BULLETIN OF THE
AMERICAN MATHEMATICAL SOCIETY
Volume 79, Number 6, November 1973

BOOK REVIEWS

The mathematical career of Pierre de Fermat by M. S. Mahoney, Princeton University Press, Princeton, N.J., 1973, 419 + xviii pp., $20.00.

Nothing could be more welcome than a book on Fermat. This has been a desideratum for many years, and one wishes one could congratulate the author (an associate professor of the history of mathematics at Princeton University) and the Princeton University Press on the publication of this volume, which comes to us in a handsome jacket decorated with Fermat's engraved portrait from the *Varia Opera* of 1679. Fermat is one of the most fascinating mathematical personalities of all times, the creator (with Descartes) of analytic geometry, one of the founders of the calculus, the undisputed founder of modern number theory. The aura of mystery that still surrounds some of his best work provides an added attraction. Nor does such a project require extensive searches in many libraries. Fermat's complete writings and correspondence have been excellently published by Ch. Henry and P. Tannery in four splendid volumes (Gauthier-Villars, Paris, 1891–1912, with a supplementary volume, ibid., 1922); this includes authoritative French translations of all Latin texts, valuable commentaries, and virtually all relevant passages from the writings and correspondence of Fermat's contemporaries. If one adds to this two short pieces published by J. E. Hofmann in 1943 (Abh. d. Preuss. Akad. d. Wiss. **1943**, No. 9, one has Fermat's entire corpus. Of course one cannot easily separate Fermat from his great predecessors and contemporaries, Viète, Galileo, Descartes, Roberval, Torricelli, Schooten, Huyghens, Pascal, Wallis; fortunately, most of their work has been very well edited and is easily accessible.

Nevertheless, in order to write even a tolerably good book on Fermat, a modicum of abilities is required, and it is the reviewer's duty to consider whether the author appears to possess them. Such requisites are

(a) *Ordinary accuracy*. This is perhaps the primary virtue of the historian; unless he carefully checks all details, how can we trust him in his major conclusions? It may be accidental that the *Jahresbericht der deutschen Mathematiker-Vereinigung* is referred to as *Jahresbericht des deutschen Mathematiker-Vereins* (p. 147); as every mathematician knows, there has never been a "deutscher Mathematiker-Verein". But it can hardly be an accident when, in one of the introductory chapters, "Pell's equation" (so-called) is *twice* given as "$x^2 - py^2 = 1$ *for prime p*" (pp. 61, 63), whereas Fermat invariably specifies the equation to be $Ny^2 + 1 = x^2$ where N is *any* (positive) *nonsquare* integer. Perhaps that is why Mr. Mahoney's discussion of "Pell's equation" in Chapter VI (pp. 319–322 and again pp. 330–336) is so confusingly and hopelessly irrelevant. Or take the following statement:

> In 1658, Digby reports that Fermat . . . having presided over the trial of a defrocked priest that resulted in death by fire, was so shaken by the episode that he could not work for a time (p. 23).

Actually Digby, after complaining to Wallis about Fermat's tardiness in sending some mathematical information he had promised, goes on to say:

> I have had nothing from him but excuses . . . . It is true it came to him upon the nick of his removing his seat of Judicature from Castres to Tholose; where he is supreme Judge in the soveraign Court of Parliament. And since that, he hath been taken up with some Capital causes of great importance; in which in the end he hath given a famous and much applauded sentence for the burning of a Priest that had abused his function; which is but newly finished; and execution done accordingly. But this which might be an excuse to many other, is none to Mons. Fermat, who is incredibly quick and smart in any thing he taketh in hand.

Sir Kenelm Digby was a fantastic character, whose *Memoirs* read like a novel and quite possibly are one; an inquisitive historian might do worse than try to find out whether the above story was not a figment of his lively imagination (certainly Fermat was not "supreme judge" in Toulouse). But the picture he chooses to draw is not one of a "gentle, retiring man" (p. 22) in distress for having had to pass a capital sentence.

