Francia. Forschungen zur westeuropäischen Geschichte
Herausgegeben vom Deutschen Historischen Institut Paris
(Institut historique allemand)
Band 4 (1976)

DOI: 10.11588/fr.1976.0.48658

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FERMAT REVIVIFIED, EXPLAINED, AND REGAINED*

Pierre de Fermat, born at Beaumont-de-Lomagne in 1601 and deceased at Castres in 1665, was one of the great mathematical geniuses living in »the century of genius.« Self-taught in mathematics, a Bachelor of Civil Laws from the University of Orléans at some unknown date before 1631 (Mahoney is apparently mistaken, according to Jean Itard, in thinking that 1631 was the date), *conseiller de parlement* at Toulouse (where he failed to distinguish himself as a particularly outstanding lawyer), a gentle man of some modest fame as a classical scholar, who dabbled in Latin poetry, knew Greek, Italian, und Spanish (but not English), Fermat had very little to learn as a mathematician from his contemporaries (with the exception of Descartes). Among his immediate predecessors, his only consequential teacher seems to have been François Viète (1540–1603). His ancient masters included Euclid, Apollonius, Archimedes, Diophantus and Pappus, from whom he absorbed the Greek synthetic geometrical methods and the rhetorical algebraic Diophantine approach. In the case of Euclid and Apollonius, Fermat's endeavours incorporate attempts at reconstruction of lost treatises, the »Porisms« (»Porismatum Euclideorum renovata doctrina et sub forma Isagoges recentioribus Geometris exhibita«, written in 1655 or 1656) and the »Plane Loci« (»Apollonii Pergaei libri duo de locis planis restituti«, written before 1636).

Fermat was called the »Prince of Amateurs« by E. T. Bell in his »Men of Mathematics« (New York, 1937) and yet left out of consideration by J. L. Coolidge in his »The Mathematics of Great Amateurs« (Oxford, 1949) precisely because of his (Fermat's) greatness and professionalism! Both assessments, though superficially inconsistent, are to the point as will become clear in what follows.

Fermat's growth as a mathematician began in the late 1620's during a stay in Bordeaux. It was there that he read Viète who supplied him with the new protosymbolic algebra, the analytic art, meant to unify mathematics (arithmetic and geometry) and to place this ancient discipline on new, analytical foundations. Like the other creators of modern mathematics (first and foremost Viète himself and René Descartes), Fermat thought he was able to find the roots of the »analytic art« in the works of ancient Greek mathematicians, primarily the so-called *topos analyomenos* expounded in Book VII of Pappus's »Mathematical Collection«. And so, typically enough, Fermat's work as a mathematician starts with attempts at reconstructing some of the lost Greek »analytical« works mentioned and discussed by Pappus in his »Treasury of Analysis« in Book VII of the »Collection«. As stated above, Fermat's efforts resulted in the reconstructions of Apollonius's »Plane Loci« and Euclid's »Porisms«. Another ancient Greek work that drew Fermat's long-standing interest was the extant »Arithmetica«

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* Zugleich Besprechung von: Michael Sean Mahoney, The Mathematical Career of Pierre de Fermat (1601–1665), Princeton, New Jersey (Princeton University Press) 1973, XVIII–419 pp.

of Diophantus, which presented Fermat with problems on which his talents of generalization and formulation of new and more general problems to be solved by means of the Vietan theory of equations exercised themselves for lengthy periods of time. In his approach to Greek mathematical works, Fermat quite often redefined the problems that preoccupied the Greeks, generalized them, provided them with general solutions, and by adroit and deft reformulation took the Greek problems away from their indigenous territory into new and foreign lands. Interestingly (and again, I think, quite typically), Fermat did not see in his novel and revolutionary methods strategies intrinsically alien to Greek mathematics, thus contributing to the creation of the pervading and pernicious myth that there are not indeed any substantive differences between the geometrical works of the Greeks and the algebraic treatment of Greek mathematics by post-Vietan mathematicians.

Fermat was basically a very healthy man. If there was, however, any one disease which affected him chronically throughout his life this must have been his *graphophobia*, as obvious in his seemingly quasi-pathological fear of his own printed word. Most of his contributions to mathematics appeared in letters written to various correspondents (Mersenne, Roberval, Carcarvi, Pascal, etc.), in papers sent to friends and others, and as marginalia in books he owned in his personal library (e. g. the »Observations on Diophantus«).

