jomard, Taylor, and Professor Smyth—not to be a royal mausoleum, but to be a marvellous metrological monument, built some forty centuries ago, as "a necessarily material centre," to hold and contain within it, and in its structure, material standards, "in a practicable and reliable shape," "down to the ends of the world," as measures of length, capacity, weight, &c., for men and nations for all time—"a monument" (in the language of Professor Smyth) "devoted to weights and measures, not so much as a place of frequent reference for them, but one where the original standards were to be preserved for some thousands of years, safe from the vicissitudes of empires and the decay of nations." Messrs Taylor and Smyth further hold that this Great Pyramid was built for these purposes of mensuration, under Divine inspiration—the standards being, through superhuman origination and guidance, made and protected by it till they came to be understood and interpreted in these latter times. For, observes Professor Smyth, "the Great Pyramid was a sealed book to all the world *until* this present day, when modern science, aided in part by the dilapidation of the building and the structural features thereby opened up—has at length been able to assign the chief interpretations." Professor Smyth has, in his remarkable devotedness and enthusiasm, lately measured most of the principal points in the Great Pyramid; and for the great zeal, labour, and ability which he has displayed in this self-imposed mission, the Society have very properly and justly bestowed upon him the Keith Medal. But the exactitude of the measures does not necessarily imply exactitude in the reasoning upon them; and on what grounds can it be possibly regarded as a metrological monument and not a sepulchre, is legitimately the subject of our present inquiry.

Mr Taylor ascribes to Noah the original idea of the metrological structure of the Great Pyramid. "To Noah" (observes Mr Taylor) "we must ascribe the original idea, the presiding mind, and the benevolent purpose. He who built the Ark was, of all men, the most competent to direct the building of the Great Pyramid. He was born 600 years before the Flood, and lived 350 years after that event, dying in the year 1998 B.C. Supposing the pyramids were commenced in 2160 B.C. (that is 4000 years ago), *they* were founded 168 years before the death of Noah. We are told "(Mr

Taylor continues) "that Noah was a 'preacher of righteousness,' but nothing could more illustrate this character of a preacher of righteousness after the Flood than that he should be the first to publish a system of weights and measures for the use of all mankind, based upon the measure of the earth." Professor Smyth, computing by another chronology, rejects the presence of Noah, and makes a shepherd—Philithion, alluded to by Herodotus*—the presiding and directing genius of the structure, holding him to be a Cushite skilled in building, and under whose inspired direction the pyramid rose, containing within it miraculous measures and standards of capacity, weight, length, heat, &c.

The Coffer in the King's Chamber in the Great Pyramid an Alleged Standard for Measures of Capacity.

A granite coffer, stone box, or sarcophagus standing in that interior cell of the pyramid called the King's Chamber, is held by Messrs Taylor and Smyth to have been hewn out and placed there as a measure of capacity for the world, so that the ancient Hebrew, Grecian, and Roman measures of capacity on the one hand, and our modern Anglo-Saxon on the other, are all derived directly or indirectly from the parent measurements of this granite vessel.

Various extracts† were read from the works of these authors, showing that, in their belief, the great object for which the whole pyramid was created, was the preservation of this coffer as a standard of measures. Further published accounts from Mr Taylor and from Mr Smyth's first work were adduced, averring that the coffer represented without and within a rectangular figure of mathemati-

* Herodotus states that the Egyptians detested the memories of the kings who built the two larger Pyramids, viz., Cheops and Cephren; and hence, he adds, "they commonly call the Pyramids after Philithion, a shepherd, who at that time fed his flocks about the place." They thus called the second, as well as the Great Pyramid, after him (iii. § 128).

† The extracts within inverted commas, here and in other parts, are from—(1.) Mr John Taylor's work entitled "The Great Pyramid—Why was it Built, and Who Built it?" London, 1859; and (2.) Professor Smyth's work, "Our Inheritance in the Great Pyramid," Edinburgh, 1864; (3.) his later three-volume work, "Life and Work at the Great Pyramid," Edinburgh, 1867; and (4.) "Recent Measures at the Great Pyramid" in the Transactions of the Royal Society of Edinburgh for 1865–66.

cal form and "exquisite geometric truth," perfectly level on its surface, and highly polished. "The chest or coffer in the Great Pyramid" (writes Mr Taylor in 1859,) "is so shaped as to be in every part rectangular from side to side, and from end to end, and the bottom is also cut at right angles to the sides and end, and made perfectly level." "The coffer," said Professor Smyth in 1864, "exhibits to us a standard measure of 4000 years ago, with the tenacity and hardness of its substance unimpaired, and the polish and evenness of its surface untouched by nature through all that length of time."

But later inquiries and observations upset entirely all these notions and strong averments in regard to the coffer. For—

(1.) *The Coffer, though an alleged actual standard of capacity-measure, has yet been found difficult or impossible to measure.*—In his first work, "Our Inheritance in the Great Pyramid," Professor Smyth had cited the measurements of it, made and published by twenty-five different observers, several of whom had gone about the matter with great mathematical accuracy. Though imagined to be a great standard of measure, yet all these twenty-five, as Professor Smyth owned, varied from each other in their accounts of this imaginary standard in "every element of length, breadth, and depth, both inside and outside." Professor Smyth has latterly measured it himself, and this twenty-sixth measurement varies again from all the preceding twenty-five. Surely a measure of capacity should be measurable; that ought to be its most unquestionable quality; but this imagined standard has proved virtually unmeasurable—in so far at least that its twenty-six different and skilled measurers all differ from each other in respect to its dimensions. Still, says Professor Smyth, "this affair of the coffer's precise size is *the* question of questions."

(2.) *Discordance between its actual and its theoretical measure.*—Professor Smyth holds that *theoretically* its capacity ought to be 71,250 "pyramidal" cubic inches, for that cubic size would make it the exact measure for a chaldron, or practically the vessel would then contain exactly four quarters of wheat, &c. Yet Professor Smyth himself found it some 60 cubic inches less than this; while also the measurements of Professor Greaves, one of the most accurate measurers of all, make it 250 cubic inches, and those of

Dr Whitman 14,000 *below* this professed standard. On the other hand, the measurements of Colonel Howard Vyse make it more than 100, those of Dr Wilson more than 500, and those of the French academicians who accompanied the Napoleonic expedition to Egypt, about 6000 cubic inches *above* the theoretical size which Professor Smyth has latterly fixed on.

(3.) *Its theoretical measure varied.*—The *actual* measure of the coffer has varied in the hands of all its twenty-six measurers. But even its *theoretical* measure is varied also, for the size which the old coffer really *ought* to have as "a grand capacity standard," is strangely enough not a determined quantity. In his last work (1867), Professor Smyth declares, as just stated ,its proper theoretical cubic capacity to be 71,250 pyramidal cubic inches. But in his first work (1864), he declared something different, for "we *elect*," says he, "to take 70,970·2 English cubic inches (or 70,900 pyramidal cubic inches) as the true, because the theoretically *proved* contents of the porphyry coffer, and therefore accept these numbers as giving the cubic size of the grand *standard* measure of capacity in the Great Pyramid." But again, Mr John Taylor, who, previously to Professor Smyth, was the great advocate of the coffer being a marvellous standard of capacity measure for all nations, ancient and modern, declares its measure to be neither the above quantities, but 71,328 cubic inches, or a cube of the ancient cubit of Karnak.* A vessel cannot be a measure of capacity whose own standard theoretical size is thus declared to vary somewhat every few years by those very men who maintain that it is a standard. But whether its capacity is 71,250, or 70,970, or 71,328, it is quite equally held up by Messrs Taylor and Smyth that the Sacred Laver of the Iraelites, and the Molten Sea of the Scriptures, "also conform and correspond to its (yet undetermined) standard with *all* conceivable practical exactness;" though the standard of capacity to which they thus "conform and correspond" is itself a size or standard which has not been yet fixed with any exactness. Professor Smyth, in speaking of the calculations and

* "Its contents," says Mr Taylor (p. 299), "are equal in cubic inches to the cube of 41,472 inches—the cubit of Karnak—viz., to 71,328 cubic inches." Elsewhere (p. 304) he states—"The Pyramid coffer contains 256 gallons of wheat ;"—"It also contains 256 gallons of water, &c."

theoretical dimensions of this coffer—as published by Mr Jopling, a believer in its wonderful standard character—critically observes, "Some very astonishing results were brought out in the play of arithmetical numerations."

(4.) *The dilapidation of the Coffer.*—Thirty years ago this stone coffer was pointed out and indeed delineated by Mr Perring as "*not* particularly well polished," and "chipped and broken at the edges." Professor Smyth, in his late travels to Egypt, states that he found every possible line and edge of it chipped away with large chips along the top, both inside and outside, "chip upon chip, wofully spoiling the original figure; along all the corners of the upright sides too, and even along every corner of the bottom, while the upper south-eastern corner of the whole vessel is positively broken away to a depth and breadth of nearly a third of the whole." Yet this stone vessel is professed to be the permanent miraculous standard of capacity measure for the world, and that it might serve this purpose it would be, according to Mr Taylor, "formed of one block of the hardest kind of material, such as porphyry or granite, *in order* that it might *not* fall into decay;" for "in this porphyry coffer we have (writes Professor Smyth in 1864) the very closing end and aim of the whole pyramid."

(5.) *Alleged mathematical form of the Coffer erroneous.*—But in regard to the coffer as an exquisite and marvellous standard of capacity to be revealed in these latter times, worse facts than these are divulged by the tables, &c., of measurements which Professor Smyth has recently published of this stone vessel or chest. His published measurements show that it is not at all a vessel, as was averred a few years ago, of pure mathematical form; for externally it is in length an inch greater on one side than another; in breadth, half-an-inch broader at one point than at some other point; its bottom at some points is nearly a whole inch thicker than it is at some other parts; and in thickness its sides vary in some points about a quarter of an inch near the top. "But," Professor Smyth adds, "if calipered lower down, it is extremely probable that a *notably* different thickness would have been found there," though it does not appear why they were not thus calipered. Further, externally "all the sides" (says Professor Smyth) "were slightly hollow, excepting the east side;" and the "two external ends" also show some "concavity" in form. "The workmanship" (he elsewhere describes) "of the inside is in advance of the outside, but yet *not* perfect." For internally there is a convergence at the bottom towards the centre; both in length and in breadth the interior differs about half-an-inch at one point from another point; the "extreme points (also) of the corners of the bottom not being perfectly worked out to the intersection of the general planes of the entire sides;" and thus its cavity seems really of a form utterly unmeasurable in a correct way by mere lineal measurement—the only measure yet attempted. If it were an object of the slightest moment, perhaps liquid measurements would be more successful.

(6.) *Coffer cut with ledges and catch-holes for a lid, like other sarcophagi.*—More damaging details still remain in relation to the coffer as "a grand standard measure of capacity," and proving that its object or function was very different. In his first work Prof. Smyth describes the coffer as showing no "symptoms" whatever of grooves or catchpins or other fastenings for a lid. "More modern accounts," he re-observes, "have been further precise in describing the smooth and geometrical finish of the upper part of the coffer's sides, *without any* of those grooves, dovetails, or steady-pinholes which have been found elsewhere in true polished sarcophagi, where the firm fastening of the lid is one of the most essential features of the whole business." Mr Perring, however, delineated the catchpin-holes for a lid in the coffer thirty years ago.* On his late visit to it Prof. Smyth found its western side lowered down in its whole extent to nearly an inch and three-quarters (or more exactly, 1·72 inch), and ledges cut out around the interior of the other sides at the same height. Should we measure on this western side from this actual ledge brim, or from the imaginary higher brim? If reckoned as the true brim, "this ledge" (according to Professor Smyth) would "take away near 4000 inches from the cubic capacity of the vessel." Besides, he found three holes cut on the top of the coffer's lowered western side, as in all the other Egyptian

* See Plate III. fig. 1, in his great folio work on the "Pyramids of Gizeh from Actual Survey and Admeasurement," Lond. 1839. "The sarcophagus is," he remarks, "of granite, not particularly well polished; at present it is chipped and broken at the edges. There are not any remains of the lid, *which was, however*, fitted on in the same manner as those of the other pyramids."

sarcophagi, where they are used with the ledge to admit and lock the lids of such stone chests.* In other words, it presents the usual ledge and apparatus pertaining to Egyptian stone sarcophagi, and served as such.

7. *Sepulchral contents of coffer when first discovered.*—When, about a thousand years ago, the Caliph Al Mamoon tunnelled into the interior of the pyramid, he detected by the accidental falling, it is said, of a granite portcullis, the passage to the King's Chamber, shut up from the building of the pyramid to that time. "Then" (to quote the words of Prof. Smyth) "the treasures of the pyramid, sealed up almost from the days of Noah, and undesecrated by mortal eye for 3000 years, lay full in their grasp before them." The Arabian historian Ibn Abd Alhokim states that on this occasion they found in the pyramid, "towards the top, a chamber [now the so-called King's Chamber] with an hollow stone [or coffer] in which there was a statue like a man, and within it a man upon whom was a breastplate of gold set with jewels; upon this breastplate was a sword of inestimable price, and at his head a carbuncle of the bigness of an egg, shining like the light of the day; and upon him were characters writ with a pen, which no man understood"†—a description stating, down to the so-called "statue," mummy-case, or cartonage, and the hieroglyphics upon the cerecloth, the arrangements now well known to belong to the higher class of Egyptian mummies.

* "The western side," observes Professor Smyth, "of the coffer is, through almost its entire length, rather lower than the other three, and these have *grooves* inside, or the remains of grooves once cut into them, about an inch or two below their summits and on a level with the western edge; *in fact*, to admit a sliding sarcophagus cover or lid; and there were the remains of three fixing pin-holes on the western side for fastening such cover into its place." (Vol. i. p. 85.)

† See Greaves' Works, vol. i. p. 61. Colonel Vyse adduces other Arabian authors who allude to this discovery of a body with golden armour, &c., &c., in the sarcophagus of the King's Chamber. He cites Alkaisi as testifying that "he himself saw the case (the cartonage or mummy-case) from which the body had been taken, and that it stood at the door of the King's Palace at Cairo, in the year 511" A.H. (See "The Pyramids of Gezah," vol. ii. p. 334. And also to the same effect Abon Szalt, p. 357.) "It may be remarked," observes Colonel Vyse, "that the Arabian authors have given the same accounts of the pyramids, with little or no variation, for above a thousand years" (vol. ii. p. 328).

In short (to quote the words of Prof. Smyth), "that wonder within a wonder of the Great Pyramid—viz., the porphyry coffer," "this vessel of exquisite meaning" and of "far-reaching characteristics," mathematically formed under alleged Divine inspiration as a measure of capacity (and, according to M. Jomard, of length also) for all men and all nations, for all time, and particularly for these latter profane times, is, in simple truth, nothing more and nothing less than—an old and somewhat misshapen stone coffin.

Standard of Linear Measure in the Great Pyramid.

The standard in the Great Pyramid, according to Messrs Taylor and Smyth, for *linear* measurements, is the length of the base line or lines of the pyramid. This, Professor Smyth states, is "the function proper of the pyramid's base." It is professed also that in this base line there has been found a new mythical inch—one-thousandth of an inch longer than the British standard inch; and Professor Smyth has attempted to show that the status of the kingdoms of Europe in the general and moral world may be measured in accordance with their present deviation or conformity to this mythical pyramidal standard in their modes of national measurement. "For the linear measure" (says Prof. Smyth), "of the base line of this colossal monument, viewed in the light of the philosophical connection between time and space, has yielded a standard measure of length which is more admirably and learnedly earth-commensurable than anything which has ever yet entered into the mind of man to conceive, even up to the last discovery in modern metrological science, whether in England, France, or Germany."

The engineers and mathematicians of different countries have repeatedly measured a meridian arc of a degree of sixty miles in order to use it as a standard for linear divisions. As part of their standard, they measure off sixty miles of the irregular earth-surface of a kingdom with almost perfect mathematical exactitude. Professor Smyth holds that the basis side of the pyramid has been laid down by Divine authority as such a guiding standard measure.

What, then, is the exact length of one of its basis lines? The sides of the pyramid have been measured by many different measurers. Linear standards have, says Professor Smyth, "been already looked for by many and many an author on the sides of the base of the Great Pyramid, even before they knew that the terminal points of those magnificent base lines had been carefully marked in the solid rock of the hill by the socket-holes of the builders." But—as in the case of the cubic capacity of the coffer these measurers sadly disagree with each other in their measurements, which, in fact, but vary from some 7500 or 8000 inches to 9000 and upwards. Thus, for example, Strabo makes it under 600 Grecian feet, or under 7500 English inches; Dr Shawe makes it 8040 inches; Shelton makes it 8184 inches; Greaves, 8316; Davison, 8952; Caviglia, 9072; the French academicians, 9163; Dr Perry, 9360, &c., &c.

At the time at which Professor Smyth was living at the pyramid, Mr Inglis, of Glasgow, visited it, and, for correct measurement, laid bare for the first time the four corner sockets. Mr Inglis' measurements not only differed from all the other measurements of "one-side" base lines made before him, but he makes the four sides differ from each other; one of them—namely, the north side—being longer than the other three. Strangely, Professor Smyth, though in Egypt for the purpose of measuring the different parts of the pyramid—and holding that its base line ought to be our grand standard of measure,—and further holding that the base line could only be accurately ascertained by measuring from socket to socket—never attempted that linear measurement himself after the sockets were cleared. These four corner sockets were never exposed before in historic times; and it may be very long before an opportunity of seeing and using them again shall ever be afforded to any other measurers.

Before the corner sockets were exposed, Professor Smyth attempted to measure the bases, and made each side of the present masonry courses "between 8900 and 9000 inches in length," or (to use his own word) "*about*" 8950 inches for the mean length of one of the four sides of the base; exclusive of the ancient casing and backing stones—which last Colonel Howard Vyse found and measured to be precisely 108 inches on each side, or 216 on both sides. These 216 inches, added to Professor Smyth's measure of "about" 8950 inches, makes one side 9166 inches. But Professor Smyth has "elected" (to use his own expression) not to take the mathematically exact measure of the casing stones as given by Colonel Vyse and Mr Perring, who alone ever saw them and measured them (for they were destroyed shortly after their discovery in 1837), but to take them, without any adequate reason and contrary to their mathematical measurement, as equal only to 202 inches, and hence "accept 9152 inches as the original length of one side of the base of the finished pyramid." He deems, however, this "determination" not to be so much depended upon as the measurements made from socket to socket.

The mean of the only four series of such socket or casing stone measures as have been recorded hitherto by Jomard (9163), Vyse (9168), Mahmoud Bey (9162), and Inglis (9110), amounts to nearly 9150. The first three of these observers were only able to measure the north side of the pyramid. Mr Inglis measured all the four sides, and made them respectively 9120, 9114, 9102, and 9102, making a difference of 18 inches between the shortest and longest. Professor Smyth thinks the measures of Mr Inglis as on the whole probably too small, and he takes two of them, 9114 and 9102—(but, strangely, not the largest, 9120)—as data, and strikes a new number out of these two and out of the three previous measures of Jomard, Vyse, and Mahmoud Bey, from these five quantities making a calculation of "means," and electing 9142 as the proper measure of the basis line of the pyramid—(which exact measure certainly none of its many measurers ever yet found it to be); and upon this *foundation*, "derived" (to use his own words) "from the best modern measures yet made," he proceeds to reason on, "as the happy, useful, and perfect representation of 9142," and the great standard for linear measure revealed to man in the Great Pyramid. Surely it is a very strange standard of linear measure that can only be thus elicited and developed—not by direct measurement, but by indirect logic, and regarding the exact and precise length of which there is as yet no kind of reliable and accurate certainty.

Lately, Sir Henry James has shown that the length of one of the sides of the pyramid base, with the casing stones added, as measured by Colonel H. Vyse—viz., 9168 inches—is precisely 360 derahs, or land cubits of Egypt; the derah being an ancient land measure still in use, of the length of nearly 25½ British inches, or, more correctly, of 25·488 inches; and he has pointed out that in the construction of the body of the Great Pyramid, the architect built 10 feet or 10 cubits of horizontal length for every 9 feet or 9 cubits of vertical height; while in the construction of the various galleries the proportion was adhered to of horizontal length to vertical height as 9 to 4,—rules which would greatly simplify the building of such a structure. The Egyptian derah of 25·48 inches is practically one-fourth more in length than the old Egyptian cubit of Memphis of 20·7 inches. Long ago Sir Isaac Newton showed from Professor Greaves' measurements of the chambers, galleries, &c., that the Memphis cubit of 20·7 British inches was apparently the *working* cubit of the masons in constructing the pyramid—an opinion so far admitted more lately by both Messrs Taylor and Smyth; "the length" (says Professor Smyth) "of the cubit employed by the masons engaged in the Great Pyramid building, or that of the ancient city of Memphis," being 20·73 British inches.* According to Mr Inglis' late measurement of the four bases of the pyramid, after its four corner sockets were exposed, the length of each base line was possibly 440 Memphis cubits, or 9108 English inches; or, if the greater length of Jomard and Vyse be held nearer the truth, 442 Memphis cubits, or 9150 British inches.

But Professor Smyth tries to show that (1.) if 9142 be granted him as the possible base line of the pyramid; and (2.) if 25 inches be allowed to be the length of the "Sacred Cubit," as revealed to the Israelites (and as revealed in the pyramid), then the base line might be found very near a multiple of this cubit by the days of the year,† or by 365·25; for these two numbers multiplied together

* Yet this, the Memphian cubit, "need not" (somewhat mysteriously adds Professor Smyth), "and actually is not, by any means the same as the cubit *typified* in the more concealed and *symbolised* metrological system of the Great Pyramid."

† Godfrey Higgins, in his work on "The Celtic Druids," shows how, among the ancients, superstitions connected with numbers, as the days of the year or the figures 365, have played a prominent part. "Amongst the ancients" (says he) "there was no end of the superstitious and trifling play upon the nature and value of numbers. The first men of antiquity indulged themselves in these fooleries" (p. 244). Mr Higgins points out that the old Welsh or British word for Stonehenge, namely Emrys, signifies, according to Davies, 365; as do the words Mithra, Neilos, &c.; that certain collections of the old Druidic stones at Abary may be made to count 365; that "the famous Abraxas only meant the solar period of 365 days, or the sun," &c. "It was all judicial astrology. . . . It comes (adds Mr Higgins) from the Druids."

amount to 9131 pyramidal inches, or 9140 British inches—the British inch being held, as already stated, to be 1000th less than the pyramidal inch. Was, however, the "Sacred Cubit"—upon whose alleged length of 25 "pyramidal" inches this idea is entirely built—really a measure of this length? In this matter—the most important and vital of all for his whole linear hypothesis—Professor Smyth seems to have fallen into an error which upsets all the calculations and inferences founded by him upon it.

Sir Isaac Newton, in his remarkable "Dissertation upon the Sacred Cubit of the Jews," long ago came to the conclusion that it measured 24·82 British inches. In this way it was one-fifth longer than the cubit of Memphis, viz., 20·7 inches, as previously deduced by him from Greaves' measurements of the King's Chamber and other parts of the interior of the Great Pyramid. Before drawing his final inference as to the Sacred Cubit being 24·82 inches, and as so many steps conducting to that inference, Sir Isaac shows that the Sacred Cubit was some measurement intermediate between a long and moderate human step or pace, between the third of the length of the body of a tall and short man, &c., &c. Professor Smyth has collected eight of the measurements thus adduced by Newton as "methods of approach" to circumscribe the length of the Sacred Cubit; and adding to these data Sir Isaac's deduction of the actual length of the Sacred Cubit as a ninth quantity, he enters the whole nine in a table as follows:—

Professor Smyth's Table of Newton's Inquiry regarding the Sacred Cubit.

First,	between	23·28	and 27·94	British inches.
Second,	„	23·3	27·9	„
Third,	„	24·80	25·02	„
Fourth,	„	24·91	25·68	„
And Fifth, somewhere near 24·82.				

"The mean of all which numbers" (Professor Smyth remarks) "amounts to 25·07 British inches. The Sacred Cubit, then, of the Hebrews" (he adds) "in the time of Moses—*according to Sir Isaac Newton*—was equal to 25·07 British inches, with a probable error of $\pm$ ·1."

But—"*according* to Sir Isaac Newton"—the Sacred Cubit of the

Jews was *not* 25·07, as Professor Smyth makes him state, but 24·82 British inches (or 25 unciæ of the Roman foot, and $\frac{8}{10}$ of an uncia), as Sir Isaac himself more than once deliberately infers in his Dissertation. Besides, in such inquiries is it not illogical to attempt to draw mathematical deductions by these calculations of "means," and especially by using the ninth quantity in the table —viz., Sir Isaac's own deliberate deduction regarding the actual length of the Sacred Cubit—as one of the nine quantities from which that length was to be deduced by the equivocal process of means? Errors, however, of a still more serious kind exist. The "mean" of the nine quantities in Professor Smyth's table is, he infers, 25·07 inches; and hence he avows that this, or near this figure, is the length of the Sacred Cubit. But the real mean of these nine quantities is not 25·07, but 25·29—a number in such a testing question as this of a very different value. For the days of the year (365·25) when multiplied by this—the true mean of these nine quantities—would make the base line of the pyramid 9237 inches, instead of Professor Smyth's theoretical number of 9142 inches—a difference altogether overturning all his inferences and calculations thereanent.

The incidentally erroneous summation which Professor Smyth makes of the nine quantities in his table, as amounting to 25·07, he declares (to use his own strong words) as a "*really glorious consummation* for the geodesical science of the present day to have brought to light;" for he avers this length of 25·07—which he forthwith elects to alter to 25·025 British inches—being "practically the sacred Hebrew cubit, is *exactly* one ten-millionth (1–10,000,000th) of the earth's semi-axis of rotation; and *that* is the very best mode of reference to the earth-ball as a whole, for a linear standard through all time, that the highest science of the existing age of the world has yet struck out, or can imagine. In a word, the Sacred Cubit, *thus* realised, forms an instance of the most advanced and perfected human science supporting the truest, purest, and most ancient religion; while a linear standard which the chosen people in the earlier ages of the world were merely told by maxim to look on as *sacred*, compared with other cubits of other lengths, is proved by the progress of human learning in the latter ages of time, to have had, and still to have, a philosophical merit about it which no men or nations at the time it was first produced, or within several thousand years thereof, could have possibly thought of for themselves." Besides, adds he elsewhere, "an *extraordinary* (sic) convenient length, too, for man to handle and use in the common affairs of life is the one ten-millionth of the earth's semi-axis of rotation when it comes to be realised, for it is extremely close to the ordinary human arm, or to the ordinary human pace in walking with a purpose to measure."

Of course, all these inferences and reasonings, regarding the Sacred Cubit being an exact segment of the polar axis, disappear when we find Sir Isaac Newton's length of the sacred cubit is not, as Professor Smyth elects it to be, 25·025 British inches; nor 25·07 as he calculated it to be from the mean of the nine quantities in his table; nor 25·29, as is the actual mean of these nine quantities; but, "*according* to Sir Isaac Newton's" own reiterated statement and conclusion, 24·82. A Sacred Cubit, according to Sir Isaac Newton's admeasurements of it of 24·82 inches, would not, by hundreds of cubits, be one ten-millionth of the measure of the semi-polar axis of the earth; provided the polar axis be, as Professor Smyth elects it to be, 500,500,000 British inches.

Axis of the Earth as a Standard of Measure.

The standards of measure in most modern civilised countries are, as is well known, referred to divisions of arcs of the meridian, measured off upon different points of the surface of the earth. These measures of arcs of the meridian, as measurements of a known and selected portion of the surface of the spheroidal globe of the earth, have, more or less, fixed mathematical relations with the axis of the earth; as the circumference of a sphere has an exact mathematical ratio to its diameter. The difference in length of arcs of the meridian at different parts of the earth's surface, in consequence of the irregular spheroidal form of the globe of the earth, has led to the idea that the polar diameter or axis of the earth would form a more perfect and more universal standard than measurements of the surface of the earth. In the last century, Cassini* and Callet† proposed, on these grounds, that the polar

* "Traite de la Grandeur et de la Figure de la Terre." Amsterdam edition (1723), p. 195.

† "Tables Portatives de Logarithmes." Paris, 1795, p. 100.

axis of the earth should be taken as the standard of measure. Without having noticed these propositions of Cassini and Callet, Professor Smyth adopts the same idea, and avers that 4000 years ago it had been adopted and used also by the builders of the Great Pyramid, who laid out and measured off the basis of the pyramid as a multiple by the days of the year of the Sacred Cubit, and hence of the pyramidal cubit; while the Sacred or Pyramidal Cubit were both the results of superhuman or divine knowledge, and were both, or each, one ten-millionth of the semi-polar axis of the earth. We have already seen, however, that the Sacred Cubit, *according* to Sir Isaac Newton, is not a multiple by the days of the year of the base line of the Great Pyramid, and is not one twenty-millionth of the polar axis of the earth, when that polar axis is laid down as measuring, according to the numbers elected by Professor Smyth, 500,500,000 British inches.

But is there any valid reason whatever for fixing and determining, as an ascertained mathematical fact, the polar axis of the earth to be this very precise and exact measure, with its formidable tail of nothings? None, except the supposed requirements or necessities of Professor Smyth's pyramid metrological theory. The latest and most exact measurements are acknowledged to be those of Captain Clarke, who, on the doctrine of the earth being a spheroid of revolution, computes the polar axis to be 500,522,804 British inches; or 500,482,296 British inches, calculating it from the results of all the known arcs of meridian measures. If we grant that the Sacred Cubit could be allowed to be exactly 25·025 inches, which Sir Isaac Newton found it not to be; and if we grant that the polar axis is exactly 500,500,000 British inches, which Captain Clarke did not find it to be; then, certainly, as shown by Professor Smyth, there would be 20,000,000 of these supposititious pyramidal cubits, or 500,000,000 of the supposititious pyramidal inches in this supposititious polar axis of the earth. "In so far, then" (writes Professor Smyth), "we have in the 5, with the many 0's that follow, a pyramidally commensurable and symbolically appropriate unit for the earth's axis of rotation." But such adjustments have been made with as great apparent exactitude when entirely different earth-axes and quantities were taken. Thus, Mr John Taylor shows the inches, cubits, and axes to answer precisely, although he took as his standard a totally different diameter of the earth from Professor Smyth. The diameter of the earth at 30° of latitude—the geographical position of the Great Pyramid—is some seventeen miles, or more exactly 17·652 miles, or above 1,100,000 inches, longer than at the poles. But Mr Taylor fixed upon this diameter of the earth at latitude 30° —and not, like Professor Smyth, upon its polar diameter—as the standard for the metrological linear measures of the Great Pyramid; and yet, though the standard was so different, he found, like Mr Smyth, 500 millions of inches also in his axis, and 20 millions of cubits also.* The resulting figures appear to fit equally as well for the one as for the other. Perhaps they answer best on Mr Taylor's scheme. For Mr Taylor maintained that the diameter of the earth before the Flood, at this selected point of 30°, was less by nearly 27 miles from what it was subsequently to the Flood,† and is now;—a point by which he accounts for otherwise unaccountable circumstances in the metrological doctrines which have been attempted to be connected with the Great Pyramid. For while Mr Taylor believes the Sacred Cubit to be 24·88, or possibly 24·90 British inches, he holds the new Pyramidal cubit to be 25 inches in full; and the Sacred and Pyramidal cubits to be different, therefore, from each other, though both inspired. In explanation of this startling difference in two measures supposed to be equally of sacred‡

* "The diameter of the earth, according to the measures taken at the Pyramids, is 41,666,667 English feet, or 500,000,000 inches." (See "The Great Pyramid," p. 75.) "Dividing this number by 20 millions, we obtain the measure of 25 (English) inches for the Sacred Cubit" (p. 67).

† "When" (says Mr Taylor, p. 91) "the *new* Earth was measured in Egypt after the Deluge, it was found that it exceeded the diameter of the *old* Earth by the difference between 497,664,000 inches and 500,000,000 inches; that is, by 2,336,000 inches, equal to 26·868 miles."

‡ *Alleged Sacred Character of the Scottish Yard or Ell Measure.*—Professor Smyth tries to show (iii. 597), that if Britain stands too low in his metrological testing of the European kingdoms and races, its "low entry is due to accepting the yard for the country's popular measure of length." But long ago the "divine" origin of the Scottish ell—as in recent times the divine origin of the so-called pyramidal cubit and inch—was pleaded rather strenuously. For when, in the 13th century, Edward I. of England laid before Pope Boniface his reasons for attaching the kingdom of Scotland to the Crown of England, he maintained, among other arguments, the justice and legality of this appropriation on the ground that his predecessor King Athelstane, after subduing a rebellion in Scotland under the auspices of St John of Beverley,

origin, Mr Taylor observes—"The smaller (24·88) is the Sacred Cubit which measured the diameter of the Earth *before* the Flood; the one by which Noah measured the Ark, as tradition says; and the one in accordance with which all the interior works of the Great Pyramid were constructed.* The larger (25) is the Sacred Cubit of the *present* Earth, according to the standard of the Great Pyramid when it was completed."

Surely such marked diversities and contradictions, and such strange hypothetical adjustments and re-adjustments of the data and calculations, entirely upset the groundless and extraordinary theory of the base of the pyramid being a standard of linear measurement; or a segment of any particular axis of the earth; or a standard for emitting a system of new inches and new cubits;—seeing, on the one hand, more particularly, that the basis line of the pyramid is still itself an unknown and undetermined linear quantity, as is also the polar axis of the earth of which it is declared and averred to be an ascertained, determined, and measured segment.

M. Pancton, in 1780, wrote a work in which he laid down the base side of the pyramid as 8754 inches; maintained, like Mr Taylor and Mr Smyth, that this length was a standard of linear measures; found it to be the measure of a portion of a degree of the meridian, such degree being itself the 360th part of a circle;—and apparently the calculations and figures answered as well as

prayed that through the intervention of that saint, it "might be granted to him to receive a visible and tangible token by which all future ages might be assured that the Scots were rightfully subject to the King of England. His prayer was granted in this way: Standing in front of one of the rocks at Dunbar, he made a cut at it with his sword, and left a score which proved to be the *precise* length of an ell, and was adopted as the regulation test of that measure of length." This legend of the "miraculously created ell-wand standard" was afterwards duly attested by a weekly service in the Church of St John of Beverley. (See Burton's "History of Scotland," ii. 319.) In the official account of the miracle, as cited by Rymer, it is declared that during its performance the rock cut like butter or soft mud under the stroke of Athelstane's sword. "Extrahens gladium de vaginâ percussit in cilicem, quæ adeo penetrabilis, Dei virtute agente, fuit gladio, quasi eâdem horâ lapis butirum esset, vel mollis glarea; . . . et usque ad presentem diem, evidens signum patet, quod Scoti, ab Anglis devicti ac subjugata; monumento tali evidenter cunctis adeuntibus demonstrante." (Foedera, tom. i. pars ii. 771.)

*Eslewhere (p. 45) Mr Taylor corroborates Sir Isaac Newton's opinion that the *working* cubit by which the Pyramid was built was the cubit of Memphis.

when the measurement was declared to be 9142 inches, and the line, not a segment of an arc of the circumference of the earth, but a segment of the polar axis of the earth; for De l'Isle lauds Pancton's meridian degree theory as one of the wondrous efforts of human genius, or (to use his own words) "as one of the chief works of the human mind!" Yet the errors into which Pancton was led in miscalculating the base line of the Pyramid as 8754 inches, and the other ways he was misled, are enough, suggests Professor Smyth, "to make poor Pancton turn in his grave."

Significance of Cyphers and Fives.

M. Pancton, Mr Taylor, and those who have adopted and followed their pyramid metrological ideas, seem to imagine that if, by multiplying one of their measures or objects, they can run the calculation out into a long tail of terminal 0's, then something very exact and marvellous is proved. "When" (upholds Mr Taylor), "we find in so complicated a series of figures as that which the measures of the Great Pyramid and of the Earth require for their expression, *round numbers* present themselves, or such as leave no remainder, we may be sure we have arrived at *primitive* measures." But many small objects, when thus multiplied sufficiently, give equally startling strings of 0's. Thus, if the polar axis of the earth be held as 500,000,000 inches, and Sir Isaac Newton's "Sacred Cubit" be held as 24·82 inches—then the long diameter of the brim of the lecturer's hat, measuring 12·4 inches, is 1-20,000,000th of the earth's polar axis; a page of the print of the Society's Transactions is 1-60,000,000th of the same; a print page of Professor Smyth's book, 6·2 inches in length, is 1-80,000,000th of this "great standard;" &c., &c.

Professor Smyth seems further to think that the figure or number "five" plays also a most important symbolical and inner part in the configuration, structure, and enumeration of the Great Pyramid. "The pyramid" (says he) "embodies in a variety of ways the importance of five." It is itself "five-angled, and with its plane a five-sided solid, in which everything went by fives, or numbers of fives and powers of five." "With five, then, as a number, times of five, and powers of five, the Great Pyramid contains a mighty system of consistently subdividing large quantities to suit human happiness." To express this, Mr Smyth suggests the new noun "fiveness." But it applies to many other matters as strongly, or more strongly, than to the Great Pyramid. For instance, the range of rooms belonging to the Royal Society is "five" in number; the hall in which it meets has five windows; the roof of that hall is divided into five transverse ornamental sections; and each of these five transverse sections is subdivided into five longitudinal ones; the books at each end of the hall are arranged in ten rows and six sections—making sixty, a multiple of five; the official chairs in the hall are ten in number, or twice five; the number of benches on one side for ordinary fellows is generally five; the office-bearers of the Society are twenty-five in number, or five times five; and so on. These arrangements were doubtless, in the first instance, made by the Royal Society without any special relation to "fiveness," or the "symbolisation" of five; and there is not the slightest ground for any belief that the apparent "fiveness" of anything in the Great Pyramid had a different origin.

Minuteness of Modern Practical Standards or Gauges.

In all these "standards" of capacity and length alleged to exist about the Great Pyramid, not only are the theoretical and actual sizes of the supposed "standards" made to vary in different books—which it is impossible for an actual "standard" to do,—but the evidences adduced in proof of the conformity of old or modern measures with them is notoriously defective in complete aptness and accuracy. Measures, to be true counterparts, must, in mathematics, be not simply "near" or "very near," which is all that is generally and vaguely claimed for the supposed pyramidal proofs; but they must be entirely and *exactly* alike, which the pyramidal proofs fail altogether in being. Mathematical measurements of lines, sizes, angles, &c., imply exactitude and not mere approximation; and without that exactitude they are not mathematical, and —far more—are they not "superhuman" and "inspired."

Besides, it must not be forgotten that our real *practical* standard measures are infinitely more refined and many thousand-fold more delicate than any indefinite and equivocal measures alleged to be found in the pyramid by even those who are most enthusiastic in the pyramidal metrological theory. At the London Exhibition in

1851, that distinguished mechanician, Mr Whitworth, of Manchester, was the first to show the possibility of ascertaining by the sense of touch alone the one-millionth of an inch in a properly-adjusted standard of linear measure; and in his great establishment at Manchester they work and construct machinery and tools of all kinds with differences in linear measurements amounting to one ten-thousandth of an inch. The standards of the English inch, &c., made by him for the Government—and now used by all the engine and tool makers, &c., of the United Kingdom—lead to the construction of machinery, &c., to such minute divisions; and the adoption of these standards has already effected enormous saving to the country by bringing all measured metal machinery, instruments, and tools, wherever constructed and wherever afterwards applied and used, to the same identical series of mathematical and precise gauges.

The Sabbath, &c., Typified in the Pyramid.

The communication next discussed some others amongst the many and diversified matters which Professor Smyth fancifully averred to be typified and symbolised in the Great Pyramid.

One, for example, of the chambers in the Great Pyramid—the so-called Queen's Chamber—has a roof composed of two large blocks of stone leaning against each other, making a kind of slanting or double roof. This double roof, and the four walls of the chamber count six, and typify, according to Professor Smyth, the six days of the week, whilst the floor counts, as it were, a seventh side to the room, "nobler and more glorious than the rest," and typifying something, he conceives, of a "nobler and more glorious order"—namely, the Sabbath. It is surely difficult to fancy anything more strange than this strange idea. In forming this theory liberties are also confessedly taken with the floor in order to make it duly larger than the other six sides of the room, and to do so he theoretically lifts up the floor till it is placed higher than the very entrance to the chamber; for originally the floor and sides are otherwise too nearly alike in size to make a symbolic *seven*-sided room with one of the sides proportionally and properly larger than the other six sides. Yet Professor Smyth holds that, in the above typical way, he has "shown," or indeed "proved entirely," that the Sabbath had been heard of before Moses, and that thus he finds unexpected and confirmatory light of a fact which, he avers, is of "extraordinary importance, and possesses a ramifying influence through many departments of religious life and progress."

He believes, also, that the corner-stone—so frequently alluded to by the Psalmist and the Apostles as a symbol of the Messiah—is the head or corner-stone of the Great Pyramid, which, though long ago removed, may yet possibly, he thinks, be discovered in the Cave of Machpelah; though how, why, or wherefore it should have found its way to that distant and special locality is not in any way solved or suggested.

Pyramid alleged to be a Superhuman, and more or less an Inspired Metrological Erection.

Professor Smyth holds the Great Pyramid to be in its emblems and intentions and work "superhuman;" as "not altogether of human origination; and in that case whereto" (he asks) "should we look for any human assistance to men but from Divine inspiration?" "Its metrology is," he conceives, "directed by a higher Power" than man; its erection "directed by the *fiat* of Infinite Wisdom;" and the whole "built under the direction of chosen men divinely inspired from on high for this purpose."

If of this Divine origin, the work should be absolutely perfect; but, as owned by Professor Smyth, the structure is not entirely correct in its orientation, in its squareness, &c., &c.—all of them matters proving that it is human, and not superhuman. It was, Professor Smyth further alleges, intended to convey standards of measures to all times down to, and perhaps beyond, these latter days, "to herald in some of those accompaniments of the promised millennial peace and goodwill to all men." Hence, if thus miraculous in its foreseen uses, it ought to have remained relatively perfect till now. But "what feature of the pyramid is there" (asks Professor Smyth) "which renders at once in its measurements in the present day its ancient proportions? None." If the pyramid were a miracle of this kind, then the Arabian Caliph El Mamoon so far upset the supposititious miracle a thousand years ago—(of course he could not have done so provided the miracle had been truly Divine)—when he broke into the King's Chamber and unveiled its contents; inasmuch as the builders, according to Professor Smyth, intended to conceal its secrets for the benefit of these latter times, and for this purpose had left a mathematical sign of a cross joint on the floor of the gallery, by which some man in the distant future, visiting the interior, should detect the entrance to the chambers; and which secret sign Professor Smyth himself was, as he believes, the first to discover two years ago. The secret, however, if any such was placed there, for the detection of the entrance to the interior chambers, has been thus discovered some 1000 years too late for the evolution of the alleged miraculous arrangement. In relation to the Great Pyramid, as to other things, we may be sure that God does not teach by the medium of miracle anything that the unaided intellect of man can find out; and we must beware of wrongously and disparagingly attributing to Divine inspiration and aid, things that are imperfect and human.

The communication concluded by a long series of remarks, in which it was pointed out that at the time at which the Great Pyramid was built, probably about 4000 years ago, mining, architecture, astronomy, &c., were so advanced in various parts of the East as to present no obstacle in the way of the erection of such magnificent mausoleums as the colossal Great Pyramid and its other congener pyramids undoubtedly are.

2. Note on the Occasional Occurrence of the Musculus Rectus Thoracis in Man. By Professor Turner.

During the last winter session, I communicated to the Society* a paper on the "Musculus Sternalis," in which I argued that the longitudinally arranged muscle, occasionally found superficial to the sternal fibres of origin of the pectoralis major, was not, as anatomists have usually described it, homologous with the anterior or thoracic fibres of the mammalian rectus, but belonged to another group of muscles.

Since that time I have met with two subjects in the dissecting-room, in each of which a longitudinal muscle occurred, lying in contact with the outer surface of the anterior extremities of the

* Proceedings, 21st January 1867, and Journal of Anatomy and Physiology, May 1867.

upper true ribs, and beneath the fibres of the pectoral muscle, which is, from the position, direction, and connection of its fibres, I believe to be regarded as homologous with the thoracic end of the mammalian rectus. Both subjects were males. In one a longitudinal ribbon-shaped muscle arose by a thin expanded tendon, from the upper border of the fifth left rib, immediately internal to the attachment of the serratus magnus. The innermost part of its tendon was continuous with the membrane covering the internal intercostal muscle, and was attached to the rib at the junction of its osseous and cartilaginous portions. From the anterior surface of the fourth rib, close to the origin of the serratus magnus, a second and smaller origin proceeded. The muscle ascended superficial to the osseous parts of the third and second ribs, as high as the first rib, into which it was inserted immediately external to the tendon of attachment of the subclavius muscle. The muscle was 6 inches long and $\frac{5}{8}$ths of an inch broad at its widest part. The breadth of the fifth rib alone separated it from the upper attachment of the rectus abdominis. A corresponding muscle existed on the right side.

In the other subject, a longitudinal muscle—on the right side only—arose beneath the pectoralis minor from the upper border of the fourth rib two inches to the outer side of the junction of its bone and cartilage. It ascended superficial to the third and second ribs, to be inserted into the bony part of the first rib, $\frac{3}{4}$ths of an inch external to the attachment of the subclavius.

In the cat, the otter, the beaver, the porcupine, and various other Mammalia, the rectus muscle extends as high as the first rib, into which it is inserted, and in this respect the arrangement may be compared with that of the occasional human muscles just described.

In these animals, however, the thoracic and abdominal parts of the rectus are directly continuous with each other, whilst in the human subject a break, corresponding in the first specimen to the fifth rib, and in the second, to the fifth rib and fourth intercostal space, occurred; but this break may be regarded as comparable to one of those transverse tendinous intersections, invariably found in the abdominal part of the human rectus, and which exist also in the recti of the greater number of the Mammalia. These specimens seem to present as complete a representation of the mammalian arrangement, though in a somewhat different form, as is afforded by the case recorded by Kaau Boerhaave, to which I alluded in my former paper, and they support the opinion I ventured to throw out in that communication, that the supracostal muscle described by Mr John Wood (*Proc. Roy. Soc. London*, June 15, 1865), is homologous with the pectoral end of the mammalian rectus.

During the past year, Mr Wood has described another specimen of the supra-costal muscle (*Proc. Roy. Soc. London*, May 23, 1867) which passed from the third rib upwards to the first rib and cervical fascia; Dr Roberts (*Liverpool Medical and Surgical Reports*, October 1867), one which extended from the fourth to the first rib; and Bochdalek, Jun. (Virchow's *Archiv.* 18th November 1867), under the name of M. supra-costalis anterior, has described and figured one, which also passed from the fourth to the first rib; but by none of these anatomists is any reference made to the probability of its being homologous with the pectoral end of the mammalian rectus.

The specimens which I have now recorded apparently correspond to the supra-costal muscles of these writers; and the arrangement more especially of the first, in which the muscle extended as low as the fifth rib, and was separated from the rectus only by the breadth of that rib, justifies me, I think, in the conclusion I have come to respecting the homology of the muscle.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VI. 1867–68. No. 76.

ERRATA.

Page 36, line 17 from bottom, *for* Dr John Alexander Smith, *read*, Dr John Smith. Dr J. A. Smith was elected in 1863.

Page 200, line 3 from the bottom, *for* twelve, *read*, six.

Page 201, line 3 from the top, *for* after the peace in 1814, *read*, in the years 1818, 1819.

Page 202, line 8 from the top, *for* he left, *read*, had.

Page 202, lines 10, 11, and 12, *from* "the wife," to the end of the paragraph, *read*, and two daughters—Mrs Isabella Gore Booth, married to a son of the late Sir Robert Gore Booth of Lissadel, Sligo; and Sabina, unmarried. Christian, wife of Walter Buchanan, Esq., M.P., died 30th April 1852; and Louisa, wife of the late William Hamilton, Esq. of Minard, died 2d March 1863.

buried city lay beneath the streets of modern Zion, as little revealed by the contour of the buildings on the surface as were the palaces beneath the sand mounds of Assyria. It was not until 1838 that any attempt was made to explore any part of Palestine in a strictly scientific manner, whether with reference to its topography, archæology, or natural phenomena. In that year the American Dr Robinson, in company with the missionary Eli Smith, made his first Biblical researches, which he continued in 1852. Whatever may be our opinion of the value of Dr Robinson's archæological theories, he remains still the prince of Palestine geographers in all that relates to general topography; and in many cases where his conclusions have subsequently been rejected as unsound, his researches have been the means of identifying or correcting the positions of long lost sites. He was followed in 1848 by his countryman, Commodore Lynch, whose celebrated expedition down the Jordan and upon the Dead Sea is familiar to every student. In 1851-52, the Dutch officer, C. W. M. Van de Velde, made his survey, and provided for us the first accurate map of modern Palestine. His work has been the basis upon which most subsequent explorers have proceeded. In the following year, Mr Porter explored the wonderful country of the Hauran, or Bashan, and revealed to us many of its ruined cities. In these regions east of Jordan Mr Cyril Graham has made yet more important discoveries, which he has published chiefly in the Transactions of the Geographical Society.

Of those who have not personally visited the country, one writer, Carl Ritter, has rendered invaluable service by his exhaustive work on the Comparative Geography of Palestine, which has recently been rendered accessible to English students by Mr Gage's translation, published by Messrs Clark, and which is almost indispensable to every student of the subject.

The Rev. G. Williams, in his learned work "The Holy City," is almost the only writer who has hitherto devoted both learning, archæological knowledge, and a long residence in the Holy City, to the elucidation of the topography of ancient Jerusalem itself, all other writers having been deficient in one or other of these indispensable requisites for investigating the vexed question of the sacred sites.

In 1864 the Palestine Exploration Fund was commenced, for the purpose of the complete and thorough exploration of the Holy Land, both as regards its archæology, topography, manners and customs, geology, and natural history. It is perfectly impossible for ordinary travellers from private resources, still less for tourists, to accomplish these great objects, especially as regards archæology.

Before proceeding to other branches of research, I will endeavour shortly to explain what the Palestine Exploration Fund has already accomplished in the examination of Jerusalem itself.

In 1864-65, Captain Wilson, R.E., completed the ordnance survey of Jerusalem, at the cost of Miss Burdett Coutts.

In December 1865, Captain Wilson again returned, and was occupied until the following May in the general survey of the country.

Shortly after his return, Lieutenant Warren, R.E., was sent out, and has been devoting himself with unwearied diligence in the exploration of ancient Jerusalem, as well as to the survey of Gilead, Moab, and Philistia.

We may classify his discoveries in Jerusalem itself under two heads—1*st*, The traces of ancient walls and streets; and 2*d*, The ancient water supply and hidden streams of the city.

The main key to the topography of ancient Jerusalem is the course of the Tyropœon valley, frequently mentioned by Josephus, and described by him as bisecting the city, and dividing the upper market, or Zion, from the lower market, or Akra. Its lower end has been identified by all explorers as coming out between Mount Zion and Moriah. But the whole portion of it within the walls, which was once a deep depression dividing the upper city from the lower and from the Temple, is now but a slight depression, and in places not at all to be detected on the surface. More than any other existing city, Jerusalem has been whelmed under heaps of rubbish. Its repeated sieges and destructions, and its rebuilding with fresh material from the inexhaustible quarries on every side, have completely hidden all remains of the city of Herod, still more of that of Ezra and of Solomon.

Walls and gates lie low beneath the accumulated debris of centuries. Towers, fortifications, houses, have come down in the course of ages, and the old ruin has always been taken as the foundation of the new edifice. Captain Wilson and Lieutenant Warren have sunk more than thirty shafts in different directions, and everywhere, after penetrating through many feet of rubbish and ruin, they have found old foundations, cisterns, or watercourses.

At the south-west corner of the Temple area, a shaft has been sunk 85 feet, and then a gallery run along, which shows the bottom of the valley to be actually 115 feet beneath the present surface, at the point known as Robinson's arch.

Josephus tells us of a magnificent viaduct across the Tyropœon, leading from the Temple area to Mount Zion. Dr Robinson was the first to detect the remains of this viaduct, in three stones, evidently the first spring of an arch, near the south-west corner of the Temple area.

This seemed to place the site of the Temple at this angle of the platform, until Captain Wilson discovered, much farther to the north, near to the Jews' wailing-place, another arch entire, but buried under the rubbish on which the modern houses stand.

Continuing Captain Wilson's researches, and sinking 50 feet below the present surface, Mr Warren has traced this magnificent viaduct by the remains of its arches right across the chasm, thus setting this question for ever at rest.

Robinson's arch appears to have been a single arch, probably for the purpose of supporting a gallery alongside of the Temple wall.

The Tyropœon, moreover, from the excavations in its upper part, proves to have been very different in form from anything hitherto supposed, viz., tolerably flat for the greater part of its width, with ample space for a lower city, as described by Josephus, and suddenly descending close below the Temple wall to a narrow gallery of great depth. Here projected Robinson's arch, the pier of which seems to have been reached, and two courses of huge dressed stone found *in situ*. What must have been the effect to one standing on the pinnacle of the Temple, which here rose to a height of 150 feet, from a platform of solid masonry 150 feet lower down, so that he looked down from an elevation of 300 feet sheer into the valley beneath? "See what manner of stones and what buildings were here."

Then, again, by the repeated sinking of shafts all over the surface of the sloping pier, which runs down from the Temple platform to the fount of the Virgin, that lower hill once known as Ophel, and an important suburb of the city, it is established by actual demonstration that the south wall of the sacred enclosure which contained the Temple is buried for more than half its depth beneath an accumulation of rubbish, probably the ruins of the successive buildings which once crowned it, and that if bared to its foundation, the wall would present an unbroken front of solid masonry of nearly 1000 feet long and 150 feet in height. The wall as it stands, with less than half that height emerging from the ground, has always been regarded as a marvel. What must it have been when entirely exposed to view! No wonder that prophets and psalmists should have rejoiced in the "walls" and "bulwarks" of the Temple, and that Tacitus should have described it as *modo arcis constructum*. "Walk about Zion, and go round about her; tell the towers thereof. Mark ye well her bulwarks, consider her palaces, that ye may tell it to the generations following."

Now, as the lower part of this wall is manifestly a substratum, in order to increase the platform space for the Temple area, which must always have been at its present elevation, that of the Sakhra, or central rock, the question naturally arises whether there are not subterranean chambers beneath those already known and described underneath the Aksa. Now, beneath the Huldah gate, in the south wall of the Haram, is an ancient double passage or tunnel. At the north end of this is a vaulted chamber 17 feet square, recently discovered, but further than this no explorations have yet been made.

To proceed to the south. The wall of Ophel has been discovered abutting on that of the Temple area at the south-east, and has been traced 300 feet from the Haram area to the south. A tower has been discovered, a face of 29 feet 6 inches projecting about 8 feet beyond the wall, built by Jotham, king of Judah (2 Chron. xxvii. 3). The bases on which the piers of the arches of the substructure rest on this point are 80 feet above the rock, and it is likely there is another system of older arches underneath those visible at present. The courses of this south wall are from 3 feet 3 inches to 4 feet in height; and 20 feet below the surface of the Haram walls, or 60 feet below the area of the Haram, has been found a passage from 12 to 16 feet high, and in it the check where a metal gate was swung, and the marks of its abrasion on the stones.

In the double passage is what is called the "Well of the Leaf," at the bottom of which was found a curious arch of tiles, perhaps an outlet to some subterranean flow of water.

Lieutenant Warren is inclined to doubt that the masonry west of Robinson's arch was a pier, as the ancient masonry is not in keeping with the Haram wall. It is 15 feet above the rock, and the remains of a house rest on the top of the wall. The pier itself is 12 feet 2 inches thick, and the distance to the Haram wall 41 feet 6 inches.

Higher up, under Wilson's arch, the depth of the shaft till the rock is reached is 53 feet. At 50 feet 9 inches the base course of the Haram wall is let into the rock, and the span of the arches is within 6 inches the same as that of Robinson's arch.

In the centre of the city the accumulation of debris is scarcely less. In Muristan the shaft was sunk 70 feet before the rock was reached; and some years since the foundation for the English Church on Mount Zion was sunk for to a depth of 40 feet, when the arch of an old aqueduct was struck.

When, however, we get to the north at the Damascus gate, we find the foundation of the old wall outside it 10½ feet thick, only 3 feet beneath the present roadway, which, it must be noted, is sunk many feet beneath the surrounding heaps. At the foot of this wall, which is very unlike the Temple walls in masonry, a Templar's cross was found, and it is probably the Crusaders' wall, destroyed when they were compelled to leave the city.

We next come to the water supply.

The most distant as well as the most important source of the supply are Solomon's pools at Etham, near Bethlehem, now called El Burak. These three artificial tanks are well known, being respectively 380, 423, and 582 feet in length, 236 feet wide, and from 28 to 50 feet deep. They are supplied from a subterranean fountain, "the sealed fountain," three-quarters of a mile to the north-west; from which a subterranean passage, leading to the pools of Solomon, has been explored by Lieutenant Warren. In the course of his researches he has met with traces which lead to the belief that there is another sealed fountain yet to be discovered to the south-east, which contributes to the supply of the pools, at the head of which is a spring or outflow, carefully concealed in the rock, from which the water rushes to the upper pool. There are two lines of aqueduct at different levels, the higher one now in ruins, from these cisterns to Bethlehem and Jerusalem. The lower level aqueduct, in which water still flows, follows the windings of the hill sides by Bethlehem to the valley of Hinnom, which it crosses upon nine low arches above the lower pool of Gihon. From hence it sweeps round the southern brow of Zion, and enters the city on the side above the Tyropœon, where it can be traced for a short distance, partly hewn in the rock, and partly supported on masonry against the side of the cliff. It enters the Haram at the gates of the Chain. Just outside of this gate is a large subterranean cistern, 84 by 42 feet, and 24 feet deep, doubtless supplied in olden times by this aqueduct. Again, near the Fountain gate, is a tank of enormous dimensions, which is within a few feet of the aqueduct, which could easily fill it. (*Burg al Kibreyt.*)

But this was not the only supply of water on which Jerusalem had to depend. In all the history of its sieges, there is not any allusion to the inhabitants suffering from thirst. All confirms the truth of Strabo's words—ἐντὸς μὲν εὔυδρον, ἐκτὸς δὲ παντελῶς διψηρόν.

There is first the upper pool of Gihon, spoken of by Isaiah, who went forth to meet Ahaz at the end of the conduit of the *upper pool* in the highway of the fuller's field. We also read that Hezekiah stopped the upper outflow of the waters of Gihon, and brought it down to the west side of the city of David. Here, too, Rabshakeh stood when he delivered the insolent message of his master the king of Assyria.

The lower pool of Gihon is also mentioned by Isaiah (xxii. 9): "Ye gathered together the waters of the lower pool."

There is next the vast system of tanks by the *fosse* of Antonia, north of the temple area, the pool of Bethesda, connected with the enormous system of tanks under the Haram area, so far as we can conjecture. Then there is the pool of Hezekiah, close to Muristan, which probably supplied Akra, or the lower city. This was probably fed by the outflow from the aqueduct from the pool of Gihon after supplying the city of David. It is mentioned in 2 Chron. xxxii. 30, that Hezekiah stopped the upper outflow of the waters of Gihon, and brought it straight down to the west side of the city of David. And in expectation of the siege by Sennacherib, "he took counsel with his princes and his mighty men to stop the waters of the fountain which were without the city. . . . So there was gathered much people together who stopped all the fountains and the brook that ran through the midst of the land, saying, Why should the king of Assyria come and find much water?"

We have not yet discovered these hidden sources, but Lieutenant Warren has explored at Lifta (Nephtoah?), two miles north-west of Jerusalem, a perpendicular chasm 155 feet deep, through which water constantly drains and runs out of sight at once, which cleft he thinks partially artificial, and connected with a similar cleft he examined near the Russian buildings north of the pool of Gihon and close to it.

Underneath the temple area is a vast system of caverns, one of which, "the sea," deserves the name of a beautiful subterranean lake, while the whole hill is pierced by wells, and honeycombed by rock-hewn cisterns.

To the west of the temple is the Bath of Healing (Hummām esh Shefa), where there is a series of vaulted cisterns, with passages and deep wells, from which the water rises.

Below Ophel is the Fountain of the Virgin, celebrated for its irregular and uncertain flow, and which appears to be fed from some outflow underneath Mount Moriah. Hence is a subterranean channel leading to the pool of Siloam, which has been explored, and another strange tunnel discovered by Lieutenant Warren, connecting it with a vast tank.

Siloah, mentioned in Scripture as by the king's gardens, has again a subterranean connection with En Rogel or Bir Eyub, Joab's Fountain, discovered by Lieutenant Warren, while this too appears to have had a yet further extension by a second hidden channel to the south. 500 yards below Bir Eyub, Lieutenant Warren opened a spring, and at a depth of 12 feet a stone suddenly rolled away, and revealed a staircase, about 25 feet deep, and at the bottom, passages leading north and south. Steps and passages are cut in the rock; the latter are about 6 feet high, and may have been for the purpose of leading off the Jerusalem waters out of the reach of an enemy invading the city. The passage north has been explored for 107 feet, and is full of silt without stones.

Then again, under the Well of the Leaf, beneath the Haram, are ducts discovered, plainly leading to some yet undiscovered cisterns.

Again, at the upper end of the Tyropœon valley, Lieutenant Warren has discovered, at the bottom of a shaft of 50 feet, a drain with a stream of sweet water running through it with a constant flow, not from the baths, nor yet from the aqueduct of Solomon's Pools, but which must be from a spring at the head of the valley within the city.

At the south-east of the *cœnaculum* he has discovered, 50 feet above the level of the present aqueduct, what is no doubt the remains of Solomon's original aqueduct from the pools of Etam. This he has penetrated 250 feet in one direction, and 200 in the other. Often the explorer had to wriggle through lying on his back, at others to crawl on hands and knees. In parts it is built of masonry, in parts cut out of solid rock, and generally of a semi-cylindrical shape; but in many parts it has the peculiar shoulders only before noticed under the triple gateway, and recently discovered also in the channel leading to Tekoah from Etam, almost certainly Solomonic in date. In part of the passage he could stand upright, it being 10 or 12 feet high, with the remains of two sets of stones for covering, as shown in Mr Piazzi Smyth's work on the Great Pyramid; the stones at the sides being of great size, 12 feet by 6. This channel cannot be so late as the Roman period; it is evidently of the most ancient construction. It is built in little spaces, as if the work had been commenced at two or three points, and had not been directed properly. The plaster is still in good preservation. This channel must have been of great consequence, both from the distance it is driven under ground, and from the well-cut shafts which lead to it. The main supply of Jerusalem was probably obtained by it, as it is perfectly concealed from any besieger.

In the Muristan a system of tanks has been discovered 53 feet below the surface. Four have already been examined, from 50 to 68 feet long and 17 feet wide, with a flight of twenty-five steps down the side, with water in some, hard mud in others, and the plaster as fresh and impervious to water as if just constructed.

Most interesting have been the explorations of the Virgin's Fountain under Ophel. Lieutenant Warren discovered in the channel from this fountain a passage branching from that to Siloam of the most extraordinary character. For 50 feet the tunnel runs. Then for 17 feet more is the new passage, opening upon a shaft 6 feet by 4, and 42 feet high, to the landing in a wide cave some 30 feet high in its slope. After creeping along for 40 feet, a passage was discovered 8 feet wide, and from 3 to 4 feet high, cut in form of a depressed arch out of rocks. After following this for 40 feet more, a wall of rough masonry was found across it. From this wall the passage rises at an angle of 45°, still cut as before out of rock, with toe holes cut in it. Fifty feet up there is another wall of rough masonry, opening into a vaulted chamber, with a descent of 20 feet from the opening, and at the bottom a smaller one 8 feet deeper, with a passage blocked up, which has not yet been explored. In the horizontal part of the passage were found curious antique glass lamps, water jars, a little pile of charcoal as if for cooking, and a cooking-dish glazed inside. Evidently this had been used as a refuge. Overhanging the shaft was an iron ring, to which a rope might have been attached for hauling up water.

Lieutenant Warren considers that which has usually been held to be the aqueduct from the Temple to the Virgin's Fount, to have been rather an aqueduct for the supply of Ophel, as evidenced by a shaft and draw-well.

While exploring the tunnel between the Virgin's Fount and Siloam, the same gurgling sound of water was heard which has been noticed by others; but the hidden channel, which has thus been detected by sound at intervals from the Damascus gate to Siloam, yet remains to be revealed.

Now, fragmentary as all these discoveries are, they indicate an amount of ancient remains below the surface, which cannot but excite our hopes, and stimulate us to strain every nerve to lay bare such interesting relics. We may find but little yet to throw direct light on the lives we so cherish, and any detail of which we so dearly prize. True, but it must be recollected that the exploration in Jerusalem is at present in the condition of a puzzle or joining map, of which only half a dozen pieces are found out of sixty or seventy. Find the others, and the whole can be put together, and will then be intelligible enough. Extend to the other parts of the city the researches here begun, and the sites of the Temple, of the Tombs of the Kings of Judah, of the Holy Sepulchre, of the Pool of Bethesda, will be problems no longer.

As Mr Grove remarks "as I read Lieutenant Warren's accounts, I seem to feel the ancient city within my grasp, to know for a certainty that its very houses, and streets, and water-courses, all the ancient life of its structures, its hills and its ravines, are lying buried, like some enchanted person, beneath that singular and solemn tomb. The cliff (says Dean Stanley) which Joab climbed, the streets which David trod, and along which Athaliah was hurried, the catacombs of the kings of Judah, the very Via Dolorosa itself, of which not even the shadow of a likeness can be found in the upper air of the modern city—all these are doubtless there. Wherever we probe, go deep enough, and we come on some solid substance of curious and ancient kind. A stone suddenly rolls away and reveals staircases, passages, subterranean halls in the heart of the rock, leading to who knows what repositories of treasure. An almost invisible crevice in the hill turns out to be an enchanted cave, 150 feet deep, containing perhaps the hidden fountain of the water supply of Jerusalem."

But outside Jerusalem how much remains for us to learn! In the valley of the Kedron, Lieutenant Warren has sunk for 50 feet through debris again and again, without meeting the native rock. He is inclined to believe the tradition of the Mishna respecting the bridge of the red heifer, and is now searching for the foundations of the arches or pillars, as it will probably prove to have been a pier bridge, if it really existed.

Of archæological researches in other parts of the country there is little to be noticed. The few examinations which have hitherto been made here and there all encourage us to hope for a rich harvest wherever the work shall be systematically undertaken. Thus Captain Wilson's exposure of the ancient synagogues of Capernaum has, with a very trifling amount of excavation, revealed to us the extent and magnificence of an edifice associated in our minds with events scarcely less deeply interesting than those which cluster round the temple of Jerusalem. The remains of Seileon (Shiloh), Samaria, Gerizim, the cities of the Philistines, Beisan, (Bethshean), Heshbon, Rabbath-Ammon, all by their extent and interest invite careful exploration.

In physical research, much too has recently been accomplished. It is only within the last four years that we have become aware of the wonderful variety of the physical phenomena of Palestine, of the very interesting geological record contained in the region surrounding the Jordan valley, and especially of the unique fauna and flora confined within the narrow limits of the cliffs which overhang the Dead Sea. There, in a depression 1300 feet below the level of the Mediterranean, we find an almost tropical fauna and flora, containing a few Indian, many Abyssinian, and several peculiar and unique species in all the different forms of life, which make, in fact, a *tropical outlier*, just as the tops of the mountains, 10,000 feet high, supply us in their botany with boreal outliers. Not the least interesting fact ascertained is the peculiar character of the fishes of the Sea of Galilee and the Jordan, many of which are peculiar, some Nilotic; but whether peculiar or African, exhibiting remarkable affinities with the African forms of the entire and central lakes as far south as the Nyanza and the Zambesi river; and pointing to a time, perhaps at the close of the Miocene period, when this depression formed the northernmost of a long chain of fresh-water lakes, extending as far as south-east Africa.

The following Gentlemen were elected Fellows of the Society:—

J. W. LAIDLAW, Esq.
W. WILLIAMS, Esq.

Monday, 17th February 1868.

PROFESSOR PLAYFAIR, C.B., Vice-President, in the Chair.

The Chairman said—Gentlemen, before beginning the business of this meeting, I wish to refer to the lamented death of two of the office-bearers of this Society. Dr Burt, a member of our Council, was well known as a public-spirited citizen, who had long usefully devoted himself to the development of our institutions, and who, by his genial disposition and honesty of character, endeared himself to all those who enjoyed his acquaintance. The other loss has had a more marked relation to us, because in the death of Sir David Brewster this Society has lost its President, and this country one of her most distinguished philosophers. This is not the time to refer to the benefits which Sir David Brewster has bestowed upon science. These have been so numerous and important that we may expect a special evening to be devoted to this consideration. If Professor Tait, who is so capable to do justice to the merits of his deceased friend, were to undertake this subject, I am sure the Royal Society would hail with pleasure the announcement of his intention. Such a record of the achievements of a great philosopher has a much higher purpose than that of an éloge, for while they become landmarks in the progress of science over new and untrodden paths, they indicate the methods by which further progress is to be attained. Sir David Brewster entered this Society so long ago as 1808, and has been a constant contributor to its Transactions. It is worthy of notice that in the year 1808 the three candidates who became Fellows of the Society were, James Wardrop, surgeon, now in London, David Brewster, and Humphry Davy. In announcing to us at the opening of the session the death of Faraday, Sir David said that there was only one person living who had, like Faraday, taken all the medals of the Royal Society of London—the Copley, Rumford, and Royal medals. There is no one living now to claim this high honour, for the "one" so modestly hinted at was himself. In Brewster and Faraday the nation has suffered a heavy loss. Both were great philosophers and ardent Christians. We point to them as conclusive proofs that science and infidelity are not akin. I dare not trust myself to speak of the last days of Brewster. The perfect calmness and kindly consideration with which he wrote farewell letters to the public bodies which had honoured themselves by honouring him during life, were perhaps to have been looked for in one who viewed death as a means of attaining a higher and purer knowledge of God and of His works. But it is given to few men to possess their mental faculties unclouded to the last. A week before his death I had a long letter in his own handwriting, showing the liveliest interest in the affairs of the University, and in some optical discoveries regarding which he frequently corresponded with me. A few days after, while his mind was still clear, but his bodily frame weaker, he dictated a letter to the Council of this Society, in which he took a touching leave of his old associates and of the Society itself, and left to it, as a precious legacy, a research, nearly completed, which formed the death-bed study of the old philosopher. I am sure that the Society would not have wished to commence the business of this evening without some allusion to the death of their venerable President, and without some expression of sympathy with his widow and family. I therefore invite, from the body of the Society, a resolution which will record the sense of our own sorrow, and of our strong sympathy with that deeper personal affliction which is felt by the widow and children of so great and good a man.

Professor Sir James Y. Simpson moved the following resolution, which was seconded by Thomas Stevenson, Esq., and unanimously adopted:—

"The Royal Society of Edinburgh hereby record their deep sense of the great loss which the Society have sustained by the death of their late venerable and esteemed President, Sir David Brewster.

"Early in life an earnest worker and a happy discoverer in some of the most recondite fields of physical knowledge, Sir David Brewster has, during the last sixty and more years, continued with ceaseless energy to pour into the contemporary stream of science and literature a series of contributions of rare excellence and originality. At last he has passed from among us, as ripe in fame as in years; for he has reaped all the highest academic and other distinctions, both domestic and foreign, which a British philosopher can possibly win, and in his chosen departments of research he has left behind him no name more illustrious than his own.

"The Society further resolve to send a copy of this minute to Lady Brewster and the other members of Sir David Brewster's family, at the same time expressing their sincere sympathy with them in their late bereavement."

The following Communications were read:—

1. Observations relative to the Desireableness of Transporting from Alexandria to Britain the prostrate Obelisk presented to George IV. by Mahomed Ali Pacha. By Colonel Sir J. E. Alexander, K.C.L.S.

In the month of September last (1867), when visiting the Great Exhibition in Paris, I was particularly struck with the fine appearance of the obelisk of Luxor in the Place de la Concorde, and I thought that as the French had taken the trouble and gone to the expense of moving this highly interesting monolith, it was a reflection on our nation and on the engineering skill of Britain that the prostrate obelisk at Alexandria (one of Cleopatra's needles, as it is commonly termed), was not occupying a place of honour in England or Scotland.

This obelisk was presented to George IV. many years ago by Mahomed Ali Pacha, who also generously offered to move it on rollers to the sea, from which it is 200 or 300 yards distant, and embarking it on rafts and lighters, convey it to a vessel for transport to England.

The state of public affairs at the time, perhaps, prevented the accomplishment of this enterprise; now the time may be more favourable for it.

Sir Gardiner Wilkinson and other writers on Egypt and its antiquities are of opinion that Cleopatra's needles (one of which is upright) were brought by one of the Ptolemies from Heliopolis, near Cairo, to decorate a palace at Alexandria. On the obelisks appear the names of Thomes III. (B.C. 1463), of Remeses the Great, and of Oserei II. (B.C. 1232), long before Cleopatra's time. Sandys, who travelled in 1610, calls the prostrate obelisk "Pharaoh's needle," and says "it is half-buried in rubbish." It is of red granite, and, looking down a hole, its top is seen with crowned hawks sculptured on it. Lord Nugent, writing in 1845, says the hieroglyphics on three sides are well preserved. Colonel Ayton, of H.M. Bombay Engineers, informed me that whilst in Egypt in 1862, and whilst there was an idea of a memorial for Prince Albert first started, Mr Clark, of the telegraph department, uncovered the prostrate obelisk, removed the sand and rubbish from it, found the hieroglyphics on three sides in good preservation, and, as the obelisk was not then wanted, he covered it up again.

This obelisk, with others, is well ascertained to have been quarried at Syene, at the extreme boundary of Upper Egypt. It is not easy to find out how the hieroglyphics were graven on such a hard surface, and what was the process of hardening the bronze tools used for this purpose. The Messrs Macdonald of Aberdeen, and other workers in granite in this country, might be able to explain this: possibly the assistance of emery powder was brought in.

Denon alludes to Cleopatra's needles, and says they might be moved without difficulty, and form interesting trophies. To remove works of art from countries where they form ornaments and are conspicuous objects of interest is quite unworthy of a great people; but the obelisk in question lies in dishonour among low huts in the outskirts of Alexandria, and might well be spared to ornament one of our capital cities. In a conversation with Mr David Laing, the well-known antiquary, about it, he suggested the open space in front of the British Museum as the most appropriate for it. Still, it might not be sufficiently seen there—further west might be better, or in our Charlotte Square.

I corresponded with Mr Newton, the keeper of antiquities, British Museum, about the obelisk, and he writes—"It seems to me that if, by public subscription, a sufficient sum could be raised to transport this obelisk to England, it would be a just matter of national satisfaction; but you will understand that, while this may be a case fully justifying an appeal to the public for a subscription, it may not be one sufficiently strong to justify the trustees and officers of the British Museum in moving in the matter officially, because we have to make so many applications to the Treasury for grants for excavations, &c."

I communicated with the Peninsular and Oriental Steam Navigation Company regarding the means of transporting the obelisk, and the secretary for the managing directors states—"We would beg to suggest that the matter should be referred by you to the Foreign Office, whose agents have made all the necessary calculations on the subject, and without whose permission nothing could be done."

The Foreign Office was accordingly communicated with, and an answer was returned that the matter is now under the consideration of Lord Stanley.

The eminent civil engineer, Professor Macquorn Rankine, was asked what he thought of the means of transporting the obelisk, and he said—"I regret I cannot form any opinion whatsoever as to the best way of transporting the obelisk without having detailed information which, I believe, I could not obtain except by visiting the spot where it lies. The subject is undoubtedly one of very great interest, and I should very much like to be present when it is discussed."

In the Royal United Service Institution, London, I found thirteen large plans, carefully drawn, illustrating how, by means of inclined planes, a flat-bottomed vessel, machinery for raising the obelisk on a pedestal, &c., it could be sent to and set up in England. These plans are supposed to have been prepared in 1820 by Captain Boswell, R.N., for the Government, but no action was then taken in the matter.

