An Autobiographical Study
Sigmund Freud (1925)

I was born on May 6th, 1856, at Freiberg in Moravia, a small town in what is now Czechoslovakia. My parents were Jews, and I have remained a Jew myself. I have reason to believe that my father's family were settled for a long time on the Rhine (at Cologne), that, as a result of a persecution of the Jews during the fourteenth or fifteenth century, they fled eastwards, and that, in the course of the nineteenth century, they migrated back from Lithuania through Galicia into German Austria. When I was a child of four I came to Vienna, and I went through the whole of my education there. At the 'Gymnasium' I was at the top of my class for seven years; I enjoyed special privileges there, and had scarcely ever to be examined in class. Although we lived in very limited circumstances, my father insisted that, in my choice of a profession, I should follow my own inclinations alone. Neither at that time, nor indeed in my later life, did I feel any particular predilection for the career of a doctor. I was moved, rather, by a sort of curiosity, which was, however, directed more towards human concerns than towards natural objects; nor had I grasped the importance of observation as one of the best means of gratifying it. My deep engrossment in the Bible story (almost as soon as I had learned the art of reading) had, as I recognized much later, an enduring effect upon the direction of my interest. Under the powerful influence of a school friendship with a boy rather my senior who grew up to be a well-known politician, I developed a wish to study law like him and to engage in social activities. At the same time, the theories of Darwin, which were then of topical interest, strongly attracted me, for they held out hopes of an extraordinary advance in our understanding of the world; and it was hearing Goethe's beautiful essay on Nature read aloud at a popular lecture by Professor Carl Bruhl just before I left school that decided me to become a medical student.

When in 1873, I first joined the University, I experienced some appreciable disappointments. Above all, I found that I was expected to feel myself inferior and an alien because I was a Jew. I refused absolutely to do the first of these things. I have never been able to see why I should feel ashamed of my descent or, as people were beginning to say, of my race. I put up, without much regret, with my non-acceptance in the community; for it seemed to me that in spite of this exclusion an active fellow worker could not fail to find some nook or cranny in the framework of humanity. These first impressions at the University, however, had one consequence which was afterwards to prove important; for at an early age I was made familiar with the fate of being in the Opposition and of being put under the ban of the 'compact majority'. The foundations were thus laid for a certain degree of independence of judgment.

I was compelled, moreover, during my first years at the University, to make the discovery that the peculiarities and limitations of my gifts denied me all success in many of the departments of science into which my youthful eagerness had plunged me. Thus I learned the truth of Mephistopheles' warning:

> "In vain you roam around scientifically/ Everyone learns only what he can learn."

At length, in Ernst Brucke's physiological laboratory, I found rest and full satisfaction--and men, too, whom I could respect and take as my models: the great Brucke himself, and his assistants, Sigmund Exner and Ernst Fleischl Von Marxow. With the last of these, a brilliant man, I was privileged to be upon terms of friendship. Brucke gave me a problem to work out in the histology of the nervous system; I succeeded in solving it to his satisfaction and in carrying the work further on my own account. I worked at this Institute, with short interruptions, from 1876 to 1882, and it was generally thought that I was marked out to fill the next post of assistant that might fall vacant there. The various branches of medicine proper, apart from psychiatry, had no attraction for me. I was decidedly negligent in pursuing my medical studies, and it was not until 1881 that I took my somewhat belated degree as a Doctor of Medicine.

The turning-point came in 1882, when my teacher, for whom I felt the highest possible esteem, corrected my father's generous improvidence by strongly advising me, in view of my bad financial position, to abandon my theoretical career. I followed his advice, left the physiological laboratory and entered the General Hospital as an Aspirant [Clinical Assistant]. I was soon afterwards promoted to being a Sekundararzt [House Physician], and worked in various departments of the hospital, among others for more than six months under Meynert, by whose work and personality I had been greatly struck while I was still a student.

In a certain sense I nevertheless remained faithful to the line of work upon which I had originally started. The subject which Brucke had proposed for my investigations had been the spinal cord of one of the lowest of the fishes (Ammocoetes Petromyzon); and I now passed on to the human central nervous system. just at this time Flechsig's discoveries of the non-simlultaneity of the formation of the medullary sheaths were throwing a revealing light upon the intricate course of its tracts. The fact that I began by choosing the medulla oblongata as the one and only subject of my work was another sign of the continuity of my development. In complete contrast to the diffuse character of my studies during my earlier years at the University, I was now developing an inclination to concentrate my work exclusively upon a single subject or problem. This inclination has persisted and has since led to my being accused of one-sidedness.

I now became as active a worker in the Institute of Cerebral Anatomy, as I had previously been in the physiological one. Some short papers upon the course of the tracts and the nuclear origins in the medulla oblongata date from these hospital years, and some notice was taken of my findings by Edinger. One day Meynert, who had given me access to the laboratory even during the times when I was not actually working under him, proposed that I should definitely devote myself to the anatomy of the brain, and promised to hand over his lecturing work to me, as he felt he was too old to manage the newer methods. This I declined, in alarm at the magnitude of the task; it is possible, too, that I had guessed already that this great man was by no means kindly disposed towards me.

From the material point of view, brain anatomy was certainly no better than physiology, and, with an eye to pecuniary considerations, I began to study nervous diseases. There were, at that time, few specialists in that branch of medicine in Vienna, the material for its study was distributed over a number of different departments of the hospital, there was no satisfactory opportunity of learning the subject, and one was forced to be one's own teacher. Even Nothnagel, who had been appointed a short time before, on account of his book upon cerebral localization, did not single out neuropathology from among the other subdivisions of medicine. In the distance shone the great name of Charcot; so I formed a plan of first obtaining an appointment as University Lecturer (Dozent) on Nervous Diseases in Vienna and of then going to Paris to continue my studies.

In the course of the following years, while I continued to work as a junior physician, I published a number of clinical observations on organic diseases of the nervous system. I gradually became familiar with the ground; I was able to localize the site of a lesion in the medulla oblongata so accurately, that the pathological anatomist had no further information to add; I was the first person in Vienna to send a case for autopsy with a diagnosis of polyneuritis acuta.

The fame of my diagnoses and of their postmortem confirmation brought me an influx of American physicians, to whom I lectured upon the patients in my department in a sort of pidgin-English. About the neuroses I understood nothing. On one occasion I introduced to my audience a neurotic suffering from a persistent headache as a case of chronic localized meningitis; they all quite rightly rose in revolt and deserted me, and my premature activities as a teacher came to an end. By way of excuse I may add that this happened at a time when greater authorities than myself in Vienna were in the habit of diagnosing neurasthenia as cerebral tumor.

In the spring of 1885 1 was appointed Lecturer (Dozent) in Neuropathology on the ground of my histological and clinical publications. Soon afterwards, as the result of a warm testimonial from Brucke, I was awarded a Travelling Bursary of considerable value. In the autumn of the same year I made the journey to Paris. I became a student at the Salpetriere, but, as one of the crowd of foreign visitors, I had little attention paid me to begin with. One day in my hearing Charcot expressed his regret that since the war he had heard nothing from the German translator of his lectures; he went on to say that he would be glad if someone would undertake to translate the new volume of his lectures into German. I wrote to him and offered to do so; I can still remember a phrase in the letter, to the effect that I suffered only from 'I'aphasie motrice' and not from 'I'aphasie sensorielle du francais'. Charcot accepted the offer, I was admitted to the circle of his personal acquaintances, and from that time forward I took a full part in all that went on at the Clinic.

As I write these lines, a number of papers and newspaper articles have reached me from France, which give evidence of a violent objection to the acceptance of psychoanalysis, and which often make the most inaccurate assertions in regard to my relations with the French school. I read, for instance, that I made use of my visit to Paris to familiarize myself with the theories of Pierre Janet and then made off with my booty. I should therefore like to say explicitly that during the whole of my visit to the Salpetriere Janet's name was never so much as mentioned.

What impressed me most of all while I was with Charcot were his latest investigations upon hysteria, some of which were carried out under my own eyes. He had proved, for instance, the genuineness of hysterical phenomena and their conformity to laws (Enter, for here too are gods), the frequent occurrence of hysteria in men, the production of hysterical paralyses and contractures by hypnotic suggestion and the fact that such artificial products showed, down to their smallest details, the same features as spontaneous attacks, which were often brought on traumatically. Many of Charcot's demonstrations began by provoking in me and in other visitors a sense of astonishment and an inclination to skepticism, which we tried to justify by an appeal to one of the theories of the day. He was always friendly and patient in dealing with such doubts, but he was also most decided; it was in one of these discussions that (speaking of theory) he remarked, 'Ca n'empeche pas d'exister', a mot which left an indelible mark upon my mind.

No doubt not the whole of what Charcot taught us at that time holds good today: some of it has become doubtful, some has definitely failed to withstand the test of time. But enough is left over that has found a permanent place in the storehouse of science. Before leaving Paris I discussed with the great man a plan for a comparative study of hysterical and organic paralyses. I wished to establish the thesis that in hysteria paralyses and anaesthesias of the various parts of the body are demarcated according to the popular idea of their limits and not according to anatomical facts. He agreed with this view, but it was easy to see that in reality he took no special interest in penetrating more deeply into the psychology of the neuroses. When all is said and done, it was from pathological anatomy that his work had started.