With Descartes, and independently of him, Fermat was the inventor of analytic geometry. After his restoration of the »Plane Loci«, Fermat produced the small treatise entitled »Ad locos planos et solidos isagoge«, in which the insight he gained in the former, namely that Apollonius's loci could be expressed in the form of indeterminate equations in two unknowns, came to full fruition. Fermat realized that it was possible to study all the properties of the locus by means of a study of the equation representing it algebraically. Furthermore, Fermat was able to reduce Apollonius's *symptomata* to indeterminate equations in two unknowns and to discover a standard means of relating the geometric locus to its representative equation by means of an idiosyncratic system of reference which correlated the two unknowns. As Mahoney has pointed out, this system is not a genuine system of coordiantes, but rather an »uniaxial system« corresponding to a single, fixed reference line on which ordinates of various lengths are erected at equal angles (usually right), such that the extremities of these ordinates lie all on the given locus (pp. 81–83). Moreover, Fermat managed to show that any indeterminate equation in two unknowns of first or second degree represents either a plane or solid locus given (or fixed) in place.

In all this work, Fermat took Viète's theory of equations for granted and toiled faithfully within the framework provided by it. He never quite secceeded in extending the approach of the »Isagoge« to the three-dimensional situation, though he tried to do just that in the »Isagoge ad locos ad superficiem«. He did succeed, however, in establishing the connection between the number of unknowns of an equation and the spacial construct of that equation, by pointing out that equations in one unknown lead to point constructions, in two unknowns to locus constructions of plane curves, and in three unknowns to locus constructions of surfaces in space. He did this in his »Novus secundarum et ulterioris ordinis radicum in analyticis usus« of 1650.

Another outcome of Fermat's use of Viète's theory of equations is the method of maxima and minima. By applying Viète's concept of *syncrisis,* Fermat was able to come up with a general method for the determination of extreme local values of polynominal expressions. The essence of Fermat's algorithm (in which we can see the rule for the determination of the first derivative of such expressions) consisted in assuming first the inequality of the roots of the polynomial and in establishing a relationship between the roots and a coefficient. Such a relationship having been established, Fermat »adequated« the roots of the polynomial in order to establish the extreme value. The procedure found by Fermat was fully general and did not appeal to the concept of limit.

Later, Fermat improved his treatment of maxima and minima by making use of Descartes' more sophisticated theory of equations which appeared in »La Géométrie« (1637). Still, due to inherent weaknesses built in in the basically geometrical nature of the problems he treated, Fermat failed to obtain more than a unique extreme value and he was unaware of the difference between relative (local) and absolute (global) extreme values.

An outgrowth of the method of maxima and minima was Fermat's method of tangents (another was the determination of centers of gravity of geometrical shapes). Concerning the former, Fermat determined the tangent to a curve given by its equation by computing the length of the corresponding subtangent. This computation involved once more the concept of »adequality« (taken over from Diophantus), according to which, in this case, the two ordinates of (1) an arbitrary point on the tangent other than the point of tangency and (2) the corresponding point on the curve determined by the intersection of that ordinate and the curve were assumed to be »adequal«. Reasoning afterwards from similar triangles, Fermat eliminated the »adequality« by taking the difference between the two abscissae (of the point of tangency and of the other arbitrary point on the tangent) to be equal to zero. This method enjoys the same generality as the method of maxima and minima from which it is derived, and both methods were made public due to Mersenne's and Hérigone's endeavours.

Another area in which Fermat made great strides was that of methods of quadrature. Initially inspired by Archimedes' treatment in »On Spirals«, Fermat later improved and generalized his method of quadrature by means of a new meaning attached to the concept of »adequality«, now taken to stand for »approximate« or »limiting« equality. Fermat was thus able to square not only curves of the form (in modern notation) $y^n = kx$, for any integer n, but also all curves of the form $y^m = kx^n$ and $x^n y^m = k$, for any integers m, n (in the second equation $m+n>2$).

It should be pointed out, as Mahoney has beautifully and convincingly shown (cf. pp. 278–79), that Fermat's methods of quadrature and tangents lacked some crucial attributes to make them immediately conductive to the development of the calculus. Thus, Fermat was unaware of the inverse relationship between the determination of the tangent to a given curve and the quadrature of that curve, and his algorithms and concepts in both cases did not involve the clearcut recognition of the fact that tangent and area were both *functions* of the given curve.

Another application of his method of quadrature led Fermat to the solution of the problem of rectification of curves in his only treatise published during his lifetime (in 1660), the »De linearum curvarum cum lineis rectis comparatione dissertatione geometrica« (cf. pp. 264–78). The treatise is couched in the language of Greek synthetic geometry, which obscures its underlying algebraic foundation. Fermat's approach consists in dividing the axis of the given curve (the ordinate) into equal intervals, the drawing of abscissae through the points of division, which abscissae cut off segments on the tangents drawn at the points of intersection of the abscissae and the given curve. The length of the curve is taken to be the limit of the sum of these tangential segments as the length of the ordinate intervals goes to zero.