It appears to me (having studied and been employed formerly as an engineer) that there might be no need for a vessel being built on purpose to carry the obelisk. A large Clyde lighter, raised upon, might transport it across the Bay of Biscay in summer; or if an old ship, sufficiently seaworthy, is got, and the masts taken out of her, and the beams cut across, the obelisk might be taken alongside, raised, and lowered into her, iron beams being ready, with bolts and screws, to connect and secure the cut beams of the vessel; then towed by a steamer to England. Once there, little difficulty would ensue before it occupied a place of distinction; but not necessarily on a pedestal, which would change its original character through giving it additional height. It is 64 feet long, weighs 284 tons, and is 7 to 8 feet square at the base of the shaft.

I understand that in an apartment in the Louvre part of the machinery is preserved by means of which the transport of the French obelisk was effected. This could be seen, or even lent to assist our engineers, and save heavy costs; and this need not be heavy, unless with gross mismanagement and a mere job made of it. Honestly gone about, the cost would be moderate.

Lord Stanley wrote me that he was not aware that the Parliament would vote a sum of money to move the obelisk. This might be asked, however.

I quite concur with Professor Piazzi Smyth in denouncing the barbarism of breaking off pieces of and carrying away Egyptian antiques; but I think we might remove the prostrate obelisk hidden and buried in the sand, leaving, of course, the twin obelisk set up in its place, and always most interesting as a "Cleopatra's needle." The prostrate one might be converted into building materials ere long, if not looked after.

Lately, in Glasgow, I made myself acquainted with the engineer of the Clyde Navigation, Mr A. Duncan. I went over the matter with him of the means of transporting the obelisk, and I suggested an iron casing or vessel built round it. He approved of this, and on my asking him to give his ideas on the matter—also to look at the plans from the United Service Institution—he kindly consented to do so; and his clear and excellent method for carrying out what is so much desired—the removal of the obelisk to Britain—is placed before the Royal Society of Edinburgh.*

2. On the Temperature of the Common Fowl (*Gallus domesticus*). By the late Dr John Davy. Communicated by Professor Allman.

Before proceeding to read Dr Davy's paper, Professor Allman made the following remarks:—

The duty has devolved on me of being the medium through which the paper just announced should be communicated to the Society—a duty not unmixed with melancholy; for when the distinguished author of that paper had placed it in the hands of the Council, there was nothing to prevent our looking forward to a continuance of those labours with whose results the pages of your Transactions have been so often enriched. But the pen which had worked unwearyingly for nearly sixty years is at last still; and the paper now before you closes in for ever the earthly labours of its author.

It has been suggested that, under these circumstances, it would be right to accompany the communication of Dr Davy's paper by a few words setting forth the salient points in his scientific life, and it is in accordance with this suggestion that I have drawn up the following short note. For the facts which it contain I am mainly indebted to his son-in-law, Professor Rolleston, of Oxford.

Dr John Davy was born at Penzance in May 1791; he died at his residence near Ambleside in January 1868. He was therefore in his seventy-seventh year at the time of his death.

* Sir William Wylde, in a pamphlet sent me by Sir James Simpson, states that, in the year 1839, he proposed that the obelisk should form a Nelson testimonial, with sphinxes at its base; two of which, after the capture of Alexandria in 1801, were built into the wall of the Custom-House near the principal landing-place. These might be recovered also without much difficulty, as they are in a manner buried where they are. At St Petersburg I saw the granite rock on which the statue of Peter the Great stands, and got the details of the manner of its transport.

His scientific labours began in controversy, and we find him, in 1811, defending the views of his brother, Mr (afterwards Sir) Humphry Davy, regarding the nature of potassium and sodium, and the composition of hydrochloric, or, as it was then called, oxymuriatic acid. His advocacy of his brother's views was in reply to the objections urged against them by Mr Murray, at that time a lecturer on Chemistry in Edinburgh. His letters on the subject were published in Nicholson's Journal, and the controversy ran through several successive numbers of that periodical.

It was at this time that he was studying medicine in Edinburgh, where he became a member and one of the annual Presidents of the Royal Medical Society—a society composed of students, and which has numbered amongst its members some of the most distinguished names by which medical science has been advanced during the present century. Among Davy's contemporaries in the Medical Society were Richard Bright and Marshall Hall. In 1814 he graduated in Edinburgh as M.D., with a thesis, "De Sanguine." It must have been about the period to which we now refer that he assisted his brother in the Royal Institution in London, and discovered chloro-carbonic gas. He became a Fellow of the Royal Institution of London on 17th February 1814.

During the Waterloo campaign in 1815, Dr Davy was attached to a general hospital in Brussels, and in the following year he was appointed staff-surgeon in Ceylon, where he remained until 1820. While there he accumulated materials for an account of Ceylon, which still continues a standard work on that island. During subsequent years he did duty on various foreign stations—the Ionian Islands, Malta, Constantinople, and the West Indies. He also served on home stations; and his value as a public servant was subsequently recognised by Government in his appointment to the post of Inspector-General of Army Hospitals.

In 1842 he was elected a Fellow of the Royal Society, Edinburgh.

The opportunities afforded by a life so singularly active and varied were not allowed to escape him; for, with an ardent love of inquiry, he possessed powers of observation which have rarely been surpassed, and a versatility which enabled him to direct these powers into the most diverse channels.

In comparative anatomy his name will be always associated with our knowledge of the circulation of the blood in the Amphibia; for though Meckel had previously discovered that the auricle in the *Pipa* or Surinam toad was divided into two chambers by a membranous septum, yet the German anatomist regarded this as an exception; and in common with every other anatomist, from the time of Harvey downwards, considered that the batrachian heart had, as a rule, only a single auricle, by which it was distinguished from that of the true reptiles. Davy, however, by showing that the auricle is double in the frog and in the common toad, established this important structural feature as an essential character of the Amphibia.

Dr Davy's extensive pathological experience afforded him the materials for his work on "Diseases of the Army." His Life of his brother Sir Humphry proves his aptitude for critical biography; while his numerous papers on Natural History, Chemistry, and Physiology, show still further how free from speciality was his active and inquiring mind.

It is his physiological researches, however, and more especially those on animal heat, which stand forth with greatest prominence in the long list of his scientific labours. His observations on temperature, which extend to almost every important group of the animal kingdom, and embrace animals in almost every possible condition, are among the most valuable with which this department of inquiry has been enriched, and will always continue to be referred to by the historian of physiological progress. Among the very earliest of his publications is a paper presented to the Royal Society in 1814, on the difference of temperature between arterial and venous blood, and his last—that now before you—is still on his favourite subject.

Dr Davy's place in the history of scientific discovery has none of the brilliancy of his brother's, but it will always be known as that of the uncompromising advocate of truth—the patient, painstaking, conscientious, and successful observer. The discovery of facts—the elucidation of positive truth—was his mission; he seldom indulged in speculation, and did not possess that genius for generalisation which shed such a lustre over the career of the great chemist.

The later years of his life were spent in his retirement at Lesketh How, near Ambleside. When I saw him there last autumn he had lost none of his love of research, and dwelt with all the delight of a young man upon the natural features of his neighbourhood; time seemed as yet to have leant but lightly on him, and his looks were no index to his years; but he was hardly what I had known him some years before, and was certainly sadder than had been his wont.

His last illness commenced with what he at first considered as only a severe attack of influenza; but alarming symptoms supervened, and against these no treatment had any power. Dr Rolleston writes to me that "he retained his faculties to the last, and to the last the watchword was, with him as with the Roman Emperor, 'Laboremus.'"

Abstract of the Paper.

The observations were made on birds of different ages and sexes, and under various conditions, and the following results were approximatively obtained:—

1. The average temperature of the common fowl is 107°·81.
2. The temperature of the sexes before maturity is comparatively high, being 108°·5, whilst that of the two sexes at this stage varies very little.
3. The temperature of the male, on the whole, irrespective of any particular age, is higher than that of the female, being as 108°·39 to 107°·3.
4. The temperature of the fully mature male is 108°·77.
5. The temperature of the laying hen is 107°·4.
6. During incubation the temperature falls, and is as low as 107°.
7. During moulting it rises to 108°·44.

The author further observed that the young fowl, soon after becoming tolerably fledged, and capable of securing adequate food, attains a comparatively high temperature, and that the temperature of nestlings is also comparatively high, facts which are scarcely in accordance with the views of Dr William Edwards, who, in his work "On the Influence of Physical Agents on Life," maintains that "the power of producing heat in warm-blooded animals is at its minimum at birth, and increases markedly until adult age."

3. Note on an Inequality. By Prof. Tait.

The tendency of the air contained in a soap-bubble is to expand in bulk, while that of the film is to contract in surface. Hence it becomes a curious question to decide whether both of these tendencies are not satisfied when two or more bubbles unite into one.

The excess of pressure inside a bubble, of radius r, over the pressure of the external air, is

$$p' - p = \frac{4\,T}{r},$$

where T is the contractile force of the film.

Hence $$\frac{p'}{p} = 1 + \frac{4\,T}{pr}.$$

Now if r' be the radius of the sphere which the contained air would occupy at the pressure p, we have

$$\frac{p'}{p} = \frac{r'^3}{r^3};$$

so that

$$r'^3 = r^3 + \frac{4\,T}{p}r^2.$$

The quantity $\frac{4\,T}{p}$ is exceedingly small, so that if we call it $3\,e$, we have very nearly

$$r' = r + e.$$

That is, the quantity by which the radius of a bubble must be increased, so that the pressure of the contained air may fall to that of the atmosphere, is independent of the radius of the bubble (unless it be very small).

Suppose two bubbles of radii r_1 and r_2 to coalesce, and let r_3 be the radius of the single bubble formed, we have (by expressing that the whole content is unchanged).

$$r_3^3 + 3\,er_3^2 = r_1^3 + r_2^3 + 3\,er_1^2 + 3\,er_2^2.$$

Hence, if δV express the increase of volume, δS that of surface we have

$$\delta V + e\,\delta S = 0.$$

Hence it appears that the volume increases and the surface contracts, or *vice versâ*, for they cannot *both* remain unaltered. And the physical circumstances show at once that the former is the case.

The formula just written shows that the volume increases by as much as that of a bubble, whose surface is equal to the diminution of surface, would change when it is so increased that the air it contains is at the pressure of the atmosphere.

It is obvious that the same process of physical reasoning may be applied to any number of soap-bubbles when made to unite; so that we may thus, without any analysis, conclude that for any number of positive quantities x, y, z, &c., we have always

$$x^2 + y^2 + z^2 + \ldots\ldots > (x^3 + y^3 + z^3 + \ldots\ldots)^{\frac{2}{3}}.$$

For if R be the radius of the bubble formed by the union of others of radii x, y, z, &c., we must have

$$R^2 < x^2 + y^2 + z^2 + \ldots\ldots$$

$$\text{and} \quad R^3 > x^3 + y^3 + z^3 + \ldots\ldots$$

from which the above result is evident.

Professor Tait exhibited to the Society a very easy mode of demonstrating some of Stewart's results regarding equality of absorption and radiation. Letters drawn with ink on a slip of platinum foil appeared brighter than the ground when the foil was heated in a large blowpipe flame, but darker than the ground on the other side of the foil. Thus, though glowing more brightly, the iron spots are really at a lower temperature than the foil beside them. The spectra of the two portions were described and their lengths compared, with the view of strengthening the proof; and the experiment was applied to suggest an explanation of the singular observation by which Secchi was led to conclude that iron is transparent at a red heat.

Monday, 2d March 1868.

DAVID MILNE-HOME, Esq., Vice-President, in the Chair.

The following Communications were read:—

1. Observations on the Spores of Cryptogamic Plants, and on the Reproductive Process in some Algæ and Fungi. By Professor Balfour.

In this paper the author alluded to the spore as the ultimate germinating cell of cryptogamic plants. The true spore is the product of impregnation, but there are also bodies called spores which, so far as known, do not depend on the process of fertilisation. Occasionally the term spore includes both the spore and the spore-case or sporangium. The structure of the spore was described, and its resemblance to the pollen grain, both as regards its anatomy and germination, was shown. Ciliated moving spores were considered, and reference was specially made to the movements of the non-ciliated spores of Helminthora, which, when discharged, continue to change their form for several hours like the Amœba, becoming in turn oblong, pyriform, rhomboidal, &c.,—lengthening and contracting, swelling at one point and shrinking at another.

The germination of spores was considered. Some of them were shown to produce a prothallus, and were hence called *Thalloid spores;* while others had no prothallus, and were therefore *Athalloid.* In the case of the former, the prothallus was sometimes produced externally, as in ferns, and hence the spore was *Exothalloid;* in other cases the prothallus was internal, as in Lycopods, and the spore was called *Endothalloid.*

In all the great divisions of cryptogamic plants, bodies called Antheridia and Archegonia have been detected,—the former usually containing spermatozoids.

The conjugation in Tyndaridea, Zygnema, and Mougeotia was then considered, and the function of a compound spore in some species of Vaucheria and in Fungi, such as *Rhizopus nigricans*. In the last-mentioned plant the compound spore is called a Zygospore.

Special attention was drawn to the fertilisation of Florideæ, as demonstrated by Bornet and Thuret. The formation of antheridia with corpuscles, and of a peculiar hair-like body called Trichogynium was noticed. The cell at the base of this latter organ, after fertilisation, is transformed into the Cystocarp, which is sometimes supported on cells called the Trichophore. The formation of the Cystocarp containing spores was exhibited in large drawings of Nemalion, Batrachospermum, Helminthoro, Liagora, Callithamnion, Bonnemaisonia, and Polysiphonia. It was shown that, in the case of Dudresnaya, there is a long Trichogynium with spiral coils near its base, and that the cells below that organ elongate so as to form flexuose filaments, which pass in a serpentine manner among the ramifications of the sea-weed, and come in contact with the fructiferous filaments which they are destined to fecundate. These conducting tubes are attached successively to the terminal cells of the filaments, becoming incorporated with them. Thus, the fructiferous filaments are united together by a sort of network of delicate tubes. These tubes convey the fertilising influence from the Trichogynium to the Ampullæ, from which the Cystocarps are formed. It was thus shown that the fecundating apparatus of the Florideæ consists of antheridia containing corpuscles, and of a small cellular body terminated by a unilocular hair or Trichogynium. Fecundation is produced by the union of antheridian corpuscles with the Trichogynium, followed by the development of the capsular fruit or Cystocarp.

In some cases, as in Nemalion, the fertilisation is direct, the influence of the antheridian corpuscle being at once conveyed by the Trichogynium to the rudimentary cell of the Cystocarp. In other cases, as in Dudresnaya, the action is less direct,—the influence of the antheridian corpuscles being conveyed by connecting tubes, which pass laterally from the base of the Trichogynium to numerous fructiferous filaments, on which the Cystocarps are finally developed.

The author then considered the case of the spores contained in the Thecæ of Mosses and Ferns. These spores have not been shown to be produced by direct fertilisation. In these plants there is a prothallus bearing the reproductive organs. The spermatozoids of the antheridian cells come into contact with the archegonial cell, and after fertilisation the cell remains attached to the prothallus, and from it is developed the stalk bearing the sporiferous theca in Mosses, and the sporangiferous frond in Ferns. These so-called spores seem to resemble buds. Remarks were made on the buds of the Marchantia, the gonidia of Lichens, and the tetraspores of Algæ, as well as on the production of separable buds in the higher classes of plants, and the production of ova and buds in Medusæ. The tendency, also, of the spores of anomalous ferns to produce fronds having the peculiar character of these varieties, was considered as indicating their alliance with buds.

The paper was illustrated by a large series of coloured drawings and by models, as well as recent specimens of ferns in various states, from the prothalloid condition up to the frondiparous state, bearing sporangia, and also at times producing viviparous buds.

2. Note on a Recent High Tide on the East Coast of Britain. By George Robertson, M. Inst. C.E. (Plate.)

The greatest height to which the tide has ever been known to rise at any given place is of the utmost consequence to the marine engineer. It forms an important element in the design and in the estimate for any works in the sea; and any error on this point might lead to the most lamentable results both to life and property, especially in the construction of embankments for the reclamation of land.

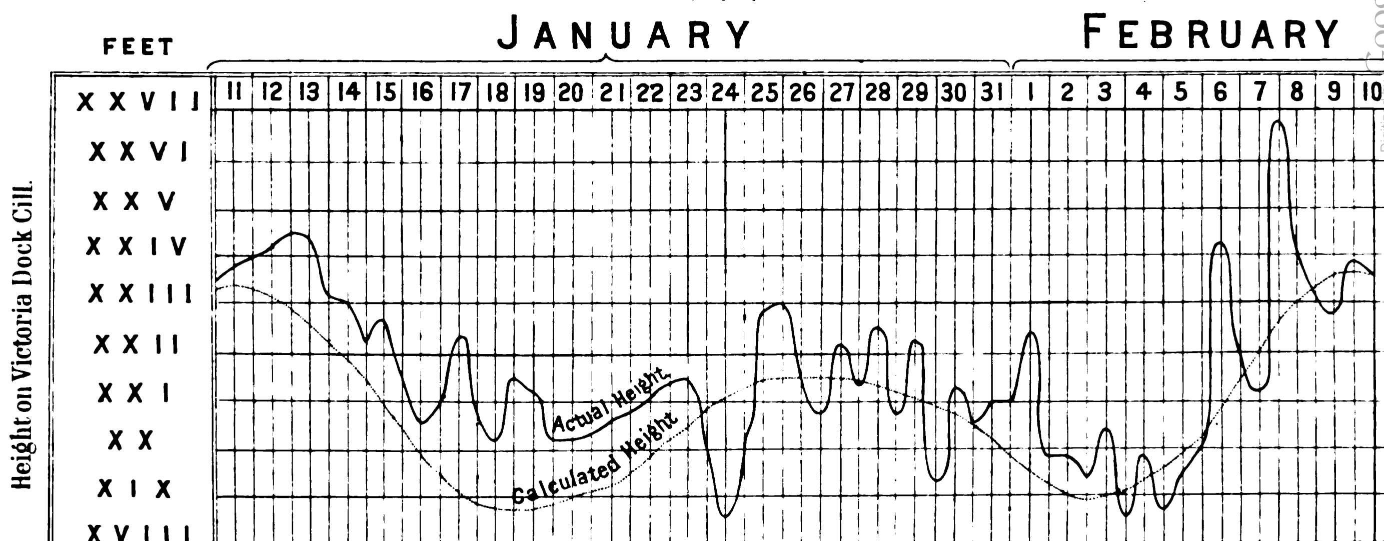
I have therefore thought that a notice of the tide of the 8th of last month might not be uninteresting to the Society, as it is one of the highest, if not indeed the highest, that has ever been recorded with accuracy on the east coast of Britain, and the most remarkable example with which I am acquainted of the effect of wind in raising the level of high water on that coast.

The months of January and February of this year have been remarkable for the long continuance and violence of westerly winds—at times approaching to the fury of a hurricane.

A long continuance of these winds has always the effect of

HIGH-WATER AT LEITH.

1868.



Date	Cal^d Height	Actual Height	Wind
January 11	23' 3"	23' 5"	ESE
	23' 4"	23' 9"	ESE
12	23' 3"	23' 11"	WSW
	23' 1"	24' 2"	SW
13	22' 10"	24' 5"	EbyS
	22' 7"	24' 4"	NNW
14	22' 3"	23' 1"	SSW
	21' 11"	23' 0"	SSW
15	21' 5"	22' 8"	SW
	20' 11"	22' 8"	WSW
16	20' 5"	21' 7"	SW
	19' 11"	20' 6"	SW
17	19' 6"	20' 11"	SW
	19' 1"	22' 3"	SW
18	18' 10"	20' 8"	SW
	18' 8"	20' 2"	ESE
19	18' 8"	21' 5"	W
	18' 8"	21' 2"	SW
20	18' 9"	20' 3"	NE
	19' 0"	20' 3"	NNW
21			WSW
	19' 3"	20' 7"	WSW
22	19' 6"	20' 9"	NNE
	19' 10"	21' 1"	NbyE
23	20' 3"	21' 4"	ESE
	20' 6"	21' 5"	WSW
24	20' 10"	20' 3"	SSE
	21' 1"	18' 7"	S
25	21' 3"	20' 2"	SW
	21' 5"	22' 10"	WSW
26	21' 6"	23' 0"	WSW
	21' 6"	21' 6"	WSW
27	21' 6"	20' 10"	SW
	21' 5"	22' 2"	WSW
28	21' 4"	21' 4"	WSW
	21' 3"	22' 6"	WSW
29	21' 2"	20' 9"	WSW
	21' 1"	22' 3"	WbyN
30	20' 11"	19' 3"	WSW
	20' 8"	21' 9"	SW
31	20' 5"	20' 6"	WSW
	20' 1"	21' 0"	SW
February 1	19' 10"	21' 0"	WSW
	19' 6"	22' 5"	WSW
2	19' 3"	19' 9"	WSW
	19' 0"	19' 8"	SW
3	18' 11"	19' 4"	WbyS
	19' 0"	20' 4"	SW
4	19' 1"	18' 7"	WSW
	19' 4"	19' 10"	SW
5	19' 9"	18' 8"	WSW
			WSW
6	20' 2"	20' 0"	WSW
	20' 9"	24' 3"	WSW
7	21' 4"	22' 3"	SSW
	22' 0"	21' 2"	SW
8	22' 6"	26' 10"	ENE
	23' 0"	24' 4"	N
9	23' 4"	23' 3"	SE
	23' 6"	22' 9"	SW
10	23' 7"	23' 10"	WSW
	23' 6"	23' 6"	WSW

Mould & Tod, Lith^rs Edinburgh

keeping up the tides round the coast of Britain; and if the wind then goes suddenly round to the north of west, it brings in the tidal wave from the Atlantic in great strength round the north of Scotland, and consequently raises the level of high water on the east coast to an abnormal height, more especially as the wave works its way southward to the shallow water and obliquely shelving shores of England.

The effect of these long-continued westerly winds on the tides culminated, on the morning of the 8th of February, in a tidal wave of perhaps unprecedented height on the east coast—the same wave which brought high water to the west of England and Ireland on the afternoon and evening of the 7th.

I shall endeavour to trace the height of this wave from the time it passed the Pentland Firth till it met, off Lowestoft, the counteracting low water of the wave which travels rounds the south of England.

Nothing remarkable in the height of the tide was noticed at Wick, where the tides seem to be more locally affected by gales from the S.W. to S.E., which raise the level of high water some 12 or 15 inches.

As the wave swept along the coast of Banffshire it increased in height, till at Aberdeen the level of high water was 3 feet 6 inches above the calculated height. High water took place here at 1.16 A.M., and was 24 feet 4 inches on the dock cill. The average of spring tides for five years at Aberdeen is 22 feet on the cill, but the tide of the 8th was not expected to rise so high as this, and it is the highest recorded since 1862, before which the records appear uncertain.

At Leith the tidal wave rose at 1.58 A.M. to the height of 26 feet 10 inches on the Victoria Dock cill. The calculated height, according to Reid's tables, which are those used at the port, should have been 22 feet 6 inches, so that the actual height was 4 feet 4 inches more than the calculated height; the greatest discrepancy I have ever heard of at Leith. Between the previous high water on the afternoon of the 7th and that on the morning of the 8th, there was no less a difference than 5 feet 8 inches. The extraordinary amount of this difference will be better understood if we bear in mind that it represents the ordinary total rise between high water of low neap tides and high water of high spring tides, an amount which it usually takes seven days and fourteen tides to accomplish by successive increasements; but which, in this case, was done at one bound.

On going over the tide-books at Leith for as far back as time has permitted, I find that during the last eighteen years there have been ninety tides, or five per annum, which reached the level of 25 feet on the Victoria Dock cill. Of these only two tides have been so high as 26 feet, viz.—the morning tide of October 26, 1865, and the morning tide of January 29, 1850. The latter tide reached the height of 26 feet 8 inches, or 2 inches lower than the one of the 8th ult.

A very high rise of tide is recorded on several of the charts as having taken place on July 18, 1829; but, on looking at Reid's Commercial List for July 21, 1829, I find that on the day recorded on the charts the tide was a very ordinary one, only rising 2 inches higher than was expected, and being exceeded by those following. So that there is some mistake which makes this high tide very doubtful, to say the least.

I have prepared a diagram of the height of high water at Leith for a tidal month, which shows the extraordinary state of oscillation in which the tides have been during the late boisterous weather. It will be observed that from the 11th to the 24th January the tides were all above the calculated height. On the 24th—the day of the great gale in Scotland—the wind went violently round to the south, and immediately the tides fell to more than 2 feet below their proper height, rising again as the wind went back to the west. After a fortnight of most unusual oscillation, the tide rose on the 8th February, with the turn of the wind to the north, to the unprecedented height alluded to in the paper.

I regret that I cannot give a diagram of the low waters for the same period, which would have been very valuable and instructive; but at Leith, as at many other ports, no regular record is kept of the low waters.

The height of the tidal wave of the 8th, when it had reached the mouth of the Tyne, in Northumberland, was much the same as at Leith—a little less, however, as might be expected from the configuration of the coast. The height of high water in Shields harbour was 4 feet more than expected, and was 3 feet 2 inches above high water of spring tides (Rennie's datum). This is 10 inches higher than any tide previously recorded. The tide on the 6th was also unusually high, being 3 feet 2 inches more than calculated.

At Sunderland the tide was 3 feet 11 inches more than the calculated height.

At Hull it was high water at 5.45 on the morning of the 8th, and the tide rose to a height of 30 feet 5 inches above the Humber Dock cill, or no less than 5 feet 5 inches more than was calculated. The tide of the previous day had risen only to 23 feet 4½ inches above the cill; so that we have at Hull the extraordinary difference of 7 feet ½ inch between two successive tides—the greatest amount of aberration I have hitherto discovered in the east coast tides.

The average height of springs at Hull is 26 feet 8 inches on the dock cill, there being very seldom tides of 28 feet. A list of the high tides at this port has been sent to me, and I find that since 1788 there have been only twenty-three tides which have reached a height corresponding to 29 feet on the Humber Dock cill—an average of only one tide in three and a half years. Of these twenty-three tides only one reached the height of 30 feet, and that was on the 18th February 1816, during a gale from the N.W. The tide was then 4 inches lower than that of the 8th ult., which is the highest tide on record at Hull.

The tidal wave appears to have reached its maximum about Hull, having travelled from Aberdeen in 4h. 29m.—according to Imray's tide-tables—exactly one hour faster than usual, as might be expected from so strong a flood. This is at the rate of nearly eighty miles per hour—a great speed for a wave of the first order in 50 fathoms water, and equal to what it usually is in 70 fathoms.

About this point, or perhaps nearer Yarmouth, the tide began to feel the counteracting influence of the low water of the wave which sweeps along the south and west of England.

By the time high water reached Lowestoft the level had apparently fallen a little; for though the tide was 20 feet 9 inches, or 4 feet 3 inches above the average, still this is 2 feet 3 inches less than the tide of the 2d December 1867, which is the highest on record at Lowestoft. On the 25th of January last—the day after the great storm in Scotland—the low water at Lowestoft was 4 feet below the average; and the harbour-master writes that they have had extraordinary fluctuations during the last three months.

When the wave reached the London Docks it was still extremely high, being 3 feet 6 inches above Trinity datum; but this is not unprecedented; for on January 20, 1850, it was an inch higher; and on November 12, 1852, it was 2 inches higher. This latter is the highest tide on record at the London Docks, and the water rose to 29 feet 1 inch on the dock cill, or 3 feet 8 inches above Trinity datum.

In examining the height of that portion of the great tidal wave from the Atlantic which sweeps along the south and west of England, I find a marked difference from the east coast tide. Instead of being the highest wave on record, it was a tide, slightly in some places only above the calculated height, and in no respect very remarkable as to height of high water.

At Cork the water did not rise above the average of springs either on the 7th or 8th February; but there was a great ebb, which left banks exposed in the harbour that had not been visible for years.

The wave which reached Portsmouth Dockyard at 11 P.M. on the 7th was the same as calculated; but on the morning of the 8th the tide rose to 21 feet 4 inches on the cill of Dock No. 6, which is the zero, or 1 foot 11 inches above calculation. This is not very remarkable, for there was a tide at Portsmouth on November 1840 which rose 2 feet higher, and is the highest on record.

At Dover the wave on the 8th was unusually high, being 2 feet 4 inches above the calculated height. High water was 21 feet 9 inches above low water of ordinary springs; but this is not the highest tide known, for the engineer of the Admiralty works writes that he has on record one of 22 feet, or 3 inches more.

That branch of the tidal wave which flows up St George's Channel towards the Irish Sea had nothing extraordinary in its height.

At Bristol the tide of the 7th was 29 feet 8 inches, or only 9 inches more than calculated. On the 8th it was 31 feet 1 inch—the height expected; this, too, at a place subject to very high tides, the highest on record being that of January 29, 1846, which was 37 feet 4 inches at the Cumberland Basin.

At Dublin the height on the 7th was 13 feet, and on the 8th 12 feet 7 inches above zero; but the engineer of the Ballast Board writes that this is nothing remarkable, and that he has a record of a tide 2 feet 9 inches higher than the one on the 7th ult.

The tides at Holyhead were much as usual, and were exceeded by those of the 9th and 10th.

At Liverpool the tide of the 7th was 1 foot 5 inches above calculation; but on the 8th it was 4 inches below what it ought to have been.

At Belfast the tide of the 7th was 11 feet above datum; on the 8th, 11 feet 2 inches; the average of tides being 10 feet. On the 13th and 14th the tides were at least 2 feet higher.

I have brought this notice of the recent high tide on the east coast before the Royal Society, not only for the purpose of having on record one of the most remarkable tide waves occasioned by wind in this country of which we have any authentic account, but also for a practical purpose.

The knowledge that there is a tidal wave of more than ordinary height on its way to any of the great commercial ports of Britain would be of great value to merchants and sailors. There are always vessels lying in harbours and docks of too great a draught of water to allow them to sail during neap tides, or even during low springs. Few ports in this country have sufficient depth of water to prevent large vessels being "neaped," as it is called. These vessels might be able to sail several days, perhaps ten or more, sooner than they could otherwise do, or be docked, as the case might be, were they timely warned of the fact that a tidal wave of more than the usual height was on its way to them, bringing, in extreme cases, a high spring during a low neap.

Information regarding high or low tides might just as easily be sent to each great port, without any extra expense, and with far greater certainty of prediction, as the warnings for high winds are at present, and might be signalled with equal simplicity.

There is always plenty of time for it. The tidal wave which reaches the west coast of Ireland about four o'clock at springs divides into two parts. The northern branch takes eight hours to reach Aberdeen, nine hours to reach Leith, fourteen hours to reach Hull, and twenty hours to reach London. The southern branch takes seven hours to reach Portsmouth, and seven hours to reach Liverpool.

The character of the winds prevailing at the time would, with a little experience, give a good idea of whether the coming tide would be felt most on the east or the west coast, and also of its probable height at different places.

The knowledge of the approach of an extraordinarily high tide might also occasionally be of some use in the preservation of property from its effects.

In these days of steam I am sure that information about tides would be quite as generally appreciated as information about winds, by both merchants and sailors. I believe that the Royal Society of Edinburgh would do a great practical service to the shipping interests of this country if they could prevail on the Meteorological Department of the Board of Trade to arrange that notice should be sent to all the principal ports of Britain, not only of the expected winds, but also of the expected tides, whether unusually high or unusually low. The storms of wind may or may not arrive—for the wind "bloweth where it listeth;" but the tidal waves flow and ebb without fail, with the speed of the hurricane, but the silence of the grave.

The following Gentlemen were elected Fellows of the Society:—

J. Samson Gamgee, Esq., Surgeon to Queen's Hospital, Birmingham.
Rev. D. T. K. Drummond, B.A. Oxon.
Rev. Joseph Taylor Goodsir.
Major John H. M. Shaw Stewart, Royal Engineers.

Monday, 16th March 1868.

Dr CHRISTISON, Vice-President, in the Chair.

The following Communications were read :—

1. The Mean Pressure of the Atmosphere over the Globe for the Months and for the Year. Part I.— January, July, and the Year. By Alexander Buchan, M.A., Secretary to the Scottish Meteorological Society.

The three charts which were exhibited, showing, by isobarometric lines, the mean atmospheric pressure over the globe, during January, July, and the year, were constructed from observations made at 358 places thus distributed over the earth,—167 in Europe; 51 in Asia; 22 in Africa and adjoining islands; 35 in South America, West Indian Islands, and Atlantic; 63 in North America; and 20 in Australasia and Antarctic Ocean. Of the European stations, 12 are in Scotland, 14 in England, 27 in Austria, 12 in Italy, 10 in France, 10 in the Netherlands, 9 in Norway, and 57 in the Russian empire, &c. The list might have been largely increased; thus a larger number might have been given from the 80 Scottish stations; but the 12 given were judged sufficient to represent the mean atmospheric pressure of this country.

In the British islands the means were uniformly taken for the ten years from 1857 to 1866, in order that they might be strictly comparable with each other; and the means of several European stations were calculated for the same year. In the United States of America the means were uniform for the six years from 1854 to 1859. In selecting the stations, respect was had to the obtaining of a good mean,—that is, observations for a sufficient number of years to show, as nearly as possible, the true mean. For example, at Bombay, from 1847 to 1860, the lowest mean for July was 29·598 inches in 1851, and the highest 29·673 in 1853. This regularity in the pressure of the same month, from year to year, is a feature common to all tropical countries; and hence, in such places, a few years were accepted as a good mean. On the other hand, since the mean pressure at Reykjavik, Iceland, during January 1867, was 29·913 inches; during February, 29·359 inches; and during March, 30·037 inches, it is evident that a good many years are required to represent the mean of the month at Reykjavik. Hence, a very subordinate position, if any at all, has been given to observations from such places, unless they embraced a considerable number of years. The following are a few of the places, with the number of years, for which the means are given:—Sitka, 15; Algiers, 10; Hobart Town, 25; St Louis, Mauritius, 13; Bogoslovsk, 26; Nigni-Tagilsk, 21; Barnaul, 19; Nertchinsk, 18; Pekin, 14; Calcutta, 11; Tiflis, 14; Baku, 17; Alagir, 15; Jakobshavn, 10; Reykjavik, 13; Hammerfest, 13; St Petersburg, 19; Archangel, 18; Zlaloust, 28; Lugan, 22; Christiania, Cracow, and Kursk, 27; Brussels, 33; Gand, 26: Geneva, 25; Ahun, 34; Verona, 73; Bologna, 45; Milan, 25; Turin, 74; most of the Austrian stations, 14 to 18, &c.

Tables were prepared ruled with columns for the insertion of the following information:—1. The place and country; 2. The source or authority from whence the observations were obtained; 3. The number of years of the means; 4. These years specified; 5. The hours of observation; 6. The latitude; 7. The longitude; 8. The height above the sea in English feet; 9 to 21. The simple arithmetic means for each month and for the year.

For reducing to sea-level a table was prepared from the formula and Table XVI. given in Guyot's Meteorological and Physical Tables, D, p. 89. This table, calculated for each 5° F. of the temperature of the air—from 40° to 90°—was used in all cases where the height did not exceed 800 feet. For higher situations, the reduction was made by means of Dippe's method, as detailed in Guyot's tables, D, p. 60. With the correction for height was included the correction for daily range.

For many means I have been indebted to the labours and writings of Buys Ballot, Carl Jelinek, Dove, Quetelet, James, and Kuppfer, and particularly to Secchi's admirable abstracts, which have appeared from time to time in the "Bulletino Metereologico."

The means, so reduced, were then entered on charts of the globe drawn on Mercator's Projection, from which isobarometric lines were drawn for every tenth of an English inch in the difference of the pressure. The lines of 30 inches and upwards were coloured red, and the lines representing pressures less than 30 inches were coloured blue. Thus, the portions of the earth's surface where the pressure was above or below the average (30 inches) could be seen at once.

Mean Atmospheric Pressure for July.—The lowest pressures occur over the land and the highest over the ocean; or, the lowest pressures occur in the north hemisphere and the highest in the south hemisphere. The greatest extent of high pressure extends quite round the globe from a little south of the equator to 40° lat. south. In the South Atlantic, in lat. 20°, this rises to 30·348 inches; in the North Atlantic there is a corresponding area of high pressure rising at lat. 35° to full 30·348 inches; and this region of high pressure extends over the south-west of Europe and the south-east of the United States. There is also a region of high pressure in the North Pacific, but it only amounts to about 30·1 inches.

The greatest extent of low pressure occurs in Asia, amounting in the central regions of the Continent only to 29·5 inches. Pressures are also low in the interior of North America, and round the north and the south poles.

Mean Atmospheric Pressure for January.—In January the highest pressures are over the land and the lowest over the ocean; or, the highest are in the north hemisphere and the lowest in the south hemisphere. Thus, taken in a general sense, the mean pressures of July and January are reversed.

The region of highest pressure occurs in the interior of Asia, where it amounts to 30·4 inches, being thus nearly one inch greater in winter than in summer. The area of high barometer (above 30 inches) is continued westward through Europe south of the North Sea and the Baltic; the north of Africa; the North Atlantic, between 15° and 45° lat.; North America, except the north and north-west; the West India Islands; and the North Pacific, as far west probably as 150° long. W. The effect of the Mediterranean and adjoining seas, which are at this season warmer than the land, in lowering the mean winter pressure, and thus breaking the continuity of the isobarometric 30·2 inches, and preventing its extension from the Pacific to the north of the Lena, in Siberia, is very striking. All the charts show similar disturbances of the lines of equal pressure over the same region. There are two other regions, though of comparatively small extent, where the mean pressure exceeds 30 inches,—one in South Atlantic, and the other between South America and Australia.

There occurs in the North Atlantic an extensive diminution of pressure, which deepens northwards till the greatest depression (29·5 inches) is reached in Iceland.* It is this low pressure over the North Atlantic, together with the high pressures in North America, in the Atlantic south of lat. 40°, and in Asia, which furnishes the key to the winter climates of North America and Europe. Another and equally remarkable depression occurs in the North Pacific, having its course of greatest depression (29·6 inches) in the ocean between Kamschatka and Sitka in the north-west of America. The pressure is also under the average in the south of Africa and in South America.

The equatorial depression stretches quite across the globe in an irregular belt, which attains its greatest breadth in Africa and its least in the Pacific. In crossing the Indian Ocean it does not lie parallel to the equator, but slants from Tamatave in Madagascar 18° lat. S. to the coast of Sumatra, in 5° lat. S. It is in this trough that nearly all the tropical storms of the Indian Ocean have their origin.

Mean Atmospheric Pressure for the Year.—There are two broad belts of high pressure passing completely round the globe,—the one north, the other south of the equator,—enclosing between them the low pressure of the tropics, through the centre of which runs a narrow belt of still lower pressure, towards which the trade winds blow. The southern belt of high pressure lies nearly parallel to the equator, and is generally of uniform breadth throughout. But the belt north of the equator has a very irregular outline and great differences in its breadth and in its inclination to the equator,—these irregularities being due to the unequal distribution of land and water in the northern hemisphere.

Considered in a broad sense, there are only two regions of low pressure, one round each pole, bounded by or contained within the belts of high pressure just referred to. The most remarkable of these

* The diminution of pressure in this region is even greater than this in February.

is the region of low pressure round the south pole, which probably is subject to little variation throughout the year. The depression in the neighbourhood of the north pole is divided into distinct centres, at each of which a diminution of pressure still further below the average pressure prevails. These are the north part of the Atlantic and the north part of the Pacific oceans. There is also a smaller area of low pressure in Hindustan, caused altogether by the low summer pressure of that region during the south-west monsoon. The centre of Asia is also a little below the average, owing to the very low summer pressure.

These differences in the pressure arise from the unequal distribution either of the temperature or of the moisture of the atmosphere. Of these, considered as disturbing agents, by far the most important is the moisture,—thus giving to this element a paramount claim on our regard in studying winds, storms, and other atmospheric changes.

The relations of the barometric pressure to the prevailing winds, the varying temperature, and the rainfall in different parts of the world, were pointed out. Isobarometric and isothermal charts,—(1.) Of Europe and Western Asia, from the 18th to the 26th of December 1866; and (2.) Of Europe, from the 19th July to the 1st of August 1867, were exhibited; and it was shown that the remarkable deviations from the mean pressure of these months which then prevailed were accompanied with equally remarkable deviations from the mean temperature of the same months.

2. On the History of the Sun's Distance Determinations; and on Scriptural and Scientific Probabilities. By William Petrie, Esq. Communicated by Professor C. Piazzi Smyth, March 16, 1868.

This paper was accompanied by two diagrams, one of them representing more particularly the features attending on space, and the probable errors of several modern determinations therein; and the other representing the chronological order of the events, or progress made by man from the earliest times, down to the present, in ascertaining that most important of all questions in astronomy and general physics, viz., the true mean distance of the earth from the sun.

At the dawn of human science, properly so called, or of efforts in schools and societies to attain to a knowledge of nature by observation and measure, and when the Greeks sowed the first seeds of those methods which have since developed into the giant tree of modern science—that is, in the ages of 600 B.C. or 500 B.C.—their greatest philosopher concluded the sun to be hardly more than 10 or 12 miles from the surface of the earth. But they increased that estimate, as their science advanced, to 14,000 miles in 50 years; and to 5 millions of miles in 200 years more. At that point, however, knowledge remained stationary for 1800 years, and continued perfectly satisfied with the truth of that result,—until Kepler showed, in 1620 A.D., that the distance was more nearly 26 millions of miles; and La Caille, in 1750 A.D., that it was nearly 78 millions of miles; and the transit of Venus, in 1769, that it was 95 millions of miles.

This last determination was, however, over the truth, and subsequent measures have been reducing the quantity again; but at the same time oscillating, sometimes on one side and sometimes on the other, of a mean quantity nearly 92 millions of miles in length.

Now this quantity—which modern science seems to be approaching only, but has not yet definitely arrived at, or settled for society, and is not likely to be able to do so until the next good transit of Venus, in 1882 A.D.—is the quantity marked off for the sun's distance in the Great Pyramid of Jeezeh, by the ancient architect or designer thereof, and in so early a period of the world as to have been 1600 years before the very embryotic commencement of Greek, or any other known human, science; or at a time when it was perfectly impossible for any man then living, to have had any means at all sufficient to solve the problem even approximately!

How, then, was it solved? and whence came that marvellously accurate physical knowledge, perfect, apparently, at its very outset, and in a building which has lived through all historic times of all nations and peoples, but never been known to possess those scientific details before, not even by the Egyptians themselves?

This great question much occupies Mr Petrie, after having detailed clearly a large quantity of numerical and other particulars connected with the establishment of all the positive and material facts of the case; and his solution leads him ultimately to some interesting correlations with certain portions of Scripture exegesis.

3. Physical Proof that the Geometric Mean of any Number of Quantities is less than the Arithmetic Mean. By Professor Tait.

If a number of equal masses of the same material be given, at different temperatures, and enclosed in an envelope impervious to heat, they will finally assume a common temperature; which is the arithmetic mean of the initial temperatures, if the material be one whose specific heat does not vary with temperature.

But they may be brought to a common temperature by means of reversible thermodynamic engines employed to obtain the utmost amount of work from the initial unequal distribution. This question was first investigated by Thomson (*Phil. Mag.* 1853, "On the Restoration of Energy from an unequally heated Space"), and the application of his method to the present problem shows that the final common temperature of the masses, when as much work as possible has been obtained from them, is the geometric mean of the initial temperatures; but this investigation introduces the condition that the temperatures must be measured from the absolute zero.

Obviously the whole energy restored is proportional to the excess of the arithmetic over the geometric mean.

Far more complex analytical theorems may easily be proved by means of the above process; for instance, if t_1, t_2, ..., c_1, c_2 . . . . be any positive quantities, we have

$$\frac{c_1 t_1 + c_2 t_2 + \ldots\ldots}{c_1 + c_2 + \ldots\ldots} > \left(t_1^{c_1} t_2^{c_2} \ldots\ldots\right)^{\frac{1}{c_1 + c_2 + \ldots}}$$

4. On the Dissipation of Energy. By Professor Tait.

The paper contains some curious applications of the principle of dissipation to the conduction of heat, the connection of heat and electricity, thermo-electric currents, the electric convection of heat, &c. But in this abstract we confine ourselves to one very simple case of the conduction of heat, as the hypothesis on which it is investigated is fundamentally assumed in all the other applications.

If an infinite plate be kept permanently heated in layers, each of equal temperature throughout—the temperature rising gradually from one side to the other—the hypothesis is made that the temperatures of any three contiguous layers (of equal thickness) so adjust themselves that the least possible energy can be restored from the system of three. From this it immediately follows that if x_1 be the thickness of the plate, t_0 and t_1 the (absolute) temperatures of its sides; and if the specific heat be the same for all temperatures between t_0 and t_1: the temperature t at a distance x from the side at t_0 will be

$$t = t_0 \epsilon^{\frac{x}{x_1} \log. \frac{t_1}{t_0}}.$$

But if k be the conductivity of the substance, at temperature t, we have for the flux of heat

$$f = k \frac{dt}{dx} \propto kt.$$

This must be the same throughout the plate, because there is equilibrium of temperature, and therefore

$$k \propto \frac{1}{t}.$$

The only published experiments, so far as I am aware, by which this result can be tested, are the very valuable series by Forbes (*Trans. Roy. Soc., Edin.* 1864), which are, unfortunately, confined to iron. They agree uncommonly well with the above theoretical result, as the following short table shows :—

t	k	kt
290° C.	0·0164	4·76
330°	0·0130	4·24
400°	0·0110	4·40
440°	0·0105	4·58
476°	0·0100	4·76
561°	0·0090	5·04

No account has, in this abstract, been taken of the alteration of specific heat with temperature, which is as yet only approximately known, but which is applied in the paper to account completely for the increase of kt with temperature. As to the increase of kt at the low temperature of 290° C., it may be remarked that the first two or three numbers in Forbes' table are (as he points out) probably much less accurate than those which follow them, on account of the temperature at which they were obtained, which was but little above that of the atmosphere.

The following Gentlemen were elected Fellows of the Society:—

John Stevenson, Esq.
Rev. J. F. Montgomery.

Monday, 6th April 1868.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Flow of Solids, with the practical application in Forgings, &c. By M. Tresca, Sub-director of the Conservatoire des Arts et Metiers, Paris. Communicated by T. C. Archer, Esq.

2. On the Figures of Equilibrium of Liquid Films. By Sir David Brewster.

This paper is a continuation of that read on 4th February 1867, and already published in the Transactions. Instead, however, of the liquid films being formed within systems of wires, hollow glass vessels are here employed. By using a single or double cone of glass open at both ends, and inserting within it small bubbles blown from a quill or tube with a small aperture, regular binary, ternary, or quaternary systems of films are produced. Thus, by first obtaining a plane film within the conical vessel, and then inserting upon it four small bubbles, a system is produced consisting of four hollow films, of curious curvature, united to one another respectively by vertical plane surfaces, and connected in their centres by a common plane film, which, by adopting certain precautions, may, with a little care, be made to assume either a horizontal or a vertical position. This system may aptly be compared to that produced within the wire cube, and previously described. The analogy of the two systems may be further carried out by inserting one or more small bubbles in the centre of the system, which will, according to the form of the cone employed, assume a form approaching to that of a cube, and therefore analogous to the system shown in figs. 4 and 5 of Plate xxxiv., or to that of a pyramid (perfect or truncated), and therefore corresponding to the systems in 10 and 11 of the same plate.

The paper contains also some experiments on the motions of films when brought in contact with surfaces of glass under certain conditions. Thus, if a film be formed on the mouth of a wine-glass by dipping it in the solution, and be then covered over by a watch-glass of more or less convexity, so long as no contact is established between the film and the watch-glass no change will take place. But let the wine-glass be inverted so as to bring the film into a vertical position (the watch-glass being meantime kept firmly in position by the thumb), the film will now attach itself to the watch-glass at its lowest margin, and will run up its inner surface, describing thereon a series of curious curves. The upper part of the film will at the same time retreat down the inner surface of the wine-glass, and form a hollow segment of a sphere.

The cause of the phenomenon is the existence of a drop of the solution which enters the wine-glass in the operation of dipping, and remains at its bottom, and which, when the glass is inverted, runs down as far as its margin, and there comes in contact with the margin of the film. A connection is thus produced between the film and the watch-glass, which originates at a single point, but quickly extends in the manner mentioned. A variety of further experiments may be made on the film thus produced, several of which are described in the paper.

The last part of the paper consists of an account of some miscellaneous experiments chiefly on the tenacity of films when brought in contact with one another, or with surfaces of fluid, and then drawn apart, and the forms of the curves which in such circumstances they assume.

The paper is illustrated by a number of coloured sketches, done for the author by a lady residing near him at Melrose, who also prepared those for his previous papers, from which Plates xxxiii., xxxiv., xxxv., and xxxvi. were lithographed, and for whose kindness and trouble the author desired that his obligation should be mentioned.

The following Gentlemen were elected Fellows of the Society:—

John Dick Peddie, Esq.
Colonel Seton Guthrie.
Samuel Raleigh, Esq.
Thos. Smith Maccall, M.D., F.R.C.P.E.

Monday, 20th April 1868.

Professor PLAYFAIR, C.B., Vice-President, in the Chair.

The following Communications were read:—

1. Notice of Works designed by Sir Charles Hartley, C.E. for the Improvement of the Danube. By David Stevenson, Civil Engineer.

Having received from Sir Charles Hartley an interesting record of the works executed for the improvement of the Danube, to be presented to one of the Public Libraries in Edinburgh, I think its fittest destination is the Royal Society; and, on behalf of Sir Charles Hartley, I have to present the volumes on the table for the Society's acceptance.

It may not be uninteresting, in presenting these books, to describe, in a very few sentences, the work that has been executed for the improvement of the Danube, and the results that have been obtained.

In 1856 the "European Commission of the Danube" was appointed under the Treaty of Paris, and consisted of seven delegates, representing England, Austria, France, Prussia, Russia, Sardinia, and Turkey, and its object was to improve the bar of the river, and open the navigation to the traffic of all nations. The Danube, after flowing over a course of 1700 miles, and draining 300,000 square miles of country, enters the Black Sea by three separate mouths—the northern called the Kilia, the central the Sulina, and the southern the St George's mouth. The first duty of the Commission, with the advice of Sir Charles Hartley, who was appointed their engineer, was to select one of the three mouths for improvement, which was by no means an easy task, as each of them presented advantages peculiar to itself, and after much consideration the Sulina or central channel was selected, and although considerable difference of opinion existed as to the propriety of the choice the result has shown that the course adopted was judicious.

What gives professional interest to works on the Danube is their object—the improvement of a bar harbour, in all cases a troublesome engineering problem, due probably, in some measure at least, to the varying circumstances which the engineer has to consider in dealing with the formation and improvement of bars. In this country, for example, we find bar harbours at the Tyne, the Wear, and other similar localities, caused by the storms on our coasts, which, by the action of the waves on the bottom, throw up sand and shingle, and but for the scour produced by other agents a continuous line of beach would soon be formed across the mouths of our tidal rivers and inlets. But this action of the sea is counteracted by the scouring flow of the tidal and fresh-water currents, which, in spite of the waves, tend to keep open a deep channel through the beach. In this way the antagonistic action of the waves of the sea on the one hand, and the currents of the estuary or river on the other, produce the well-known feature of a submerged beach or sand-bank extending across our inlets, and having a channel through them of more or less depth, according to circumstances, which channel is termed "the bar." The conditions under which such accumulations are formed are as I have elsewhere stated :—*

1*st*, A bottom composed of materials easily moved;

2*d*, Water of depth so limited as to admit of the waves acting on the bottom ; and

3*d*, Such exposure as shall allow of waves being generated of sufficient size to operate on the submerged materials.

But in such places as the Firth of Forth, for example, before the

* Encyclopædia Britannica. Art. *Inland Navigation*.

first two of these necessary conditions occur, viz., sufficient shallow water and presence of sand, the sea, owing to the configuration of the coast, is so landlocked that waves of sufficient size to produce the third condition cannot be generated, and hence in the Forth there is the phenomenon of a barless river.

At the Danube, a large river, discharging in ordinary flood no less than twenty millions of cubic feet of water per minute, enters a tideless sea, and we have a totally different class of phenomena to deal with. The river brings down an amount of detritus which has been ascertained by Sir Charles Hartley to be equal to 27 cubic inches per cubic yard, and to be equal to no less, in cases of high flood, than 600,000 cubic yards of solid deposit in 24 hours. Like the Mississippi and the Nile, the Danube owes its extensive delta to the gradual accretion of this sedimentary deposit, and the bar at its mouth is due to the same action. It therefore differs entirely from the bars in this country, as is well exemplified in the fact that, whereas in our harbours the bars are always *deepest* when the sea is calm and the rivers are *in flood*, and therefore most efficient as scouring agents, at the Danube the bar is, on the contrary, invariably *shallowest* when the river is in flood, because it is then charged with a larger amount of deposit.

Another feature of difference in the treatment of such a case as the Danube is to be found in the circumstance that there is no reversal of the current due to tidal influence, and therefore it is unnecessary, in fixing the direction of the piers, or indeed in designing any of the works, to provide for the admission of tidal water to act as a scour on its return to the ocean, a provision which always demands special attention in designing tidal works on our coasts.

The works executed at the Sulina mouth consist of a north pier 4640 feet in length, and a south pier 3000 feet in length, both built of *pierres perdues* surmounted by a timber staging, with an entrance between of 600 feet, and the slightness of their structure indicates the modified character of the waves to which they are exposed.

The works, which are understood to have cost about £100,000, are highly creditable to the talent and energy of Sir Charles Hartley, and have now been completed for five years, and their effect has been most satisfactory, as proved by the fact, that previous to their construction the depth on the bar never exceeded 11 feet and frequently fell to 8 feet; whereas, according to the last accounts from Sir Charles Hartley, the depth for the last five years has never been less than 15 feet, and has often been as much as 17½ feet.

It is obvious, however, that as the Danube must continue to bring down an enormous mass of detritus, so, in course of time the works which have proved so successful must be extended—an event which has been fully anticipated by its projectors, and in this respect we find an interesting difference between such works as the Danube piers and the harbour works of this country, for here the object being to prevent the waves from acting on the bottom, the engineer extends his works out into a depth of water where there is little or no disturbance of the bottom, and if this is once secured he may calculate on the increased depth of water remaining permanent, whereas, at the Danube the piers must be projected to keep pace with the gradually increasing delta at the river's mouth.

2. Remarks on two Pyramid Papers in the last published Number of the "Proceedings" of the Royal Society of Edinburgh. By Prof. Piazzi Smyth, April 20, 1868.

On the 10th of March last I had the honour of receiving, in the usual course of publication, a copy of the 75th Number of our Society's *Proceedings*.

Within that Number's few pages, there are no less than two distinct essays, mainly, if not entirely, on "*the Great Pyramid,* "*and my views respecting it.*"

Both these essays, though by able and ingenious men, are yet unfortunately based to such an extent on needlessly imperfect data, that the superintending Secretary might have justifiably returned the MSS. to their respective eminent authors, with suggestions for improvement in the improvable particulars, before publishing them under the auspices, and at the expense of the Society.

This course, however, was not followed; and the words of friendly advice, which might before have been offered in private, require now to be stated in public; for not only have the printed copies been distributed amongst the members, but one copy having reached the "Medical Times" of London, was commented on thus definitively in its impression of March 28, 1868:—

"SIR J. Y. SIMPSON ON THE PYRAMIDS.

"It is characteristic of the brilliant and diversified gifts of the "great Northern Professor, that he should have taken in hand the "demolition of a curious notion about the Great Pyramid of Egypt, "which has been promulgated by Professor Piazzi Smyth, and has "found favour with many religious persons. That notion is, that "the Pyramid was constructed under Divine guidance, to show to "all time a correct standard of weight and measure, and that the "coffer contained in the central chamber is an inspired measure of "capacity, and the Base of the Pyramid an inspired measure of "length, having a definite relation to the earth's polar axis. Sir "James Simpson shows, *in a paper read before the Royal Society* "*of Edinburgh*, that the Pyramid has all the characters of the "huge sepulchral monuments scattered over the earth; that the "coffer in the King's Chamber was a sarcophagus; that it contained "a body till despoiled by the Caliph El Mamoon about a thousand "years ago; that it is irregular in form, incapable of being exactly "measured, and hence no standard of measure. In fact, any one "who desires a treat, in seeing how a thorough '*craze*' is melted "away before common sense, should read Professor Simpson's "paper. The kind of argument of Professor Piazzi Smyth, to "show that the Pyramid was built by Noah, is one which we "thought peculiar to the medical profession:—Noah was a preacher "of righteousness; a just system of weights and measures is "righteous: *ergo*, Noah built the Pyramid!"

M. WACKERBARTH'S PAPER.

The first of the two papers printed by the Society appears, indeed, only in the shape of an abstract of the original essay; was composed by a native of Sweden, and is, in truth, such a *naive* production, in the total want of knowledge which it displays of the progress of all Pyramid research during the last three or four years;

—that I have thought it enough to send the author a letter, nearly to the following effect:—

"Dear Sir,—I have read to-day your communication *on the* "*Great Pyramid of Jeezeh, and my Views concerning it*, in the 75th "Number of the Proceedings of the Royal Society of Edinburgh "(pp. 235–238).

"So much ability appears in the Memoir, that I regret you had "not something better to work upon than those few pages of mine, "written four years ago, especially as I have since then visited the "Great Pyramid, examined it steadily during four months, and "published the results last year in full.

"Your time would surely be more usefully spent in criticising "what I now maintain, and have myself measured at the Great "Pyramid, than in running down details which I have long since "discarded, because proved to be without sufficient foundation.

"Some important leading points, however, both in theory and "induction, which I still hold implicitly, and always have held, "with regard to the Great Pyramid, since I first published any-"thing upon it—you seem to have entirely failed to apprehend. "As, for instance, the very broad and striking fact, that none of "the standards deduced from the Great Pyramid's earth-com-"mensurabilities, are similar to those of the idolatrous Egypt of "history. Even the stars of reference in the sky, are absolutely "and totally different. The very essence, in fact, of the Great "Pyramid theory, as to its earth-founded weights and measures "is, that they are actively opposed to those of profane Egypt, and "could not have been invented by, nor were they ever known to, "any of her idolatrous sons.

"Therefore you do not bring up anything new, either to the "theory or myself, when you discover and publish that some *one* "in particular of the Great Pyramid standards was not in general "use in ancient Egypt. Nor again do you really injure my next "argument about the high antiquity of the British *inch*, by show-"ing, as I also had done, that our *yard* is of comparatively recent "origin.

"If your special desire is to upset the metrical theory of the "Great Pyramid, prove, if you can, that I am largely wrong in "those measures of length and angle throughout the building, on "which the theory is mainly founded,—and you may succeed in "your design; especially, too, if you can explain all the *well-* "measured and already explained facts still more accurately, on "some other single, consistent, and connected theory.

"But if, in place of doing any of these things, you merely pro-"duce more specimens of the *isolated* and perverse examples which "appear at the close of your paper, wherein you go off to new and "separate subjects on every occasion, and by much trying, succeed "at last in catching a single coincidence of a purposely ridiculous "character each time,—do not flatter yourself that you are thereby "reducing to an absurdity, on mathematical principles, the whole "Great Pyramid theory; whose strength consists—not only in the "large number and mutually connected character, of the series of "close coincidences with a united and consilient purpose, which it "proves between that primeval building and nature,—but also, in "the universal earth-importance of the things and system there "symbolized: and I remain, &c. &c. &c."

The gentleman addressed in the preceding letter, has duly returned me an answer, wherein he acknowledges his want of acquaintance with recent Pyramid explorations, so honestly, that I have had nothing else left me to do, than to ask his acceptance of a copy of my "Life and Work at the Great Pyramid;" and the volumes went off to him accordingly, last week.

SIR J. Y. SIMPSON'S PAPER.

So far then for the first of these two essays, in its shortness and simplicity.

Very far otherwise, however, must be the second essay, when it extends, though professedly only an abstract, to no less than twenty-six pages; is written by a home-member and well instructed, of this Society; and is entitled at page 243,—

"*Pyramidal Structures in Egypt and elsewhere, and the objects* "*of their erection.*"

That is the printed title; yet after only two pages on such proposed extensive subject, all the other twenty-four pages are filled up with hardly anything else than severest animadversion upon, merely the one Great Pyramid of Egypt and myself.

I trust that I shall not be accused of conceit for saying in such company *myself*, seeing that in the course of those twenty-four pages my name is printed no less than 85 times; and there are not fewer than 70 cases of extracts in inverted commas, from some one or another of my several Pyramid writings during four years past, besides passages, ideas, facts and numbers more numerous still, similarly derived, but without the commas!

Such a masked battery against one individual, was probably never before opened in the pages of a journal intended for the advancement of science alone; and yet, if its fire had been directed against actual and positive errors, not only might the previous masking of the battery under another title have been condoned, but great praise and extensive thanks would have been due to its engineer—so all-important in the present day of earnest prosecutions of science is it, that the pathways of knowledge should be cleared of every fault, slip, or mistake of each of its previous cultivators. After having, however, carefully read the twenty-four accusatory pages, I am sorry to say, that the general effect on my mind, is not admiration at the clear proofs of error afforded, so much as astonishment at the ingenious and intricate manner in which an *opposite* meaning is given to the majority of those very numerous extracts just alluded to, than what I had intended them to convey.

This fact *may* imply, that my style of writing was imperfect; and perhaps it was; seeing that, like most scientific authors, I wrote for those only who were anxious to understand me. Sad loss of time too would it be, and needless extension of treatises, to attempt anything further; for we see elsewhere, that after the best lawyers of the land have striven to make the wording of some Act of Parliament as exact and stringent as possible, common report tells that other clever men, but of rather perverse inclinations, very soon contrive to find out how to drive a "coach and four horses" through that very, and the self-same, expensive piece of parchment. And if those clever men are further allowed to cut up the one Act of Parliament they are attacking, into quotations of any number of words, from one, to two, three and upwards, and distribute them confusingly among similar shreds of certain abrogated Acts, and other still more extraneous matter also,—you may easily imagine what an erroneous view of the existing law of the land could easily be prepared; but also how totally unusual such a method would be considered among scientific men in the cause of science.

Yet something closely approaching the above has been enacted in the second *Proceedings*' treatise; where little extracts of my last and completest book, are mixed up, generally without distinction or reference, with portions of my earliest, crudest, and now superseded books and papers, on the same subject; and with bits of the late Mr Taylor's book, which bits I never accepted at all.

To prove each and every one of the almost innumerable cases of altered meaning thus produced, would require more time than either the Society is likely to grant, or I have to spare; but I will attempt to separate and exhibit a few, as samples of the rest; and also, point out whatever of new and important matter for our knowledge of the Great Pyramid, the essay appears in my judgment to possess.

LITERARY POINTS.

(1.)

Among more purely literary matters, I would mention first, and with much commendation, the author's reminder at p. 260, of both *Cassini* (in a book published in Amsterdam in 1723), and *Callet* (in Paris in 1795), having proposed "that the Polar axis of the earth should be taken as the standard of measure."

I was not previously aware of their having so done; and have not even yet been able to obtain a sight of Cassini's book.* But a friend in London, who has kindly searched for it at the British Museum, informs me that the statement is not quite exact,—for Cassini does not allude to the *Polar* axis or semi-axis for the purpose, but merely to a general semi-*diameter* of the earth; and he appears to have shared in the scientific uncertainties of most or all men of the period, as to whether the earth is flattened or elongated at the Poles.

The case is interesting, therefore, as setting limits to the date at which *modern* science *could* have begun to single out the Polar axis as a unique length in the earth and appropriate to form a

* After having failed in procuring the book at the chief public libraries in Edinburgh, my venerable and learned friend Dr Daun has found it amongst his collection of valuable French mathematical works; and having kindly lent it to me, I can verify the account which follows.

reference for linear measure. But it does not touch the completely independent question in the latter-day theory of the ancient Great Pyramid, as to whether such a standard was known to the primeval designer of that monument 4000 years ago.

All the parties mentioned do, indeed, propose to divide *a* quantity obtainable from the earth, into ten million parts; but the rod or cubit developed in that manner was not still further proposed to be subdivided by either Cassini or Callet, in the very peculiar manner now attributed to the architect of the Great Pyramid: and his method, if it can be proved, must have had a character, origin, and priority entirely its own.

(2.)

Next, Sir, I have to confess, that when speaking of that ten-millionth part of the earth's Polar radius, as realised for its length only, by a cubit rod of 25 inches $\pm \cdot 1$,—when speaking of it in that sense and in some stray sentence, in some one or another of my books, I know not which, nor by what accident the error occurred, —the word *extraordinary* was printed instead of *extraordinarily!*

At least so I gather, because the author of the paper, at his p. 260, drags up the word *extraordinary*, prints it conspicuously in *italics*, puts a *sic* in a parenthesis after it, and publishes that denunciation of his, of what he would indicate as the abominable vulgarity in me, viz., of saying that something was "*extraordinary* convenient;" and he publishes the shocking phrase as a necessary part of his treatise on a scientific subject.

Mr Chairman, and gentlemen,—I acknowledge before you all, that it is a *grievous* fault in polite circles, to use *extraordinary* instead of *extraordinarily*, when the latter is the right word to be used; and my thanks are due to that acute individual who has, it would seem, both detected the error, and made a scare-crow of it before the public. An error light as air perhaps, but admirably adapted on that very account to act as a *wind-vane;* and thereby, even to react on the acute individual, so as to show the world whether the current of his thoughts was setting purely towards the promotion of science for its own sake, when he was writing the remarkable essay now printed in the Society's *Proceedings.*

As to myself, I should, of course, like amazingly to correct this sad error in my books, wherever it exists; but unhappily, no reference being given, I have not been able to find it. I have, indeed, come upon a passage in the 3rd volume (p. 135) of my last book, "Life and Work at the Great Pyramid," identical almost word for word with the passage accused, except in this, that it has not the vulgarism *extraordinary*, but the right word in that place, or *extraordinarily.*

Can the Council assist me in finding where the real sin may lie?

Here is the accusation in their printed *Proceedings*, p. 260.

"An *extraordinary* (sic) convenient length, too, for man to handle and use in the common affairs of life, is the one ten-millionth of the earth's semi-axis of rotation when it comes to be realised, for it is extremely close to the ordinary human arm, or to the ordinary human pace, in walking with a purpose to measure."

And here is the passage in my volume iii. p. 135 of "Life and Work at the Great Pyramid," which seems to be the one referred to, but does not contain the evil thing attributed to it:—

"An extraordinarily convenient length too, for man to handle and use in the common affairs of life, is the one ten-millionth of the earth's semi-axis of rotation, when it comes to be realized (as a linear standard); for it is extremely close, *either to the length of* the ordinary human arm, or to the ordinary human pace, in walking with a purpose to measure."

For the more complete understanding of my version of the original sentence—taken away, as it is here, from the other sentences of its paragraph in its own book—I have inserted, in parenthesis, the words "as a linear standard" after "realized:" because a main purport of the whole is to show, that the modern French metre, being nearly 39 inches in length, is too long; and the ancient Egyptian cubit, only 20·7 inches in length, too short; but the Great Pyramid cubit, being 25 inches long, is most appropriate and handy to use as a measuring rod. And I have called attention above by *italics* to the words "either to the length of," which are found in my book, and assist the above intended meaning, but are omitted in the condemnatory *Proceedings*' edition.

(3.)

Lastly, I would remark under the literary heading, that, in the general description given by the *Proceedings*' author of the present exterior of the Great Pyramid, I am specially quoted for the words "now it is so injured as to be, in the eyes of some passing travellers "little better than a heap of stones." And the essay then proceeds, in the spirit of that statement, utterly to confound the true figure and peculiar structure of the Great Pyramid, with sundry mere rounded heaps of loose or soft material, sometimes having rude chambers inside them, and sometimes not, in various parts of the world.

It may be quite true, that in one portion of my largest book, where I was anxious to let every side of the question appear in order, the above few quoted words are to be found. But are there not also pages, and pages, and pages giving much broader records of the other side of the question as well; giving my own experience, for instance, through months of patient observation, on the mechanical excellence and conscientious performance of the masonry courses, both composing the Great Pyramid's present faces and general body, and contributing towards one of its most important symbolizations?

How many times, too, have I not had exhibited before large public meetings, both in Edinburgh and various cities of Scotland, amongst other of my own photographs of the Great Pyramid, several which were specially adapted to display that astonishing regularity which extends, with a purpose, throughout each of the gigantic courses of squared, cemented, horizontal, hard masonry? And yet all this accumulated evidence of various kinds, in favour of an ancient excellence, is to be annihilated,—for what? For half a dozen words attributed to, merely "some passing travellers;" and the accounts of passing travellers at the Great Pyramid, are again and again shown, in the same book, to be almost always untrustworthy and liable to gross mistake.

Hence, it is quite clear that the damaging extract to the Great Pyramid, picked out of my book and printed as above, does not exhibit the general sense and total conclusion of my whole book on the particular point concerned, but rather the very opposite. And the superabundant literary acumen which induced a gentleman to make so much use of the word *extraordinary* instead of *extraordinarily* in a minor and rather problematical case, has failed him totally on a grander occasion.

Nor do I see any prospect of that gentleman ever understanding the mode of construction of that noblest work of the world 4000 years ago, viz., the Great Pyramid; and how vastly it differs from all the moats, tumuli, howes and barrows which he will insist on comparing it to, and confusing it with,* until he has himself both ascended and descended the ancient monument, climbing up each of the steep steps of its colossal masonry courses (without any assistance from Arabs or other attendants), and measuring every such step carefully, from bottom to top of the pyramid, and top to bottom; and comparing such collections of measures, made on every face, and every arris line of the structure.

A long and painful process, and perhaps a little dangerous;—but one whose teaching cannot be ignored by any unwilling scholar, so easily as the contents of a printed book.

* As to the total diversity in quality of construction between the Great Pyramid of Egypt, and the little more than mounds of mud and pebble stones forming the barrows of Ireland, frequent testimonies are borne in the volumes of Colonel Howard Vyse—volumes known to the *Proceedings'* author, but seldom quoted by him, when they tend to elevate one's conceptions of the Great Pyramid. As an example of the very important character of some of these omitted witnesses to exceeding perfection of work in one of the earliest, if not the very earliest, of stone buildings now existing, or ever existing, upon the earth; and which has stood there through all the human historic period,—an august witness of what took place under the sun in primeval ages of which we have no other contemporary record, I insert the following from pp. 261 and 262 of the first volume of Colonel Howard Vyse's "Pyramids of Gizeh." The Colonel is speaking of the two casing stones which he discovered by excavating down, through the rubbish accumulated in modern times, to the middle of the north side of the base of the Great Pyramid; and says of them—

"They were quite perfect, had been hewn into the required angle before "they were built in, and had then been polished down to one uniform sur-"face; the joints were scarcely perceptible, and not wider than the thickness "of silver paper; and such is the tenacity of the cement with which they "are held together, that a fragment of one, that has been destroyed, "remained firmly fixed in its original alignement, notwithstanding the lapse "of time and the violence to which it had been exposed. The pavement "beyond the line of the building was well laid, and beautifully finished; but "beneath the edifice it was worked with even greater exactness, and to the "most perfect level, in order, probably, to obtain a lasting foundation for the "magnificent structure to be built upon it. I consider that the workman-"ship displayed in the King's Chamber, in this pavement, and in the casing "stones, is perfectly unrivalled; and there is no reason to doubt that the "whole exterior of this vast structure was covered with the same excellent "masonry."

NUMERICAL POINTS.

The Coffer.

What has now been mentioned, is a prevailing characteristic of the literary parts of this second *Proceedings'* paper; as readers, who can spare the time, will find on tracing up the quotations to the original contexts in my books. More quickly provable matters, however, are alluded to under the headings dealing more particularly with *mensuration* affairs. Therein, too, although the author begins by saying (p. 247), that he approves of the Society having conferred the honour of their Keith Medal upon me, for my measures at the Great Pyramid,—yet within a couple of pages after that, he attacks those measures bitterly, especially as touching the *coffer* in the King's Chamber; and he would make them out to be disgraceful, rather than creditable. For whereas I had cited, as he implies, before going to Egypt, twenty-five different observers,—whose measures of the coffer differed irreconcilably, and by monstrous differences,—yet when I, a twenty-sixth observer, measured it, my observations also differed from every other person's, were apparently no better than any of them, and made a previous confusion only worse confounded.

Now my measures did indeed differ from those of every preceding traveller, in this,—that they were perhaps fifty times more numerous. And, by multiplying them over every part of the coffer,—I was enabled both to show what were the limits of variation in the structure itself;—and also, to prove that all those older recorded measures which differed from certain mean quantities by a whole inch, or even half-an-inch (and some of them varied from the same in any number of inches from two, four, and upwards to so many as forty), were sheer mistakes on the part of the observers: some of whom, it should be stated in apology for them, lived far back in the 16th century, and had guessed the lengths rather than measured them; not having any idea of the importance of extreme accuracy, touching what appeared to them generally,—merely as a burial sarcophagus or coffin.

Irregularities of the Coffer.

This important limitation in old asserted differences of the chief dimensions being thus accomplished,—I next entered into an examination of certain *residual* features, of the coffer.

1. The *first* of these consisted in modern breakages, lamentably large, but sufficiently simple to inquire into.

(They are shown in this model which is $\frac{1}{10}$ the linear size of the real coffer, also in the frontispiece to vol. i. of my "Life and "Work at the Great Pyramid.")

2. The *second* class was caused by traces of a *ledge* cut inside the top of three sides, and all across the fourth side of the coffer.

(In this 2nd model, the breakages being restored, the ledge is exhibited more clearly; they are also shown in the frontispiece plate of vol. i. of above work.)

Now this ledge has a little history connected with it.

Trusting to Professor Greaves, the great French work on Egypt, and other authorities, I had unfortunately described the coffer as without any ledge, *before* I went to Egypt. But on afterwards seeing the coffer there, I found that it *had* traces of a ledge. On my return to this country, I discovered, in a book of thirty years ago,* a notice, short and imperfect, but still a notice, to the same ultimate effect.

On finding these things, I both got that book added to the College Library,—called much attention to it in my own Pyramid book of last year; and published my own observations of the ledge with numerous particulars, both in description and in measure, down to hundredths of inches, in a manner, and with a completeness never before attempted by any one, so far as I am aware.

After all this has been before the public for nearly a year, the *Proceedings'* author republishes at page 252,—and as if elsewhere ascertained—some of my own particulars; and adds to them so much of the defective notice in the old book just mentioned, as to extinguish a very radical difference really existing between the ledge of the coffer in the Great Pyramid, and the ledges on the sarcophagi in all the other Pyramids of Jeezeh. And which difference in form produces this variation in effect.

* An enormous folio book or portfolio, usually termed *Perring's Plates of the Pyramids*, and containing many excellent lithographs of them from his drawings; but the book was got up, and its letterpress edited by Colonel Howard Vyse, and includes contributions from Dr Birch, Mr Lane, and Mr Andrews as well.

When you put on the cover of one of these smaller pyramid sarcophagi, the combination of *acute-angled* grooves and falling pins fixes and locks the lid, in a manner most suitable to the safe preservation of the contents of a burial sarcophagus or coffin.

But when you put on, to a true model of the restored *coffer* of the Great Pyramid, any sort of sarcophagus lid prepared suitably to its *rectangular* grooves,—such lid has no fixing power, and can be lifted up with the utmost ease; or in a manner very unsuitable to the usual duty of a pure coffin-lid.

Moreover, such a lid seems most untoward to the *coffer* in its place in the King's Chamber; because, while the doorway is of such a height as only *just* to allow the coffer to pass through as a lidless box, or in the state in which every known historian has invariably described it,—put on a lid, prepared in modern times according to the proportions of the lids of sarcophagi of that period, and you can neither get the vessel into, or out of, the room; by an amount too of 5 or 6 inches of solid granite, for that is the space by which the doorway, more than 100 inches thick, is then too low to admit the lidded or sarcophagised coffer.