(b) *The ability to express simple ideas in plain English.* One begins to have doubts when one learns in the Introduction that "time and place define the two-dimensional matrix of history" (p. x). Look now at the chapter headings: I. *The personal touch*; II. *Nullum non problema solvere*; III. *The Royal Road*; IV. *Fashioning one's own luck*; V. *Archimedes and the theory of equations*; VI. *Between traditions.* Who would guess that they designate respectively Fermat's biography, a description of his scientific career, his analytic geometry, his differential calculus, his work on integration, and his number theory? Or take the 51 lines (pp. 84–85) devoted to a tiresomely obscure exposition of Fermat's perfectly lucid 8-line proof for the fact that an equation of the first degree in the plane defines a straight line (a fact that Descartes regarded as too elementary to deserve an explicit proof). According to Mr. Mahoney, that proof "illustrates Fermat's habit of abbreviating his proofs almost to the point of obscurity", but it is also "a classical example of a reductive analysis using Euclid's *Data*"[1] (p. 84). Equally obscure expositions of rather simple mathematical matters fill up many pages, sprinkled with an *ad hoc* vocabulary of questionable value ("biconditional", pp. 85, 319; "counterfactual", pp. 212–213, 190; "instantiation", pp. 99–102; "open-ended", pp. ix, xi, 71; "attitudinal", p. 142). Less serious no doubt, but hardly less jarring on the nerves of the unhappy reviewer, is the regular use of all the vulgarisms of modern jargon; a sharp transition is a "quantum jump" (pp. 345–347, 350); a piece of writing is not good or mediocre, it has to be "seminal" (p. 282) or "pedestrian" (p. xiii; why this gratuitous slur upon a fast vanishing species?); it is not enough that something should be clear, it must be "tragically clear" (p. 185); Fermat is not merely silent about a certain matter, "his silence is deafening" (p. 252), etc.

(c) *Some knowledge of French.* As to this, there is the famous remark made by Descartes to Schooten: "Monsieur Fermat est Gascon, moy non"; here "Gascon" refers of course to Fermat's native Gascogne, but it also carries the connotation of "a braggart" (no more, no less). There are indeed not a few passages in Fermat's letters to justify this reproof, but Fermat, who always spoke of Descartes (his senior by five years) with great respect, could have found ample reason to return the compliment. Unaccountably, Mr. Mahoney, on two separate occasions (pp. 15 and 59) and with particular emphasis, translates "Gascon" by "a rowdy".

(d) *Some knowledge of Latin.* It may be by pure negligence that Mr. Mahoney translates a passive (*implicabuntur*) by an active, a subjunctive (*quaerantur*, *designentur*) by an indicative (p. 212, lines 1–2), and interprets *in hac figura et similibus* (i.e. "in this figure, and others of a similar nature") as meaning that two paraboloids *CAV*, *BAR* are "similar solids" (p. 239), which they are not. One could quibble about his lengthy discussion (p. 265) on the word "*adaequarunt*" (which he translates "compare", when the very passages quoted in his footnote show that it means "show [two magnitudes] to be equal"); strangely enough, he sees there an "almost Freudian slip", because of the technical meaning which Fermat, in an altogether different context, has elsewhere attached to the same word. But on p. 78 he devotes nearly an entire page to his discovery of an ablative where the text has a dative,[2] thereby attributing some bad Latin to Fermat (who was famous for his elegant latinity) and proudly reading into his text a "stronger statement" than "previous translators" had noticed. The latter, he says, "have ignored the ablative *loco*". Suffice it to

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[1] The implicit reference is to *Data* 41, i.e. to no more than one of the classical "cases of similitude" for triangles.

[2] *Fit locus loco*; this is modelled e.g. on *Huic edicto locus est*, Paul. Dig. 37.10.6. Mr. Mahoney's error might perhaps be explained, though hardly excused, by a reference to Commandinus's poor latin; cf. P. Tannery's footnote, p. 6 of Fermat's *Oeuvres*, vol. I.

say that this reproof is addressed to no other than Paul Tannery, one of the best philologists of the turn of the century, who not only edited, but translated into Latin the whole of Diophantus.