Fermat's work in the areas discussed above affected only in a very limited manner the work of his contemporaries, primarily because of Fermat's selfimposed isolation and his reluctance to publish his findings. The same is true to an even greater extent about Fermat's work in number theory which, although containing fundamental breakthroughs, remained largely unknown until at least the times of Euler and Gauss. Fermat's great achievements in number theory are indeed very impressive; and because of our almost total ignorance of Fermat's methods in reaching his results, there is an enigmatic aura of mystery and concealment surrounding Fermat's unproved theorems, undemonstrated conjectures, and the many specific examples of number-theoretical truths filling his numerous letters and the margins of his copy of Bachet's Diophantus. The only method in the realm of number theory – which, in a very real sense, was Fermat's brainchild – that Fermat was willing to unveil partially was the method of infinite descent.

Fermat limited what has traditionally been called *Diophantine analysis* to integral solutions, and he focused his endeavours in number theory primarily on the area of prime numbers, divisibility, and the securing of general families of solutions from single known solutions. Among his more remarkable results, it must suffice to mention here only the following: If p is prime and a, p are mutually prime then $a^{p-1}-1$ is divisible by p (the so-called »lesser theorem of Fermat«); every prime number of the form $4n+1$ can be written uniquely as the sum of two squares (proved by »descente infinie«); no number of the form n^2m, where m is not a square but is divisible by $4k-1$, is either a square or decomposable into the sum of two squares; all primes of the form $8k+1$ and $8k+3$ are expressible as the sum of a square and the double of a square; every prime number of the form $3k+1$ is expressible as the sum of a square and the triple of square; finally, the very famous »last theorem«, there are no integral solutions for the equation $x^n+y^n=z^n$ for any $n>2$.

As already intimated above, many of Fermat's conclusions are not supported by proofs (indeed the »last theorem« is a case in point) and, furthermore, if Fermat did in fact possess proofs for them (which is not always certain) his methods and line of attack are in the majority of cases unknown.

Fermat is (with Pascal) the founding father of the modern theory of probability (cf. pp. 390–398). He also did some work in geostatics (cf. pp. 359–375) and optics (where he gave a rigorous mathematical proof of the sine law of refraction based on the principle that nature does nothing in vain (cf. pp. 375–390).

II

All this and much more is intelligently, competently, and insightfully dealt with by Mahoney in his book. Having ploughed through Fermat's »Oeuvres«, edited by Paul

Tannery and Charles Henry (4 vols., Paris, 1891–1912; a »Supplément« was published in 1922 at Paris by C. de WAARD), Mahoney presents the reader with a thoughtful, perceptive, and penetrating analysis of the rather often obscure and puzzling Fermatian text. Mahoney's guidance through the Fermatian forest is sure, his sane and sober instincts of orientation exude the confidence of the traveller through friendly and known territory, and his interpretations of troublesome passages and works are historically persuasive and carry deep mathematico-historical conviction. This is no hack job. Neither is it hagiography. It is solid schorlarly work, anchored firmly in a deep knowledge of Fermat's mathematics and representing the results of many years of thinking and research going all the way back to the author's career as a graduate student. (Indeed it incorporates, though it is not limited to, the structures erected in the author's Princeton Ph. D. dissertation).

The book is well written, appealing stylistically, the cadences of the phrases are impressive, the author writes with ease and verve (sometimes this leads him to hackneyed expressions and repetitive turns of phrase), there is pungency to his descriptions, and the book is truly readable in its non-technical parts, possessing thus a characteristic not too common among the more recent works of historians of science.

A few illustrations of the above characterization must suffice. A case in point is Mahoney's discussion of Fermat's method of maxima and minima (cf., expecially pp. 143–165). After a careful analysis of Fermat's approach, Mahoney dwells on what strikes the modern reader as the least defensible step in Fermat's algorithm, namely the division by zero. However, as Mahoney persuasively and perceptively shows, Fermat's apparent blunder takes on an entirely different significance when seen in the following light:

> Thus, the apparent division by a quantity equal to 0 . . . obtrudes directly on the sensitivity of the reader. One takes the expression to be maximized, sets it equal to another expression in which $x+y$ has been substituted for x in the original, cancels common terms, divides by y, and sets $y=0$. Though . . . obvious now, the difficulty had the [following] resolution in Fermat's own mind. In the application of syncrisis to the original equation expressing the general problem to be maximized, y represented a real difference between the roots. It only became 0 when one took the particular instance of a maximum (or minimum). Making it 0 did no violence to what had previously been obtained, since syncrisis yielded fully general relationships irrespective of any particular values of the constants in the equation (p. 160).