3. A *third* subject of my minute examinations of the coffer, consisted in certain small residual but original errors or defalcations in its figure, from a pure geometric form; and these are generally so slight, or are effected by curves of such long radius, as to have escaped all my predecessors.

Yet some gentlemen at home, are not always easy to please. And when I supplemented in my printed book some of these additional numbers of mine, by a further verbal warning,—intended for those who are not yet much experienced in the coffer's peculiarities,—the Argus-eyed *Proceedings*' author immediately quotes me, at his p. 251, for the warning, or that, if the sides of the coffer were calipered lower down than the usual place of measuring, they might present a notably different thickness to those who measure to two places of decimals of an inch,—and then adds his own deep insinuation of blame against me, "though it does not appear "why they were not thus calipered."

Pray allow me, Mr Chairman, cheerfully to explain to you why, and how, they both were, and were not, calipered.

They were not *actually* so calipered, because I could not obtain when the idea occurred to me out there, the unusual size of calipers that would have been required, by applying to the scanty resources for civilized life existing in the Libyan desert. But the coffer sides were *virtually* so calipered, by the measures which I took both inside and outside, with such apparatus as I had with me; and any one who is desirous of ascertaining, within small limits, what the sides of the coffer would *actually* caliper at the place indicated, has only to project my printed observations, and see.

CUBIC CONTENTS OF THE COFFER.

My own chief object, however, was not so much to get the thickness of the coffer's sides at one special point, as to ascertain the whole cubical contents of the original vessel.

Now those small *residual* features just mentioned, and which I, by myself, have been chiefly instrumental in bringing to light, having been used by the *Proceedings'* author out of my own pages, for the purpose for calumniating the ancient coffer, and implying (p. 252) that its cavity is "of a form utterly unmeasurable in a correct way "by mere lineal measurement," and pronouncing it to be (p. 254), "in simple truth, nothing more and nothing less than—an old and "somewhat misshapen stone coffin,"—a duty devolves on me to show, both within what limits it can be measured in that lineal way; and also, what remarkable purposes some of those apparent residual errors of figure, but really important adjustments of cubical size, do subserve.

We take the vessel therefore, restored from modern breakages; fill up the ledge in the top of the sides, and then ascertain the cubical contents of the hollow, by multiplying together the mean of all the observed internal lengths, breadths, and depths. From hence we obtain 71,317 cubic Pyramid inches;* a quantity which is many thousands of inches different from the contents of any known sarcophagus of the burial kind at the Pyramids.

When future observers shall have visited the Great Pyramid, and measured the coffer with still more care than I have done,—their results will afford a desirable test as to my limits of error. But meanwhile the coffer itself may testify something, by having (when

* Mean length = 77·85; mean breadth = 26·70; and mean height = 34·31 Pyramid inches. See "Life and Work," vol. iii. p. 154.

its ledge is filled up, and only then) a certain inter-commensurability of parts; and which is not known to prevail in any Egyptian sarcophagus, or to be now, or ever to have been, a necessary principle amongst makers of mere coffins anywhere.

The first of these commensurabilities is,—that within the limits of errors of measure, the inside contents of the vessel are just half those of the outside (as per model shown at the meeting).

And the second is, that the cubical contents of the bottom are similarly half those of the sides (as per model).

But these distinct proportions would not have obtained within many hundreds of cubic inches, if the recently detected hollowing of some of the sides of the coffer had not been performed of old. So much pains do indeed seem to have been taken in that matter at the primeval coffer, that we may be justified in adding beneath our first quantity in the numerical table, half the observed contents of the outside of the coffer, = 71,160;* and the sum of the walls and floor, obtained chiefly by separate measures, and = 71,266, on a principle discovered by Mr Henry Perigal.

These results are beginning to look close: and although the *Proceedings'* author would vitiate them utterly, by insinuating that I have measured them from what he stigmatises as an "imaginary "higher brim" (p. 252),—yet I point on this plate of my published book, "Life and Work" (vol. i. frontispiece), and also on this model, to the still remaining traces of the actual and ancient top

* Table of Coffer—

Measures above referred to, all expressed in Pyramid inches.

Number of Method.	Nature of Method.	Numerical materials employed.	Cubic Contents of Interior of Coffer.
1	By direct measures of interior,	77·85 × 26·70 × 34·31 =	71,317·
2	By half the exterior measures,	$\frac{89·62 \times 38·61 \times 41·13}{2}$ =	71,160·
3	By contents of bottom and sides,	89·62×38·61×6·866 = 23,758· 2 (89·62 + 26·70)×34·31×5·952 = 47,508·	71,266·
4	By new depth derived from ledge frame,	77·85 × 26·70 × 34·282 =	71,258·
		Mean =	71,250·

of the sides: from which my measures were scrupulously taken. Indeed I repel that word *imaginary*, and fling it back to its author; and declare that those traces of the original top of the sides were to the best of my testing and examination as unmistakably ancient, finished, and precise as any part of the coffer whatever.

I have also been led since then to perceive, that the frame adapted to fill up the ledge, seems to be an independent confirmation of the sides' original height. It is so, at least, within less than three hundredths of an inch; * and that difference being introduced into our first computation, gives us a fourth approach to the true cubical contents of the coffer = 71,258.

These four being combined, give 71,250 cubic Pyramid inches, as the best result of all my measurings, true probably, for the coffer itself, to within twenty cubic inches. And I venture to say this, notwithstanding that the author in the *Proceedings* attacks me violently by means of some of the old and exploded twenty-five observers of former times, setting forth (at p. 250) that one of their results differs by 6000 cubic inches, and another by no less than 14,000 cubic inches, from mine.

But Sir, is that to my, or to their, confusion?

If you will examine the particulars, you will find that the 6000 inches of difference, arose from that party having made an absolute error of 3 whole inches in the depth of the coffer.† While the 14,000 inches case rests on one Dr Whitman, who himself never went inside the Great Pyramid, but depended on a friend.

If the author in your *Proceedings* will think that Dr Whitman could understand the coffer better without going near it,

* For full numerical particulars want of space here compels me to refer to a work which I have now almost ready for publication, and entitled, "On the Antiquity of Intellectual Man." *See* its chapter 29, p. 300.

† The faulty measure was that of the French Academicians in 1799, and when reduced from the metre to British inches, = 37·285.

Professor Greaves, in 1638, had previously stated the depth = 34·320 British inches; and Colonel Howard Vyse had subsequently, or in 1837, made it = 34·5 British inches. But still many persons thought, "surely " the French Academicians could not have made so great a mistake in their " measurings; it must be the fault of the coffer, whose depth is different in " different parts of its length or breadth?"

The following measures, however, taken by myself in 1865, will show clearly that though the depth may vary over different parts of the bottom by or seeing it, than I could by spending many days over it,—who can prevent him? I certainly cannot, and do not intend to try. I have published the particulars of my modes of measurement, the nature of the apparatus employed, the method of comparing with a standard, the circumstances under which the observations were made, and the actual numbers procured from the measurings and remeasurings arranged so as to test each other on various parts of the coffer; and if all these particulars fail to impress the gentleman's mind, as of some weight or importance in a case where next to nothing can be said on the opposite side, I can offer no more to him either in explanation or defence of myself.

But before the world, I must still continue to maintain, that so far as both care and number are concerned, no coffer measures have yet appeared that can compare, on the usual principles for judging measures amongst scientific men, with mine; that mine, therefore, however much below ideal perfection, have a practical claim to represent the true size of the coffer, until better observations shall be taken; and that, if distressingly bad measures, or mere guesses at measures, have been published during times long past—their badness is the fault of the observers, not of the coffer.* Yet, to

hundredths, and occasionally even by tenths, of inches,—a whole inch is perfectly out of the question.

Extract from "Life and Work," vol. ii. p. 123.

"INSIDE DEPTH of *Coffer*, from original top of North, East, and South Sides. (By Slider 25, not requiring any correction.)

Part of Length where Observations were taken.	Part of Breadth where Observations were taken.			
	Near East. Side.	Near Middle.	Near West Side.	Mean at each part of Length.
British inches.	B. inches.	B. inches.	B. inches.	B. inches.
0·6 south of inner N. end.	34·30	34·28	34·26	34·28
3·0 " "	34·44	34·36	34·35	34·38
5·0 " "	34·42	34·41	34·28	34·37
10·0 " "	34·40	34·38	34·28	34·35
24·0 " "	34·36	34·38	34·26	34·33
Mean at each part of Breadth,	34·38	34·36	34·29	34·34
General Mean of all the Measures, or Mean *Depth* of Coffer, inside, . .	= 34·34 British inches."			

* For cause of error in Dr Whitman's numbers, see note on p. 338.

attribute all the fault, and prove it heinous, to the coffer, whose makers and friends have been dead for 4000 years, would seem to be the very object of the *Proceedings'* author; in order that he may then take away the primeval vessel's power to symbolize a standard of measure. For says he at p. 249, "Surely a measure of capacity should be measurable; "that ought to be its most unquestionable "quality; but this imagined standard has proved virtually unmeasur- "able,—in so far at least as its twenty-six different and skilled "measurers all differ from each other in respect to its dimensions."

And as the same author repeats, at his page 252, that "the coffer's "cavity seems really of a form utterly unmeasurable in a correct "way by mere lineal measurement,"—and indicates that though "perhaps liquid measurements would be more successful,"* yet the object is not "of the slightest moment;"—it must now appear plainly, that the grand purport of that gentleman's essay is not to ascertain more correctly than I have done, or attempted to do, what is the real capacity of the interior of the coffer, nor to furnish the world with correcter numbers than mine,—but merely to damage the ancient vessel's character, as to being considered capable of showing any particular capacity, even within 14,000 cubic inches.

He may argue that this is a mode of inquiring legitimately into whether the Pyramid can be possibly regarded as a metrological monument, or a sepulchre (p. 247). But seeing that the style of workmanship of the whole king's chamber, as well as of the coffer that stands in it, has for ages excited the admiration of all nations, for the truthful character of its rectangular shapes, polished surfaces, and close fitting joints,—seeing, I say, that such good and simple mechanical work in a hard material is precisely what admits being well measured by competent men,—and that Professor Greave's length for the whole room 200 years ago, differed from my measure of the same, by less than $\frac{1}{10,000}$ part; why the *Proceedings'* author, if he *will* maintain the "skilled" and trustworthy measures of *all* the twenty-five observers,—would imply that the hollow granite block forming the coffer, and reported by those observers in different years, as of various sizes,—although it is resting quietly in a dark room of equal temperature,—must

* How such measure is to be applied to a vessel broken down at one corner to more than a third of its height, the *Proceedings'* author does not say.

actually, with time, alter its size:—alter it too so hugely, that the granite swells out sometimes to a length of 144 inches, sometimes sinks down to 78 inches only, while still preserving the same general rectangular box-like figure.

Whether other persons may be inclined to maintain that hard red granite *can* undergo such changes, I do not stop to inquire. I only vouch that I saw no inclinations to it, on comparing my measures, taken at the beginning of my stay at the Great Pyramid, or in January, with my last on leaving it in April. Moreover I firmly believe that any or all my twenty-five predecessors, if,—instead of, as with the generality of them, remaining in the coffer room only a few minutes on a single occasion,—if they had visited it day after day, for hours each time, and during several months of the year,—and if they had taken the same pains that I did, to compare their measuring scales accurately with the Government standard yard of 36 British inches,—they would have very nearly brought out as their result for the coffer's cubic contents (before breakage had been effected or a ledge had been cut into it), the same quantity which I have found, viz., 71,250 cubic pyramid inches;* and that equally, whether they look on the coffer as a burial sarcophagus only, or a a measure of capacity, or both combined.

If *I* consider the coffer chiefly as a measure of capacity,—though freely confessing that some subsidiary depositing of a mummied corpse in state, may once have been performed there,—it is because, after having found that the amount of cubic space, both in length, breadth, but more especially in depth, is much greater than would have been positively required for a mere coffin purpose,—considerably greater too, in depth, than in the manifestly burial sarcophagi of other pyramids,—it yet tallies exactly with an expression deducible,—on what has otherwise been called "Pyramid principles,"—from the size and weight of the whole earth, for appropriately representing a grand standard of capacity, and weight, measures. The very essence of the question turns, in fact, on whether there be a neat and close correspondence between the one practically measured, and this other theoretically computed, quantity.

What then has not been my surprise to find the *Proceedings'*

* A pyramid linear inch = 1·001 British inch.

author, positively giving as my theoretical reason for the coffer having been made in ancient days the size it now proves to be, not the noble, appropriate, and earth-reference reason which I have published,—but something totally opposite, chronologically foolish beyond expression, and which, so far as I know, never either entered my head or escaped my lips on any occasion whatever.

I subjoin the two statements, in order that the Council of the Society may judge for themselves :—

Proceedings' paper, p. 249.

"Professor Smyth holds that "*theoretically* its (the coffer's) "capacity ought to be 71,250 "'pyramidal' cubic inches, for that "cubic size would make it the exact "measure for a chaldron, or practi- "cally the vessel would then contain "exactly four quarters of wheat."

"*Life and Work*," vol. iii. p. 151.

"A cubic space is to be formed, "with sides having a length equal "to one ten-millionth of the earth's "axis of rotation, or 50 Pyramid "inches. A *tenth* part of such space, "or 12,500 cubic inches (agreeably "with the Coptic interpretation of "the name of Pyramid), is then to "be filled with matter of the mean- "density, or specific gravity of the "earth as a whole. In which case, "such a mass will form the grand "*weight standard* of the Pyramid; "while the *space* occupied by an "equal weight of pure water at a "given temperature, will form the "grand *capacity standard* of the "Great Pyramid; or, as we believe, "will represent, and be represented "by, the cubic contents of the "hollow of the coffer.

And further on it is shown, that the mean density of the earth is nearly 5·7 times that of water at a temperature of 68° Fahr. Whence the theoretical expression for the coffer's capacity is, $\frac{50^3 \times 5 \cdot 7}{10} = 71{,}250$, Pyramid cubic inches.

Arabian Authorities.

There is only one more circumstance which I have time to notice, connected with this *Proceedings'* author's account of the coffer, but *that* one is said to have procured him many believers.

It is, that the Arabian writer, Al Hokm, states that Khaliph Al Mamoon, when he broke into the Great Pyramid, in or near to 830 A.D., found in the coffer an embalmed human corpse; proving, therefore, as some will have it, that the coffer had been from the beginning *only a coffin.* Wherefore, let us inquire, who is this Al Hokm, who vouches, and is so readily accepted as a voucher, that Al Mamoon's chief finding was a dead body?

Mr St John Day showed two months ago, before the Philosophical Society of Glasgow, on the strength of the publications of Colonel Howard Vyse and Dr Sprenger, that Al Hokm lived 600 years after Al Mamoon; and could not, therefore, be any certain or *contemporary* authority for what that Khaliph found in a dark chamber in the Great Pyramid, six centuries before. Mr Day also showed, out of Vyse's book, that there is no known Arab writer who begins to attribute body-finding to Al Mamoon, within the first three centuries after his death; those who are earlier, and closer to that Khaliph's times, having completely different stories to tell; and telling them too in the most positive manner.

Yet the *Proceedings*' author will by no means give up his selected Mohammedan latter-day tales (or those which Mr Day had criticised in their newspaper edition of January 22, 1868); and after adducing, in a note, two other Arab writers, to a similar effect with Al Hokm,—but both of them living between 300 and 400 years after Al Mamoon, according to the same either Howard Vyse's or Dr Sprenger's authority,—he virtually clenches up these later Arab accounts on to, or makes them as good as if written in, Al Mamoon's own day, by stating, most remarkably, at the end of the same note,—that "*Colonel Vyse observes, that the Arabian " authors have given the same accounts of the pyramids, with little or " no variation, for above a thousand years.*"

Yet neither the words, nor the sentiments in the above juxtaposition, are Colonel Howard Vyse's. The words are out of his book, no doubt, but they are spoken by Dr Sprenger; and, what is much more important, he is applying them to authors of far earlier date than any of those who mentioned Al Mamoon.

That it was to these far earlier men, talking of perfectly different matters, that Dr Sprenger was alluding,—would have appeared clearly enough in the *Proceedings* and even from the very sentence which the *Proceedings'* author has quoted,—had not this gentleman cut off the last half of it.

Here, Sir, accordingly, from the note to *Proceedings*, page 253, where the first half of Dr Sprenger's sentence is made to corroborate statements which the Dr never intended it to be seen in company with,—is that which is quoted there, or thus—

" 'It may be remarked,' observes Colonel Vyse, 'that the
" Arabian authors have given the same accounts of the pyramids,
" with little or no variation, for above a thousand years.' (Vol. ii.
" p. 328.)"

And here, from that page of Howard Vyse's book, is the full sentence, as it appears in the whole note which has Dr Sprenger's name conspicuously subscribed to it:—

" It may be remarked that the Arabian authors have given the
" same accounts of the Pyramids, with little or no variation, for
" above a thousand years; and that they appear to have repeated
" the traditions of the ancient Egyptians, mixed up with fabulous
" stories and incidents, certainly not of Mahometan invention."

In fact, the main purpose of the note is to explain what were those unvaried stories of a thousand years; and it is attached by Dr Sprenger directly to the account of one ***Masoudi***, who, living within 140 years of ***Al Mamoon***, mentions him not, but abounds in histories of the wonderful circumstances attending the building of the Pyramids by certain kings before the Flood; and also of the magical treasures and enchanted wonders which had been found in the monuments in recent times.

Neither Dr Sprenger himself, therefore, nor Colonel Howard Vyse could have intended the sentence to apply, either partially or wholly, to new contents of the latter's subsequent pages, 351, 352; where he introduces, in his chronological arrangement of Arab writers,* the *Proceedings'* author's Al Mamoon authority, Ebn Abd Al Hokm, between Firazabadi, who died in 1438 A.D, and Makrizi, who died in 1467 A.D. Nor to his pages 333 and 355, where he mentions the other two Al Mamoon-noticing writers of the *Pro-*

* " The Arabic authorities have been translated by Dr Sprenger, and I " have endeavoured to arrange them chronologically; a task which has been " attended with some difficulty, as many of them are only known by quota- " tions in the works of posterior writers."—Colonel Howard Vyse's *Pyramids of Gizeh*, vol. ii. p. 179.

ceedings' author, viz., Alkaisi, died in 1187 A.D., and Abou Szalt, of Spain, possibly of about 1400 A.D.

(Hence the attempt made in the *Proceedings* note to p. 253, to produce out of Vyse's books an affirmative testimony to the contemporary character of Al Hokm's relation touching the Khaliph Al Mamoon's reported discovery of a body in the coffer,—entirely breaks down. And as it is of very little consequence for identification with the original builders of the Great Pyramid and their intentions or work, *what* the Khaliph found in the coffer;—seeing that the Pyramid is now considered to have been entered forcibly by men of alien faith, some 1500 years after it was built, but more than 1400 years before the time of Al Mamoon,—we might leave the matter now at rest, but for an additional statement made by the *Proceedings'* author before the Royal Society on April 20. For on that occasion he announced having received a letter from the British Museum, in effect completely ignoring his former authorities, Col. Howard Vyse and Dr Sprenger; and affirming that Al Hakm lived contemporarily with the Khaliph Al Mamoon.

Whether that letter is to be regarded as throwing every other authority into the shade, I do not pretend to know; but the whole document should now be printed and submitted to the same public opinion which has so long sat approvingly on Col. Howard Vyse's and Dr Sprenger's opposite and fuller statements).*†

SIZE OF THE BASE OF THE GREAT PYRAMID.

After stating in his page 254, in a singularly objectionable manner, some of my reasons for considering the determination of the

* Added to the original paper after the meeting.

† This is the note referred to at foot of page 332, respecting Dr Whitman's numbers for the cubic contents of the coffer in the Great Pyramid.

Having already shown in the note to pages 331, 332, how the 6000 cubic inches, case of difference, from my *coffer* measures, is explainable by the former observer, M. Jomard, having without doubt made an absolute error (probably in copying his notes) of 3 whole inches in the depth of the coffer; (for these 3 inches being subtracted before the multiplications are performed, the alleged difference nearly vanishes):—I have now to show that the other alleged case of a difference, or that under the name of Dr Whitman, and to the horrifying extent of 14,000 cubic inches, depends mainly on a blunder of still more transparent character.

Its component numbers are published in Howard Vyse's second volume, length of the sides of the base of the Great Pyramid to be a most important problem in a metrological point of view,—the *Proceedings'* author thus addresses himself *apparently* to that question, under the express title of "What, then, is the exact length of one of its "basis lines?" but *really* to the purpose of blaming me for nearly everything which I either have done, or have not done, with regard to both computations and measurements in the matter.

Apparently for the first purpose, I say, because I cannot find, after going through all his pages, that he has advanced the question beyond the point where I left it, or come to any other distinct conclusion, or discovered any new authorities. But *really* for the second purpose, from what follows: though I shall only touch upon such of the numerous insinuations as may tend, in their

p. 287, as given to Dr Whitman by a British officer of Engineers, and appear there as follows:—

		Feet.	Inches.
Sarcophagus......	Length,	6	6
"	Height,	3	5½
"	Thickness of Stone,	0	6
"	Width within side,	2	2¾
"	Depth, ditto,	2	8

The general aspect of these measures, taken, as they are, either to whole inches only, or mere halves and quarters, shows that no great accuracy was aimed at by the said engineer officer. We may also conclude similarly from the thickness of sides, ends, and bottom being all indiscriminately lumped together as "thickness of stone;" especially when we find that the difference of height (outside measure evidently from the term) and depth (inside measure also evidently from the term, and from its likewise being expressly so stated) makes the bottom thicker by more than one-half of the previously stated general thickness of anything and everything about the coffer.

But the chief anomaly touches the *Length.* That is given only once, and without any direct statement of whether it applies to outside or inside of the vessel; while there is the indirect symptom that it means outside measure, from its standing immediately above "*Height,*" which is a confessed outside reference, and as far as possible from depth and width, both stated to be inside. Hence, looking to the given measures-list, *per se,* we can only take the length given there, or 78 inches, as outside measure,—and when we subtract from it double the "thickness of stone," there result 66 inches for the *inside* length; and that quantity used with the given inside width and depth does undoubtedly give a capacity content, smaller than my determination by about 14,000 inches.

That 66 inches, however, for inside length, is close upon a foot smaller than my measure, which is supported within a very small fraction of an inch explanations, to throw some useful light on either the proportions of the Great Pyramid, or the principles of that more careful mensuration which should be applied to it.

General Principles of Accurate Linear Mensuration.

In order to measure, then, any length of earth lines, in the accurate manner of the best base-line operations of trigonometrical surveys—in which I have myself, in former years, borne a practical part—and which are a very fair ideal of accuracy to look towards in Great Pyramid outside measurements,—it is necessary

1*st*, To have well-defined and fixed terminal points at either end of the line to be measured.

2*d*, To have the ground tolerably well cleared, levelled, or reduced to gentle gradients, between the points. And,

3*d*, To have scientific apparatus, by which the measuring bars may be placed in definite linear positions, and to microscopic accuracy both absolutely level and parallel to the line joining the two terminal points; under temperature circumstances, also, where the deviation of length of the measuring bars, from *a chosen standard, shall be accurately computable.*

by Col. Howard Vyse, the French Academicians, Prof. Greaves, and many others. Wherefore arise the following questions:—

(1.) Was the coffer really of a different length when Dr Whitman's engineer visited it, than in the times of the other measurers alluded to?

(2.) Supposing that the above was not the case, then, A, did the engineer officer make a mistake in his measures themselves, to the extent of 1 foot in 6 feet? or, B, did he, or perhaps Dr Whitman, merely misplace in his notes what he had measured fairly as inside measure, and place it amongst outside measures heedlessly?

I incline, now that Dr Whitman's published numbers have been dragged up as being a very high authority on the coffer's size, to choose supposition B as being the most probable. And then have to take for his *Length* inside, 78 inches; *Breadth* inside, 26·75 inches; and for *Depth* inside, 32 inches by direct measure, and 35·5 if we subtract his "thickness of stone" from his outside *Height*, the mean of these two being 33·75 inches.

Reducing these values from British inches to Pyramid inches, we have 77·92 × 26·72 × 33·72 = 70,206 Pyramid cubic inches, where the alleged 14,000 inches of difference are reduced to nearly 1000 inches; and even that is chiefly chargeable on evident rudeness and mistakes in the *Depth* measure; for the true quantity (say 34·31) is included within the two very wide determinations given.

These Principles applied to the higher Surveying.

Requirements 1 and 2 are comparatively simple to most geodesists; for these gentlemen purposely choose out a plain already nearly level, and insert at either end of the line they select therein, a defining mark of their own, say a block of stone,—plugged near the middle of its upper surface with a brass bolt, and that again with a platinum stud, and the platinum further marked with a microscopic black dot in its centre. Then the horizontal distance from the minute dot at one end of the line, say from 3 to 8 miles long, to the similar minute dot at the other end of that line, is the practical problem to be solved by the measurers, through employment of what we have indicated under head No. 3.

But then comes the grand difficulty to them; and it would be fearful to tell the number of tens of thousands of pounds which the Ordnance Survey of this country has expended, both directly and indirectly, on various methods of base-line measuring instruments, from General Roy's wooden bars, glass bars, and steel chains, up to Gen. Colby's compound compensation bars, with their microscopic and virtual, not actual, contacts. This last description of apparatus is now allowed to be very near perfection when properly worked; but to work it as it should be, requires amounts of officers, non-commissioned officers, men, tents, camp-equipage, measuring bars, microscopes, bar-comparing stations, lining theodolites, adjusting screw tripods, wooden stands, ground-clearing implements, sun-warding means, &c., &c.,—which I believe even still bring up the cost of a base line, either on the Indian or the British Trigonometrical Survey, to several thousand pounds. Yet, whatever the expense, it is cheerfully incurred there; because the accurate length of the line is required to be known, and it cannot be ascertained at a less cost of labour, instruments, and money.

How the Principles should be applied to Pyramid-base Mensuration.

Next we have to apply the same acknowledged principles to the mensuration of the Great Pyramid, or rather, as at present the case is presented to us, the four sides of its base.

Under head No. 1, *defining points* exist there already in the shape of the outer corners of the sockets cut anciently into the solid rock of the hill, to receive the corner-stones of the exterior casing of the Pyramid. These sockets are easy to find and uncover, as I have described in "Life and Work," vol. i. ch. 17; but they cannot be fully depended on for being such, until they shall have been checked by comparing them with traces, such as Colonel Howard Vyse discovered in 1837 on the north side of the Pyramid, in the shape of some of the casing-stones forming the middle of that base-side of the Pyramid, and still *in situ*, or firmly attached to the rock, or lower pavement; (see note to p. 325.)

Equally therefore to test the sockets found, being the right sockets, and also to have the ground between them fit for employing accurate measuring instruments upon,—must attention be paid, to realizing our head No. 2, at the Great Pyramid. There, indeed, this No. 2 becomes the most necessary beginning, and the most expensive part of the whole operation. For every side of the base is at present encumbered by such huge hills of compacted stone rubbish, that no accurate mensuration can be performed over them, and they completely hide under their substance all the fiducial traces of ancient workmanship which we require to get at; and without getting at which we cannot tell whether what we are measuring was once the base-side of thePyramid, or perhaps something else.

If these requirements of No. 2, should on some occasion be somehow or other performed, then No. 3 must be put into execution with all the niceties, and abundance of instruments and men described under the same head for an ordinary trigonometrical survey base-line. The distance indeed to be measured would be much less, but the attention of intelligent and educated men would require to be exerted in an original manner on the marks and meanings left in the lower parts of the Pyramid, the pavement surrounding it, and the rock on which it was founded by its builders of 4000 years ago,—in a manner and degree never exacted from Ordnance officers and men when merely measuring a base-line in a natural plain, for purposes of modern science alone.

Hence I am rather disposed to make the rough estimate, that if No. 1 at the Great Pyramid could be accomplished by an expenditure of L.10, No. 2 would require L.7000, and No. 3 L.3000; and the fully accurate mensuration of the Great Pyramid will never be realised until all these three heads have been carried out by competent parties, and *simultaneously.*

Failure of efforts to have the Principles applied in 1864–5.

I need hardly say, therefore, that when I went out to Egypt in 1864, at my own private expense, and on the savings of a salary of L.300 per annum, I did not contemplate, of myself or unaided, measuring the base-sides of the Great Pyramid at an expense of L.10,000. Knowing, however, from my former experience on a trigonometrical survey, how the scientific part of the operations ought to be performed, I did all I could, before going to the Great Pyramid from Cairo, to urge the adoption of the necessary measures both on His Highness the Viceroy of Egypt and on Egyptian Society. But no one would look at the proposal. The expense of carrying out the preliminary head No. 2, frightened them all. They remembered that even Colonel Howard Vyse, who spent his money *en prince*, had only made three crosscuts *into* the rubbish-mound on the north side of the Pyramid's base, and had not attempted the far greater work of cutting through the *length* if it; while the similar mounds on the other three sides have remained unattempted in any way by any one for the last 700 years at least. And if no one in Cairo would undertake this operation No. 2, of course it was futile to ask them to enter into head No. 3; while it was likely to lead to premature and irreparable mischief being done to the fiducial points of the Pyramid, to ask them to undertake, or sanction my undertaking, what is implied under head No. 1, by itself. I had, therefore, to go out to the Great Pyramid unaided for that work; leave the proper measurement of its base-sides as a legacy to richer men and future times; and address myself to what I could do, alone, with my own hands and very modest apparatus. That accordingly formed my daily toil * from early morning to late at evening, during the whole four months I was at the Pyramid; unassisted all that time by any other measurer, until the very last few days, when a new party

* Since published in the 1st and 2d volumes of "Life and Work at the Great Pyramid."

appeared on the scene, and occupied thereafter the place which—my time and means being then just exhausted,—I was shortly after obliged to vacate.

Libel the First.

The apparition of this new party, however, has furnished the *Proceedings'* author with an opportunity for serious misstatements and grave insinuations, which he has availed himself of in the following manner in p. 255 :—

"At the time at which Professor Smyth was living at the " Pyramid, Mr Inglis, of Glasgow, visited it, and, for correct " measurement, laid bare for the first time the four corner sockets. " Mr Inglis' measurements not only differed from all the other " measurements of *one side* base-lines made before him, but he " makes the four sides differ from each other; one of them— " namely, the north side—being longer than the other three. " Strangely, Professor Smyth, though in Egypt for the purpose of " measuring the different parts of the pyramid,—and holding that " its base-line ought to be our grand standard of measure,—and " further holding that the base-line could only be accurately as- " certained by measuring from socket to socket—never attempted " that linear measurement himself after the sockets were cleared. " These four corner sockets were never exposed before in historic " times; and it may be very long before an opportunity of seeing " and using them again shall ever be afforded to any other " measurers."

This paragraph plainly begins with implying, that *at*, or during, the time I was living at the Pyramid, Mr Inglis (a young and worthy, but poor, engineer's assistant) visited it, made all the preparations necessary for correct measure of the sides of the base; measured them, got into some anomaly about the results; and was not assisted by me, who, although there for the express purpose of performing such a measure, yet "*strangely*" did nothing; and have caused an opportunity to be lost, the like of which never occurred before within history, and may be very long before it will occur again. Wherefore, some of our members may remark, "is that " the person to whom the Society should have given their Keith " medal for his *measures at the Great Pyramid!*"

But I will now set forth how vastly different from the truth, is the above statement of the *Proceedings'* author.

1. Mr Inglis' visit was not made *during* my stay at the Great Pyramid, but close to the latter end of, and after, it. He arrived, when I was, in fact, preparing to leave, had many of my instruments already packed up for removal, and had even fixed a day with the Egyptian government for removing, and had to keep to that day; and he not only outstayed me, but occupied my rooms after I had left, and was much more my successor than contemporary at the Great Pyramid.

2. Mr Inglis did not visit the Great Pyramid on his own scanty means, as I did on mine; or for his own purposes: he was sent there, paid for being there, and furnished with funds for engaging assistants by his employer, Mr Aiton, a weathy contractor with a hundred times the amount of money that I had. A very fearless and independent man too, this Mr Aiton—who had obtained from the Egyptian government greater powers for exploration than I had received; held his own ideas about the Great Pyramid, and amongst others—as rumour said—wished to upset everything that I was doing or concluding about, touching the ancient monument. And I do believe, from all that I have seen since, that if Mr Aiton's employé who stepped into my place as above described, when I left the Pyramid in April 1865, had found anything materially wrong in my account of the same, Mr Aiton was precisely the man to let all the world know of it, and even ring with it, long before this. While, if he *would* open the sockets, and expose them thereby to the chance of irreparable mischief, *he* was the person morally bound to have them properly measured.

3. But Mr Inglis, with the Arabs whom he engaged with Mr Aiton's money, did not by any means do everything that was required for the correct measure of the sides of the Pyramid base. Indeed, they only executed No. 1—the least expensive of the *three* necessary heads which I have noted under p. 340. And even one half of that No. 1, they did not achieve without my assistance, as narrated in "Life and Work," vol. i. p. 532–535.

4. When all the four corner sockets were thus imperfectly opened up, Mr Inglis and myself divided between us such observations as it seemed possible then to make; he chose the *linear*

because he had some tolerable linear, but *no angular*, apparatus:—and I freely left him with the linear duty, which I foresaw could only be approximate, and, in so far, could be as well done by him as by me—while I undertook the angular work; and which, if I had not undertaken, then and there, would not have been attended to at all, on that occasion of the sockets being opened up. Moreover, my time had become so limited that I could not have undertaken both species of measurement; nay, even with the angular measures alone, I was not only occupied with them from the time of the sockets being discovered, to the moment of my being removed from the Pyramid by the Egyptian government,—and then had to leave some of those angular quantities very imperfectly observed and some not at all,—but Mr Inglis kindly assisted me in what I was enabled to accomplish; as fully described and gratefully acknowledged in "Life and Work," vol. i. And it was not until after I had left the Pyramid altogether, that Mr Inglis found time to complete his promised *linear* part of the measure, or the lengths, and levellings, from socket to socket. He laboured at them, as I believe, very conscientiously, and sent me the results by a special messenger when I was leaving Egypt for Scotland: and by me, after having obtained Mr Aiton's consent and approval, they have been published to the world in the name of that gentleman and his assistant.

What credit there is, therefore, in those linear measures—the first that had ever been taken from socket to socket *round all four* of the sides of the Pyramid—belongs, worthily to Messrs Aiton and Inglis, and no one can take it from them. But what discredit is now sought to be attached to them by a certain party, because their measures are not of the exalted accuracy of a well-measured trigonometrical survey base-line, is not theirs (Messrs Aiton and Inglis)—much less is it mine, as the *Proceedings'* author tries to insinuate—but it is the necessary consequence, which I expected from the first, of the measurement being attempted on the strength of No. 1 only having been performed, and no attention paid to Nos. 2 and 3 both of my p. 340, and of my representation, from the first moment of arriving in Egypt, to His Highness the Viceroy.

Libel Second.

Yet, Sir, however far from the accuracy compassable by employing methods 1, 2, and 3, *simultaneously*, may be Messrs Aiton and Inglis' measures, made on the strength of method 1 alone,—they are vastly above anything that can be done in the present ruinous and rubbish encumbered state of the outside of the Great Pyramid, *without* the assistance of that No. 1.

After once, therefore, having obtained, from any observers, any *socket* measures of the base-sides, I have deemed it waste of time to try to get the true base-side length from any older measures taken by observers who had no view of the sockets to guide them; and I have condemned my own attempts as well as those of others irretrievably, when taken under such impossible circumstances for accuracy. Those ante-socket measures were well enough in their day, as the only approximations then procurable; but their day closed the moment that the vastly better approximations, caused by any sort of measurings from socket to socket, had been obtained.

Not a little surprised, therefore, am I to find in a paragraph extending from p. 255 to p. 256, that the *Proceedings'* author, after having begun to deal with *socket* measures, goes back to some ante-socket measures, or the rudest possible approximation of mine, taken merely to check a rumour that two of the Pyramid basis sides were 1000 or 2000 inches longer than the other two; and he enters into a long argument as to whether I am not egregiously wrong in applying to them a correction for a double thickness of casing-stones, less by the comparatively insignificant quantity of 14 inches, than what *he* would have applied.

I shall not go into that argument at all on the present occasion, or attempt to refute one chief insinuation involved, viz., that that rude approximation is a sample of what my best measuring for final purposes would be; because—*first*, I have allowed that my rude observation to be corrected, may be in itself erroneous by *many times* the amount of 14 inches; *second*, I have nothing final founded on it, having positively rejected the whole affair, observation, corrections, and all, as unworthy of being looked to for a moment, as unworthy of "any attention" for the theory, since Messrs Aiton and Inglis' *socket* observations were procured: as may be seen in my "Life and Work," vol. iii. pp. 123 and 124. And *third*, I still think that the course I followed was the correct one to be pursued under the circumstances then existing.*

* Although I would not interfere with the current of the argument in the above pages, concerning the socket measures for the lengths of the base-sides of the Great Pyramid, by introducing there any refutations of charges brought against me on a different matter by the *Proceedings*' author,—I had no intention of eluding altogether any Pyramid accusation by him, which implies faults of a most serious nature.

Such an accusation is this (pp. 255 and 256)—"But Professor Smyth has " 'elected' (to use his own expression) not to take the mathematically exact " measure of the casing stones as given by Colonel Vyse and Mr Perring, " who alone ever saw them and measured them (for they were destroyed " shortly after their discovery in 1837), but to take them, without any " adequate reason, and contrary to their mathematical measurement, as equal " only to 202 inches,———."

This passage evidently implies that I had taken as 202 inches only, what a mathematically accurate measure had made something more; how much then? In the previous two sentences of the *Proceedings*' author (p. 255) we read that Professor Smyth "made each side of the present masonry courses " (of the base of the Great Pyramid) 'between 8900 and 9000 inches in " length,' or (to use his own word) '*about*' 8950 inches for the mean length " of one of the four sides of the base; exclusive of the ancient casing and " backing stones—which last Colonel Howard Vyse found and measured to " be precisely 108 inches on each side, or 216 on both sides. These 216 " inches, added to Professor Smyth's measure of 'about' 8950 inches, makes " one side (of the base of the Pyramid) 9166 inches."

Here then, evidently, we may see that what the *Proceedings*' author attaches 216 to, is not the casing stones alone, as mentioned in the sentence previously quoted; but, either the backing stones by themselves, or backing stones with casing stones; yet whichever it was, he plainly says that Colonel Howard Vyse measured the quantity *to be precisely* 108 *inches on each side, or* 216 *on both sides;* and we must presume that this is the *mathematically exact measure* which he would have had Professor Smyth adopt instead of the quantity of *only* 202 inches, and which he declares was taken by Professor Smyth at that figure *without any adequate reason.*

But Professor Smyth states in answer,—

1. Colonel Howard Vyse did most positively not measure casing stones on *two sides* of the Pyramid's base; he himself does not say, or hint, that he did, and the condition of the rubbish mounds at the Pyramid, testifies to the error of any one who, like the *Proceedings*' author, makes the assertion for him.

2. Colonel Howard Vyse did not measure any lengths about the casing stones which he found on the north side of the Pyramid with "mathematical " accuracy;" for, the casing stones themselves, he and Mr Perring measured only to the nearest whole inch; making the greatest base-breadth = 8 feet 3 inches; and stating elsewhere generally, that the outside of the casing stones is distant from the Pyramid courses of rude and now broken masonry, at that particular part, "about 9 feet." At least that is all that Professor

Libel Third.

In his next paragraph on p. 256, the *Proceedings'* author takes up all the known socket measures of the base-sides of the Pyramid; and as they are precisely those which I have used for the same purpose, and are even taken from my pages,—it might be hoped that there at least, we might agree.

But no! for in the course of the paragraph that author indulges both in a most damaging (if true) accusation against me for my use of the numbers, and in ridicule against the building and its mechanical qualities, for the strange accusation of,—not having been better measured by modern men.

Now the charge against me lies within the simple rules of arithmetic. Three observers, M. Jomard, Colonel Howard Vyse, and Mahmoud Bey, had each of them measured the north side only, of the Pyramid's base, making it 9163, 9168, and 9162 British inches in length, respectively to each observer.

But Messrs Aiton and Inglis had measured all four sides, and made them respectively to each side, beginning with the north one, 9120, 9114, 9102, and 9102 of the same inches long.

From all these numbers then, what shall we conclude to be the length of a mean side of the Great Pyramid's base?

When only the three first determinations were in existence, the plain process was, to take a mean of them; every computer assuming that the other sides of the base were of the same length. But Messrs Aiton and Inglis' measures indicate, *inter se*, that the northern side is longer than the others. Can we, however, trust their measures to the greatest difference indicated, viz., 18 inches?

Smyth has been able to find in the Colonel's books,—and he requests the Royal Society of Edinburgh to ascertain from the *Proceedings'* author where Colonel Howard Vyse has said anything about 108 inches being a *precise and mathematically exact* measure by him of any part of the casing stones, or backing stones, or both together.

3. Professor Smyth under the accusation of having had "no adequate reason" for employing 101 inches rather than 108, as a thickness to be added on to a present masonry course, as he estimated it, to give the ancient bevelled outside surface,—requests attention to pages 22, 23, 24, 25, 26, and 27, of vol. iii. of his *Life and Work* where he had discussed the matter on the best data known to him; and in a work which was in the hands of the *Proceedings'* author when he composed his accusations.

I think not; both because of the necessary roughness of their measuring without having made preparations 2 and 3 (of p. 340); and because their quantity for the north side differs by more than twice that difference from any other person's measures of the same side. But then again, who will vouch for these other measurers? Some persons maintain that civil-engineers of the present day are so superior to those of the past, that Mr Inglis' measures must be the best in the field; others again rather hold the opposite. How then ought the mean of all the just-mentioned numbers be taken? "Shall I take the mean of all four of Mr Inglis' sides as one side, "and so give him a weight of one only, against each of the three "older observers? Or shall I take each of his sides as a determi-"nation as good as each of theirs, and in that case give him a "weight of four, to their one each?" That, however, would not be paying sufficient respect to the exceeding care with which M. Jomard, and the Academicians accompanying him, say that they measured the north side; while again, to give Messrs Aiton and Inglis the weight of *one* only, would not sufficiently mark a sense of approval of their having been the first known men to measure all four sides of the Great Pyramid's base, from socket to socket; and I was bound in honour, and as a witness, to testify for them there. So I settled to give them the weight of two.

I do not pretend to say that I was quite right in that estimation of the probable worth of their measures. But I do mean to say, that having first of all printed those measures pure, simple, and complete—and then stated the principle on which I should take the mean—I took that mean honestly and fairly; and its final result, to the nearest whole inch, came out truly 9142 inches.

But the *Proceedings'* author implies that I did *not* take that mean honestly and fairly; and there is the difference between us. His words are, "he (Professor Smyth) takes two of them (Mr Inglis' "measures) 9114 and 9102—(*but strangely not the largest*, 9120)—as "data; and strikes a new number out of these two, and out of the "three previous measures of Jomard, Vyse, and Mahmoud Bey."

Now, if I had wilfully left out the largest number of the Inglis series, where each member had been considered of equal weight,—I should have been dishonest, and unworthy of fulfilling any scientific appointment. But I did not do so, either wilfully or accidentally. For this is how I proceeded to take equal account of each and every one of the Aiton and Inglis' sides, while I gave their whole, the predetermined weight of two, against the one of each of the other three observers.

Aiton and Inglis,	9120	Jomard, . .	= 9163·
,,	9114	Vyse, . .	= 9168·
,,	9102	Mahmoud Bey, .	= 9162·
,,	9102	Aiton and Inglis, mean of	= 9109·5
		Same repeated, .	= 9109·5
Mean to tenths, = 9109·5.		Mean, . .	= 9142·4
		or to nearest whole inch,	= 9142· *

Now here is no *strange* exclusion of 9120, when 9114 and 9102 are admitted; and, in fact, such an unwarrantable piece of cooking as that would have been, never entered my head, until I read it charged upon me by the *Proceedings*' author. It is, in fact, a pure invention of his, so far as I know or can see—though I must request the Council of the Society, who have printed the erroneous accusation to ascertain what its author has to say in his defence.

Reported additional Authority.

After venturing the statement at the end of the same paragraph, on p. 256—that the above mean length of 9142 inches, is not derived by "direct measurement, but by indirect logic"—the *Proceedings*' author adds to my "Life and Work" data, the following particulars from a subsequently printed number of the *Athenæum*:—

"Lately, Sir Henry James has shown that the length of one of "the sides of the pyramid's base, with the casing stone added, as "measured by Colonel H. Vyse,—viz. 9168 inches—is precisely "360 derahs, or land-cubits of Egypt; the derah being an ancient "land measure still in use, of the length of nearly 25½ British "inches, or, more correctly, of 25·488 inches."

In this sentence, many persons, recognising the name of the

* While using this quantity in most calculations, I usually state that it is uncertain, from the large difference of its factors, to the extent of ± 25 inches. With such differences, we need not descend to fractions; except where, as above, the *doubling* decided on brings the fraction up to a whole inch.

principal head of the Ordnance Survey, may think that there is a new and very authoritative determination of the length of the base side of the Great Pyramid. But a little examination will soon show,—*first*, a slight inaccuracy; for 360 times 25·488, or 9175·68, is not the *precise* equivalent of 9168 inches; *second*, that it (9175·68) does not lie within the limits of *any of the known socket measures* of the Great Pyramid; and *third*, that the derah of 25·488 British inches long, is not known to be an *ancient* Egyptian land measure.

For on what does the alleged statement that it is such a measure depend? So far as I can ascertain, merely this: that Sir Henry James saw the length and name mentioned—in one of Weales' rudimentary treatises giving the weights and measures of all nations for 1*s* and 6*d*—as a *modern* Egyptian measure. But more important works on modern Egypt, as Lane's and Wilkinson's, do not acknowledge, so far as I can find, such a modern land measure, neither do they mention it as belonging to past history. While all of the works together, or Weales' also as well as these last, agree that *the* standard national cubit of *ancient* Egypt was very near to 20·7 British inches in length, or totally different from 25·488 of the same inches.

Hence I am inclined to fall back after all on my former conclusions from the Pyramid socket observers above stated; or to believe that the true length of a mean side of the Great Pyramid, when properly measured under guidance of the 1, 2, and 3 of p. 340, will be found to lie within the limits of 9142 $\pm$ 25 British inches: *i.e.* the present uncertainty is within $\frac{1}{366}$ of the whole. A large proportional error—compared to what it might be reduced to any day, if the Government were consenting to spend on the measures the merest fraction of what the surveying of this country costs them every year; but only a small proportional error—as contrasted with what affected our knowledge of the Great Pyramid 100 years ago; and sufficiently limited to enable us to test, with considerable approach to sufficiency, several theories already before the world.

SIZE OF THE EARTH.

In the course of my investigations into the nature and objects of the Great Pyramid, I have had—beginning with 1864—to compare its measured size in parts, with definite portions of the length of the earth's axis of rotation. The length of that axis I have taken, successively, from means of the best determinations thereof by the greatest geodesists of the age, giving their names, and generally their original quantities; and my adopted mean has always been within the limits enclosed by their several measures; these, slightly differing from each other in the smaller numbers—but so slightly as not to prevent the earth's polar axis being now considered an exceedingly well determined quantity. That is, it is known within a very small fraction of the whole length; which is all that man with his merely finite powers can expect to attain to in any practical problem, whether it refers to the length of a moderate bar of metal, or the dimensions of a cosmical globe.

Accusation First.

Nevertheless, the *Proceedings*' author accuses me of having acted so flagrantly as to have used in my researches "a *supposititious* polar axis of the earth," p. 261; also a something which Captain Clarke (an admitted authority) "did not find it to be;" and finally he states, on his p. 263, that the polar axis of the earth "is still itself an unknown and undetermined linear quantity."

This is the three-fold charge, and now I have to show that each item is utterly erroneous, and is followed by worse things still.

1. As to having used a "supposititious," instead of the measured, length of the earth's polar axis,—I point to p. 450, vol. ii. of my "Life and Work,"—where I have quoted at length, and have followed in vol. iii. the results of calculations based on all the best known measures of the earth, from the worthily authoritative quarto recently published by the Ordnance Survey Office under Sir H. James,—chiefly computed by his excellent mathematical assistant Captain Clarke.

Accusation Second.

2. But it is next said by the *Proceedings*' author, that I, having employed 500,500,000 British inches for the length of the polar axis,—have used for it, that "which Captain Clarke did not find it "to be." Let us see if this charge is true, from the pages of the *Proceedings*' author himself. He states that Captain Clarke found the length of the earth's polar axis by one mode of computation

= 500,522,804; and by another mode 500,482,296 British inches. The *Proceedings*' author does indeed considerably confuse his explanation of the principles of each method; and wherefore the final difference out of the same observational quantities to begin with, —but his numbers are correct, and are probably derived from my own pages with their peculiar mode of expressing Captain Clarke's results of feet or yards, in inches, and showing them, sometimes for the whole axis (vol. iii. p. 134) and sometimes for the semiaxis (as in vol. ii. p. 450.)

Now, Captain Clarke himself does not attempt (as I had already shown in "Life and Work," vol. ii. p. 451) to decide entirely between the two calculated results he has given,—but he states each of them, and seems to leave his readers to do as they like between the two. Anywhere between the limits of his two quantities, therefore, is to be looked on as derived from him, and as, in fact, his determination, rather than any one else's, especially if with some reference to his limits of error or variation.

Now the exact arithmetical mean of the Captain's two published quantities, stands thus—

First computation,	500,482,296
Second ,,	500,522,804
Mean,	500,502,550

or differing from 500,500,000, by only $\frac{1}{16}$th of the amount of the debateable ground between the two almost equally important factors given by him. If, moreover, a very little increase of weight be extended to the first result, on account of the rather higher analysis by which it is brought out by Captain Clarke, the tendency is to reduce even that small difference, or perhaps throw it to the opposite side. Wherefore, if any one will only trouble himself with as many rough digits as he can be certain of,—Captain Clarke's whole result may be stated in a compendious manner as 500,500,000 with a probable error of $\pm$ 20,000 British inches.

If then I had stated the length of the earth's axis as outside all Captain Clarke's debateable ground between his two results, say at 500,550,000 or 500,450,000 inches,—the *Proceedings*' author might have truly said that I had stated it at a figure which Captain

Clarke did not find it to be; but when I stated it close to the middle of his debateable ground, and after giving his two limits of that ground,—this second charge of the *Proceedings'* author is no truer than the first; and appears to me a deliberate misrepresentation of the numerical results before him.

Second Part of Accusation Second.

2. 2*d part.*—Not content, however, with making the charges indicated above, the *Proceedings'* author, on his p. 261, asks—

" But is there any valid reason whatever for fixing and determining as an ascertained mathematical fact, the polar axis of the earth to be this very precise and exact measure (viz., 500,500,000 British inches), with its formidable tail of nothings?"

And then he answers, with implied crimination, "None, except the supposed requirements or necessities of Professor Smyth's pyramid metrological theory."

But upon which Professor Smyth remarks, that—*first*, he has not done what the author describes in his question; but, as already shown, has taken his numbers from the results of computed measures of practical observers; and *second*, it is an uncalled for calumny to say that Professor Smyth's pyramid metrological theory requires, or necessitates, that there should be any particular or very precise number, and with many 0's at the end of the British inches in the length of the earth's axis of rotation. From his first quotations from Airy, Schubert, Herschel, Bessel, and others, down to his last from the Ordnance Survey volume, Professor Smyth, in prosecution of the Pyramid metrological theory, has merely sought to know how many British inches long, those who have measured the said axis, by the nearest approximations that can be made,—have found it to be; and whatever the latest and best measurers have found and proved it, he has always accepted; and he is still ready to act in the same manner, for any geodesical results of the future, if they shall be proved intrinsically better than the existing ones.

Accusation Third.

3. Further on, however (at p. 263), comes a more wide-sweeping libel; and now against, not Professor Smyth, but Captain Clarke, Sir H. James, and all the greatest geodesists of every country

throughout the civilized world; for after all their efforts during the last fifty years, and notwithstanding the stupendously large surveys they have been carrying on, at an enormous expense to the nations concerned, in order to ascertain the size and shape of the earth,—the *Proceedings'* author declares, that the length of the polar axis thereof, "is still an unknown and undetermined linear "quantity."

Yet, if we take even twice Captain Clarke's two limits, already given,—the difference of their mean from either one or other, amounts to less than $\frac{1}{10000}$th part of the whole axial length:—a proportional quantity, which, on a foot-rule, would be equal to a lengthening or a shortening of it by much less than the breadth of a hair of the finer human order. And inasmuch as rules are sold in good opticians' shops as foot rules, or as being of the known length of a foot, though they may differ from each other in length by more than ten whole hairbreadths,—why, it is plain that the length of the axis of the earth is to be considered, even as compared with some modern measuring rules, to be remarkably well determined and known. The statement of the *Proceedings'* author is therefore an unfounded slur on the work and reputation of the best astronomers, geodesists, and surveyors of many countries, both in the present and past generations; men who have deserved exceedingly well, both of the scientific and general public, throughout the whole civilized world.

Accusation Fourth.

4. That in the same *Proceedings'* paper, where the length of the earth's axis has been declared by its author to be "an unknown "and undetermined linear quantity,"—there should be the following sentence:—"The engineers and mathematicians of different "countries have repeatedly measured a meridian arc of a degree of "sixty miles, in order to use it as a standard for linear divisions. "As part of their standard, they measure off sixty miles of the ir-"regular earth-surface of a kingdom with almost perfect mathe-"matical exactitude,"—may seem extraordinary.

Extraordinary in the *first* place—because, though a project has been started from time to time, of using some special round and even subdivision of the length of a sexagesimal degree of the meridian for a standard of measure,—it has certainly not become a common practice among nations, nay I doubt whether a single people has adopted it: and extraordinary in the *second* place, because, if such degrees of the meridian can be measured with "almost perfect mathematical exactitude,"—the length of the earth's axis can be computed from them to the same proportional amount of most remarkable perfection; instead of remaining as the *Proceedings'* author says it does remain, "an unknown and undetermined linear quantity."

The question arises, therefore, why has the calculation not been made? And the answer must be—that no one but the *Proceedings'* author knows of any such marvellous data for the purpose: and he is either so inexpert at the computation that he cannot, or so careless of the state of geodesical knowledge in his time, that he does not choose to, make the very great improvement, which his statement implies that he might do, in our physical knowledge, or in our idea of the exact length of the earth's polar axis,—the most important base which men can refer to for measuring the sizes and distances of the sun, planets, and stars.

No one else, I repeat, knows of these almost absolutely perfect data. Such earth-axial knowledge as that represented by the last volume of the Ordnance Survey, is based certainly upon the measured lengths of arcs of the meridian,—but not depending on measurements of one degree only. On the contrary, there is a class of errors concerned, whose proportional effects increase so rapidly, with the smallness of the arc, that the tendency of the age has long been to measure an arc of as many degrees as possible, instead of only one degree. Hence, both in India, and Russia, Britain, France, and Germany, the arcs measured have been made to contain even 10°, 30°, and 60°, by extending them, where possible, across neighbouring countries. Thus the Indian arc, begun near Cape Comorin, is being pushed northward over the Himalayas. The Russian meridian arc, commenced at North Cape, has crossed the Danube, and is approaching the Mediterranean; while all the states of Europe are at this moment engaged in a grand arc of parallel, stretching from Valentia in Ireland to the Ural Mountains in Russia.

Yet though both Captain Clarke's and other geodesists' results are based on all these large arcs, and which *must* give more accurate results than if the same parties had measured single degrees only,—the *Proceedings'* author pronounces their final deductions worthless, in so far as, in spite of them and the faultless formulæ of computation employed, the length of the earth's axis is "still an "unknown and undetermined linear quantity;"—what, then, must not be the overwhelming excellence of the single degree arc-of-the-meridian measures, which the same fault-finding author describes in the same critical paper, as endued "with almost perfect mathe-"matical exactitude?" Why, of course, they must be supernaturally perfect, *i.e.*, if they exist. But do they exist? At present we have only the word of the *Proceedings'* author that they do. And why has he favoured the world, through the Royal Society of Edinburgh, with his statement to that effect?

"Because he knows of their existence, and can produce them," ought to be the answer. But if that answer be not rendered? Why then we have only to look to the sentence following that one which we quoted near the head of this article 4; and there stands a reason of a very exceptionable order, and which manifests its nature increasingly in the latter portions of the *Proceedings'* paper. That reason is given in the words,—"Professor Smyth holds that the "basis side of the Pyramid has been laid down by *Divine* authority "as such a guiding measure."

Accusation Fifth.

In the above sentence then, we have touched at last on the origin of the unnatural excellence attributed to certain unknown surveyors' and mathematicians' measures of 60 miles long: viz., Professor Smyth holds that something about the designing of the Great Pyramid was due to *Divine inspiration* afforded to its architect;* and immediately the *Proceedings'* author insinuates that just such a something is often accomplished by unassisted man in the present day,

* In the same manner as we read in the Bible, it did occasionally please the Almighty to issue instructions for practical works, showing the pattern thereof, pronouncing the sizes they were to be made in all their chief parts, and sometimes even inspiring the workmen with the requisite skill to prepare them. See Exodus xxxi. 1–11; 1 Chronicles xxviii. 11, 12, and 19, 20; Acts vii. 44; and Hebrews viii. 5.

and carried out by him both on a vastly larger scale, and with infinitely more accuracy.

Of course a line 60 miles long, *is* on a larger scale in mere length, than one side of the base of the Great Pyramid, only $\frac{1}{7}$th of one mile long. But then what is there similar, between measuring 60 miles' length of open country in the latter days of the world, and the building of a transcendent monument in primeval times, with an excellence of masonry never since surpassed, and able to endure and bring down through more than 4000 years proof of a pure mathematical figure of important meaning, besides many physical symbolizations expressed in the size of the whole and its parts, as well as in their astronomical emplacement?

The two things are evidently of a totally different kind, and we should have nothing more to reply to under the head of accusation (5), unless the *Proceedings'* author had further propounded the following depreciation of both the modern measures of the Great Pyramid's base-side, and the said base-side itself; in the following words (p. 256): —

" Surely it (the base-side of the Great Pyramid) is a very strange
" standard of linear measure that can only be thus elicited and de-
" veloped—not by direct measurement, but by indirect logic, and
" regarding the exact and precise length of which there is, as yet,
" no kind of reliable and accurate certainty."

Now, the recorded and received measures of the Pyramid's base-sides are, no doubt, as we have already seen at p. 351, sadly rough and rude; but then they are in so far true measures, made by actual surveyors, French, British, and Egpytian, at various periods during the last 70 years, and not results ascertained by " logic."

There is also, at present, as already shown at p. 352, *some* amount of reliable certainty as to the measured length; and if any one desires to have that amount of certainty increased,—let them, especially if they are rich men, adopt the necessary practical means for obtaining it: as indicated on p. 340.

The *Proceedings'* author, indeed, intimates that the length of the Pyramid's base-side *cannot* be obtained by the ordinary human means, viz., direct measurement, but only by indirect logic. How logic, whether direct or indirect, is to procure it, I do not presume to know. And equally am I ignorant why direct measurement should not be equal to the problem; for there, defined by the incised corner sockets, is a length marked in a levelled surface of foundational rock, and capable, when certain modern, adventitious mounds of rubbish are cleared away, of being measured as accurately as any base-line that modern surveyors have measured on any part of the earth's surface.

Therefore, if modern men do not yet know that Pyramid base-side length accurately, whose is the fault, but their own; and instead of vilifying the character of the Great Pyramid, it would be more to the purpose, were they, even now at the eleventh hour, to take shame to themselves, and send out a strong party of surveyors and measurers, to undertake the work they have not yet performed, and with all those same means and appliances, which they employ in any other case where good lineal measures over long distances are required.

THE SACRED CUBIT OF THE HEBREWS.

In his pages 257 and 258, the *Proceedings'* author states, rather ambiguously, that Professor Smyth believes that the length of the base-side of the Great Pyramid, when measured with a standard whose length is the ten-millionth part of the semi-axis of the earth,* exhibits the number of days and parts of a day in the year; that that standard is the Pyramid Cubit; that it is the same in length as the Sacred Cubit of the Hebrews—such cubit measuring 25 Pyramid inches, = 25·025 British inches; and that the whole idea, apparently, of the metrical theory of the Great Pyramid, is built upon the correctness of this Hebrew cubit datum.

The author then goes on more distinctly to attack that datum, and to show that it is exceedingly incorrect: That Sir Isaac Newton came long ago to the conclusion that the true length of the Sacred Hebrew Cubit was 24·82 inches, from which it must not be altered: And that Professor Smyth, in taking the means of Sir Isaac Newton's original quantities, and representing them as 25·07

* According to Captain Clarke's two limits already given, such fraction must lie between 25·026 and 25·024 British inches; wherefore I have usually taken 25·025 ± ·001 British inches as the practical quantity to apply: in so far I believe in accordance with the eminent and exemplary authority of Sir John Herschel.

± ·10 British inches, made a gross mistake; for the mean, of certain intermediate numbers printed by him, is really 25·29.

Now, in the first place, it must be seen pretty clearly by most persons that the length of the Sacred Cubit of the Hebrews cannot be really necessary, in a direct way, to the metrical theory of a building erected long before the time of Abraham. And, indeed, the modern Pyramid scientific theory was elaborated without any dependence upon it, and still stands on its own foundation and references to science. But after the Pyramid theory was so far worked out, and had produced a Pyramid cubit of a particular length,—then that length was compared with other cubits: and, though found utterly different from the cubits of Egypt, Babylon, Assyria, and other pagan Gentile nations,—yet came very close to what Sir Isaac Newton had deduced for the length of the *Sacred* Cubit of the Hebrews.

Sir Isaac Newton's treatise was therefore examined, and while it was found that he looked upon his adopted result as only temporary, imperfect, and even distinctly to be replaced at a future time by some other person's completer researches,—a careful re-examination of his data, according to modern methods, led Professor Smyth to a slightly different quantity, as being a better approximation to the truth therein enveloped, viz., the length of the sacred Cubit of the Hebrews, than that quantity which Sir Isaac had employed. This difference was not of an amount to alter our most eminent philosopher's grand conclusion as to the total difference between that Sacred Cubit of the Hebrews, and the profane cubit of the Egyptians,—but only to affect a residual question of a much minor lineal difference, and which had not occurred to him, or to any one in his day.

I fully believe, therefore, that Sir Isaac Newton himself, were he still alive, would allow that my deduction of 25·07 ± ·10 British inches, from his several data, expressed chiefly in Roman unciæ, is a truer result than his of 25·6 Roman unciæ, interpreted variously by modern authors as anywhere between 24·75, 24·82, and 24·89 British inches.

In order that there might be no mistake about what Sir Isaac Newton's opinions and data were, I reprinted in the 2nd vol. of "Life and Work," the whole of his original paper: and only commenced the discussion upon them in a subsequent part of the volume. In that portion of my work, it has recently been discovered that there is an unfortunate misprint of certain intervening computed quantities, between Sir Isaac's original data, and my final deduction from them; some erroneous slip of paper having apparently got introduced into the MS. finally sent to the printer. I acknowledge that there is such an error, thank those cordially who have discovered it; and have already rescinded it from the volume by printing it as an error in a list of errata, of which list I place a bundle of examples on the table, so that any member who has a copy of "Life and Work," may supply himself with this addition.

A very little attention in collating and comparing, would have indicated to the *Proceedings'* author that these intermediate figures, as they are printed in the 2d vol. of my "Life and Work," p. 458, are simply misprinted: for, they are neither correctly derivable from the original quantities, nor is the following *mean* derived from them. But he has chosen rather to suppress the fact of my having given, in the same volume, Sir Isaac Newton's original numbers, and given them correctly and completely, to the extent of several pages; omits Sir Isaac's statement that he looked on his one chosen and adopted quantity as temporary,—parades in *Proceedings*, p. 258, my misprinted intermediate figures, as if they were all important original ones; and glories in pointing out that the *mean* put by me underneath them, is not the correct mean of those numbers. Of course it is not; because it was derived from other numbers. And as both that final mean of mine is rightly printed, as intended, and the original quantities are also correctly printed,—what occasion was there for bringing up this accidental misprint of some intervening figures so very prominently, and alluding to it as something radically subversive of the whole theory of the Great Pyramid?

To assist those who may try to ascertain, I subjoin, in a note, a description of the numerical particulars of the case, prepared originally for the *errata* list in "Life and Work;"* and now I pass on to the consideration of the next principal section.

* *Sir Isaac Newton's Numbers for the Length of the Sacred Cubit of the Hebrews.*

At p. 458, vol. ii. of *Life and Work at the Great Pyramid*, there is an unfortunate misprinting of the calculated numbers representing in British inches,

SIGNIFICANCE OF CYPHERS AND FIVES.

Under the above head, p. 264, the *Proceedings'* author has some remarks as to certain persons, in their comparisons of lengths in the Great Pyramid, with the size of the earth, imagining " that if

the quantities from which the mean "25·07, ± ·10 British inches," for a new statement of the length of the above cubit, was derived. This *final* mean is correctly given, as intended; so likewise are the *original* terms, expressed chiefly in Roman Unciæ, in Sir Isaac Newton's Dissertation on Cubits, reprinted at pp. 354–366. No important mischief therefore is likely to have accrued, from this error in printing one of the *intermediate* steps. But as the error is an undoubted blemish, which I much regret, have cancelled in the list of *errata*, and sincerely thank those who have called my attention to it,—I hasten to give the following discussion *de novo*.

At p. 365, of Sir Isaac's treatise above mentioned, he assumes 25⅝ Roman unciæ, to represent the length of the Sacred Cubit of the Hebrews,—a cubit which he had elsewhere shown, there were grounds for believing that that people possessed before they went down into Egypt, and had had specially brought to their attention again, for religious matters, after leaving Egypt under Moses.

But Sir Isaac Newton was not at all confident of having obtained the precise length, to the last figure put down in his arithmetical expression. And he particularly and almost prophetically says,—

" This is what I thought proper to lay down *at present* with regard to the " magnitude of this cubit. Hereafter, perhaps those who shall view the " sacred mount, and the monuments of the *Chaldæans*, by taking accurately " the various dimensions of the stones, bricks, foundations, and walls, ' and comparing them together, will discover something *more certain and* " *exact*."

Now what Sir Isaac laid down at that then present time, was abundantly sufficient for his then purpose; or to prove, that there existed a most sensible and positive difference in the length of that sacred (or 25⅝ unciæ) cubit of the Hebrews,—and, of the profane cubit of the Egyptians, whose length, expressed in the same Roman unciæ, was hardly more than 21·3. And in this last conclusion, he is so eminently borne out by all subsequent investigators, that that subject—or the length of the *profane*, or ancient Egyptian national cubit—need not be stirred again.

But within the last few years, another, and a more refined, or a residual question has arisen, which apparently never crossed Sir Isaac Newton's mind, viz., was the Sacred Cubit of the Hebrews, taken by itself, accurately the ten-millionth part of the length of the Polar semi-axis of the Earth? And as this quantity in Nature, according to modern science, is something very close to 25·8 Roman unciæ,—Sir Isaac's determination of 25 and ⅝, *i.e.*, 25·6 of the same unciæ for the Sacred Cubit, is, to say the least of it, so near—especially for a confessed imperfect approximation, from a portion only of the materials collected,—that it becomes intensely important to submit *all* the data to a more rigid scrutiny than before; with the caution moreover in view,

" by multiplying one of their measures or objects, they can run the
" calculation into a long tail of terminal 0's, then something very
" exact and marvellous is proved." This he derides, because, ac-

of assigning some limits, within which we may feel tolerably certain of the result.

The several quantities therefore, extracted from my reprint of Sir Isaac Newton's paper (but to which in the original, I cordially refer all readers), and reduced to British inches—at the approximately assumed rate of 12·15 for 1 Attic foot; and 0·97 for 1 Roman *uncia*—are as follows :—

No. of Approximation.	METHOD OF REFERENCE.	Attic Feet.	Roman Feet.	Roman Unciæ.	British Inches.	British Inches.
						Mean of limits.
1.	From Talmudists and Josephus, by the Greek cubit (p. 355), Vol. ii., of *Life and Work*, between	$2\frac{4}{7}$ and 2	... ...	... ...	31·24 24·30	27·77
2.	From Talmudists, by proportions of human body (p. 355), between		... ..	$28\frac{4}{5}$ 24·	27·94 23·28	25 61
3.	Josephus by pillars of Temple, in proportions to the human body (p. 356), between		$2\frac{1}{3}$ 2	28·0 24·0	27·16 23·28	25·22
4.	By Talmudists' and " all Jews' " ideas of a Sabbath-day's journey (p. 358), between . . .		$2\frac{1}{3}$ 2	28·0 24·0	27·16 23·28	25·22
5.	By Talmudists and Josephus, on proportion of steps ascending to Inner Court (p. 358), between			27·0 24·0	26·19 23·28	24·74
6.	By many Chaldaic and Hebrew proportions to Cubit of Memphis (p. 362) .			25·6	24·83	24·83
7.	From a statement by Mersennus, as to the length of a supposed copy of the Sacred Cubit of the Hebrews, preserved in a secret way from the knowledge of the Christians (p. 363) . .			25·68	24·91	24·91

The simple mean of the last column, = 25·47 British inches. But that is not a proper method there ; because, not only has Sir Isaac Newton evidently cording to him, "many small objects, when thus multiplied sufficiently, give equally startling strings of 0's;" and he then goes on to furnish three such examples; the first being made gratuitously

shown that he had most confidence in his two last determinations; but his first, by its very wide limits, shows that it is by far the least trustworthy of all. Some decrease of weight, therefore, for No 1, and increase for Nos. 6 and 7, require to be made. How much precisely, it is impossible to say: but perhaps ½ for the former, and 3 for each of the two latter, the intermediate quantities being reckoned at 1 each,—may be considered fair and probable. In which case the mean comes out, 25·05 British inches.

While, simply,—and in fact as I did on the first occasion, using then a slightly different value of the Roman uncia,—throwing away the one very objectionable observation, and taking a mean of the rest, unweighted, gives 25·09 of the same inches.

But neither 25·09, nor 25·05 are fully safe, either in the second, or perhaps the first, place of decimals;—for—besides the uncertainty connected with the proper weighting of each of the results, according to the different kind of documentary evidence obtained by Sir Isaac Newton on each occasion,—there is considerable uncertainty in the value of a Roman uncia, expressed in British inches. We have assumed as above, that the former = 0·97 of the latter: but modern scientific and architectural authorities are found anywhere, between Zach at 0·9681 and Penrose at 0·97286; and might require us to reduce our final quantities by — ·05, or increase them by + ·06 of an inch; or by any intermediate figure.

Wherefore, the statement already printed at p. 458 of vol. ii. of *Life and Work at the Great Pyramid*,—*i.e.* 25·07 ± ·10 British inches, for the best result deducible from all Sir Isaac Newton's approved approximations for the length of the Sacred Cubit of the Hebrews,—is, if not as good a statement as can be made,—at least a great deal better than the 24·82 inches, absolute, which has been hitherto current in most English works; and beyond comparison better than the 20·7 inches, nearly, of the ORDNANCE SURVEY *Map of Jerusalem*.

This Ordnance quantity of 20·7 inches is evidently not the *sacred* cubit at all, but the *profane* cubit; and in the explanations of the scale at the foot of the above map, the revered names of "Sacred," and "Cubit of the Tabernacle," are given to precisely what Moses was so anxious to keep them from being confounded with—viz., the cubits of idolatrous Egypt and other Gentile nations; the inscriptions at one end of one of the Ordnance-map scale-lines being—"*Egyptian, Hebrew, Babylonian*," and at the other end of one and the same line—"*Royal or Sacred Cubits*, also *named Cubits of the Tabernacle*."

If this map is one of those prepared, as believed by some, at the expense and to the orders of the Fathers of the Palestine Exploration Association—such a radical error with regard to the *sacred cubit of the Hebrews* may well excite surprise. But if, on the contrary, the map is purely the work of the several Ordnance officers whose names are conspicuously engraved upon it—the nation must regret that they should have so entirely ignored the researches of Sir Isaac Newton, the greatest philosopher their country ever produced, and in one of the most important of all questions that have ever been brought forward in either the science or history of metrical standards.

to insult religious feelings and scientific ideas, in its mode of reference to Sir Isaac Newton's *Sacred Cubit* of the Hebrews,—the cubit according to whose standard Sir Isaac believed that the true God was once pleased to define to men the size of the Ark of the Covenant, the Tabernacle, and the Temple of Solomon, in the several revelations averred in the Holy Scriptures to have been made for that end both to Moses and David. As insulting, I am sorry to say, to other men's, both religious and scientific, ideas as possible; for what is it, which the *Proceedings*' author brings up as being in length a half, or as good as a half, of that *sacred* primeval measure of the earth,—why his own ill-defined and flexible hat. And at the time of delivering the discourse, of which the *Proceedings*' paper purports to be only a part representation of the substance, its author even went through the hypocritical form of demurely measuring the said variable and inexact hat (placed, together with a measuring scale, on his reading desk by an assistant, before the lecture began), and then finding, with smiles all over his countenance, before the members assembled, that the larger diameter of said hat's brim was just half of the "Sacred Cubit!"

This act,—much to be regretted,—has crowned itself with confusion in the *Proceedings*' paper, and at the performer's own hands, by all his three examples failing miserably.

Thus the misguided, though in other things learned doctor, says —that his hat's brim of 12·4 inches in length, is 1-20,000,000th of the earth's polar axis, that being taken at 500,000,000 inches; but any school-boy may see that it is rather the 1–40,322,581st part thereof; a result where there are few 0's, and a very large difference at the beginning.

Next, he says that "a page of the print of the Society's *Trans-* " *actions* is 1–60,000,000th of the same." Here, indeed, he prudently avoids giving the measure of the said page; but I, having measured it on the same principles as his third case indicates, find the limits as being not less than 8·37, nor more than 8·43 inches, on complete pages of a recent volume. Wherefore taking the mean = 8·4 inches,—that will be found, not the 1–60,000,000th even, but the 1–59,523,810th part of the same axial length.

And finally he states that "a print page of Professor Smyth's "book, 6·2 inches in length, is 1–80,000,000th of this *great* "*standard;* &c., &c." But 6·2 inches form, evidently, not that even fraction, but the 1–80,645,161st part of the same 500,000,000. A length, however, this last, in *British* inches (and no others are there alluded to by the British *Proceedings'* author), which neither Professor Smyth stated, nor Captain Clarke, nor any other known geodesist, ever found, the earth's axis to measure. Nor is the length 24·82 inches, as shown in the previous section, the length which Professor Smyth has deduced, or considers to be deducible from all Sir Isaac Newton's numbers, taken together according to modern scientific principles, for the length of the Sacred Cubit.

All the three examples thus given by the *Proceedings'* author have totally failed in producing his promised "startling strings of "0's:" and though he does put "&c., &c." after his last case,—yet if these symbols refer to other examples of the same order as his previous ones, they will not strengthen his case.

With a very little cleverness in looking out through the world, no doubt other persons might find some trifle coming more nearly to an even fraction of the earth's polar axis than any of the *Proceedings'* author's given examples. But then it must be remembered, that when the length of that said axis has already been ascertained by national efforts,—there is no more difficulty in finding a practical equivalent to a small fractional part thereof, than in performing a sum in simple arithmetic: and after this little thing should have been done, it would really possess no similarity with the alleged case of standards of measure in the Great Pyramid,—deposited or built in there, on a gigantic scale, 3000 years before any accurate determination of the length of the earth's polar axis had been made by men,—and yet found now to be an apparently exact and even decimal and quinary fraction thereof.