(e) *Some historical sense.* Anyone who has taken any interest in the men of the XVIIth century knows how sensitive they were to class distinctions. It is therefore with no little surprise that one finds Mr. Mahoney conferring a peerage upon Fermat and Roberval, who appear as "Lord de Fermat" (p. 327), "Lord de Roberval" (p. 169). Actually these are his translations for the latin *Dominus de Fermat*, *Dominus de Roberval*, where of course *Dominus* is the normal latin translation for Monsieur. Sir Kenelm Digby is less lucky at his hands; when he appears in Latin as D. Digby (i.e. *Dominus Digby*), he loses both his knighthood and his initial to become, in Mr. Mahoney's English, plain D. Digby (p. 327).

(f) *Some familiarity with the work of Fermat's contemporaries and of his successors.* In fact, this is essential for at least two reasons. On the one hand, the gradual development of differential and integral calculus in the XVIIth century is very largely the product of a collective effort; however outstanding Fermat may have been, his work cannot be separated from that of Galileo, Cavalieri, Neper, Roberval, Descartes, Debeaune, Torricelli, Pascal, Heurat.

On the other hand, while Fermat was far ahead of the few who were also interested in number theory during his lifetime, and owed nothing to them, most of the work of Euler in that field may be regarded as an inspired commentary on the work of Fermat; in fact it is exceedingly plausible that many of the proofs devised by Euler for Fermat's statements were not substantially different from those which Fermat said he possessed.

Mr. Mahoney gives no sign of having read Euler. He cannot have read Neper, since he professes not to have understood Fermat's very interesting reference to logarithms (quoted p. 247, footnote 47). He does not seem to have read Cavalieri, whose deep influence on Fermat's late work on integration is obvious, but nowhere mentioned. Worse still, he has not read Pascal; had he done so,[3] he would not twice have called "fully unprecedented" (p. 257) the important theorem which for Fermat replaced both our integration by parts and our change of variables. He would have known that this is identical with propositions I–IV in Pascal's *Traité des trilignes* and is contained as a special case in the "general lemma" which opens that beautiful piece, published early in 1659. According to Mr. Mahoney, "1658 or 1659 seems most likely" (p. 244) as the date of Fermat's memoir; therefore he will not contradict us if we

[3] Or had he read N. Bourbaki's *Eléments d'histoire des mathématiques*, where those passages of Fermat and Pascal are discussed together (p. 199). This is not the only case where that book could have given Mr. Mahoney some useful hints.

assume, with N. Bourbaki, that the date is 1659. As Pascal gives (in 31 lines) a complete and beautiful proof of his "general lemma", one need not then ask why Fermat did not find it necessary to give a proof himself (even though one may suppose that he had discovered the theorem independently and found a proof for it, perhaps not substantially different from Pascal's); nor need one give, as Mr. Mahoney does clumsily in 59 lines (pp. 258–260), a thoroughly implausible "conjectural reconstruction" (for which, as he rightly says, there is "not a shred of evidence") of Fermat's proof.

But perhaps Mr. Mahoney has read Goldbach, or so he says; for, on the very last page of his chapter on Fermat's number theory (p. 348, footnote 141), he praises Goldbach's proof (*Demonstratio theorematis Fermatiani, Acta erud.* 1724) for Fermat's assertion that no triangular number except 1 can be a fourth power. "The proof", says Mr. Mahoney, "does not rely on infinite descent and is elegant in its simplicity"; that this is not a casual slip is shown by Mr. Mahoney's article GOLDBACH in the *Dictionary of scientific biography*, vol. V, where he has it that "Goldbach could be provocative on a fundamental level, as his articles *Demonstratio* . . . and *Criteria quaedam* . . . show". The theorem is not an easy one, and Euler gave a proof for it (by infinite descent, of course) in 1738; as to Goldbach's "proof", it is very brief and consists of a trivial and obvious blunder, as Goldbach cheerfully acknowledged in his letter to Euler of 9 October 1730.