Before I returned to Vienna I stopped for a few weeks in Berlin, in order to gain a little knowledge of the general disorders of childhood. Max Kassowitz, who was at the head of a public institute in Vienna for the treatment of children's diseases, had promised to put me in charge of a department for the nervous diseases of children. In Berlin I was given assistance and a friendly reception by Adolph Baginsky. In the course of the next few years I published, from the Kassowitz Institute, several monographs of considerable size on unilateral and bilateral cerebral palsies in children. And for that reason, at a later date (in 1897), Nothnagel made me responsible for dealing with the same subject in his great *Handbuch der allgemeinen und speziellen Therapie.*

In the autumn of 1886 I settled down in Vienna as a physician, and married the girl who had been waiting for me in a distant city for more than four years. I may here go back a little and explain how it was the fault of my fiancee that I was not already famous at that youthful age. A side interest, though it was a deep one, had led me in 1884 to obtain from Merck some of what was then the little-known alkaloid cocaine and to study its physiological action. While I was in the middle of this work, an opportunity arose for making a journey to visit my fiancee, from whom I had been parted for two years. I hastily wound up my investigation of cocaine and contented myself in my monograph on the subject with prophesying that further uses for it would soon be found. I suggested, however, to my friend Konigstein, the ophthalmologist, that he should investigate the question of how far the anaesthetizing properties of cocaine were applicable in diseases of the eye. When I returned from my holiday, I found that not he, but another of my friends, Carl Koller (now in New York), whom I had also spoken to about cocaine, had made the decisive experiments upon animals' eyes and had demonstrated them at the Ophthalmological Congress at Heidelberg. Koller is therefore rightly regarded as the discoverer of local anaesthesia by cocaine, which has become so important in minor surgery; but I bore my fiancee no grudge for the interruption.

I will now return to the year 1886, the time of my settling down in Vienna as a specialist in nervous diseases. The duty devolved upon me of giving a report before the Society of Medicine upon what I had seen and learnt with Charcot. But I met with a bad reception. Persons of authority such as the chairman (Bamberger, the physician), declared that what I said was incredible. Meynert challenged me to find some cases in Vienna similar to those which I had described and to present them before the Society. I tried to do so; but the senior physicians in whose departments I found any such cases refused to allow me to observe them or to work at them. One of them, an old surgeon, actually broke out with the exclamation: 'But, my dear sir, how can you talk such nonsense? Hysteron (sic) means the uterus. So how can a man be hysterical'?' I objected in vain that what I wanted was not to have my diagnosis approved, but to have the case put at my disposal. At length, outside the hospital, I came upon a case of classical hysterical hemi-anaesthesia in a man, and demonstrated it before the 'Gesellschaft der Aerzte' [1886]. This time I was applauded, but no further interest was taken in me. The impression that the high authorities had rejected my innovations remained unshaken; and, with my hysteria in men and my production of hysterical paralyses by suggestion, I found myself forced into the Opposition. As I was soon afterwards excluded from the laboratory of cerebral anatomy and for terms on end had nowhere to deliver my lectures, I withdrew from academic life and ceased to attend the learned societies. It is a whole generation since I have visited the ' Gesellschaft der Aerztc'.

Anyone who wants to make a living from the treatment of nervous patients must clearly be able to do something to help them. My therapeutic arsenal contained only two weapons, electrotherapy and hypnotism, for prescribing a visit to a hydropathic establishment after a single consultation was an inadequate source of income. My knowledge of electrotherapy was derived from W. Erb's text-book [1882], which provided detailed instructions for the treatment of all the symptoms of nervous diseases. Unluckily I was soon driven to see that following these instructions was of no help whatever and that what I had taken for an epitome of exact observations was merely the construction of phantasy. The realization that the work of the greatest name in German neuropathology had no more relation to reality than some Egyptian dreambook, such as is sold in cheap book-shops, was painful, but it helped to rid me of another shred of the innocent faith in authority from which I was not yet free. So I put my electrical apparatus aside, even before Moebius had saved the situation by explaining that the successes of electric treatment in nervous disorders (in so far as there were any) were the effect of suggestion on the part of the physician.

With hypnotism the case was better. While I was still a student I had attended a public exhibition given by Hansen the 'magnetist', and had noticed that one of the subjects experimented upon had become deathly pale at the onset of cataleptic rigidity and had remained so as long as that condition lasted. This firmly convinced me of the genuineness of the phenomena of hypnosis. Scientific support was soon afterwards given to this view by Heidenhain; but that did not restrain the professors of psychiatry from declaring for a long time to come that hypnotism was not only fraudulent but dangerous and from regarding hypnotists with contempt. In Paris I had seen hypnotism used freely as a method for producing symptoms in patients and then removing them again. And now the news reached us that a school had arisen at Nancy which made an extensive and remarkably successful use of suggestion, with or without hypnosis, for therapeutic purposes. It thus came about, as a matter of course, that in the first years of my activity as a physician my principal instrument of work, apart from haphazard and unsystematic psychotherapeutic methods, was hypnotic suggestion.

This implied, of course, that I abandoned the treatment of organic nervous diseases; but that was of little importance. For on the one hand the prospects in the treatment of such disorders were in any case never promising, while, on the other hand, in the private practice of a physician working in a large town, the quantity of such patients was nothing compared to the crowds of neurotics, whose number seemed further multiplied by the way in which they hurried, with their troubles unsolved, from one physician to another. And, apart from this, there was something positively seductive in working with hypnotism. For the first time there was a sense of having overcome one's helplessness; and it was highly flattering to enjoy the reputation of being a miracle-worker. It was not until later that I was to discover the drawbacks of the procedure. At the moment there were only two points to complain of. First, that I could not succeed in hypnotizing every patient, and secondly, that I was unable to put individual patients into as deep a state of hypnosis as I should have wished. With the idea of perfecting my hypnotic technique, I made a journey to Nancy in the summer of 1889 and spent several weeks there. I witnessed the moving spectacle of old Auguste Liebeault working among the poor women and children of the labouring classes. I was a spectator of Professor Hippolytel Bernheim's astonishing experiments upon his hospital patients, and I received the profoundest impression of the possibility that there could be powerful mental processes which nevertheless remained hidden from the consciousness of men. Thinking it would be instructive, I had persuaded one of my patients to follow me to Nancy. This patient was a very highly gifted hysteric, a woman of good birth, who had been handed over to me because no one knew what to do with her. By hypnotic influence I had made it possible for her to lead a tolerable existence and I was always able to take her out of the misery of her condition. But she always relapsed again after a short time, and in my ignorance I attributed this to the fact that her hypnosis had never reached the stage of somnambulism with amnesia. Bernheim now attempted several times to bring this about, but he too failed. He frankly admitted to me that his great therapeutic successes by means of suggestion were only achieved in his hospital practice and not with his private patients.

During the period from 1886 to 1891 I did little scientific work, and published scarcely anything. I was occupied with establishing myself in my new profession and with assuring my own material existence as well as that of a rapidly increasing family. In 1891 there appeared the first of my studies on the cerebral palsies of children, which was written in collaboration with my friend and assistant, Dr. Oskar Rie. An invitation which I received in the same year to contribute to an encyclopaedia of medicine led me to investigate the theory of aphasia. This was at the time dominated by the views of Wernicke and Lichtheim, which laid stress exclusively upon localization. The fruit of this inquiry was a small critical and speculative book, *Zur Auffassung der Aphasien*.

II.

I must supplement what I have just said by explaining that from the very first I made use of hypnosis in another manner, apart from hypnotic suggestion. I used it for questioning the patient upon the origin of his symptom, which in his waking state he could often describe only very imperfectly or not at all. Not only did this method seem more effective than mere suggestive commands or prohibitions, but it also satisfied the curiosity of the physician, who, after all, had a right to learn something of the origin of the phenomenon which he was striving to remove by the monotonous procedure of suggestion.

The manner in which I arrived at this other procedure was as follows. While I was still working in Brucke's laboratory I had made the acquaintance of Dr. Josef Breuer, who was one of the most respected family physicians in Vienna, but who also had a scientific past, since he had produced several works of permanent value upon the physiology of respiration and upon the organ of equilibrium. He was a man of striking intelligence and fourteen years older than myself. Our relations soon became more intimate and he became my friend and helper in my difficult circumstances. We grew accustomed to share all our scientific interests with each other. In this relationship the gain was naturally mine. The development of psychoanalysis afterwards cost me his friendship. It was not easy for me to pay such a price, but I could not escape it.

Even before I went to Paris, Breuer had told me about a case of hysteria which, between 1880 and 1882, he had treated in a peculiar manner which had allowed him to penetrate deeply into the causation and significance of hysterical symptoms. This was at a time, therefore, when Janet's works still belonged to the future. He repeatedly read me pieces of the case history, and I had an impression that it accomplished more towards all understanding of neuroses than any previous observation. I determined to inform Charcot of these discoveries when I reached Paris, and I actually did so. But the great man showed no interest in my first outline of the subject, so that I never returned to it and allowed it to pass from my mind.

When I was back in Vienna I turned once more to Breuer's observation and made him tell me more about it. The patient had been a young girl of unusual education and gifts, who had fallen ill while she was nursing her father, of whom she was devotedly fond. When Breuer took over her case it presented a variegated picture of paralyses with contractures, inhibitions and states of mental confusion. A chance observation showed her physician that she could be relieved of these clouded states of consciousness if she was induced to express in words the affective phantasy by which she was at the moment dominated. From this discovery, Breuer arrived at a new method of treatment. He put her into deep hypnosis and made her tell him each time what it was that was oppressing her mind. After the attacks of depressive confusion had been overcome in this way, he employed the same procedure for removing her inhibitions and physical disorders. In her waking state the girl could no more describe than other patients how her symptoms had arisen, and she could discover no link between them and any experiences of her life. In hypnosis she immediately discovered the missing connection. It turned out that all her symptoms went back to moving events which she had experienced while nursing her father; that is to say, her symptoms had a meaning and were residues or reminiscences of those emotional situations. It was found in most instances that there had been some thought or impulse which she had had to suppress while she was by her father's sick-bed, and that, in place of it, as a substitute for it, the symptom had afterwards appeared. But as a rule the symptom was not the precipitate of a single such 'traumatic' scene, but the result of a summation of a number of similar situations. When the patient recalled a situation of this kind in a hallucinatory way under hypnosis and carried through to its conclusion, with a free expression of emotion, the mental act which she had originally suppressed, the symptom was abolished and did not return. By this procedure Breuer succeeded, after long and painful efforts, in relieving his patient of all her symptoms.