This really astonishing geodesical character of the Pyramid standards, is a matter to be disproved, only by still more accurate measures of both the Great Pyramid and the earth being instituted; and then, perhaps, found *not* to be commensurable by quantities larger than the limits of error of the measures. At present there *is* a coincidence *within* the limits of errors of the best modern measurements of both; but in the case of the Great Pyramid these are not by any means so refined as they might be. The proper course, therefore, for both religious minded, and scientifically inclined, persons is, or should be, not to take to "scoffing and deriding," but to visit the Great Pyramid in a calm and working philosophical spirit, and to remeasure more exactly all those still existing fiducial markings which have marvellously descended to our latter times from primeval ages, closed long before history began.

With regard, next, to the *Proceedings'* author's termination of his section; or, that many arrangements in fives are to be seen in the rooms of the Royal Society of Edinburgh,—*i.e.* in a modern building of a most confused multiplicity of parts, and made up in design by copyings from several foreign, and some ancient, architectures,—and his conclusion that the arrangements were not carried out there for any special purpose of symbolizing five; and "that there is not the slightest ground for any belief that the "apparent *fiveness* of anything in the Great Pyramid had a different "origin;"—I can only spare time to remark that the two cases are almost awfully different; for—

1*st.* The Great Pyramid was built 4000 years ago, in the symbolic ages of the world, according to many indications in Biblical history, and we have no other guide for those very early times.

2*d.* The Great Pyramid's whole mass and whole surface,—and those so huge as rightly to have procured it the title from all the nations of the world of "the Great Pyramid,"—are arranged so as to make two most colossal representations of fives; and have been beheld by nearly all peoples from the beginning of history; and—

3*d.* Compared to the above, either in size, entireness, or publicity —the few fives here and there inside the very modern Royal Institution building of Edinburgh, and in one part merely of which the Royal Society of Edinburgh meets and the fives are said to be chiefly found,—are only like the fives which any child may discover in a modern multiplication table; *i.e.*, they are small both in themselves and in proportion to the other numbers represented, and not dominant over the whole structure.

Arithmetical Means.

When the *Proceedings'* author attacks some of my stated results of Pyramid measures for the reason that they have been obtained by "the equivocal process of means" (p. 259), and insinuates against one particular final quantity—which is the mean of the largely varying results of four different observers,—that it is "an "exact measure which certainly none of its many measurers ever "yet found it (the thing itself measured) to be" (p. 256),—I can only hold up my hands in astonishment.

For here is a person criminating utterly the simple arithmetic process of taking arithmetical means in physical science; and bringing up the special accusation, equally applicable, if at all, in pure, as well as applied, arithmetic,—that the mean does not come out exactly the same as one of its component numbers.

This is surely something worse than advocating perpetual motion, spirit-rapping, or any other absurdity which would not be permitted to enter the books of the Royal Society of London: and indicates that the *Proceedings*' author is running a-muk, in the manner of certain of the Javanese, against the method of averaging employed in every scientific, commercial, and insurance establishment in every country throughout the civilized world. And if the Royal Society of Edinburgh has permitted such sentiments to be uttered at their meeting and printed in their *Proceedings*,—I would beg them to explain, through their Council or otherwise,—if they believe the sentiments,—how they maintain them; and if they do not believe them,—why they give them honourable publicity, and against the Pyramid subject alone?

(In the meanwhile* that there may be no doubt of the fact as held by the *Proceedings*' author, I subjoin, in a note,† an extract from one of the daily papers of April 21,—expressing the gentleman's sentiments as delivered to the Royal Society of Edinburgh, on April 20, more fully; and I hope completely relieving me of any necessity for answering him on the point.)

MINUTENESS OF MODERN PRACTICAL STANDARDS OR GAUGES.

Under the above heading the *Proceedings*' author has tabled some

* This paragraph in parenthesis, was added after the reading of the paper.

† "It was an erroneous method of procedure to take the mean of different "measurements. Such a method of procedure Sir J. Y. Simpson alleged "was childish: it was a species of mathematical aberration, and it ran "through the whole of Professor Smyth's book."—*Scotsman* of April 21.

objections to results deduced from the Great Pyramid,—but the objections are founded on such erroneous principles of practical science, that I notice them only for the sake of the Royal Society of Edinburgh, in whose publication they now appear.

The first objection is, that the theoretical and actual sizes of the supposed *standards* of the Pyramid "are made to vary in "different books, *which it is impossible for an actual standard* to do." (p. 265.)

Yet it is a notorious fact that the theoretical and recorded sizes of the actual national standards do vary in different books and even sometimes in the same book; and this, whether we take the pages of micrometric measures for the length of our standard yard from the Philosophical Transactions, or the Memoirs of the Royal Astronomical Society, and compare these with the different ideas of the *savants* of various countries, at different times within the last 100 years, as to the true length of a pendulum vibrating seconds in a particular position on the earth, (the British yard's theoretical reference); or whether we enter into the conflicting ideas of the French nation, at successive epochs, as to the real length of a ten-millionth of the earth's meridian quadrant, and its proportion to their standard metre, at different temperatures.

Thanks to the exertions of the many great men who have laboured splendidly in the cause both of the theory and practice of such standards,—the differences of the modern standards alluded to, are now reduced within very narrow limits. But differences to some extent there are still, and will be for ever, according to the nature of things, touching all practical standards, their theoretical references, and every subsequent, additional determination of the numerical value of the same,—if pushed to sufficient refinement, and whether by one or many observations.

The more man strives after practical perfection, the more impossible it is found to attain to it. An uneducated countryman thinks he has the time *perfectly*, if he knows it to the nearest minute. But an astronomer, though he obtains the time by observations to a fraction of a second, yet always finds his results attended by some still smaller fractional portion of error. Again, a carpenter compares two specimens of three-foot rules, and if they agree within half of $\frac{1}{16}$th of an inch, he at once pronounces them perfect: but a learned man who measures their respective lengths to $\frac{1}{10000}$ of $\frac{1}{10}$th of an inch, can seldom or never reach perfect decision in his last place of figures; and finds anomalies of expansion, imperfections of the eye, or the touch, plasticity of the hardest metals, and all the powers of nature interfering between him and his proposed goal of perfect accuracy.

Well therefore has one of the best men whom Cambridge has sent forth within this century, summed up these ruling facts of all the higher natural philosophy,—by stating "that no human "hand or machine ever yet drew a straight line, or erected a per-"pendicular, or fixed an instrument in perfect adjustment." And in truth it is an attribute higher than man's, to perform any practical mechanical operation with perfect exactness.

To acknowledge this fact and work within it,—tends to lead men on to that old and beautiful confession of Sir Isaac Newton, *i.e.*, that all that even he had been able to do, through a long life spent in scientific observation,—was, to pick up a few pebbles on the shore of the boundless ocean of knowledge. It tends, in fact to keep man in his proper place, in humility of mind, but in earnest contemplation of a high ideal, and in efforts always to rise above his last endeavour,—assured that there is hope for some further improvement, when the distance between himself and ideal perfection is infinite.

But to deny the fact of man's incapacity for perfect performance of problems in practical science, and to refuse its evidences—is either to exhibit an uneducated ignorance of what accuracy is,—or to endeavour to apply to the creature that which is an attribute of the Creator.

Hence, when the *Proceedings'* author (at p. 265) lays it down, that
"Measures, to be true counterparts, must, in mathematics, be "not simply 'near' or 'very near,' which is all that is generally "and vaguely claimed for the supposed pyramidal proofs; but "they must be entirely and *exactly* alike, which the pyramidal "proofs fail altogether in being. Mathematical measurements of "lines, sizes, angles, &c., imply exactitude and not mere approxi-"mation; and without that exactitude they are not mathema-"tical, ——"
we may well ask, Is the gentleman showing simple ignorance of

the difference between pure mathematics and applied? or, Is he unjustly and knowingly stating against the Pyramid's deduced standards, as an acknowledged metrological fact, that of which he knows the very contrary has been found to prevail by all those men who have devoted themselves specially to the most accurate possible mensurations.

Into this nice question I shall not attempt to penetrate,—but will pass on to a statement at the foot of the same *Proceedings'* page, asserting that "our real *practical* standard measures are infinitely "more refined and many thousand-fold more delicate than any in-"definite and equivocal measures alleged to be found in the Py-"ramid by even those who are most enthusiastic in the pyramidal "metrological theory."

This is clearly stated there, and then its writer brings forward as proofs of that *infinitely* superior accuracy, refinement, and delicacy, the thickness-gauges of Mr Whitworth.

Now these gauges are no doubt very admirable things in their way and in their own place; and as they range generally from 0·2 to 6·0 inches in diameter, they are exceedingly useful for testing the thickness of iron rods and plates in engineers' workshops. But they are not yet employed by the Government as the standard length of the country; they would form also a most awkward shape for a standard to refer to, in measuring any long length on the surface of the earth; and from their shortness, would require to be multiplied so often as to lead to serious chances of error; while finally comes the question, are they adapted to last?

A national or scientific standard ought to hand down the national measure unchanged for thousands of years. But will so oxidizable a substance as iron, and this is what Mr Whitworth employs, be safe through such long ages? When so little of the iron of the Greeks and Romans, and less of the Egyptians, has come down to our time—grave fears may be entertained for the Manchester iron plugs and sockets.

Now the standard of the Great Pyramid, on the contrary, comes before us with the proof on its brow of *having* stood that test, which Mr Whitworth's thickness gauges have not yet gone through; —viz., of having lasted before the world, and in spite of conquering and insulting nations, for 4000 years. In spite too of conquerors who did everything they could to annoy and crush the patriotic spirit of the Egyptians, by ransacking their tombs, polluting their temples, overturning their statues. And certainly, if by leaving out to rust in the dewy night-air a few iron gauges, the Persian Cambyses could have spoiled, nay destroyed, the Egyptian national measure of length,—would he not most assuredly have done so?

This *first* point of comparison then, is no problematical, but a certain and proved, superiority of the Great Pyramid over any little iron gauges for national standards. And a *second* similar point is, that taking account of all the various lengths which man has to measure, and the importance for accuracy of having to multiply the standard as little as possible,— a length like a side of the Great Pyramid base, of more than 9000 inches, and that in a material preserved by nature at a practically equal temperature and constant length,—must be preferable to something that a man might put into his waistcoat pocket; but have proportionally large temperature and other corrections to apply to it.

In the *third* place, it is not right to say that our modern practical standards are many thousandfold more delicate, and *infinitely* more refined, than those of the Great Pyramid. For while some of the modern standards, when they have passed out of the hands of their makers, have been found on testing after a few years to be from $\frac{1}{10000}$th to $\frac{1}{1000000}$th wrong, and while the ordinary divided scales in opticians' shops may be found from $\frac{1}{300}$ to $\frac{1}{5000}$th wrong,—the base-side of the Great Pyramid, according to the description of its masonry by Colonel Howard Vyse (see note p. 325), must have admitted of measure to less than 0·01 of an inch at either end, or equivalent to $\frac{1}{500000}$th of the whole; *i.e.* if, in all justice, the same refined means of now making the measurements were to be employed there, as are so freely spent on our ordinary modern standards at home.

Evidently, in fairness, either the Great Pyramid's base-side should be as carefully measured, and with reference to the state in which its builders left it,—as are our modern standards with regard to their makers; or the modern standards should be treated as the Pyramid's base-side has been hitherto, and have a few thousand tons of rubbish accumulated over them; in which case who could even guess what size they might or might not be. In either case the powers of the Great Pyramid's base-side, for accuracy and lasting quality combined, would probably be found vastly above those of any standard of measure at present known to be possessed by any nation in Christendom.

PYRAMID ALLEGED TO BE A SUPERHUMAN, AND MORE OR LESS AN INSPIRED, METROLOGICAL ERECTION.

To the above very serious title the *Proceedings'* author arrives in the last section of his paper (p. 268); and seems to decide that Khaliph Al Mamoon by breaking in, "upset the supposititious "miracle," (of the Great Pyramid, as begun to be interpreted by the late John Taylor), "a thousand years ago;" also, that something else was recently discovered in the building, "some thou-"sand years too late for the evolution of the alleged miraculous "arrangement;" and finally, that Professor Smyth has been "wrongously and disparagingly attributing to Divine Inspiration "and aid, things that are imperfect and human."

When from nearly first to last, in that over remarkable paper in the *Proceedings* (the second of the two papers on which I have been discoursing this evening),* there has now been shown proof that it is characterised by erroneous statements, perverted meanings, unworthy insinuations, and denials of some of the most commonly known principles of arithmetic, and most widely followed methods of physical science,—the members of the Royal Society may see, that there is no possibility of my discussing the Pyramid question generally with that gentleman on *scientific* grounds.

As little can I attempt to do so on a *religious* foundation. Allusion indeed to all religious argument the Society ordinarily eschews, though the *Proceedings'* author was allowed to introduce it into his pages 267 and 268, besides inserting several rather scoffing and derisive passages at pp. 254, 256, 260, and 264. But even though the Society should give me equal liberty to answer,—no discussion of a peaceful, improving kind can be instituted without some first

* The present essay, especially in its latter half, was much shortened on the occasion of reading before the Society on April 20, in order to bring it within the limits of time allowed. All the leading remarks of it were, however, noted, and are now again brought forth with fuller materials for proof or disproof.

principles being agreed on. And as the rationalistic doctrines, and *a priori* theories laid down by the *Proceedings'* author, are totally opposed to my inductive gatherings of what *are* the characteristics of the only generally admitted example of Divine Inspiration, viz., the Sacred Scriptures,—I decline to argue that part of the question with that gentleman at all.

To the Society, therefore, only would I appeal,—after what they have already printed and published,—earnestly inquiring of them why they patronize arguments which, if of force against the supposed primeval, and pre-Biblical, Inspiration of the design of the Great Pyramid, are of equal force against the Bible itself?

How, for instance, can it be justly maintained by those who hold to the Bible, that Al Mamoon's breaking into the Great Pyramid, and exercising a power over it, to injure it,—proves the structure *not* to have been built originally by Divine Inspiration,—when the Bible relates that the Temple of Solomon, though actually so built, or according to plans furnished in number and measure by Divine Inspiration, was nevertheless destroyed by the Babylonians, and its sacred vessels carried away, to be used in the service of idols; and now, together with the Ark of the Covenant, they have vanished from the surface of the earth?

Does any one venture to assert that Al Mamoon "upset the "miracle of the Great Pyramid by, as some say, unveiling, a thou-"sand years before the appointed time, the contents of the King's "Chamber?" Let such persons first prove that Al Mamoon did unveil, or even see, any of *those* contents which are supposed to have been above the powers of unassisted men to have constructed intentionally at the date of the Pyramid's foundation,—viz., the earth and heaven commensurabilities of the Pyramid's several parts. These proportions have only begun to be thus recognized, so far as I know, within the last 10 years; and Al Mamoon no more saw them or published them to the world, when he broke into the Pyramid by brute violence in 830 A.D.,—than did Pompey, when he burst into the Holy of Holies in Jerusalem, see that presence into which he would have forced himself impiously.

Does the critic object,—that modern men find such exceeding difficulty in trying to ascertain the exact lengths and breadths of the various symbolical parts of the Pyramid,—that they settle, and consider themselves entitled to settle thereby, that the structure could not have had an all-wise prompter, planner, or designer;—let the critic confess whether there are not many words and passages both in the Hebrew and Greek Scriptures, on which neither divines nor scholars have yet been able to come to a full understanding, and where they are still bringing forth their various readings!

Or, because the Great Pyramid has not been successfully interpreted by mankind during so long a period as 4000 years; or because, during all that space of time its presumed mission has not been accomplished in the world, and so many centuries have passed away fruitlessly since the seed was sown, in an age before any human intellectual history began,—shall that be considered a good *Biblical* reason against there having been Inspiration at the beginning, and against a Divinely intended result still to appear,—seeing that the Bible has announced many prophecies, which are not yet fulfilled; and tells us repeatedly of mysteries which were prepared for, from the very beginning of the world, and yet only made manifest in latter times?

But I will not attempt to follow any further, or to set in order, the arguments of which there are so many, connected with Revealed religion;—for I am too much grieved and humbled to find, that the most conscientious labours and the justest deductions that I have been working at for years past, and whereby I had hoped to be instrumental in affording to some other earnest thinkers a ray of light on a sacred, as well as a scientific, topic,—have issued before the Society merely in this, that my labours, their subject, and my friend's religious convictions have all been dragged contemptuously in the dirt, and made a mockery of, in the fearful manner accomplished by the *Proceedings'* author.

Of *Revealed* Religion therefore I will not presume to say more in this assembly: and I hope pardon may be extended to me for what I have said, if imperfectly, or mistakenly, yet with the best intentions.

But I would beg, before concluding, to remind the members, one and all, of a remark by the notable Kepler, bearing on *Natural* Theology. The existing state of our Great Pyramid knowledge is very similar to what prevailed with regard to astronomy in the time of that acutest genius of mediæval Germany; for, observations were then rough, and rude hypotheses succeeded each other as found from time to time best adapted to explain all the known facts, and were not yet worked up into a complete theory.

So many failures had at that time already taken place, that when success did come at last, hardly any one would believe Kepler's brilliant discovery of the elliptical motion of the planets. His friends would hardly allow that he was sound in his mind; and yet his discovery proved the morning star of the epoch of the Law of Gravitation. They ridiculed and despised: but thus he comforted himself in a knowledge of the reality, and a presentiment of the importance, of what he had learned; and with an intellectual enthusiasm, not often seen to an equal degree in the present day of a more delicate education, he thus breathes forth his noble spirit:—

"Nothing holds me. I will indulge my sacred fury! If you "forgive me, I rejoice; if you are angry, I can bear it. The die is "cast. The book is written, to be read either now, or by posterity. "I care not which. It may well wait a century for a reader, since "God has waited 6000 years for an observer!"

Not more than 4000 years has the solution of the mystery of the Great Pyramid waited; but yet what an interval is that, as measured by nations and peoples, and languages and tongues. Through all that mighty period, what other building than the Great Pyramid has brought down to us in the present day a symbolical language of forms, and accurate details capable of still giving forth exact, contemporary, and high scientific information of that early time?

Surely the Great Pyramid building is unique in its majesty. Take it all in all, it is solemnly alone!

The second author in the Society's *Proceedings* referred to does, indeed, indicate in his very last sentence, p. 268, that he knows of just as good traces of equally advanced "architecture and astro- "nomy" belonging to the same age, "*in various parts of the* "*East*." I can only hope that the Council will examine him on that point;—for, according to the best of my researches among the writings of those who have explored the Eastern lands—no such examples have there been found.

CONTENTS

Of the Preceding Paper, entitled "Remarks on two Pyramid Papers in the last published Number of the Proceedings of the Royal Society, Edinburgh."

3. On the Products of the Destructive Distillation of Animal Substances. Part V. Action of Sodium on Pyridine. By Professor ANDERSON, Glasgow.

4. On the Structure of the British Nemerteans; and new British Annelids. By W. Carmichael M'Intosh, M.D. F.L.S.

In the first part of this paper the anatomy and physiology of the Ommatopleans is described from the typical form *O. alba*, the variations and peculiarities presented by other genera and species being contrasted therewith. The descriptions are grouped under the following heads:—Dermal Tissues; Proboscidian Sheath; Proboscis; Digestive, Circulatory, and Nervous Systems; Cephalic Furrows, Pits, and Glands; Organs of Reproduction and Development.

The second part consists of the structure and physiology of the Borlasians, under the same (or similar) heads; together with such anomalous genera as differ from both great groups of Nemerteans.

The third division treats chiefly of the structure of certain (upwards of forty) Annelids new to science or to Britain.

The following Gentlemen were elected Fellows of the Society:—

Rev. THOMAS GUTHRIE, D.D.
THOMAS KEY, Esq., Dep.-Inspector Gen. of Hospitals.
ADAM GILLIES SMITH, Esq., C.A.
JOHN MACMILLAN, Esq., M.A.

The following Donations to the Library were announced:—

Boeck (Thorvald). Oversight over Literattur, Love, Forordninger Rescripter M.M. Vedrorende de Norske Fiskeriar. Christiania, 1866. 8vo.—*From the Author.*

Broch (Dr O. J.). Traité Elémentaire des Fouchons Elliptiques. Fasc. 1. Christiania, 1866. 8vo.—*From the Author.*

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Caspari (Dr C. P.). Ungedruckte, Unbeachtete und Wenig Beachtete Quellen zur Geschichte des Taufsymbols und der Glaubensregel. Christiania, 1866. 8vo.—*From the Author.*

Catalogue of the Printed Books in the Library of the Faculty of Advocates. Part III. Edinburgh, 1867. 4to.—*From the Faculty.*

Catalogue of Contributions transmitted from British Guiana to the Paris Universal Exhibition. London, 1867. 8vo.—*From the Committee of Correspondence of British Guiana.*

Catalogue of the Surgical Section of the United States Army Medical Museum. Washington, 1866. 4to.—*From the American Government.*

Chevreul (M. E.). De la Baguette Divinatoire du pendule dit Explorateur et des Tables Tournantes au point de vue de l'Histoire, de la Critique et de la Méthode Expérimentale. Paris, 1854. 8vo.—*From the Author.*

——— Notes Historiques sur la nature immédiate de l'Amer de Welter et de l'Amer au Minimum. Paris, 1864. 4to.—*From the Author.*

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Crisp (Edwards) M.D. On some Points connected with the Anatomy of the Hippopotamus. 8vo.—*From the Author.*

Cunningham (Alex. W.). Notes on the History, Methods, and Technological Importance of Descriptive Geometry. Edinburgh, 1868. 8vo.—*From the Author.*

Danube,—Mémoire sur les Travaux d'Amelioration exécutés aux Embouchures du Danube, par la Commission Européene (4to). accompagné d'un Atlas de 40 Planches (Fol.) 1867.—*From the Commission.*

Fayrer (Joseph) M.D. Address delivered at the Annual Meeting of the Asiatic Society of Bengal. Calcutta, 1868. 8vo.—*From the Author.*

Flora Batava, afbeelding en beschrigving van Nederlandsche Gewassen door Wijlen Jan Kops, vervolgd door Jhr. F. A. Hartsen, afgebeeld onder opzigt van J. C. Sepp en Zoon. No. 200–203. Amsterdam. 4to.—*From the Authors.*

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Holmboe (C. A.). Ezechiels Syner og Chaldæernes Astrolab. Christiania, 1866. 4to.—*From the Author.*

Journal (American) of Science and Arts, conducted by Benjamin Silliman. Nos. 132, 133, and 134. New Haven. 8vo.—*From the Editor.*

Lawson's Pinetum Britannicum. Parts XXVII. and XXVIII. Folio.—*From Charles Lawson, Esq.*

Leuckart (Rudolf). Die Menschlichen parasiten und die von Ihnen Herrührenden Krankheiten. Ein Hand und Lehrbuch für Naturforscher und Aerzte. Band II. Lief. 2. Leipzig, 1868. 8vo.—*From the Author.*

——— Die Menschlichen Parasiten und die von Ihnen Herrührenden Krankheiten. Leipzig, 1868. 8vo. — *From the Author.*

Manuscripts. Facsimiles of National Manuscripts of England, selected under the direction of Colonel Sir Henry James. Part III. Fol.—*From the Secretary of State for War.*

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——— Tenth detailed Annual Report of the Registrar-General of Births, Deaths, and Marriages in Scotland. Edinburgh, 1867. 8vo.—*From the Registrar-General.*

——— On the Amputations at the Hip-Joint in Military Surgery. Washington, 1867. 4to.—*From the American Government.*

Rutherford (William), M.D. Electronus: A Physiological Demonstration given in the Physiological Laboratory of the University of Edinburgh. 8vo.—*From the Author.*

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Smart (Andrew), M.D., F.R.C.P.E. Reports to the Lord Provost and Magistrates of the City of Edinburgh, on the Pathological Appearances, Symptoms, Treatment, and Means of Preventing Cattle Plague. Edinburgh, 1866. 4to.—*From the Author.*

Sproat (Gilbert Malcolm). Scenes and Studies of Savage Life. London, 1868. 8vo.—*From the Author.*

Sundt (Gilert). Om Sædeligheds-Tilftanden i Worge 3 die, Beretning. Christiania, 1866. 8vo.—*From the Author.*

TRANSACTIONS AND PROCEEDINGS OF SOCIETIES, ACADEMIES, &c.

Amsterdam.—Jaarboek van de Koninklijke Akademie van Wetenschappen gevestigd. 1866. 8vo.—*From the Academy.*

Rapport fait à l'Académie Royale des Sciences des Pays-Bas, Section Physique. 8vo.—*From the Academy.*

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Verhandlungen der Naturforschenden Gesellschaft in Basel. Ersted Theil, Zweiter Theil, Dritter Theil, Vierter Theil. 8vo.—*From the Society.*

Berlin.—Abhandlungen der Königlichen Akademie der Wissenschaften. 1866. 4to.—*From the Academy.*

Monatsbericht der Königliche Preussischen Akademie der Wissenschaften. August, October, November, December, 1867. 8vo.—*From the Academy.*

Berne.—Mittheilungen der Naturforschenden Gesellschaft in Bern, No. 603–618. 8vo.—*From the Society.*

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Annales de l'Observatoire Royale. Tome XVIII. 4to.—*From the Observatory.*

Brussels.—Annuaire de l'Observatoire Royale. 1868. 12mo.—*From the Observatory.*

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Proceedings of the Asiatic Society of Bengal. Nos. 8–11. 1867. 8vo.—*From the Society.*

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Frankfort-on-Maine.—Tageblatt der 41 Versammlung Deutscher Naturforscher und Aerzte. 1867. 4to.

Glasgow.—Proceedings of the Philosophical Society. Vol. III. Nos. 5 and 6; Vol. IV., Vol. V., and Vol. VI., Nos. 1–3. 8vo.—*From the Society.*

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Rendiconto delle Tornate e dei Savori dell' Accademia di Scienze Morali e Politiche. Jan. 1868. 8vo.—*From the Academy.*

Neuchatel.—Actes de la Société Hélvétique des Sciences Naturelles. Compte-Rendu, 1866. 8vo.—*From the Society.*

Bulletin de la Société des Sciences Naturelles de Neuchatel. Tome VII. 8vo.—*From the Society.*

Paris.—Annales des Mines. Tome XI. Liv. 2^e, 3^e. Tome XII. Liv. 4^e, 5^e. 8vo.—*From the Ecole de Mines.*

Publications of the Dépot de la Marine. Nos. 408 to 439. With Charts. 8vo.—*From the Dépot.*

Bulletin de la Société de Géographie. Nov. Dec. 1867, January 1868. 8vo.—*From the Society.*

Bulletin de la Société de Géographie. Février, Mars, 1868. 8vo.—*From the Society.*

Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. 1867-68. 4to.—*From the Academy.*

Rotterdam.—Nieueve Verhandelingen van het Bataafsch Genootschap der Proefondervindelijke Wijsbegeerte. Deel I. Stuk 1–3. 4to.—*From the Society.*

Torino.—Atti della Reale Accademia delle Scienze. Vol. II. Disp. 4–7. 8vo.—*From the Academy.*

Bullettino Meteorologico ed Astronomico del Regio Osservatorio dell' Università di Torino. 1867. 4to.—*From the University.*

Memorie della Reale Accademia delle Scienze. Serie Seconda, Tomo XXIII. 4to.—*From the Academy.*

Truro.—Journal of the Royal Institution of Cornwall, with the 49th Annual Report. No. VII. 8vo.—*From the Institution.*

Utrecht.—Nederlandsch Meteorologisch Jaarboek voor 1866–67. 4to.—*From the Meteorological Institute.*

Aanteekeningen van het verhandelde in de Sectie-vergaderingen, van het provinciaal Utrechtsch Genootschap van Kunsten en Wetenschappen. 1867. 8vo.—*From the Society.*

Natuurkundige Verhandelingen uitgegeven door het Provinciaal Utrechtsch Genootschap van Kunsten en Wetenschappen. Deel I. Stuk 1. 4to.—*From the Society.*

Verslag van het Verhandelde in de Algemeene vergadering van het provinciaal Utrechtsch Genootschap van Kunsten en Wetenschappen. 1867. 8vo.—*From the Society.*

Venezia.—Atti del Reale Istituto Veneto di Scienze, Lettere ed Arti. Tomo XII. Dispensa 4–9. 8vo.—*From the Institute.*

Vienna.—Almanach der Kaiserlichen Akademie der Wissenschaften. 1867. 8vo.—*From the Academy.*

Denkschriften der Kaiserlichen Akademie der Wissenschaften. Mathematisch Nat. Classe, Band XXVI.; Philosophisch. Hist. Classe, Band XV. 4to.—*From the Academy.*

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften. Phil. Hist. Classe, Band LIV. LV. LVI. Heft 1, 2; Mat. Nat. Classe, Band LV. LVI. Heft 1, 2; Mineralogie, Botanik, etc., Band LIV. Heft 4, 5; Band LV. LVI. Heft 1. 8vo.—*From the Academy.*

Vienna.—Jahrbuch der Kaiserlich-Königlichen Geologischen Reichsanstalt. Band XVII. Nr 3. 8vo.—*From the Society.*

Verhandlungen der Kaiserlich-Königlichen, Geologischen Reichsanstalt. 1867. Nos. 10, 11. 8vo.—*From the Society.*

Whitby.—Forty-Fifth Report of the Literary and Philosophical Society. 8vo.—*From the Society.*

Zurich.—Neue Denkschriften der Allgemeinen Schweizerischen Gesellschaft für die gesammten Naturwissenschaften. [Nouveaux Mémoires de la Société Helvétique des Sciences Naturelles.] Band XXII., mit XX. Tafeln. 4to.—*From the Society.*

Unger (C. R.) Morkinskinna. Pergamentsbog fra forste Halvdel af det Trettende Aarhundrede. Christiania, 1867. 8vo.—*From the Author.*

Waage (P.) et C. M. Guldberg. Etudes sur les Affinités Chimiques. Christiania 1867. 4to.—*From the Authors.*

Will (H). Jahresbericht über die Fortschritte der Chemie, etc. Für 1866. Heft 1–3. Giessen. 8vo.—*From the Editor.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VI. 1868-69. No. 77.

EIGHTY SIXTH SESSION.

Monday, 23d November 1868.

DAVID MILNE-HOME, Esq., Vice-President, in the Chair.

The following Council were elected:—

President.
PROFESSOR CHRISTISON, M.D.

Honorary Vice-Presidents.
HIS GRACE THE DUKE OF ARGYLL.
JAMES D. FORBES, Esq., LL.D.

Vice-Presidents.

Professor C. INNES.
Prof. LYON PLAYFAIR, C.B.
D. MILNE HOME, Esq.
Professor KELLAND.
The Hon. Lord NEAVES.
Professor Sir WILLIAM THOMPSON.

General Secretary—Dr JOHN HUTTON BALFOUR.

Secretaries to the Ordinary Meetings.
Dr GEORGE JAMES ALLMAN.
Professor TAIT.

Treasurer—DAVID SMITH, Esq.
Curator of Library and Museum—Dr DOUGLAS MACLAGAN.

Councillors.

Dr JOHN MUIR.
Rev. THOMAS BROWN.
JAMES SANDERSON, Esq.
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GEORGE ROBERTSON, Esq., C.E.
Professor PIAZZI SMYTH.
PATRICK DUDGEON, Esq. of Cargen.
Dr HUGH CLEGHORN.
W. DITTMAR, Esq.
JAMES M'BAIN, Esq., Surgeon, R.N.
Dr WILLIAM ROBERTSON.
THOMAS STEVENSON, Esq., C.E.

Monday, 7th December 1868.

Professor Christison, the President, delivered the following Opening Address:—

Gentlemen,—It is now nearly five-and-forty years since I first opened my lips in this Society, a venturous young man, undertaking to instruct both practical men and men of science, as well as the public at large, all at the time keenly interested in the inquiry,—What were the principles for properly constructing burners for combustion of the light-giving gases? What the relative values of oil-gas and coal-gas for giving out light, and which of the two should thenceforth be used for illuminating the world? At that time it was assuredly the farthest thing of all from my dreams that a period might come round when, by the voices of my Fellow-Members, I should be promoted to an office, the highest in learning or science, as the case may be, which Scotland can offer to their votaries,—held last by Sir David Brewster, and previously filled by the Duke of Argyll, Sir Thomas Brisbane, Sir Walter Scott, and Sir James Hall.

All these eminent men have been Presidents of the Royal Society of Edinburgh during the period of my Fellowship, except the last: whom, however, I often met in the literary and scientific circles of which my father was a member. When I recall the remarkable services of these five men to learning and science; their contributions to the meetings and Transactions of this Society; their bearing among its Fellows, and the universal homage paid to them as our official heads,—I am apt to ask myself why it is that I am now in the place previously adorned by such men as these? To this question I have but one answer, and it may be an inadequate one. The office was not of my seeking. It was not within the range of my hopes, or even of my thoughts. But when my appointment to the Presidentship of the Society was spontaneously recommended to the Society by the Council, I felt that I could not but bow to their decision, as being the persons best qualified to know what is most for the advantage of the Society, and having undoubtedly reasons, sufficient to their own minds, for placing in me, for the due discharge of the duties of your President, a confidence which

I can truthfully say I do not myself entertain. And nevertheless, having undertaken these duties, and possessing some knowledge of what they are, I must hope to discharge them to your satisfaction, with the help of a willing mind, a grateful heart, and your indulgence.

While pondering over these reflections, it occurred to me to inquire whether any mark had been left on the work of this Society by my several predecessors in office. It was, I confess, some comfort to me to find that at all periods the work done in the Society depended more on the Vice-Presidents, the Secretaries, and the other members of Council, as well as the Fellows at large, than upon your President for the time being; who may have felt, perhaps, that, having generally done good work previously in an inferior capacity, he might be allowed to repose a little on his dignity, and transfer much of the arduous labour of literary and scientific research to those enjoying the inestimable advantages of greater vigour of life, and a younger enthusiasm.

In the course of my searches into this matter, I had to go farther back than my personal recollections, even to the Presidency of Henry, Duke of Buccleuch, and thus was imperceptibly led into the midst of the earliest proceedings of the Soeiety, when it first started into existence in 1783 as the Royal Society of Edinburgh, under the leadership of that distinguished nobleman. The study of the Proceedings and Transactions of the Society during his Presidentship, which lasted for the long term of twenty-eight years till his death in 1811, seized upon me with a fascination which no one can thoroughly understand without following my example. But it has seemed to me that I might convey some portion of the pleasure of that retrospect were I to summarise and cull the interesting and very diverse materials thus presented to view, and offer you, as my Address on the present occasion, a history of some portion at least of the Life of the Royal Society of Edinburgh.

The History of our Society has never yet been written. But it is a duty owing to our predecessors, who earned for it a great reputation, that some time soon such a history should be written. The task, however, is one which will need the labour of several hands—the materials being, as already said, so very multifarious.

All I can pretend to do, with the small ability and little leisure of which I can avail myself, is to attempt, in a desultory way, as much at present as my time admits of my overtaking, and yours of your listening to. But I have some hope, nevertheless, that so much may be said as to induce my able coadjutors, who annually in succession address you from the Chair, to look also a little into the materials I have been examining, and to show, by each taking up that branch of our history which is most allied to his pursuits and congenial to his inclinations, the various branches of learning and science on which our Society has more or less shed its rays during the last eighty-five years,—the most eventful era, probably, that has occurred in the literary and scientific history of this nation.

It is scarcely necessary to remind you that the Royal Society of Edinburgh was created mainly through the influence of Principal Robertson, from a pre-existing association of the most illustrious Scotsmen of the time, called the Philosophical Society; which again had arisen, through the exertions of Colin Maclaurin, from a combination of learned and scientific men who published between 1731 and 1739 the "Medical Essays and Observations."

The Royal Society, in its early years, was chiefly composed of the following remarkable galaxy of genius and talent:—Between 1783 and 1805 we find, in the branch of *Literature*, and most of them taking their share in the business of the Society, the names of Principal Robertson, the Rev. Drs Hugh Blair, Carlyle, and Henry; Professors Hill, Dalzel, Ferguson and Fraser-Tytler of Edinburgh, Beatty of Aberdeen, Hunter of St Andrews, and Young of Glasgow; Dr Doig of Stirling, Henry Mackenzie, and William Smellie. In *Philosophy*—Adam Smith, Thomas Reid, Dugald Stewart, and John Bruce. In *Mathematics* and *Physics*—Professors Matthew Stewart, John Robison, Robert Blair, John Playfair, John Leslie, and William Wallace, of Edinburgh; John Anderson of Glasgow, and James Ivory. In *Chemistry*—Black, Rutherford, Hope, Roebuck, and Lord Dundonald. In the *Natural Sciences*—Professors Walker and John Hope, Hutton, Playfair, and Colonel Imray. In *Medicine*—Cullen, the second Monro, James Gregory, Francis Home, Andrew Duncan, Sir Gilbert Blane, and Benjamin Bell; and to these must be added a host of conspicuous lawyers and country gentlemen of the time, all more or less conversant with literary or scientific pursuits, among whom the most conspicuous were Lord-President Dundas, Lord Justice-Clerk Miller, Lords Meadowbank and Glenlee, Barons Montgomery, Norton, and Hume, Sir Henry Campbell, Sir James Hall of Dunglass, Sir George Clerk Maxwell of Penicuick, Sir Alexander Dick of Prestonfield, Sir George Mackenzie of Coull, Sir James Hunter Blair of Dunskey, Sir William Forbes of Pitsligo, and John Clerk of Eldin.

It will be acknowledged to be difficult to discover in any country, and at any period, united in a single association, sixty names more remarkable than these for the impression they have made on the peaceful part of the history of a nation.

The medical element of the Society was in those days peculiarly rich in great names. This was the element, too, from which the Society originally sprung. We should therefore naturally expect medicine to flourish in a Society descended from the medical essayists of the middle of last century. But medicine makes only a rare, and for the most part insignificant appearance in the business of the Royal Society. It is a matter of curiosity, however, to see how, even so far down as 1792, when very much smothered by literature and pure science, medicine still sometimes cropped out in naked simplicity. Among the early papers read in the Society, Dr Hope describes a disagreeable case of death from impaction of a gall-stone in the bile-ducts; Dr Butter of London proclaims hemlock to be a sovereign cure for St Vitus's dance; Dr Duncan intimates that he had cured an inveterate hiccup with a single dose of diluted sulphuric acid; at a later period, Mr James Russell, afterwards Professor Russell, a well-known colleague, communicates a singular case of hernia, and Dr Francis Home treats of the disease Amaurosis. If this be all which medicine could do in its most palmy days in Edinburgh to hold up its head in the Royal Society, I confess it is not a subject of regret that, by gradual and tacit consent, papers on pure medical practice have been allowed to drop from our Proceedings. For assuredly there is nothing at all so remarkable or peculiarly instructive in death from an impacted gall-stone, or from any form of hernia, as to deserve being recorded in the Proceedings of a Royal Society; nor would I advise patient or physician to trust much either to Dr Butter's cure for St Vitus's dance, or to the remedy which seemed to Dr Duncan to put an end to inveterate hiccup. The first volume of our Transactions, however, contains one able paper on a subject of pure practical medicine—a treatise by an eminent English physician of last century, Dr Hamilton of Lynn-Regis, upon the Mumps, or Cynanche parotidæa, a disease which had previously been little studied in England. But this treatise was originally produced to the Philosophical Society in 1773, and the Royal Society, deeming it worthy of publication, printed it in the Transactions.

In the more congenial fundamental sciences of medicine, anatomy and physiology, the work done in the Society in its early years presents features of greater interest. Mr Blizard of London gives an account of an extra-uterine fœtus; Dr Monro describes a remarkable male monster; investigates the anatomy of hydrocephalus; inquires into the communications existing between the ventricles of the brain—a field in which he had long before been a famous discoverer; explains the action of those muscles which consist of oblique fibres; and narrates the results of experiments, among the earliest made in this country, in 1792, confirmatory of the immortal discovery by Galvani of galvanic, or, as it was then called, animal electricity. The investigations of Dr Francis Home on the relative strength of tonics, and his medical experiments on foxglove, read in the Society, but published in a separate treatise, show that there is another fundamental branch of medicine proper—therapeutics, which may be cultivated, as a science, by physiological experiment, as well as by mere observation of empirical facts; and until this view be taken generally by physicians of the true method of studying the action of remedies, medicine will make but little progress in this the most important of all its departments.

There are other medical inquiries of interest to be found in the Society's early Proceedings and Transactions, such as Dr Alexander Wilson's paper on the action of opium on animals, and that of Dr James Johnston on the functions and diseases of the lymphatic glands. But I hasten from these and other inquiries, which might be usefully put in detail before a medical audience, to other topics of more general interest.

Before doing so, I will venture to touch for one moment on a rather delicate topic—the connection of the Society in its early days with theological subjects of discussion—because I find at that time an incident which might have helped the Council and the Society when a question was lately raised as to this matter. In 1791 and 1792 the Rev. Dr Ogilvy of Midmar read no less than three successive papers on the Theology of Plato, whereupon there occurs the following minute:—"The Society observed with regret that the discussion of a religious nature, contained in this learned communication, rendered an admission of it among their papers inconsistent with their plan; and therefore it was not put into the hands of the Committee for publication." If it was found necessary to discourage such discussions in 1790, it is not less so in these days, when religious differences are not less general, diverse, and keen. Appealed to on a recent occasion to state the law or usage of the Society on this subject, the Council had only one difficulty; which was to define what amounted to the nature of a theological communication. It would be clearly absurd, for example, to say that when a scientific observation or inquiry threw light upon a scripture fact, or some incident in scripture history, the author should be debarred from calling attention, in passing, to such a practical application of his inquiries. On the other hand, no Fellow of the Society would probably ever dream of introducing into it an express dissertation on a pure theological theme. Between these extremes there must be a limit which ought not to be crossed, and which it must be left in the first place to every man's discretion to observe; but if it be passed, I apprehend that it is still the duty of the Society, or its Council acting by its authority, to see that the offending communication, as of old, "be not put into the hands of the Committee for publication."

The first twenty years of the life of the Society is rich in literary inquiries. I venture to hope that some time soon, a literary Vice-President will undertake to tell us something of these communications, both on account of the information they contain, and for the sake of the light they throw upon the character, in point of learning, of their authors, who were among the most learned Scotsmen of the period. It appears that no fewer than thirty-eight papers on literary subjects were read at the Society's meetings, and are in general either summarised or published at length in the first six volumes of the Transactions. The most material are ten Grammatical papers by Dalzel, Hill, Hunter, Young, and Gregory, chiefly; one Etymological by Dr Jamieson; three Critical, on the Æneid, by Beatty, the character of Hamlet by Dr T. Robertson, and the German theatre, by Henry Mackenzie; three on Historical Composition by Hill and Fraser Tytler; three General Dissertations connected with the subject of poetry, by Dalzel and Hill; on the Standard of Taste, by Dalzel; on the Principles of Translation, by Fraser-Tytler; on the Argonautic Expedition, by the Rev. Mr Marshall of Cockpen; on the Origin of the Hellenes and of their name, by Dr Doig, of Stirling, who traces the Greeks to Chaldea; on Written Language as a Sign of Speech, by Dr Hutton; four Ethnological papers,—1. By Dr Gregory, on a Lacustrine Fort in Loch Urr, Kirkcudbrightshire; 2. By Colonel Montgomery, on an Ancient Sculptured Stone in Coilsfield; 3. By Fraser-Tytler, on the Vitrified Forts in the Highlands; 4. By Dr Anderson, on the Ancient Circular Buildings, or Houses of Scotland; and last, and one of the chief of all, the elaborate researches of Chevalier into the Plain of Troy, read by the author himself to the Society in 1791, and published *in extenso* in the French language, in the third volume of the Society's Transactions. I must leave to the literary members of your number to say, whether there is not, in these diversified papers, rare materials for such a historical summary of the work done at our meetings of old as I have indicated. I myself can only aim for the present at a brief allusion to the remarkable investigations of Chevalier.

Doubts have often been raised, and keen controversy has arisen among the learned, as to the reality of the existence of a Homer in poetry, an ancient Troy in geography, and a Trojan war in history. Chevalier, by his careful personal survey of the plain of Troy and its neighbourhood, contributed much to dispel these doubts, in so far as he ascertained that the leading incidents described in the Iliad fall in exactly with the remarkable structure of the Plain and surrounding country; thus furnishing internal evidence that the composer of the Iliad must have been intimately acquainted with the district where the Trojan war was carried on. It is only in the most recent times that writers of romance have thought it worth their while to know and portray the natural features of the places where the incidents they invent are represented to have occurred. It is a matter of doubt, whether even the most recent and scrupulous works of the kind could stand successfully so minute a topographical scrutiny as that to which the Iliad has been subjected by Chevalier and others since his time. But it is scarcely to be supposed that a composer of mere fiction, in so remote a period of antiquity and barbarism, and when intercommunication between countries was rare and difficult, would, as must be held by the advocates of the Iliad being a romance, undertake so troublesome and unnecessary a condition in those days, for a work of the imagination, as a minute personal study of a wide and complex field of strife, which he chose for the theatre of a romance. It is impossible, therefore, to deny that the researches of Chevalier added greatly to the pre-existing probability that Homer was an entity, and the siege of Troy an event, in real history.

The main features of Chevalier's *Tableau de la Plaine de Troye*, remain undisputed to the present day, save one, and rather an important one, the actual site of ancient Troy itself. Our late much-esteemed Fellow, Mr Charles Maclaren, without having visited the plain (though he did so subsequently), called in question the conclusion at which Chevalier arrived as to the position of the town for the possession of which Homer's heroes fought. The district may be briefly described as a plain about eight miles long and five miles in breadth, stretching from the westerly spurs of mount Ida to the Ægean Sea, bounded on the north and south by ridges of moderate elevation, and separated from the Ægean by a line of low heights, except at the mouth of the river Mindere or Simöis. Chevalier placed the site of ancient Troy near the south-east angle of the plain, in the lower region of the bounding ridge on the south, and describes traces of ancient walls in this locality, close to a modern Turkish village, Bounarbashi. But Maclaren points out that this position,—nine miles from the coast, where the Greeks were encamped, with their vessels drawn up on shore,—is much too great for the time allowed by Homer for certain incidents of the war which took place between the two limits, the town and the ships; and bringing to his aid the knowledge of a skilful geologist to explain changes, evident to such an eye, which in the course of ages may have taken place in the channels of the rivers and the structure of their banks since the time of the siege, he arrives at the result, that the site of the ancient town was the same with that of Strabo's Novum Ilium. There is a long tongue of wide, elevated land, steep on the sides, but not precipitous, which stretches from the lower heights of mount Ida for more than two miles westward into the very heart of the plain of Troy. The western terminal portion of this ridge was the site of Novum Ilium; the ground-plan of which is still indicated by remains of walls, and by such an accumulation of broken bricks and shattered porcelain all over the surface, that when friends of mine visited the place, while in the army medical service at Renkioi Hospital, during the late Turkish war, they found it a troublesome matter to measure the ground, by pacing through the covering of loose fragments. Now, it is scarcely to be conceived that, when the people of the country, represented by Homer to be well acquainted with the art of defensive fortification, determined to erect a fortified town for the protection of their territory, they would overlook this conspicuous spot, eminently suitable for all the conditions of refuge and defence in these early times, and would choose rather a remote corner of their land, leaving the whole cultivated plain between it and the sea open to sudden piratical incursions. But on other grounds than this Mr Maclaren has proved, to the satisfaction of all scholars who have studied the subject since, that Troy and Novum Ilium must have stood on nearly the same site. I may mention, as a new argument in support of that view, that Dr Kirk, one of the visitors referred to, found Chevalier's supposed foundations of ancient walls at Bounarbashi to be really the remains of trap-dykes.

So far Chevalier has been corrected. But the criticisms of Maclaren do not take away from the light which Chevalier was the first to throw over the most interesting of all events described as occurring in remote secular history.

Among the literary labours of the Society must be arranged a crowd of authentic biographies of the most eminent Scotsmen of the time, Fellows of the Society, written by men generally not less distinguished than those whom they have commemorated. This is a branch of the Proceedings of the Society which it is impossible to summarise. But I have alluded to it on account of a very surprising statement in the biography of Sir George Clerk Maxwell of Penicuick, father of our lately deceased Fellow, Sir George Clerk, which was read in 1784 by John Clerk, junior, of Eldin, afterwards Lord Eldin of the Scottish bench. The author says, Sir George "had an excellent taste for the fine arts, and was solicitous to encourage them. As one instance of this, he had the principal concern in establishing and procuring an endowment for the drawing-school in the University of Edinburgh, where twenty pupils are instructed *gratis* in the art of designing. These are selected from among such young people of either sex as give signs of genius, who are destined to apply to those professions in which a skill in that art is requisite. This institution has contributed more than any other circumstance to the great improvement of ornamental manufactory which this country has made during the last twenty years. And who ever recollects the old patterns of carpet, damask, gauze, and other manufactures of that sort, and compares them with those of the present day, must allow the superior elegance of design now exhibited in these productions, and which may be reasonably ascribed in a great measure to the happy effects produced by the institution we have mentioned."—*Trans*. i. 51.

It is impossible that Mr Clerk, himself an ardent admirer of art, and addressing a Society composed largely of Professors of the University, could be mistaken in making this precise statement. Nevertheless, I never heard of such a school in the University. There was most assuredly no such school in existence when I first joined it as a student in 1811. It cannot have merged in the present excellent School of Design, because that school, as now constituted, was founded only in recent times; and to several of its governors this passage from Sir George Clerk Maxwell's biography has seemed quite as novel and surprising as to myself. What, then, has become of the University School? When did it expire? How did it vanish? Above all, what has become of the endowment? All I can say upon the last head is that positively it has not been swallowed up by the Senatus Academicus since I became a member of that body in 1822.*

* The statement of Mr Clerk seems to be explained by an observation, to

Philosophy seldom ventured to appear before the Royal Society. In 1784 Dugald Stewart read an essay on Cause and Effect, and in the same session Dr Gregory read another on Cause and Effect in Physics. But of neither is there even an abstract in the Society's Proceedings.

The Chemical papers produced before the Society during the first twenty years of its existence—sixteen in number—are not so numerous as we might expect, when it is considered that the great discovery by Black of the composition of the carbonates, and the nature of carbonic acid, was still of rather recent date. But some of these papers are of interest, and retain their value in the present day. The most important of them are as follows:—

That remarkable genius, Lord Dundonald, father of the lately deceased naval hero, read in 1784 a characteristic inquiry into the purification of sea-salt. Having found that a concentrated solution of common sea-salt possesses the property of dissolving a large quantity of the magnesian salts, which are always to be found in it in small proportion,—rendering it, however, attractive of moisture in damp air, and less fit for the curing of meat,—he placed the salt to be purified in a conical filter, and passed through it two or three successive portions of a concentrated hot solution of the same impure salt, with the effect of removing almost entirely the salts of magnesia. This method was plainly applicable to the large manufacturing scale; and the inventor had only, as in the purification of loaf-sugar by pure syrup, to displace the last saline liquor remaining in the interstices of the salt, by a solution of pure chloride of sodium, when the whole of his product would have been quite pure.

Drs Black and Hutton being appointed with Mr Russell a committee to examine and report on a process proposed for manufacturing spirit from carrots by two enterprising experimentalists, Hunter and Thornby, they find that twenty tons of this root yield

which my attention has been turned in Hugo Arnot's "History of Edinburgh" (1789, p. 323), that the Board of Trustees for Encouragement of Manufactures appointed an artist in 1772 to teach twenty boys or girls drawing, and obtained for the purpose, from the Town Council, the use of two rooms in the University. (Dec. 14.)

two hundred gallons of proof-spirit, equal in quality and cheapness to the best sort of corn-spirit. It is easy, through action of rectified spirit on extract of carrot-juice, to crystallise from it fine cane-sugar, which will account for this result. But the Society's records do not state anywhere what success the inventors attained with their new manufacture.

Dr Kennedy, well known and esteemed at the period as a chemical analyst, contributes a careful analysis of a new Zeolite from the greenstone of Salisbury Crags, showing that it consists almost entirely of silica, lime, and 8½ per cent. of soda; and in 1798 he communicates a much more interesting account of the composition of basalt, greenstone, and lava. In the latter paper he comes to the conclusion, that the basalts and greenstones around Edinburgh, and the lavas around Etna, all agree both in the nature and the proportion of their main constituents, which are silica, alumina, and oxide of iron, with, in all, about 4 per cent. of soda. The interest of these facts, in their application to the prevailing geological doctrines of that period, as well as of the present time, will be seen presently, and, indeed, is obvious on the bare mention of them.

In 1791 Dr Black presents his last communication to the Royal Society, his famous analysis of the spouting hot springs of Geyser and Rykum in Iceland. In the Transactions this important paper is accompanied by another from Mr Stanley of Alderley, M.P., from whom Black obtained the waters for examination, and who himself collected them during a visit to Iceland in 1789. There has appeared since that time no better account of these extraordinary springs than that given by Mr Stanley, and in some respects it surpasses more recent narratives in complete and graphic description. I know not, indeed, that any subsequent visitor has added much to our knowledge until our deceased Fellow, Mr Alexander Bryson, communicated to this Society his excellent thermometric observations on the very high temperature attained by the waters at great depths in the funnel whence they are projected. In Black's time geologists and chemists were puzzled to account for the solution in the water of the great amount of silica necessary to form the extensive deposits of hard siliceous sinter around these fountains. Black's analysis gave the explanation. Of the eight grains in the Rykum water, and ten grains in the Geyser water, of solids in 10,000, Black showed that about one-half consisted of silica dissolved by soda; that the soda amounted to half a grain in the former, and a whole grain in the latter; that it was the means of dissolving more than six times its own weight of silica; and he conjectured that this great dissolving power was partly communicated by very high heat, existing where the process of solution was constantly going on.

In 1793, in a postscript to the Proceedings of the Society for that year, Dr Hope produces his well-known discovery of the mineral strontianite, his analysis of it, his discovery of the new earth, strontia, and his investigation of the properties of the earth itself and its compounds. This paper, which at once established for him a great name among chemists, and was given to the world when he was quite a young man, supplies internal evidence that Dr Hope possessed in an eminent degree all the qualifications for a profound analyst and discoverer—patience, inventiveness, accuracy, and acute discrimination. The wonder is,—which all his scientific friends have felt, but no one has thoroughly explained to his own satisfaction,—that with this first-rate investigation, and at the mere outset of scientific life, Dr Hope's career as an analyst both began and ended. New earths, new alkalies, new acids, new metals, were constantly announced from all sides around by those engaged on the same field which he had shown he could fruitfully cultivate. But Dr Hope never undertook another chemical analysis.

In 1788 Sir James Hall read an exposition of Lavoisier's new Theory of Chemistry, being probably the first account given in Scotland of that philosopher's great discoveries. Soon afterwards Hutton read a reply to Lavoisier and Sir James Hall, in an essay on Phlogiston. Both papers are lost to the world; for, at the time when they were read, authors did not desire to print, and the Society did not publish, either in its Proceedings or its Transactions, scientific papers of a merely critical nature. We can easily understand, however, how stoutly Hutton would at that period stand up for the existence of phlogiston; which, nevertheless, was doomed to die a sudden death at the hands of the French executioner.

In 1796 Dr Hutton, in a paper entitled "A New Phenomenon in the Sulphurating of Metals," proves that when sulphur and iron are heated together, and combine to form the sulphide of iron, the process is one not of inflammation, as might appear on a hasty survey of the phenomenon, but of incandescence, which lasts only during the short time combination goes on, and without any of the essentials of combustion. He annexes some inferences in respect to the connection between heat and light, which I may have to revert to under a future head, as in some measure anticipating a great modern doctrine. The experiment, a striking one, has been rendered long familiar in consequence of Dr Hope, who assisted Hutton in this inquiry, having constantly made it the subject of brilliant demonstration in his lectures, as many of us must well remember.

In 1800 Sir George Mackenzie presented to the Society a very conclusive inquiry as to the combustion of the diamond. It had been burnt, and its nature deduced from the product, in various complex ways, by several eminent chemical philosophers. But Sir George had the merit of showing how it might be consumed in a simple muffle with unaided heat at 15° of Wedgewood's pyrometer,—that steel might be made by heating iron in contact with its dust,—and that, by duly heating a mixture of pulverised iron with a fourth of its weight of diamond dust, a fused mass may be obtained which is quite undistinguishable from cast-iron.

I conclude these chemical notices with a second original investigation of great excellence and interest by Dr Hope. It has been long well known that, as water cools down from a mean temperature, it contracts, and consequently the cooled particles sink until the thermometer indicates about 39°·5 Fahr.; that on farther cooling, however, this general law which regulates the cooling of liquids is upset; that the water then actually expands, and consequently the cooled particles now rise; and that this expansion continues, till at 32° a much greater expansion suddenly takes place, when the water is converted into ice. The contrary phenomena, of course, occur as water heats from 32°. It contracts till the temperature rises to 39°·5, and after that expands according to the general law. But the fact of the density of water thus deviating from the general law which governs the influence of heat on liquids, had been often called in question by high authorities in chemical physics; and among those who acknowledged the reality of the deviation there was irreconcilable disagreement as to the temperature at which in the descending scale the deviation begins. Hope in 1804 settled both points by a set of admirably contrived experiments, in which he took the temperature in various parts of a long column of water,—at the bottom, at the middle, and near the surface,—cold or heat being applied variably at these several points to produce the necessary intestine movements among the particles of the water. The whole paper on this subject is an admirable example of experimental reasoning, which definitively settled, as we now know it, a great fact in nature's laws of much practical value in the economy of this earth, relative to the freezing and thawing of water, and the influence of these changes in its condition on the air, the land, and animal life. This was the second of Hope's original researches, and the last; and again our astonishment is raised that it should have been the last.

In the highest walks of Mathematics and Physics, the Proceedings of this Society have always abounded with important investigations. No one can doubt that such must have been the case when Robison, Playfair, Leslie, Ivory, and Wallace took a large share in the Society's business. But this is a branch of my subject to which I cannot myself do full justice; and time has not sufficed for me to complete my undertaking by asking aid from my well-qualified colleagues, who, I am sure, would have cheerfully granted me their assistance. The papers are chiefly researches in geometry and algebra of the most abstruse kind. But there are two of a different stamp—on the confines between chemistry and physics—which may be here shortly noticed, as they are the work of one whose labours will call presently for a larger share of our attention.

In 1794 Dr Hutton communicates a dissertation "On the Philosophy of Light, Heat, and Fire." In this inquiry he reasons against the existence of radiant heat, apart from light, which had been announced recently before as an important discovery by Saussure and Pictet. Hutton maintained that the real radiation is of "invisible light"—light so faint as not to be cognisable by our senses. In the course of his argument he advances the proposition that light is the "immediate cause of burning," and that the light which appears in combustion is "the extrication of phlogiston, fixed light, or a certain modification of the solar substance which had existed in the inflammable bodies, chemically united with their elements." Finally, he winds up with the doctrine that "light, heat, phlogiston, and electricity, are so many different modifications of the solar substance." Converting these "substances" into qualities of matter, and striking out the lingering remnant of phlogiston, we have here the modern doctrine that heat, light, and electricity are mere varieties of the same quality of matter, and that the Sun is the primary source of them all.

Theother chemico-physical paper is "on the Force Exerted by Water in Freezing," deduced from some experiments made in Canada in 1786 by Major Williams of the Royal Artillery. He exposed to intense frost a thirteen-inch shell filled with water, and plugged by an iron bolt. The metal of the shell was about an inch and a-half in thickness round the fuse-hole, and two inches and a quarter opposite the hole; and the plug, which weighed two pounds and a half, was driven into the hole with great force. At the moment of freezing the plug was driven out with such violence as to be carried 325, 387, and even 415 feet; and at the same moment a column of ice was thrust out of the fuse-hole of the length of four, six, and even eight inches and a half. If the plug, however, was secured by means of springs, like spiking nails, the shell was split, and the ice was thrust out in plates from the fissure.

From its first foundation the Royal Society of Edinburgh has been lavish in its contributions to the several departments of Natural Science. At least forty papers were read during the first twenty years, and many of them have been published in the Transactions, on the branches of Zoology, Botany, Topography, Meteorology, Mineralogy, and Geology.

In the branch of Zoology, however, there was extreme barrenness at that period in the Society. Mr Kerr, in 1790, notices an "Animal ignotum" in the University Museum; and this is all we learn of the animal. That astounding personage, the Great Sea Serpent, makes his appearance once on our boards, under the patronage of Mr John R. L'Amy, Justice of Peace for Forfarshire, in the character of a Kraken, three miles long, as seen at the statutory distance of about one mile by a credible master-mariner and his mate, off the east coast of Scotland, on 5th August 1786. This is all that the Society contributes before 1803 to the science of Zoology.

Belonging to Botany there are only six papers, of which five possess interest. Dr James Anderson of Madras describes, in 1791, the *Oldenlandia umbellata*, from the root of which is obtained in India a valuable red dye-stuff. The Transactions for 1785–90 contain an excellent paper, with illustrative drawings, by Dr Wright, Physician-General of the Army in Jamaica, on the *Quassia simaruba* of that island. The root of that species, now the *Simaruba officinalis*, was strongly recommended by him as a remedy for chronic dysentery, and has still great credit with many in the treatment of that disease. Dr Wright was the first to identify and accurately describe the tree, and did so in this paper, which was read in 1778 in the Philosophical Society, but was first published in the second volume of the Royal Society Transactions.

In 1791, Mr John Lindsay, surgeon in Jamaica, communicates a paper connected with the subject of the last-mentioned inquiry, on the *Quassia polygama* of Jamaica, describing the plant as a magnificent tree 100 feet high and 10 feet in girth, and representing it to possess all the virtues of Quassia wood, the produce of the *Quassia amara* of Surinam. He adds, he is credibly informed that the former is sold in London for the other. This is a curious fact, as fixing the time when the true Quassia wood of the *Quassia amara* of existing botanists was displaced in the markets of Europe by the wood of Lindsay's tree, the modern *Picræna excelsa*, without either druggist or physician having noticed any difference in their virtues, or observed, until a few years ago, that the wood of a great forest tree had been substituted for that of a low bush about twelve feet in height. Different views may be taken of this apparent blindness of the medical profession. For my part, I recognise in the whole incident an interesting proof of the resemblance in action on the human body among different plants belonging to the same natural family. For the great tree of Jamaica, like the little bush of Surinam, is a powerful, simply-bitter, tonic stomachic; and I believe the two may be used indifferently for all medicinal purposes. Mr Lindsay is not so fortunate in a notice he has given in the same paper of *Cinchona brachycarpa*, a new Jesuits'-bark tree, and cure for intermittent fever. This is now known to be no true cinchona at all, and to be immeasurably behind the true cinchonas as a febrifuge. The tree is now the *Exostemma brachycarpa;* and, like all other species of Exostemma, its bark is more emetic than anything else in point of action—a property which, it is fair to say, had been recognised in it by Mr Lindsay himself.

Dr Hutton described in 1778,* and endeavours to explain, a phenomenon of vegetation on Arthur's Seat, which still remains open to further inquiry. It is well known to those who frequent the upper regions of the hill, that on various parts of the slope towards the east are to be generally seen grey zig-zag stripes on the grass, very conspicuous if the general herbage be fresh, almost always tending downwards, from a foot to two feet in width, continuous in some places, but interrupted in others, and stretching for many yards, occasionally for more than a hundred yards. On examining these marks, the grass is found to be completely withered to the roots; the roots themselves are destroyed; and many years elapse before the vegetation is restored. They are most frequent and well-marked in the hollow to the south of the basaltic summit, descending to Dunsappie Loch, but rather to the right towards Duddingston, and also on the subsidiary broad eminence north-east from the summit. Dr Hutton tries various theories for explaining these "Fairy footpaths," but cannot satisfy himself with any of them. Among the rest, he rejects lightning; which, however, I suspect is the only agent which will account for them. Many years ago, when wandering over the upper part of the hill at midsummer, I remarked that these marks were unusually scanty and imperfect. A thunder storm brewing in the south-west compelled me to effect a hasty retreat; and was followed by a very severe storm which passed over the hill, the city, and all the surrounding country. A few days afterwards I found the east slopes of the hill presented many extensive, recent-looking marks of the nature now described.

* To the Philosophical Society. Published in Roy. Soc. Trans. vol. ii.

The remaining Botanical inquiry I have to notice is one well known to naturalists, and of more general interest than the preceding. Great doubts had been long entertained among good authorities about the nature of the motion of the sap in trees. Dr Walker, Professor of Natural History in the University of Edinburgh, undertook a series of well-devised and precise experiments to determine how the sap moves in trees in the spring; and having with philosophical caution repeated them in several successive years, with the same results, he communicated the whole inquiry to the Royal Society in 1783 and 1785. This inquiry is still held to be authoritative proof, that the movement of the sap in trees, on the arrival of genial weather in the spring, is not a movement of circulation, by ascent and descent, as many had before contended, but invariably a simple movement of ascent; that the sap ascends neither in the pith, as some had maintained, nor in the bark, as insisted on by others, but in the wood, and between the wood and bark; that the date of its commencement, and its rate, both depend on the earliness and geniality of the warm season; that the ascent varies in rate, from six to nine inches daily, according to the prevailing temperature of the air; and that those buds always open first into leaves which the sap first reaches, so that its arrival is the essential cause of their growth. The author, however, points out that his experiments fix only the nature of the motion of the sap in the spring, when the tree has no leaves, and takes no account of what the movement may be when the leaves are developed. He is inclined, indeed, to presume that it may then be different; and accordingly ulterior inquiries have shown that, when the tree is in leaf, the sap moves downwards as well as upwards, observing now a circulation.

In Meteorology, if a man be only a good looker, he may one day become an original observer. It is, therefore, a favourite study with those fond of Natural Science. Accordingly, we find upwards of twenty communications, and not a few of them very valuable, on the subject of meteorology in the Society's early Proceedings.

Playfair in 1784 acutely investigates the causes which affect the accuracy of Barometric Observations. In 1790 Dr Rutherford describes a self-registering thermometer, by which the maximum and minimum of temperature may be ascertained between any given periods of time; and this is still the method usually preferred for ascertaining the daily maxima and minima. In 1795, Mr Alexander Keith, afterwards our benefactor Sir Alexander Keith of Ravelstone, describes both a thermometer and barometer, which, by the application of the same kind of contrivance to each, may be made to register the state of these instruments continually, with the aid of clock-work. Mr Macgowan contributes meteorological observations for six years, ending with 1776, made at Hawkhill, near Lochend, in this neighbourhood; the Duke of Buccleuch, President of the Society, contributes the observations of ten years, ending with 1783, made at Branxholme, his seat in Roxburghshire; and Playfair adds abstracts of observations, made at Windmill Street, in the city, for six years, ending with 1798. In 1796, Dr Balfour of the Bengal Medical Service reproduces an inquiry made a good many years previously, in which, by regular half-hourly observations, he was the first to ascertain that at Calcutta, near the equator, the barometer observes a double diurnal revolution of about a tenth or twentieth of an inch, the highest positions being at ten in the forenoon and evening, and the lowest at six in the morning and afternoon; and in 1799 Playfair points out that this result had been also obtained in 1785 by independent observations made near the equator by Lemanon, a naturalist attached to the ill-starred expedition of La Peyrouse.

Mr Hall of Whitehall describes a remarkable lunar halo, consisting of a small concentric ring, of about ten degrees in diameter, round the moon, and a great ring, seven or eight times that diameter, which passed through the moon, cutting the concentric halo in two. Playfair describes a rare rainbow which he saw over the sea from Dunglass, consisting of a lofty perfect primary arch, almost a complete semicircle, and a secondary bow springing from the south limb of the other, and bending outwards in a southerly direction. The Reverend Dr Graham of Aberfoyle notices an Aurora Borealis which he observed at that place in the day-time on February 10, 1799, at half-past three, and states his belief that this was only the second on record, but conjectures that such observations would not be infrequent, if frequently searched for in the circumstances he describes—viz., "when the sky, being for the most part cloudless, is suffused with thin pale vapours especially in longitudinal streaks."

I beg here to be allowed to make a short digression. The phenomena of the aurora borealis in this country have often been minutely described on the occurrence of unusually fine displays of it. But no one, so far as I am aware, has studied carefully its prognostications. Thoroughly inquired into, however, these may prove practically valuable, as the following illustration will serve to show. Every one knows that when the aurora first begins to exhibit in the autumn it is regarded as a sign of broken weather following. But at that period of the year it supplies a prognostic of far greater precision and importance. I have repeatedly mentioned to my friends the observation I have invariably made, that the first great aurora after autumn is well advanced, and following a long tract of fine weather, is a sign of a great storm of rain and wind on the forenoon of the second day afterwards. I must have noticed this fact very early, because I applied it on the occasion of the first meeting of the British Association in Edinburgh on 8th September 1834. There had been a long tract of very fine weather, for a fortnight and more, when on Saturday evening, the 6th of the month, there appeared the widest, brightest, and most flashing aurora I have ever seen. Next day the weather continuing remarkably fine, Professor Sedgewick described, at breakfast at Dr Alison's, in glowing language, the magnificent exhibition which the philosophers of Edinburgh had provided for their southern visitors. Presenting then to him the dark side of the picture, I told him that the Association meeting was to be inaugurated with a great storm. He was surprised at this, and appealed to the continuing cloudless sunny sky against me; but I told him the particulars of the prognostication, and that the storm would not begin till the middle of the following day. Next morning the weather was equally splendid. But soon after eleven the eastern sky began to be overcast, an ominous low north-easterly black cloud rose by degrees; at twelve, as the offices of the Association opened, rain began to fall from that direction, and in a short time there commenced the most incessant and heaviest fall of north-east rain I ever witnessed, lasting without intermission till one o'clock on Wednesday the 10th, when the fine weather was again restored to us and our visitors. I have often made the same prognostication since, and with invariable accuracy; and several friends to whom I have mentioned it have made the same observation—viz., that the first great aurora occurring after a long tract of fine autumnal weather, foretells a storm commencing between twelve and two o'clock in the afternoon of the second day thereafter. I restrict the prognostication to these conditions. It is evident how valuable the knowledge of it may often be to agriculturists. Nevertheless, I never met with farmer or farm-servant who knew it. On one occasion it was the means of saving the corn crop of a friend in Dumfriesshire, whose farm-steward was about to leave his corn half led on the day after a very great aurora, and, deceived by the beauty of the weather, was on the point of taking his labourers to other work not at all pressing. His master, trusting to my positive assurances, ordered him to make haste in leading and thatching everything, and great was the steward's astonishment when a furious three days' storm set in on the forenoon of the second day.

In the pneumatic branch of meteorology three papers were produced to the Society, in the latter part of last century, on topics of great interest to us.

Mr George Wallace, a member of the bar, read, in 1787, "A Dissertation on the Causes of the Disagreeableness and Coldness of the East Wind;" but as the notice in the Society's Proceedings merely informs us that "the author did not incline that any abstract should be given of his dissertation," and I cannot find that it was published elsewhere, we are left in ignorance what were the discoveries or opinions of Mr Wallace on this knotty question, in which we must all take a lively personal interest. It may be that this gentleman has withheld from the world one of the most valuable practical discoveries which remain to be made in meteorology. It may be, however, that he was deterred from having any notice taken of his lucubrations by learning certain views entertained of the same subject by Hutton; who communicated them to the Society in an able paper "On our Vernal and Autumnal Monsoon Winds," in February 1791.

In this paper he points out that winds are shiftings of the air, occasioned by changes in its temperature; that these changes arise from the alternations of day and night, the alternate crossing of the equator by the sun northward and southward, the different degrees in which the sea and land are heated by the sun's rays, the effect of cloud in interrupting the heating action of the sun, and the influence of rain in cooling the air by the evaporation of its drops in descending towards the earth; and he argues that we might explain all the phenomena of the winds, could we thoroughly appreciate in each instance under investigation the interference and relative energy of these and other subsidiary disturbing forces. He then illustrates these principles of inquiry by reference to our East winds in spring, and our West winds in summer and autumn. For the sake of brevity, I shall confine myself to the former topic, which is investigated with Hutton's characteristic force and simplicity. In spring, says he, easterly winds prevail here, because the cold wintry air of the polar regions is drawn southward over the warmer continent of Europe and westward by the warmth of the German Ocean. For some time after it begins to blow in March and April it occasions in us a cold, uncomfortable feeling, because it is thermometrically cold, and also unduly dry. Its lowness of temperature, compared with the west wind, which occasionally interrupts it for a short period at this season, no one can dispute; and Hutton had frequently remarked a thermometric difference of 10° F. in favour of the interpolated west wind. The cold feeling thus occasioned is aggravated, he says, by the warmth created by the sun's rays in sheltered places; but, in particular, it is increased by the withering dryness of the wind,—for the common notion that an east wind is a damp wind is quite a mistake. During a long period of the spring it is a dry, parching wind, on that account alike disagreeable to the human race and blighting to vegetation. He tells us quaintly that he "never had a hygrometer;" but he improvised one, being nothing else than a somewhat rude wet-bulb thermometer, such as is now in constant use in a more perfect form; and he tells us that in the east winds of early spring he sometimes found a difference of 10° F. between the wet and dry thermometers, while he never could observe a difference of more than 4° F. in the driest days of summer. In the month of May, however, a change takes place in the character of the east wind. Still dominant, it encounters in its passage across the German Ocean a more and more powerful sun, which both raises the temperature of the air as it approaches this island, and, by warming the sea, raises watery vapour to moisten the lower stratum of air. Hence the May east wind arrives both warmer and less dry. It is now therefore for the most part much less unpleasant to the feeling than in March and April. Towards evening, however, as the sun's rays fail in force, the air, cooling down, parts with moisture to form mist, which creeps up our Firth, spreads over the neighbouring land, prevails all night, and lasts into the next forenoon, till it vanishes under the renewal of the heat of the air from the sun's rays. During the misty time the uncomfortable impression on the human frame is renewed in spite of the air being now moist, partly because the temperature is lowered, partly because mist—being a form of water in the liquid state—is a more powerful conductor of heat from the body than the same water dissolved in the air as gas. Accordingly, as the mist is dispelled with the advance of day, the sense of discomfort vanishes, because the air is warmer, and is loaded merely with perfectly dissolved aëriform moisture. It is remarkable, he adds, that the unpleasant influence of these east winds on man do not then correspond with their influence on vegetation. In March and April the cold, dry east winds wither up and destroy leaves prematurely unfolded. Later in the season an east wind, equally cold, but loaded with moisture, does them no harm, because it loses its parching, withering property. This important difference every careful observer must have noticed. In the case of the animal frame, however, the mere cold of an easterly mist is adequate to cause discomfort, and injury to the health. Thus Hutton maintains that all the disagreeableness and injury occasioned by our east winds, both in spring and in early summer, may be adequately explained by reference simply to their condition in point of temperature and humidity, without our requiring to take account of any other agency, hidden and mysterious in its nature and operation. I have dwelt a little on these simple, yet profound, views of Hutton, because, like other inquiries of his, they have been too much lost sight of in our time, and they may help to clear up and satisfy many minds which have been hitherto much obfuscated and discontent in respect of the present matter.

Hutton appears never to have had an opportunity of studying hurricanes. If he had, it is not too much to say that his profound penetration into Nature's laws would scarcely have failed to recognise those which govern the mightiest of atmospherical movements. It is not likely, for example, that he would have left in a state of dry detail of bare facts the luminous description by Sir Gilbert Blane, communicated to the Society in 1785, of the terrible hurricane at Barbadoes in 1780;—a description containing incidents which nothing but the modern theory of cyclones can explain, and which, duly considered, might have led so acute a mind as that of Hutton to the right solution, even at that early period. This storm lasted the greater part of two days, and raged with unexampled fury for twelve hours, destroying the fort at Bridgeton, levelling an immense number of houses, laying waste the whole crops of the island, and occasioning the sudden death of at least 3000 inhabitants. Notwithstanding, however, its uncommon swiftness, people were surprised at the comparative slowness with which it passed from island to island. They evidently confounded the impetuous whirlwind within the cyclone itself with the slower progress made by the whole cyclone from place to place. Ships at sea, adds Sir Gilbert, found that the storm blew from all points of the compass; a phenomenon explicable only by the theory of the cyclone. The fate of one vessel is particularly mentioned as unaccountable; for being blown from her anchorage in Carlisle Bay, and losing all her compasses, she was driven before the blast, as her crew supposed, to a distance of at least a hundred leagues in two days, when, to their astonishment, they found themselves very near the place from which they set out. There was enough in these striking facts to direct an acute and inquiring mind to a true theory of hurricanes, as now viewed by meteorologists.