(g) "Knowledge and sensitivity to mathematics", says Mr. Mahoney (p. x), "constitutes the biographer's most important tool for understanding Fermat", and one cannot but heartily agree. Take for instance Fermat's famous proof for the fact that the Diophantine equation $xy(x^2 - y^2) = z^2$ (or, equivalently, the equation $Z^2 = X^4 - Y^4$) has no solution; it is described briefly but lucidly in Obs. XLV on Diophantus (*Oeuvres*, vol. I, p. 340); the few missing details have been supplied, among others, by H. G. Zeuthen (*Gesch. d. Math. im* 16. *und* 17. *Jahrh.*, p. 163) and T. L. Heath (*Diophantus*, 2nd ed., p. 294). The proof makes use of the fact that, if $x^2 = y^2 + 2z^2$ in mutually prime integers, x must be of the form $p^2 + 2q^2$ "as we can prove very easily", Fermat says. On the latter assertion, Mr. Mahoney has this comment: "Fermat left no demonstration of this theorem, easy or otherwise, and one may seriously doubt that more recent proofs bear any relation to what he had in mind; they, like the theory of quadratic forms to which they belong, rely ultimately on the complex number field. That is, any rigorous demonstration of Fermat's assertion requires the use of numbers of the form $a + b\sqrt{-2}$, which lay totally beyond the conceptual realm of Fermat's mathematics", and further on: "As in the case of numbers of the form $p^2 + q^2$, he was relying on his algebraic intuition" (p. 346). Let us leave aside, for the moment, the fact that, beyond any reasonable doubt, one must credit Fermat with having possessed proofs (similar to those later given by Euler) for the representation of integers by the quadratic forms $x^2 + y^2$, $x^2 + 3y^2$, $x^2 \pm 2y^2$. As to the result quoted above, however, the proof is as follows. We have $2z^2 = (x + y)(x - y)$; as $x \pm y$ must be even, put $x + y = 2u$, $x - y = 2v$; then $z^2 = 2uv$; as u, v must be mutually prime, this gives either $u = p^2$, $v = 2q^2$ or $u = 2q^2$, $v = p^2$, and in either case $x = p^2 + 2q^2$. Not only is this to be found in Zeuthen and in Heath; it is a rather easy exercise for a first course in elementary number theory; to anyone who has had any experience with Fermat, or even with Diophantus, such proofs are a dime a dozen.

Take now the problem of representing an integer as $x^2 + y^2$. Until 1638, Fermat's correspondence shows him up as the rawest novice in number theory. Not only does Frenicle, as late as 1640, express himself quite contemptuously about him; but Fermat himself, in 1636, seems quite proud of having found that an integer N of the form $8n - 1$ cannot be a sum of four squares, even in fractions. His similar observation that N is not a sum of two squares if it is of the form $4n - 1$ must go back to the same period; both are discussed in Descartes's correspondence with Mersenne in March and May, 1638. Descartes rightly regards the question as trivial, proves part of it by writing congruences modulo 8, and then turns it over to a young man in his employment, Jean Gillot, who completes the solution, also by using congruences modulo 4 and modulo 8. In 1640, on resuming (after an interruption of three years) his correspondence with Roberval, Fermat reminds the latter of his old observation on numbers $4n - 1$, but then goes on to state a theorem of an entirely different type (a "quantum jump" if ever there was one, as Mr. Mahoney might have said): if $N = r^2s$ with s squarefree, and s has a prime factor of the form $4n - 1$, then N is not a sum of two squares; in other words, if the odd prime p divides $a^2 + b^2$ with a, b mutually prime, then it has the form $4n + 1$.