The patient had recovered and had remained well and, in fact, had become capable of doing serious work. But over the final stage of this hypnotic treatment there rested a veil of obscurity, which Breuer never raised for me; and I could not understand why he had so long kept secret what seemed to me an invaluable discovery instead of making science the richer by it. The immediate question, however, was whether it was possible to generalize from what he had found in a single case. The state of things which he had discovered seemed to me to be of so fundamental a nature that I could not believe it could fail to be present in any case of hysteria if it had been proved to occur in a single one. But the question could only be decided by experience. I therefore began to repeat Breuer's investigations with my own patients and eventually, especially after my visit to Bernheim in 1889 had taught me the limitations of hypnotic suggestion, I worked at nothing else. After observing for several years that his findings were invariably confirmed in every case of hysteria that was accessible to such treatment, and after having accumulated a considerable amount of material in the shape of observations analogous to his, I proposed to him that we should issue a joint publication. At first he objected vehemently, but in the end he gave way, especially since, in the meantime, Janet's works had anticipated some of his results, such as the tracing back of hysterical symptoms to events in the patient's life, and their removal by means of hypnotic reproduction in *statu nascendi*. In 1893 we issued a preliminary communication, 'On the Psychical Mechanism of Hysterical Phenomena', and in 1895 there followed our book, *Studies on Hysteria*.

If the account I have so far given has led the reader to expect that the *Studies on Hysteria* must, in all essentials of their material content, be the product of Breuer's mind, that is precisely what I myself have always maintained and what it has been my aim to repeat here. As regards the theory put forward in the book, I was partly responsible, but to an extent which it is today no longer possible to determine. That theory was in any case unpretentious and hardly went beyond the direct description of the observations. It did not seek to establish the nature of hysteria but merely to throw light upon the origin of its symptoms. Thus it laid stress upon the significance of the life of the emotions and upon the importance of distinguishing between mental acts which are unconscious and those which are conscious (or rather capable of being conscious); it introduced a dynamic factor, by supposing that a symptom arises through the damming-up of an affect, and an economic factor, by regarding that same symptom as the product of the transformation of an amount of energy which would otherwise have been employed in some other way. (This latter process was described as conversion,) Breuer spoke of our method as cathartic; its therapeutic aim was explained as being to provide that the quota of affect used for maintaining the symptom, which had got on to the wrong lines and had, as it were, become strangulated there, should be directed on to the normal path along which it could obtain discharge (or abreaction). The practical results of the cathartic procedure were excellent. Its defects, which became evident later, were those of all forms of hypnotic treatment. Its value as an abridged method of treatment was shown afresh by Simmel [1918] in his treatment of war neuroses in the German army during the Great War. The theory of catharsis had not much to say on the subject of sexuality. In the case histories which I contributed to the Studies, sexual factors played a certain part, but scarcely more attention was paid to them than to other emotional excitations. Breuer wrote of the girl, who has since become famous as his first patient, that her sexual side was extraordinarily undeveloped. It would have been difficult to guess from the Studies on Hysteria what an importance sexuality has in the aetiology of the neuroses.

The event which formed the opening of this (transitional) period (from catharsis to psychoanalysis) was Breuer's retirement from our common work, so that I became the sole administrator of his legacy. There had been differences of opinion between us at quite an early stage, but they had not been a ground for our separating. In answering the question of when it is that a mental process becomes pathogenic--that is, when it is that it becomes impossible for it to be dealt with normally--Breuer preferred what might be called a physiological theory: he thought that the processes which could not find a normal outcome were such as had originated during unusual, 'hypnoid', mental states. This opened the further question of the origin of these hypnoid states. I, on the other hand, was inclined to suspect the existence of an interplay of forces and the operation of intentions and purposes such as are to be observed in normal life. Thus it was a case of 'hypnoid hysteria' versus 'neuroses of defense'. But such differences as this would scarcely have alienated him from the subject if there had not been other factors at work. One of these was undoubtedly that his work as a physician and family doctor took up much of his time, and that he could not, like me, devote his whole strength to the work of catharsis. Again, he was affected by the reception which our book had received both in Vienna and in Germany. His self-confidence and powers of resistance were not developed so fully as the rest of his mental organization. When, for instance, the *Studies* met with a severe rebuff from Adolf von Strumpell, I was able to laugh at the lack of comprehension which his criticism showed, but Breuer felt hurt and grew discouraged. But what contributed chiefly to his decision was that my own further work led in a direction to which he found it impossible to reconcile himself.

The theory which we had attempted to construct in the *Studies* remained, as I have said, very incomplete; and in particular we had scarcely touched on the problem of aetiology, on the question of the ground in which the pathogenic process takes root. I now learned from my rapidly increasing experience that it was not any kind of emotional excitation that was in action behind the phenomena of neurosis but habitually one of a sexual nature, whether it was a current sexual conflict or the effect of earlier sexual experiences. I was not prepared for this conclusion and my expectations played no part in it, for I had begun my investigation of neurotics quite unsuspectingly. While I was writing my 'History of the Psycho-Analytic Movement' in 1914, there recurred to my mind some remarks that had been made to me by Breuer, Charcot, and Chrobak, which might have led me to this discovery earlier. But at the time I heard them I did not understand what these authorities meant; indeed they had told me more than they knew themselves or were prepared to defend. What I heard from them lay dormant and inactive within me, until the chance of my cathartic experiments brought it out as an apparently original discovery. Nor was I then aware that in deriving hysteria from sexuality I was going back to the very beginnings of medicine and following up a thought of Plato's. It was not until later that I learnt this from an essay by Havelock Ellis.

Under the influence of my surprising discovery, I now took a momentous step. I went beyond the domain of hysteria and began to investigate the sexual life of the so-called neurasthenics who used to visit me in numbers during my consultation hours. This experiment cost me, it is true, my popularity as a doctor, but it brought me convictions which today, almost thirty years later, have lost none of their force. There was a great deal of equivocation and mystery-making to be overcome, but, once that had been done, it turned out that in all of these patients grave abuses of the sexual function were present. Considering how extremely widespread are these abuses on the one hand and neurasthenia on the other, a frequent coincidence between the two would not have proved much; but there was more in it than that one bald fact. Closer observation suggested to me that it was possible to pick out from the confused jumble of clinical pictures covered by the name of neurasthenia two fundamentally different types, which might appear in an degree of mixture but which were nevertheless to be observed in their pure forms. In the one type the central phenomenon was the anxiety attack with its equivalents, rudimentary forms and chronic substitutive symptoms; I consequently gave it the name of anxiety neurosis, and limited the term neurasthenia to the other type. Now it was easy to establish the fact that each of these types had a different abnormality of sexual life as its corresponding aetiological factor: in the former, coitus interruptus, unconsummated excitation and sexual abstinence, and in the latter, excessive masturbation and too numerous nocturnal emissions. In a few specially instructive cases, which had shown a surprising alteration in the clinical picture from one type to the other, it could be proved that there had been a corresponding change in the underlying sexual regime. If it was possible to put an end to the abuse and allow its place to be taken by normal sexual activity, a striking improvement in the condition was the reward.

I was thus led into regarding the neuroses as being without exception disturbances of the sexual function, the so-called 'actual neuroses' being the direct toxic expression of such disturbances and the psychoneuroses their mental expression. My medical conscience felt pleased at my having arrived at this conclusion. I hoped that I had filled up a gap in medical science, which, in dealing with a function of such great biological importance, had failed to take into account any injuries beyond those caused by infection or by gross anatomical lesions. The medical aspect of the matter was, moreover, supported by the fact that sexuality was not something purely mental. It had a somatic side as well, and it was possible to assign special chemical processes to it and to attribute sexual excitation to the presence of some particular, though at present unknown, substances. There must also have been some good reason why the true spontaneous neuroses resembled no group of diseases more closely than the phenomena of intoxication and abstinence, which are produced by the administration or privation of certain toxic substances, or than exophthalmic goitre, which is known to depend upon the product of the thyroid gland.

Since that time I have had no opportunity of returning to the investigation of the 'actual neuroses'; nor has this part of my work been continued by anyone else. If I look back today at my early findings, they strike me as being the first rough outlines of what is probably a far more complicated subject. But on the whole they seem to me still to hold good. I should have been very glad if I had been able, later on, to make a psychoanalytic examination of some more cases of simple juvenile neurasthenia, but unluckily the occasion did not arise. To avoid misconceptions, I should like to make it clear that I am far from denying the existence of mental conflicts and of neurotic complexes in neurasthenia. All that I am asserting is that the symptoms of these patients are not mentally determined or removable by analysis, but that they must be regarded as direct toxic consequences of disturbed sexual chemical processes.