In the department of Thermometric Meteorology, Mr Hall of Whitehall records a precise fact as to the great cold of the winter of 1795, having observed the thermometer so low as —6° F. on the 22d of January in that year. In order to appreciate duly the great intensity of this cold for a Scottish locality, it is necessary to know that the station where the observation was made is ten miles from the sea at Berwick, on the north bank of the Whittadder, certainly not more than 150 feet above high-water level. About this time the winters in Scotland were very hard.

In 1781 Professor Wilson observed in the Observatory Park at Glasgow a cold of —4° Fahr., and in 1780 actually one of —14° Fahr.

Dr Guthrie, one of many Scottish physicians of the time who settled in Russia, describes in a dissertation read to the Society in 1789 a remarkable phenomenon he had observed in the thawing of thick ice. When ice on the river Neva had been reduced by thaw to two-thirds of its thickness, it became so brittle as not to bear even the weight of a dog, though still eighteen inches thick. The cause he discovered to be that the ice is then composed of solid crystals "like organ-pipes," about eighteen inches long, and with scarcely any cohesion among themselves. This appears a structure somewhat analogous on the large scale to the loss of cohesiveness among the minute particles of ice, which gives occasion to the downward descent of glaciers.

The only original inquiry in Thermometric Meteorology produced to the Royal Society of Edinburgh in its early life, is one of great importance in a scientific respect, as well as practically by reason of its bearings on atmospherical thermometric observations. This is the dissertation well known to learned meteorologists, but lost sight of by too many others, of Patrick Wilson, Professor of Astronomy in Glasgow, "On a remarkable cold which accompanies the separation of hoar-frost in a clear air." This inquiry, read in 1784, and carried on during several previous winters, in continuation of researches of the same kind communicated to the Royal Society of London in 1780 and 1781, is the germ which ultimately produced in the hands of Dr Wells the true theory of the formation of dew and hoar-frost; and the author approaches so near that theory as to create regret that, having stepped on the right path, he had not the luck to follow it to the end. Wilson was the first to observe the difference in clear frosty weather between the temperature of snow, hoar-frost, sand, and many other objects, and the temperature of the atmosphere a few feet above. He frequently observed a difference of 4°, 8°, 12°, and even on one occasion 16° of greater cold on the surface of snow, than in the air four feet, or even only two feet and a half above it. He also noticed that this difference is always attended with an increase of weight from the deposition of hoar-frost; that the difference is always greatest when the atmosphere is clearest and stillest; that wind, even in clear weather, annihilates the difference; and that such is also the effect of the atmosphere becoming hazy, or the sky overcast with clouds. Wilson laboured to account for all these variable phenomena on the assumption that the deposition of hoar-frost occasioned the cold. But it is easy to see that, in spite of some ingenious suggestions upon that basis, he did not succeed in satisfying entirely his own mind. Had he begun with the converse assumption, that cold was the cause of deposition of hoar-frost, he would probably have anticipated more modern discovery. He was indeed very near doing so. For, speaking of the influence of a passing cloud in putting an end to the formation of hoar-frost and depression of the thermometer, he uses these words, "When the atmosphere becomes suddenly clouded, it is certain that this change must be attended with the extrication of much sensible heat in the higher regions, where these vapours are congregated. A store of heat so produced must soon affect the mass of air which lies below." But how? he might have asked himself. Not surely by the process of conduction, because heated air rises; it does not sink. Not, then, by the process of conduction, but by that of radiation, which, instantly darting heat from the clouds, replaces the loss which in clear weather is sustained by snow and other objects on the surface of the earth through radiation of heat from them into the cold attenuated atmosphere of the far firmament. Unfortunately, however, the theory of radiant heat was too little advanced to suggest to him this explanation,—obvious and easy after the discoveries of Pictet in 1790, and the admirable researches of Leslie fourteen years later.

No one could investigate carefully the theory of the winds without having his attention directed to the Theory of Rain; and, accordingly this is one of the branches of meteorology which Hutton was the first to investigate successfully. This he did in a dissertation read in 1784 and enforced in 1787, in reply to adverse criticisms by the philosopher De Luc. The power which the atmosphere possesses of dissolving or suspending moisture in invisible vapour as transparent as the air increases with the temperature. Hutton's discovery was the proof, by mathematical demonstration, that rain or mist could not be formed when two masses of air of different temperatures are mingled, unless the power to dissolve moisture increased in a greater ratio than the increase of temperature. Hutton's mathematical deduction has been since proved experimentally to be true. As the solvent power of atmospheric air increases in a greater ratio than that at which the temperature rises, when two masses of transparent air of different temperatures are mixed together, more moisture is present than suffices to saturate the mixed air at the intermediate temperature which is produced; and hence the excess must separate in the form of either mist, if the excess be slight, or rain, if the excess be considerable.

In the topographical branch of Natural History the early Proceedings of the Society present several papers which must have possessed at the time much interest, such as an account of the Caves of Elephanta by Dr Buchanan, of Prince of Wales' Island by Mr Howison, of the Trinidad Petroleum Lakes by Mr Lochead, and of the Natural History of Guiana, and of Madeira, by the same gentleman. But these narratives have been rendered obsolete by more elaborate descriptions published since.

It remains for me, under the head of the Natural Sciences, to take notice of the early labours of the Royal Society in Mineralogy and Geology. In this department the Society, during the first twenty years of its life, shone with a brilliancy unsurpassed by any of the scientific academies of Europe; for during that time were produced Hutton's Theory of the Earth, and the illustrative experiments of Sir James Hall.

The Society's papers on Mineralogy and Geology are eleven in number, but of three of them the Proceedings contain not even an abstract.

Colonel Imray, in a well-told description of the "Mineralogy of Gibraltar" in 1797, corrects some prevailing errors as to the species, composition, and geological position of the famous deposit of bones in various parts of the Rock, and is, I believe, the first to point out that the hill must have been at one time for a long period covered by the sea to the height of at least 900 feet, as he found at that elevation numerous "pot-holes," formed by trituration under water with shingle-stones kept in motion by currents.

Dr Richardson, in 1803, describes three remarkable basalts which he found on the coast of Antrim, not far from Portrush: one an ochrous basalt, apparently undergoing decomposition; another containing fossil shells, but a dubious basalt; and a third containing bladders of compressed liquid. "This basalt," he says, "contains small cavities in its interior, many of them full of fresh water, which gushes out when the stone is broken by the hammer, as if it had been in a state of compression." Here we have, I apprehend, one of the earliest notices of the presence of liquids in the interior of perfectly solid minerals.

In 1791 Dr Hutton explains the cause of the flexibility of the Brazilian stone, or flexible sandstone. "When a stone," says he, "of any considerable thickness is said to have flexibility, we are led to think that here is something very extraordinary, and we wish to know upon what depends that quality, nowise proper to a stone." Accordingly he set about inquiring, and, after being for some time much puzzled with his problem, he considers that the property is owing to a certain structure, recognisable only with the aid of the microscope, constituted by minute particles of thin mica thickly disseminated through the mass, and always parallel to one another, by which a certain jointed character is given to the stone.

The Rev. Mr Christopher Tait, minister of Kincardine on Forth, has delineated with much care the condition in 1792 of the great Flanders and Kincardine mosses in Stirlingshire and the adjacent eastward coasts of the Firth. Extending on both sides of the Firth, from the line of Kincardine westward as far up the Carse of Stirling as Cardross, the unredeemed desert of peat covered in 1770 a territory 22 miles long, between three-fourths of a mile and seven miles wide, and not less probably than 30,000 acres, assuming an average width of two miles, which I take to be within the mark. He describes the composition of the moss, notices an ancient corduroy road through part of it, shows that at one time its place must have been occupied by a forest of great trees, proves that these had been mostly cut, probably by the early Roman invaders, in order to destroy a retreat and place of assembly for their native enemies, and gives a good succinct account of the famous design of Lord Kames, which had been in successful operation during twenty-two years, for clearing away the peat, uncovering the underlying soil, and converting the moss into agricultural fields. I wish

I could state in contrast, which must be very great indeed, the present condition of that territory after seventy-six years more of enterprise. My limited time has not sufficed to ascertain this point.

I do not know at how early a period we possess a scientific record of the Comrie earthquakes; but there is a precise one by Mr Ralph Taylor in the Society's Transactions, read in 1790, with additions in 1793, describing several visitations, but especially one in 1789, during market-day on 10th November, which made the earthenware vessels clatter in the market-place, terrified horses, and caused the people to think the surrounding mountains were falling on them.

In March 1785 Dr Hutton commenced the reading of his theory of the earth, under the title of "Investigation of the Laws observable in the Composition, Dissolution, and Restoration of Land upon the Globe." The Huttonian theory may be shortly stated as follows:—

Providence, for the wise purpose of preserving and maintaining the excellence of its works, has ordained that all creation, so far as we can study it, shall be subject to alternate decay and renovation arising out of that decay. For this purpose are provided suitable materials and the necessary forces. The earth itself is not excepted. Its present stage of decay is obvious to all eyes on surveying its surface; a prior renovation is almost equally obvious on examining into its crust; and a decay antecedent to that renovation is abundantly evident on careful inquiry in the same quarter.

Under the force of the waves, currents, and alternate flow and ebb of the sea, we may observe that the waters are gradually stealing upon the land, sweeping into their depths the waste so occasioned; but much more under the action of alternate frost and thaw, rains and winds, rivers and floods, earthquakes and other forces, we may behold a never-ceasing wear, slow indeed, but continual and universal, going on over the surface of the dry land; the result being that the waste, along with that of animal and vegetable forms, is constantly carried by the rivers into the ocean. In the depths of the ocean the waste settles down in stillness; and we may safely assume that it settles in the shape of layers, varying in nature and kind, in different ages and at different places, with the prevailing soil, rocks, and vegetable and animal remains from whose wear upon land the waste has been derived. So much we may discover from what is now going on before our eyes.

If we next assume that at some former epoch a similar waste had been carried on, so long and so far that the earth became less and less suitable for the maintenance of vegetable and animal life on its surface, we then find that a provision exists for renovation of that surface to its pristine condition through the agency of subterranean fire. This power, of whose existence in tremendous energy we have sensible proof in many regions of the globe, had only to approach the subaqueous beds of waste matter in order to fuse them; and then, with a little accumulation of force, to raise them high out of the ocean from its bosom, and even to burst through them, driving far upwards into light and air immense masses of fused materials, long pent up in that state deep in the interior of the earth.

In conformity with this theory, we should find in the present dry land the stratiform portion of the earth's crust presenting in its deepest beds a crystalliform structure corresponding with the laws which govern concretion from a state of perfect fluidity under very slow cooling. On approaching the present actual surface from these deep beds, we ought to find in the superimposed beds rocks presenting signs of agglutination merely, from softening rather than downright fusion. Close to the present surface we should remark perhaps little more than that amount of loose cohesion among the particles composing the rocky beds, which may be fairly ascribed to mere compression sustained while the beds lay under an enormous mass of superincumbent sea. We ought also to remark, that the remains of animals and vegetables are most distinct and least altered by heat from below in the uppermost layers of the stratiform rocks, less and less so as we descend, and at length unrecognisable in the deepest, most perfectly fused beds,—not because vegetable and animal remains were not there deposited along with the earth's waste, but simply because their form was entirely destroyed, and their substance completely incorporated through perfect fusion with the matter in which they lay. If cavities be formed from any cause in the beds of consolidated rocks, we should expect to find in the deep, highly fused beds, but not in those of stony matter agglutinated by mere softening, that these cavities are lined with crystals in point of composition much simpler than the matrix, or even consisting of a single element only, and yet deriving their ingredient or ingredients from the matrix—such being a law generally of crystallisation from composite mixtures in a state of fusion or of solution. We should moreover expect to find where the melted matter itself of subterranean fire has burst upwards through the stratiform rocks, that it carried broken masses of these rocks along with it, and on cooling showed them included in the invading and now concreted liquid. We should see the disrupting liquid mass diffusing itself in veins in every direction through the shattered beds, and even insinuating itself between the beds, separating them from each other, and now itself forming beds betwixt them. We might also expect to see in the huge masses of subterraneous matter which had been thrown up in a melted state, that, by virtue of their perfect fusion and very slow cooling, they now present us with the most crystalline texture of all rocks, show even a separation into crystalline bodies totally different from one another in composition, and display, in accidental cavities in their interior, lining crystals of the most perfect form, and of the simplest materials which the matrix of rock can yield.

Now, all the phenomena thus described, as it were by anticipation, we do actually witness on examining carefully the several rocky beds and masses which form the present crust of our earth. Therefore there can be no doubt that this crust has been formed from the crumbling of a more ancient dry land, deposited in the sea, and afterwards fused and raised above the sea by the agency of subterranean fire.

Such is in brief terms a summary of the main points and proofs of Hutton's theory, beautifully but tersely set forth in his own dissertation in our Transactions. I might have taken this summary from the admirable extension and illumination of the theory in the classical work of his pupil and friend, Professor Playfair, "The Illustrations of the Huttonian Theory of the Earth;" but I have preferred to use for the purpose Hutton's own exposition, because, though he has often been called obscure, I cannot find obscurity anywhere when he is read with the light of details since acquired to science, and probably not altogether unknown to him, notwithstanding that for brevity's sake he has not put them forward.

Two subjects of reflection have been forcibly brought before me in lately renewing, after a long interval of time, my acquaintance with Hutton's own treatise. Firstly, Though he was cultivating a field of inquiry almost entirely new, and developing a vast multitude of new argumentative facts and views, there is scarcely a proposition made from first to last to which a well-instructed modern geologist may not give his assent. Secondly, In his essay of ninety-six quarto pages, he has given his successors in all branches of science a remarkable lesson—a most luminous narrative on a most novel subject, without coining a single new term, or quitting plain English words, unless in the case of a very few German names for rocks previously in universal use among geologists. Much of the repulsiveness of many branches of science to the general student of the present time is no doubt owing to the apparent necessity of a recondite and mysterious nomenclature. Hutton has shown that the most novel and profound inquiries may be propounded with precision, in his day at any rate, without such aid.

The Huttonian theory, though welcomed by many able proselytes, likewise encountered not a few equally able adversaries. These belonged chiefly to the followers of Werner, or Neptunians, who recognised nothing but the force of water in all the apparent revolutions on the earth's surface. The controversy against Huttonianism was for several years carried on at the meetings of this Society with great talent and energy under the leadership of Professor Jameson, a favourite pupil of Werner himself, and the greatest adherent ever gained to the side of that philosopher. Huttonianism may therefore be truly said to have attained its highest triumph when, in the apartment where we are now met, Jameson, not many years before his death, as some of us must well remember, publicly renounced the creed he had taught for half a century, and paid an uncompromising tribute to the truth and profoundness of the Huttonian theory of the earth.

While Hutton's theory was undergoing probation in its early days, some rather troublesome objections were brought against it by its ingenious adversaries. Among these may be here mentioned two, and two only, because they directly gave birth to certain able experimental researches by Sir James Hall, who became our second

President, about twenty years later, on the death of Henry Duke of Buccleuch, in 1811. It was objected that many of the rocks, whose structure was ascribed to fusion by Hutton, become, on cooling from fusion, a slag or a glass, and cannot afterwards recover their crystalline texture. It was also objected that carbonate of lime, which constitutes a large proportion of the stratiform beds in the crust of the earth, cannot be fused, because, long before the heat is raised high enough, it parts with its carbonic acid and becomes lime, which proves refractory under the most intense artificial heat which can be applied. Hutton's answer was that Nature's operations in this matter are carried on upon a vast scale, with unlimited heat, and under enormous pressure—three conditions wholly unlike those in which all experimental imitations must be attempted. Hutton, in his Dissertation seems to have anticipated these objections. But though a skilful and inventive chemist, he did not venture to meet them by experimental evidence. He even threw discouragement over the proposals of his enthusiastic disciples to find a reply by daring to drag Nature into their laboratories. "What!" said he, in a different essay, "judge of the great operations of the mineral kingdom from having kindled a fire and looked into the bottom of a little crucible!"

Sir James Hall, however, resolved not to be thus discouraged. Such was his veneration for his friend and teacher, that he tells us he would not execute his plan during the lifetime of Hutton. But after Hutton's death Hall kindled his fire, and looked into his little crucible; when behold! Nature at work there, exactly as in the vast profound.

In a paper on granite, read to the Society in 1790, he pointed out, that, although the quartz, felspar, and mica, which make up that rock, are fused into glass by artificial heat, there is no reason why, under slow cooling, the crystals of felspar, quartz, and mica should not separate and crystallise apart, in the same way as the crystalline particles of salt and ice separate in the freezing of sea-water, or like the crystallisation which renders transparent glass an opaque, rock-like body, when from a state of fusion it is made to consolidate very slowly by gradual cooling.

Sir James tells us afterwards that, at the time this paper was read, undeterred by the taunts of his friend, he determined to subject these opinions to the test of experiment. The issue was his essay in 1798, entitled "Experiments on Whinstone and Lava." He first by fusion and quick cooling obtained a black vitreous mass from basalt, greenstone, porphyry, and greywacke; and on again fusing these glassy bodies, and cooling them very slowly, he recovered stony masses, "entirely crystalline, with facets appearing in the solid parts," much resembling the original rocks. In the case of basalt from our Castle rock, the resemblance was "so strong in colour and texture, that it would be difficult to distinguish them." Extending these experiments to lava, he tells us, in the first place, that travellers have given rise to erroneous ideas of the characters of lava, by bringing away with them only the superficial scoriæ, and that the deeper parts present very much the appearance and texture of our trap rocks. Specimens of this kind he accordingly found to comport themselves exactly like greenstone and basalt, according to the rate of cooling. The lava of Mount Etna, near Catania, he found to resemble closely the columnar basalt of Arthur Seat, and that near Santa Venere was very like the basalt of the Castle rock; and both of them presented the same varying phenomena as these rocks, when fused and then cooled quickly or slowly.

But Sir James Hall's greatest triumph was his subsequent experimental inquiry, not produced to the Society till 1805, and consequently a little beyond the period included in my present sketch, "On the Effects of Compression in modifying the Action of Heat." By a series of difficult, dangerous, costly, but skilfully contrived experiments, he ascertained that carbonate of lime is a fusible body, if, while exposed to intense heat, it be also subjected to powerful pressure, so as to prevent the conversion of its carbonic acid into gas. He also found that, according to the degree of heat, and consequently of approach to perfect fusion, he could produce, under proportionally high pressures, varying from 52 up to 173 atmospheres, the latter of which corresponds to a mile of sea, every essential character which carbonate of lime variously assumes in the mineral world, from the slightly cohesive chalk to the firm solid structure of opaque secondary limestone, the crystalline structure of translucent marble, and even the transparency and rhomboidal form of calcareous spar. Sir James valued these striking results chiefly on account of their irresistible bearing on the Huttonian theory. But, however important they may be in that relationship, they possess high intrinsic merit; and it seems surprising that his method of inquiry has not been extended to other substances.

Hutton made a collection of specimens for illustrating his theory of the earth. This collection was presented to Dr Black by Hutton's sister and representative; Black made it over to the Royal Society; and as the Society had determined not to keep up a museum of its own, Hutton's collection was transferred to the Museum of the University. It must be a subject of keen grief to every lover of geological science, and to every man who feels the respect which is due to the great men of former days, that there is great reason to fear that this collection has been lost sight of, and may not now be capable of being identified. It must have been extensive; for I find in the old manuscripts of the Society's Proceedings, that a committee of Fellows, appointed to conduct its transference to the University Museum, asked no less than a twelvemonth to arrange it. We can scarcely doubt that it was transferred; for it certainly has not been in this Society's possession since I became a Fellow. But it has never been displayed in the University Museum; for indeed it was a perpetual complaint of the late Professor Jameson, that he had no space for exhibiting any collection of rocks at all in the museum. Mr Archer informs me that no such collection has yet come to light in the course of the examination of the vast geological accumulations during the keepership of Jameson, now in the National Museum of Science and Art; and that he doubts, from the state of the portion already examined, whether it will be possible to identify any special part of it. The Society will confidently rely on his zeal and care in this matter; and I submit whether it is not the Society's duty to consider what it can do to save geology from a calamity so deplorable and so discreditable to the scientific history of Edinburgh, as would be the loss of Hutton's own collection which illustrated the Huttonian theory.

The summary now given of the Proceedings of the Royal Society of Edinburgh during the first twenty years of its existence, does not include a few papers of a kind which cannot be well brought under any of the groups adopted in the preceding arrangement. But some of these merit attention.

Dr Donald Monro describes in 1783 the mode at that time followed in India for obtaining attar of roses without having recourse to distillation. This method consists simply in exposing for six or seven days to the sun picked rose-petals, merely covered with water, in an earthenware jar, and removing with a pellet of cotton on a stick, the volatile oil which gradually forms on the surface. Roebuck communicates in 1798 experiments on the effects of compressed air, made by him in the air-vault of the blowing apparatus of the Devon Iron-works. Only one man was found so venturous as to go with him into the air-vault while the steam-engine was working. The vault was 72 feet long, 14 feet wide, and 13 feet high; and the compressing force was 2·75 pounds per square inch, by which a mercurial column was raised between five and six inches, adding, therefore, nearly a fifth to the mean pressure of the atmosphere. He observed a sense of pressure on the ears, great increase of sound, no perceptible augmentation of heat, but a damp sensation which speedily passed off. No inconvenience was felt during the hour that the experiment lasted. Sir James Hall, in a dissertation on the Origin of Gothic Architecture, deduces ingeniously the Gothic arch from the form assumed by tree poles bent at the top to meet or cross one another, and the florid ornaments of the arch from spontaneous fractures and outward bending of the tree bark on the curves of the arch as the bark dries. Sir Gilbert Blane endeavours to trace the Arabic figures for numerals to India. Lord Ancrum proposes improvements in the arms and accoutrements of cavalry, principally for lessening the weight on the horse, and facilitating the movements of the horseman. Clerk of Eldin shows his well-known nautical propensities, by proposing a scheme for raising sunken ships.

According to custom, I annex a statement of the changes which have taken place during the last twelve months in the membership of the Society.

In November 1867 the Society consisted of 56 Honorary Fellows and 288 Ordinary Fellows. During last session 20 new Ordinary Fellows have been elected, viz.:—

Rev. David Aitken, D.D.; Robert Daun, M.D.; Rev. D. T. K. Drummond, A.M.; Robt. M. Ferguson, Ph.D.; J. Sampson Gamgee, M.C.S. Eng.; Rev. Jos. T. Goodsir; Colonel Seton Guthrie; Rev. Thomas Guthrie, D.D.; Thomas Key, Dep. Insp.-Gen. Indian Army; J. W. Laidley, Esq., Sea Cliff House; Thomas Smith M'Call, M.D.; J. F. Maclennan, Advocate; John Macmillan, M.A.; Rev. J. F. Montgomery; John Dick Peddie, Esq., Architect; Samuel Raleigh, Esq., C.A.; Adam Gillies Smith, Esq., C.A.; John J. Stevenson, Esq.; Major J. H. M. Stewart; W. Williams.

During the same period two Honorary Fellows and nineteen Ordinary Fellows have died; and one Ordinary Fellow has resigned, viz.:—

Honorary Fellows.—J. B. L. Foucault, and C. F. Schönbein.

Ordinary Fellows.—James Anstruther, W.S. [died in 1866, but omitted last year]; Professor G. A. Walker-Arnott; Principal Sir David Brewster; John Burt, M.D.; Henry Cheyne, W.S.; Right Hon. Sir Geo. Clerk, Bart.; John Davy, M.D.; Rt. Hon. Lord Dunfermline; Robert Hamilton, M.D.; Wm. Bird Herapath, M.D.; Rev. Professor Robert Lee, D.D.; Professor Macdougall; Patrick B. Mure Macredie, Advocate; Thomas Mansfield, Accountant; Dr Manson, Nottingham [died some years ago, but hitherto omitted]; R. Mayne, Esq., Indian C.S.; James Richardson, Esq.; Alex. Thomson, Esq. of Banchory.

Resigned.—Robert Campbell, Esq.

The Society will observe that this obituary includes many names of great distinction in learning and science. In other circumstances it would have been a great pleasure, as also I feel it to have been my duty, to present to this meeting a sketch of the life of these deceased Fellows. But, called on to prepare my address with brief time to do so, and overwhelmed, too, with unusual University duty, as in charge of the election both of our Chancellor and of our first member of Parliament,—it has been altogether impossible for me, even with the assistance kindly proffered by members of Council, to do anything like justice to such biographies as those of Sir David Brewster, Professor Walker-Arnott, Dr John Davy, Dr Bird Herapath, Dr Robert Lee, Dr Manson, Sir George Clerk, Mr Thomson of Banchory, and others of the Ordinary

Fellows whom we have lost; or those of our late eminent Honorary Fellows, Schönbein and Foucault. A full biography, however, of our late President, Sir David Brewster, will appear ere long from some well-qualified pen, and one of Professor Walker-Arnott by Dr Cleghorn.

The following Gentlemen were elected Foreign Honorary Fellows of the Society:—

GUSTAV ROBERT KIRCHHOFF, Professor of Physics in the University of Heidelberg.

RUDOLPH VIRCHOW, Professor of Pathological Anatomy in the University of Berlin.

Monday, 21st December 1868.

The following Communications were read: —

1. On the Colour of Aërial Blue. By Sir George Harvey.

This paper is intended to prove that the colour of blue in the sky and in the landscape is simply the result, in the former, of the darkness of space, as seen through the white light contained in the atmosphere; and, in the latter, of the same cause as shown in the dark and distant portions of the landscape being viewed through the interposed medium of air filled with white light. The colour of aërial blue being due in both cases to the same cause, namely—a dark body neutralised as to its darkness by being seen through a white and transparent medium.

2 On the Rotation of a Rigid Body about a Fixed Point. By Professor Tait.

(*Abstract*).

This paper contains an attempt to exhibit the mutual relations of some of the more important of the various processes which have been employed in solving one of the most celebrated problems of Dynamics. The Quaternion analysis has been throughout used, as far more briefly comprehensive and more suggestively expressive than the ordinary Cartesian analysis.

A brief sketch of the *kinematical* relations of the problem is first

given, partly after Hamilton; and in it the main object sought is usually the quaternion, q, on which depends the operator

$$q(\quad)q^{-1}$$

which turns the body from any initial position whatever to its position at time t. The investigation of the axis and amount of the single rotation by which the body may be thus changed in position was first suggested by Euler, but it was greatly simplified and extended by Rodrigues and Cayley. The fundamental kinematical formula of the present paper, which connects the quaternion, q, with the instantaneous axis of rotation, ϵ, is

$$\epsilon = 2V\dot{q}q^{-1},$$

and had been obtained by Cayley, though not in this very simple form, as a quaternion translation from some of his Cartesian results.

From this equation the formulæ, connecting the angular velocities about the principal axes with the various sets of three angular co-ordinates which have been employed to determine the position of the body at time t, are deduced, mainly to show how complex are these systems as compared with those suggested at once by quaternions.

Hamilton has pointed out that, if ϖ be the vector of an element m of the mass, the whole *kinetic* properties of the motion are contained in the equation (which is really that of Lagrange)

$$\Sigma . mV\varpi(\ddot{\varpi} - \psi) = 0,$$

where ψ is the vectorex pressing the applied force on unit of mass at m. He has also given the *kinematical* relation

$$\dot{\varpi} = V\epsilon\varpi.$$

By means of this he obtains

$$\Sigma . m\varpi V\epsilon\varpi = \gamma,$$

where γ is a constant vector if no forces act, otherwise it is the time-integral of the vector-couple

In the paper it is proved that if we write

$$\eta = q^{-1}\epsilon q$$
$$\zeta = q^{-1}\gamma q$$

(where η and ζ are certain vectors in the body in its initial position) the whole kinetic properties of the motion are expressed by the equation

$$\phi\eta = \zeta,$$

where ϕ is a *linear and vector function*, which here introduces (as the roots of its determining cubic) the three moments of inertia.

As the tensor of q may have any value whatever, let

$$Tq = \text{constant}.$$

Then our equations become

$$q\eta = 2\dot{q},$$
$$\gamma q = q\zeta,$$
$$\phi\eta = \zeta.$$

On the integration of these very simple forms the solution of the problem depends. They give

$$q\phi^{-1}(q^{-1}\gamma q) = 2\dot{q}$$

as the quaternion equation for q; where, however, if forces act, γ is to be considered as a function of q; and they supply the counterpart of Euler's equations in the form

$$\phi\dot{\eta} = -V\eta\phi\eta,$$

when γ is constant.

If we seek the actual equations of Euler, referred to the moving principal axes, we obtain

$$\varphi\dot{\epsilon} = -V\epsilon\varphi\epsilon,$$

where φ differs from ϕ simply in the fact that its rectangular unit-system is fixed in, and moves with, the body.

If we write

$$q = w+ix+jy+kz$$

the equation above (for q) gives us the following set of ordinary differential equations containing the complete solution of the problem when no external forces act:

$$\frac{dt}{2} = \frac{dw}{W} = \frac{dx}{X} = \frac{dy}{Y} = \frac{dz}{Z},$$

where

$$\left.\begin{aligned} W &= - x\mathfrak{A} - y\mathfrak{B} - z\mathfrak{C} \\ X &= \quad w\mathfrak{A} + y\mathfrak{C} - z\mathfrak{B} \\ Y &= \quad w\mathfrak{B} + z\mathfrak{A} - x\mathfrak{C} \\ Z &= \quad w\mathfrak{C} + x\mathfrak{B} - y\mathfrak{A} \end{aligned}\right\}$$

and

$$\mathfrak{A} = \frac{1}{A}\Big(a(w^2-x^2-y^2-z^2)+2x(ax+by+cz)+2w(bz-cy)\Big)$$

$$\mathfrak{B} = \frac{1}{B}\Big(b(w^2-x^2-y^2-z^2)+2y(ax+by+cz)+2w(cx-az)\Big)$$

$$\mathfrak{C} = \frac{1}{C}\Big(c(w^2-x^2-y^2-z^2)+2z(ax+by+cz)+2w(ay-bx)\Big).$$

Here A, B, C are the principal moments of inertia, and

$$\gamma = ia + jb + kc$$

is the constant vector of moment of momentum.

Thus we see that W, X, Y, Z are *homogeneous* functions of w, x, y, z, of the third degree. Equations of this nature, but not so symmetrical, have been given by Cayley, and completely integrated (in the sense of being reduced to quadratures) by assuming the previous integration of Euler's equations.

Other modes of integration are employed; and the problem is also solved by seeking the *homogeneous strain* which will bring the body from any initial position to its position at time t.

This part of the paper concludes with the complete determination of q for the case of no forces and two equal moments of inertia.

The remainder of the paper deals with some simple cases of applied forces, when two moments of inertia are equal. If α denote a unit vector in the direction of the unequal axis of inertia, and if the motion be that of a heavy solid of revolution (such as a top) about a point in its axis, it is shown that

$$BV\alpha\ddot{\alpha} - A\Omega\dot{\alpha} = V\alpha\gamma$$

where γ is a constant vertical vector, and

$$\Omega = S\alpha\epsilon = \text{constant}.$$

This is the equation of motion of a simple pendulum disturbed by a force constantly perpendicular to the cone described by the string, and proportional to the rate at which the area of the surface of the cone is swept out by the string. The locus of the extremity of ϵ is shown to be a sphere fixed in space.

The problem of Precession and Nutation is next considered, and shown to depend on the integration of the very simple equation

$$BV\alpha\ddot{\alpha} - A\Omega\dot{\alpha} = \frac{3M}{T^5\rho}(A-B)S\alpha\rho V\alpha\rho .$$

where M is the mass, and ρ the vector, of the disturbing body.

The complete developments of the solutions of these equations are reserved for another occasion.

3. An Investigation into some previously undescribed Tetanic Symptoms produced by Atropia in Cold-Blooded Animals. By Dr Thomas R. Fraser.

Authorities in toxicology appear to agree in including convulsions among the effects of belladonna and of its active principle—atropia—on man. Convulsive and tetanic symptoms would appear to be also nearly constantly produced when fatal doses of this poison are administered to dogs, rabbits, and other animals, and to various birds. The recent remarkable progress of our knowledge of the exact and intimate physiological action of various medicinal substances is greatly due to investigations that have been made on animals of a lower type of organisation; and, accordingly, numerous observers have instituted experiments with atropia on such animals, and, especially, on frogs. Hitherto, however, tetanus has not been described as one of the effects of atropia-poisoning in cold-blooded animals.*

In some experiments undertaken to determine the minimum fatal dose of atropia for frogs, I was surprised to find that increased reflex excitability, convulsions, and tetanus occurred, occasionally, at a certain stage in the poisoning. Since first observing these unexpected symptoms, I have made a number of experiments to

* Since this was written, I have communicated with Dr John Harley of London (the author of several important papers on the physiological action and therapeutical employment of belladonna), and have had the pleasure of learning that he has also observed tetanus, and other symptoms of abnormal reflex activity, in frogs during protracted atropia-poisoning.

determine, accurately, the character of these convulsive effects, to ascertain the dose necessary for their production, and to differentiate, as far as possible, the structures on whose affection they depend.

Soon after a small fatal dose, or one rather less than fatal, of a salt of atropia is administered to a frog, a slight degree of weakness occurs in the anterior extremities, the respiratory movements of the chest cease, and the motor power becomes gradually more and more impaired, until, at length, all voluntary and respiratory movements cease, and the animal lies on the abdomen and chest, in a perfectly flaccid state. If the condition of the heart be now examined, it will be observed that the cardiac impulse is scarcely perceptible, and that the contractions are reduced to a very few in the minute. At this time, the application of various stimuli shows that the functions of the afferent and efferent nerves and of the spinal cord are retained, though in a greatly impaired condition. Several hours afterwards, it may be on the following day, the action of the poison is still further advanced; for the functions of the afferent and efferent nerves and of the spinal cord are completely paralysed, while only an occasional and scarcely perceptible cardiac impulse can be discovered, the only signs of vitality being this imperfect cardiac action and the continuing irritability of the striped muscles. This state may last for many hours or for several days—in one experiment it continued for as many as five days. Previous observers have apparently mistaken it for one of death, and have, therefore, failed to observe the symptoms that subsequently appear, and to which I wish more particularly to draw attention. The first of these symptoms is, usually, a change that occurs in the flaccid condition of the animal; the anterior extremities becoming flexed, and gradually more and more arched, until, at length, they are rigidly contracted, while tonic spasm occurs in the muscles of the chest also. At this time, a touch of any portion of the skin increases the tonic spasm of the anterior extremities and of the chest muscles, and causes some slight spasmodic movements in the posterior extremities. In varying periods after this, the respiratory movements reappear, and the cardiac impulse improves greatly in strength and in frequency, while the posterior extremities assume an extended position, with the webs stretched. If the skin be now touched, a violent attack of opisthotonic tetanus occurs, which may last for from two to ten seconds, and which is succeeded by a series of clonic spasms. During the tetanus, the posterior extremities are often more or less abducted, and immediately after it they become flaccid; but the anterior extremities almost always remain rigidly flexed. As a somewhat later period, tetanus of a still more violent character may be excited; the attacks are now emprosthotonic, and during them, the posterior extremities are rigidly extended, while at their conclusion, not only do the anterior extremities remain arched, but the head is bent downwards by tonic spasm of the muscles of the abdomen, chest, and neck.

A succession of such attacks may be produced by repeated touches of the skin, but, after a number have been excited in quick succession, the subsequent convulsions become shorter and rather less powerful, though they reacquire all their former violence after a period of rest.

When the animal is not suffering from an attack of tetanus, it may execute various movements, but these are performed with difficulty, even when they do not themselves excite spasms and convulsions, and it is apparent that the power of voluntary movement is still considerably impaired.

The period during which this tetanic condition remains was found to vary greatly in different experiments. It has been observed to continue for only a few hours, or for several days, and, in one experiment, for as long as fourteen days.

This description indicates the usual characters and sequence of the phenomena with such a dose of atropia as produces tetanus. Experiments have, however, been made in which the functions of the cerebro-spinal nervous system were not observed to be completely paralysed, in the stage of the poisoning antecedent to the appearance of tetanus. Only impairment of these functions was observed, but, as the state of flaccidity often lasts for several days, it is obviously impossible to make observations so frequently during this period as to authorize the assertion that total destruction did not occur.

It is almost superfluous to allude to the resemblance between the tetanic symptoms of atropia and those of strychnia. There are, however, certain peculiarities connected with the tetanus which atropia causes—altogether apart from the remarkable fact that this tetanus succeeds paralysis—which distinguishes it from that of strychnia. After poisoning with atropia, and during the stage of exaggerated reflex excitability, the attacks of tetanus cannot be excited by the very slight stimuli which are sufficient to do so in strychnia-poisoning. Various irregularities, also, are frequently met with in the tetanus of atropia. Some of these have been already described, and of the others it is sufficient to mention the occurrence of tonic spasm of one group of muscles in one limb, and of another group in another; of tetanus in the posterior extremities, with only slight increase of reflex excitability in the anterior; and of contractions of unequal force in the muscles at the sides of the chest and neck, causing lateral curvature during a tetanic convulsion.

The numerous experiments that have been made have so far solved the problem of the dose required to produce these remarkable phenomena, that they may now be almost unfailingly produced. Tetanus, or, at least, a state of greatly exaggerated reflex excitability, nearly invariably occurs when a dose of the sulphate or acetate of atropia, equivalent to the one-thousandth of the weight of the frog, is administered by injection, either under the skin or into the abdominal cavity. Doses varying from the one eight-hundredth to the one twelve-hundredth of the frog's weight, may also produce these effects. The larger doses always produce the most violent tetanic symptoms, if they are not fatal during the stage of paralysis; and they may be given with confidence to very small animals, and to such as have been kept in a laboratory for several months. The smaller doses are best adapted for large frogs, and for such as have been recently obtained from their natural habitats. If a dose be employed smaller than those above indicated, impairment of the functions of the cerebro spinal nervous system and of the heart may be caused, but general tetanus will not follow, although spasms restricted to certain regions may occasionally appear. The tetanic state resulting from the administration of the largest doses usually terminates in death, that from the smallest in recovery.

There are some special difficulties to be overcome in determining what structures are concerned in the production of this tetanic action of atropia; for in following the only available plan, that, namely, of preventing the poison reaching certain regions while it has access to others, it is essential to remember that important fallacies might arise because of the long interval that often elapses between the administration of the poison and the appearance of tetanus. Experiments were made in some of which the blood-vessels of one posterior extremity, and in others of both, were tied before atropia was administered, and, by frequently modifying the dose, tetanus was on several occasions produced sufficiently soon to give results that were not materially influenced by the previous ligature of vessels. It was observed, in these experiments, that spasms and tetanus occurred in the limbs to which the access of the poison had been prevented, during the stage in which the nerves of the poisoned regions were regaining their functions. This is sufficient to demonstrate that the tetanus does not depend on an action on motor or sensory nerves, nor on muscles; and it is, therefore, apparent that it must depend on an action on the central nerve-organs. The predominence of cerebral symptoms during atropia-poisoning in animals of a higher development, suggested the possibility of the tetanic symptoms being caused in frogs by an influence originating in the cerebral lobes, or, more probably, in the ganglia at the summit of the medulla. Accordingly, on several occasions, the cord of a frog in the stage of tetanus was divided immediately below the brachial enlargement. After this operation, however, the tetanic condition continued in both the anterior and posterior segments. Violent tetanus could be readily excited in either segment; and this condition frequently lasted for many days after the division of the cord.

There can, therefore, be no doubt that these tetanic symptoms are caused by an action of atropia on the spinal cord.

4. On *Rhabdopleura*, a New Genus of Polyzoa. By Professor Allman.

Professor Allman described a new genus of Polyzoa, obtained by the Rev. A. M. Norman and Mr J. Gwyn Jeffreys, from deep-sea dredgings in Shetland.

Its cœnœcium consists of a branched tube, partly adherent and partly free, the free portion forming tubes of egress, through which the polypides move in the acts of exsertion and retraction. In the walls of the adherent portion a rigid chitinous rod is developed along their attached side, and to this rod the polypides are connected from distance to distance each by a flexible cord or funiculus.

The polypides are hippocrepian, and each carries a shield-like process on the hæmal side of its lophophore external to the tentacular series.

The development of the bud was traced, and it was shown that in an early stage the polypide is included between two fleshy plates, which are placed, one on the right and the other on the left side, and are united to one another along a portion of their circumference, while they are disunited along the rest. For some time the two plates keep pace with the general development of the bud, but ultimately they cease to increase in size, and then remain as the shield-like process carried by the lophophore of the adult polyzoon.

The author regarded these plates as representing the right and left lobes of the mantle in a Lamellibranchite mollusc, from which it followed that the relations of the Polyzoa are more intimate with the Lamellibranchiata than with the Brachiopoda, with which of late years they had been associated, but whose mantle lobes lie dorsally and ventrally, instead of lying right and left, as in the Lamellibranchiata. The lophophore of the Polyzoa was considered by the author as having its representative in the labial palps of the Lamellibranchiata.

The following were given as the generic and specific diagnosis of the new Polyzoon:—

Genus Rhabdopleura, *Allman.*

Cœnæicum consisting of a branched adherent membranous tube, in whose walls along their adherent side a rigid chitinous rod extends, and whose branches terminate each in a free open tube through which the polypide emerges.

Lophophore hippocrepial, with a shield-like process on the hæmal side of the tentacles. Polypides connected to the chitinous rod by a flexible cord or funiculus.

Rhabdopleura Normani, Allman.

Cœnœcium sub-alternately branched, delicate, transparent, and colourless; free portion of the cœnœcial tubes of the same diameter as the adherent portion, and very distinctly and regularly annulated.

Habitat.—Creeping over the surface of dead shells, from a depth of 93 fathoms.

Locality.—Shetland seas. J. Gwyn Jeffreys, Esq., and Rev. A. M. Norman.

Though we cannot expect, in spirit specimens, to demonstrate by direct observation the presence of an epistome, we may yet take for granted that Rhabdopleura, like all other polyzoa with hippocrepian lophophores, is provided with this organ, and belongs to the Order Phylactolæmata. Its structure, however, is so peculiar as to justify us in assuming it as the type of a special section of the Order, which may be thus divided—

POLYZOA PHYLACTOLÆMATA { *Scutata*, Rhabdopleura.
Inermia, Cristatella, Plumatella, &c. }

The following Gentlemen were balloted for and elected Fellows of the Society:—

OLIVER G. MILLER, Esq., Panmure House, Forfarshire.
JOHN L. DOUGLAS STEWART, Esq. of Nateby Hall.
ALEXANDER BUCHAN, Esq., M.A.
Professor FLEEMING JENKIN.
WILLIAM DICKSON, Esq.
JOHN PENDER, Esq.

The following Donations to the Library were announced:—

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——— of the Printed Books and Manuscripts in the Library of the New College, Edinburgh. 1868. 4to.—*From the College.*

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Journal (American) of Science and Arts. Conducted by Benjamin Silliman. Nos. 135, 136, 137. New Haven. 8vo.—*From the Editor.*

Karajan (Th. G. von). Das Verbrüderungs buch des Stiftes S. Peter zu Salzburg aus dem achten bis dreizehnten Jahrhundert mit Erlänterungen. Wien, 1852. Fol.—*From the Author.*

Lawson's Pinetum Britannicum. Parts XXIX. XXX. Imp. fol.—*From Charles Lawson, Esq.*

Meiller (Andreas von). Regesten zur Geschichte der Markgrafen und Herzoge Österreichs aus dem Hause Babenberg. Wien, 1850. 4to.—*From the Author.*

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Nouvelles Archives du Muséum d'Histoire Naturelle de Paris. Tome III. IV. 4to.—*From the Museum.*

Philadelphia.—Philadelphia Journal of the Academy of Natural Sciences of Philadelphia. New series. Vol. VI. Part 2. 4to.—*From the Academy.*

Proceedings of the Academy of Natural Sciences of Philadelphia. Nos. 1–4. 1867. 8vo.—*From the Academy.*

Proceedings of the American Philosophical Society. Vol. X. No. 77. 8vo.—*From the Society.*

Salem (U.S.)—Proceedings of the Essex Institute. Vol. V. Nos. 5 and 6. 8vo.—*From the Institute.*

St Petersburg.—Bulletin de l'Académie Impérial des Sciences de St Pétersbourg. Tome XII. Nos. 2–5. 4to.—*From the Academy.*

Mémoires de l'Académie des Sciences de St Pétersbourg. VII^e^ serie. Tome XI. Nos. 9–18. 4to.—*From the Academy.*

Switzerland.—Verhandlungen der Schweizerischen Naturforschenden Gesellschaft in Rheinfelden. Am. 9, 10, and 11 Sept. 1867. 8vo.—*From the Society.*

Toronto.—Canadian Journal of Industry, Science, and Art. New series. No. 66. 8vo.—*From the Canadian Institute.*

Upsala.—Nova Acta Regiæ Societatis Scientiarum Upsaliensis. Vol. VI. Fasc 2. 4to.—*From the Society.*

Victoria.—Statistical Notes on the Progress of Victoria in relation to Agriculture and Live Stock, from 1835 to 1867. Melbourne. 4to.—*From the Registrar-General of Victoria.*

Victoria.—Statistical Register of Victoria, with Astronomical Calendar for 1855. Melbourne, 1854. 8vo.—*From the Registrar-General.*

Statistical Summary of the Progress of the Colony of Victoria to the Year 1865. Melbourne, 1865. 8vo.—*From the Registrar-General.*

Statistics of the Colony for 1867. Parts 1 and 2. Melbourne. Fol.—*From the Registrar-General.*

Statistical Tables of the Colony of Victoria. Fol. Melbourne, 1865.—*From the Registrar-General.*

Vienna. — Jahrbuch der Kaiserlich-Königlichen Geologischen Reichsanstalt. Band XVII. No. 4; Band XVIII. Nos. 1, 2. 8vo.—*From the Society.*

Verhandlungen der Kaiserlich-Königlichen Geologischen Reichsanstalt. 1867, Nos. 13–18; 1868, Nos. 1–10. —*From the Society.*

Verhandlungen der Kaiserlich-Königlichen Zoologisch-Botanischen Gesellschaft in Wien. Band XVII. 8vo. —*From the Society.*

Die Fossilen Mollusken des Tertiär-beckens von Wien von Dr Moritz Hörnes. 4to.—*From the Geol. Society of Vienna.*

Denkschriften der Kaiserlichen Akademie der Wissen schaften. Math.-Nat. Classe. Band XXVII. 4to.—*From the Academy.*

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften. — Math.-Nat. Classe (Mineralogie, Botanik). Band LVI. Heft 2–5.—Math.-Nat. Classe. B. LVI. Heft 3–5.—Phil.-Hist. Classe. B. LVI. Heft 3; B. LVII. Heft 1. 8vo.—*From the Academy.*

Washington.—Astronomy. Astronomical and Meteorological Observations made at the United States Naval Observatory, during 1865. 4to. Washington, 1867.—*From the United States Government.*

Discussion of Meteorological Phenomena observed at the U.S. Naval Observatory, from June 1842 to January 1867. 4to.—*From the Observatory.*

Monthly Reports of the Department of Agriculture for 1866-67. 8vo.—*From the Commissioner.*

Washington.—Report of the Commissioner of Agriculture for 1866. 8vo.—*From the Commissioner.*

Twenty-second Annual Report of the Board of Trustees of the Public Schools of the City of Washington. 1867. 8vo.—*From the Trustees.*

Report of the Board of Regents of the Smithsonian Institution for 1866. 8vo.—*From the Institution.*

Smithsonian Contributions to Knowledge. Vol. XV. 4to. *From the Smithsonian Institution.*

Tschudi (J. J. von). Die Kechua-Sprache. Abtheilung I. II. III. Vienna, 1853. 8vo.—*From the Author.*

Vogel (August). Denkrede auf Heinrich August von Vogel. Munich, 1868.

Voit (Carl). Ueber die Theorien der Ernährung der thierischen Organismen. Munich, 1868. 4to.—*From the Author.*

Watson (J. Forbes), A.M., M.D. Index to the Native and Scientific Names of Indian Plants and Products. London, 1868. 4to.—*From the Author.*

Will (H.). Jahresbericht über die Fortschritte der Chemie, etc., Register zu den Berichten für. 1857 bis 1866. 8vo.—*From the Editor.*

Winnertz (Joh.). Beitrag zu einer Monographie der Sciarinen. Wien, 1867. 8vo.—*From the Author.*

Ximenez (El.R.P.F.F.). Las Historias del Origen de los Indios de esta Provincia de Guatemala. Vienna, 1857. 8vo.—*From the Author.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VI. 1868–69. No. 78.

At the first ordinary meeting of the Society on 7th December, the Makdougall-Brisbane Prize, for the Biennial Period 1866–68, was awarded to Dr Alexander Crum Brown and Dr Thomas Richard Fraser, for their paper on the Connection between Chemical Constitution and Physiological Action.

At the same meeting the Neill Prize, for the Triennial Period 1865–68, was awarded to Dr William Carmichael M'Intosh, for his paper on the British Nemerteans and on some new British Annelids.

Monday, 4th January 1869.

Dr LYON PLAYFAIR, C.B., M.P., Vice-President, in the Chair, who said—

It was my painful duty last year to allude to the death of that great philosopher, Sir David Brewster, and within a few months we have now to deplore the loss of another philosopher, also great —I need not say that I allude to Principal Forbes. This Society is intimately identified with his life and labours. Long our Secretary, he did all that was in his power to promote its success. As a man of science, we are too intimate in this place with his researches to render it necessary that, on the present occasion, I should make a detailed allusion to them. His early and his latest researches were upon heat. He established the polarisation and double refraction of the heat ray, and proved the identity of thermal and luminous radiations. His last published research is upon the conduction of heat by iron. In this he has established that, like electricity, heat passes more slowly through a bar of elevated temperature than through one which is cold. He also showed how the absolute conductivity of a metal for heat might be measured.

But it is probably in connection with his researches on glaciers that Forbes's name is best known to the general public. His first Memoir on the veined or ribboned structure of Ice was published in our Transactions in December 1841. Reading that memoir with the light of recent researches, we rise profoundly impressed with the accurate observations and clearness of judgment of their author. As he himself observes in that paper, it is astonishing how little we see until we are taught how to observe. This veined structure of glaciers is intimately connected with their mode of formation, and with the remarkable phenomena which render them so interesting to all investigators; and yet no one observed with the eyes of science this important veined structure until Forbes described it. Even with the new experiments of James Thomson on the lowering of the freezing point of ice under pressure, and of those of his brother, Sir William Thomson, on the rupture produced in a viscous solid by continued shearing, we could scarcely at the present day observe the phenomena more accurately than was done by Forbes twenty-nine years ago, or connect them more lucidly with the occurrence and position of the cracks and crevasses of the glacier.

But this and all his subsequent researches on the motions of glaciers as a viscous mass exhibit the peculiar characteristic of Forbes's mind—scrupulous conscientiousness in his scientific labours, scrupulous conscientiousness in his life as a man.

It is not my duty to say more than I have done; but at the first meeting of the Society which has occurred after his death, I thought it right to allude to our own loss; and I now move that the Society instruct the Council to express to Mrs Forbes and her family our sympathies for their bereavement, and our sense of the loss which science has sustained by the death of this distinguished philosopher.

Professor Jenkin, at the request of the Council, delivered an Address on Cable Testing.

The following Communications were read:—

1. Notice of a Heart in which the Superior Vena Cava possessed a Valve at its Auricular Orifice. By Professor Turner.

Whilst examining the heart in the body of one of the male subjects undergoing dissection in my practical rooms, in the early part of this session, the large size and fenestrated condition of the Eustachian valve attracted my attention. When the heart was removed from the body, and the auricle submitted to a more careful scrutiny, I observed that a valve was situated at the mouth of the superior vena cava.

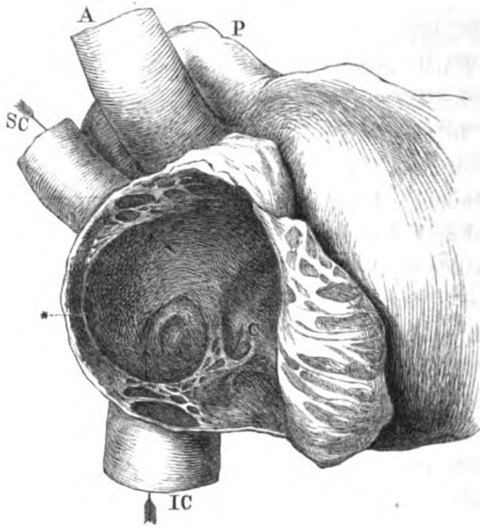
As I have not, up to this present time, seen any record in the various anatomical works which I have consulted of the existence of a valve in this locality, I am induced to bring the specimen before the notice of the Society.

A membranous valve, formed by a reduplication of the endocardial lining membrane, lay across the anterior and inner border of the auricular orifice of the superior vena cava, and hung pendulous in the auricular cavity. It measured 1⅓ inch in its long or transverse diameter, but was scarcely ½ an inch deep; so that when drawn across the mouth of the vein, it did not cover over much more than one third of the orifice. Its free border was almost straight. The attached border was semilunar in form, and connected to the wall of the auricle, close to its line of junction with the anterior wall of the vein, not by a continuous membrane, but by numerous slender fibrous bands. Between these bands were apertures of various sizes, one of which, larger than the rest, was situated at the upper and inner part of the valve, which consequently had a fenestrated appearance. From the outer (right) end of the valve, and continuous with its free border, a fibrous cord (*) arched downwards beneath the lining membrane of the right wall of the auricle, and became continuous with the right border of the Eustachian valve.* The inner (left) end of the valve was connected to a short papillary muscle, which was continuous with the muscular wall of

* Since the specimen was shown to the Society I have recognised in two hearts, which have come under my notice in my dissecting room, a similar fibrous cord, extending from the Eustachian valve along the right posterior

the auricle. There did not appear to be any deficiency in the thickness of the muscular coat of the superior cava.

The Eustachian valve projected for upwards of an inch into the auricular cavity, and presented in a remarkable degree the fenestrated character which that valve occasionally exhibits in the adult heart. At its left extremity it subdivided into two parts, one of which passed in the usual way to the auricular septum, and became continuous with the annulus ovalis, whilst the other was blended with the valve at the mouth of the coronary sinus, which also exhibited a fenestrated appearance.



A. Aorta. P. Pulmonary artery. SC. Superior, and IC. Inferior cava. * The fibrous cord connecting the two valves. C Coronary sinus.—*From a drawing of the specimen by Mr T. D. Nicholson.*

Owing to its fenestrated condition, and small size, when compared with the orifice of the superior cava, it is obvious that the valve situated at the mouth of that great vein could have had but little influence in preventing the regurgitation of blood during the contraction of the auricle in the adult heart. But it is probable that in the fœtal stage of this heart the backward flow

wall of the auricle, almost as far as the orifice of the superior cava, but no valve existed at the mouth of the superior cava.

into the vein would have been very considerably impeded by its presence; for there is reason to believe that the fenestrated state, not only of this, but of the other valves at the mouths of the great veins, is due to atrophy taking place after birth. As the muscular coat of the superior cava possessed its usual thickness, the valve was obviously not developed to compensate for any deficiency in that portion of the wall of the great vein.

As the Eustachian valve at the mouth of the inferior cava serves in the fœtus to direct the current of blood passing upwards along that vein through the foramen ovale, it is possible that the valve at the mouth of the superior cava may have exercised some directing effect on the blood which entered the auricle by the latter vessel. From its position it would, I think, have directed the blood of the superior cava away from the auricular septum, and thus have aided in preventing, during the fœtal condition, the mingling of the blood of the two cavæ in the auricular cavity.

The occurrence of such a valve is not, however, of interest merely in its physiological relations: it possesses also a morphological value. For it may be regarded as presenting in the human heart a rudimentary example of an arrangement which is met with in the heart of the bird. If the heart of a large bird, *e.g.*, the ostrich (*Struthio camelus*), be examined, it may be seen that the sinus, into which the venæ cavæ open, is separated from the auricle proper by a large double muscular valve. The right segment of this valve is related not only to the mouth of the right superior cava, but extends down the wall of the auricle to the mouth of the inferior cava, and is then prolonged as far as the mouth of the left superior cava, which may be regarded as representing in position the coronary vein in the human heart. Now, in this specimen it will be remembered that the valve at the mouth of the superior cava was continued, through the intermediation of a fibrous cord (*), into the Eustachian valve, and that the latter again was directly united with the valve at the mouth of the coronary sinus.

As additional illustrations of the tendency to the development of rudimental structures in this individual, it is of interest also to mention that he had in each upper arm a *processus supra-condyloideus humeri internus*, the relations of which to the supra-condyloid foramen of various of the mammalia, more especially of the carnivora, was described in 1839 by A. W. Otto, and in 1840 and 1841 by Robert Knox, and many examples of which have since that time been recorded by other anatomists. From the tip of this process a band of fascia passed down to the inner condyle, so as to complete the boundary of the foramen, through which the median nerve and the ulnar artery passed; for in both upper arms a high division of the brachial artery had taken place; and whilst the radial branch closely followed the inner border of the biceps, the ulnar was deflected from its course, and passed, along with the median nerve, behind the supra-condyloid process.

2. On the Motion of a Pendulum affected by the Rotation of the Earth and other Disturbing Causes. By Professor Tait.

(*Abstract.*)

1. Let α be the vector (from the earth's centre) of the point of suspension, λ its inclination to the plane of the equator, a the earth's radius drawn to that point; and let the unit vectors i, j, k be fixed in space, so that i is parallel to the earth's axis of rotation; then, if ω be the angular velocity of that rotation

$$\alpha = a\left[i\sin\lambda + (j\cos\omega t + k\sin\omega t)\cos\lambda\right] \quad . \quad . \quad (1).$$

This gives

$$\dot{\alpha} = a\omega(-j\sin\omega t + k\cos\omega t)\cos\lambda$$
$$= \omega\,\mathrm{V}i\alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

Similarly

$$\ddot{\alpha} = \omega\,\mathrm{V}i\dot{\alpha} = -\omega^2(\alpha - ai\sin\lambda) \quad . \quad . \quad . \quad . \quad (3).$$

2. Let ρ be the vector of the bob m referred to the point of suspension, R the tension of the string, then if α_1 be the direction of pure gravity

$$m(\ddot{\alpha} + \ddot{\rho}) = -mg\,\mathrm{U}\alpha_1 - \mathrm{RU}\rho \quad . \quad . \quad . \quad . \quad (4),$$

which may be written

$$\mathrm{V}\rho\ddot{\alpha} + \mathrm{V}\rho\ddot{\rho} = \frac{g}{\mathrm{T}\alpha_1}\mathrm{V}\alpha_1\rho \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

To this must be added, since r (the length of the string) is constant,

$$\mathrm{T}\rho = r \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6),$$

and the equations of motion are complete.

3. These two equations (5) and (6) contain every possible case of the motion, from the most infinitesimal oscillations to the most rapid rotation about the point of suspension, so that it is necessary to adapt different processes for their solution in different cases. In this abstract we take only the ordinary Foucault case, to the degree of approximation usually given.

4. Here we neglect terms involving ω^2. Thus we write

$$\ddot{\alpha} = 0,$$

and we write α for α_1, as the difference depends upon the ellipticity of the earth. Also, attending to this, we have

$$\rho = -\frac{r}{a}\alpha + \varpi \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

where (by (6))

$$S\alpha\varpi = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8),$$

and terms of the order ϖ^2 are neglected.

With (7), (5) becomes

$$-\frac{r}{a}V\alpha\ddot{\varpi} = \frac{g}{a}V\alpha\varpi\;;$$

so that, if we write

$$\frac{g}{r} = n^2\,, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

we have

$$V\alpha(\ddot{\varpi} + n^2\varpi) = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10).$$

Now, the two vectors

$$\alpha i - \alpha\sin\lambda \quad \text{and } Vi\alpha$$

have, as is easily seen, equal tensors; the first is parallel to the line drawn horizontally *northwards* from the point of suspension, the second horizontally *eastwards.*

Let, therefore,

$$\varpi = x(\alpha i - \alpha\sin\lambda) + y\,Vi\alpha \quad . \quad . \quad . \quad . \quad . \quad (11),$$

which (x and y being very small) is consistent with (6.)

From this we have (employing (2) and (3), and omitting ω^2)

$$\dot{\varpi} = \dot{x}(\alpha i - \alpha\sin\lambda) + \dot{y}\,Vi\alpha - x\omega\sin\lambda\,Vi\alpha - y\omega(\alpha - \alpha i\sin\lambda),$$

$$\ddot{\varpi} = \ddot{x}(\alpha i - \alpha\sin\lambda) + \ddot{y}\,Vi\alpha - 2\dot{x}\omega\sin\lambda\,Vi\alpha - 2\dot{y}\omega(\alpha - \alpha i\sin\lambda).$$

With this (10) becomes

$$V\alpha\left[\ddot{x}(\alpha i - \alpha\sin\lambda) + \ddot{y}\,Vi\alpha - 2\dot{x}\omega\sin\lambda\,Vi\alpha - 2\dot{y}\omega(\alpha - \alpha i\sin\lambda) + n^2x(qi - \alpha\sin\lambda) + n^2y\,Vi\alpha\right] = 0$$

or, if we note that

$$\mathrm{V}.a\,\mathrm{V}ia = a(ai - a\sin\lambda),$$

$$(-\ddot{x} - 2\dot{y}\omega\sin\lambda - n^2x)a\,\mathrm{V}ia + (\ddot{y} - 2\dot{x}\omega\sin\lambda + n^2y)a(ai - a\sin\lambda) = 0.$$

This gives at once

$$\left.\begin{array}{l}\ddot{x} + n^2x + 2\omega\,\dot{y}\sin\lambda = 0\\ \ddot{y} + n^2y - 2\omega\,\dot{x}\sin\lambda = 0\end{array}\right\} \quad . \; . \; . \; . \; . \; . \; . \; (12),$$

which are the equations usually obtained; and of which the solution is as follows:—

If we transform to a set of axes revolving in the horizontal plane at the point of suspension, the direction of motion being from the positive (northward) axis of x to the positive (eastward) axis of y, with angular velocity Ω, so that

$$\left.\begin{array}{l}x = \xi\cos\Omega t - \eta\sin\Omega t\\ y = \xi\sin\Omega t + \eta\cos\Omega t\end{array}\right\} \quad . \; . \; . \; . \; . \; . \; . \; (13),$$

and omit the terms in Ω^2 and in $\omega\Omega$ (a process justified by the results, *see* equation (15)), we have

$$\left.\begin{array}{l}(\ddot{\xi} + n^2\xi)\cos\Omega t - (\ddot{\eta} + n^2\eta)\sin\Omega t - 2\dot{\eta}(\Omega - \omega\sin\lambda) = 0\\ (\ddot{\xi} + n^2\xi)\sin\Omega t + (\ddot{\eta} + n^2\eta)\cos\Omega t + 2\dot{\xi}(\Omega - \omega\sin\lambda) = 0\end{array}\right\} \quad (14).$$

So that, if we put

$$\Omega = \omega\sin\lambda \quad . \; . \; . \; . \; . \; . \; . \; . \; (15),$$

we have simply

$$\left.\begin{array}{l}\ddot{\xi} + n^2\xi = 0\\ \ddot{\eta} + n^2\eta = 0\end{array}\right\} \quad . \; . \; . \; . \; . \; . \; . \; (16),$$

the usual equations of elliptic motion about a centre of force in the centre of the Ellipse.

5. In the paper this problem is treated with a closer approximation, terms in ω^2, &c., being retained. The conical pendulum—the path of the bob being very nearly a horizontal circle, *i.e.*, the tension of the cord being nearly constant—is next treated; then the case of very great angular velocity, when the path is nearly a circle (in any plane) with centre at the point of suspension. A few sections are devoted to the consideration of the effect of a disturbing body, such as the moon or the sun.

The following Gentlemen were elected Fellows of the Society:—

Isaac Anderson-Henry, Esq.
George Elder, Esq.
Sir Charles A. Hartley, C.E.
David MacGibbon, Esq.
Rev. Thomas Melville Raven, M.A.
Alexander Howe, Esq.
Viscount Walden.
Professor Alexander Dickson.

Monday, 18th January 1869.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Connection between Chemical Constitution and Physiological Action.

On the Physiological Action of the Salts of the Ammonium Bases derived from Atropia and Conia. By Dr A. Crum Brown and Dr Thomas R. Fraser.

Atropia.—Atropia is a nitrile base. All we know of its constitution is, that by the action of strong acids and bases it is decomposed, in accordance with the equation.*

$$\underset{\text{Atropia.}}{C_{17}H_{23}NO_3} + \underset{\text{Water.}}{H_2O} = \underset{\text{Tropic Acid.}}{C_9H_{10}O_3} + \underset{\text{Tropia.}}{C_8H_{15}NO}$$

So that atropia may be considered as tropia, in which one atom of hydrogen has been replaced by tropyl, the radical of tropic acid. Tropic acid belongs to the aromatic series, and is considered by Kraut to be phenylsarcolactic acid—$HO \cdot CH_2 \cdot CH(C_6H_5)COOH$. Of the constitution of tropia we know nothing whatever, except that it is a nitrile base.

Iodide of methyl-atropium.—Iodide of methyl acts very readily on atropia, a good deal of heat is produced, and after the reaction is over, the iodide of methyl-atropium remains as a white mass. From this the excess of iodide of methyl was removed by a current of air, and the dry salt dissolved in water, filtered, and evaporated at a temperature not exceeding 40° C. The concentrated solution thus obtained, on cooling, deposits the salt in prismatic crystals, apparently belonging to the monoclinic system; sometimes part of

* Kraut.—Annalen der Ch. u. Ph., cxxviii. 280, cxxxiii. 87, cxlviii. 238. Lossen.—*Ib.*, cxxxi. 43, cxxxviii. 230.

the salt separates as a heavy oil, which soon crystallises. These crystals have the composition $C_{17}H_{23}NO_3CH_3I$. They are tolerably stable, bearing a temperature of 100° C. without much alteration. When they are powdered, or when their solution is warmed, a pleasant fruity smell is observed.

Sulphate of methyl-atropium.—The sulphate was prepared from the iodide by the method formerly described for the preparation of sulphate of methyl-strychnium, &c. It is a white crystalline salt, very deliquescent, and very soluble in water.

Iodide of ethyl-atropium.—Iodide of ethyl acts readily on atropia, but not so energetically as iodide of methyl. In preparing the iodide of ethyl-atropium, atropia was treated with a considerable excess of iodide of ethyl in sealed tubes at 100° C. for an hour. The remainder of the process, and the preparation of the sulphate, are the same as in the case of the methyl derivative.

The authors intend, on some future occasion, to describe these substances more minutely; for their present purpose, the description given above seems sufficient.

Atropia has a somewhat complicated physiological action, for it directly influences the functions of the cerebro-spinal, and also of the sympathetic nervous system.

The action of the methyl and ethyl derivatives on the cerebro-spinal nervous system is different from that of the natural base, while the action on the sympathetic system is essentially the same.

The principal effects produced by atropia on the cerebro-spinal nervous system are excitation of the spinal cord,* and paralysis of the motor and sensory nerves. In a previous paper, the authors showed that the spinal stimulant action of strychnia, brucia, thebaia, codeia, and morphia, is not possessed by the salts of the ammonium bases derived from these alkaloids, but that in its place these derivatives possess a markedly different paralysing action on the peripheral terminations of motor nerves. They now announce that a similar change occurs in the methyl and ethyl derivatives of atropia. These derivatives are more powerful paralysing substances than atropia itself.

A considerable amount of spinal stimulant and of paralysing action may be produced by a non-fatal dose of atropia; and it is

* See Proced. Roy. Soc. of Ed. vol. vi. 1868–69, p. 434.

probable that the one action is, to a certain extent, antagonistic to the other. As the methyl and ethyl derivatives, however, combine with the ordinary paralysing action of atropia, an additional amount of paralysing action bearing some ratio to the absent spinal stimulant action of the natural base, these derivatives affect the motor nerves much more powerfully than atropia itself. Probably, for these reasons, the salts of methyl- and ethyl-atropium are fatal to the lower animals in much smaller doses than the salts of atropia itself.

Paralysis of the vagi nerves and dilatation of the pupil are caused by these derivatives of atropia.

Conia.—The alkaloid prepared from *Conium maculatum* (hemlock) has been shown by Von Planta and Kekulé* to be a variable mixture of two bases, to which they give the names of "conia" and "methyl-conia." These bases resemble one another very closely in physical properties. Their composition is represented by the formulæ $C_8H_{15}N$ and $C_9H_{17}N$. The chemists above named investigated very completely the action of iodide of ethyl on conia, and proved that conia (or, as it is called in the present paper, *normal conia*) is an imide base, and that methyl-conia is a nitrile base.

The substances examined in the present paper are:—

1*st*, *Conia*—samples of which were obtained from Messrs Duncan and Flockhart, Macfarlan & Co., and Morson. The authors are also indebted to the kindness of Dr Christison for the opportunity of examining the action of a specimen of conia which he prepared in the year 1835.

2*d*, *Methyl-conia*—prepared from hydriodate of methyl-conia, produced by the union of iodide of methyl and *normal conia.*

And 3*d*, *Salts of dimethyl-conium*—obtained by the union of iodide of methyl and the methyl-conia contained in conia as obtained from the plant.

Iodide of methyl acts readily upon conia, producing a syrupy or crystalline substance, which is a mixture of hydriodate of methyl-conia and iodide of dimethyl-conium,—the former produced from the normal conia, and the latter from the methyl-conia. The action of caustic potash decomposes the hydriodate of methyl-conia, setting the base free, while the iodide of dimethyl-conium is unattacked. The two substances can thus be readily separated from each other.

* Annalen der Chemie und Pharmacie, lxxxix. 5.

The authors find that the salts of conia and of methyl-conia very closely resemble each other in action and in poisonous activity. Their action agrees with the descriptions of the effects of conia by the more trustworthy of previous observers. Among the most obvious of the effects on rabbits were stiffness of the limbs, causing difficulty in moving about; spasmodic starts; distinct increase of reflex excitability; gradually increasing paralysis with diminution, and afterwards disappearance, of the increased reflex excitability; and, finally, death by asphyxia. Shortly before death a few starts and feeble convulsions usually occurred, but these symptoms were apparently caused by the advancing asphyxia.

The symptoms in frogs were mainly those of paralysis, and the authors confirm the observations of Kölliker and Guttmann, that this paralysis in the case of ordinary conia is due to a curare-like action. They further find that methyl-conia also acts by paralysing the terminations of motor nerves.

The salts of dimethyl-conia differ from those of conia and of methyl-conia in never directly producing convulsant effects or other symptoms of abnormal activity of the reflex function, and in being much less active as poisons. In rabbits and frogs, the symptoms were invariably those of paralysis; and, in the latter animal, the authors have demonstrated that this paralysis is due to an action on the terminations of the motor nerves.

The samples of conia which have been examined by the authors were found to contain very varying proportions of normal conia and of methyl-conia; but as these two substances appear to be about equally active as poisons, it is probable that the very variable potency of commercial conia is due to its adulteration with a greater or less amount of water, and, possibly, also to the presence of varying quantities of ammonia.*

* When the authors had nearly concluded their investigation on conia, they received a communication from MM. Jolyet and Cahours of Paris, informing them that these physiologists were ready to publish a paper upon the relative action of the salts of conia, ethyl-conia, and diethyl-conium. In order to secure simultaneous publication, it was arranged that the two papers should be communicated on the same day—the one to the Academy of Sciences of Paris, and the other to this Society.

2. Note on the Determination of Heights, chiefly in the Interior of Continents, from Observations of Atmospheric Pressure. By Alexander Buchan, M.A., Secretary of the Scottish Meteorological Society.

The weight or pressure of the atmosphere is ascertained by the mercurial barometer, the aneroid, or from the temperature of the boiling point of water. The height of a hill is measured barometrically, from observations made simultaneously at its base and top, and the application of certain well-known formulæ. The height of a place at no great distance from another place whose height is known, and at which observations are made about the same time, may similarly be ascertained with a close approximation to the truth.

But, with regard to places far from any place of known elevation, or from any place at which meteorological observations are made, it is plain that the height can only be computed by assuming a certain pressure as the sea-level pressure at that place.

In the Table giving the reductions of heights from Captain Speke's observations, it is stated (Journal of the Royal Geographical Society, vol. xxxiii.) that a mean pressure of 29·92 inches was assumed as the mean sea-level pressure,—that is, if those parts of Africa visited by Speke had been on the same level with the sea, it is assumed that the mean pressure of the atmosphere would have been 29·92 inches.

In the last revised "Hints to Travellers," prepared by the Royal Geographical Society, and published in the Journal, vol. xxxiv., it is stated at page 286, "When the boiling point at the upper station alone is observed, we may assume 30·00 inches, or a little less, as the average height of the barometer at the level of the sea. The altitude of the upper station is then at once approximately obtained from the tables." So far as I have been able to ascertain, this mean height of the barometer has been generally accepted by travellers as applicable to all seasons, and to all parts of the globe at great distances from Meteorological Observatories. Unfortunately, it has hitherto been generally the practice for travellers, or those who have been intrusted with reducing their observations, to give only the heights deduced from the observations, with a curious minuteness of accuracy, and not the observations themselves. Since the tables which have been prepared for travellers are calculated on the assumption that 29·92 inches, or 30·0 inches, is the zero point for heights, there can be little doubt that, by this method, the heights of many plateaux and mountains of the globe have thus been determined.

From my paper, read before this Society in March 1868, on the Mean Pressure of the Atmosphere over the Globe, illustrated with three charts, showing the *Mean Isobaric Curves* for July, January, and the year, it may be seen that a pressure of from 29·9 to 30·0 inches is very near the mean annual pressure over the greater part of the globe, particularly over those portions of it explored by travellers. But when we examine the months, it is at once apparent that 29·9 inches is very far from the mean pressure in many regions. This point will be illustrated by the pressures at Barnaul, Siberia, which on an average of 19 years are, reduced to 32° and sea-level, as follows:—

Mean atmospheric pressure at Barnaul in	July,	29·536 inches.
" " "	January,	30·293 "
" " "	Year,	29·954 "

Suppose, now, it be proposed to ascertain the height of Lake Balkash on some day in July, the pressure at the time being the average of the month. Let the observed pressure be 28·8 inches reduced to 32° F., and the temperature of the air be 70°·0, then if the sea-level pressure be assumed to be 29·9 inches, it is plain that the difference due to height is 1·10 inches; in other words, the height of the lake would be, in round numbers, 1080 feet. But since the sea-level pressure of this locality, which is nearly that of Barnaul, is 29·536 inches, the difference of pressure due to height is only 0·736 inch; the height, therefore, is only about 730 feet. Again, if in January, when the barometer is the mean of the month, the pressure at Lake Balkash was observed to be 29·42 inches, and the temperature of the air 1°·0, assuming that 29·9 inches is the mean sea-level pressure of January, 0·48 inch is the difference of pressure due to height—that is, the lake is about 400 feet above the sea. But since the mean pressure is nearly 30·3 inches, 0·88 inch is the pressure due to height; the lake is therefore nearly 730 feet above the sea. Thus in July the lake would be made 350 feet too high, and in January 330 feet too low—the difference of the two observations, each being here supposed to be taken under the most favourable circumstances, and with the greatest accuracy, being 680 feet.* Observations made in the first half of April, or in the latter half of October, when the pressure is the mean of the year, supply the best data for the calculation of heights.

If the best physical atlases be examined, and the heights, given by different authorities, of table-lands and mountains, of Central Asia, Central Africa, and the highlands of the United States and British America be compared, considerable confusion will be found to prevail.