Incidentally, this would seem to dispose of the purple passage which concludes the book: "The core of Fermat's creative achievement is located in the period 1629–1636; that is the period in which he laid the foundation of his analytic geometry ... and his work on number-theory ... It was the creative product of a young mind .... In that, too, Fermat joins the ranks of the greatest mathematicians in history" (pp. 354–355). But let us go back to the point at issue. A full page (pp. 316–317) is devoted to the description of a "fairly direct proof, in line with Fermat's habits of thought" for "Fermat's central theorem concerning numbers of the form $4n - 1$", firstly (in 10 lines) for the fact that these are not sums of two squares in integers, and then (in 16 lines) for the fact that they are not so in fractions. "Not only", says Mr. Mahoney, "does the above proof employ concepts fully at Fermat's command [and, we may add, at young Jean Gillot's command; actually Gillot's proof is rather more lucid than Mr. Mahoney's], but it has a feature . . . that lends it weight as a possible reconstruction of Fermat's own proof" (p. 317). This is mere verbiage; but there follow two pages of utter confusion, where the little sense that emerges is either tautological or palpably false (see p. 318, lines 15–18).

How is one to review such a book? Merely to set things right would require another volume. We will try to be brief.

The first two chapters are largely biographical. The first one, after attempting to describe the general mathematical scene in the XVIth and the early XVIIth century, gives the basic facts about Fermat's life and his career as a magistrate; after the painstaking investigations of Ch. Henry and P. Tannery, it was not to be expected that any new light could be shed upon it, and none is. Chapter II, the least unsatisfactory in the book, begins by going into some detail about Viète's algebra, rightly emphasizing its influence on Fermat, but wrongly belittling (pp. 26–27) that of Archimedes, which appears to have been no less profound. Then it proceeds to a description of Fermat's mathematical career, punctuated as it was, not by publications (he had almost none) but by his scientific correspondence and by his controversies with Descartes and Wallis.

Chapter III deals with Fermat's analytic geometry. We have first an exposition of his central work on the subject, the *Isagoge* or "Introduction"; then this is traced back to its sources, Apollonius, Pappus and Fermat's own restitution of Apollonius's *Plane loci*; then we are asked to proceed to Fermat's work after the *Isagoge*. As to the *Loci*, they contain two particularly interesting proofs, those of Proposition I.7 and of Proposition II.5. Tannery conjectured that the former was the one which Fermat once said had eluded him for several years; Mr. Mahoney argues that it was the latter, which he discusses at great length; he only fails to notice that it amounts to finding the coordinates of the center of gravity of a finite number of unit masses in the plane and might well be related to Fermat's early interest in "geostatics". On the other hand, he does not discuss Proposition I.7, which is doubly interesting, firstly because it is perhaps the earliest example of a proof clearly worked out by induction from n to $n + 1$ (a point raised by Mr. Mahoney in footnote 52, p. 102, without any mention of Proposition I.7), and also because in substance it already contains the essential fact of the linearity of changes of coordinates in the plane. Because of the clumsy organization of this chapter, the *Isagoge* is thus separated from Fermat's later work, which shows his increasing awareness of the basic principles underlying the *Isagoge*. One such principle is what we call the invariance of the dimension; Fermat came to realize that the solution of every problem depends upon a certain number $n \geqq 0$ of arbitrary parameters, so that problems must be classified accordingly. The second principle is that of the invariance of the degree of a plane curve with respect to what we would call an arbitrary change of affine coordinates. These principles, together with the discovery (made also by Descartes) that curves of degree 1 and 2 are, respectively, the straight lines and the conics, make up Fermat's main innovations in this topic; neither one is clearly brought out by Mr. Mahoney; the word "degree" does not even occur in his index.

If Chapter III appears confusing, what is one to say of Chapters IV and V on Fermat's differential and integral calculus? "The confusion in Fermat's papers . . . makes the narrative argument of this chapter a complex one . . . . Hence the argument turns back on itself" (p. 146). It does indeed, with a vengeance. In order to achieve some elementary clarity, we must begin by indicating our own reading of this very interesting story.