During the years that followed the publication of the *Studies*, having reached these conclusions upon the part played by sexuality in the aetiology of the neuroses, I read some papers on the subject before various medical societies, but was only met with incredulity and contradiction. Breuer did what he could for some time longer to throw the great weight of his personal influence into the scales in my favor, but he effected nothing and it was easy to see that he too shrank from recognizing the sexual aetiology of the neuroses. He might have crushed me or at least disconcerted me by pointing to his own first patient, in whose case sexual factors had ostensibly played no part whatever. But he never did so, and I could not understand why this was, until I came to interpret the case correctly and to reconstruct, from some remarks which he had made, the conclusion of his treatment of it. After the work of catharsis had seemed to be completed, the girl had suddenly developed a condition of 'transference love'; he had not connected this with her illness, and had therefore retired in dismay. It was obviously painful to him to be reminded of this apparent contretemps. His attitude towards me oscillated for some time between appreciation and sharp criticism; then accidental difficulties arose, as they never fail to do in a strained situation, and we parted.

Another result of my taking up the study of nervous disorders in general was that I altered the technique of catharsis. I abandoned hypnotism and sought to replace it by some other method, because I was anxious not to be restricted to treating hysteriform conditions. Increasing experience had also given rise to two grave doubts in my mind as to the use of hypnotism even as a means to catharsis. The first was that even the most brilliant results were liable to be suddenly wiped away if my personal relation with the patient became disturbed. It was true that they would be re-established if a reconciliation could be effected; but such an occurrence proved that the personal emotional relation between doctor and patient was after all stronger than the whole cathartic process, and it was precisely that factor which escaped every effort at control. And one day I had an experience which showed me in the crudest light what I had long suspected. It related to one of my most acquiescent patients, with whom hypnotism had enabled me to bring about the most marvelous results, and whom I was engaged in relieving of her suffering by tracing back her attacks of pain to their origins. As she woke up on one occasion, she threw her arms round my neck. The unexpected entrance of a servant relieved us from a painful discussion, but from that time onwards there was a tacit understanding between us that the hypnotic treatment should be discontinued. I was modest enough not to attribute the event to my own irresistible personal attraction, and felt that I had now grasped the nature of the mysterious element that was at work behind hypnotism. In order to exclude it, or at all events to isolate it, it was necessary to abandon hypnotism.

But hypnotism had been of immense help in the cathartic treatment, by widening the field of the patient's consciousness and putting within his reach knowledge which he did not possess in his waking life. It seemed no easy task to find a substitute for it. While I was in this perplexity there came to my help the recollection of an experiment which I had often witnessed while I was with Bernheim. When the subject awoke from the state of somnambulism, he seemed to have lost all memory of what had happened while he was in that state. But Bernheim maintained that the memory was present all the same; and if he insisted on the subject remembering, if he asseverated that the subject knew it all and had only to say it, and if at the same time he laid his hand on the subject's forehead, then the forgotten memories used in fact to return, hesitatingly at first, but eventually in a flood and with complete clarity. I determined that I would act in the same way. My patients, I reflected, must in fact 'know' all the things which had hitherto only been made accessible to them in hypnosis; and assurances and encouragement on my part, assisted perhaps by the touch of my hand, would, I thought, have the power of forcing the forgotten facts and connections into consciousness. No doubt this seemed a more laborious process than putting the patients into hypnosis, but it might prove highly instructive. So I abandoned hypnotism, only retaining my practice of requiring the patient to lie upon a sofa while I sat behind him, seeing him, but not seen myself.

III.

My expectations were fulfilled; I was set free from hypnotism. But along with the change in technique the work of catharsis took on a new complexion. Hypnosis had screened from view an interplay of forces which now came in sight and the understanding of which gave a solid foundation to my theory.

How had it come about that the patients had forgotten so many of the facts of their external and internal lives but could nevertheless recollect them if a particular technique was applied? Observation supplied an exhaustive answer to these questions. Everything that had been forgotten had in some way or other been distressing; it had been either alarming or painful or shameful by the standards of the subject's personality. It was impossible not to conclude that that was precisely why it had been forgotten--that is, why it had not remained conscious. In order to make it conscious again in spite of this, it was necessary to overcome something that fought against one in the patient; it was necessary to make efforts on one's own part so as to urge and compel him to remember. The amount of effort required of the physician varied in different cases; it increased in direct proportion to the difficulty of what had to be remembered. The expenditure of force on the part of the physician was evidently the measure of a resistance on the part of the patient. It was only necessary to translate into words what I myself had observed, and I was in possession of the theory of repression.

It was now easy to reconstruct the pathogenic process. Let us keep to a simple example, in which a particular impulse had arisen in the subject's mind but was opposed by other powerful impulses. We should have expected the mental conflict which now arose to take the following course. The two dynamic quantities--for our present purposes let us call them 'the instinct' and 'the resistance'--would struggle with each other for some time in the fullest light of consciousness, until the instinct was repudiated and the cathexis of energy withdrawn from its impulsion. This would have been the normal solution. In a neurosis, however (for reasons which were still unknown), the conflict found a different outcome. The ego drew back, as it were, on its first collision with the objectionable instinctual impulse; it debarred the impulse from access to consciousness and to direct motor discharge, but at the same time the impulse retained its full cathexis of energy. I named this process repression; it was a novelty, and nothing like it had ever before been recognized in mental life. It was obviously a primary mechanism of defense, comparable to an attempt at flight, and was only a forerunner of the later-developed normal condemning judgement. The first act of repression involved further consequences. In the first place the ego was obliged to protect itself against the constant threat of a renewed advance on the part of the repressed impulse by making a permanent expenditure of energy, an anticathexis, and it thus impoverished itself. On the other hand, the repressed impulse, which was now unconscious, was able to find means of discharge and of substitutive satisfaction by circuitous routes and thus to bring the whole purpose of the repression to nothing. In the case of conversion hysteria the circuitous route led to the somatic innervation; the repressed impulse broke its way through at some point or other and produced symptoms. The symptoms were thus results of a compromise, for although they were substitutive satisfactions they were nevertheless distorted and deflected from their aim owing to the resistance of the ego.

The theory of repression became the corner-stone of our understanding of the neuroses. A different view had now to be taken of the task of therapy. Its aim was no longer to 'abreact' an affect which had got on to the wrong lines but to uncover repressions and replace them by acts of judgement which might result either in the accepting or in the condemning of what had formerly been repudiated. I showed my recognition of the new situation by no longer calling my method of investigation and treatment catharsis but psychoanalysis.

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But the study of pathogenic repressions and of other phenomena which have still to be mentioned compelled psychoanalysis to take the concept of the 'unconscious' seriously. Psychoanlaysis regarded everything mental as being in the first instance unconscious; the further quality of 'consciousness' might also be present, or again it might be absent. This of course provoked a denial from the philosophers, for whom 'conscious' and 'mental' were identical, and who protested that they could not conceive of such an absurdity as the 'unconscious mental'. There was no help for it, however, and this idiosyncrasy of the philosophers could only be disregarded with a shrug. Experience (gained from pathological material, of which the philosophers were ignorant) of the frequency and power of impulses of which one knew nothing directly, and whose existence had to be inferred like some fact in the external world, left no alternative open. It could be pointed out, incidentally, that this was only treating one's own mental life as one had always treated other people's. One did not hesitate to ascribe mental processes to other people, although one had no immediate consciousness of them and could only infer them from their words and actions. But what held good for other people must be applicable to oneself. Anyone who tried to push the argument further and to conclude from it that one's own hidden processes belonged actually to a second consciousness would be faced with the concept of a consciousness of which one knew nothing, of an 'unconscious consciousness'--and this would scarcely be preferable to the assumption of an 'unconscious mental'. If on the other hand one declared, like some other philosophers, that one was prepared to take pathological phenomena into account, but that the processes underlying them ought not to be described as mental but as 'psychological', the difference of opinion would degenerate into an unfruitful dispute about words, though even so expediency would decide in favor of keeping the expression 'unconscious mental'. The further question as to the ultimate nature of this unconscious is no more sensible or profitable than the older one as to the nature of the conscious.

It would be more difficult to explain concisely how it came about that psychoanalysis made a further distinction in the unconscious, and separated it into a preconscious and an unconscious proper. It will be sufficient to say that it appeared a legitimate course to supplement the theories that were a direct expression of experience with hypotheses that were designed to facilitate the handling of the material and related to matters which could not be a subject of immediate observation. The very same procedure is adopted by the older sciences. The subdivision of the unconscious is part of an attempt to picture the apparatus of the mind as being built up of a number of agencies or systems whose relations to one another are expressed in spatial terms, without, however, implying any connection with the actual anatomy of the brain. (I have described this as the topographical method of approach.) Such ideas as these are part of a speculative superstructure of psychoanalysis, any portion of which can be abandoned or changed without loss or regret the moment its inadequacy has been proved. But there is still plenty to be described that lies closer to actual experience.

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I have already mentioned that my investigation of the precipitating and underlying causes of the neuroses led me more and more frequently to conflicts between the subject's sexual impulses and his resistances to sexuality. In my search for the pathogenic situations in which the repressions of sexuality had set in and in which the symptoms, as substitutes for what was repressed, had had their origin, I was carried further and further back into the patient's life and ended by reaching the first years of his childhood. What poets and students of human nature had always asserted turned out to be true: the impressions of that early period of life, though they were for the most part buried in amnesia, left ineradicable traces upon the individual's growth and in particular laid down the disposition to any nervous disorder that was to follow. But since these experiences of childhood were always concerned with sexual excitations and the reaction against them, I found myself faced by the fact of infantile sexuality--once again a novelty and a contradiction of one of the strongest of human prejudices. Childhood was looked upon as 'innocent' and free from the lusts of sex, and the fight with the demon of 'sensuality' was not thought to begin until the troubled age of puberty. Such occasional sexual activities as it had been impossible to overlook in children were put down as signs of degeneracy or premature depravity or as a curious freak of nature. Few of the findings of psychoanalysis have met with such universal contradiction or have aroused such an outburst of indignation as the assertion that the sexual function starts at the beginning of life and reveals its presence by important signs even in childhood. And yet no other finding of analysis can be demonstrated so easily and so competely.