One or two examples may be given to show the application of all this. From barometric observations made on the 28th November 1838, the level of the Dead Sea below that of the Mediterranean was calculated to be 1429 feet. The real depth of this sea below the level of the Mediterranean, as determined by the English engineers by levelling, is 1296 feet. Now, since the mean pressure of the atmosphere over the region of the Dead Sea in the end of November is about 30·035 inches, it is seen, if the sea-level pressure was assumed to be 29·9 inches, how the lake came to be lowered 133 feet.

Much interest is at present attached to the heights of Central Africa. The following mean pressures at 32° and sea-level bear on this interesting question:—

	January. inches.	July. inches.
Malta	30·07	30·01
Algiers	30·15	30·06
Laghouat (Algeria)	30·07	29·86
Gibraltar	30·18	30·06
Christiansborg	29·92	30·04
St Helena	30·05	30·18
Grahamstown	29·91	30·15
Cape Town	29·97	30·20
Graff Reinet	29·91	30·22
Maritzburg	29·89	30·19
Mauritius	29·95	30·19
Aden	30·03	29·69
Alexandria	30·06	29·80

Thus the difference at Graff Reinet and Maritzburg between the January and July pressures amounts to about 0·30 inches. From this it may be inferred that, in calculating heights along the Zambesi, from observations made at different seasons, if no allowance be made for the monthly variation, but if 29·92 inches be assumed as

* The height of Lake Balkash, according to the Russian explorers Ssemonoff and Golubeff, may be anywhere between 530 feet as given by the former, and 1200 feet as given by the latter. For a large number of heights made use of in writing this note, the author is indebted to Mr Keith Johnston, jun.

the height for all seasons, the results from observations made in January will differ from 250 to 300 feet from those obtained from observations made in July at the same place. If no account be taken of the daily variation of the pressure, the observations made in July at 9 A.M. will give a difference of from 350 to 400 feet in height as compared with results from observations made in July at 4 P.M. All this large error is avoided when the monthly and the daily variations are allowed for.

It has been seen that the summer pressure in Central Asia falls in July to about 29·500 inches. It might be inferred by analogy that the pressure in Central Africa also falls considerably below 29·92 inches over those regions where the sun is nearly vertical; and, as a consequence, that this space of low pressure moves north and south with the sun, attaining its northern limit in July, and its southern in January. The figures in the table given above fully bear out this supposition. Thus, in July at Algiers the mean pressure is 30·06; but at Laghouat, between 280 and 300 miles inland, the pressure is only about 29·86 inches; at Alexandria it is 29·80; and at Aden, only 29·69 inches; and since, in the same month, according to Speke, the wind in Central Africa near the equator and long. 32° 20′ E. is almost constantly S.E., it is probable that the pressure there is lower than at Aden. Taking the whole facts into consideration, it can scarcely be less than 29·70 inches, though probably it is lower. Again, in January the pressure at Cape Town being 29·97 inches, at Graff Reinet 29·91 inches, and at Maritzburg 29·89 inches, points still further to a diminution of pressure in the centre of southern Africa at this season, increasing from the coast—falling, probably to between 29·70 and 29·80 inches. Hence, if we assume 29·70 inches as the low pressure which accompanies the sun over those parts of Africa where he is nearly vertical, we shall not be far from the truth.

Let us apply this reasoning to the determination of the height of Albert Nyanza from Sir Samuel W. Baker's observation of the boiling point of water. The observation was made in lat. 1° 14′ N., long. 30° 50′ E, on 14th March 1864, between 8 and 10 A.M., probably at 9 A.M. The boiling point of the thermometer was 207°·8, but as it changed while in Sir Samuel Baker's possession, it is supposed that the true reading was about 207°·3, which corresponds to a pressure of 27·231 inches.* But since the observation was made about 9 A.M., when the pressure is about the maximum of the day, subtracting ·043 inch as the correction for daily range in July, we obtain as the mean pressure of the day 27·188 inches. If we assume the sea-level pressure to be 29·70 inches, the difference due to difference of height will be 2·512 inches, and the temperature of the air being at the time 84·0, the height of Albert Nyanza will be in round numbers about 2550 feet, or considerably under the height usually given.

Similarly, by the same reasoning, Gondokoro, calculated from Sir Samuel W. Baker's observations to be 1999 feet in height, will be only about 1800 feet above the level of the sea.

Considering the small difference within the tropics in the mean pressure of any month, say July, from year to year, it follows that if recent African travellers had been provided with good thermometers for determining the boiling-point of water, and had made carefully conducted observations with them, noting the precise hour and month of the observations, one of the great problems of African travel would have been already solved, viz., whether Lake Tanganyika does or does not flow into Albert Nyanza, unless the difference of level between these two lakes is comparatively small. But since travellers have been given to understand that the heights deduced from their observations may be in error to the extent of from 300 to 500 feet, less care has been bestowed in making such observations than would otherwise have been the case.

In extra-tropical regions the height of the barometer is much more fluctuating, and the pressure during any month from year to year varies more than within the tropics. But even in these regions the limits of error are much less than are usually supposed, if care be taken to make the observations full and precise, so that when they come to be reduced it may be in the power of the meteorologist to value them at their proper worth. This remark may require a little explanation.

In temperate regions barometric fluctuations are more frequent and of greater amplitude in such countries as Great Britain, which are situated between a continent on the one hand and an ocean on

* Regnault's Tables, revised by Moritz.

the other, than in the interior of continents in the same latitudes. Now, since it is to the interior of continents, viz., Asia, Africa, North and South America, and Australia, that these remarks on the discussing of heights are intended to apply, the limits of error of single observations, or groups of observations, of the pressure of the atmosphere, are much less than one accustomed to observe barometric fluctuations in Great Britain might be led to suppose. Hence, if the mean monthly sea-level pressure of the part of the earth's surface where the observation is made be kept in mind, the difference between this pressure and the observed pressure will be a tolerable approximation to the true difference of pressure due to the elevation of the place.

But a still closer approximation may be reached. All examination of weather on a large scale shows, in the most conclusive manner, that barometric fluctuations are always attended with changes of weather of a well-marked and determinate character. Hence, conversely, if travellers kept a careful record of the weather some time before and some time after they made their observations of the pressure of the atmosphere, some idea could be formed as to whether the observed pressure was above or below the mean pressure of the season at the place.

Thus, suppose that for some time before and after the observation the weather was fine and of a steady character throughout, the nights not much colder and the days not much hotter than usual, the winds light, or if moderate, continuing in one direction, and the state of the sky with respect to cloud much the same from day to day, it might be assumed that the pressure was the average of the season. Observations carefully made under these conditions are entitled to be ranked in the first class, as being the most trustworthy that can be obtained.

But if the nights have been for a day or two colder, the days hotter (in the sun), the air drier, and the winds lighter, and calm weather more prevalent than usual, then it is probable that the pressure at the time of observation was above the average of the season.

Again, suppose, in the north temperate zone, the air to have become warmer and moister, the sky clouded, rain to have fallen, and the wind veered from E. or S.E., by S. and S.W. to N.W., or suddenly shifted to W. or N.W., and the weather then to have become colder and clearer and the air drier, it is certain that a storm of greater or less magnitude has passed over the region, and since such storms are attended with great fluctuations of the barometer, it is plain that if the observation of pressure was made during these changes, it is worse than useless as a datum for the determination of the height of the place. It should, therefore, be altogether rejected.

These cases are given as examples of the method by which observations, as made by travellers, should be critically examined before they are made use of in calculating heights. It is probably from inattention to these simple directions—travellers not recording the required data, which can all be recorded without instruments, and computers not giving weight to such observations when recorded—that a large number of the grosser discrepancies, given in works of Physical Geography, have arisen. Many of the larger errors are, of course, due to the use of imperfect instruments and a want of practice in the observer.

An illustration of errors in the statement of heights may be given. The following places are situated in the neighbourhood of the Ural Mountains; the heights are those given by the most recent authorities, and a column is given showing the number of years for which the averages of mean annual pressure have been calculated :—

Place.	Lat. N.	Long. E.	Height in Feet.	Years of Average.	Mean Pressure at 32° and sea-level
	° ′	° ′			Inches.
Bogoslovsk, . .	59 45	60 2	600	26	29·862
Nijni-Tagilsk, .	57 57	59 53	730	21	30·088
Catherinenburg, .	66 49	60 35	800	18	29·835
Zlatoust, . . .	55 10	59 40	1200	28	29·835

From the above annual mean pressures it is evident that the height of Nijni-Tagilsk is over stated, the true height being, probably about 250 feet less than what has hitherto been assigned to it.

Since it has, unfortunately, been the general practice not to publish the original observations, but only the heights deduced from them, it will be impossible, except in a comparatively small number of instances, to apply the principle brought forward in this paper to past observations.

Observations for the ascertaining of heights must, to be satisfactory, include the following particulars:—

1. Latitude and longitude of the place.
2. The date of the observation, giving exactly the year, the month, the day of the month, and the hour of the day.
3. The observation itself exactly as made; if with a barometer or aneroid, the pressure to be given; if with a thermometer, the boiling-point to be given, and not merely its equivalent in pressure.
4. The temperature of the air in shade.
5. The weather for two days before and after the observation, showing the temperature of the air, its probable humidity as made known by the feelings or by its effects on surrounding objects, the amount of cloud, the rainfall, the direction, veerings, shifts, and force of the wind, together with any striking phenomena that may occur.

To these might be added, if possible, observations of the wet-bulb thermometer.

It will be evident from these remarks that the Physical Geographer will require the practised Meteorologist to aid him in settling the important physical problem of heights for large portions of the earth's surface.

3. Notice of a Remarkable Mirage Observed in the Firth of Forth. By Dr Christison.

Some years ago, when visiting on the 31st of May the beautiful scenery of Dalmeny Park, I observed about three in the afternoon, from the terrace on which stand the ruins of Barenbougle Castle, some remarkable examples of mirage on the Firth of Forth, which are perhaps worthy of record.

The atmosphere was uncommonly clear in every direction, sunshiny, warm, calm; and what little wind there was came from the south west. But a black, sultry-looking cloud was forming at the same time in the north-west, from which in an hour and a half afterwards, as I was on my way home to town, a severe thunderstorm gradually spread south-eastward over the whole Firth and surrounding country.

Looking eastward towards the mouth of the Firth, while the weather continued very fine, I was surprised to observe that the northern edge of the cone of North Berwick Law, twenty-four miles distant, at about one-third of its slope from the summit, suddenly ceased, and had given place to a sheer perpendicular precipice, overhanging its base. At the same time the spit of land of East Lothian, which is usually seen to extend more than a mile from the Law towards the Fife coast, had disappeared; the ocean had taken its place, and the overhanging precipice of the hill seemed to dip into the water. I carefully examined these appearances with a Ross's telescope of very sharp definition, and could easily ascertain that there was no mist anywhere; and the apparent precipice and apparent sea presented no character different from those of the true sky-line of the hill above, or the true sea northward from where Gullane Point terminates the land as seen from my station.

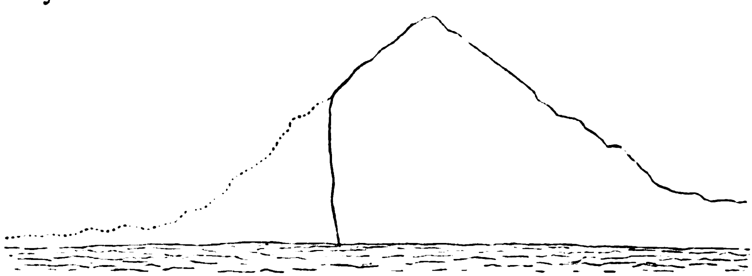


Fig. 1.—North Berwick Law, twenty-four miles.

I then also noticed that what appeared to be the south point of Inchkeith as seen from Barnbougle, eight miles distant, had thrown out southward a line of three detached rocks, with sea separating them from one another, and likewise with sea apparently passing under them, as if they were suspended in the air a few yards above the water; and that in like manner the water seemed, as it were, to have passed to some distance under the point of the island itself, raising a long low line of dark land a little into the air. It was impossible to put an end to this illusion with the telescope; on the contrary, it was rendered more distinct and apparently real.

But the most remarkable transformation was effected in a sloop which was sailing obliquely across my line of vision from south-west to north-east, apparently at the Granton Ferry, about four miles off. From the set of its sails, and the darkness of a great extent of the Firth around, the vessel was evidently carrying with it a brisk favouring breeze from the south-west; and during the fifteen or twenty minutes that my observation lasted it must have changed its place by two miles. But during all that time it presented the appearance of a narrow gap in its entire hull immediately before its single mast, and a very much wider one behind the mast. The sea was visible through both gaps; and with the telescope it was easy to observe the helmsman at the tiller on a poop completely and widely separated from the forward part of the vessel. The ferry steamer appearing from Granton on her passage to Burnt-

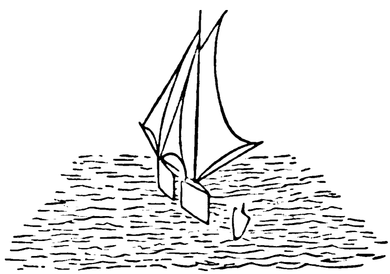


Fig. 2.—Vessel six miles off.

island, I was anxious to see what might happen to her when she arrived in the same water with the sloop. But when she came into a line with the sloop, it was at once apparent that the sloop was about two miles farther off; and she had previously appeared to be nearer, in consequence of the sheet of dark broken water in which she had been sailing being apparently raised above the level of the calm white water surrounding it on all sides over the rest of the Firth.

During a period of about twenty minutes, spent in watching these alterations of form in North Berwick Law, Inchkeith, and the sailing vessel, I could not detect the slightest change in the character of the appearances.

I am unwilling to attempt an explanation of these singular deviations from optical rectitude, because I do not see how recognised theory will explain them all. But I may venture to suggest that such instances of mirage in our Firth and elsewhere may deserve attention as possible prognostics of weather. On the occasion in question there were evident signs of a widely extended disturbance of the usual condition of the atmosphere; and the occurrence of a violent thunderstorm afterwards, which lasted far into the evening, is not unworthy of notice.

Often as I have been at Barnbougle Castle in beautiful summer afternoons, and often as I have been on the shores of the Firth in all parts, as well as on its waters, I never happened to notice any mirage before. Subsequently, however,—I do not recollect from what position, but I rather think from Leith Pier,—I observed that Aberlady Bay seemed to lead to a strait with perpendicular cliffs on either side, the promontory on which Gosford, Dirleton, Gullane, and North Berwick stand, being converted into a beautiful and extensive island; and the illusion was rendered complete by a vessel under full sail bearing to all appearance directly for the mouth of the imaginary strait.

My son also informs me that on another occasion he observed with the naked eye, but still better with the aid of a telescope, from Leith Pier, the same promontory cut into eight or ten islands, apparently reflected in an apparently calm sea, which joined by a well-defined margin the real sea itself, much disturbed at the time by a brisk breeze. The appearance of these islands, as seen through the telescope, was exceedingly beautiful, as they seemed to be clothed with fine trees, and, reflected in the calm ocean, were suggestive rather of a tropical than a northern scene.

It is probable that such appearances are not infrequent—so frequent, at least, as to give adequate opportunities for studying them; and that we do not hear of them merely from inobservance; for my son adds that, although many people, sailors as well as others, were walking on the pier at the time, not one seemed to take notice of the remarkable phenomena, which strongly attracted his attention and that of his brother who accompanied him.

The following gentlemen were elected Fellows of the Society:—

William C. M'Intosh, M.D.
Henry Marshall, M.D., Clifton.
William Rutherford, M.D.

Monday, 1st February, 1869.

The Hon. LORD NEAVES, Vice-President, in the Chair.

The following Minute of Council was read:—

"The Council of the Royal Society desire to record in their minutes the grief which they have felt in the death of James D. Forbes, Esq., lately Principal of St Andrews University, and their sense of the loss thus sustained—of one who was so great an ornament to science, and so long and so intimately connected with this Society. As a scientific inquirer, and as an academical instructor, the name of Dr Forbes will be held in reverence by all who knew him, or who benefited by his exertions; and it is a subject of deep regret that he should be cut off in the prime of life, and the scientific world deprived of his services at a time when his appointment as Principal of St Andrews University had placed him in a position where, if life and health had allowed, he might, by his further labours, have added to his own reputation and to the range of scientific discovery. The Council cannot remember the able, assiduous, and conscientious services rendered to the Royal Society, when he held the appointment of their Secretary, without feeling how deep a debt of gratitude the Society owes him. The Council direct that a copy of this minute be transmitted to Mrs Forbes, with a suitable expression of their sympathy with her in her great affliction. The Council trust that Mrs Forbes will be sustained under her severe bereavement, and they are sure that if public and individual sympathy can assist in alleviating grief, that consolation will be afforded in no ordinary degree."

The Secretary announced the receipt of letters from Professors Kirchhoff and Virchow, thanking the Society for their election as Foreign Honorary Fellows.

The following Communications were read :—

1. Practical Note on Intensified Gravity in Centrifugal Governors. By Professor C. Piazzi Smyth.

This paper, after a short introduction upon previous publications and experiences, describes in a plain and practical manner four steps of improvement recently made in the centrifugal governor of an equatorial driving clock; and touches on

1. The number of the pendulums;
2. Their weight and momentum;
3. Their chronometric principle; and
4. The intensification of the effect of gravity upon them, chiefly to increase the promptitude and energy of the centrifugal action.

This last improvement the author regards as the most important to draw attention to, because it is capable of imparting extreme quickness and sensibility to the actions of all kinds of centrifugal governors; and although not absolutely new, it does not seem yet to be sufficiently known or employed in many practical cases, where it might be of the utmost use, as in preventing some classes of disasters which are in these days happening far too frequently to sea-going screw-steamers.

The paper has since been printed in full in the "Practical Mechanic's Journal" for March 1, 1869.

2. Mr Mill's Theory of Geometrical Reasoning Mathematically Tested. By W. R. Smith, Esq., Assistant to the Professor of Natural Philosophy. Communicated by Professor Tait.

An amusing and instructive example of the way in which logicians are accustomed to dogmatise upon the theory of sciences that they do not understand, is afforded by Mr Mill's explanation of the nature of geometrical reasoning.

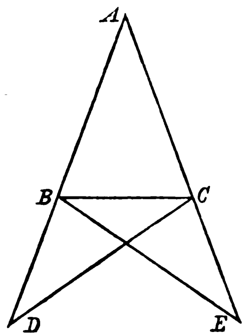
Those who remember that Mr Mill assures Dr Whewell that he has conscientiously studied geometry (Logic, 7th ed. I. 270), will probably find some difficulty in believing that the demonstration of Euc. I⁵, which Mr Mill offers as an illustration of the justice of his theory of geometrical reasoning, depends on the axiom, that triangles, having two sides equal each to each, are equal in all respects. Such, nevertheless, is the case; and when one sees this absurdity pass unmodified from edition to edition of Mr Mill's Logic, and when even Mansel, Mr Mill's watchful enemy, tells us that "against the form of the geometrical syllogism, as exhibited by Mr Mill, the logician will have no objections to allege" (Mansel's Aldrich, 3d ed., p. 255), one cannot but think that logic would make more progress if logicians would give a little more attention to the processes they profess to explain.

It may perhaps be worth while to show how Mr Mill was led into this extraordinary mistake. We shall find that Mr Mill chooses rather to sacrifice geometry to his philosophy, than to modify his philosophy in accordance with the facts of geometry.

Mr Mill holds that all general knowledge is derived from experience; meaning by experience the comparison of at least two distinct experimental facts. In other words, all knowledge is ultimately gained by induction from a *series* of observed facts. That any general truth can be got at intuitively, by merely looking at one case, Mr Mill emphatically denies. The fact that two straight lines cannot enclose a space, is not self-evident as soon as we know what straight lines are [*i.e.*, can mentally construct such lines]; but is got at only by experiments on "real" or "imaginary" lines (Logic, I. pp. 259, 262). Now it is certain, that in the demonstrations of Euclid, we are satisfied of the truth of the general proposition enunciated, as soon as we have read the proof for the special figure laid down. There is no need for an induction from the comparison of several figures. Since then one figure is as good as half a dozen, Mr Mill is forced to the conclusion that the figure is no essential part of the proof, or that "by dropping the use of diagrams, and substituting, in the demonstrations, general phrases for the letters of the alphabet, we might prove the general theorem directly" from "the axioms and definitions in their general form" (p. 213).

We may just mention, in passing, that this view, combined with the doctrine that the definitions of geometry are purely hypothetical, leads Mr Mill to the curious opinion that we might make any number of imaginary sciences as complicated as geometry, by applying real axioms to imaginary definitions. We mention this merely to illustrate Mr Mill's position—our present business is to see how these views of geometry work in practice.

Mr Mill's example, as we have said, is Euclid I[5], which he undertakes to deduce from the original deductive foundations. We have first (p. 241) some preliminary remarks, which afford a remarkably happy instance of the way in which Mr Mill is accustomed to keep himself safe from all opponents, by alternately supporting each of two contrary views of a subject. "First," says he, speaking of the angles ABE, CBE; ACD, BCD, "it could be perceived intuitively that their differences were the angles at the base." If this intuition is really a step in the proof, then, since intuition is just actual looking at the figure, what becomes of the doctrine that the figure is not essential, or of the still more fundamental doctrine, that no general truth can flow from a single intuition?* In this, however, Mr Mill only falls foul of himself. A more serious matter is, that when he sets about his regular demonstration, he falls foul of the truths of geometry.



Having shown that AD=AE, Mr Mill proceeds thus:—"Both these pairs of straight lines" [AC, AB: AD, AE] "have the property of equality; which is a mark that, if applied to each other they will coincide. Coinciding altogether, means coinciding in every part, and of course at their extremities, D, E, and B, C." Now, "straight lines, having their extremities coincident, coincide. BE and CD have been brought within this formula by the preceding induction; they will, therefore, coincide." [!] If Mr Mill generalises this conclusion, I think he will find it to be that two triangles, having two sides of each equal, are equal in all respects; and from this theorem he may at once conclude, by his own fourth formula [" angles having their sides coincident, coincide "], that

* This is no mere slip on Mr Mill's part. To show that the angles at the base are the differences of the angles in question, without appealing to the figure, we must have a new axiom [proved, of course, by induction !] viz., that if a side of a triangle be produced to any point, the line joining that point with the opposite angle falls wholly without the triangle.

angles contained by equal straight lines are equal! It is clear that Mr Mill did not see that the point is to show that, *the triangles ABE, ACD, remaining rigid*, AB may be applied to AC, and AE to AD at the same time. But this can only be brought out by figuring to oneself AB moved round to coincide with AC, and then the triangle ABE rotated about AB through two right angles; and this process was not competent to Mr Mill, whose theory bound him to prove the equality of the triangles by pure syllogism from the two formulas, "equal straight lines, being applied to one another, coincide," and "straight lines, having their extremities coincident, coincide."

But Mr Mill may say, "I have only to add, that equal angles applied to one another coincide."

Very well, you have then three syllogisms:—

Equal straight lines coincide if applied;	Equal straight lines coincide if applied;
AC, AB, are equal.	AD, AE, are equal.

Equal angles coincide if applied;
CAD, BAE, are equal.

Logically these three syllogisms can give only three independent conclusions:—

AC, AB coincide if applied.	AD, AE coincide if applied.

The angles CAD, BAE coincide if applied:—

but by no means the ONE conclusion that the rigid figures ABE, ACD coincide if applied. If Mr Mill still contends that there is no need for intuition here, let him substitute for the words "equal straight lines," "equal arcs of great circles." The premises of his syllogisms are still all right; but, owing to circumstances that must be *seen* to be understood, the spherical triangles cannot be made to coincide.

There are only two courses open to Mr Mill—either to confess that the attempt to square geometry with a preconceived theory has forced him into a grossly erroneous demonstration, or to invent a new formula—viz., that if in two plane figures any number of consecutive sides and angles taken one by one may be made to coincide, they may also be made to coincide as rigidly connected wholes. But, then, Mr Mill must maintain that the man who reads Euc. I[4] for the first time does not at once conclude the general truth of this formula from the one figure before him, but either brings the formula with him to the proof as a result of previous induction, or requires to pause in the proof, and satisfy himself of the truth of the formula by a comparison of a series of figures.

It is easily shown, by the same species of analysis as we have adopted here, that wherever a real step is made in geometry we must either use the figure or introduce a new general axiom [not of course in mere converses, as Euc. I^{19}, I^{25}]. All geometrical construction is in the last resort a means of making clear to the eye complicated relations of figures.

Now, if we can at once and with certainty conclude from the one case figured in the diagram to the general case—if, that is, axioms are proved not by induction, but by intuition, and are necessarily true—there is no difficulty about geometrical reasoning; but if each new axiom is gained by a new induction (and that on Mr Mill's showing an "*inductio per enumerationem simplicem*,") we get a difficulty which Mr Mill curiously enables us to state in his own words (I. p. 301)—"If it were necessary," in adding a second step to an argument, "to assume some other axiom, the argument would no doubt be weakened." But, says Mr Mill, it is the same axiom which is repeated at each step. If this were not so, "the deductions of pure mathematics could hardly fail to be among the most uncertain of argumentative processes, since they are the longest." If now we do call in new axioms whenever we construct an essentially new figure, must not Mr Mill admit, on his own showing, that every advance in geometry involves an advance in uncertainty; that the geometry of the circle is less certain than that of the straight line, solid geometry than plane, conic sections than Euclid, &c.? Surely this is a *reductio ad absurdum* of the whole theory.

The principles of geometry involved in the question are so important that we may profitably separate them from Mr Mill's blunders in a special case.

I. The proofs of geometry are clearly not inductive. There is no mental comparison of various figures needed during the proof. The inductions involved (if any), must have been previously formed.

II. The proofs then must be reduced either to actual perception (intuition), or to deduction from axioms. But since the proof is general, the former assumption involves the reality of general intuition, *i.e.*, of a general judgment from a single perception.

III. The theory of intuition is sufficient, but is disputed in two interests:—

(*α*) In the interest of syllogism, which claims to give indefinitely extensive conclusions from limited premises [but many, as Whewell, hold that these premises are intuitive axioms].

(*β*) In the interests of empiricism, which makes all arguments be ultimately from particulars to particulars.

Mr Mill combines the two objections.

Now we have seen that if objection (*a*) falls (*i.e.*, if the premises of geometry are not reducible to a limited number of axioms from which everything follows analytically), the security of geometric reasoning can be established only if each premise has apodictic certainty. To overthrow Mill's whole theory, it is therefore enough to show the fallacy of the limited-number-of-axioms hypothesis. On this we observe:—

1*st*, The axioms are more numerous than Mr Mill thinks, for his proof of Euc. I^5 is lost for want of more axioms.

2*d*, The indefinite extension of geometry depends on the power of indefinitely extended construction [but where there is construction there is intuition—nay, mental intuition is mental construction]. Now here our opponents may suppose [A] that the general conclusion really flows from the particular construction, which, in the language of logic, supplies the middle term. But since the construction is particular, we should thus be involved in the fallacy of the undistributed middle. Again, [B] it may be said that the construction is only the sensible representation of a general axiom. But as the construction is new and indispensable, the general axiom must be so also. Therefore, if geometry is proved from axioms, these axioms must be unlimited in number.

3*d*, Obviously it is not by logic that we can satisfactorily determine how far geometry contains synthetic elements peculiar to itself. We have, however, in analytical geometry a ready criterion how far geometry can be developed without the addition of new geometrical considerations.

Now we find that we cannot begin analytical geometry from the mere axioms and definitions. We must by synthetic geometry, by actual seeing, learn the qualities of lines and angles before we can begin to use analysis. Then, given so many synthetic propositions, we can deduce others by algebra; but only by a use of actual intuition, *first*, in translating the geometrical enunciation into algebraic formulæ; and, *second*, in translating the algebraic result (if that result is not merely quantitative) into its geometric meaning. The answer to a proposition in analytical geometry is simply a rule to guide us in actually constructing, by a new use of our eyes or imagination, the new lines which we must have to interpret the result. Analysis does not enable us to dispense with synthetic constructions, but simply serves to guide us in these constructions, and so to dispense more or less completely with the tact required to find out the geometrical solution. This is true in every case, but most obviously in the investigation of new curves. The tracing of curves, from their equations, is a process in which no man can succeed by mere rule without the use of his eyes. Suppose asymptotes, cusps, concavity, everything else found, the union of these features in *one* curve will remain a synthetic process.

Still more remarkable is the use made in analysis of imaginary quantities. To the logician an imaginary quantity is nonsense, but geometrically it has a real interpretation. The geometrical power gained by a new method like quaternions, is radically distinct from that gained by the solution of a new differential equation. The latter is a triumph of algebra, the former is a triumph of synthetic geometry—the discovery of a whole class of new guides to construction.

Professor Tait remarked that an excellent and interesting instance of the incapacity of metaphysicians to understand even the most elementary mathematical demonstrations, had been of late revived under the auspices of Dr J. H. Stirling. His name, with those of Berkeley and Hegel, formed a sufficient warrant for calling attention to the point.

It is where Newton, seeking the fluxion of a product, as ab, writes it in a form equivalent to

$$\frac{1}{dt}\left[(a + \tfrac{1}{2}\dot{a}dt)(b + \tfrac{1}{2}\dot{b}dt) - (a - \tfrac{1}{2}\dot{a}dt)(b - \tfrac{1}{2}\dot{b}dt)\right]$$

which gives, at once, the correct value

$$a\dot{b} + b\dot{a}\,.$$

Now Berkeley, Hegel, Stirling, and others, have all in turn censured this process as a mere trick (or in terms somewhat similar) and say, in effect, that it is essentially erroneous. The fact, however, is that, as in far greater matters, Newton here shows his profound knowledge of the question in hand; and adopts, without any parade, a method which gives the result *true to the second order of small quantities.* The metaphysicians cannot see this, and Dr Stirling speaks with enthusiastic admiration of the clear sightedness and profundity of Hegel in detecting this blunder, and for it "harpooning" Newton!

What Newton seeks is the rate of increase of a quantity at a particular instant. Instead of measuring it by the rate of increase *after* that instant (as the metaphysicians would require) he measures it by observing, as it were, for equal intervals of time *before and after* the instant in question.

Any one who is not a metaphysician can see at once the superior accuracy of Newton's method, by applying both methods to the case of a rapidly varying velocity; such as that of a falling stone, or of a railway train near a station.

In reference to what Professor Tait had said, Mr Sang remarked, that the line of argument attributed to Newton had been used by John Nepair before Newton's birth. Nepair's definition of a logarithm runs thus (Descriptio, lib. i. cap. i. def. 6), (Constructio, 23, 25) that if two points move synchronously along two lines, the one with a uniform velocity (arithmeticè), the other (geometricè) with a velocity proportional to its distance from a fixed point, the distance passed over by the first point is the logarithm of the distance of the second from the fixed point. In order to compare this variable velocity at any instant with the constant velocity, he takes a small interval of time preceding, and another succeeding the given instant, shows that the true velocity is included between the two velocities thus obtained, and (28, 31) takes the arithmetical mean as better than either, and as true (*inter terminos*).

It may be added, that Nepair devotes several sections of his

Constructio to the discussion of the doctrine of limits *(de accuratione);* that his logarithms were denounced by the metaphysicians of his day as founded on the false system of approximation, but that, fortunately for the progress of exact science, their objections were unheeded.

Sir W. Thomson said, that the metaphysicians, wishing to find the speed of a vessel at 12 o'clock from an hour's run, would choose the hour from 12 to 1; whereas Napier, Newton, and the rest of the world, would take it from 11·30 to 12·30.

3. Note on Captain A. Moncrieff's system of working Artillery. By R. W. Thomson, Esq., C.E. A Model of Captain Moncrieff's Gun Carriage was exhibited, and its mode of action was shown.

4. Note on an undescribed variety of Flexible Sandstone. By T. C. Archer, Esq.

The following gentleman was admitted a Fellow of the Society:—

Dr R. Craig Maclagan, F.R.C.P.E.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VI. | 1868–69. | No. 79.

Monday, 15th February 1869.

DR CHRISTISON, President, in the Chair.

At the request of the Council, Mr Geikie gave the following Address on the Progress of the Geological Survey of Scotland.*

Before proceeding to the special subject of this address, I have thought that it might be of interest to the Society to lay before them a brief outline of the history of geological map-making in Scotland, previous to the time when the task was undertaken by the Geological Survey. I do not, indeed, presume to enter upon any general retrospect of the literature of Scottish geology, but will content myself with selecting for remark a few of the more eminent contributors, on whose labours the present general geological maps of the country are based. These maps are compiled from the results obtained by many different geologists, working independently during the last fifty or sixty years. Some of the men whose researches have in this way been made use of, never themselves produced any map, but their descriptions of the districts traversed by them served afterwards as a basis for the maps of others.

The first geologist who attempted to map any large district of

* As a rule, the results of the Geological Survey are not published until they appear in the proper official form. When, however, the Council of the Royal Society requested me to give an account of the progress of the Survey, I felt that the ordinary rule might here with much advantage be set aside; and on referring the matter to the Director-General, Sir Roderick Murchison, he took the same view, and cordially gave his assent.

Scotland in considerable detail, was Dr MacCulloch. During a series of years he employed himself in carrying on a geological survey of the Western Islands, and published the results from time to time in the Transactions of the Geological Society of London. At length, in the summer of 1819, he collected these scattered notices, rearranged and enlarged them, and published them as his well-known work on the Western Islands. Accompanying the text was a volume of plates, including detailed maps of the islands, with sections explanatory of their geological structure. This is one of the most important contributions ever made to Scottish Geology. It laid down with some detail, and for the first time, the geological structure of the long chain of islands from the Isle of Man to the Butt of Lewis, along with a portion of the adjoining mainland. It was accomplished, too, at a time ere steamers had been established, and when travelling in that region entailed no little inconvenience, and even personal risk.

In the early part of this century there existed at Edinburgh a flourishing School of mineralogy and geology, under the leadership of Professor Jameson. That able and enthusiastic teacher infused into his pupils much of the zeal with which he himself pursued his favourite path of science. Though he travelled far and near over Scotland, and gave frequent notices of his labours to the various learned societies with which he was connected, he published but a small part of the mass of information which he had accumulated. It was indeed to his class at the University that he made known his observations and deductions, and he lived to see the seed which he had there sown bear goodly fruit in the career of not a few eminent naturalists. One of his early pupils, to whom the cause of geology in this country stands largely indebted, was Dr Ami Boué, a native of Hamburg, who had come to Edinburgh to study medicine, and who took his degree in the year 1817. During his residence in Scotland Boué greatly distinguished himself by his zeal in the prosecution of geology. He made long and frequent journeys in almost every part of the country, comparing the rocks of one district with those of another, and gathering together from his own observations, as well as from the recorded researches of others, a large and valuable mass of information regarding the geology of Scotland, which he published in Paris in the year 1822 as an "Essai Géologique sur l'Ecosse." This volume, besides its detailed descriptions of the rocks of the country, contains a coloured geological map, and a series of plates of illustrative sections. The map has a peculiar interest in our present inquiry. The outlines of the geological structure of the kingdom are there broadly sketched. He traces the general area occupied by the gneiss, and what he calls the "chloritic and quartzose rocks" of the Highlands, the position and extent of the great coal-field of the Midland Valley, and the limits of the "greywacke" of the Southern Uplands. He distinguishes the granites, syenites, and porphyries, and roughly separates the rocks of volcanic or trappean origin, according to their respective ages. In this latter subject his observations were far in advance of his time. He had studied some of the extinct volcanic districts of the Continent, and gained a familiarity with volcanic phenomena which gave him a great advantage over his Scottish contemporaries, who were still disputing whether or not the trap-rocks of their country had really a volcanic origin. The map on which Dr Boué inserted his broad generalisations was of course merely an outline or sketch, necessarily full of inaccuracies and omissions. But in some important respects it foreshadowed the maps that have succeeded it.*

While the Edinburgh School of Geologists continued to furnish descriptions, and sometimes also maps of different geological districts, Dr MacCulloch carried on his own independent researches, and brought them at intervals before the Geological Society of London. He had been required by the Board of Ordnance to make some special investigations, partly with a view to discover some one mountain, the geological structure of which might afford a prospect of repeating with greater reliability the experiments made by Dr Maskelyne upon Schihallien to estimate the weight of the earth. In the course of these and other journeys he had noted upon a map the geological structure of each district examined; and finding at last that a considerable area of the country had been thus delineated, he represented to the Board of Ordnance the propriety

* Dr Boué still retains his youthful enthusiasm. He is resident at Vienna, from which place I have recently had some interesting letters from him, full of gossip about Edinburgh in the early part of this century, and of references to his own wanderings in Scotland.

of securing a grant from Parliament to ensure a complete geological map of the whole country. This application was eventually acceded to by the Treasury, and Dr MacCulloch was empowered to extend and complete his survey at the public cost. The map on which he laid down his geological lines was that of Arrowsmith, on the scale of a quarter of an inch to a mile. It was published in 1832, and the memoir descriptive of it appeared in 1836, the author having meanwhile died from a melancholy accident in 1835.

MacCulloch's Geological Map of Scotland has long taken its place among the classics of geology. The outlines of the geological structure of the country are much more fully traced than Boué attempted on his little sketch-map; and although, like the latter, it is, and could not fail to be, full of errors and omissions, it must ever remain a remarkable monument of the industry of a single observer. It would have been well for MacCulloch's reputation had he lived to see through the press, the memoir explanatory of the map. We can hardly believe that, though he had written, he would in his calmer moments have published, so much intemperate invective against possible critics of his map, or so much special pleading which showed only too well how thoroughly he was himself aware of the shortcomings of his work. He would doubtless have come to see that the only course open to him was to show simply what he had done, what he wished to do had his materials and opportunities permitted, and to confess that his map, with all its imperfections, could only be regarded as a first rough draft of what he conceived a geological map of Scotland should be.*

The Transactions of the Geological Society of London, besides the numerous contributions of Dr MacCulloch, were likewise enriched with some valuable papers in Scottish geology by Sir Roderick (then Mr) Murchison and Professor Sedgwick. Such were

* In speaking of future corrections and improvements of his map, Dr MacCulloch remarks, that they will not require the energies of "a refined geologist." "The rocks are few, and it is easy to learn to recognise them; there is nothing which any man may not attain, on this narrow subject, with a few weeks of experience. It will confer no particular fame on any future self-constituted geologist, to have done what could have been effected by a surveyor's drudge, or a Scottish quarryman." [!]—*Memoirs to his Majesty's Treasury respecting a Geological Survey of Scotland*, 1836, p. 17.

their memoirs on the Red Sandstones of the northern counties, on the Structure of Arran, and Sir Roderick's first important essay—that on the Secondary Rocks of the north of Scotland.

Shortly after the publication of MacCulloch's map, the Highland and Agricultural Society of Scotland, impressed with the necessity of more detailed and accurate geological information than had yet been given, began to offer liberal premiums for surveys and descriptions of various parts of the kingdom. The result of this scheme was the publication of a series of prize essays and maps of different counties and districts, full of valuable information, and much more in detail than anything which had been before attempted for the same parts of the country. The first essay in the order of issue was that of Mr Milne-Home on Berwickshire, which appeared in 1836, and afterwards there came at intervals memoirs on the counties of Roxburgh, Peebles, Sutherland, Banff, Renfrew, Dumfries, Kirkcudbright, &c.

The Wernerian Society, also, about the same time, proposed a series of geological essays, with premiums for the successful competitors. One of the subjects for competition was a geological account and map of the three Lothians, and the prize was awarded in 1836 to Mr Hay Cunningham for his now well-known memoir on that subject published in the Society's Transactions.

The published journals of the different scientific societies continued to receive occasional contributions in descriptive geology with accompanying maps, such as Mr Milne-Home's account of Roxburghshire, Mr Stevenson's Geology of Cockburn Law and of the Eastern Lammermuirs, Sir R. Murchison's Geology of Carrick, and others; while now and then there appeared a separate work, such as Mr Maclaren's Geology of Fife and the Lothians.

Using MacCulloch's map as a basis, amending it, and adding to it the work of the different subsequent labourers in the field, Professor Nicol published a Geological Map of Scotland in 1858. Its scale was ten miles to an inch. Among the improvements which distinguished it from all preceding maps of the country was the separation of the red sandstone of the north-west coast from the old red sandstone of the rest of the island—a change rendered necessary by the results of the conjoint researches of Sir Roderick Murchison and Mr Nicol. In the year 1859, Sir Roderick, who since 1826 had from time to time published important contributions on the geology of his native country, gave to the world a geological sketch-map of the Scottish Highlands, based upon the results of the labours of several summers spent by him in the north-western counties. In this map the crystalline rocks of the Highlands were for the first time brought into recognisable geological relationship to the other rocks in the British islands. Taken along with the different memoirs of which it was an embodiment, it may indeed be said to have revolutionised the geology of the northern half of the kingdom. Subsequently, after an exploration of the Western and Central Highlands by Sir Roderick and myself in company, we published in 1861 a first sketch of a new geological map of Scotland, in which the geological structure of the Highlands (as distinguished from the mere distribution of different kinds of rock) was more definitely traced; the subdivisions of the old red sandstone and carboniferous formations were put down, and an attempt was made to arrange chronologically the different trappean rocks of the kingdom. The map was on a very small scale, and was necessarily imperfect, and in places erroneous, being offered only as a rough outline to show in a graphic form our views of the general geological structure of the country.

The necessity of a detailed investigation of the structure of the United Kingdom was recognised by Parliament in 1835, and Sir Henry de la Beche, who had at his own charges made some progress in a survey of the south-west of England, was empowered to employ some assistants, under the Board of Ordnance. His small staff was called the Ordnance Geological Survey. Subsequently, in 1845, the great advantages of such a survey having by that time been more fully perceived by Government, the number of geologists was increased, and the staff transferred from the Board of Ordnance to the Board of Works. Since that period the geological survey has grown still further, and is now under the Department of Science and Art.

The object of this national Survey is to ascertain in detail the geological structure of the country, to publish illustrative maps, sections, and descriptive memoirs, and to collect and exhibit specimens of the rocks, minerals, and fossils met with in the course of the inquiry. The maps on which the field work is conducted are those prepared by the Ordnance Survey. Hence as these maps must be obtained before geological surveying can go on, the progress of the Geological is necessarily in a great measure dependent upon that of the Ordnance Survey. Owing to the want of the Ordnance maps, it was not possible to extend the Geological Survey to Scotland until the year 1854, when a beginning was made by Professor Ramsay. Only two geologists could be spared for the Scottish survey, and this continued for several years to be the whole of the staff employed in this part of the United Kingdom, the backward state of the Ordnance Survey rendering any considerable increase of the geological surveyors impossible. At last, a more liberal Parliamentary grant enabled the Director of the Ordnance Survey to proceed much more rapidly with the publication of the Ordnance maps. It therefore became desirable to expedite at the same time the progress of the Geological Survey, and the late Government, after a full consideration of the subject, towards the close of the year 1866 resolved to reconstruct the staff of the Geological Survey, and largely to increase its numbers. Hitherto the establishment had consisted of two branches—one for Great Britain, with head-quarters in London, and one for Ireland, with head-quarters in Dublin. It was now determined that there should be three branches, each under a separate resident director, and with an adequate staff—one for England and Wales, under Professor A. C. Ramsay; one for Ireland, under Mr Jukes; and one for Scotland, under my own charge; the three branches still remaining under the control of the Director-General, Sir R. I. Murchison. With this great increase of strength, the Geological Survey of Scotland is now making rapid progress; and as the Ordnance maps of the whole of the southern half of Scotland are published, no hindrance is anticipated in the completion of the geological investigation of that part of the country.

On a former occasion,* I had an opportunity of laying before the Society a narrative of the progress of the Survey during the first ten years of its existence. I pointed out that, commencing in East Lothian, it gradually crept westward to the borders of Stirlingshire, and southwards to those of Dumfries, Roxburgh, and

* Address given at the request of the Council on 6th February 1865. See Proceedings, vol. v. p. 355.

Berwick; that its further advance into the central coal-field being impossible for want of the Ordnance maps, it was transferred to Fife, and spread over the whole of that county and Kinross to the Firth of Tay and the confines of Perthshire; and that the maps of the Clydesdale and Stirlingshire coal-field still failing, the geological surveying was at last commenced from a new centre in the south of Ayrshire during the autumn of the year 1863. Since that time the work has advanced steadily northwards and eastwards. The large county of Ayr is now surveyed, and the maps of it partly published, and partly engraving. Half of Renfrewshire is finished, and the survey of the Clyde coal-field under my colleague Mr Hull, is advancing round Glasgow. The work of the Survey is now extended across the island from sea to sea, and is at present diverging from this completed belt, northwards through the rich mineral-fields of Lanarkshire and Stirlingshire, and southwards over the counties of Wigtown, Kirkcudbright, Dumfries, and Peebles.

The field-work is traced upon the six-inch maps of the Ordnance Survey, and is then reduced and engraved upon the one-inch maps; the general geological map of the whole kingdom being published on the scale of one inch to a mile. But wherever any workable minerals occur, in addition to this general map, there are likewise issued sheets on the six-inch scale, on which all necessary geological details are inserted. In this manner all coal-fields are published on the large scale, though also included in the general geological map on the smaller scale. In order, however, that the large amount of detail necessarily accumulated as the Survey advances may be made as useful to the public as possible, manuscript copies of six-inch maps, which are not to be published, are furnished under certain conditions. Landed proprietors, engineers, and others thus obtain detailed geological surveys merely at the cost of the manual transcription of the original field-maps.

The area which has now been mapped amounts in all to about 4100 square miles. Of this area, 2269 square miles have been published on the one-inch scale. About 800 square miles are now in the course of being engraved, and the rest is in progress. Nine sheets of the one-inch map have been published, comprising the whole or parts of the counties of Edinburgh, Haddington, Linlithgow, Fife, Kinross, Peebles, Selkirk, Lanark, Ayr, and Kirkcudbright. In addition to these maps on the scale of one inch to a mile, thirty-six sheets of the coal-field maps, on the scale of six inches to a mile, have been published, including the coal-fields of Mid-Lothian, East Lothian, Fife, and Ayrshire. A considerable number more, now in the hands of the engraver, embrace the northern half of the Ayrshire coal-field, with part of the great coal-basin of the Clyde. In addition to the maps, sheets of horizontal sections are published, to explain the structure of the country, and also vertical sections, to illustrate the strata of the different coal-fields. Descriptive memoirs are likewise issued along with the maps, giving the detailed evidence on which the geological lines have been drawn, list of fossils, and all important information obtained in the course of the survey.*

I shall now proceed to indicate some of the more important scientific results which have been obtained by the Geological Survey of Scotland during the four years which have elapsed since I last addressed the Society upon this subject. Beginning with the oldest rocks we have examined, I may remark that the complicated Silurian geology of Carrick has been mapped, and a large suite of fossils collected from that district. From the south-west of Ayrshire the lower Silurian rocks have been traced across Nithsdale and Clydesdale into the valley of the Tweed. In the prosecution of the work, within the last few months an important discovery has been made by one of the younger members of the Survey, Mr R. L. Jack—viz., the occurrence of a bed of fossiliferous conglomerate, which has been traced for about five miles among the Leadhills. The fossils have not yet been examined, but there seems much probability that this bed will prove to be a prolongation of the well-known Wrae limestone, and thus define the age of the strata of the Leadhill district. A large area of Old Red Sandstone has now been examined, including the whole region between St Abb's Head and

* In the course of his remarks, the speaker pointed to a large map of the southern half of Scotland, on the scale of one inch to a mile, on which all the area yet surveyed by the Geological Survey was coloured. There were likewise suspended on the wall specimens of the maps of the coal-fields of Fife and Ayrshire, on the scale of six inches to a mile; specimens of the horizontal and vertical sections, and of the duplicate manuscript maps preserved in the office of the Survey.

the south-west of Ayrshire. Three distinct divisions have been ascertained—an upper series, graduating upwards into the Carboniferous formation, well seen in East Lothian; a middle group, extensively developed, to the south of the town of Ayr; and a lower group, which reaches an enormous thickness, and passes down into the Upper Silurian. In this lower division, as it approaches its north-eastern limit, a remarkable local unconformity has been ascertained by the recent researches of Mr B. N. Peach and myself. In the Pentland Hills the conglomerates and sandstones rest upon the vertical abraded edges of the Upper Silurian rocks; while only a few miles distant, in the parish of Lesmahagow, there is no such break, but the one series passes regularly into the other. The Pentland Hills, therefore, contain two groups of strata, both belonging to the lower Old Red Sandstone, but the one lying quite unconformably on the other. It seems not improbable that the great development of contemporaneous igneous rocks associated with these strata in the Pentland area may have had something to do with the origin of this local break in the succession of the deposits.* Much attention has been bestowed by the Survey during the last few years upon the lower members of the Carboniferous system. The result is that a twofold division has been made of that hitherto vaguely defined set of strata called by the late Mr Charles Maclaren the Calciferous Sandstones, and lying between the top of the Old Red Sandstone and the base of the Carboniferous limestone. New light has consequently been thrown upon the history of the earlier portions of the Carboniferous period in Scotland. The lower part of the Calciferous sandstones consists of a thick but variable group of red sandstones extensively developed in Ayrshire, Arran,

* In Vol. V. of the Proceedings of this Society, p. 360, I have given a section of the Pentland Hills, which remains true, though I have since learnt more of the relations of the rocks there shown to the structure of this country at large. The series in the section marked *c* is stated to be "a middle division of the Old Red Sandstone," which is locally true, though more extended researches show that the great discordance between *c* and *b*, disappears in the course of a few miles. *c* is in reality only an upper unconformable portion of *b*. Again, the relation of the strata marked *e* to those below is correctly shown in the section, but I am now convinced that the red sandstones of the Cairn Hills (*e*) are only a prolongation of that great band of red sandstones which forms the base of the Carboniferous series throughout the west of Scotland.

and Bute, and stretching, as I have now ascertained, across the island to the coast of Haddington and Berwick. Between these lower sandstones and the base of the Carboniferous limestone comes a group of strata possessing much interest from the variations which it exhibits in thickness and in the nature of its component strata. In some districts it is altogether absent, and then the limestones and the red sandstones come together. But again, at no great distance, it reappears, and soon acquires a considerable thickness. In the western part of the country, it consists of grey and white sandstones, pale grey, green, or mottled marls and shales, with bands of cement stone, sometimes with dark shale and ironstone. The Ballagan beds of the Campsie Hills belong to this group. In the eastern half of the country, throughout the Lothians, it is made up of thick white sandstones, black shales, ironstones, some beds of limestone, and even of coal, its most important components being those bituminous shales from which oil is now so extensively obtained. In the east of Fife it contains a great many bands of limestone, having the fossils and general aspect of true carboniferous limestone beds, while in Berwickshire it reassumes the aspect which it shows in Ayrshire and the west.

During the progress of the Survey in Ayrshire, we have been fortunate enough to bring to light a new page in the history of volcanic action in this country—viz., the existence of a remarkable series of melaphyres, porphyrites, ashes and volcanic agglomerates, interbedded in the Permian series of that county and of Dumfriesshire. These rocks are of Permian age, and prove the existence of a group of active volcanic vents at that geological period in the south-west of Scotland.* In connection with this subject, I may mention that we have now accumulated a large body of evidence regarding the dates and extent of ancient volcanic action in Scotland. The oldest volcanic rocks we have yet encountered belong to the Lower Old Red Sandstone; they are well seen in the chain of the Ochil Hills, and in the range of broken heights which stretches from the Braid Hills at Edinburgh south-westward across Clydesdale and Nithsdale, almost to the coast of Ayrshire. Then comes a group which seems to form a middle division in the Old Red Sandstone, and is seen in the south of the county of Ayr. The

* See Geol. Mag. for June 1866.

Upper Old Red Sandstone shows traces of contemporaneous volcanic activity in Berwickshire. During the early half of the Carboniferous period the volcanic forces were amazingly busy all over central Scotland. To that time must be referred the hills of the south of Arran and of Bute, the north of Ayrshire, Renfrewshire, the Campsie Fells, and most of the craggy hills which roughen the basin of the Forth from Stirling to the May Island and the Bass Rock. Then come the Permian volcanic rocks of the south-western counties. And lastly, we have those strange persistent dolerite dykes, which cross all the other rocks and even large faults, and which range from south-east to north-west, or from east to west towards the great mass of dolerites and basalts stretching from the south of Antrim along the western seaboard of Scotland. Those dykes I have formerly shown to the Society to be probably of miocene age.*

Several areas of metamorphic rocks have been mapped. The gradual passage of ordinary sedimentary rocks into crystalline compounds, and thence into various porphyries, syenites, and granites has been traced, particularly by Mr James Geikie, both among the Lower Siluran and Lower Old Red Sandstone series of Ayrshire. The details which have been gathered in the course of these investigations will, it is hoped, throw considerable light upon the metamorphism of rock-masses.

While the structure of the rocks underneath is thus delineated on the maps, attention at the same time is directed to the superficial deposits, which are all mapped out in the same detail. The various divisions of the Drift series are traced, and as the work advances the movements of the ice of the glacial period become from month to month more clear. We are now at work among the uplands of Galloway, and I anticipate thence a large accession of information regarding the history of the Ice Age in Scotland.

It is impossible that, while these various investigations are in progress, attention should not often be called to the relation between the structure of the rocks underneath and the form that they assume above ground. The nature of our work necessitates frequent reflection upon this subject, and implies the accumulation of a

* See Proceed. vol. vi.; and Address as President of the Geological Section of British Association, 1867.

large mass of data from which the origin of the present outlines of the country may be deduced. These data are published upon the maps and sections. They seem to me to prove irresistibly the truth of the doctrine, which was first propounded within the walls of this Society by Hutton, that the existing contour of the land is not to be traced back to the operation of earthquakes, upheavals, and depressions, but mainly to the eroding influences of rain, springs, rivers, ice, and the sea. In illustration, I would point to the map suspended upon the wall, and show how the lines of large fracture ascertained by the Geological Survey are not marked at the surface, as a general rule, by any corresponding valley or depression; while, on the other hand, the existing valleys and ravines, instead of coinciding with lines of fault, as is too often assumed, in reality run across these lines in all directions, and without any reference to them at all. It must be a subject of congratulation among the Fellows of this Society to find that the views propounded here by their illustrious predecessor Dr Hutton, after being almost lost sight of for more than half a century, are now everywhere gaining ground rapidly, and that there is a growing recognition of the genius of that great man. Hutton, as it appears to me, is the true father of modern physical geology. Only now are we coming abreast of him, so far was he in advance of his time.

The following Communication was read:—

On Traces in the Adult Heart of its transitions in form during Fœtal Life. (Part I.) By P. D. Handyside, M.D. (With a Plate.)

In this paper the author described in the adult human heart *certain vestiges of structures* which, during fœtal life, exist in an entire and perfect state. At birth, however, these, ceasing to be of use, generally disappear.

In allusion, *first*, to the Eustachian valve, he exhibited (and illustrated by the annexed sketch, fig. 1), an apparently unique specimen—obtained in his dissecting-rooms about six years ago—namely, a very large and reticulated Eustachian valve, prolonged at its middle third in the form of a semi-insulated cribriform fibrous lamina, taking a crescentic form continuously throughout, and after a valvular manner ending at the distance of three-sixteenths of an inch from the crest or rim of entrance of the superior vena cava.

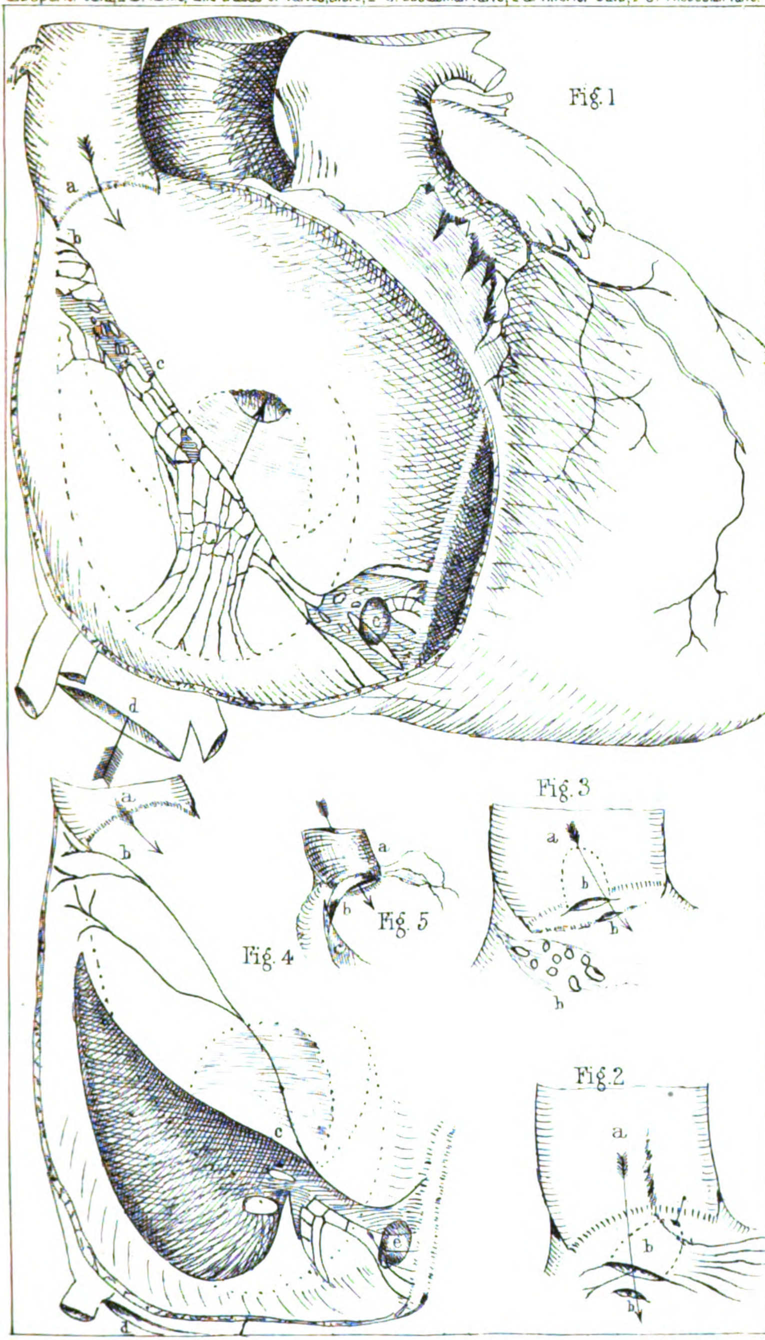
After alluding, *secondly*, to the significant case shown and described to the Society on the 4th ult. by Professor Turner, Dr Handyside showed (fig. 2), and gave a sketch of—

Thirdly, another specimen from his rooms, which presents on the posterior wall, and at the rim of entrance, of the superior cava a well defined and deep lacuna, within which open five large Thebesian foramina. The caval half of the patulous border of this lacuna is protected by an oblique semilunar valve, one quarter of an-inch in breadth and of a like depth. This valve is composed of a duplicature of endocardium, and is evidently designed to secure the entrance of blood into the auricle.*

The author next compared shortly these three abnormal cases, demonstrating how they formed at once a graduated scale or anatomical series mutually illustrative, each specimen introducing to the mind its own peculiar features of morphological interest. He considered, however, these cases as chiefly interesting—when compared with the results of certain changes undergone by the human embryo between the third and the seventeenth weeks after conception—in relation with the structural affinities they present to permanently impressed forms in some cold and warm-blooded

* The additional sketches, figs. 3 and 4, represent peculiarities in two adult hearts, aud fig. 5, in a fœtal heart, dissected in Dr Handyside's rooms. In fig. 3 a valve (¼ inch broad by ¼ inch deep), formed of endocardium with an intervening lamina of striped muscle, lay within and parallel to the posterior segment of the rim of the superior cava. In fig. 4 is an unusually large persistent Eustachian valve, continuous at its left insertion with a still more remarkable Thebesian valve. The former, which is very large, has an insulated fibrous offset from its left extremity, which runs half-way across the sinus venosus towards the tubercle of Lower, and then dichotomously divides and subdivides thrice before it is implanted into the arched line between the right cornu of the Eustachian valve and the right segment of the rim of the superior cava. Fig. 5 represents, in a male fœtus of 6½ months, presented to Dr Handyside's museum during last summer, the occurrence of a complete semilunar valve situated at the termination of the upper vena cava, its convex border being attached to the anterior and right wall of the vein, its concave free border projecting into the auricle. The right crus of this valve is inserted into the auricular wall on a plane ¾ of an inch behind, and decussating, the right ascending cornu of the prolonged Eustachian valve.

a. Superior Cava, _ b. Valves, and traces of Valves, there, _ c. Eustachian Valve, _ d. Inferior Cava, _ e. Thebesian Valve.



W. & A. K. Johnston Edinburgh.

TRACES OF TRANSITIONS IN THE FŒTAL HEART.

(natural size)

animals; after a like manner to what is seen in the venous system of fishes as a *permanent* form of structure, and yet presents itself, but *temporarily* in the history of the development, during their embryo state, of *higher* vertebrate animals. He referred to homologues in the Myxinoidei family of *fishes;* to the Ophidian order of *reptiles;* among *birds*, to the *Aquila chrysäetos* and the *Casuarius emeu;* and among *mammals*, to the *Delphinus phocœna*, the *Macropus*, the *Elephas*, the *Stenops potto*, the *Simia gorilla*, and the *Simia troglodites*.

The author's view then, morphologically, is—1, That structures evolved *progressively*, and corresponding at given points to *certain permanent states in the vegetable and brute creation*, may uniformly be traced in man at fixed periods of his embryonic and fœtal existence; and accordingly,—2, that vestiges in the adult heart of early arrested and merely temporary structures—such as those narrated by the author in this paper—record distinctly, in his opinion, the existence *of definite stages of embryonic and fœtal development in man*, through which stages he invariably passes towards his perfect adaptation for higher functions.

(*Part Second* of those observations—On the Permanence of the Foramen Ovale—is reserved for another paper).

The following Gentleman was elected a Fellow of the Society:—

James Dewar, Esq., Assistant to the Professor of Chemistry in the University of Edinburgh.

Monday, 1*st March* 1869.

Sir WILLIAM THOMSON, Vice-President, in the Chair.

At the request of the Council, Professor Allman gave an account of the Anthropoid Apes, chiefly with reference to specimens recently acquired by the Edinburgh Museum of Science and Art.

In taking cranial capacity as a ground of comparison between the Anthropoids and Man, he gave some measurements of skulls contained in the Museum. The capacity of the cranial cavity in the Gorilla skeleton belonging to the Museum is 29 cubic inches. Considerably larger gorilla skulls, however, have been measured. The smallest healthy adult human skulls in the Museum are those of two Andaman Islanders. Of these, one has a cranial capacity of 75·5 cubic inches, and the other a capacity of 72 cubic inches. Next to these comes a Peruvian skull; its cranial cavity measures 76 cubic inches. The smallest healthy adult human skull measured by Morton was also that of a Peruvian, whose cranial capacity measured only 58 cubic inches.

In taking the other points of comparison of the Anthropoid Apes with Man, it was shown how most of the alleged differences of conformation disappear on a strict examination; and the author defended the views of Huxley and those anatomists who insist on the development of the posterior lobes of the cerebrum and their associated structures in the Quadrumana, as in Man.

A question, however, still remains for determination. After eliminating all the tenable points of divergence, what value are we justified in assigning to the characters which still remain?

It is quite certain, as has been urged with great force by Huxley, after an exhaustive comparison, that in physical conformation Man approaches more nearly to the Gorilla and the Chimpanzee than these do to the lower Quadrumana. Must we, therefore, while the Gorilla and these lower Quadrumana are included in a common zoological order, unite Man with them as another member of that order?

He believed not. It had been already urged as an argument in favour of the unity and generally accepted limits of the order Quadrumana, that while the Gorilla graduates by intervening forms into the lower monkeys, there are no connecting forms yet discovered between Man and the Gorilla. He considered this argument, so far as it goes, a valid one, but another might be derived from a comparison of the Lemurs, or lowest Quadrumana with other Mammalian orders. Such a comparison will show that man is more widely removed from the higher apes than the Insectivora are from the Lemurs, and as long as the Lemurs are retained among the Quadrumana there will be less tenable grounds for admitting man into the same zoological order with the apes than there would be for admitting into this order the insectivorous moles and hedgehogs.

The relations here insisted on are rendered apparent by the following diagram, in which the Lemurs and the Gorilla are taken as the two extremes of the Quadrumanous series, and the close relation of the Insectivora to the series through the Lemurs indicated by converging arrows, while the less intimate relation of Man to the same series through the Gorilla is indicated by diverging arrows.

INSECTIVORA ➣→←◄ Lemurs, Monkeys, Gorilla ←◄➣→ MAN.

From these facts, the author found himself compelled to retain the zoological group BIMANA as a legitimately constituted order distinct from that of the QUADRUMANA.

But after all, it is not in any recognisable physical conformation, but in psychological phenomena, that the grand differences are to be sought for between man and even the most intelligent of the brutes; and the author argued, that though there be some intellectual endowments which are undoubtedly common to man and the higher brutes, there are others which are exclusively man's, which differ from those of the brutes not only in degree but in kind, and of which, after the most careful analysis, not even a rudiment can be detected in the intellectual phenomena of any animal below man.

It was maintained that a resemblance between certain acts performed by brutes, and others met with in man, may occasion an erroneous interpretation of the former, while a rigid comparison would show that the resemblance was only superficial and deceptive. Such, for instance, as the apparent faculty of conceiving the relation between cause and effect, the acts which so frequently seem to indicate a conception of causation in the lower animals being the result either of instinctive impulse or of suggestive association.

Among the faculties which the author thus regarded as eminently distinguishing man, there was one which did not appear to have hitherto obtained the attention it deserves. He alluded to what may be termed *imitative delineation* and *imitative constructiveness*, or the faculty of imitatively expressing the forms of objects by drawing and construction. While a faculty of imitating gestures and sounds is possessed by many brutes, and while *instinctive* constructiveness is almost universally found throughout the animal kingdom, the faculty of imitative delineation and imitative constructiveness is eminently a human one; we find it in the lowest and most undeveloped savage, and almost the very earliest evidence we possess of human intelligence—the intelligence of a period when man was as yet the contemporary of the mammoth, and when the reindeer extended its range into Southern Europe—is to be found in portraits of that mammoth and of that reindeer scratched upon fragments of their tusks and horns. And yet there is no physical obstacle to the exercise of this power by animals; the beak and the claw are implements as effective as the rude flint point of the savage, and there is nothing in the physical conformation of the Gorilla to prevent that flint point from becoming in his hand an implement of design.

The following Gentleman was elected a Fellow of the Society:—

Rev. H. Calderwood, LL.D., Professor of Moral Philosophy in the University of Edinburgh.

Monday, 15th March 1869.

DAVID MILNE-HOME, Esq., Vice-President, in the Chair.

The following Communications were read:—

1. Motion of a Palladium Plate during the formation of Graham's Hydrogenium. By James Dewar, Esq.

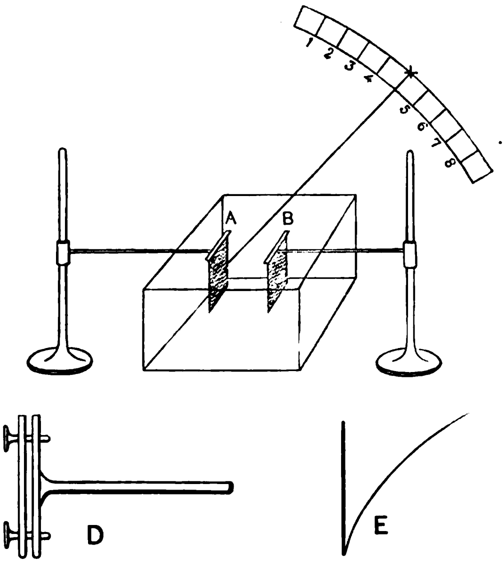
(Received 1st March 1869.)

Graham, in continuing his exhaustive researches on diffusion, has recently examined the relation of gases to various colloid septa. The remarkable discovery of Deville and Troost of the permeability of platinum and iron by hydrogen at a red heat, he has expanded into a general examination of the relative rates of passage, at high temperature, of the various gases through different metallic septa. Further, he has proved that different metals have a specific occluding power over certain gaseous elements, retaining them in combination at low temperatures, although the absorption took place at a red heat. Of the many astonishing discoveries made during the course of these investigations, probably the most remarkable is the occlusion of hydrogen by palladium. This metal, whether in the form of sponge or hammered foil, when heated and cooled in an atmosphere of hydrogen, absorbed between six and seven hundred times its volume—increasing to the enormous occlusion of 982 volumes, when the metal used had been deposited by voltaic action. This occlusion of hydrogen, Graham has shown, can be easily effected at low temperatures by making palladium the negative electrode during the electrolysis of water. He has also shown that the metal, charged with hydrogen, has increased greatly in volume, and that its physical properties are entirely modified. So marked is the change in the physical, electrical, and magnetic properties of the combination, that the only class of compounds we can compare it with are the metallic alloys. In the occluded state the chemical intensity of hydrogen is increased, many reactions being effected by its agency beyond the power of the element in the free state. Graham, as a general result of his experiments, considers the occluded gas to exist in the form of a solid, with all the physical properties of a metal. During the course of an experimental exhibition of Graham's discovery, I noted several phenomena associated with the occlusion of hydrogen by palladium, when it is made the negative electrode during the electrolysis of water; and as they illustrate, in a new form, the results already arrived at by the Master of the Mint, with his permission I am induced to bring them before the Society.

If a palladium plate, used as the negative electrode during the decomposition of water, be arranged at right angles, instead of parallel to a similar platinum plate, the hydrogen in a short time is evolved at the edge of the palladium plate nearest to the platinum electrode—no trace of hydrogen coming from any other part of the plate. Gradually, as the saturation takes place, the hydrogen seems to travel slowly along the plate, and only after saturation is it freely evolved from the whole surface of the electrode. If we now reverse the current, so as to evolve oxygen at the palladium plate, immediately the nearest edge begins to evolve gas, the rest of the plate remaining tranquil, the evolution of oxygen moves along the plate in a gradual manner. This gradual transference depends on the time necessary to effect the occlusion, and on the relative intensity of the lines of force.

When a palladium plate, charged with hydrogen, is brought in contact with a platinum electrode freely evolving oxygen, the evolution of gas is immediately arrested over the entire surface of the electrode. The same plate, free from hydrogen, when brought in contact with a platinum electrode evolving hydrogen, shifted the evolution of gas only on the same side on which it was firmly pressed. In order to examine the action of mixed electrodes, the palladium plate was welded with a similar platinum plate into a V shaped electrode, when the apex of the combination could be placed in or out of the liquid. If the platino-palladium electrode is made the negative pole, hydrogen makes its appearance immediately on the platinum plate; no gas is evolved by the palladium for some time. If, after saturation, by reversing the poles, oxygen is thrown on the mixed electrode, no gas is evolved from the platinum, and when the gas began to be evolved, it appeared simultaneously on both plates. The same result is observed whatever may be the position of the compound plate relatively to the other pole in the liquid, relative distance from the other pole having no effect. This proves that the whole of the oxygen that ought to be evolved on the platinum is transferred by a polar chain, through the liquid, on to the palladium plate, so long as this plate contains occluded hydrogen. The presence of the strong electric current is shown by connecting the hydrogenised palladium with a platinum plate in an acid liquid, the circuit containing a galvanometer. If the apex of the V electrode is placed in the acid liquid additional phenomena are witnessed, depending upon which side of the compound electrode is next to the other electrode. If the platinum side of the V electrode is firmly clipped in a stand, a glass rod keeping the apex in the same position in the liquid, and if the palladium plate is next to the positive electrode, we observe the following change during the course of the hydrogenation. The angle of the V continually diminishes by the motion of the palladium towards the perpendicular, the hydrogen evolved coming only from the outer surface to the compound plate. After some time the plate returns to its original position, and would curve beyond it if the action was continued. If oxygen is now evolved on the compound plates, the first effect of the oxygen is to curve it beyond its first position, or to increase the angle of the V. If the palladium plate was furthest from the positive electrode, the first effect of the hydrogenation is to increase the angle of the compound plate by the palladium moving outwards; after some time it returns. Similar observations, as with the palladium clipped in stand, but made with the junction out of the liquid, showed a decided movement depending on the relative position of the plates. Seeing the palladium moved, although firmly clipped in a socket out of the liquid, it was evident the motion could be examined without the use of compound plates.

After devising several arrangements in order to examine with ease the motion of the plate, the following plan was found to be the best in practice:—



The electrodes of palladium and platinum were firmly clamped in the little vice represented by D, and arranged as shown in the figure, where A represents the palladium and B the platinum. To the lower edge of the palladium plate a narrow strip of the same metal is fused by the oxyhydrogen blow-pipe; the strip is of such a length as to project above the level of the acid liquid when the plate is immersed. To the end of the strip of palladium a thread of glass is fused so as to have a radius in all of 40 or 50 centimetres; the arc of a divided circle of the same radius, on a piece of cardboard, is supported by a stand at the extremity of the index.

By this apparatus the small deflection of the palladium plate is greatly magnified, and the direction of motion well defined. Suppose the palladium plate, A in the figure, is connected with the negative pole of the battery, the glass index, after a short time, begins to move from left to right on the plane of the diagram, to the extent of 8 or 10 centimetres on the scale. As the saturation goes on, the index begins to move backwards from its first position, going towards the left even to a greater extent than its first deflection towards the right. Continuing the action, it again returns to near its original position. The power of being able to return to the position it had at starting seems to depend on the position of the plate, as regards the distribution of tensile strain, produced by rolling; at least, after repeated use the plate lost the power of returning, after having passed towards the left. If the plate, after saturation, is connected with the positive pole of the battery, the first effect on the index is to move quickly towards the left, then to return to where it was—this double motion taking place before any gas makes its appearance on the palladium. If the platinum electrode B is placed on the opposite side of A, and the saturation of the plate repeated, the index goes through the same series of positions, but the direction of motion is reversed. The direction of motion depends, therefore, on the relative positions of the electrodes, but is constant for the same position. This is easily shown by allowing the index to commence its motion, say from left to right; then, by moving the positive electrode to the other side of the palladium plate, the motion immediately commences in the opposite direction, although the saturation was far from being complete. The motion of the index, when oxygen is thrown on the hydrogenised plate, depends also on the position of the electrodes. The index has also a motion at right angles to the plane of the scale, the resultant motion being compounded of the separate flexures of the plate. Many other devices could be used to show the motion, such as a plate bent into the form of a cylinder with a narrow channel left between the two edges, which should shut and open alternately, according as the positive electrode is without or within the cylinder.

Graham has shown that the formation of the alloy of hydrogenium and palladium is attended with an enormous increase in the volume of the metal. He found that a wire of palladium 100 millimetres in length became 101·5 millimetres when saturated with hydrogen. Now, if a uniform hydrogen atmosphere surrounds a symmetrical piece of palladium, there is no reason why it should penetrate with a greater rapidity one surface rather than any other. But if the absorption is not uniform on all the surfaces, from want of uniformity in the hydrogen atmosphere, the surface absorbing must produce a flexure of the plate from the expansion of the metal. If a thin plate of this rigid metal can be so arranged as to induce absorption on one side rather than the other, then, as a necessary consequence, the plate will become convex on the side where the greatest relative absorption is taking place; and as the saturation approaches uniformity, the convexity should disappear, the plate regaining its original form if the elasticity of the metal is not changed during the action. A plate of palladium, when it functions as the negative pole during the electrolysis of water, is subjected during the course of the action to the supposed non-uniformity of the gaseous atmosphere—if the surface of the plate is parallel to a similar platinum electrode. The amount of chemical action effected by the unit surface of the electrode, and its distribution, depends upon the position of the plate relatively to the lines of force emanating from the poles. Now, of the two surfaces of the palladium plate, the surface next to the positive pole has the greatest concentration of the lines of force, and therefore the greatest evolution of hydrogen takes place on this surface, so that the quantity of alloy formed in the first instance is in excess of the amount produced on the other side. The plate, from the great expansion on the outer side, becomes convex, until the progression of the action from the other side is able to compensate this flexure by a corresponding expansion, thereby bringing the plate to its original form. But I have already explained how this backward motion goes beyond the original position, producing a flexure in the opposite direction, only in some cases returning to its normal shape. This, in all probability, arises from the combined effect of expansion and compression. Graham has shown that this tenacity of the alloy is considerably diminished as compared with that of the palladium, so that in the first flexure of the plate the expansion has to compress the rigid palladium, whereas, when it begins to return, the compression is effected on the less rigid hydrogenium already formed on the other side, the elasticity of form being also probably relatively small. This is the general explanation of the motion of the plate, but it must be remembered that the smaller flexures produced by different forms and positions of the electrodes are the result of non-homogeneity or excessive strain. This is one position, at least, of the palladium relatively to the other electrode, where the plate should have no lateral motion—that is, when it is at right angles to the surface of the positive plate; but all the attempts made with the small palladium plate at my disposal failed to prevent lateral motion, probably from want of uniformity of surface producing different rates of penetration.