In the same context, Mahoney illuminates Fermat's use of the concept of »adequality« in the written form of the »Methodus ad Disquirendam Maximam et Minimam«. Borrowed from Diophantus, the concept involved only a finitistic approach without appeal to infinitesimals and (therefore) limits. In the »Methodus«, the concept meant »temporary equality« and ». . . it has certainly led historians of mathematics astray. For into it they have read the pseudoequality of the differential calculus; for them it becomes one more peg on which to hang a quasi-modern interpretation of Fermat's method. It cannot, however, provide that service. Fermat's method was finitistic, and so too was his use of the term *adequality*« (p. 164).

Another example of Mahoney's sympathetic and perceptive historical understanding is provided by his simple, ingenious, and convincing reconstruction of the unavailable proof of Fermat's theorem, mentioned in a letter of 1640 to Roberval (cf. »Oeuvres«, vol. 2, pp. 203–204), that if a given number is divided by the greatest square that measures it, and if the quotient is measured by a prime number of the form 4k-1, then the given number is neither a square nor the sum of two squares, not in integers nor in fractions (cf. p. 317).

Finally, as a last illustration of that segment of Mahoney's historiography with which I agree wholeheartedly, the one without anachronisms and unwarrented assumptions, I call the attention of the reader to Mahoney's discussion of Fermat's challenge of 1657 in number theory (cf. pp. 323–337). What Mahoney has to say there can also serve as a specimen of his stylistic vigour and fluency alluded to above. The subsequent quotation will, I hope, bring the point home:

> Like that of many a noble house of Fermat's day, the pedigree was fanciful, a product of myopia and wishful thinking conditioned by the analytic tradition imbued in Fermat. True to that tradition, Fermat saw Viète's translation of [Diophantus's] »Arithmetic« into the algebraic language of the *»analytic art«* as evidence for the original presence of an algebra of continuous magnitude in that work. In fact it had not been present. Diophantus' unknowns never denoted continuous geometrical magnitudes; his »algebra« dealt solely with rational numbers. Heir to his own Greek tradition, Diophantus knew precisely the difference between continuous and discrete magnitude and the need for methods peculiar to each. It was Viète, not Diophantus or any other Greek writer, who made algebra the *»analytic art«*, the system of formal reasonings that united the realms of the continuous and the discrete. It was Fermat, not Apollonius or Archimedes, who translated geometry into the theory of equations. And it was Fermat, not Diophantus, who used that same theory of equations to unleash the full power of the method of *»single«* and *»double«* equations. If in Fermat's day geometry obtruded on arithmetic, it did so because Viète and his followers, foremost among them Fermat, had used the *»analytic art«* to blur the distinction between the two, a distinction that is the very hallmark of classical Greek mathematics. And now, Fermat would reassert the distinction; he would *»redeem the patrimony«*. But, again like many a noble house of the day, the *»patrimony«* was in fact newly gained wealth, the result of the recent conquests. It is the irony . . . of Fermat's career as a whole that the ancients had never dreamed of the mathematics that Fermat had in mind (pp. 329–330).

III

My main complaints about Mahoney's book can be summarized as follows. I deem Mahoney's approach to the formal writing and handling of history too careless to be acceptable. In too many instances, quotations are actually misquotations, references are inaccurate, original diagrams are modified in reproduced proofs without acknowledgement, division into paragraphs in quoted passages is eliminated, translations are mistranslations, etc. This cannot but reflect negatively on the author's scholarship.

Instances of misquotation abound, but I shall confine myself to the subsequent selection: On pp. 140–41, Mahoney gives a translation of a passage from Fermat's »Dissertatio Tripartita«. However, in his translation Mahoney has failed to introduce an ellipsis for the phrase he skipped (. . . *nempe esse primos 3, 5, 17, 257, 65, 573, etc. in infinitum* . . ., »Oeuvres«, I, 131) leaving thus the impression that his translation includes the Fermatian passage in its entirety, while, in reality, it does not. Furthermore, » . . . I will derive from it . . .« seems an inappropriate translation because Fermat says *derivabitur* (ibid.), i. e., »it will be derived,« which has the ring of a challenge to others, rather than being a publicly undertaken vow, which furthermore was against Fermat's character as described by Mahoney.

Another instance of misquotation appears on p. 187, where Mahoney arbitrarily introduces italics in a quoted passage from Descartes where these italics are missing. Graver still, on p. 186, *within* another quotation from Descartes, Mahoney modifies the notation without further ado. He does the same on p. 188 with a quotation from Fermat, changing Fermat's Viètan notation to the modern, Cartesian notation without qualms. In the same place he also introduces italics in a quoted text where the original contains no italics. On the other hand, on p. 191, Mahoney omits italics where they do exist in the quoted original and, in his translation of the French text fails to indicate that a certain phrase appears in the original in Latin (cf. »Oeuvres«, II, 162).