Before going further into the question of infantile sexuality I must mention an error into which I fell for a while and which might well have had fatal consequences for the whole of my work. Under the influence of the technical procedure which I used at that time, the majority of my patients reproduced from their childhood scenes in which they were sexually seduced by some grown-up person. With female patients the part of seducer was almost always assigned to their father. I believed these stories, and consequently supposed that I had discovered the roots of the subsequent neurosis in these experiences of sexual seduction in childhood. My confidence was strengthened by a few cases in which relations of this kind with a father, uncle, or elder brother had continued up to an age at which memory was to be trusted. If the reader feels inclined to shake his head at my credulity, I cannot altogether blame him; though I may plead that this was at a time when I was intentionally keeping my critical faculty in abeyance so as to preserve an unprejudiced and receptive attitude towards the many novelties which were coming to my notice every day. When, however, I was at last obliged to recognize that these scenes of seduction had never taken place, and that they were only fantasies which my patients had made up or which I myself had perhaps forced on them, I was for some time completely at a loss. My confidence alike in my technique and in its results suffered a severe blow; it could not be disputed that I had arrived at these scenes by a technical method which I considered correct, and their subject-matter was unquestionably related to the symptoms from which my investigation had started. When I had pulled myself together, I was able to draw the right conclusions from my discovery: namely, that the neurotic symptoms were not related directly to actual events but to wishful fantasies, and that as far as the neurosis was concerned psychical reality was of more importance than material reality. I do not believe even now that I forced the seduction-fantasies on my patients, or that I 'suggested' them. I had in fact stumbled for the first time upon the Oedipus complex, which was later to assume such an overwhelming importance, but which I did not recognize as yet in its disguise of fantasy. Moreover, seduction during childhood retained a certain share, though a humbler one, in the aetiology of neuroses. But the seducers turned out as a rule to have been older children.

It will be seen, then, that my mistake was of the same kind as would be made by someone who believed that the legendary story of the early kings of Rome (as told by Livy) was historical truth instead of what it is in fact--a reaction against the memory of times and circumstances that were insignificant and occasionally, perhaps, inglorious. When the mistake had been cleared up, the path to the study of the sexual life of children lay open. It thus became possible to apply psychoanalysis to another field of science and to use its data as a means of discovering a new piece of biological knowledge.

The sexual function, as I found, is in existence from the very beginning of the individual's life, though at first it is attached to the other vital functions and does not become independent of them until later; it has to pass through a long and complicated process of development before it becomes what we are familiar with as the normal sexual life of the adult. It begins by manifesting itself in the activity of a whole number of component instincts. These are dependent upon erotogenic zones in the body; some of them make their appearance in pairs of opposite impulses (such as sadism and masochism or the impulses to look and to be looked at); they operate independently of one another in a search for pleasure, and they find their object for the most part in the subject's own body. Thus at first the sexual function is non-centralized and predominately auto-erotic. Later, syntheses begin to appear in it; a first stage of organization is reached under the dominance of the oral components, an anal-sadistic stage follows, and it is only after the third stage has at last been reached that the primacy of the genitals is established and that the sexual function begins to serve the ends of reproduction. In the course of this process of development a number of elements of the various component instincts turn out to be unserviceable for this last end and are therefore left on one side or turned to other uses, while others are diverted from their aims and carried over into the genital organization. I gave the name of libido to the energy of the sexual instincts and to that form of energy alone. I was next driven to suppose that the libido does not always pass through its prescribed course of development smoothly. As a result either of the excessive strength of certain of the components or of experiences involving premature satisfaction, fixations of the libido may occur at various points in the course of its development. if subsequently a repression takes place, the libido flows back to these points (a process described as regression), and it is from them that the energy breaks through in the form of a symptom. Later on it further became clear that the localization of the point of fixation is what determines the choice of neurosis, that is, the form in which the subsequent illness makes its appearance.

The process of arriving at an object, which plays such an important part in mental life, takes place alongside of the organization of the libido. After the stage of auto-erotism, the first love-object in the case of both sexes is the mother; and it seems probable that to begin with a child does not distinguish its mother's organ of nutrition from its own body. Later, but still in the first years of infancy, the relation known as the Oedipus complex becomes established: boys concentrate their sexual wishes upon their mother and develop hostile impulses against their father as being a rival, while girls adopt an analogous attitude. All of the different variations and consequences of the Oedipus complex are important; and the innately bisexual constitution of human beings makes itself felt and increases the number of simultaneously active tendencies. Children do not become clear for quite a long time about the differences between the sexes; and during this period of sexual researches they produce typical sexual theories which, being circumscribed by the incompleteness of their authors' own physical development, are a mixture of truth and error and fail to solve the problems of sexual life (the riddle of the Sphinx-that is, the question of where babies come from). We see, then, that a child's first object-choice is an incestuous one. The whole course of development that I have described is run through rapidly. For the most remarkable feature of the sexual life of man is its diphasic onset, its onset in two waves, with an interval between them. It reaches a first climax in the fourth or fifth year of a child's life. But thereafter this early efflorescence of sexuality passes off; the sexual impulses which have shown such liveliness are overcome by repression, and a period of latency follows, which lasts until puberty and during which the reaction formations of morality, shame, and disgust are built up. Of all living creatures man alone seems to show this diphasic onset of sexual growth and it may perhaps be the biological determinant of his predisposition to neuroses. At puberty the impulses and object-relations of a child's early years become re-animated, and amongst them the emotional ties of its Oedipus complex. In the sexual life of puberty there is a struggle between the urges of early years and the inhibitions of the latency period. Before this, and while the child is at the highest point of its infantile sexual development, a genital organization of a sort is established; but only the male genitals play a part in it, and the female ones remain undiscovered. (I have described this as the period of phallic primacy.) At this stage the contrast between the sexes is not stated in terms of 'male' or 'female' but of 'possessing a penis' or 'castrated'. The castration complex which arises in this connection is of the profoundest importance in the formation alike of character and of neuroses.

The second of my alleged extensions of the concept of sexuality finds its justification in the fact revealed by psychoanalytic investigation that all of these affectionate impulses were originally of a completely sexual nature but have become inhibited in their aim or sublimated. The manner in which the sexual instincts can thus be influenced and diverted enables them to be employed for cultural activities of every kind, to which indeed they bring the most important contributions.

My surprising discoveries as to the sexuality of children were made in the first instance through the analysis of adults. But later (from about 1908 onwards) it became possible to confirm them fully and in every detail by direct observations upon children. Indeed, it is so easy to convince oneself of the regular sexual activities of children that one cannot help asking in astonishment how the human race can have succeeded in overlooking the facts and in maintaining for so long the wishful legend of the asexuality of childhood. This surprising circumstance must be connected with the amnesia which, with the majority of adults, hides their own infancy.

IV

The means which I first adopted for overcoming the patient's resistance, by insistence and encouragement, had been indispensable for the purpose of giving me a first general survey of what was to be expected. But in the long run it proved to be too much of a strain on both sides, and further, it seemed open to certain obvious criticisms. It therefore gave place to another method which was in one sense its opposite. Instead of urging the patient to say something upon some particular subject, I now asked him to abandon himself to a process of free association--that is, to say whatever came into his head, while ceasing to give any conscious direction to his thoughts. It was essential, however, that he should bind himself to report literally everything that occurred to his self-perception and not to give way to critical objections which sought to put certain associations on one side on the ground that they were not sufficiently important or that they were irrelevant or that they were altogether meaningless. There was no necessity to repeat explicitly the demand for candor on the patient's part in reporting his thoughts, for it was the precondition of the whole analytic treatment.

It may seem surprising that this method of free association, carried out subject to the observation of the fundamental rule of psychoanalysis, should have achieved what was expected of it, namely the bringing into consciousness of the repressed material which was held back by resistances. We must, however, bear in mind that free association is not really free. The patient remains under the influence of the analytic situation even though he is not directing his mental activities on to a particular subject. We shall be justified in assuming that nothing will occur to him that has not some reference to that situation. His resistance against reproducing the repressed material will now be expressed in two ways. Firstly it will be shown by critical objections; and it was to deal with these that the fundamental rule of psychoanalysis was invented. But if the patient observes that rule and so overcomes his reticences, the resistance will find another means of expression. It will so arrange it that the repressed material itself will never occur to the patient but only something which approximates to it in an allusive way; and the greater the resistance, the more remote from the actual idea that the analyst is in search of will be the substitutive association which the patient has to report. The analyst, who listens composedly but without any constrained effort to the stream of associations and who, from his experience, has a general notion of what to expect, can make use of the material brought to light by the patient according to two possibilities. If the resistance is slight, he will be able from the patient's allusions to infer the unconscious material itself; or if the resistance is stronger he will be able to recognize its character from the associations, as they seem to become more remote from the topic in hand, and will explain it to the patient. Uncovering the resistance, however, is the first step towards overcoming it. Thus the work of analysis involves an art of interpretation, the successful handling of which may require tact and practice but which is not hard to acquire. But it is not only in the saving of labor that the method of free association has an advantage over the earlier method. It exposes the patient to the least possible amount of compulsion, it never allows of contact being lost with the actual current situation, it guarantees to a great extent that no factor in the structure of the neurosis will be overlooked and that nothing will be introduced into it by the expectations of the analyst. It is left to the patient in all essentials to determine the course of the analysis and the arrangement of the material; any systematic handling of particular symptoms or complexes thus becomes impossible. In complete contrast to what happened with hypnotism and with the urging method, interrelated material makes its appearance at different times and at different points in the treatment. To a spectator, therefore--though in fact there must be none--an analytic treatment would seem completely obscure.