Effect on the Current during the Formation of the Hydrogenium.

In order to determine the effect on the current during the occlusion of the hydrogen and its reoxidation, two Bunsen's cells were connected, through a tangent galvanometer, with a plate of palladium and platinum as poles in a cell containing acidulated water. The intensity of the current was determined—1st, during the decomposition of the water, when the palladium pole evolved oxygen; then during the absorption of hydrogen, when the current was reversed; and lastly, when the oxidation of the occluded hydrogen was taking place.

	Palladium. Positive Pole.	Palladium. Negative Pole.	Hydrogenium. Positive Pole.
Angles on tangent galvanometer,	23°·5	21°·5	40°·3
Tangents,	0·4348	0·3939	0·8541

The diminution in the intensity of the current during the formation of the hydrogenium arises from the strong current, generated in the decomposing cell by the secondary polarity of the electrodes, acting in the opposite direction, whereas the oxidation of the hydrogenium reduces a current acting in the same direction as the battery. The intensity of the current during the oxidation of the hydrogenium is nearly doubled, so that if we consider the resistance in the circuit to remain constant, the additional electro-motive force added by the oxidation of the hydrogen is equivalent to two cells of Bunsen. The great increase in the intensity of the current during the oxidation of the hydrogenium may be shown in the following manner:—Use two palladium plates in the decomposing cell, with index to show the motion of the plate attached to each, as in the figure formerly given, and include in the circuit a commutator and a fine spiral of platinum wire. During the occlusion of hydrogen the platinum spiral may be made of such a length as to remain dark, the motion of the index proving the absorption of hydrogen. If the direction of the current is now reversed the spiral of platinum becomes red hot, the index moving rapidly back to its original position, while the index of the other plate begins to move. The oxidation of the occluded hydrogen is the limit to the brightness of the platinum spiral, so that a reversal of the current produces a renewed brightness. By this arrangement we keep both indices moving along the scale, and the platinum spiral alternately bright and dark. If the electro-motive force is really equal to a Bunsen's cell, this arrangement must produce a current far more intense than any similar gas-battery, where oxygen and hydrogen are the reacting elements; but as the resistance in the circuit might vary, several indirect experiments were made. When a palladium plate, saturated with hydrogen, was associated with a platinum plate in an acid solution of permanganic acid, and connected through a voltameter, the arrangement could decompose water. A similar result was obtained when a platinum plate coated with platinum-black saturated with oxygen was employed; but the action in this case was very slow, and sometimes did not succeed. A platinum plate covered with peroxide of lead was opposed to the hydrogenium; the combination decomposed water with facility. Grove found two pairs of his gas-battery could effect a slow decomposition of water, and a single pair did as well if the oxygen was replaced by peroxide of lead. (The intensity of palladium and peroxide of lead combination must render it particularly well adapted to form secondary piles of great intensity, by substituting it instead of a lead plate in the arrangement devised by M. G. Planté). The transformation of gaseous hydrogen into the occluded state would seem to have little effect in reducing its total chemical energy, so that the occluded hydrogen must retain a relatively large proportion of the total gaseous energy in a potential form. If we compare the occlusion of gases to work done on the gas, the elaborate researches of Joule and Thomson on the "Thermal Effects of Fluids in Motion," would lead us to believe that hydrogen of all gases would in this new condition retain the greatest amount of its original store of energy. But before a just comparison can be made with the results of Joule and Thomson, careful determinations must be made of the electro-motive force, latent heat, &c., of hydrogenium. Professor Tait has determined by a new process the electro-motive force of platinum and palladium covered with oxygen and hydrogen, and the result will be communicated to the Society in a short time.

2. Some Observations on Free, An-atomic, or Transmissible Power. By R. S. Wyld, Esq.

This is an abstract of the first of two papers on the subject, prepared for the Society.

Physical bodies may be held, in accordance with chemical science, to consist of aggregations, either of material atoms, or of atomic circles, or centres of force.

There are certain exhibitions of physical power observed in connection with those atomic bodies, the laws and actions of which are in the highest degree worthy of study. Mr Wyld calls power exhibited in this way, free an-atomic or tranmissible power.

Mr Wyld called attention to seven exhibitions of this free power, viz.—

1st, The attraction of gravity; 2d, cohesive power; 3d, the power of chemical affinity; 4th, animal power; 5th, the momentum of moving bodies; 6th, the electric force set free on the mechanical disintegration of quartz and many other crystals; 7th, the force obtained alike through the decomposition, the dissolution, and the combustion of simple and compound bodies.

The consideration of the nature and source of animal power Mr Wyld reserved to his second paper.

In the present paper Mr Wyld directed attention to the *momentum* or force of moving bodies, and to the law of its transmission from one body to another, as illustrated in the case of one rolling ball hitting another and imparting its force to it—and in the case of those atmospheric and ethereal vibrations which affect us with the sensations of sound and light.

When I roll a ball along the ground I communicate force to it; this force carries it forward till it encounters a body at rest, to which it communicates this force, and the force which came from me, may thus be transmitted or handed over, from ball to ball, through a lengthened series, subject only to such decrease as may arise from friction or other accidental obstructions.

Mr Wyld called attention to the circumstance—first, that the balls successively received something which we call force; second, that it is force which causes their movement, and not their movement which gives them their momentum or force. Their movement, however, seeing it is the result of the force, may be taken as a just measure of the force applied.

Another instance of the transmission of force we have when we direct, what has for convenience been called the electric or galvanic fluid, through wire conductors. The transmission of this manifestation of force may be explained on nearly the same principle as the transfer of the *momentum* of moving bodies, and of the mechanical vibratory force transmitted from molecule to molecule of the ethereal *medium*, which vibratory force we call light, and which reaches us after a passage of more than ninety-one millions of miles.

Mr Wyld considered these to be instances of the action, and transmission of what may be called free, an-atomic, or transmissible power, which is not to be viewed as a substance or fluid, but either as an imponderable immaterial agent, or simply as *the action of an unseen cause.*

Neither the nature of its origin, nor the nature of its transmission, nor the variety of its actions and operations, permit us to consider it as a substance. When we disintegrate crystals, when we separate the parts of fluids by evaporation, when we dissolve metals, or decompose compound bodies, organic or inorganic, we seem to obtain that force which held their parts together, in the form of free transmissible power. To this power we can by certain arrangements give a definite direction, and can transmit it through wire conductors. We can thus make it available for the production of many of the most important natural phenomena, for instance—

1. The production of intense artificial light.

2. The production of heat.

3. The decomposition of compound bodies.

4. The volatilising of metals and other bodies by the sheer violence of its movements.

5. The production of magnetic attraction.

6. When transmitted through the animal frame it overpowers our natural voluntary movements, and produces spasmodic muscular contractions.

To these different phenomena, for purposes of convenience, we give different names, but they are all the action of the same agent, viz., of power operating under different circumstances.

We very much simplify and clear up our conceptions of nature when we attend to these two facts—first, that every change observed in the physical world,—whether effected by natural or by artificial means, and whether connected with animal or vegetable living organisms, or with inorganic matter, may be described as a movement either of physical bodies in mass, or as a movement of their constituent parts or atoms; and second, that all these movements are effected by free an-atomic power, and by the transmission of it from one body to another.

Mr Wyld further summed up his views thus—

1. In the physical world we have a manifestation of power working uniformly according to physical law, *i.e.*, it is measured in amount, and it is fixed in its modes of action.

2. Physical power we ascertain by an examination of physical phenomena is subject neither to augmentation nor to diminution. Were it not so the world would be entirely different from what we observe it to be.

3. Power in all physical operations is transferred from one object to another, but is never lost.

4. We may regard physical power as an immaterial agent, or we may be satisfied to consider it simply as a mode of action brought about by a *cause unknown*. But whether we view it as an agent or as an action, and in whatever way we may account for its existence, it is evident that as it conducts every change in the physical world, it must act according to invariable physical law.

5. Physical power manifests itself in two distinct forms—namely, in the form of atomic bodies, which are discoverable by the senses,

and which consist of aggregations of atomic circles or centres of force; and in the form of free an-atomic or transmissible power which is not discoverable directly by the senses, but is known only by its effects on atomic bodies. The question whether the chemical atom is a material atom, or is purely dynamical, does not affect the truth of the writer's statements.

6. The mind seems conscious of possessing power, and of being able to exercise it for the production of physical movements. It has evidence of this when it exerts itself in the production of those muscular efforts which are subject to the control of the will. The mind is thus the agent in the production of voluntary movement through the free transmissible power which it sends to the muscle, as will be explained in the subsequent paper on this subject.

7. Free power effects the various important physical phenomena alluded to, *e.g.*, the production of motion—and the continuance of it as momentum, on which depends, not only the movements of the earth and planetary system, but the transmission of light and sound as above explained.

Mr Wyld explained that by the words physical law he meant to express simply the invariable modes of action observed in physical phenomena. The existence of action, however, always infers the existence of an agent or actor. This is not the most suitable place to inquire regarding the origin or cause of power, but it will be admitted that we discover nothing in atomic bodies to account for its existence. These seem rather to be the subjects of power than the cause of it, for all their movements and changes seem regulated and caused by it. They are therefore quite insufficient to account for it, or to explain it.

This inability to discover a visible cause for the laws of nature compels us to believe in an invisible cause, adequate to account for them. When, therefore, we discover power acting in the production of order, beauty, and enjoyment—and when we recognise ourselves as moral and intelligent beings brought into close connection with this physical system, we are drawn all but irresistibly to the conclusion that physical power is the sustained action of a mighty Being, possessed of the self-consciousness and intelligence with which we find ourselves to be endowed.

3. On the Practical Application of Reciprocal Figures to the Calculation of Strains on Framework. By Professor Fleeming Jenkin.

4. On a New Synthesis of Ammonia. By A. R. Catton, Esq., late Fellow, and P. T. Main, Esq., Fellow of St John's College, Cambridge.

It having been shown by one of us that formic acid is produced by the union of carbonic acid and nascent hydrogen, it occurred to the authors to try whether other gases might not be made to unite directly with nascent hydrogen. Such experiments gave the prospect of being of especial interest in the case of nitrogen, as it was possible the synthesis of ammonia might thus be effected by the direct union of nitrogen and hydrogen.

With this view pure nitrogen and aqueous vapour were passed over sodium. The nitrogen was prepared in the usual manner, by passing atmospheric air over heated copper, and was freed from ammonia by passing it through sulphuric acid. Not a trace of ammonia, however, was obtained in this way. When, however, nitrogen and aqueous vapour were passed over heated magnesium ribbon, a very appreciable quantity of ammonia was produced, and the quantity appeared, within certain limits, to be greater the higher the temperature to which the magnesium was heated. The ammonia was recognised in the usual way—by the smell, by its browning considerable quantities of turmeric paper, and by the formation of the double chloride of platinum and ammonium. The theory of this reaction evidently is, that the magnesium decomposes the aqueous vapour, and that the hydrogen thus produced, when in the nascent state, combines with some of the nitrogen, and forms ammonia. Only a small part of the nitrogen, however, is in this way converted into ammonia. One cause of this evidently is that the magnesia produced during the reaction forms a coating on the surface of the magnesium ribbon, and so protects it from further action. For this reason it has occurred to the authors that larger quantities of ammonia may be obtained by using powdered magnesium, either by itself or mixed with fine sand, as in Larkin's magnesium lamp. The authors, however, have no hope of modifying the process in such a manner as to convert any considerable proportion of the nitrogen of the atmosphere into ammonia, a problem of the highest practical importance. Nevertheless, the formation of ammonia in this way is of considerable theoretical interest, especially when viewed in connection with the source of the ammonia in nature, a question to which no satisfactory solution has yet been given.

We purpose, when circumstances permit, to continue our experiments on the subjects indicated in this paper.

The following Gentlemen were elected Fellows of the Society:—

Principal Sir ALEXANDER GRANT, Bart.
Captain T. P. WHITE, Royal Engineers.

The following alteration proposed by the Council on the Law relative to the Election of Honorary Fellows was adopted by the Society:—

Law XII., lines 6 and 7, in place of the words, "and printed in the Circular for the meeting at which the ballot is to take place," read, "and printed in the Circulars for two ordinary meetings of the Society, previous to the day of election."

Monday, 5th April 1869.

Dr CHRISTISON, President, in the Chair.

The following Communications were read:—

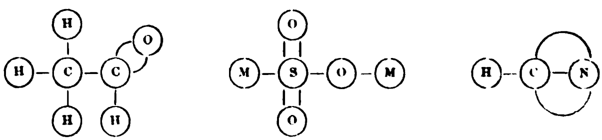
1. On the Present State of the Sun's Distance Question—*continued.* By W. Petrie, Esq. Communicated by Professor C. Piazzi Smyth.

This paper is a compilation and discussion of all the best determinations of the sun's mean distance and parallax within the last few years, and up to the present time. The comparative weights to be given to the different observations or results are entered into at length, on principles which are stated, and a final mean of the whole is arrived at, giving for the solar parallax 8·″877. This is then compared with the result of the so-called Great Pyramid Sun Distance, which, combined with the best modern determination of the size and shape of the earth, gives a parallax of 8·″876.

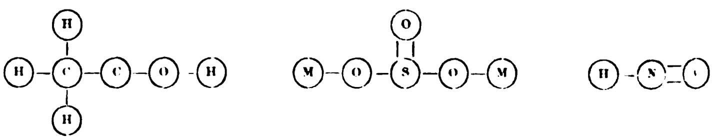
The direct results of modern science for the solar parallax vary anywhere between 8·″80 and 8·″96; and to assist in apprehending the present state of a very contested question as to one of the most important of all physical facts, Mr Petrie added to his paper two very instructive diagrams, in one of which the chief data were arranged chronologically, and in the other quantitatively.

2. On Chemical Structure. By Dr A. Crum Brown.

In this paper the author drew attention to the nature of the process of *Chemical Exchange* (including substitution, double decomposition, and some oases of direct addition and subtraction), as a process by which the *number* of relations in which any given atom stands is unaffected. He pointed out the connection between this process and the notion of chemical structure, and discussed the nature of those chemical processes in which the number of relations of an atom is increased or diminished. He concluded by some remarks on the ambiguous structure of such bodies as aldehyde, the sulphites, and hydrocyanic acid, which from some points of view may be represented as—



and from others as



3. Observations on the Temperature of Newly-born Children. By T. J. Maclagan, M.D., Dundee. Communicated by J. Matthews Duncan, M.D.

That the temperature of the child is not the same as that of the adult has long been a generally acknowledged fact, but wherein and to what extent it differs is a point on which definite information is wanting. With the object of determining this point, the author took advantage of the opportunities afforded by a residence in the Edinburgh Maternity Hospital, to note the temperature of a number of children at the time of birth, and at frequently repeated intervals during the first few days of extra-uterine life. The result of these observations was to show that the child at birth partook of the temperature of the mother—that as soon as it commenced its separate existence the heat imparted by the parent was no longer maintained, but was rapidly lost, till, in a few hours (the exact period varying in different cases), the thermometer indicated a temperature one, two, three, or even six degrees below the normal standard of the adult. A rise then took place, and generally within twenty-four hours after birth the range might be regarded as normal. This normal range of the child, however, was about one degree lower than the adult range of health. The rapid fall which was shown to take place immediately after birth, was believed to be due to the refrigerating influence of the external air on the blood in the lungs, the nervous influence which is requisite to give to the respiratory act its heat-producing effect, not being in full force at the time of birth. The lower range which existed during the first few days, after the primary temporary depression had been recovered from, was believed to be caused, in part at least, by the defective purification of the blood, consequent on the patent condition of the foramen ovale and ductus arteriosus.

In delicate and premature children all the peculiarities indicated were observed in an exaggerated degree.

4. On the Growth, Development, and Situation of the Human Fœtal Heart. By Professor Macdonald.

The earliest condition of the embryonic vascular system, as described by Wagner and other embryologists, consists of a venovascular chain of sinuses along the ventral surface of the serous or animal lamina of the embryonic germ disc, which, being suddenly arrested by a large projection of the germ vesicle at the anterior part beyond the vascular and mucous lamina, causes a stagnation in the form of a conical sac, whose apex points forward below the head of the embryo at that time consisting of the mesocrane and procrane, with the visceral segments of the face-kist, and exhibiting three facial clefts.

1. The ophthalmic, under the procrane, forming the roof of the orbit, and completes its floor resting on the antrum highmorianum.

2. The olfactory cleft, the nasal passage beneath the antrum, the ethnoid and turbinated bones, its floor being the hard palate.

3. The oral cleft, below the palate, forming the roof of the mouth, while the hyoid and tongue form the floor, and the maxilla and muscles of the cheek complete the cavity of the mouth.

I. As soon as the conical sac of the venous sinuses and azygeal veins from the chorda centralis (the future spinal column) extend to the upper angle of the first formed embryonic heart, where two tubes or inferent veins enter from the lower angle of its base, a single efferent tube—the ductus venosus—leading backwards, and sending forwards from the umbilicus a vein on each side (in birds towards the circular sinus, from which a fringe of minute capillaries extend below the chorion). In the capillary area, where the veins and arteries communicate, the venous blood becomes oxidised, and returns by the arterial area to the embryo; the minute arterial rootlets becoming larger and larger, till the blastine artery from each side enter the lower part of the embryo above the highly upturned coccyx, the lower orifice of the future umbilicus, when the abdomen is closed. From this point these arteries (the future umbilical of the fœtus) descend to the iliacs, which unite opposite the promontory of the sacrum, and carry the arterial blood upward along the chorda centralis, as high as the fovea cardiaca, or pharyngeal hollow, from whence the double aortæ each divide into two terminal branches, which unite behind the base of the heart to form the arterial aortic bulb, quite unconnected with the cavity of the heart, the top of the bulb seen rising higher than the heart, has misled embryologists to fancy it was a part of a twisted tube, which has sometimes been described as the first form of the embryonic heart. Pander, Prevost, and Dumas, as well as Wagner and Allen Thomson, describe the first heart as a conical sac, having its apex anterior beneath the head of the embryo, with two inferent veins entering from above, and only one efferent tube, the ductus venosus. Wagner mentions a certain amount of motion among the blood-discs, previous to the contraction of the conical heart, having only a single cavity. It is at first transparent and membranous, but contractile, and filled with a yellowish or pale orange fluid, the plasma or liquor sanguinis. As soon as the punctum saliens in the base of the heart starts its contraction, its fluid is forced down the ductus venosus to the oxidising area of the chorion, where it is fully arterialised, and follows the course, already shortly described, to the aorta, its terminal branches, forming the aortic bulb, terminate in the carotid and subclavian arteries to the head, upper trunk, and extremities. From thence the blood, reduced to a venous condition, is returned by the jugular and subclavian veins forming the superior venæ cavæ, which flow into the single cavity of the primary heart. The thin condition of its transparent walls might lead us to view it as analogous to the lymph heart of the frog and tail of the eel, so well described by Mr Wharton Jones, F.R.S., in a communication published in the last volume of the Philosophical Transactions, forcing on a column of dark venous blood discs.

II. Soon after the blood has circulated once or twice, there is a ventricle added to the base of the auricle, which is then perforated by the auriculo-ventricular orifice. Still there is only the same outlet for the venous blood from the heart, which is, during fœtal non-breathing existence, entirely connected alone with the venous circulation.

III. The next change in the structure of the heart is the dividing the ventricle by a muscular septum, which, till it is complete, allows the venous blood to enter freely into both ventricles (a system which continues in some reptiles), from which it is instantly returned to the auricle by their systole, and forced down the ductus venosus to be oxidised and returned as arterial by the umbilical arteries into the iliacs, and upwards by the aortæ, as already described.

IV. After the completion of the ventricular septum there commences a septum in the auricle, never completed for at least a year after birth, leaving the foramen ovale soon partially covered by a valve. Even subsequent to the formation of the quadrilocular heart of the fœtus, still the ductus venosus is the only outlet of venous blood sent to be oxidised and returned by the arteries to the aortic bulb for circulation. The foramen ovale (rationally explained) permits the free passing of the blood to and from the ventricles, and readily explains what Dr Churchhill could not—"That the pulse detected by the stethescope, when applied to the maternal abdomen, indicated twice the rapidity detected at the cord in cases of prolapsus or turning." The systole of the auricles preceding that of the ventricles may be detected by the ear; but being immediately followed by the stronger contraction of the ventricles tilting the apex against the sides of the chest, as in the adult, impresses the finger when applied to the cord with half the rapidity of the pulse indicated by the stethoscope.

V. Towards the close of pregnancy a much greater change is being inaugurated preparatory to the inflation of the lung-cells. The very beautiful demonstration by Dr Tonge by a well-conducted series of experiments on the aortic bulb of the chick, in a paper communicated to the Royal Society last year by Professor Beale.

Dr Tonge describes, towards the close of incubation, that there may be traced, extending from the upper left angle of the bulb, below the entrance of one of the left terminal branches of the aorta, the commencement of a fine film of its lining membrane descending diagonally to the lower right corner (the right aorta and its terminal branches having disappeared). This division into two tubes he describes as being closed by three semilunar valves, preventing the flow of the arterial blood into the ventricles.

While these changes are going on in the bulb, the right ventricle pushes a horn over the auriculo-ventricular septum and auricles, from the right margin of the heart upwards to the left side of the bulb, and passing behind it ramifies into right and left pulmonary trunks. The left ventricle, in like manner, pushes its horn behind the mitral valve, under the right auricle, and, rising over the upper part of the aortic bulb, carries off the innominata, carotid and subclavian arteries, as well as crushing off the last upper left terminal branch of the aorta (leaving a small portion, which quickly becomes merely tendinous, hitherto fancied to be "the ductus arteriosus," which has really no existence) between its own trunk and that of the right pulmonary artery, forming the arch of the aorta, it continues its course by the left embryonic ascending aorta (as soon as the first inflation of the lung-cells permits the flow of blood from the ventricles by their new outlet). The now arterialised blood flows down the thoracic and abdominal aorta to be distributed through the system.

The following Gentleman was elected a Fellow of the Society:—

J. Wilson Johnston, M.D., Bengal.

Monday, 19th April 1869.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. The Mean Pressure of the Atmosphere, and the Prevailing Winds for the Months and for the Year. Part II. By Alexander Buchan, Secretary of the Meteorological Society.

In Part I., read 16th March 1868, in which was discussed the Mean Pressure of the Atmosphere over the Globe for July, January, and the year, the method by which the Isobaric Charts were constructed was detailed at length. Since March 1868, valuable additional information has been obtained from Australia, New Zealand, Tasmania, Africa, South America, the west coast of North America, Iceland, and from a few isolated stations in Europe and Asia. The period for the British Islands has been extended so as to include the eleven years from 1857 to 1867.

Part II. gives the complete set of Charts for the twelve months and for the year. In this Part the prevailing winds were shown from good averages collected or calculated for upwards of two hundred places over the globe. The general result is that the surface winds of the globe flow round and inwards upon regions of low pressure on every side from which observations have been obtained, and that surface winds flow out of spaces of high pressure in every direction. This *outflow* of the winds from regions of high pressure, and *inflow* upon regions of low pressure, may be explained by the principle of gravitation. Indeed, so strongly is this force marked, on comparing isobaric lines with prevailing winds, that if there be any other force or forces concerned in setting the winds in motion, they must hold a very subordinate place. The influence of a mountain range in diverting aerial currents from their normal courses was illustrated by the prevailing winds in Norway; and an explanation was offered of the anomalous direction of the winds on the coast of West Greenland.

The relation of the data, graphically delineated on the charts, to climate was pointed out; for since winds bring with them the temperature of the parts of the earth's surface they have traversed, the charts may be regarded as furnishing the key to the climates of different countries. Their relation to the rainfall of the different months of the year was adverted to. Thus, by a careful study of the isobaric lines, and the prevailing winds, in connection with the isothermal lines, the climate of any region may be more closely approximated to than has hitherto been possible.

It was pointed out that the position of the isobaric lines in the different months appears to be entirely determined by the sun and the geographical distribution of land and water; and since these lines, in their turn, determine the climates of the globe, there is here evidently a principle applicable not only to the present state of the earth, but also to other distributions of land and water in past times; in other words, there is here a principle of the utmost importance to the geologist in attempting to account for glacial and warm periods through which the climates of Great Britain and other countries are known to have passed. If this conclusion be just, we are now in a position to give an approximate numerical statement of Lyall's idea of the effect produced on climate by the displacements of continents.

2. On the Rectangular Current Theory of Storms, illustrated by two Storms which passed over the United States, from the 13th to 19th March 1869. By Robert Russell, Esq.

The author's former views on American storms were first alluded to—*i.e.*, that the phenomena could only be accounted for on the supposition that there were two distinct currents. Instead of these being parallel, they were more nearly at right angles to each other. The southerly current on the eastern axis of storms brought up heat and moisture from the Gulf of Mexico, causing the barometers to fall. The west or north-west wind, on the other hand, cleared the southerly current away, causing the temperature everywhere to fall and the barometer to rise.

The storms under review were then examined by M. Le Verrier's mode of laying down isobarometric lines on charts, but for every tenth of an inch of pressure instead of two-tenths. The southerly current was shown to have distinct lines of contour and order from the other—the west or north-west at right angles to it.

The minimum line of the barometer where the currents meet, or where the west flowed into the southerly, was described and pointed out; the barometers obtaining their lowest reading, and thermometers their highest on that line, on any latitude, from the Lakes to the Gulf of Mexico. As formerly defined, in the Proceedings of this Society, "the latitudinal line of minimum barometer represents that space where the air, from the surface of the earth to the top of the atmosphere, is warmest on any latitude, and consequently lightest."

The charts of 13th and 14th March 1859 were first exhibited, showing that thunder storms with rains prevailed up the Mississippi valley, as far north as the southern border of the State of Michigan. At the same time a severe snow storm from the north-east prevailed over Minnesota, Northern Wisconsin, and Northern Michigan—a foot of snow falling over a large extent of country. The line of minimum barometer was greatly curved, or, what is the same thing, stretching from south-east to north-west in its northern part. The remarkable analogy between the main phenomena of this storm and that of the "Royal Charter" storm of 1859, was pointed out by the author reading a passage which he had formerly written regarding that storm, and merely substituting American dates and places for British.

The chart of 15th showed that the cold west wind, which was first felt in Texas at 9 P.M. of the 13th, and was at first travelling faster in the south than the north, had swept the whole country as far east as St Johns in South Carolina, and northwards as far as Oswego at the east end of Lake Ontario. The line of minimum barometer was then nearly straight from St Johns to Oswego, cold winds prevailing on the west, and warm on the east side of it.

The author then drew attention to the phenomena of calms that appear in the front and rear of all the American storms, as well as our own. The calms stretching southwards from Toronto, on the night of the 13th March, advanced eastwards at the rate of 10 to 12 degrees of longitude a day. On the morning of the 15th the calms were east of Nova Scotia. At the very same time another series of calms was produced on the west from Texas north to the west end of Lake Superior. Frost now visits Texas, where thirty-four hours before the thermometer stood at 82° in the shade. This series of calms was the forerunner of another and more severe storm then to the west of them. On the morning of the 16th the central axis of the calms was over the 85th longitude, having again advanced 10 to 12 degrees in twenty-four hours. The calms, then seen where there were a multiplicity of stations, showed that they were of the form of a pyramid, rounded at the top, but wide at the base. Along the shores of the Gulf of Mexico the region of calms was at least 700 miles broad, but not more than 200 miles on the Lakes. The advance eastwards of these calms from day to day, with the storm to the west of them, was then traced.

The storm on the west of the calms of the 16th was traced out. Charts were exhibited of the morning of the 18th and night of the 18th, showing its progress from west to east over the States, and that the west wind advanced faster over the Southern States than the Northern. As a consequence, the minimum line of barometer was largely bent over the Eastern States, stretching from north-west to south-east—this direction being exactly reversed from that of the morning of the 15th March.

The author then pointed out the relation which the winds to the west of line of minimum barometer bore to the winds on the east, according to the direction of this line with respect to the meridians.

1*st*, When the minimum line is straight, or nearly so, on the meridians, as on morning of 18th March, the winds change from a southerly or easterly direction to west or north-west. This holds true as far to the northwards as the pressure decreases from south to north on the line of minimum barometer, as it did over an extent of the line, on the morning of 18th March, for upwards of 800 miles. Under these conditions, a calm or lull of the wind occurs along the line before the wind changes suddenly to the west or north-west.

2*d*, On the northern border of the storm, where the line of minimum barometer is curved, or lying from south-east to north-west, as in Scotland during "Royal Charter" storm, the wind does not calm down, but veers from north-east to north and west.

3*d*, On the southern border of the storm, when the line lies from north-east to south-west, as in common in the valley of the Mississippi west of the river, the winds are more northerly there and along the Gulf than in any other part of the United States. When this direction of the line occurs in Scotland, we are liable to such eruptions of northerly winds as wrecked the "Ivanhoe" in December 1867.

4*th*, When the minimum line of barometer on the southern border of the storm lies from south-east to north-west an opposite state of things occur. The winds do not lull or veer at first; the cold current from the west rushes in behind the south-westerly warm winds, and causes the temperature to fall and the barometer to rise. The terminating winds on the east and west sides of the minimum line on the 15th and the 19th March were contrasted in this respect.

The relation as well as want of conformity in contour between the isobarometric lines of the cold west winds and those of the warm southerly winds were pointed out. The phenomenon of the kneeing-in of the isobarometric lines on the west of the minimum line was exhibited. The phenomenon was parallel in principle to that formerly shown by the author, as existing in the south of England and north of France on morning of 3d December 1863.

The author then pointed out the extraordinary different results that were obtained in measuring the rate at which storms progressed, but taking either the minimum line or minimum point. The French, and indeed the common method, is to take the minimum point, while Espy took the minimum line. Neither the one nor the other can be relied on to show the actual rate of progress of the storm. Taking either of these methods, the storm of the 14th advanced eastwards from Border Plains in Iowa to Oswego in New York State, about 900 miles in twenty-four hours, or upwards of 37 miles an hour. That of the 17th, by either of these methods, from Dubuque, in Iowa, on the Mississippi, to a little to the east of Lake Michigan, 240 miles, or 10 miles an hour. The further progress of this storm by either of these methods was 20 miles an hour. The author then maintained that the rate at which the calms shifted their place in front and rear of these storms furnished the true measure of their rate of propagation.

The author then gave his views as to the mode in which storms are propagated. The idea still maintained by the French, as well as by most meteorologists, that the falling barometers, high temperatures, and rains on the east side of storms, were actually translated from west to east, was quite indefensible. It was argued that the calms along the Gulf of Mexico, which were propagated in front of storms, at the rate of nearly 600 miles a day, or about 25 miles an hour, could not possibly be imagined to travel or be pushed along from west to east. If they were so, there could be no calm. So the red lines on the charts, showing the falling barometers and rains, like the calms, apparently travel from west to east. There can be no translation however. These phenomena are the effects of causes which are acting on the spot. On the morning of the 18th the southerly winds to the east of the line of minimum barometer, over an immense area, were all light, mixed with calms, and from almost every point of the compass. They could not possibly be in a state of translation from west to east at the rate of 20 miles an hour. The explanation is, the southerly winds bring in moisture, which is condensed by the cold of expansion, as the air over the region, swept by the warm southerly winds, ascends. It is a process which once begun tends to perpetuate itself. An inch of rain on an average fell over an immense area. According to Espy and Tyndall, as much force would be evolved on 50 square miles of any of these States (a mere spect on the charts) than the 100,000,000 of tons of coals that Great Britain raises in a year. The vapour, as it becomes condensed in rains, converts the atmosphere into a vast caloric engine, and produces vast effects. Twenty-five miles of calms on the west side of the calm region are every hour converted into easterly winds, which blow towards the line of minimum barometer, and 25 miles of calms are produced every hour on the east side; and besides, the air, as seen in chart of 16th March, is tossed over the calms, as an upper current from the west, and as it cools by radiation, descends and supplies the cold and dry winds that blow in the rear of the minimum line of barometer on the west side of the previous storm.

The rate at which the calms are propagated along the latitudes is the true index of the rate of a storm's progress. Whatever may be the particular character of the disturbance within, the calms on the east and on the west contain the whole body of the storm within their bounds. Their rate of propagation is the true rate of that of the disturbances within. With all deference to Espy, storms must be propagated faster along the south; for so long as the central axis of the calms is meridional, it must move more rapidly in the south, for the simple reason that the meridians are wider there.

3. On the Boulder-Clay of Europe. By D. Milne-Home, Esq.

It was explained that this deposit, known in Scotland as Till or Boulder-Clay, prevails also in Ireland, the Isle of Man, England (north of the Thames), Denmark, and Sweden, though much less abundantly in these last countries. It exists also in the Hudson's Bay districts, and at the south extremity of South America.

It had been a subject of perplexity to geologists ever since attention was drawn to it, fifty years ago, by Sir James Hall of Dunglass, whose papers are in the Transactions of this Society.

About the year 1840 a theory was started to account for the boulder-clay, and other phenomena allowed to be connected with it, by the action of glaciers.

At first, local glaciers were proposed; but latterly there seemed to be a disposition among Scotch geologists to adopt the startling speculations of Agassiz, that a gigantic glacier formerly existed, emanating from the North Polar regions, and flowing over the whole northern hemisphere.

The author said he did not adopt this view. He held that the boulder-clay was not a land deposit, but one which had been formed at the bottom of the sea; that it originally consisted of the ordinary beds of mud, sand, gravel, or boulders, such as are found generally at a sea-bottom; and that these beds were invaded by icebergs, which ploughed them up, and mingled them all together, so as to produce what is called boulder-clay. His reasons for thinking so were these—

1. True boulder-clay is not found in Switzerland, though, if its formation is due to the action of glaciers, it ought to be found there.

2. Boulder-clay is found in many places where it is not conceivable any glacier could have existed, or, at all events, have had any connection with the deposit.

3. An examination of the materials composing the boulder-clay showed that these materials had been in motion; and as it appeared that the motion was everywhere from the same direction, viz., from the west or north-west, it was impossible to explain such a uniformity of direction, extending over so large an area, without supposing that there had been a great oceanic current, which operated in producing that motion.

4. The circumstance, that almost everywhere, boulder-clay is associated with marine Pleistocene strata, and that in very many places in Scotland, Ireland, and England it contains marine fauna, is a strong proof that it has been formed at the bottom of the sea.

5. The boulder-clay shows, however, from the absence of stratification, from the broken state of the sea shells, and from the unnatural mixture in it of littoral and deep-water shells, that some agent has operated on it, causing great disturbance, and acting with immense mechanical power.

6. The accounts given by voyagers to the arctic regions, show that icebergs there disturb the sea-bottom, and produce operations very analogous to those which are inferred to have occurred in the production of the boulder-clay.

7. The character of the shells in the boulder-clay, and in other

associated beds, as well as the nature of the animals whose bones are found in it and in these beds, shows that at this epoch the climate of North-Western Europe was arctic; and the height at which shell beds of that period, as well as other marine strata, have been found, shows that Scotland must have sunk to the depth of at least 3000 feet below the present sea-level.

When the land was so submerged, icebergs came over the submerged land, and grating upon the sea-bottom, converted many of the beds into boulder-clay.

The phenomena of smoothed ridges and water sheds, the position of *blocs percés*, and the transport of boulders generally, can be better explained by floating ice than by glaciers.

8. The arctic climate of North-Western Europe was probably caused—(1.) By the Gulf-Stream flowing in some other direction than that now followed by it; (2.) By an arctic current flowing from the west or north-west; (3.) By the existence of high land in the North Atlantic, which increased the cold, produced glaciers and icebergs, and gave an easterly or south-east direction to the arctic current.

4. On the Development of the Flower of *Pinguicula vulgaris*, L., with Remarks on the Embryos of *P. vulgaris*, *P. grandiflora*, *P. lusitanica*, *P. caudata*,* and *Utricularia minor*. By Alexander Dickson, M.D. Edin. & Dublin., Regius Professor of Botany in the University of Glasgow.

The affinities of the order Lentibulariaceæ having hitherto been somewhat obscure, the author undertook the investigation of the floral organogeny of *P. vulgaris*, in the hope of being able to throw some light on the subject.

A plant of *P. vulgaris*, examined during the flowering season, exhibits a short axis with "radical" leaves spreading as a rosette, and terminated by an unstalked umbel of ebracteate flowers. In the axil of the last leaf is produced a bud, which, after the matur-

* The observations on the embryo of this species were made after the paper had been submitted to the Society.

ation of the fruit, supplants the now decayed main axis and forms the "autumn rosette." This autumn rosette, on the approach of winter, loses its expanded outer leaves, its central portion remaining as a firm bulb-like winter-resting bud, the outer leaves of which are developed as somewhat fleshy scales. In this bulb-like bud the rudiment of the inflorescence for the next season, along with that of the axillary bud of the last leaf, is to be seen. On the return of warm weather, towards the end of spring, the winter bud expands as the flowering rosette, while the axillary bud of the last leaf is being developed so as to form the autumn rosette as above described.

The author's principal results are as follows:—

1. The receptacle exhibits irregularity before there is any appearance of floral parts, its extremity being obliquely flattened downwards and posteriorly.
2. The calyx, corolla (apparently), androecium, and ovary appear first in their anterior part or parts.
3. The androecium in the young flower consists of two fertile stamens superposed to the anterior sepals, and two staminodes superposed to the lateral ones. The staminodes usually disappear at an early period.
4. The pistil originates as a semilunar elevation to the anterior side of the receptacular centre. Its extremities gradually extend themselves around that centre until the ovarian wall is completed posteriorly.
5. The placenta is strictly "free-central" being always unconnected with the ovarian wall. It has no barren apex corresponding to that in Primulaceæ; and the ovules (which are anatropal and present a single integument) appear in basipetal succession over its surface.

The author drew attention to several monstrosities, chiefly affecting the pistil. In some of these the posterior (small) lip of the stigma was bipartite, and in others the anterior (large) lip was tripartite. Combining these monstrosities, he drew the inference (which he held to be quite in harmony with the developmental facts) that the pistil of *P. vulgaris* consists of five connate carpels; its bilabiation being comparable to that of the corolla in *Utricularia minor*, where five parts are united into a structure with two *entire* lips.

The author is inclined to set aside the ordinary idea of a close affinity with Scrophulariaceæ. If the supposed correspondence in the number of carpels has no reality, the orders have actually nothing in common save bilabiation and didynamy; but these characters are found in so many different types that they are of very small value in determining the affinities of a given plant. On the other hand, he showed that Lentibulariaceæ, in having a free-central placenta and (probably) five carpels, differ little from Primulaceæ except in the superposition of the stamens to the sepals, and the exalbuminous character of the seed.

The author thought that Salvadoraceæ should be placed near Lentibulariaceæ, as had been done by Payer in his "Leçons sur les fam. nat. des plantes," and suggested that *Salvadora*, with oppositisepalous stamens and solitary exalbuminous seed, bears the same relation to Lentibulariaceæ with numerous exalbuminous seeds, as Plumbaginaceæ with oppositipetalous stamens and solitary albuminous seed, bears to Primulaceæ with numerous albuminous seeds: Salvadoraceæ with Lentibulariaceæ, on the one hand, and Plumbaginaceæ with Primulaceæ on the other, forming two parallel, nearly allied series.

The author also drew attention to the embryos of *Pinguicula vulgaris*, *P. grandiflora*, *P. lusitanica*, and *Utricularia minor*. A very brief statement by him of the differences between those of the two first had already appeared in the report of a meeting of the Dublin Microscopical Club, on 21st November 1867 (Microscopical Journal, viii. pp. 121–2); but a more detailed account was now submitted along with figures. He was able to confirm St. Hilaire's statement as to the dicotyledony of *P. lusitanica*, which, when contrasted with the monocotyledony in the two first named species of the genus, is very remarkable; and in a Mexican species, *P. caudata*, the author has also found two cotyledons. In *Utricularia minor* the embryo is somewhat globular, and at first sight appears to have a smooth undivided surface; on careful inspection, however, a remarkable conformation is to be observed of that end of the embryo which is remote from the hilum of the seed, viz., a minute *punctum vegetationis* surrounded by four very slight elevations forming the somewhat incurved sides of a square. He was not prepared to call these four elevations *cotyledons*, but considered the whole structure interesting as showing this embryo to be a little in advance of a mere "embryonal globule."

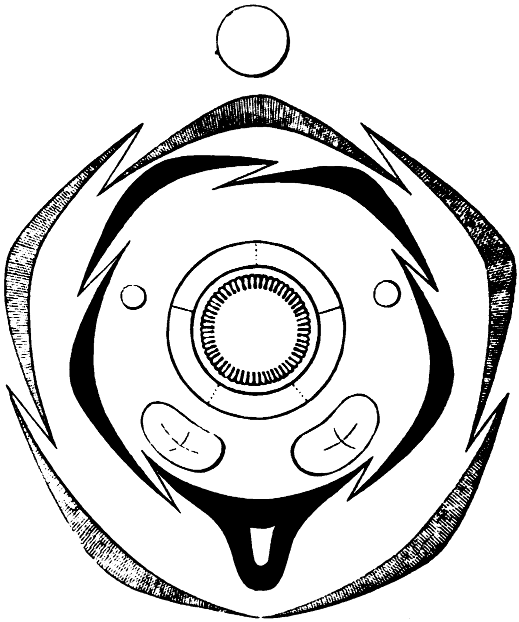


Diagram of the flower of *Pinguicula vulgaris*, L., showing the æstivation of calyx and corolla, the stamens and staminodes superposed to the anterior and lateral sepals, and the one-celled ovary with free-central placenta. The wall of the ovary is represented as divided into five parts by two plain and three dotted lines, the two plain lines representing the division of the stigma into two lips or of the capsule into two valves, the three dotted lines representing the abnormal fissures in the above mentioned monstrosities.

5. Observations on New Lichenicolous Micro-Fungi. By Dr W. Lauder Lindsay.

The author's "Observations" relate mainly to the Microscopic Anatomy of the reproductive tissues and corpuscles of about fifty Lichenicolous Parasites—apparently new species—referable partly to the true *Fungi*, and partly to the *Fungo-Lichenes*. These parasites, themselves microscopic, have accumulated in the author's Herbarium during the last twelve or fifteen years: or were examined by him while determining the species of various British and foreign Lichen-collections submitted for that purpose during the same period.

The types of the parasites referred to are—

Torula lichenicola.
Coniothecium lichenicolum.
Sphæria ventosaria.
Microthelia Cookei.
Stereocaulicola.
Umbilicariæ.
. Nephromii.
Microthelia rugulosaria.
Stictaria.
Parietinaria.
Bæomycearia.
atricola.
vesicularia.

Detailed observations are included on the variations of *Torula lichenicola* and *Coniothecium lichenicolum.*

The group of parasites described (accompanied with two plates of seventy figures illustrative of their minute structure) is representative of a large and important section of obscure and little understood organisms that are midway in character between Lichens and Fungi, and which have been scarcely studied by either Lichenologists or Fungologists. The observations made upon them by the author illustrate several points of great interest in the physiology, anatomy, morphology, and classification of the lower orders of Lichens and Fungi, *e.g.*—

1. The extent to which Lichens are affected with parasitic growths.
2. The external resemblance between these parasites—whether Lichens, Fungi, or Fungo-Lichens—and the
   Spermogonia,
   Pycnidia,
   Apothecia or Perithecia,
   } of Lichens or Fungi.
3. The connection between parasitic growth and deformities or degenerations of the Lichen-thallus or apothecia.
4. The closeness of the alliance between Lichens and Fungi: the impossibility of drawing lines of demarcation.
5. The fallacies of supposed diagnostic characters or differential tests between Lichens and Fungi: especially the iodine-reaction-test.
6. The usefulness of establishing a group intermediate between Fungi and Lichens—of *Fungo-Lichenes.*

7. The variations of spores or sporidia in the same species.
8. The occurrence of different forms of reproductive corpuscles in the same perithecium.

The author's paper includes also descriptions of two new parasitic *Micro-Lichens*—

Lecidea Endocarpicola, and
Abrothallus Moorei :

and of one non-parasitic *Micro-Fungus*—

Peziza lichenoides.

He contributes also to a fuller knowledge of the minute anatomy of various lichenicolous parasites, whose names and specific characters have already been published, but which are nevertheless as yet little known, *e.g.*—

Sphæria Hookeri.
Spilomium Graphideorum.
Gassicurtia silacea.
Microthelia Collemaria.

6. On the Theory of the Formation of Ammonia when Nitrogen and Aqueous Vapour are passed over Magnesium. By A. R. Catton, M.A.

In a paper recently communicated to the Society by Mr Main and myself, we showed that when nitrogen and aqueous vapour are passed over magnesium ammonia is formed. We took for granted the theory of the reaction to be that the magnesium decomposes the aqueous vapour, and that the hydrogen thus produced, when in the nascent state, combines with nitrogen and forms ammonia. In a discussion which followed the reading of the paper, Mr Dittmar maintained that such was not the theory of the reaction, but that nitride of magnesium is first produced, and then decomposed by the aqueous vapour into ammonia and magnesia.

I gave what appeared to me a conclusive answer to this supposition—which I was certainly surprised to find seriously maintained—for it seemed in every point of view so entirely untenable, that I did not think it worthy of notice in our paper.

As, however, the formation of ammonia in the manner we described is a point of considerable interest, I have thought it right to embody the remarks which I then made in the note now presented to the Society, in order to remove the doubt which I find exists in the minds of some as to the theory of the reaction.

That the ammonia is not due to the action of aqueous vapour on nitride of magnesium is, I think, clearly shown by the following considerations:—

(1.) Nitride of magnesium is a very unstable compound, and is stated to decompose water with such violence that it makes the water boil. *A priori* it is a very strong objection to an explanation which assumes the formation of a body under the very conditions which decompose it. To say that nitride of magnesium could be formed in the presence of aqueous vapour, is pretty much the same thing as to say that potassium could be formed in the presence of water. It is certainly the duty of those who make a supposition so entirely at variance, as I think, with dynamical considerations, to support it by some experimental evidence.

(2.) Nitride of magnesium is only formed when magnesium is heated to a high temperature in presence of nitrogen. But ammonia is formed in small quantity in our experiments when nitrogen and aqueous vapour are passed at *low temperatures* over magnesium. In this case, it is certain that nitride of magnesium is not produced; for it would not be produced if aqueous vapour were not present, and it surely will not be pretended that the presence of aqueous vapour might induce the formation of nitride of magnesium under conditions in which it would not be formed in its absence.

It is equally certain that at low temperatures magnesium decomposes aqueous vapour with formation of hydrogen; and in the union of nascent hydrogen and nitrogen, we have a sufficient, and, as I maintain, the only explanation of the formation of ammonia.

(3.) To suppose that nitride of magnesium could be formed is equivalent to maintaining that magnesium has, in the conditions of our experiments, a greater affinity for nitrogen than for oxygen, which the instability of the nitride and the great stability of the oxide of magnesium shows is not the case. This reason alone is sufficient to negative the supposition of the formation of nitride of magnesium.

(4.) Nitrogen and aqueous vapour passed over *sodium* yielded no ammonia. The reason for this was said to be that a nitride of sodium does not exist. The real reason for the non-formation of ammonia appeared to be due to the violent action of the aqueous vapour on the sodium. If sodium amalgam were used in order to modify the action of the sodium, we believe that ammonia would be produced. It is our intention to try this experiment.

[A discussion followed the reading of this paper, in which Professor Crum Brown, Mr Dewar, and Mr Catton, took part.]

Monday, 3*d May* 1869.

DR CHRISTISON, President, in the Chair.

The following Communications were read:—

1. Influence of the Vagus upon the Vascular System. By William Rutherford, M.D., F.R.S.E., Demonstrator of Practical Physiology in the University of Edinburgh.

Abstract.

The author gave the *chief* results of 120 experiments upon frogs, rabbits, and dogs, as follows:—

1. He showed, by novel methods of experimentation, that the inferior cardiac branches of the vagi are—as the brothers Weber pointed out—inhibitory nerves of the heart, and that they cannot in any sense be regarded as motor nerves of the heart, as maintained by Schiff, Moleschott, Lister, and others. The experiments bearing on this question were performed in 1866–67.

2. Because the heart usually beats more rapidly after division of the vagi, Von Bezold and others have concluded that the vagus is continually restraining the heart's movements. The author showed that *increase of the blood pressure* is generally the cause of the quickened cardiac action after division of the vagi, a fact which had been overlooked by all previous observers. Von Bezold's theory, that the vagi continually inhibit the heart, was therefore shown to be devoid of the necessary proof.

3. It had been noticed by many observers that after division of both vagi the blood-pressure usually rises. This has always been ascribed to the quickened rate of cardiac action; but the author proved that it is usually due to the contraction of abdominal blood-vessels, chiefly those of the stomach. Division of the vagi is followed by rise in the blood pressure—in general—only when the animal is digesting its food; that is, when the gastric blood-vessels are dilated previous to the division of the nerves.

4. The vessels of the stomach appear to be dilated during digestion, chiefly by the vaso-inhibitory action of incident filaments of the vagi upon the splanchnic nerves (the vaso-motor nerves for the gastric vessels).

5. By the discovery of the above fact additional support is given to the theory that the contractile elements of the entire vascular system are presided over by two kinds of nerves—one motor, the other inhibitory. The former brings about contraction of the vascular system, the latter causes its dilatation by throwing the vaso-motor nerves into a state of rest.

The experiments bearing on the questions embraced in 2, 3, 4, and 5, were chiefly performed in 1868.

2. On Free An-atomic or Transmissible Power. By R. S. Wyld, Esq. (*Second Paper.*)

Observations on Animal Power, its Nature and Source. And on Sense-perception, as a Perception of Power.

In the first part of the paper I adduce probable, or *prima facie*, evidence in favour of the theory that the power which produces voluntary motion is not derived solely, nor even chiefly, from the decomposition of the muscular tissues, as has been generally, if not universally, held by recent investigators, but is derived primarily and principally from the decomposition of the cerebral tissues; and second, that this decomposition of the brain is effected by a direct voluntary mental effort.

My views may be briefly stated in the following propositions:—

1*st*, Free force is liberated by the dissolution and decomposition of simple and compound bodies, and especially by the decomposition of vegetable and animal organic tissues.

2*d*, An immense amount of free force is being constantly liberated by the process of decomposition going on in the body of the living animal, and it is thus that heat, one of the prime agents in the animal economy, is supplied to all parts of the body.

3*d*, Free force is also in this way, viz., by the decomposition of the substance of the brain, supplied to the muscles of voluntary motion, and it is the agent which causes their contractions, for the purposes of animal movement.

4*th*, This decomposition is effected by a direct mental effort, which we are conscious of exerting during voluntary muscular action.

5*th*, Though there is a notable difference between the tension of brain force and what is called galvanic force, as is marked by its ascertained inferior velocity—about 95 feet in the second—and by its having a feebler and more easily interrupted current, yet brain or nervous force seems substantially the same in its nature, source, and action, as galvanic force.

The grounds for holding that the free force which produces muscular action is derived, at least chiefly, from the brain, and not from the muscles, whose main function is to be the mechanical instruments of motion, are the following:—

1*st ground.* The brain is the centre of the nervous system. It is now, also, by the experiments of Helmholtz, Harles, Fick, Schelske, and others, ascertained to be the sole seat of sensation. Being thus the direct and only organ of the mind, it is natural, if not necessary, to conclude it to be the immediate source of animal power during voluntary muscular movement.

2*d ground.* We have a ***direct experience*** that the ***amount of physical power obtained is***, cæteris paribus, ***in constant proportion to the amount of the mental effort which we are conscious of exerting in producing this physical power.***

3*d ground.* The brain gets, according to reliable computation, probably five times the average supply of blood which is furnished to the other parts of the body, weight for weight. This circumstance requires to be accounted for, and physiological principles mark it as a proof that the brain is an organ discharging very heavy and important work, by the decomposition and reconstruction of its substance, and by the consequent liberation of free transmissible force.

4th ground. The circumstance that the complex albuminous and oleaginous constitution of the cerebral substance yields readily to decomposition, and that it is the part whose chemical combinations are the first to break up after death, serves further to strengthen the views here set forth.

The subject is, doubtless, one very difficult to investigate; for even if, in the face of much contradictory evidence, we accept the opinion that the amount of *Urea* and other nitrogenous products discharged from the body affords a correct measure of the amount of animal work performed in a given time, yet from the circumstance that both the brain and the muscles are chiefly nitrogenous in their constitution, it must ever remain difficult, if not impossible, by any mere examination of the nitrogenous excretions, to ascertain whether they are derived from the one organ or from the other, and therefore, the most ingenious and careful investigations will be required before we are entitled to dispossess the brain of what appears, on pretty strong grounds, to be one of its most important functions, viz., that of being the organ to the mind for the production of animal motion and power.

On Perception.

The second part of this paper has reference to the subject of sense-perception, or the impressions which the mind obtains by reason of its connection with the physical world.

The writer directs attention to the fact that an examination of the laws of momentum, and of the transmission of the rapid concussions and vibrations which pass through the atmospheric and ethereal media, makes it apparent that *free force* or power is transmitted from molecule to molecule of these media, and that, philosophically speaking, it is this *free force* which thus enters the brain through the appropriate nerves, and which impresses the mind with the sensations of light and sound.

The difficulty which has hitherto been felt by philosophers, not only of the various continental schools, but also of our Scottish school of philosophy, has ever been—how matter or physical substance could act on mind? This difficulty prompted Malebranche to promulgate the theory of our perceiving all things in God. It also prompted Leibnitz to maintain that there was no operative connection between the mind and external physical nature, but merely a pre-established harmony; and much the same difficulty led Berkeley to deny the existence of an external world, and to declare that our perceptions were not real, but only apparent, being mere ideas evoked in the mind by Deity.

Reid and Stewart also represented perception in the light of an inexplicable miracle, on which the study of the physical laws not only threw no light, but rather served to mislead and pervert the inquirer. The Deity they held to be the only efficient cause in perception.

Mr Wyld holds that the difficulty thus experienced by philosophers disappears the moment we carefully examine the physical phenomena. Thus in the sensations of light and sound, it is not matter or substance which affects us, but it is the force or power transmitted by the respective *media*, which affects the mind. Force or power Mr Wyld defines to be the action of an unseen immaterial agent, as proved in his former paper on "Free Transmissible Power." And wherever *movement* is observed to exist, we have evidence of the existence of *force* as its cause.

When we explore, says Mr Wyld, the links of cause and effect in the physical world, and also when we contemplate the operations of the mind, we are made aware that the seen and the unseen, the physical and the mental, rest alike on the spiritual foundation of power. Power is the immaterial or spiritual element in nature—that which produces and governs every physical phenomena; it is the copula uniting cause and effect. It is also the attribute of the active principle within us, by which we feel, and think, and move, and exercise control over external nature.

Reduced, then, to a philosophical form, we stand in this position: It is the power of the Supreme Being which constitutes the energies and forces of nature. These forces act on the mind, and the mind, as a real agent, acts on them. Our intercourse with nature thus strictly implies a connection of mind with mind,—the connection of the Great Mind with his creatures; not, however, as maintained by Malebranche, Berkeley, Leibnitz, or Reid, by miraculous interposition or action, but, on the contrary, expressly by and through the operation of the natural laws we have endeavoured to explain in the previous paper—Deity being the sustainer of these laws and of the active energies of nature.

We know that the mind of man deals with and directs free power or force, when it sends it to the muscles for the purposes of voluntary motion; and we are therefore justified by parity of reason in holding that, when free force comes in upon the mind from the outer world, through the vibratory movements above referred to, the mind will be susceptible to its influence. This is not only a consistent, but a necessary conclusion.

It is, be it observed, not substance, but power, which the vibrating molecules bring to the mind, and which the mind receives. We may hold also that the force or power thus received or appropriated by the mind, and the physical movements which accompany it, take end in the brain, being there neutralised by the control which we know the mind to exert over physical power.

It is common, we admit, for physicists to explain the perception of light and sound as due to the vibratory *movements* of the physical molecules which impinge on the retina and auditory nerve. But in doing so they give us merely the outer manifestation, and neglect the unseen essence and efficacy. For it is the force which causes the movement, and not the movement which causes the force. It is force which causes the cannon ball to fly with its destructive violence. Take away the force, and the iron lies harmless on the ground.

Without the iron, indeed, or other atomic body, we could neither obtain nor use physical force for physical purposes, and for these reasons—first, though we can take force from one body and pass it to another, we can never obtain it freed from its physical connection. The second reason that physical substance is necessary for physical purposes is this—Though the iron is passive, and the sport of its Master—Force, and though it is force that carries it through the air, still the iron evidently performs an important part in physical dynamics. It is because iron and other atomic bodies are impenetrable by each other, that when the cannon ball meets the iron target, one or other of them must yield. The question is not, which of them is the hardest—for a piece of tallow, we know, may be fired through a deal board—the question is—whether the cohesive power of the target or the onward force of the ball is greater. If the latter preponderates, then the force necessarily carries the ball with it through the iron plate, with violent disruption of its parts.

On the same principle, it is not the movement of the molecules which, hitting the retina and auditory nerve, affects the mind with the sensations of light and sound, it is the force which they carry which accomplishes this.

Though force is the operative principle in all physical and mental action, yet mark the result were force not regulated, and bound up in connection with physical bodies. Were force freed from this connection, it would attain immediate equilibrium, and the physical universe would instantly cease to exist—action and reaction, momentum and inertia, resistance and localised force being at an end, physical law and the physical world would be at end with them—for the physical world consists but of the antagonism of contending forces.

Mr Wyld concluded by referring to the distinctive nature of our perception of the primary or solid resisting properties of physical objects, and of our other perceptions. In our sensations of light and sound, taste, smell, and touch, the mind is passive. But our perception of the primary properties of external nature depends entirely on another principle, namely, on our consciousness that the mind possesses, and exerts a command over active physical power, or the power of animal motion. When we are conscious of possessing this power, and perceive that the Will which exerts and directs it, is thwarted by our coming into visible contact with outer objects, we discover at once that these objects possess resisting power, seeing that they counteract at once our power, and our Will, and our motion.

This distinction between passive sensations and the exercise of our active mental and animal powers has not been sufficiently urged in refutation of Berkeley's Idealism. The vast majority of realists in philosophy succumb under the apparent force of the absurd argument, that all our perceptions are resolvable into mental sensations, and therefore that our belief of an outer world hangs on faith alone. Mr Wyld maintains, on the contrary, that according to the principles of his theory we have a ready and complete confutation of Berkeleyism. For the mind not only possesses a command of physical power, but it is *conscious* that it possesses it, and that it exerts it, and that its exertions are resisted by external objects; and thus, through a direct consciousness of mental power, we have an indirect, but indefeasible proof and perception of the resistance of external objects, and of the external world.

3. On the Affinities and Classification of the Nemerteans. By W. C. M'Intosh, M.D., &c.

The immediate allies of the Nemerteans are evidently the Planarians, and Ehrenberg's class *Turbellaria*, as now amended, is therefore a very natural one. There is, however, a considerable divergence between the two groups; and since it is only through the Planarians that the Nemerteans can in any way be linked on to the Trematoda, it is found that there is little structural resemblance between the two last-mentioned types. The various parts of the organism in the Trematodes and Planarians (through which the connection of the Nemerteans with the former may be supposed to occur) were contrasted, and their homologies explained. The structural relations of the Planarians and Nemerteans were then considered; and, thirdly, the connections of the latter with the higher Annelids were alluded to.

In regard to classification, the arrangements of Linnæus, Ehrenberg, Œrsted, De Quatrefages, Max Schultze, Schmarda, Keferstein, and others were examined, and the reasons why they could not now be adopted, at least without essential modifications, explained. The following classification is considered less liable to alterations in principle, since it is founded on an anatomical basis.

Order *Nemertinea.*—Worms, with more or less elongated, soft, ciliated bodies; nervous system composed of two conspicuous ganglia connected by a double commissure, and two main lateral cords; digestive system, in the form of a ciliated canal, with two apertures; circulatory system, consisting of a series of closed contractile vessels. The proboscis forms the typical organ of the order, and is surrounded by a special muscular sheath, within which it glides in a corpusculated fluid; in front it passes between the commissures of the ganglia, while the digestive tract is placed inferiorly. Sexes separate in the majority; oviparous or ovoviviparous.

The Order may most naturally be separated into two great *Sub-Orders*, distinguished from each other by the presence or absence of stylets in the proboscis, the former being called after Max Schultze, (but with amended characters), *Enopla*, and the latter *Anopla*.

The Sub-Order *Enopla* is characterised further by the rounded and somewhat double nature of the nerve-ganglion on each side, and by the fact that the lateral nerve-cords are placed within the proper muscular walls of the body. The mouth, moreover, opens on the ventral surface of the snout in front of the commissures of the ganglia. The blood-vessels are more differentiated than in the *Anopla*. The young, so far as known, do not undergo any noteworthy metamorphosis.

In the *Enopla* there exists one great group, and a subordinate one, which, however, retains so many of the characters of the former, that it has not been considered advisable to separate it into a distinct Family, but both have been placed under the Family *Ommatopleidæ*. The animals forming this Family have two muscular layers in the body-wall, an external circular and an internal longitudinal; the proboscis is composed of three divisions, anterior, middle, and posterior,—the former having in the typical forms seven coats, viz., external elastic, external longitudinal (muscular), reticulated or beaded layer, inner longitudinal, circular, basement layer, glandular layer. The middle region bears the stylets, and the posterior forms a long sac, with two muscular coats—external circular and internal longitudinal. There are three great longitudinal vascular trunks, two lateral and one median. The cephalic pits and glands are accompanied by long tubes or ducts. The subdivision of the Family referred to may be made by separating those (A) with comparatively short and thick bodies, and proportionally large proboscides; and (B) those having more or less elongated bodies, and proportionally short proboscides.

The Sub-Order *Anopla*, again, is further distinguished by having the nerve-cords placed between the muscular layers of the body-wall. The mouth opens on the ventral surface behind the commissures of the ganglia. The blood-vessels are somewhat less differentiated than in the Enopla. The young in the most conspicuous Families undergo a remarkable metamorphosis.

In this Sub-Order there are several Families, the most conspicuous and typical of which is that of the Borlasidæ, characterised by the more or less elongated shape of the ganglia, the arrangement having with the commissure the form of a horse-shoe. The muscular covering of the body is composed of three layers—external longitudinal, circular, and internal longitudinal. The proboscis is furnished with five coats, viz., external elastic, external longitudinal and accessory bands, circular, basement, and glandular layers. The circulatory system consists of three great longitudinal trunks, two lateral and a dorsal, which frequently anastomose by transverse branches, form a *rete mirabile* in the œsophageal region, and unite in lacunæ behind the ganglia. Head, with deep lateral fissures in connection with the cephalic gland, which is rounded, and not furnished with long tubes or ducts.

A curious specimen from Herm forms the type of a group that would require to be raised to the rank of a Sub-Family, but as only one specimen has yet been found, it is thought advisable to postpone this at present, and distinguish it only generically. In this animal the proboscis is extremely slender in proportion to the bulk of the body, and in its minute structure differs from the type of the Borlasidæ in having no accessory bands cut off from the longitudinal layer. Externally the organ has an elastic investing layer, then a longitudinal, within which are a thin circular and a glandular coat. The reddish colour of the muscles of this species, and the tinted circulation, are likewise quite characteristic.

A more distinct Sub-Family of the Borlasidæ than the foregoing, perhaps, is that of the Borlasian without lateral slits. The anatomy of the body-wall agrees with Lineus, but there are no cephalic fissures. The structure of the proboscis is also characteristic, for there is externally no distinct investing layer, the outer coat consisting of spiral and interwoven muscular fibres, the middle of longitudinal, and the internal of the usual glandular coat.

The *Meckelidæ* constitute a very distinct Family of this Sub-Order. The general structure of their nervous system agrees with Borlasia, but the lateral nerve-cords are placed between the circular (external) and the longitudinal (internal), these being, moreover, the only two muscular coats of the body. There are no lateral fissures. The circulatory system consists of two great lateral trunks. The proboscis has externally a double elastic layer, a thick longitudinal coat, and internally a glandular layer.

The Family of the *Cephalothricidæ* deviates still more from the typical group. The arrangement of the ganglia differs, and the commissures are separated by a considerable antero-posterior interval. The lateral nerve-cords lie between the longitudinal layer and an isolated inner band of fibres having the same direction. The proboscis is supplied with acicular papillæ, and seems to have an external circular and an internal longitudinal coat. Snout devoid of fissures. Circulatory system composed of two great longitudinal trunks, which communicate anteriorly and posteriorly. Oviparous: the young undergoing no true metamorphosis, although they possess eyes, whereas the adults are generally eyeless.

4. On the Alkaloids contained in the Wood of the Bebeeru or Greenheart Tree (*Nectandra Rodiœi*, Schomb.) By Douglas Maclagan, M.D., F.R.S.E., Professor of Medical Jurisprudence in the University of Edinburgh, and Arthur Gamgee, M.D., F.R.S.E.

When the wood of the bebeeru tree is subjected to a process similar to that recommended in the British Pharmacopœia for the separation of bebeerine from the bark, a mixture of several alkaloids is obtained.

In this memoir the authors specially describe the properties of one of these alkaloids, to which they have assigned the name Nectandria and the formula $C_{20}H_{23}NO_4$. (Bebeerine, according to V. Planta, has the formula $C_{18}H_{21}NO_3$). Nectandria is obtained by treating the mixed alkaloids obtained from the wood with chloroform, in which it is abundantly soluble.

From the chloroform solution it is obtained in the form of a fawn-coloured substance, which is quite amorphous. It differs from Bebeerine in being almost absolutely insoluble in ether, and in fusing when placed in boiling water. By treating the solution of the hydrochlorate of this base with animal charcoal, and then precipitating the solution with ammonia, the authors succeeded in obtaining the base of an almost pure white colour. When dissolved in acid this powder, however, yielded yellow solutions.

When Nectandria is treated with sulphuric acid and binoxide of manganese a magnificent green colour is developed, which slowly changes to a violet, which cannot be distinguished from that produced under similar circumstances with strychnia. This reaction, which is of exceeding delicacy, is not possessed by bebeerine. Nectandria has no effect in rotating the ray of polarised light.

After separating nectandria by means of chloroform, the authors treated the mixed bases with boiling water, which readily acquired a rich yellow colour, and an intensely alkaline reaction. On cooling, the water deposited a yellow precipitate, which was found to consist of microscopic nodules. The substance dissolved by the water is a very powerful base; its compound with platinum is noular, and differs from the double salt of platinum and nectandria in being fusible in hot water. This new base is insoluble in chloroform, decidedly soluble in water (one part is soluble in 56·8 parts of cold water) and insoluble in ether.

When treated with binoxide of manganese and sulphuric acid it gives exactly the same reaction as nectandria.

The taste of this base is peculiar, being both bitter and astringent.

The platinum compound of the new base was analysed, and found to contain (1) 20·1 per cent. of platinum and (2) 20·57 per cent of platinum.

After separating the two bases above referred to a brown residue remains, which is insoluble in chloroform, ether, and water, but soluble in alcohol. The alcoholic solution possesses an alkaline reaction. This substance forms compounds with acids, and yields an amorphous compound with platinum.

The authors propose to examine in the sequel the physiological and therapeutical properties of the true bases treated of in this paper, as well as to investigate in a special manner the chemical relations which exist between them.

Monday, 17*th May* 1869.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Basin of the Firth of Forth, and some of its Geological Phenomena. By David Milne-Home, Esq.

I. An account was given of the dimensions of the estuary, in respect of length, width, and depth; and also of the general direction of the axis of the estuary.

II. It was next shown that the formation of the estuary was probably due to the great geological fractures and dislocations which had taken place in this part of Scotland, inasmuch as the direction of these was parallel, or nearly so, to the axis of the estuary, and as the downcasts in the adjoining counties on the north and the south were all towards the estuary, thus producing a trough or valley, deep enough to be entered and filled by the sea.

III. The author, before describing the newer deposits filling the basin of the Frith of Forth and covering its adjoining shores, stated that the materials of these deposits were most probably derived from the upcast strata which, along the slips and fractures, had formed high exposed cliffs, and which yielded to the influence of the sea, if the sea covered them, or atmospheric changes, if they were above the sea-level.

1. There were no data for determining *when* these dislocations took place. There was clear evidence, however, that in the Western Highlands there had been outbursts of trap at a period later than the chalk; and Mr Cumming thought, that in the Isle of Man part of the boulder-clay had been formed after great dislocations there.

2. An account was then given of the various kinds of deposits, chiefly by means of reports obtained of borings, which showed the layers of clay, sand, and gravel passed through, with the thickness of each.

3. It was observed that clay generally occupied the lowest levels, sand and gravel the higher levels, a reason for which was assigned. The bed called "*sleech*" or "*sludge*" was described, as also the "*brown*" or "*carse clay;*" and the question was raised, whether the *peat* beds, which also appeared among the deposits, were *in situ*, or whether they were formed of drifted materials. The sub-marine forest on the Fife coast was noticed.

Some account was given of the *sea-shells* found in the districts, it being explained that they occurred sometimes in positions where they were entire, and also in other places where they were broken. It was supposed that in the former case the shells were now where they lived and died; in the latter case, where they had been thrown up on beaches.

An enumeration of the *Whale skeletons* discovered was next given.

4. Some details were entered into regarding beds of *sand* and *gravel*, and the pebbles found in both, particularly the fragments of coal, as indicating the quarter from which the transporting agent had moved. The subject of *kaims* was adverted to, which the author ascribed to the action of sea-tides and currents.

Some ancient *deltas* along the foot of the Ochil Hills were noticed and shown in a map.

5. *Boulders* of a large size were pointed out, with a notice of striæ on several, and of the direction of the striæ.

These boulders were described as belonging to two distinct classes, the one being rounded, the other angular, and each being in such different circumstances as to suggest a different mode of transport for each class. All the boulders in this district, however, appeared, from the nature of the stones composing them, to have come from the west or north-west, in this respect agreeing with the direction of the striæ on the boulders and rocks, and with the direction, in most instances, of the longer axis of the boulders.

6. The next topic adverted to was the lines of rocky cliffs and caves, at a height of from 15 to 30 feet above the present sea-level, which proved that the sea had stood that much higher formerly.

The author then proceeded to show that almost all the hills in the district presented smoothed or bared fronts to the west, and that many of them were worn down and grooved in such a way as to show that some powerful and heavy agent had passed over them

These markings were particularly visible in the gorge of the estuary at Stirling, and up to a level of above 350 feet.

7. Attention was next called to the boulder-clay of the district, whose irregularity, both of distribution and of thickness, was pointed out.

The fact of marine beds found above, below, and in the heart of the boulder-clay was noticed, as also the force with which it appeared to have been pushed by some agent or agents in an easterly direction.

The colour of the boulder-clay, varying as it does in different districts, seemed to be owing to the colour of the rocks situated in each case to the westward, and it was observed that beds of stratified or laminated clay sometimes showed different colours, concurrently with the boulder-clay.

The author next adverted to the two theories proposed to explain the origin of the boulder clay, the one being that it was mud formed by a glacier grinding the rocks, over which the glacier flowed, and the other that it was formed by icebergs ploughing through and mixing up a previously existing sea-bottom composed of mud, sand, clay, gravel, and boulders. He stated that he adopted the second view, and specified some of the difficulties of the question, all of which he thought could be more completely solved by the iceberg than by the glacier hypothesis.

8. The author then adverted to the subject of ancient sea-margins, four or five of which he specified, mentioning the names of places where they had been observed by himself or others.

The lowest of these sea-margins he had drawn on a map, which he exhibited, and the height of this margin he stated was, at the head of the old estuary (west of Stirling), about 30 feet above the same old sea-margin at Dunbar. This rise he attributed to tidal action exclusively, pointing out that even now, when the estuary is so much shallower, the sea-level at Alloa is 6½ feet higher than at Dunbar, and that there are other estuaries, in this country and others, where now the sea-level is at their head more than 30 feet above the level at their mouth.

9. The author said that the change in the relative levels of sea and land, which these ancient sea-margins indicated, might have been brought about either by the land rising or by the sea sinking;

and he was inclined to think the last process, however it was to be accounted for, seemed more in accordance with the facts, and, in particular, with the horizontality of these sea-margins, which, being over an extensive area, could scarcely have resulted from volcanic elevation of the land.

10. The author adverted to an opinion by Mr Geikie, and countenanced by Sir Charles Lyell, that the last change in the relative levels of sea and land took place in the Firth of Forth since the time of the Romans, which opinion he thought was disproved by the number of military works of the Romans in the district about Stirling, which could never have been made had the sea then stood 20 or 30 feet higher than at present.

At the same time, he thought there was evidence to show that human beings occupied this part of Scotland when the sea had stood at that higher level, and that as there were also grounds for believing that the climate of Scotland had then an arctic character, the subterranean dwellings, called "weems" and "Picts' houses," found in many parts of Scotland, might very probably be referred to the epoch which preceded the change of sea-level.

2. On Comets. By Professor Tait.

(Abstract.)

The principal object of the paper is to investigate how far the singular phenomena exhibited by the tails of comets, and by the envelopes of their nuclei, the shrinking of their nuclei as they approach the sun, and *vice versâ*, as well as the diminution of period presented by some of them, can be explained on the probable supposition that a comet is a mere cloud of small masses, such as stones and fragments of meteoric iron, shining by reflected light alone, except where these masses impinge on one another, or on other matter circulating round the sun, and thus produce luminous gases, along with considerable modifications of their relative motion. Thus the gaseous spectrum of the nucleus is assigned to the same impacts which throw out from the ranks those masses which form the tail.

Some of the most wonderful of the singular phenomena presented by comets, such as the almost *sudden* development of tails

of many millions of miles in length, the occurrence of comets with many tails, and the observed fact that there is no definite relation of direction between a comet's tail and its solar radius-vector, are here looked upon as due to the differences of motion of these discrete fragments relatively to the earth, in a manner somewhat analagous to the appearances presented by a distant flock of sea-birds flying in nearly one plane, and only becoming visible (as a long streak) when the plane of the flock passes approximately through the spectator's eye. The so-called envelopes are compared with the curious phenomena presented by tobacco-smoke (which seems, however, to be emitted in a form apparently resembling thin *continuous* films of small particles of carbon), and the so-called "gaseous jets," which appear to be projected from the nucleus and to be *repelled* from the sun, are not difficult of explanation from the general point of view here taken.

The investigations are mainly conducted by quaternions, and show how a group of discrete masses, so small that their mutual perturbations are not of great moment, except in the case of actual impact, gradually changes its form as it revolves about the sun; independently of any hypothesis as to the cause (planetary attraction or otherwise) by which it was first introduced into the solar system.