Misquotations also appear on pp. 201 (notes 112, 113), 203 (cf. »Supplément«, pp. 124–25), 214, 217 (n. 5, cf. »Oeuvres«, II, 337), 220 (where the beginning of a new paragraph in Fermat is eliminated; cf. »Oeuvres«, II, 73), 226 (n. 24; cf. »Oeuvres«, II, 13), 227, where italics are arbitrarily eliminated (cf. »Supplément, 15–16), 230, 248, 266, n. 73 (where Mahoney introduces in the quoted Latin text arbitrary changes of punctuation, capitalization, and even changes the ending of a word), 303, n. 47 (omission of division into paragraphs), 315–16 (where, among many other sins of omission and commission, Mahoney fails to indicate, in a lengthy quotation, a two-paragraphs-long ellipsis; cf. »Oeuvres«, II, 204), 327, 329, 331 (cf. »Oeuvres«, III, 404), 332–33 (cf. »Oeuvres«, II, 346), 340, 343 (cf. »Oeuvres«, I, 327), 345, (cf. »Oeuvres«, II, 433–34), etc., etc.

I shall now provide the reader with a selective sample of inaccurate references accompanied by their corrections. On p. 217, n. 5, the correct reference is »Oeuvres«, II, 337–38 (and not 337 as Mahoney writes). On p. 219, n. 12, Mahoney's reference (»Oeuvres«, II, 72) is wrong; the correct reference is p. 73. On p. 230 a quotation from »Oeuvres«, II, 84–85 is referenced improperly as appearing on pp. 66–70 and 83–86, leaving the reader at a loss. On p. 257, n. 56, Mahoney's wrong reference (»Oeuvres«, I, 272–273) should be corrected to pp. 271–73. On p. 273, n. 79, Mahoney's wrong reference (»Oeuvres«, II, 173) should be corrected to p. 172. There are also faulty references on pp. 289 (correct reference in n. 18 is »Oeuvres«, II, 21–22), 290, n. 21 (correct reference is »Oeuvres«, II, 176–77), 294, n. 35 (correct reference is »Oeuvres«, II, 209–210), 296, n. 38 (correct reference is »Oeuvres«, II, 210–11), 320, n. 78 (correct reference is »Oeuvres«, II, 256–57, since the quoted passage appears on these two pages and not on p. 258; the fragment of the letter referred to in Mahoney's previous sentence, on the other hand, does appear on pp. 256–58; the entire confusion could have been easily removed by placing the number 78 at the end of the previous sentence), 336, n. 121 (correct reference is »Oeuvres«, II, 403), 337, n. 122 (where the reader is misleadingly sent to n. 91; correct reference is »Oeuvres«, II, 404), etc.

Mahoney's treatment of Fermat's diagrams is also too perfunctory to please the discriminating historian. Thus, sometimes Mahoney modifies Fermat's diagrams when merely paraphrasing Fermat's procedures, without it being at all clear first, that a modification is involved and second, why Mahoney's modification of Fermat's diagram is preferable to the original. A case in point is the diagram appearing on p. 81, which should be compared with Fermat's original appearing in »Oeuvres«, I, 92. Moreover, quite often Mahoney alters Fermat's diagrams within the context of a quoted passage (which clearly amounts to a misquotation), or, in more dubious circumstances, he produces a diagram which is NOT Fermat's, presenting it to the reader as the genuine thing. This, I think, is unacceptable in historical studies. Instances of this blameworthy procedure literally abound. I shall simply enumerate some of the more blatant cases so that the reader can compare Mahoney's modified and »improved« diagrams with their respective counterparts appearing in »Oeuvres«. In what follows the first page numbers refer to diagrams appearing on those pages in Mahoney, while the references in parentheses correspond to Fermat's drawings in the »Oeuvres«: p. 84 (I, 92), p. 86 (I, 96, fig. 82), p. 89 (I, 101, fig. 86), p. 103, 2nd diagram (I, 35, fig. 35); on p. 105 Mahoney inverts the order of Fermat's figures, so that Fermat's ›figura prima‹ becomes Mahoney's fig. no. 3, Fermat's ›figura tertia‹ becomes Mahoney's fig. no. 1, while only Fermat's ›figura secunda‹ keeps its proper numbering (cf. »Oeuvres«, I, 39); p. 111 (I, 247); the figure appearing on p. 136 is NOT Descartes' original diagram in the »Géométrie« or its faithful reproduction by Fermat in »Oeuvres«, I, 122, though Mahoney's exposition leaves no doubt in the reader's mind that Mahoney reproduces Descartes' own, unchanged diagram (cf. Mahoney's book, p. 135); p. 166 (I, 135, fig. 92), p. 186 (II, 143), p. 188 (II, 155, fig. 67), p. 201 (I, 166), p. 208 (I, 154), p. 212 (I, 163), p. 219 (II, 55, fig. 35), p. 246 (I, 256, fig. 142), p. 257 (I, 271, fig. 145), p. 272 (»Supplément«, p. 88, fig. 22), p. 273 (II, 173, fig. 73), in which the quoted text from Fermat makes no sense because of the different figure drawn by Mahoney; first diagram on p. 274 (I, 227, fig. 128 (7)), etc.