Another advantage of the method is that it need never break down. It must theoretically always be possible to have an association, provided that no conditions are made as to its character. Yet there is one case in which in fact a breakdown occurs with absolute regularity; from its very uniqueness, however, this case too can be interpreted.

I now come to the description of a factor which adds an essential feature to my picture of analysis and which can claim, alike technically and theoretically, to be regarded as of the first importance. In every analytic treatment there arises, without the physician's agency, an intense emotional relationship between the patient and the analyst which is not to be accounted for by the actual situation. It can be of a positive or of a negative character and can vary between the extremes of a passionate, completely sensual love and the unbridled expression of an embittered defiance and hatred. This transference--to give it its short name--soon replaces in the patient's mind the desire to be cured, and, so long as it is affectionate and moderate, becomes the agent of the physician's influence and neither more nor less than the mainspring of the joint work of analysis. Later on, when it has become passionate or has been converted into hostility, it becomes the principal tool of the resistance. It may then happen that it will paralyze the patient's powers of associating and endanger the success of the treatment. Yet it would be senseless to try to evade it; for an analysis without transference is an impossibility. It must not be supposed, however, that transference is created by analysis and does not occur apart from it. Transference is merely uncovered and isolated by analysis. It is a universal phenomenon of the human mind, it decides the success of all medical influence, and in fact dominates the whole of each person's relations to his human environment. We can easily recognize it as the same dynamic factor which the hypnotists have named 'suggestibility', which is the agent of hypnotic rapport and whose incalculable behavior led to difficulties with the cathartic method as well. When there is no inclination to a transference of emotion such as this, or when it has become entirely negative, as happens in dementia praecox or paranoia, then there is also no possibility of influencing the patient by psychological means.

It is perfectly true that psychoanalysis, like other psychotherapeutic methods, employs the instrument of suggestion (or transference). But the difference is this: that in analysis it is not allowed to play the decisive part in determining the therapeutic results. It is used instead to induce the patient to perform a piece of psychical work--the overcoming of his transference-resistances--which involves a permanent alteration in his mental economy. The transference is made conscious to the patient by the analyst, and it is resolved by convincing him that in his transference attitude he is re-experiencing emotional relations which had their origin in his earliest object-attachments during the repressed period of his childhood. In this way the transference is changed from the strongest weapon of the resistance into the best instrument of the analytic treatment. Nevertheless its handling remains the most difficult as well as the most important part of the technique of analysis.

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With the help of the method of free association and of the related art of interpretation, psychoanalysis succeeded in achieving one thing which appeared to be of no practical importance but which in fact necessarily led to a totally fresh attitude and a fresh scale of values in scientific thought. It became possible to prove that dreams have a meaning, and to discover it. In classical antiquity great importance was attached to dreams as foretelling the future; but modern science would have nothing to do with them, it handed them over to superstition, declaring them to be purely 'somatic' processes--a kind of twitching of a mind that is otherwise asleep. It seemed quite inconceivable that anyone who had done serious scientific work could make his appearance as an 'interpreter of dreams'. But by disregarding the excommunication pronounced upon dreams, by treating them as unexplained neurotic symptoms, as delusional or obsessional ideas, by neglecting their apparent content and by making their separate component images into subjects for free association, psychoanalysis arrived at a different conclusion. The numerous associations produced by the dreamer led to the discovery of a thought-structure which could no longer be described as absurd or confused, which ranked as a completely valid psychical product, and of which the manifest dream was no more than a distorted, abbreviated, and misunderstood translation, and for the most part a translation into visual images. These latent dream-thoughts contained the meaning of the dream, while its manifest content was simply a make-believe, a facade, which could serve as a starting-point for the associations but not for the interpretation.

There were now a whole series of questions to be answered, among the most important of them being whether the formation of dreams had a motive, under what conditions it took place, by what methods the dream-thoughts (which are invariably full of sense) become converted into the dream (which is often senseless), and others besides. I attempted to solve all of these problems in *The Interpretation of Dreams*, which I published in the year 1900. I can only find space here for the briefest abstract of my investigation. When the latent dream-thoughts that are revealed by the analysis of a dream are examined, one of them is found to stand out from among the rest, which are intelligible and well known to the dreamer. These latter thoughts are residues of waking life (the day's residues, as they are called technically); but the isolated thought is found to be a wishful impulse, often of a very repellent kind, which is foreign to the waking life of the dreamer and is consequently disavowed by him with surprise or indignation. This impulse is the actual constructor of the dream: it provides the energy for its production and makes use of the day's residues as material. The dream which thus originates represents a situation of satisfaction for the impulse, it is the fulfillment of its wish. It would not be possible for this process to take place without being favored by the presence of something in the nature of a state of sleep. The necessary mental precondition of sleep is the concentration of the ego upon the wish to sleep and the withdrawal of psychical energy from all the interests of life. Since at the same time all the paths of approach to mobility are blocked, the ego is also able to reduce the expenditure [of energy] by which at other times it maintains the repressions. The unconscious impulse makes use of this nocturnal relaxation of repression in order to push its way into consciousness with the dream. But the repressive resistance of the ego is not abolished in sleep but merely reduced. Some of it remains in the shape of a censorship of dreams and forbids the unconscious impulse to express itself in the forms which it would properly assume. In consequence of the severity of the censorship of dreams, the latent dream-thoughts are obliged to submit to being altered and softened so as to make the forbidden meaning of the dream unrecognizable. This is the explanation of dream-distortion, which accounts for the most striking characteristics of the manifest dream. We are therefore justified in asserting that a dream is the (disguised) fulfillment of a (repressed) wish. It will now be seen that dreams are constructed like a neurotic symptom: they are compromises between the demands of a repressed impulse and the resistance of a censoring force in the ego. Since they have a similar origin they are equally unintelligible and stand in equal need of interpretation.

There is no difficulty in discovering the general function of dreaming. It serves the purpose of fending off, by a kind of soothing action, external or internal stimuli which would tend to arouse the sleeper, and thus of securing sleep against interruption. External stimuli are fended off by being given a new interpretation and by being woven into some harmless dream situation; internal stimuli, caused by instinctual demands, are given free play by the sleeper and allowed to find satisfaction in the formation of dreams, so long as the latent dream-thoughts submit to the control of the censorship. But if they threaten to break free and the meaning of the dream becomes too plain, the sleeper cuts short the dream and wakes in a fright. (Dreams of this class are known as anxiety-dreams.) A similar failure in the function of dreaming occurs if an external stimulus becomes too strong to be fended off. (This is the class of arousal-dreams.) I have given the name of dream-work to the process which, with the co-operation of the censorship, converts the latent thoughts into the manifest content of the dream. It consists of a peculiar way of treating the preconscious material of thought, so that its component parts become condensed, its psychical emphasis becomes displaced, and the whole of it is translated into visual images or dramatized, and completed by a deceptive secondary revision. The dream-work is an excellent example of the processes occurring in the deeper, unconscious layers of the mind, which differ considerably from the familiar normal processes of thought. It also displays a number of archaic characteristics, such as the use of a symbolism (in this case of a predominantly sexual kind) which it has since also been possible to discover in other spheres of mental activity.

We have explained that the unconscious instinctual impulse of the dream connects itself with a residue of the day, with some interest of waking life which has not been disposed of; it thus gives the dream which it constructs a double value for the work of analysis. For on the one hand a dream that has been analyzed reveals itself as the fulfillment of a repressed wish; but on the other hand it may be a continuation of some preconscious activity of the day before and may contain every kind of subject-matter and give expression to an intention, a warning, a reflection, or once more to the fulfillment of a wish. Analysis exploits the dream in both directions, as a means of obtaining knowledge alike of the patient's conscious and of his unconscious processes. It also profits from the fact that dreams have access to the forgotten material of childhood, and so it happens that infantile amnesia is for the most part overcome in connection with the interpretation of dreams. In this respect dreams achieve a part of what was previously the task of hypnotism. On the other hand, I have never maintained the assertion which has so often been ascribed to me that dream-interpretation shows that all dreams have a sexual content or are derived from sexual motive forces. It is easy to see that hunger, thirst, or the need to excrete, can produce dreams of satisfaction just as well as any repressed sexual or egoistic impulse. The case of young children affords us a convenient test of the validity of our theory of dreams. In them the various psychical systems are not yet sharply divided and the repressions have not yet grown deep, so that we often come upon dreams which are nothing more than undisguised fulfillments of wishful impulses left over from waking life. Under the influence of imperative needs, adults may also produce dreams of this infantile type.

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In the same way that psychoanalysis makes use of dream interpretation, it also profits by the study of the numerous little slips and mistakes which people make--symptomatic actions, as they are called. I investigated this subject in a series of papers which were published for the first time in book form in 1904 under the title of *The Psychopathology of Everyday Life*. In this widely circulated work I have pointed out that these phenomena are not accidental, that they require more than physiological explanations, that they have a meaning and can be interpreted, and that one is justified in inferring from them the presence of restrained or repressed impulses and intentions. But what constitutes the enormous importance of dream interpretation, as well as of this latter study, is not the assistance they give to the work of analysis but another of their attributes. Previously psychoanalysis had only been concerned with solving pathological phenomena and in order to explain them it had often been driven into making assumptions whose comprehensiveness was out of all proportion to the importance of the actual material under consideration. But when it came to dreams, it was no longer dealing with a pathological symptom, but with a phenomenon of normal mental life which might occur in any healthy person. If dreams turned out to be constructed like symptoms, if their explanation required the same assumptions--the repression of impulses, substitutive formation, compromise-formation, the dividing of the conscious and the unconscious into various psychical systems--then psychoanalysis was no longer an auxiliary science in the field of psychopathology, it was rather the starting-point of a new and deeper science of the mind which would be equally indispensable for the understanding of the normal. Its postulates and findings could be carried over to other regions of mental happening; a path lay open to it that led far afield, into spheres of universal interest.