The author had some hesitation in bringing forward this paper, as he did not know how far Schiaparelli, whose recent discoveries have been so very novel and remarkable, had carried his investigations. The ideas here brought forward had occurred to him immediately on his being made aware (more than two years ago) of the identity of the orbits of the August meteorites and of Comet II., 1862: but they seemed so obviously to follow from that identity that it was only on reading Dr Tyndall's recent speculations, which seem to have been well received; and on being informed by Professor Newton (of Yale Coll. U.S.A.) that the questions of tails, envelopes, and "gaseous jets" had been treated by Schiaparelli as proving the existence of a repulsive force (such as seems to have been admitted by Bessel and others, who observed Halley's comet on the occasion of its last return), that he ventured to produce to the Society an explanation so apparently simple, yet so inconsistent with what appears to be held by the majority of astronomers. The mathematical part of the necessary investigations could hardly have been made so simple and yet comprehensive as it now is had quaternions not been employed.

3. Hegel and the Metaphysics of the Fluxional Calculus. By W. R. Smith, Esq. Communicated by Professor Tait.

(*Abstract.*)

The object of this paper is to consider the mathematical value of Hegel's discussion of the fluxional calculus. This discussion, as contained in several notes in the "Logik," professes to evolve the true principle of the calculus in a form free from the inconsistencies of the usual processes, and to show how this principle may be rendered useful in the solution of problems. To clear the way for this new theory, Hegel engages in a sharp polemic against the expositions of several parts of the calculus given by Newton and others.

As these strictures on Newton have, at least in part, been received with great satisfaction by metaphysicians, the paper seeks first to show that the notion on which Newton based his doctrine of fluxions, viz., the generation of magnitudes by continuous motion at definite (but not necessarily constant) velocity, really has a place in nature. If we remember that Newton always views fluxions as a method belonging to physics (kinematics), we shall find the whole developments of his calculus to be thoroughly consistent and simple. It is shown, however, that Hegel, while he professes to approve of Newton's fundamental notions, has so completely misunderstood the whole method as to propose to eliminate as inessential the conceptions of movement and velocity, and then to blame Newton for attempting to use the calculus to do work for which it is fitted just because it is based on the notion of continuous movement. Not only does it appear that Hegel has failed to appreciate Newton's general principle, he seems also to have quite failed to understand what mathematicians mean by a limit. His strictures on the doctrine of limits are in no sense directed against any tenet of mathematicians, but only against certain absurdities which he supposes mathematicians to hold. This especially appears in his inability to understand what is meant by the evaluation of a

function which takes the form $\frac{0}{0}$; but the most interesting point in the details of this part of Hegel's work is his criticism of Princip. ii. lem. 2. Hegel accuses Newton of an error in elementary algebra, but in reality Newton's work is strictly accurate, and Hegel has merely failed to see the point of the problem. Hegel's own views of the calculus were got from Lagrange, whose treatise, with all its analytical skill, is yet a fruit of the very *Aufklärung* which in other matters Hegel especially opposes. Mistaking the abstract formalism of Lagrange's intrinsically erroneous and now quite abandoned method for superior generality, Hegel still thinks it possible to reject certain incumbrances that cling to Lagrange, and which are in fact inevitable concessions to the *physical* view of the calculus, without which the method could have no value.

Absolutely identifying analytical with algebraical method, and thus freed from all the difficulties about continuity which occur in the ordinary processes of algebra, Hegel, of course, cannot appreciate these parts of Lagrange's work. He gives a simplified theory, and applies it, among other things, to the problem of drawing tangents. Hegel professes to deduce the correct rule, but he does so only by adopting an utterly false definition of a tangent, which, in fact, gives an infinite number of tangents at every point of a curve. In short, in this and other cases, Hegel makes errors of a mathematical character sufficient to show that his knowledge of the calculus was absolutely worthless.

4. On the Connection between Chemical Constitution and Physiological Action:—On the Physiological Action of the Salts of Ammonia, of Tri-methylamine, and of Tetra-methyl-ammonium; of the Salts of Tropia, and of the Ammonium Bases derived from it; and of Tropic, Atropic, and Isatropic Acids and their Salts. With further details on the Physiological Action of the Salts of Methyl-Strychnium and of Ethyl-Strychnium. By Professor A. Crum Brown and Dr Thomas R. Fraser.

(*Abstract.*)

In papers which the authors have already communicated to this Society, they have shown that there exists a very marked difference

between the action of bases containing nitrogen as a triad and those in which that element is pentad. This result was obtained from an examination of the physiological action of substances having an unknown and complex constitution. It appeared to be necessary to institute a series of experiments with bodies having a simpler and fully known constitution. For this purpose, the salts of ammonia, tri-methylamine, and tetra-methyl-ammonium were selected. A comparison of the structural formulæ of the hydrochlorates of these bases shows that tri-methylamine stands in the same relation to the salts of tetra-methyl-ammonium as strychnia to the salts of methyl-strychnium.

The iodide of tetra-methyl-ammonium was prepared by mixing iodide of methyl with an excess of ammonia dissolved in alcohol, and recrystallising the crystalline precipitate from hot water. From the iodide the hydrate was obtained by the action of oxide of silver; and by distilling the hydrate and conducting the vapours into hydrochloric acid, the hydrochlorate of tri-methylamine was obtained.

In their general physiological effects these substances very closely resemble each other. The most obvious symptoms which they produce are paralysis and slight muscular spasm; and when these symptoms were carefully analysed, by restricting the poisonous action to certain defined regions in frogs, it was ascertained that they are caused mainly by a direct action on the cerebro-spinal nervous system and on striated muscles. When large doses are given, the functional activity of these structures is first impaired and then destroyed; but during the stage of impairment, a slight degree of spasmodic action is produced, which may probably be referred, in frogs, to the stage of irritation which precedes the final action of the majority of muscular poisons, and, in mammals, to this action, aided by changes in the vascular condition of the central nerve organs, due to an influence on non-striated muscles.

The physiological effects of chloride of ammonium and of hydrochlorate of tri-methylamine were found to be extremely similar, both in degree and in kind, and to differ, in several important respects, from those of iodide of tetra-methyl-ammonium. The two former substances are comparatively feeble in their action; the latter is a poison of considerable energy. Chloride of ammonium and hydrochlorate of tri-methylamine develop their full effects sluggishly; but the different structures which they influence have their activity destroyed nearly simultaneously, so that it is a matter of considerable difficulty to ascertain whether the muscles are paralysed before the motor nerves, or the latter before the sensory, or whether the nerve-trunks or periphery are first affected. Iodide of tetra-methyl-ammonium, however, very rapidly destroys the conductivity of the motor nerves by an action on their peripheral terminations, and an interval of several hours may elapse before its other effects are fully developed.

While the change of physiological action, produced by the addition of iodide of methyl to tri-methylamine, differs in some respects from that produced by the performance of the same operation upon strychnia, the observations now communicated tend to confirm the conclusion drawn from the previous experiments of the authors, that paralysis of the peripheral terminations of motor nerves is a characteristic effect of the salts of the ammonium bases.

The decomposition of atropia by means of acids and bases has lately been completely studied by Kraut* and by Lossen.† It appeared of interest to examine the physiological actions of the products of this decomposition, and to compare them with that of atropia. The reaction may be expressed by the equation—

$$\underset{\text{Atropia.}}{C_{17}H_{23}NO_3} + \underset{\text{Water.}}{H_2O} = \underset{\text{Tropic acid.}}{C_9H_{10}O_3} + \underset{\text{Tropia.}}{C_8H_{15}NO}$$

The tropic acid is further changed by loss of water into two isomeric acids, atropic and isatropic ($C_9H_8O_2$), the former being produced most abundantly in the presence of alkalies, and the latter in the presence of acids. These substances were prepared by the methods given by Kraut and by Lossen; and, on account of the readiness with which the acids pass by oxidisation into formic and *a* toluic acids, the action of the latter acid, prepared by the method which one of the authors communicated to this Society some years ago,‡ was also examined.

Each of these substances has been examined by the authors, and it was found that none of them possesses the well-known dilating

* Annalen der Chem. u. Pharm., cxxviii. 280; cxxxiii. 87; cxlviii. 238.
† Ibid. cxxxi. 43; cxxxviii. 230.
‡ Proceed. Roy. Soc. Edin. v. pp. 409, 455.

action of atropia on the pupil. The experiments were made with the hydrochlorate of tropia and with soda salts of each of the acids. Although, however, tropia differs so strikingly from atropia in being quite unable to influence the pupil, it resembles it in some of its other physiological effects. Like atropia, it is a powerful paralysing agent, and it produces paralysis in very much the same way as atropia does. In virtue of this action, tropia is an active poison.

Apart from the immediate object of these researches, some interest is attached to this portion of the investigation on account of its bearing on practical therapeutics. It has been shown by Professor Garrod, that when small quantities of caustic potash or soda are added to solutions of hyoscyamus, stramonium, belladonna, or atropia, the activity of these substances appears to be destroyed.* More recently, Dr John Harley, of London, has pointed out that the same effect is produced by caustic lime and by ammonia.† This conclusion was arrived at, principally, by observing that the pupil was not affected by preparations to which they had previously added one or other of these alkalies. The decomposition effected by potash, soda, lime, and ammonia, is the same as that which the authors have described; and by an examination of the separate products of this decomposition they are enabled to confirm the observation of Drs Garrod and Harley, and to add to it the additional fact that the products of this decomposition are not altogether inert.

The last portion of this paper, to which the authors think it advisable to draw attention, is that in which they describe some experiments with the salts of methyl- and of ethyl-strychnium, which were performed subsequently to their first communication. These additional experiments were made with the view to establish beyond the possibility of doubt, the truth of their statement, that these substances act as simple paralysers of motor nerves. In a paper recently communicated to the French Academy of Sciences, two able observers, Messrs Jolyet and Cahours, have confirmed the result that these substances possess an action analogous to that of wourali (curara); but they have, besides, observed symptoms which

* Medico-Chirurgical Transactions, vol. xli. 1858, p. 53.

† The Old Vegetable Neurotics, 1869, p. 211.

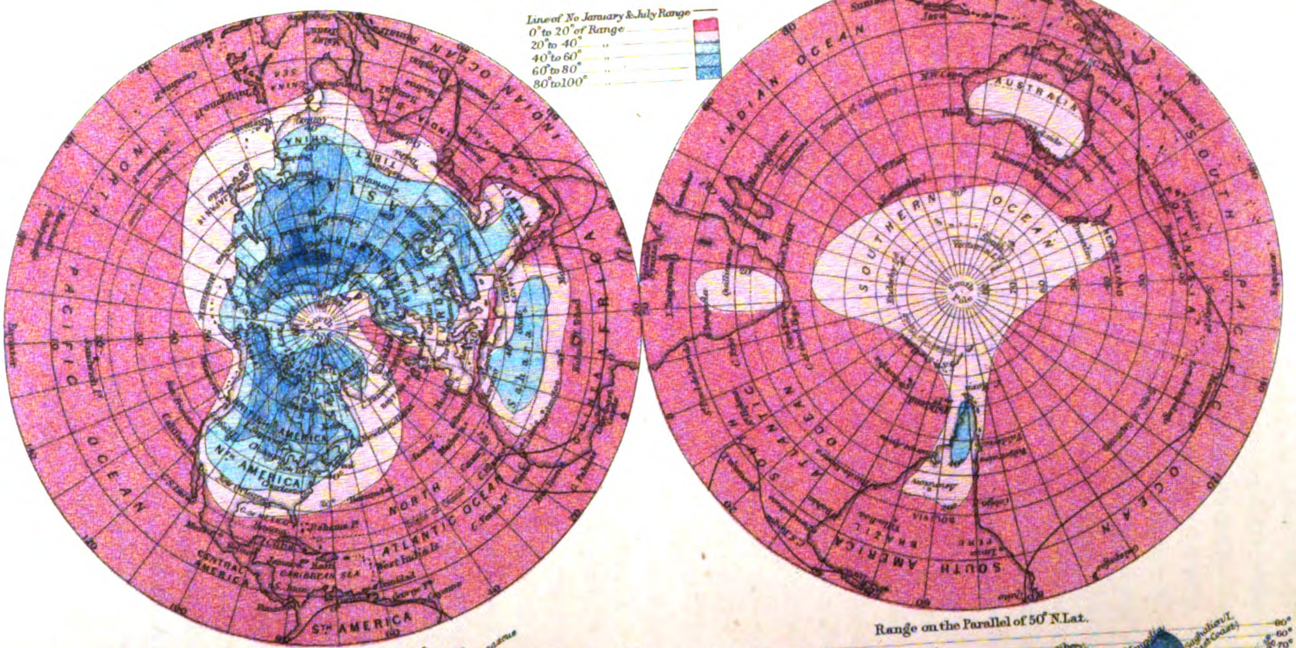
induce them to believe that this analogy is not a very perfect one, as the methyl- and ethyl-derivatives of strychnia seem to retain a certain degree of the characteristic convulsant action of strychnia.* They found their opinion on the appearance of tetanic spasms in the limb of a frog, whose vessels had been ligatured before the poisoning, and on the production of coexisting paralysis and convulsions in mammals poisoned by these substances. Both of these effects are obviously explainable by the presence of a minute trace of strychnia. In the case of the frog with the vessels of one limb tied, the methyl- or ethyl-strychnium salt paralysed all the motor nerves to which it had access; but as strychnia was also administered, *by accident*, the excitability of the spinal cord was exaggerated, and tetanic spasms therefore occurred in the non-poisoned limb—its motor and sensory nerves being protected from the paralysing action of the methyl- or ethyl-derivative. The appearance of strychnic effects in mammals may likewise be explained by the presence of strychnia. One of the authors has shown, in a paper communicated to this Society, that when a sufficient dose of a substance that paralyses the terminations of motor nerves is administered to a frog along with a certain proportion of one that stimulates the spinal cord, the symptoms are those of paralysis alone; but when this combination is administered, in the same relative proportions, to a mammal, the symptoms are those of paralysis coexisting with convulsions.† This result is sufficient to account for the different symptoms observed by Messrs Jolyet and Cahours on frogs and on mammals, on the supposition that the methyl- and ethyl-strychnium salts they employed contained strychnia.

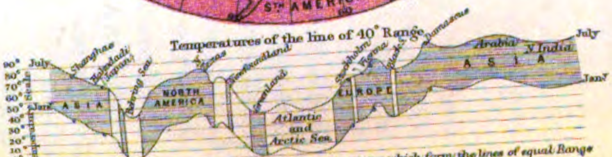
The authors have made a number of experiments which support this supposition. Several specimens of these salts, prepared by them, were found to produce such complicated effects as the French physiologists describe; but by carefully treating them a second time with iodide of methyl or of ethyl they succeeded in removing the convulsant action. They have also treated a specimen of iodide of ethyl-strychnium that produced strychnic effects, with iodide of methyl, and thus obtained a substance whose action was a purely

* Comptes Rendus, Nov. 2, 1868, p. 904.

† Proceed. Roy. Soc. Edin. vol. vi. p. 434.

ANNUAL RANGE OF TEMPERATURE
OVER THE GLOBE



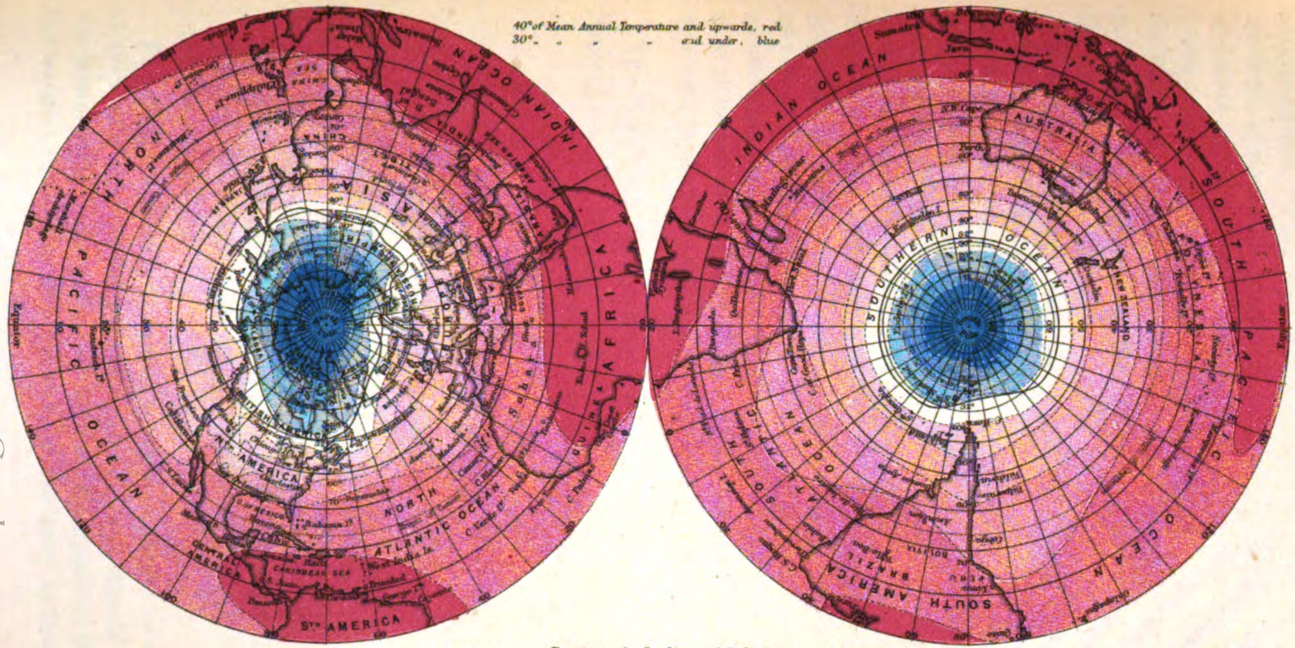


To show the Variations in the January and July temperatures which form the lines of equal Range

To show the Variations of Range in the same Latitude and the difference of Range on East and West Coasts

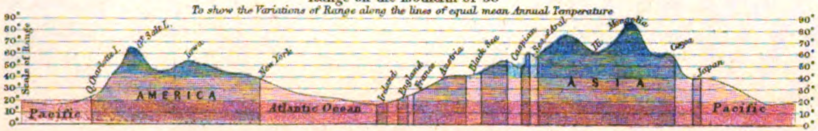
W. & A. K. Johnston, Edinburgh.

ANNUAL ISOTHERMAL LINES



Range on the Isotherm of 50°

To show the Variations of Range along the lines of equal mean Annual Temperature



W. & A. K. Johnston, Edinburgh.

paralysing one. They have, besides, added minute quantities of strychnia to specimens of salts of methyl and of ethyl-strychnium of perfect purity, and they found that the effects which were then produced were exactly the same as those described by Messrs Jolyet and Cahours. The correctness of the statements previously communicated to this Society by the authors was thus established in the most undoubted manner.

In their first experiments with these strychnia derivatives, and especially with that formed by iodide of ethyl, the authors obtained results similar to those described by Messrs Jolyet and Cahours; and they have been induced to enter thus fully into the subject from the knowledge, gained by their experience, of the importance as well as of the difficulty of obtaining these bodies in a state of absolute purity. This is apparent if it be recollected that the presence of 0·1 per cent. of strychnia is sufficient to vitiate the results of such experiments. It was only after the authors had adopted precautions, which seemed at first to be perfectly unnecessary, that they succeeded in eliminating this source of error, and in obtaining their substances in a state of *physiological* purity.

The following Gentleman was elected a Fellow of the Society:—

Robert Henry Bow, Esq., C.E.

Monday, 31*st May* 1869.

Dr CHRISTISON, President, in the Chair.

The following Communications were read:—

1. On the Annual Range of Temperature over the Globe. By Mr Keith Johnston, jun. Communicated by Mr Buchan, Secretary of the Scottish Meteorological Society. (With a Plate.)

The subject of range of temperature has been divided by meteorologists into the two main heads of *Diurnal* and *Annual* range, the former being measured by the variation of temperature between the warmest and coldest hours of the day, the latter by the difference of temperature between the warmest and coldest months of the year.

For a study of *diurnal range* we should require a series of observations for every hour of the day and night from all parts of the earth, and the places where such laborious observations have been made are as yet very few; but the returns of daily and monthly temperatures now obtainable from all countries are sufficient for a tolerably complete study of the simpler head of *annual range*.

It is believed that the subject of annual range has never before been systematically worked out for any large portion of the earth's surface, though its general conditions may have been recognised from the comparison of a few isolated points.

In preparing the charts of annual range now shown, it was first assumed that the months of January and July are respectively the coldest and warmest months of the northern, and the warmest and coldest months of the southern hemisphere. This proves to be the case, with few exceptions, in all parts of the earth, excepting those near the equator, which have two maxima and minima; but the annual range of temperature in these regions is so very small, that the difference between the temperatures of January and July may be taken as the measure for every part of the earth where range is considerable. January and July temperatures were then collected from all available sources, and their differences being taken, the annual range of twelve hundred places in all countries was obtained. These figures were then set down on maps, on the positions of the places of observation, and lines were drawn through those which have an equal range, at intervals of ten degrees of increase,* thus presenting the subject in a graphic form. The places of observation in Europe are so numerous as to enable the lines to be laid down there with almost absolute precision. Then the collection of temperatures throughout the United States, Mexico, and the West Indies by Laurin Blodget, the returns from Canada and the Forts of the Hudson Bay Company, observations for a long series of years at the Danish stations in Western Greenland, and the

* In the reduced chart which accompanies this paper the lines of 10, 30, 50, 70, and 90 degrees of range have, for the sake of clearness, been omitted, but the course of these intermediate lines may perhaps be traced from the description which follows.

numerous registers of temperature kept in vessels exploring the north-west passage, complete the lines for North America. In Asia the observations made at Russian government stations in Siberia in the north, at the ports of China and Japan on the east, at the Dutch Colonies in the East Indies, and in British India on the south, give the direction of these lines with certainty. For Africa we have the returns of numerous French stations in Algeria and Marocco, from Egypt, from the colonies on the Guinea Coast, from Cape Colony and Natal on the south, and a few checking points from the observations of travellers in Central Africa and Abyssinia. In South America the ascertained points are fewest; but observations made at the ports on both sides of the continent, from Panama to Patagonia, those of Ross in the Falkland Islands, a collection of temperatures in the Argentine Confederation given in the work on that country by M. Martin de Moussy, along with a few isolated points in Brazil, enable the range lines to be drawn here also with confidence. Lastly, for the coasts and south-eastern part of the interior of Australia, and for New Zealand, we have a large number of observations made chiefly by the Colonial Government. On the charts shown the parts of the earth's surface which have a *less* annual range than 40° have been tinted in *red*, the strength of the colour increasing as the range diminishes; and those regions which have a *greater* range than 40° in *blue*, the depth of colour increasing with the range.

At the point where January and July, the coldest and warmest months of the northern hemisphere, turn to January, the warmest, and July, the coldest month of the southern hemisphere, we find a line passing round the globe on which there is no difference between the temperatures of these two months.

Starting from the first meridian we find this zero line passing along the Guinea Coast of Africa, through the centre of that continent, bent northwards by the table land of Abyssinia, and again by the Indian Ocean, entering India below Bombay and running curiously down the line of the Western Ghauts to Cape Comorin; then by the south of Sumatra through Java to New Caledonia, and across the Pacific between the parallels of 10° and 20° south latitude to the South American Coast, which it meets between latitude 5° and 10° south. Now it passes northwardly along the line of the Andes to Quito, and curves round the whole north coast of South America to beyond the mouth of the Amazon, then down into the South Atlantic to include the island of Ascension, which has a northerly range, and again northwards to the 20th parallel of north latitude, where it enters the African continent. It is observed that this zero line is very far from coincident with the mathematical equator of the globe, and that the parts of it which pass through the great land masses are in the northern hemisphere, in Africa, India, and South America, whilst those which pass through the Atlantic and Pacific Oceans are carried far into the southern hemisphere.

The line of 10° of range in the northern hemisphere passes through the centre of North Africa above Lake Tchad, is carried to the north by the influence of the Red Sea to beyond Djedda on its east coast, round the west and south coasts of Arabia, through Aden, and thence to India near the Gulf of Cutch. Here it reaches southward, taking the form of the peninsula, to beyond Madras, then crosses the Gulf of Bengal to Further India, passes round the coast of Cochin China and through the Philippine Islands in latitude 10° north, then turning to the north-east, keeps in this direction through the Pacific till it enters North America above the parallel of 40° north latitude; from that it turns down the coast of California to near the south of Mexico at Vera Cruz, through the West Indies just to the northward of Cuba, and across the Atlantic, with a northerly bend towards the Azores, to the African coast in latitude 25° north.

The line of 20° range in the northern hemisphere is one of the most interesting of all, since it shows in the most marked manner the difference of range between the west and east coasts of the continents. It passes through North Africa and Southern Asia, carried to the north at the Red Sea and Persian Gulf, leaves the east coast of Asia at Macao, near Hong-Kong, in 15° north latitude, runs well into the Pacific, keeping away from the Asiatic coast, and reaches the American west coast in latitude 55° north, or 40° further to the north than where it left the Asiatic coast, then it turns south to Mexico, across the north of the Gulf of Mexico to Florida, leaves the east coast between latitude 25° and 30° north, then into the Atlantic to the Bermudas, after which, bending due north-east, it reaches as far as Iceland, and perhaps Spitzbergen, before turning south to the Norway coast, again showing, as in the Pacific, a difference of 40° in latitude between the same range on an east and west coast. From the coast of Norway, at Stavanger, this line of 20° range dips down into the North Sea, then rises northwards along the coast of Scotland to Braemar in Aberdeenshire, bends thence to the west and south-west to between Glasgow and Greenock, then down the west coast of England by Liverpool and Chester, and Pembroke in South Wales, to Devonport on the coast of Cornwall, then along the south coast to the Isle of Wight and the Channel Islands, thus enclosing the main part of Great Britain with a greater range than Ireland, though the central and eastern parts of that island have a range of nearly 20°. The difference of range between the south-east coast of England and the west coast of Ireland is nearly 10°.

The line of 20° range then passes along the west coast of France and Spain, bending completely round the south-west and south coasts of the peninsula, through Lisbon, Cadiz, and Gibraltar into the Mediterranean, where it seems to form a great loop extending to the east nearly as far as Alexandria (21° range), touching in its return at Tunis, Algiers, and Oran in North Africa, and keeping along this coast as far as the Atlantic shores of Marocco, where it turns into the interior of Africa.

The line of 30° range runs through North Africa, Arabia, and Southern Asia to the north of the line of 20° range, and parallel to it, excepting in Northern India, where it forms a curious bend round the high mass of the Himalaya mountains, which appear to have a less range than is observed in the plain to the south of them. Leaving the Asiatic east coast, a little to the north of Canton, the line of 30° range passes between the Loo-Choo Islands and Japan, and thence north-easterly to the American coast, which it reaches just to the south of the peninsula of Aliaska. It runs southward along the west American coast, close to the line of 20° range, to near the 25th parallel of north latitude in Mexico; then bending round, first north and then east, follows along the coast of the Mexican Gulf and through the Southern States to Charleston on the east coast; from this it turns north-east and north, away from the east coast, but in its general direction, till it reaches land again at Godthaab on the west coast of Greenland; thence the line runs to the north-west of Iceland, and probably round Spitzbergen, turns south again to the coast of Norway at Hammerfest, follows down the whole west coast of Greenland, keeping near the head of the numerous fiords with which its coast is intersected, whilst the line of 20° range falls just at their mouths; through the Cattegat and Eastern Denmark, leaving Copenhagen just to the east of it; then following still the general direction of the west coast, though inland from it, runs through Brussels and Paris to the south of France. Here the range of the Pyrenees seems to cause a break—the greater elevation having a smaller range—and to isolate the portion of this line which passes round the Spanish peninsula. This separate portion of the line passes inland round the east and south coasts of Spain, but close to the north and west coasts. Taking up the main line again at Toulouse, we find it skirting the Mediterranean coasts of South France and Western Italy, through Genoa, Rome, and Naples, but leaving the east coast of Italy to cross the Adriatic to Dalmatia. A second small isolated part of this line is found in the eastern part of the island of Sicily, where Catania, on the east coast (on an average of sixty years' observations), has a range of 39°, whilst Palermo, on the north-west coast, has but 25°. From the Dalmatian coast the main line of 30° range turns to the south-east nearly through the centre of Greece and bends slightly north again in the Ægean to near Smyrna. A third isolated portion of this line is found in the south-east of the Black Sea, passing round Trebizond and along the coast of Caucasia. From the Ægean we find this line running through Cyprus to Syria, near Beyrout and Jerusalem, then turning west through the isthmus of Suez, just south of Cairo, and all along the north of Africa, not far inland from the coast, numerous observations in Algeria, showing that its average distance from the sea, in that country, is not more than 20 miles. On approaching the west coast it bends first south and then east into the centre of North Africa.

At 40° of range the lines begin to keep to the land masses—no part of the *open* ocean in any part of the globe having a range of more than 40°. In North Africa we find a large area of the Sahara with this range, where the north part of the line 40° reaches to within a hundred miles of the Mediterranean in Algeria, and extends probably south to the 20th parallel of north latitude. Taking up the line in Asia, we find it passing completely round the interior of Asia Minor, at no great distance from its coasts, then through Damascus into Arabia, round the head of the Persian Gulf, and through the Punjab, south of Peshawar, to the table-land of Tibet and the coast of China, near Shanghae; thence it turns north-east through the Yellow Sea to Hakodadi, in the north island of Japan, along the line of islands to the east coast of Kamtchatka, near Petropaulovsk, across the Behring Sea to Aliaska; thence down the west coast of America, close to the lines of less range, to the 35th parallel of north latitude, where it turns east across the continent to the west coast at Chesapeake Bay; thence it skirts the coasts of Nova Scotia, touches on Newfoundland at St John's, and, taking a northern direction, reaches the Greenland coast near Disco Island. Carried south by the influence of Greenland, it leaves the east coast, perhaps in the latitude of Iceland, and stretches thence probably beyond the North Pole, returning to the islands of Novaia Zemlia, where observation at two points gives a range of only 39°. From this the line of 40° range keeps near the north coast of Russia, and passes down through the centre of the Scandinavian Peninsula, by Christiana and Gottenburg to Stockholm, is carried northwards into the Gulf of Bothnia, then half way into the Gulf of Finland to Helsingfors, along the coasts of the Baltic provinces, and southwards through central Europe to Vienna. As we found the chain of the Pyrenees breaking the line of 30° range, so here we observe that the range of the Alps isolates the part of this line of 40°, which marks out the plain of Northern Italy. Another separate area, of above 40° range, is observed in the valley of the Ebro, in North-East Spain, and the heights of the Carpathians and Transylvanian Alps form a third exceptional portion of this line, since, though within the main line of 40°, they have a slightly less range. From Vienna the main line of 40° passes round the Hungarian plain, through Turkey to the north of the Balkan range, across the Black Sea south of the Crimea, into Trans Caucasia, nearly as far as Tiflis, and thence bends back again round the coasts of Asia Minor.

It is doubtful whether an area of any great extent, of above 50° of range, is to be found in Northern Africa, though one observation would seem to show that such does exist. The observed range at Biskra, in the interior of Algeria, is 54°; but since the observations at that station have not been made for any considerable period, no weight has been given to their indication. We find the line of 50° range entering Europe at the north of Norway, passing south to the centre of the Peninsula, then turning east to the coast at Hernösand, bent north by the Gulf of Bothnia to Carlö or the Finland coast, then turning south inland to near the Gulf of Finland, and again carried northward by the influence of the great Russian lakes; thence it passes southward midway between Moscow and St Petersburg to near the north-west side of the Black Sea, where it turns sharply to the eastward above the Black Sea and the Sea of Azov, and along the northern side of the ridge of the Caucasus to the Caspian. Round this sea the line of 50° range forms a loop which passes through the north-western part of the sea, but close to the remaining coasts, back to the south slope of the Caucasus; thence it passes westward through Erzerum into the interior of Asia Minor, curves round, and reversing its course, turns back into Asia through Persia, Tibet, and China to the eastern coast in the Yellow Sea, through the Corea, Saghalien Island, and Kamtchatka to the American coast at the mouth of the Kwichpak River in Alaska; thence southward into the interior of the continent to beyond the Great Salt Lake, where it turns to the east with two curious bends, and is carried to the north round the coasts of the great American lakes, only cutting off the north-west coast of Lake Superior with a greater range. It is curious to observe that the three greatest inland sea and lake regions in the northern hemisphere, the lakes of European Russia, the Caspian Sea, and the American lakes, have the same range of temperature; and that Fort-William, on the north-west coast of Lake Superior, has exactly the same range of 58°, observed at Astrakhan in the same relative position to the Caspian. From the Canadian side of the lakes near Ottawa, the line of 50° range bends south into the United States to as far as Albany in New York, then north-eastward through New Brunswick to the coast of Labrador, where three stations have this range. Keeping still northwards, it passes through Davis Strait to nearly as far north on the West Greenland coast at Upernivik, then through Greenland and round the Arctic Ocean, between North America and Asia (Point Barrow on the Arctic coast of North America having a range of 55°) to the Asiatic coast, probably near the mouth of the Obi river, and thence along the coast to the north of Norway, where we first took it up.

The line of 60° range, and those above it, are confined to the Asiatic and American Continents, and may be called land lines. Taking up the line of 60° of range in the north of European Russia, we observe it making a bend to the north-west round the town of Krasnoiarsk (which has a range of 65°) back again eastward to the Ural Mountains beyond Ufa, then forming a second western projection to as far as Kharkov, on the north of the Sea of Azov, then passing back round the east side of the Caspian Sea, and sending out a narrow tongue-like extension through Northern Persia and the town of Urumiah towards Asia Minor, then returning eastwards through Central Asia to the south of the town of Ili in Eastern Turkestan (62° range), through China, with a probable bend to the southward, rising again to near Pekin and Tientsin, through Manchuria, near its coasts, to Port Ayan (60°), on the Sea of Okhotsk; thence this line seems to pass round the north-eastern projection of Asia, and to turn westward through the Arctic Ocean, but not far from the land, to the north of the Ural Chain, where it enters Russia.

In America, taking up the line of 60° range at Port-Clarence on the coast of Behring Strait, we find it passing down into the interior to the east of British Columbia as far as Oregon, then following the same curves as the line of 50° range through the centre of the continent, to the north of the lakes, and bending round with the coast in Eastern Canada and Labrador, northwards through Melville Peninsula and Baffin Bay to Greenland in latitude 75° north. After passing through Greenland it most probably bends backwards along its north coasts and those of the islands of the Arctic Archipelago to where we find proofs of its existence in the westmost of these islands, thence to the north coast of America, along which it passes south from Point Barrow to Behring Strait.

A third area, which we find surrounded by a line of 60° range, is that in the north-east of Norway, in Lapland, and in the Kola

Peninsula. This line reaches as far south as Umea on the east coast of the Gulf of Bothnia, and passes just to the west of Tornea, at the head of the Gulf; thence across to the White Sea, and embracing the Kola Peninsula and the coast to westward of the Waranger Fiord, to where it enters Norway.

Beginning at Tobolsk in Siberia, we observe the line of 70° of range turning south-west to Orenburg and forming a western extension into European Russia, north of the Caspian Sea, as far as the Volga; thence round the north and east of the sea of Aral (where Raimsk has a range of 69°), and probably south from this into the deserts of Western Turkestan to the latitude of the south of the Caspian Sea; thence north-east through Barnaul on the Obi to the north of Krasnoiarsk on the Yenisei River (which has a range of 63°); south again through Irkutsk on the west side of Lake Baikal, probably to the northern frontier of China-proper, enclosing the desert of Mongolia; then north-eastward, taking the general direction of the coast to near Okhotsk (66° range). Afterwards bending round parallel with the line of 60° range this line must keep along near the north coast of Asia, re-entering the land at the Obi river and following along the east side of the Ural chain to where we found it first, at Tobolsk.

The American line of 70° range passes nearly parallel with that of 60° from near the coast at Behring Strait into the continent past the south end of Lake Winnipeg, and thence has most probably an extention into the mass of land to the west of Labrador, but avoiding the sea till it returns to the west coast of Hudson Bay, which it crosses near Fort-York; from this it passes north-east through Southampton Island and Boothia to where Kane wintered in Smith Sound, probably extends thence into Greenland, and turns back from there to Melville Island and Banks Land, through which it passes south-west into the mainland of America. A small and remarkable area in the north-east of Norway has also a range of above 70°. It extends from Enontekis (73° range) in the centre of Lapland on the south, to beyond Wardö, on the east coast of Norway, which latter place has a range of 78°.

From Tara, in the government of Tobolsk in Siberia, the Asiatic line of 80° range passes eastward to the north of Lake Baikal, and then south between that lake and the town of Chita, which has a range 85°, probably far into the desert of Mongolia, then north-eastwardly parallel with the lines of 70° and 60° range, but further inland to the north of Nijnie Kolymsk (85° range), and along the north coast westwardly to beyond the mouth of the Lena, where it probably re-enters the continent, and passes south-west through the government of Tobolsk to Tara. The observing stations on the chain of lakes in America which stretch north-west from Lake Winnipeg, have all a less range than 80° till we arrive at the most northerly, the Great Slave lake, whose north coast at Fort Confidence has this range; but two belts of country, one on each side of this lake-region, appear to have a range of upwards of 80°. These two belts unite at the north of the Great Slave lake, and stretch out thence westwards into Alaska territory, and eastward as far as Victoria Land and Boothia. A solitary observation of 93° of range, on the Yukon river in the centre of Alaska territory, might justify the enclosing of a small area in that region as having a range of above 90°, and this is certainly the part of the American continent which has the greatest range.

In Asia, the area which has a greater range than 90°, probably extends in Siberia from the Yenisei river on the west to the Stanovoi range of mountains in the east, and from near the north coast (where at Ust Yansk we find a range of 92°) to the Yablonoi range in the south. The town of Chita to the south of these mountains has a range of 85°. A smaller area in the interior of this one must be surrounded by a line of 100° of range. Near the centre of this terrible region is Yakutsk, the point of the earth's surface which has the greatest range of temperature, whose climate undergoes a change between the months of January and July of the fearful amount of 106 degrees.

In the Southern Hemisphere of range, the line of 10° seems to enter North Africa on the west coast not far north of Cape Colony, and to extend northwards into the centre of the continent to the north of the equator at Gondokoro on the Nile (whose January temperature is 11° above that of July), with a further extension towards the plateau of Abyssinia, since observations made by the traveller Bruce at Gondar give the January temperature at that place 8° above that of July. From this the line of 10° of range seems to keep due south, through Tete on the Zambesi to where it crosses the east coast in Natal; thence it passes over the Indian Ocean, with a slight bend to the northward south of the islands of St Denis and Bourbon, both of which have a range of 8°, to the south coast of Australia at Adelaide; from this, turning westward again, it runs along the south and west coasts, till, entering the latter coast about latitude 20° south, it crosses the north of the continent below the Gulf of Carpentaria into the Pacific, passes thence between New Caledonia and New Zealand, across the Pacific to the coast of South America southward of Valparaiso (9° range); thence it runs northwardly along the west coast of the continent to near Lima in latitude 12° south, bends up into the interior of the country, and crosses it in a general south-east direction to the east coast on the north of Rio de Janeiro (13° range), and finally passes over the Atlantic south-eastwardly to Africa.

The line of 20° range in the southern hemisphere is divided into three parts. The coasts of South Africa have nowhere an observed range of more than 16°; but in the interior an area showing slightly more than 20° of range extends from near the coasts of Cape Colony inland to perhaps 10° or 15° south latitude. Within Australia a line of 20° range passes from near Perth in Western Australia along the south coast, then close to the east coast (Brisbane and Newcastle showing 20° and 19° of range), and across the continent again westwardly near the parallel of 20° south latitude. The third line of 20° range enters the west coast of South America in about latitude 40° south, passes north near the coast to 25° south latitude, turns inland round the northern boundaries of the Argentine Confederation and Paraguay, and then curves round south and south-west to Monte Video on the east coast; thence it runs due south, round the Falkland Islands, which have a range of 16° to 18°, crosses the ocean towards the Cape of Good Hope, and thence east to New Zealand, passing through the south-eastern parts of both its islands, and from that over the Pacific to South America.

A line of 30° range follows close upon that of 20° in south Australia, and is probably confined to the southern half of the continent. In South America the range line of 30° passes along the west coast, perhaps from Cape Horn, northwards into the Argentine Confederation, through Catamarca and Santiago at its furthest north, returning thence perhaps outside the east coast.

The central and eastern parts of Patagonia have the highest range in the southern hemisphere, and this does not exceed 40°; observations at Mendoza in the north giving only 38° of range.

The lines of equal yearly temperature over the globe, or the annual isotherms, are sometimes termed "climate lines," and one is very apt to imagine that every place which lies under the same isotherm has the same climate, which is by no means the case. With a view to showing the variation of range in those parts of the earth which have the same mean annual temperature, six of the annual isotherms have been opened out, as it were, on each side of the meridian of Greenwich, and the amount of range for each point of these lines has been projected vertically above it, giving remarkable curves, of which the one shown beneath the chart of the annual isotherms* which accompanies this paper is a sample.

On the isotherm of 10° in the northern hemisphere, it is observed that the range may vary from 30° in the Arctic Ocean to above 100° in Eastern Siberia, near Yakutsk. The mean range on this isotherm is 65°.

The isotherm of 20° has also a mean range of 65°, but may have as little as 25° in the Arctic Ocean, and as much as 100° in Asia.

On the isotherm of 30°, or nearly a mean annual temperature of the freezing point, the range may vary from below 20° between Greenland and Norway, to above 90° in Eastern Asia, the mean range on this line being 60°.

The isotherm of 40° shows a variation of range of from less than 20° in the Atlantic Ocean south of Iceland and in the North Pacific, to above 80° in Mongolia; whilst the mean range is 47°.

The line of 50° of mean annual temperature has a mean range of 41°; but this varies between 20° in the west of Ireland, to above 80° in Asia.

On the isotherm of 60° we find a mean range of 40°, but only 10° in the Pacific, and above 70° in Central Asia.

From these curves then, it is evident that hardly any two regions

* The annual isothermal lines shown on this chart are those of Dove, recently revised by Mr Buchan.

having an equal mean temperature, even though of small extent, and at a very short distance from one another, have an equal amount of range, and that the places on the earth's surface in which these two conditions of equal mean annual temperature and equal annual range are the same, are very few and far between.

Again, to show the variations of range in the same latitude, some of the parallels of the northern hemisphere have been opened out, as the isotherms were in the former instance, on both sides of the first meridian, and the range on each point projected up from these as before, giving curves analagous to that shown below the southern hemisphere of the accompanying range chart.

One of the peculiarities shown by the curves thus produced, is that of the smallness of the range on seas and lakes, and the great amount on the land surface of the globe, as also the very immediate increase of range from some coasts towards the interior of the land. But by far the most interesting and curious feature presented by this diagram, or indeed by the whole subject, is that of the difference in the amount of range on the opposite coasts of continents and seas.

In the temperate regions, and even to some distance beyond these into the torrid zone and the arctic regions, *the range on coasts facing west is invariably less than that of coasts facing east, in the same latitude*, and this holds good not only in the case of great seas and land masses, but also on the shores of inland seas and lakes.

The comparison of the range at a few places in the same latitudes, but on the opposite coasts of the continents, given in the following tables, may serve to show the great amount of this difference.

In North America—

Latitude N.	West Coast.	Range.	East Coast.	Range.	Difference of Range.
57°	Sitka, . . .	23°	Port Nelson, Hudson Bay,	65°	42°
			Nain, Labrador,	52°	29°
48°	Port Orford, .	11°	Boston, . .	44°	33°
37° 30′	San Francisco,	8°	Richmond, Virginia, .	40°	32°
32° 30′	San Diego, California, . .	21°	Charleston, S. Carolina, .	31°	10°
			Mean Difference, ...		29°

The mean difference of these temperate coasts of North America, as determined by the crossings of the range lines on every fifth parallel of latitude, is 25°.

In Europe and Asia—

Latitude N.	West Coast.	Range.	East Coast.	Range.	Difference of Range.
60°	Bergen, Norway,	25°	Okhotsk, . .	66°	41°
57°	Riga, Russia, .	41°	PortAyan, Sea of Okhotsk,	60°	19°
	Mandal, Norway,	28°			32°
51°	Ostend, France,	26°	Mariinsk (Amur),	55°	29°
40°	Lisbon, . . .	15°	Pekin, . . .	56°	41°
31°	Jerusalem, . .	28°	Shanghae, . .	43°	15°
22°	Djedda, Arabia,	8°	Canton, . . .	35°	27°
			Mean Difference, ...		29°

The difference of range on these coasts, determined as before from the range lines, is again 25°, the same amount as we obtained for North America, thus establishing a close agreement in the relations of the coasts of these two continents.

The mean difference between the east and west coasts of that portion of North Africa which falls in the temperate regions is 10°, the Red Sea coast having that amount of range more than the Atlantic coast.

In South America we find Valparaiso in latitude 33° S. on the west coast, with a range of only 9°, and in latitude 35° S. on the east coast we have Monte Video and Buenos Ayres, with ranges of 21° and 25°, thus showing a difference between the coasts at this point of 14°. The range lines give a mean difference of 12°.

Observations on the west coast of South Africa, in the temperate region, are entirely wanting; but since the line of no January and July range falls 10° of latitude farther to the south in the Atlantic on the west, than it does on the east coast, where it just reaches the equator, we may perhaps assume that the west coast of South Africa has also a less range than the east, probably amounting to 5°.

In the temperate regions of South Australia the range lines give a mean difference between the coasts of 10°, the range on the *east* coast again predominating to this amount.

Taking a mean of all these differences, we find that, *in the temperate regions of the globe, the west coasts of the continents have* 15° *less range than the east coasts*, and we have seen that this difference may rise to above 40°.

An examination of the ranges on the coasts of inland seas, and even lakes, leads to the same general conclusion.

In the *Mediterranean* the following places—Alicante (30°), Valencia (40°), Barcelona (30°), Perpignan (36°), Montpellier (36°), Catania (39°), and Athens (38°), on *east* facing coasts, give a mean range of 36°; whilst Oran (21°), Algiers (21°), Palermo (25°), Naples (29°), Rome (30°), Corfu (28°), and Beyrout (28°), give a mean of 26° for the *west* facing coasts, or a difference of 10° between the two.

The mean of Odessa (47°) and Constantinople (33°) gives 40° of range for the *east* facing coast of the *Black Sea;* and Trebizond (29°) and Redut Kale in Caucasia (31°) give 30° as the range of the *west* facing coast; again showing a difference of 10° less on the latter coast. On the *east* facing coast of the *Caspian Sea*, taking the observations at Astrakhan and Lenkoran (58° and 42°), we have a mean of 50°, whilst Novo Petrovsk, the only observing station on the *west* facing coast, has 47° of range; Fort William (58°) and Bay City (55°) give 56° as the mean range of the *east* facing coast of *Lake Superior;* and Michipicoten, in the centre of its *west* facing coast, has only 46° of range, again showing a difference of 10°.

The causes of this uniform predominance of range on east coasts over west, might form an interesting subject for investigation. On no two continental coasts do we find exactly the same prevailing conditions of winds and currents, much less of elevation and form, so that the explanation of this phenomenon must rather be looked for in a special combination of these influences for each individual coast, than in any one cause acting over the whole earth's surface.

The lines of equal range are formed between different January and July temperatures in different parts of the globe. To show the manner in which these equal range lines move up and down the thermometer scale in their passage round the earth, a few of the range lines in the northern hemisphere have been opened out, as the isotherms were before, on each side of the meridian of Greenwich. The curved lines thus obtained, and of which a specimen is given under the northern hemisphere of the range chart, show the fluctuations of the temperatures forming the range lines of these amounts; the upper line shows the *July*, and the under line parallel to it the *January* temperatures, according to the scale shown at the side of the lines.

From this we observe that the line of 60° range lies between the temperatures of 30° Fahr. in January, and 90° Fahr. in July at its *maximum*, in Persia, and between −20° Fahr. and + 40° Fahr. in Baffin Bay at its *minimum*.

The line of 50° of range, again, has its maximum in Central Asia between 40° Fahr. in January and 90° Fahr. in July, and its minimum in the Arctic Sea between −15° Fahr. in January and + 35° Fahr. in July.

Forty degrees of range stands *highest* on the temperature scale in Arabia and Persia between 95° Fahr. in July, and 55° Fahr. in January; and is at its *lowest* point between −5° Fahr. and + 35° Fahr. in the Arctic Sea.

The line of 30° of range has its maximum in Central Africa between 95° Fahr. and 65° Fahr. in July and January, and its minimum between + 5° and 35° in the North Atlantic.

Lastly, the line of 20° of range is at its highest temperature in Central Africa between 95° Fahr. and 75° Fahr. in July and January, and at its lowest in North Norway between 47° and 27° Fahr. The next lowest temperatures on the line of 20° fall in the British Isles, between 38° in January and 58° in July at Braemar in North Scotland, and 42° in January and 62° in July at Ventnor in South England.

The points on these lines which pass through the same isotherms, or whose January and July temperatures are at the same heights on the scale, have, so far as regards temperature, exactly the same yearly climate. Thus, on the line of 60° of range we may compare Minnesota in the United States with South Russia; on the line of 50° range, the Great Salt Lake region, in America, with the Caucasus in Europe; and the east coast of China in Asia, or the American Lake district, with that of North Russia. Again, on the line of 40° range, Newfoundland has exactly the same climate as the Baltic provinces of Russia, and the coasts of

Kamtschatka may be compared with those of North Norway, or *Hakodadi* in Japan with *Vienna* in Austria.

On the 30° range line we find the north coasts of British Columbia comparable with those of North Norway, *Paris* with *Fort Vancouver* in Oregon, and the south of Spain with the north of Mexico; and lastly, on the 20° range line, Macao Island near Hong Kong in China with the same climate as Florida in America.

A point worthy of notice, but for the establishment of which few data are as yet to be had, is that of the apparent diminution of range on elevated parts of mountains or mountain chains. The best authenticated example of this is that presented by the Alps of Switzerland. Observations for ten and twenty years at the St Gothard and St Bernhard hospices, at elevations of above 6000 feet, give a range of only 27° for each, whilst the plain of Italy to the south has a range of upwards of 40°. This diminution for elevation is also observed in stations in the Pyrenees, the Transylvanian Alps, and the Himalayas.

By the aid of these charts of annual range we can at once predicate of any point of the earth's surface whether it has a uniform and even climate, or an extreme one, or what precise place it holds between these limits. These charts may thus be of considerable utility in themselves, but it is when taken as companions to the annual isothermal charts, that they have their highest value. From the *isothermal chart* we may find that any required place has a certain mean yearly temperature; but we have no means of ascertaining from it how far the temperatures of its coldest month may descend, or how high its warmest month may ascend the thermometer scale. Again, from the *range chart* taken by itself, we can only tell that the climate of the place in question is limited to a certain number of degrees, without being able to say what position these degrees occupy in respect to heat and cold. But let the charts be used in company, and then we may have all that is required. First, from the one chart find the mean annual temperature of the place, and from the other its annual range; then, *since the mean annual temperature of places in the temperate and arctic regions of the globe corresponds very closely with the midway point between their January and July temperatures*—if the one-half of the amount of the range be placed *above* and the other half *below* the mean annual temperature on the thermometer scale, we have at once the temperatures of the warmest and coldest months, and the yearly temperature of the place, a knowledge sufficient for almost every purpose. This is best illustrated by an example. If we take from the isothermal chart the mean annual temperature of Paris, we find it 52°, and from the other chart its annual range is 30°; then the mean annual temperature *plus* the half of this range gives 67°, the July, and the mean annual *minus* half of the range, 37°, the January temperature of Paris.

In conclusion, it is submitted that the chart of annual range is a useful companion to the isothermal chart, and that used together these two form a very complete guide to the temperature of the globe.*

2. Note on Electrolytic Polarisation. By Professor Tait.

The following note refers to some experiments instituted at the request of Mr Dewar, who asked me to determine the polarisation of the Palladium electrodes whose singular behaviour he recently described to the Society.

I had just obtained one of Sir W. Thomson's most recent forms of quadrant electrometer, and it occurred to me that *this* must be the proper instrument for determining polarisation, as its indica-cations are not affected by electric resistance, and give directly—that is, without assuming the truth of Ohm's law for reverse electromotive forces, and the consequent necessary determinations of resistance—the quantities required. The method employed by Wheatstone, Poggendorff, Buff, and others, assumes that the whole electromotive force in the circuit is the algebraic sum of those of the decomposing battery and of the electrodes,—an assumption whose truth some may consider to require proof, and which it is certainly useful to verify by an independent process. Again, after

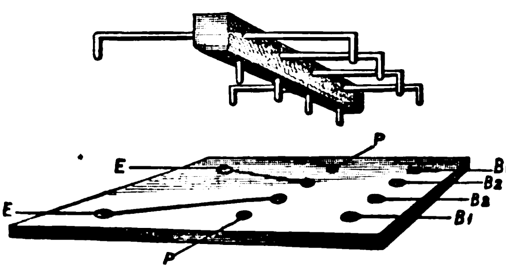
* The author has much pleasure in returning his best thanks to Mr Buchan, the Secretary of the Scottish Meteorological Society, for much assistance in the preparation of the materials for the chart, and for placing at his disposal a large private and unpublished collection of temperatures in all parts of the globe.

the decomposing action has ceased, the resistance of the films (of gas or oxide) which are deposited on the electrodes may change in value. That neither of these circumstances produces any marked effect is, however, amply proved by the numbers which follow, which, though given only as first approximations, are within the limits of difference of the results given (from galvanometric determinations) by former experimenters.

The experiments were all made in my laboratory, mainly under my own direction, but sometimes under the eye of my assistant, Mr W. R. Smith. Able assistance was rendered by several of my practical students,—two months ago by Messrs Russell Smith and J. C. Young, more recently by Messrs Browning and Nichol.

As the polarisation in most cases diminishes with very great rapidity from the instant of breaking contact with the decomposing battery, and as (for this and other reasons) the mode of measurement by the first swing of the index-needle of the electrometer is not deserving of much confidence, it was necessary to devise a process by which the electrometer could be charged at leisure up to any desired potential, and then, for an instant only, placed in connection with the electrodes. The apparatus I employed bears a certain analogy to the *wippe* of Poggendorff, but differs from it in some essential particulars, both of construction and mode of working.

In a plate of vulcanite, or other good insulator, ten holes are cut as below, and filled with mercury. Those marked E are connected



with pairs of opposite quadrants of the electrometer, P with the electrodes, B_1 with the decomposing battery, and B_2 with the auxiliary (or charging) battery. Also metallic connection, as indicated in the sketch, is permanently established between the two central holes and the holes connected with the electrometer.

The rocker consists of four wires, supported on an insulating bar of vulcanite, the two outermost having three points, the middle one longer than the others, and the two inner being similar, but wanting one of the extremities. When the four middle stems dip vertically into the four central mercury cups, the other stems do not reach the mercury in any of the other six cups. If the instrument be inclined to the right the four prongs enter the holes to the right—thus simultaneously connecting the electrodes with the decomposing battery, and the electrometer with the charging battery. When the instrument inclines to the left, the electrodes are shunted from the decomposing battery on to the electrometer,—the latter having just before, by the same action, been cut off from the charging battery, and thus left charged.

The *modus operandi* is simply this:—Leave the rocker leaning to the right by its own gravity, decomposition and polarisation going on; adjust the wires B_2 to different points in a wet string (or a narrow canal of water) closing the circuit of the charging battery; work the rocker quickly to the left, and allow it instantly to fall back again,—a process which need not occupy more than a small fraction of a second; yet which must not be performed too quickly, on account of the inertia (small as it is) of the needle and mirror of the electrometer. If the deflection of the electrometer be suddenly increased or diminished by this action, slide one of the wires B_2 along the wet string, a little farther from or nearer to the other, and rock again,—continuing this process till a charge is found which leaves the electrometer at rest when the rocking to and fro is performed. Reverse a commutator attached to the wires E, and repeat the operation. The difference of the scale readings in these two cases gives a number proportional to the electromotive force of the polarised plates—(I say *difference*, because the scales commonly used with Sir W. Thomson's instruments are, to avoid confusion, graduated from one end to the other, as they ought to be, instead of being graduated opposite ways from the middle). To enable this measure to be reduced to absolute units, a normal Daniell's cell was applied at intervals, during each day's work, directly to the electrodes of the electrometer, then reversed; and the difference of the readings was tabulated as representing its electromotive force.

In the earlier experiments I used a plate of gutta-percha in which the ten holes were bored, but for a time discontinued its use on suspecting that it sometimes led to irregular working of the apparatus by imperfect insulation. The cups were then *separately* mounted on insulators three inches high, but this was not found to be an improvement of any consequence; and the holes are now made in a small, but thick, plate of vulcanite.

In this note the numbers presented must be looked upon only as first approximations; but the apparatus has now been carefully constructed by an instrument maker, and Mr Dewar has begun an elaborate series of experiments with it, from which valuable results may soon be expected. In the trials which have as yet been made we employed a temporary apparatus, rudely built up of wires, sealing-wax, and gutta-percha. We have rather been endeavouring to determine whether the process, complicated as it is by the inertia of the movable part of the electrometer, the quickness with which the rocking can be conducted, and the rate at which the polarisation begins to diminish as soon as the polarised plates are detached from the decomposing battery, is capable of being made to give good results, than in actually attempting to get such. So far as I can yet see, the first of these complications is alone likely to cause any serious embarrassment; and should such be the case, which I do not anticipate, a form of experiment a little more laborious than that above described, and which I have already once or twice tried, seems to be well adapted to meet it.

The following are, for the most part, means of a great number of determinations. The electrolyte was usually dilute commercial sulphuric acid, 1 part acid to 10 of water; and to the lead and other impurities it was found to contain, we may ascribe the fact that the results were not very accordant from day to day, so that it was not easy to decide how to take the means. Mr Dewar is now working with substances chemically pure, and obtains much more constant results.

The unit employed is the electromotive force of an ordinary Daniell's cell. The Grove's cells used in the electrolysis had (very constantly) an electromotive force about 1·74 times as great.

I. Freshly Burned Platinum Plates.

No. of Grove's cells in decomposing battery, . . .	1	2	3	4	8
Resulting polarisation, . .	1·64	1·98	2·01	2·12	2·30

II. Platinum +, Palladium −.

Cells, ...	1	2	4
Polarisation, ...	1·50	1·82	1·85

III. Palladium +, Platinum −.

Cells, ...	1	2	4
Polarisation, ...	1·60	1·92	1·91 (?)

IV. With Three Cells.

	Platinum +, Iron −.	Platinum −, Iron +.	Iron Plates.
Polarisation, .	2·16	0·0	0·0

V. Aluminium Plates.

Cells, ...	1	2	3	4	6
Polarisation, . .	1·09	2·17	2·44 (?)	4·01	5·20

The last results are very remarkable, showing, as they do, from aluminium electrodes a reverse electromotive force of more than five Daniell's when six Grove's are in circuit. The polarisation alters so rapidly during the electrolysis (in the case of aluminium) that I cannot be certain that the numbers above given represent fully the maximum effect. Various other combinations have been tried, but are being repeated by Mr Dewar.

3. On the Causes of Volcanic Action. By Joseph John Murphy, Esq. Communicated by Dr Wyville Thomson.

In offering these remarks on the causes of volcanic action, I take it as proved that volcanoes are in communication with a reservoir of heat, occupying the centre of the earth: which heat, however, the earth is gradually, though very slowly, losing; partly by volcanic eruptions, partly by hot springs, and partly by the slow conduction of heat towards the surface through the strata.

I also take it as proved, that the earth is not fluid but solid throughout; or, at least, that its solid crust is of such thickness that the earth is to be regarded for all purposes as a solid body. Consequently the production of lava—that is to say, the liquefaction from heat of a small portion of the solid mass of the earth—is a strictly local occurrence; this is sufficiently proved by the well-known fact, that the lava stands at different levels in different volcanic craters, even when they are in eruption at the same time, and very near each other: as in the case of Mauna-Loa and Kilanea, in Hawaii (Scrope on Volcanoes, p. 262), which would be impossible if there were any free hydraulic communication between the different reservoirs of lava. The strata at great depth, notwithstanding their intense heat, are kept solid, as we have every reason to believe, by the pressure of the superincumbent strata, and *their liquefaction in particular places, producing lava, is due,* as I shall endeavour to prove, *to local relief from pressure.*

Darwin has brought forward what appears to be nearly conclusive evidence for believing that active volcanoes are to be found only in areas that are undergoing elevation. To quote his own words:—

"It may, I think, be considered as almost established, that volcanoes are often (not necessarily always) present in those areas where the subterranean motive power has lately forced, or is now forcing outwards the crust of the earth, but that they are invariably absent in those where the surface has lately subsided or is still subsiding."—Darwin on Coral Islands, p. 142.

It appears to be generally taken for granted that the expansive force of the earth's internal heat is the motive power that produces elevation, and also volcanic action. In this theory, however, there is a difficulty which is not, I think, generally perceived. At the beginning of geological time, the earth must have been sufficiently cooled down to permit the formation over its surface of a cold solid stratum. The lower strata are not hotter now than they were then, and may perhaps be *sensibly* colder, as they are constantly, though very slowly, parting with heat. If, then, the temperature of the lower strata, millions of ages ago, was low enough to permit a cold solid crust to form above them, how can it now, when it has not risen, be high enough to raise up that crust in upheavals, and break through it in eruptions? It seems almost a contradiction to think this possible. To use a familiar illustration:—When an iron casting is cooling in the sand, and, of course, like the earth, cooling from the surface; when once the cold hard crust begins to form, the greater heat of the interior has no tendency whatever to break through the crust. Were there no other force to upheave, or break through the earth's crust, I do not believe the internal heat could ever do it. I have now to state how I believe such actions are brought about.

The earth is cooling, and as it cools it must contract. But this is true only of the deeper strata: the surface is cooling and contracting no longer. But the surface, by reason of its weight, is clinging round the centre.

The earth is a sphere, whereof the centre is contracting, while the surface clings to the centre and yet cannot contract. What, then, will necessarily follow? The same that happens when an apple contracts, while its skin clings to it and yet cannot contract—the surface will form *wrinkles*, rising in some places and sinking in others. This, I believe, is the explanation of those gradual movements of elevation and subsidence, which, as we know, have taken place everywhere, and are now going on at least over extensive areas. But the formation of mountain chains having axes of igneous rocks, and of volcanoes, needs further explanation.

Over areas of elevation the superficial strata are, in a slight degree, being lifted up; over areas of subsidence they are, in an equal degree, being forced down. When they are lifted up, there will be a diminution of pressure on the lower strata. (This may be made evident by considering that if the upper strata were lifted up into an arch, which, however, is probably impossible, they would not press on the lower strata at all.) A diminution of pressure on the lower heated strata will cause them to melt into lava, expanding at the same time (igneous rocks, in the act of melting, expand by from a ninth to a third of their volume when unmelted—Bischoff, quoted in Scrope on Volcanoes, p. 44), and the same cause will make their imprisoned vapours expand. (For the existence of these vapours, see Scrope on Volcanoes, p. 56.) This action probably begins suddenly, as soon as the diminution of pressure is sufficient to permit the rocks to begin to melt. In this way, as I believe, the igneous axes of mountain chains have been thrust up. Two causes there are which so operate—the general subsidence of the crust of the earth, thrusting it in particular places over regions of elevation, into huge anticlinal wrinkles, and the expansive force of the heat from below, *when once liberated* by local relief from pressure. The former, as I have explained above, is what determines igneous action in any particular place; the latter accounts for all violent action, such as highly inclined and overturned strata; without it there would be no mountains, but only gentle elevations.

A volcano is formed whenever the igneous matter from below either forces its way to the surface, or (what is perhaps a possible case) has way made for it through a crack in the superficial strata, formed as they bend into wrinkles. But the fact that volcanoes are found only in regions of elevation, proves to my mind that the first determining cause of volcanic action is the elevation, causing local relief from pressure, and enabling the solids and vapours to expand from the heat they contain.

Proof has been found of the existence of active and recent volcanoes in the heart of the Asiatic and North American continents (see Scrope on Volcanoes, pp. 405, 453), but they are very much more numerous on islands and coasts. There must be some reason for this, and, I believe, the reason is, that the ocean is the great agent of denudation and deposition; consequently changes in the weight pressing on the various positions of the bed of the ocean are constantly taking place, and such changes cannot fail to cause changes in the limits of the areas of elevation and subsidence, and to produce irregularity in the wrinkling action; and the more of such changes and irregularities there are, the more multiplied will be the chances of local diminution of pressure sufficient to cause volcanic action. Over great continental areas, on the oontrary, the wrinkling action will be undisturbed, and will go on for the most part more evenly, though it will still produce igneous action in some cases.

4. Preliminary Note on the Antagonism between the Actions of Physostigma (Calabar Bean) and Belladonna. By Dr Thomas R. Fraser.

(*Abstract.*)

In this paper the author described a number of observations which appear to demonstrate that the fatal action of certain doses of physostigma may be prevented by the administration of atropia.

The following plan was adopted for the investigation:—After the minimum fatal dose of each poison had been approximately determined for rabbits and dogs of different weights, a fatal dose of one of the poisons was administered along with a certain dose of the other, and, if death did not result, the same animal was subsequently killed by a dose of one of the poisons equal to or less than that given in the combination.

To illustrate the general character of the results, the following experiments, among others, were described:—

Experiment 1.—A solution of four grains of sulphate of atropia, in twenty minims of distilled water, was injected under the skin at the left flank of a young rabbit, and, immediately afterwards, a solution of 0·5 gr. of physostigma, in fifteen minims of distilled water, was injected under the skin at the opposite flank. In two minutes the pupils were dilated. Fibrillary twitches then occurred, and in about ten minutes paralysis was present to such a decided extent that the rabbit had great difficulty in moving about, and usually lay extended on the abdomen and chest, with the head resting on the lower law. It continued in this state for about twenty minutes, and then the symptoms gradually disappeared; so that, in rather more than an hour and thirty minutes after the injections, it appeared to have recovered perfectly, with the exception that the pupil-dilatation remained.

Experiment 2.—Several days subsequently, 0·5 grain of extract of physostigma, in fifteen minims of distilled water, was injected under the skin of this animal. Death occurred in fifteen minutes.

Experiment 3.—In the next experiment, a rabbit of nearly the same weight as that used in the last received six grains of sulphate of atropia, and then 0·5 grain of extract of physostigma. The symptoms were less serious and of shorter duration than in the

experiment where the same dose of extract of physostigma was given along with only four grains of sulphate of atropia, and they appeared to be caused by the latter substance chiefly.

Experiment 4.—Four days afterwards, it was found that 0·5 grain of extract of physostigma was a fatal dose for this rabbit also.

Evidence of a still more satisfactory character in support of the result that the toxic action of physostigma may be prevented by atropia was obtained with dogs.

Experiment 5.—Eight grains of sulphate of atropia and three grains of extract of physostigma, in distilled water, were injected under the skin of a vigorous English terrier dog, weighing ten pounds. The chief symptoms were dilatation of the pupils, partial paralysis, and hypnotism. Of these, the first continued for several days, and the last for less than twenty-four hours. The partial paralysis continued for only forty minutes, after which the dog was in a perfectly normal condition, except that the pupils were in full dilatation, and that a tendency to indulge in sleep was manifested.

Experiment 6.—Three weeks afterwards, the same dog received eight grains of sulphate of atropia and six grains of extract of physostigma—the latter being twice as large a dose as that given in the previous experiment. Dilatation of the pupils and considerable loss of motor power were again produced; but, in addition, certain symptoms were prominently exhibited that were undoubtedly due to the physostigma, such as tremors and exaggerated bronchial secretion. The partial paralysis and tremors continued for more than three hours, and the dilatation of the pupils for several days, after which the dog perfectly regained its former condition.

Experiment 7.—In order to show distinctly that the atropia had prevented the fatal action of the physostigma given in these two experiments, this dog received, some weeks afterwards, three grains of extract of physostigma—a dose equal to that from which it recovered in the first experiment, and *only half* as large as that from which it recovered in the second. The results were that partial paralysis and tremors were quickly produced, that the lachrymal and salivary secretions were profusely increased, and that the respirations became more and more laboured and jerking, until they ceased in death, at seventeen minutes after the administration.

These experiments appear to prove that in rabbits and dogs the fatal action of certain doses of physostigma may be prevented by atropia.

Many examples can be referred to in which several of the actions of various substances may be impeded, prevented, or counteracted by those of others. Of these we may instance the antagonism between the actions of physostigma and atropia and of opium and atropia on the iris, and also of these pairs of substances and of quinia and morphia on the minute blood-vessels. It is, however, doubtful if any absolutely demonstrated example of antagonism can be referred to in which the toxic action of one substance may be prevented by the physiological action of another. The only instance deserving of notice in which antagonism of the latter class is asserted to exist, is that between opium on the one hand, and belladonna, hyosciamus, and other active substances derived from the *Solanaceæ* on the other. The truth of this reputed antagonism has not, however, been established. The observations on man that appear to support it are found, on careful examination, to be liable to many objections; while the experiments that have been made on the lower animals have produced evidence that is directly opposed to its existence.

In bringing these experiments before the Society the author's purpose has only been to show that there exists an antagonism between the toxic action of certain doses of physostigma and of belladonna or atropia. In the further investigation of this subject it will be necessary to determine exactly on what special actions this antagonism depends, and also within what limits of dosage it is maintained. Being concerned with two substances each of which possesses a number of actions, it is not unreasonable to anticipate that several of them are not mutually antagonistic. Above certain doses a region may, therefore, be entered where the non-antagonised actions are present in sufficient degrees to be themselves able to produce fatal effects.

These, and many other important questions, remain for examination. Their elucidation may be expected to have a direct bearing on the principles of therapeutics, for they require the demonstration of the manner in which certain accurately defined abnormal conditions are restored to normality by actions of an equally defined and accurately determined character.

In the meantime, the few facts that have been obtained indicate a valuable practical application. Although the existence of an antagonism between the fatal effects of physostigma and belladonna has been ascertained by experiments on rabbits and dogs only, there is no reason to doubt that it will be found to exist in man also. We may thus have at our disposal a physiological antidote for physostigma-poisoning, occasion for applying which may occur in this country, and already daily occurs in West Africa, where physostigma is extensively used as an ordeal poison.

5. On the Thermal Energy of Molecular Vortices. By W. J. Macquorn Rankine, C.E., LL.D., F.R.SS. Lond. and Edin., &c.

(*Abstract.*)

In a previous paper, presented to the Royal Society of Edinburgh in December 1849, and read on the 5th of February 1850 (Trans. vol. xx.), the author deduced the principles of thermodynamics, and various properties of elastic fluids, from the hypothesis of molecular vortices, under certain special suppositions as to the figure and arrangement of the vortices, and as to the properties of the matter which moves in them. In subsequent papers, he showed how the hypothesis might be simplified, by dispensing with some of the special suppositions. In the present paper he makes further progress in the same direction, and shows how the general equation of thermodynamics, and other propositions, are deduced from the hypothesis of molecular vortices when freed from all special suppositions as to the figure and arrangement of the vortices, and the properties of the matter that moves in them, and reduced simply to the following form: *that thermometric heat consists in a motion of the particles of bodies in circulating streams, with a velocity either constant or fluctuating periodically.* This, of course, implies that the forces acting amongst those particles are capable of transmitting that motion.

The principal conclusions arrived at are the following:—

1. In a substance in which the action of the vortices is isotropic, the intensity of the centrifugal pressure per unit of area is *two-thirds* of the energy due to the steady circulation in an unit of

volume. The centrifugal pressure is the pressure exerted by the substance in the perfectly gaseous state.

2.* If there be substances in which the action of the vortices is not isotropic, then in such substances the proportion already stated applies to the mean of the intensities of the centrifugal pressures in any three orthogonal directions.

3.* The proportion which the whole energy of the vortices, including that of the periodic disturbances, bears to the energy of the steady circulation alone, may be constant or variable.

4. Absolute temperature is proportional to the energy of the steady circulation in unity of mass, and to the specific volume in the perfectly gaseous state.

5. In substances which are nearly in the perfectly gaseous state, experiment shows the proportion in which the whole energy exceeds that of the steady circulation to be sensibly constant; and its value may be found by computing in what proportion the dynamical value of the specific heat at constant volume exceeds once and a half the quotient found by dividing the product of the pressure and volume by the absolute temperature. *The following are examples:—air, 1·634; nitrogen, 1·630; oxygen, 1·667; hydrogen, 1·614; steam-gas, 2·242.

6. The known general equation of thermodynamics is deduced from the hypothesis of molecular vortices,* freed from the special suppositions made in the paper of 1849–50.

The new conclusions obtained in the present paper are marked *. Those not so marked were arrived at in the paper of 1849–50.

[The general equation of thermodynamics is here stated for convenience; let dQ be the thermal energy which must be given to unity of mass of a given substance, in order to produce a given indefinitely small change in its temperature and dimensions: then—

$$dQ = \tau\, d \cdot \phi;$$

in which τ is the absolute temperature, and ϕ the thermodynamic function. The value of that function is—

$$\phi = Jc \text{ hyplog } \tau + \chi(\tau) + \frac{dU}{d\tau};$$

Jc being the dynamical value of the real specific heat; U, the potential energy of the elasticity of the body at constant temperature; and $\chi(\tau)$, a function of the absolute temperature, which is null or inappreciable in a substance capable, at that temperature, of approximating indefinitely to the perfectly gaseous state, and is included in the formula, in order to provide for the possibility, suggested by Clausius, that there may be substances which have not that property at all temperatures].

6. On the Rotation of a Rigid Body round a Fixed Point. Part II. By Professor Tait.

The following Gentlemen were elected Fellows of the Society:—

MAURICE LOTHIAN, Esq.
JOHN MACLAREN, Esq.

The following Donations to the Library were announced:—

Abbe (Cleveland). Dorpat and Poulkova. Washington, 1867. 8vo.—*From the Author.*

Baars (Herman). Berelning om den Internationale Fiskerindstilling i Boulogne-sur-Mer, 1866. 12mo.—*From the Author.*

——— Les Péches de la Norwége. Boulogne-sur-Mer, 1866. 8vo.—*From the Author.*

Bigsby (John J.), M.D., F.G.S. Flora and Fauna of the Silurian Period, with Addenda. London, 1868. 4to.—*From the Author.*

Broch (Dr O. J.). Traité Élémentaire des Fonctions Elliptiques. Fasc. II. Christiania, 1867. 8vo.—*From the Author.*

Brown (Robert). Das Innere der Vancouver-Insel. 4to. Being a German translation of his paper.—*Fro m Author.*

Castello de Paiva (Barone de). Monographia Molluscorum Terrestrium, Fluvialium, Lacustrium, Insularium Maderensium. Olisipone, 1867. 4to—*From the Author.*

Cohen (Henri). Description des Médailles Grecques, Romaines, etc. Paris, 1869. 8vo.—*From the Author.*

Day (St John Vincent), C.E. Malleable Iron Manufacture and the Richardson Process. Glasgow, 1868. 8vo.—*From the Author.*

——— Present State of some Branches of Iron Metallurgy. Glasgow, 1868. 8vo.—*From the Author.*

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Jenkin (Fleeming). On the Education of Civil and Mechanical Engineers. Edinburgh, 1868. 8vo.—*From the Author.*

Journal (American) of Science and Arts. Conducted by Benjamin Silliman. Nos. 138, 139, 140. New Haven. 8vo.—*From the Editor.*

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Lawson's Pinetum Britannicum. Parts XXXI. and XXXII. Edinburgh. Fol.—*From Charles Lawson, Esq.*

Maclaren (Charles). Select Writings. Edited by Robert Cox, F.S.A., and James Nicol, F.R.S.E. Vols. I. and II. 8vo.—*From Mrs Maclaren.*

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Manuscripts. Facsimiles of National Manuscripts of England. Selected under the direction of Colonel Sir Henry James. Part IV. Fol.—*From the Secretary of State for War.*

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Transactions and Proceedings of Learned Societies, Academies, &c.

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Monatsbericht der Königliche Preussischen Akademie der Wissenschaften zu Berlin. August, September, October, November, December 1868; January, February 1869. 8vo.—*From the Academy.*

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Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. Tomes XXV., XXVI., XXVII. Nos. 1, 2, 3, 4. 8vo.—*From the Academy.*

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