Concerning Mahoney's translations, though as a rule they are reliable and sometimes they are even a sensible improvement over Tannery's translations in vol. 3 of the »Oeuvres«, there are a very few cases of infelicitous renderings or plain mistranslations from both the French and the Latin. To illustrate briefly, Descartes' statement that *Fermat est Gascon, moi non* (p. 15) is understood by Mahoney as if ›Gascon‹ meant ›rowdy.‹ It actually means, as Mahoney surely knows, ›braggart.‹ (In his article on Fermat in the Dictionary of Scientific Biography, Mahoney takes ›Gascon‹ to somehow connote ›troublemaker‹, which I think, is not correct). On p. 276, Mahoney translates a passage from the »Treatise on Rectification«, ending his translation in the middle of one of Fermat's sentences, without any indication of an ellipsis to the reader, thus contributing, at least potentially, to a slight misunderstanding of the thrust of Fermat's statement. On p. 332, there is another instance of too careless and curt an attitude toward translating faithfully the Fermatian text to satisfy a historically minded reader (cf. »Oeuvres«, II, 343). Finally, on the next page, 333, ›vos Anglois‹ becomes ›you English‹.

There are other places in the book where statements are made with which I cannot agree. Thus, on p. 122, n. 83, Mahoney seems to think that Theodosius's »Sphaerics«

was first translated into Latin in the 16th century. Well, Witelo knew it in the 13th century, so Mahoney's statement seems to be wrong. There is a clearly mistaken statement in n. 113, p. 202 about the length of the subtangent. The dates for Eudoxus are given (on p. 223, n. 20) as 390–337 B. C. without any substantiation for changing the »standard« dates, i. e. ca. 408– ca. 355. (In the *D.S.B.*, G. L. Huxley takes the dates: ca. 400– ca. 347). Mahoney's tentative reconstruction of Fermat's unstated proof of the rule for the summation of the consecutive n-th powers of the series of natural numbers is not particulary convincing. There is on p. 318 an example of a mathematical *non sequitur.*

In enumerating Fermat's requests for proofs from Wallis and his friends, Mahoney mistakenly reports one proposition. Thus Mahoney says: »the double of every prime of the form $8k+1$ can be expressed as the sum of three squares« (p. 377). The correct form of the prime should be $8k-1$, since Fermat says (»Oeuvres«, II, 405): *Duplum cujuslibet numeri primi unitate minoris quam multiplex octonarii, componitur ex tribus quadratis* (my emphasis). In the same place, Mahoney reports Fermat's third proposition, for which he appealed to Wallis and Company for help, as follows: »the product of any two primes of the form $20k+3$ or $20k+7$ can be expressed as the sum of a square and five times a square«, while what Fermat says is: *si duo numeri primi, desinentes aut in 3 aut in 7, et quaternarii multiplicem ternario superantes* [i.e. of the form $4k+3$], *inter se ducantur, productum componitur ex quadrata et quintuplo alterius quadrati* (»Oeuvres«, II, 405).

I do not think Mahoney made convincing his statement that Fermat ›borrowed‹ from Descartes a phrase in his (Fermat's) ›Relation‹ to Carcavi (pp. 340–41). There is nothing beyond the slightest of similarities to substantiate Mahoney's unconvincing claim. Finally, Mahoney is wrong when he says that » . . . any rigorous demonstration of Fermat's assertion [i. e., that if $x^2=y^2+2z^2$, where x, y, z are mutually prime, then x is itself of the same form, that $x = p^2 + 2q^2$] requires the use of numbers of the form $a + b \sqrt{-2}$ . . .« (p. 346). Indeed, an elementary proof of this proposition can be given without appeal to complex numbers.

I also think that it is possible to find a better principle of organization for chapter 4 – and perhaps also for chapter 3 – which would make the matters taken up in these chapters clearer to the reader. For instance, if preceeding each of these chapters one would find first a continuous and systematic summary discussion of all the various steps, refinements, and changes in Fermat's analytic geometry and his theory of equations, this would certainly help the reader, later, to locate easily each change and further sophistication undertaken by Fermat in his methods, when these are analysed in greater historical and mathematical detail. The way these chapters stand, however, the reader is buried under a mountain of detail, in which issues are taken up, abandoned, returned to, modified, amplified, etc., without a clear overview and grasp of the historico-mathematical flow of events.