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I must interrupt my account of the internal growth of psychoanalysis and turn to its external history. What I have so far described of its discoveries has related for the most part to the results of my own work; but I have also filled in my story with material from later dates and have not distinguished between my own contributions and those of my pupils and followers.

For more than ten years after my separation from Breuer I had no followers. I was completely isolated. In Vienna I was shunned; abroad no notice was taken of me. My *Interpretation of Dreams*, published in 1900, was scarcely reviewed in the technical journals. In my paper 'On the History of the Psycho-Analytic Movement' I mentioned as an instance of the attitude adopted by psychiatric circles in Vienna a conversation with an assistant at the clinic (at which I lectured), who had written a book against my theories but had never read my *Interpretation of Dreams*. He had been told at the clinic that it was not worth while. The man in question, who has since become a professor, has gone so far as to repudiate my report of the conversation and to throw doubts in general upon the accuracy of my recollection. I can only say that I stand by every word of the account I then gave.

As soon as I realized the inevitable nature of what I had come up against, my sensitiveness greatly diminished. Moreover my isolation gradually came to an end. To begin with, a small circle of pupils gathered round me in Vienna; and then, after 1906, came the news that the psychiatrists at Zurich, Eugen Bleuler, his assistant C. G. Jung, and others, were taking a lively interest in psychoanalysis. We got into personal touch with one another, and at Easter 1908 the friends of the young science met at Salzburg, agreed upon the regular repetition of similar informal congresses and arranged for the publication of a journal which was edited by Jung and was given the title of *Jahrbuch fur psychoanalytische und psychopathologische Forschungen [Yearbook for Psychoanalytic and Psychopathological Researches]*. It was brought out under the direction of Bleuler and myself and ceased publication at the beginning of the First World War. At the same time that the Swiss psychiatrists joined the movement, interest in psychoanalysis began to be aroused all over Germany as well; it became the subject of a large number of written comments and of lively discussions at scientific congresses. But its reception was nowhere friendly or even benevolently non-committal. After the briefest acquaintance with psychoanalysis, German science was united in rejecting it.

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One of my opponents boasted of silencing his patients as soon as they began to talk of anything sexual and evidently thought that this technique gave him a right to judge the part played by sexuality in the aetiology of the neuroses. Apart from emotional resistances, which were so easily explicable by the psychoanalytic theory that it was impossible to be misled by them, it seemed to me that the main obstacle to agreement lay in the fact that my opponents regarded psychoanalysis as a product of my speculative imagination and were unwilling to believe in the long, patient and unbiased work which had gone to its making. Since in their opinion analysis had nothing to do with observation or experience, they believed that they themselves were justified in rejecting it without experience. Others again, who did not feel so strongly convinced of this, repeated in their resistance the classical maneuver of not looking through the microscope so as to avoid seeing what they had denied. It is remarkable, indeed, how incorrectly most people act when they are obliged to form a judgement of their own on some new subject. For years I have been told by 'benevolent' critics--and I hear the same thing even today--that psychoanalysis is right up to such-and-such a point but that there it begins to exaggerate and to generalize without justification. And I know that, though nothing is more difficult than to decide where such a point lies, these critics had been completely ignorant of the whole subject only a few weeks or days earlier.

The result of the official anathema against psychoanalysis was that the analysts began to come closer together. At the second Congress, held at Nuremberg in 1910, they formed themselves, on the proposal of Ferenczi, into an 'International Psycho-Analytical Association' divided into a number of local societies but under a common president. The Association survived the Great War and still exists, consisting today of branch societies in Austria, Germany, Hungary, Switzerland, Great Britain, Holland, Russia, and India, as well as two in the United States. I arranged that C. G. Jung should be appointed as the first president, which turned out later to have been a most unfortunate step. At the same time a second journal devoted to psychoanalysis was started, the *Zentralblatt fur Psychoanalyse [Central Journal for Psycho-Analysis*], edited by Adler and Stekel, and a little later a third, Imago, edited by two nonmedical analysts, H. Sachs and Otto Rank, and intended to deal with the application of analysis to the mental sciences. Soon afterwards Bleuler [1910] published a paper in defense of psychoanalysis. Though it was a relief to find honesty and straightforward logic for once taking part in the dispute, yet I could not feel completely satisfied by Bleuler's essay. He strove too eagerly after an appearance of impartiality; nor is it a matter of chance that it is to him that our science owes the valuable concept of ambivalence. In later papers Bleuler adopted such a critical attitude towards the theoretical structure of analysis and rejected or threw doubts upon such essential parts of it that I could not help asking myself in astonishment what could be left of it for him to admire. Yet not only has he subsequently uttered the strongest pleas in favor of 'depth psychology', but he based his comprehensive study of schizophrenia upon it. Nevertheless, Bleuler did not for long remain a member of the International Psycho-Analytical Association; he resigned from it as a result of misunderstandings with Jung, and the Burgholzli was lost to analysis.

In 1909 G. Stanley Hall invited Jung and me to America to go to Clark University, Worcester, Mass., of which he was President, and to spend a week giving lectures (in German) at the celebration of the twentieth anniversary of that body's foundation. Hall was justly esteemed as a psychologist and educationalist, and had introduced psychoanalysis into his courses several years earlier; there was a touch of the 'king-maker' about him, a pleasure in setting up authorities and in then deposing them. We also met James J. Putnam there, the Harvard neurologist, who in spite of his age was an enthusiastic supporter of psychoanalysis and threw the whole weight of a personality that was universally respected into the defense of the cultural value of analysis and the purity of its aims. He was an estimable man, in whom, as a reaction against a predisposition to obsessional neurosis, an ethical bias predominated; and the only thing in him that was disquieting was his inclination to attach psychoanalysis to a particular philosophical system and to make it the servant of moral aims. Another event of this time which made a lasting impression on me was a meeting with William James the philosopher. I shall never forget one little scene that occurred as we were on a walk together. He stopped suddenly, handed me a bag he was carrying and asked me to walk on, saying that he would catch me up as soon as he had got through an attack of angina pectoris which was just coming on. He died of that disease a year later; and I have always wished that I might be as fearless as he was in the face of approaching death.

At that time I was only fifty-three. I felt young and healthy, and my short visit to the new world encouraged my self-respect in every way. In Europe I felt as though I were despised; but over there I found myself received by the foremost men as an equal. As I stepped on to the platform at Worcester to deliver my Five Lectures on Psycho-Analysis [1910] it seemed like the realization of some incredible day-dream: psychoanalysis was no longer a product of delusion, it had become a valuable part of reality. It has not lost ground in America since our visit; it is extremely popular among the lay public and is recognized by a number of official psychiatrists as an important element in medical training. Unfortunately, however, it has suffered a great deal from being watered down. Moreover, many abuses which have no relation to it find a cover under its name, and there are few opportunities for any thorough training in technique or theory. In America, too, it has come in conflict with Behaviorism, a theory which is naive enough to boast that it has put the whole problem of psychology completely out of court.

In Europe during the years 1911-13 two secessionist movements from psychoanalysis took place, led by men who had previously played a considerable part in the young science, Alfred Adler and C. G. Jung. Both movements seemed most threatening and quickly obtained a large following. But their strength lay, not in their own content, but in the temptation which they offered of being freed from what were felt as the repellent findings of psychoanalysis even though its actual material was no longer rejected. Jung attempted to give to the facts of analysis a fresh interpretation of an abstract, impersonal and non-historical character, and thus hoped to escape the need for recognizing the importance of infantile sexuality and of the Oedipus complex as well as the necessity for any analysis of childhood. Adler seemed to depart still further from psychoanalysis; he entirely repudiated the importance of sexuality, traced back the formation both of character and of the neuroses solely to men's desire for power and to their need to compensate for their constitutional inferiorities, and threw all the psychological discoveries of psychoanalysis to the winds. But what he had rejected forced its way back into his closed system under other names; his 'masculine protest' is nothing else than repression unjustifiably sexualized. The criticism with which the two heretics were met was a mild one; I only insisted that both Adler and Jung should cease to describe their theories as 'psychoanalysis'. After a lapse of ten years it can be asserted that both of these attempts against psychoanalysis have blown over without doing any harm.

If a community is based on agreement upon a few cardinal points, it is obvious that people who have abandoned that common ground will cease to belong to it. Yet the secession of former pupils has often been brought up against me as a sign of my intolerance or has been regarded as evidence of some special fatality that hangs over me. It is a sufficient answer to point out that in contrast to those who have left me, like Jung, Adler, Stekel, and a few besides, there are a great number of men, like Abraham, Eitingon, Ferenczi, Rank, Jones, Brill, Sachs, Pfister, van Emden, Reik, and others, who have worked with me for some fifteen years in loyal collaboration and for the most part in uninterrupted friendship. I have only mentioned the oldest of my pupils, who have already made a distinguished name for themselves in the literature of psychoanalysis; if I have passed over others, that is not to be taken as a slight, and indeed among those who are young and have joined me lately talents are to be found on which great hopes may be set. But I think I can say in my defense that an intolerant man, dominated by an arrogant belief in his own infallibility, would never have been able to maintain his hold upon so large a number of intellectually eminent people, especially if he had at his command as few practical attractions as I had.