When Fermat began, in or about 1629, very little was known about tangents and about maxima and minima. A tangent to a conic was a line which had just one point in common with it. On the other hand, Archimedes had given a brilliant determination of the tangent to a transcendental curve, the spiral, with an "apagogic" proof (i.e. a proof by reduction *ad absurdum* in the typical Archimedean style). About integration, much was known from Archimedes, who gave "apagogic" proofs for all his theorems, and much work was being done in Italy but did not become known to Fermat until a later date. Every competent mathematician of that time, having studied Archimedes, was expected to be able to construct a rigorous, i.e. Archimedean or "apagogic", proof in each specific case; but this always required the previous knowledge of the result for that case. Thus, what Fermat and others were after was "a method"[4] for obtaining such results; once the result was found, the rest was routine, as they never tired of repeating.

In the search for "a method" for tangents, one could be guided by the case of conics; this led to the purely algebraic problem of determining a parameter so that an algebraic equation acquires a double root. This method, which (in modern terms) belongs to algebraic geometry rather than differential calculus, was the one adopted by Descartes; of course, when he was challenged to find the tangent to the cycloid, he found himself in a quandary and got out of it by being illogical, improvising a beautiful kinematic method and inventing the instantaneous center of

[4] The Greek word *methodos* is explained by G. Budé and H. Estienne as "a way to be followed in pursuit of" something.

rotation. Fermat, led by a surer instinct, developed a method which slowly but surely brought him very close to modern infinitesimal concepts. What he did was to write congruences between functions of x modulo suitable powers of $x - x_0$; for such congruences, he introduces the technical term *adaequalitas*, *adaequare*, etc., which he says he has borrowed from Diophantus. As Diophantus V.11 shows, it means an approximate equality, and this is indeed how Fermat explains the word in one of his later writings.[5]

At first Fermat applies the method only to polynomials, in which case it is of course purely algebraic; later he extends it to increasingly general problems, including the cycloid. A similar development occurs in Fermat's no less outstanding work on integration, where the word "adequality" is again used more and more frequently to denote an increasingly conscious passage to the limit.[6] We will not attempt to describe the verbiage of Mr. Mahoney's two chapters on the whole subject, nor to enumerate his sins of omission or commission. About one point we should warn the reader; according to Mr. Mahoney, "the modern notation [for the integral] suggests the inverse relationship of differentiation and integration and for that reason presents a real danger of anachronism . . . . It will help to borrow from Leibniz . . . . We will write $\mathrm{Omn}_d\, y$ for "all y applied to the segment d" or $\mathrm{Omn}_x\, y$ when the axis of reference is defined, but not the segment" (p. 258). In other words, he writes $\mathrm{Omn}_d\, x$ for the modern $\int_d y\, dx$, where d is an interval on the x-axis, and $\mathrm{Omn}_x\, y$ for $\int y\, dx$; thus the latter denotes the *indefinite* integral (a Leibnizian concept, utterly foreign to Fermat's way of thinking), and implies precisely the anachronism in question. Worse still, Leibniz never wrote $\mathrm{Omn}_d\, y$ or $\mathrm{Omn}_x\, y$; for a brief period, he wrote $\mathrm{Omn}\, y$, and soon switched, first to $\int y$, then to $\int y\, dx$; and why should this be more anachronistic than $\mathrm{Omn}_x\, y$?

But let us examine Mr. Mahoney's conclusions in those chapters: "Almost every secondary account of it [i.e. of Fermat's work] sees in it some form of infinitesimal calculus . . . . By contrast, the *Analytic investigation* [i.e. the *Methodus de maxima et minima*, which Mr. Mahoney, rightly or wrongly, regards as describing Fermat's first version of his "method"] shows clearly that it contains no such thing, that it rests on purely finite algebraic concepts . . . . The two later versions, too, are devoid of infinitesimal considerations . . . " (p. 146); and later, when the evidence becomes overwhelming: "Those techniques, but adequality in particular, had exercised a dynamic of their own . . . . Adequation,

[5] *Adaequetur, ut ait Diophantus, aut fere aequetur*; in Mr. Mahoney's translation: "adequal, or almost equal" (p. 246).