Finally, I call the attention to two mistakes in the notation of two diagrams, which may prevent the unwary reader from grasping the line of proof: In the diagram on p. 99, *B* and *C* should be interchanged (cf. »Oeuvres«, I, 29), and in diagram on p. 387, *O* should be replaced by *C* (cf. »Oeuvres«, I, 170).

IV

The chief dissatisfaction I have with Mahoney's book lies, however, in a different direction. Basically, it has to do with Mahoney's unwarranted concessions toward the use of modern algebraic symbolism in analysing pre-symbolic and proto-symbolic mathematics. Such concessions are even more offensive when they come from an author who is acutely aware of the dangers involved in applying this ahistorical procedure. It was after all Mahoney who said (pp. XII–XIII):

> Any historian of mathematics conscious of the perils and pitfalls of Whig history quickly discovers that the translation of past mathematics into modern symbolism and terminology represents the greatest danger of all. The symbols and terms of modern mathematics are the bearers of its concepts and methods. Their application to historical material always involves the risk of imposing on that material a content it does not in fact possess.

Furthermore, this is not the only place in his work (both in this book and in articles and reviews) that Mahoney has come out strongly against automatically translating pre-modern mathematics into the modern algebraic language. And yet, his book teems with precisely such unwarranted translations.

Though it is true that Fermat is one of the originators of modern mathematics, a man who applied the algebraic reasoning and approach to Greek mathematics and who as a rule reasoned algebraically (analytically) in his work, it is also true that, on the one hand, some of Fermat's work is couched exclusively in the terms of the geometric language of the Greeks and, on the other hand, Fermat's algebraic symbolism is Viète's symbolism which is different from Descartes' symbolism out of which modern symbolism developed. It is, therefore, imperative, if one does not want to read into Fermat foreign concepts and methods, to stick faithfully to Fermat's own way of expression and to use very gingerly (if at all) the modern algebraic symbols and manipulations.

Form and Content are NOT independent in mathematics, precisely as they are NOT independent in any other scientific discipline. The way one says things has a very profound bearing on what one could or could not say. It is, therefore, in principle a historically unforgiveable sin to transcribe rhetorical mathematics into symbolic mathematics and to assume wrongly that mathematical equivalence is tantamount to historical equivalence. Language is the immediate reality of thought. Differences in language (the form of expression) correspond to genuine differences in thought (the content of expression). If Greek mathematics (with very few clear-cut exceptions) is largely Greek geometry, and if there are very serious differences between the geometrical and the algebraic way of thinking (as Mahoney has forcefully shown in other places), which make the one historically irreducible to the other, then any mechanical transcription of a geometric text into what seems to the mathematician its legitimate algebraic counterpart is historically illegitimate, counterproductive, and, therefore, indefensible.

What is Mahoney's approach to this very important historico-philosophical issue in his book? At best he is hedging, compromising, where no compromises are justified; at worst he betrays Fermat's line of thought, clothing Fermat's rhetorical propositions and proofs in the unsuitable and unbecoming garb of modern, non-Viètan algebraic symbolism. Thus, after having said the right thing about the dangers of using modern symbolism, Mahoney goes on to state that a judicious application of modern symbolism to past mathematical texts ». . . should serve historical analysis by enabling one to cut through to the core of past mathematics without introducing anachronisms« (p. XIII). I wonder if this is at all possible! To reach a sympathetic historical understanding of past mathematics with its idiosyncratic concepts, methods, and *hidden* structures, one should, most certainly, NOT appeal to »universal keys« which, while unravelling the hidden structure and making it obvious, impose on it, at the same time (and unavoidably) ». . . a content it does not in fact possess« (ibid.). In other words, the abstract symbolism of modern mathematics, unavailable to pre-modern mathematicians, is the necessary tool without which it is impossible to talk of THE structure and THE conceptual apparatus of mathematics.