The World War, which broke up so many other organizations, could do nothing against our 'International'. The first meeting after the war took place in 1920, at The Hague, on neutral ground. It was moving to see how hospitably the Dutch welcomed the starving and impoverished subjects of the Central European states; and I believe this was the first occasion in a ruined world on which Englishmen and Germans sat at the same table for the friendly discussion of scientific interests. Both in Germany and in the countries of Western Europe the war had actually stimulated interest in psychoanalysis. The observation of war neuroses had at last opened the eyes of the medical profession to the importance of psychogenesis in neurotic disturbances, and some of our psychological conceptions, such as the 'gain from illness' and the 'flight into illness', quickly became popular. The last Congress before the German collapse, which was held at Budapest in 1918, was attended by official representatives of the allied governments of the Central European powers, and they agreed to the establishment of psychoanalytic centers for the treatment of war neuroses. But this point was never reached. Similarly too the comprehensive plans made by one of our leading members, Dr. Anton von Freund, for establishing in Budapest a center for analytic study and treatment came to grief as a result of the political upheavals that followed soon afterwards and of the premature death of their irreplaceable author. At a later date some of his ideas were put into execution by Max Eitingon, who in 1920 founded a psychoanalytical clinic in Berlin. During the brief period of Bolshevik rule in Hungary, Ferenczi was still able to carry on a successful course of instruction as the official representative of psychoanalysis at the University of Budapest. After the war our opponents were pleased to announce that events had produced a conclusive argument against the validity of the theses of analysis. The war neuroses, they said, had proved that sexual factors were unnecessary to the aetiology of neurotic disorders. But their triumph was frivolous and premature. For on the one hand no one had been able to carry out a thorough analysis of a case of war neurosis, so that in fact nothing whatever was known for certain as to their motivation and no conclusions could be drawn from this uncertainty; while on the other hand psychoanalysis had long before arrived at the concept of narcissism and of narcissistic neuroses, in which the subject's libido is attached to his own ego instead of to an object. Though on other occasions, therefore, the charge was brought against psychoanalysis of having made an unjustifiable extension of the concept of sexuality, yet, when it became convenient for controversial ends, this crime was forgotten and we were once more held down to the narrowest meaning of the word.

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I must begin by adding that increasing experience showed more and more plainly that the Oedipus complex was the nucleus of the neurosis. It was at once the climax of infantile sexual life and the point of junction from which all of its later developments proceeded. But if so, it was no longer possible to expect analysis to discover a factor that was specific in the aetiology of the neuroses. It must be true, as Jung expressed it so well in the early days when he was still an analyst, that neuroses have no peculiar content which belongs exclusively to them but that neurotics break down at the same difficulties that are successfully overcome by normal people. This discovery was very far from being a disappointment. It was in complete harmony with another one: that the depth-psychology revealed by psychoanalysis was in fact the psychology of the normal mind. Our path had been like that of chemistry: the great qualitative differences between substances were traced back to quantitative variations in the proportions in which the same elements were combined.

In the Oedipus complex the libido was seen to be attached to the image of the parental figures. But earlier there was a period in which there were no such objects. There followed from this fact the concept (of fundamental importance for the libido theory) of a state in which the subject's libido filled his own ego and had that for its object. This state could be called narcissism or self-love. A moment's reflection showed that this state never completely ceases. All through the subject's life his ego remains the great reservoir of his libido, from which object cathexes are sent out and into which the libido can stream back again from the objects. Thus narcissistic libido is constantly being transformed into object-libido, and vice versa. An excellent instance of the length to which this transformation can go is afforded by the state of being in love, whether in a sexual or sublimated manner, which goes so far as involving a sacrifice of the self. Whereas hitherto in considering the process of repression attention had only been paid to what was repressed, these ideas made it possible to form a correct estimate of the repressing forces too. It had been said that repression was set in action by the instincts of self-preservation operating in the ego (the 'ego-instincts') and that it was brought to bear upon the libidinal instincts. But since the instincts of self-preservation were now recognized as also being of a libidinal nature, as being narcissistic libido, the process of repression was seen to be a process occurring within the libido itself; narcissistic libido was opposed to object-libido, the interest of self-preservation was defending itself against the demands of object-love, and therefore against the demands of sexuality in the narrower sense as well.

There is no more urgent need in psychology than for a securely founded theory of the instincts on which it might then be possible to build further. Nothing of the sort exists, however, and psychoanalysis is driven to making tentative efforts towards some such theory. It began by drawing a contrast between the ego-instincts (the instinct of self-preservation, hunger) and the libidinal instincts (love), but later replaced it by a new contrast between narcissistic and object-libido. This was clearly not the last word on the subject; biological considerations seemed to make it impossible to remain content with assuming the existence of only a single class of instincts.

In the works of my later years: *Beyond the Pleasure Principle* [1920], *Group Psychology and the Analysis of the Ego* [1921], and *The Ego and the Id* [1923], I have given free rein to the inclination, which I kept down for so long, to speculation, and I have also contemplated a new solution of the problem of the instincts. I have combined the instincts for self-preservation and for the preservation of the species under the concept of Eros and have contrasted with it an instinct of death or destruction which works in silence. Instinct in general is regarded as a kind of elasticity of living things, an impulsion towards the restoration of a situation which once existed but was brought to an end by some external disturbance. This essentially conservative character of instincts is exemplified by the phenomena of the compulsion to repeat. The picture which life presents to us is the result of the concurrent and mutually opposing action of Eros and the death instinct.

It remains to be seen whether this construction will turn out to be serviceable. Although it arose from a desire to fix some of the most important theoretical ideas of psychoanalysis, it goes far beyond psychoanalysis. I have repeatedly heard it said contemptuously that it is impossible to take a science seriously whose most general concepts are as lacking in precision as those of libido and of instinct in psychoanalysis. But this reproach rests on a complete misconception of the facts. Clear basic concepts and sharply drawn definitions are only possible in the mental sciences in so far as the latter seek to fit a region of facts into the frame of a logical system. In the natural sciences, of which psychology is one, such clear-cut general concepts are superfluous and indeed impossible. Zoology and Botany did not start from correct and adequate definitions of an animal and a plant; to this very day biology has been unable to give any certain meaning to the concept of life. Physics itself, indeed, would never have made any advance if it had had to wait until its concepts of matter, force, gravitation, and so on, had reached the desirable degree of clarity and precision. The basic ideas or most general concepts in any of the disciplines of science are always left indeterminate at first and are only explained to begin with by reference to the realm of phenomena from which they were derived; it is only by means of a progressive analysis of the material of observation that they can be made clear and can find a significant and consistent meaning. I have always felt it as a gross injustice that people have refused to treat psychoanalysis like any other science. This refusal found an expression in the raising of the most obstinate objections. Psychoanalysis was constantly reproached for its incompleteness and insufficiencies; though it is plain that a science based upon observation has no alternative but to work out its findings piecemeal and to solve its problems step by step. Again, when I endeavored to obtain for the sexual function the recognition which had so long been withheld from it, psychoanalytic theory was branded as 'pan-sexualism'. And when I laid stress on the hitherto neglected importance of the part played by the accidental impressions of early youth, I was told that psychoanalysis was denying constitutional and hereditary factors--a thing which I had never dreamt of doing. It was a case of contradiction at any price and by any methods.

I had already made attempts at earlier stages of my work to arrive at some more general points of view on the basis of psychoanalytic observation. In a short essay, 'Formulations on the Two Principles of Mental Functioning' [1911], I drew attention (and there was, of course nothing original in this) to the domination of the pleasure-unpleasure principle in mental life and to its displacement by what is called the reality principle. Later on [in 1915] I made an attempt to produce a 'meta-psychology'. By this I meant a method of approach according to which every mental process is considered in relation to three coordinates, which I described as dynamic, topographical, and economic respectively; and this seemed to me to represent the furthest goal that psychology could attain. The attempt remained no more than a torso; after writing two or three papers--'Instincts and their Vicissitudes' [1915], 'Repression' [1915], 'The Unconscious' (1915], 'Mourning and Melancholia' [1917], etc.--I broke off, wisely perhaps, since the time for theoretical predications of this kind had not yet come. In my latest speculative works I have set about the task of dissecting our mental apparatus on the basis of the analytic view of pathological facts and have divided it into an ego, an id, and a super-ego. The super-ego is the heir of the Oedipus complex and represents the ethical standards of mankind.

I should not like to create an impression that during this last period of my work I have turned my back upon patient observation and have abandoned myself entirely to speculation. I have on the contrary always remained in the closest touch with the analytic material and have never ceased working at detailed points of clinical or technical importance. Even when I have moved away from observation, I have carefully avoided any contact with philosophy proper. This avoidance has been greatly facilitated by constitutional incapacity. I was always open to the ideas of G. T. Fechner and have followed that thinker upon many important points. The large extent to which psychoanalysis coincides with the philosophy of Schopenhauer--not only did he assert the dominance of the emotions and the supreme importance of sexuality, but he was even aware of the mechanism of repression--is not to be traced to my acquaintance with his teaching. I read Schopenhauer very late in my life. Nietzsche, another philosopher whose guesses and intuitions often agree in the most astonishing way with the laborious findings of psychoanalysis, was for a long time avoided by me on that very account; I was less concerned with the question of priority than with keeping my mind unembarrassed.