[6] As to all this, cf. also N. Bourbaki, op. cit., pp. 182–200.

limit-sums . . . at all these concepts Archimedes would have shuddered. That Fermat could not or did not see this fact, that he did not fully appreciate the almost revolutionary character of his own advances, serves to show, etc." (pp. 263–264); "Hence, one cannot say with any degree of fairness or objectivity that Fermat's work in analysis of curves was even heading in the direction of the calculus. For it was not pointed toward the concept that underlies the calculus and its fundamental theorem . . . . Descartes was probably quite right when he said that Fermat was a brilliant problem-solver but basically inept at conceiving systematic questions" (p. 279; lest the reader be deceived, he should be warned that this is not translated from Descartes; it is a paraphrase in Mr. Mahoney's own style). Of course, if one still identifies "the calculus" with its so-called "fundamental theorem" (i.e. the inverse relationship between differentiation and integration), Mr. Mahoney is right to say that Fermat "did not invent the calculus" (p. 279). One wonders what may be the point of such a statement.

All this is nothing, alas, to what meets us in Chapter VI on Fermat's number theory. Mr. Mahoney calls it "a riddle wrapped in a mystery inside an enigma" (p. 282). It is such indeed, in his account of it; but it need not be so, especially if one seeks Euler's help. Take for instance Fermat's theorem $a^{p-1} \equiv 1$ modulo p, for any prime p and for a prime to p. Two proofs are known for this, a multiplicative one based on the fact that a generates a subgroup of the multiplicative group of the prime field modulo p, and an additive one, based on the binomial theorem and the fact that $(a + b)^p \equiv a^p + b^p$ modulo p. The latter proof occurs in Leibniz's unpublished manuscripts; Euler discovered it in 1736. He found the other proof somewhat later, and thought it the better one of the two. Looking at Fermat's formulation of the theorem, it is hardly possible to doubt that this was the proof he had in mind, of course not in group-theoretical language, but in the form in which Euler expressed it (and which became an essential step for the later development of the theory of finite groups). Not only does Mr. Mahoney state views to the contrary (without any evidence), but he makes a distinction between the "modern" form of the theorem, viz., $a^p \equiv a$ mod p for all a, and its "original" form $a^{p-1} \equiv 1$ mod p for a prime to p, which no mathematician would care to make.

The rest of the chapter is a hopeless jumble, where no classification of Fermat's results is even attempted, although this is both easy and illuminating. Lacking experience and models, Fermat began by studying all kinds of problems with little regard for their possible theoretical value. Thus, in his early career, he paid much attention to questions connected with the function $s(n)$, the sum of the divisors of n other than n; thus the solutions of $s(n) = n$ are the so-called perfect numbers, the solutions of $s(n) = n'$, $s(n') = n$ are the "amicable" numbers, etc. Although he long retained a fondness for such questions, it is clear that he soon realized their peripheral character. Fairly early, too, he considered problems about the representation of integers by quadratic forms $x^2 \pm Ny^2$, on which he focussed his attention more and more as time went on. It is idle to doubt that he eventually developed a complete theory for the forms $x^2 + y^2$, $x^2 + 3y^2$, $x^2 \pm 2y^2$, with proofs by infinite descent which cannot have been very different from those later found by Euler. The same can plausibly be said of the representation of integers by sums of four squares; on the other hand, no suggestion can be offered at present as to how Fermat could possibly have proved that every integer is a sum of three triangular numbers, and one cannot help thinking that on this point he may have deceived himself. As to "Pell's equation" $x^2 - Ny^2 = 1$ (where N is any nonsquare positive integer), there is every reason to think that Fermat's method was in substance much the same as Lord Brouncker's, as described by Wallis in the *Commercium epistolicum*; one also may well assume that he had found the way to add to this a rigorous proof of existence, since he criticizes Wallis on that point; actually it only takes the addition of one or two elementary lemmas to do this.

Finally, it is also striking (but nowhere noticed by Mr. Mahoney) that the greater part of Fermat's work on diophantine equations concerns curves of genus 1 (given by one equation in 2 unknowns, or 2 equations in 3 unknowns), and that his "method" in dealing with such curves (a method whose germ he found in Diophantus) consists simply in the duplication of the elliptic argument, expressed of course in purely algebraic terms. This includes the equation $z^2 = x^4 \pm 1$ in rational numbers (or, what amounts to the same, $z^2 = x^4 \pm y^4$ in integers), for which we have already said that Fermat's proof is available and quite complete, Mr. Mahoney notwithstanding. It also includes $z^3 = x^3 \pm 1$ in rationals (or $z^3 = x^3 \pm y^3$ in integers), for which Euler found a proof by infinite descent; it is true that the only proof published by Euler rests on the field $Q(\sqrt{-3})$ and an unproved assumption concerning that field, but we know from his correspondence that he had earlier obtained a complete proof based on the theory of the quadratic form $x^2 + 3y^2$, and there is no reason to doubt that Fermat had also found it. But what is one to say to Mr. Mahoney's final assertion in that chapter, that Kummer, while trying to prove Fermat's last theorem, "devised a complete theory of the complex number field" (p. 348)?

To all this is added an appendix, "Sidelights", where Fermat's work on mechanics, optics, probabilities is rather perfunctorily discussed. For lack of competence, I shall leave it aside, merely noting that some experts in modern probability theory[7] have found in Fermat's and Pascal's correspondence the germ of some important modern methods and concepts; this can be contrasted with Mr. Mahoney's statement: "Probability is not physics, and one may well wonder why Fermat did so little with the subject" (p. 358). But something must be said of the general characterization given by Mr. Mahoney of Fermat's mathematical personality.

He never tires of repeating that Fermat was a "problem-solver" (pp. 25, 91, 204, 205, 253, 278, 279, 306, 328, 341), that "proofs were not his forte" (p. 203; cf. pp. 25, 47–48, 242, 341, 347); "he could not be bothered by detailed demonstrations of theorems his superb mathematical intuition told him were true" (p. 347). At the same time, it is not easy to see what he means by "a problem-solver", since he once speaks of "Fermat's concern for general method as well as for solving particular problems" (p. 76), which is the very opposite of what mathematicians usually mean by a problem-solver, and which contradicts the passage quoted above: "Descartes was right to say that Fermat was a brilliant problem-solver but inept at conceiving systematic questions" (p. 279). Actually Fermat's weakness lay in the extreme difficulty he always experienced at writing up his discoveries. This is why he once tried to persuade Pascal to help him to do so, at least for his number theory, and one wishes Pascal had been more responsive. But no man can be a good mathematician, let alone a great one, if he cannot make the difference between a theorem and a conjecture, between intuition and proof. And Fermat was no more and no less of a problem-solver than any great mathematician in history. As Hilbert said, it is by solving problems that mathematics keeps alive.

Has then this book no redeeming feature at all? As we have noted, it does contain a lively account (Chapter II, §II, pp. 48–65) of Fermat's scientific career, of his position among his contemporaries as a scientist and of the human aspects of his controversies with Descartes and Wallis. That section can be read with profit by any one who is not already conversant with the scientific personalities of that period. Apart from that, a student of XVIIth century mathematics will find little in that volume that could be helpful to him, and much that can only confuse and mislead him.

Unfortunately, a book on such a subject, published with the imprint of the Princeton University Press, tends to pre-empt the field. Surely Fermat deserved a better treatment. Let us hope he still gets it.

A. WEIL

[7] E.g. P. Cartier (personal communication).