In the absence of such a symbolism (and the rather mechanical technics of manipulation of symbols) it is not at all obvious that, say, geometry IS algebra. And yet WE know it is! Pre-modern mathematical texts exhibit a structure of their own, which, though reducible to THE structure of modern mathematics, in principle, is not on the face of it (and this is very significant) the modern structure. The crucial thing is that read through pre-modern glasses (the only acceptable historical reading), by a smart individual whose mind was not »corrupted« by modern mathematics and who, therefore, strives to understand them in their own right (since he does not have at his disposal short-cuts and »universal keys«), pre-modern mathematical texts are not reducible to anything less cumbersome, awkward, etc. It seems to me that, historically speaking, pre-modern and modern mathematical texts are *heteromorphic*, though (due exclusively to modern developments in mathematics) WE can see that they are both reducible to the same unique structure, i. e., that fundamentally (logically) they are indeed *isomorphic*. Therefore, applying to past mathematical texts modern symbolism in order ». . . to lay bare the basic structure of concepts and methods« (ibid.), is tantamount to abandoning the historical criterion in favour of the logical criterion which, though worthwhile perhaps, is, to speak in euphemisms (and Mahoney recognizes this), historically unrewarding and (most often) distorting.

Mahoney's book swarms with more or less distorting transcriptions of Fermatian rhetorical and proto-symbolic algebra and geometry into post-Fermatian symbolism. Furthermore, though aware of the irreducible differences between Greek and modern mathematics, Mahoney speaks illegitimately of ». . . the solution of quadratic equations . . . known since antiquity« (p. 6). Where are there equations as such in antiquity? Nowhere! Mahoney's statement is, I think, an instance of carelessness. What Mahoney should have said (and he also could have said it; cf. his »Babylonian Algebra: Form Vs. Content,« in: *Studies in History and Philosophy of Science,* vol. 1, No. 4 (1971), pp. 369–380), had he been on his guard, is that there are in antiquity instances of specific, numerical problems (and of geometrical constructions and propositions) which, when translated by us into modern symbolism, lead to quadratic equations. To speak of equations, you must have an operational algebraic symbolism! (Mahoney, again, knows this; cf. his »Die Anfänge der algebraischen Denkweise im 17. Jahrhundert«, in: *Rete,* vol. 1 (1971), pp. 15–31; however, for reasons which have nothing to do with the merits of the case, Mahoney has abandoned his own caveats).

The same carelessness is, I think, responsible for many other indefensible positions advanced by Mahoney in his book. Thus, Mahoney seems to think that somehow, historically, algebra does indeed lie at the root of Greek geometry and the Greeks hid it from sight (cf. pp. 32, 33). He thinks that the Greeks translated ». . . Babylonian algebraic techniques into . . . geometrical form« (p. 33) and that »problem-solving analysis subsumes algebra« (ibid.). Whatever the latter statement may mean historically (and this is far from clear to me), the former is certainly unsupported by any genuine historical evidence. Historians can and do speak of such a translation of Babylonian algebra into Greek geometry (cf. NEUGEBAUER's and Van der WAERDEN's work) only because, by translating both the Babylonian cuneiform numerical tables and the Greek geometrical propositions into algebraic language, they can discern similarities (or even identities) and consequently ask about influences, and so forth. In saying, »From its beginning in Babylonia[?] to its culmination in the Renaissance cossist tradition, algebra constituted a sophisticated form of arithmetical problem-solving« (p. 34), that is, in deciding to call unequivocally Babylonian number manipulations ***algebra***, Mahoney undercuts his own articulate position in his forceful review of NEUGEBAUER's »Vorgriechische Mathematik« (»Form Vs. Content«; reference above).

Mahoney accepts uncritically (and, I think, paradoxically for a man with his sophisticated views on the historical incommensurability of geometry and algebra) the legitimacy of the concept »geometrical algebra« (cf. pp. 94, 123, 153, etc.). Such a beast, however, never walked the paths of the mathematical domain. It is the brain-child of modern mathematicians and historians of mathematics who, starting with Pierre de la Ramée and Viète in the 16th century (cf. Mahoney's *Rete* article quoted above), continuing with Descartes, Fermat, William Oughtred, etc. and going all the way to Paul TANNERY, H. G. ZEUTHEN, T. L. HEATH, B. L. Van der WAERDEN, etc., have managed to identify the nonexistent, hidden algebraic roots of Greek geometry. Such a feat was possible only after the beginnings of what may be called the algebraic stage in the development of mathematics, and it was due primarily to the unfounded assumption that mathematics was a *scientia universalis*, an algebra of thought containing universal ways of inference, everlasting structures, and timeless, ideal patterns of investigation which can be identified throughout the history of civilized man and which are completely independent of the form in which they happen to appear at a particular juncture in time.

Such a hackneyed and objectionable methodological and interpretive position overlooks the historically unbridgeable chasm separating geometry from algebra, and takes it for granted that since we, after Fermat and Descartes, know that geometry is indeed reducible to algebra, *therefore*, geometry has always been »disguised«, »clumsy«, »unwieldy«, »cumbersome« algebra. This position, is, however, historically unrewarding, self-defeating for the historian, and it overlooks the profound differences between the geometric and algebraic way of thinking. It is philosophically naive and offensive, it leaves many fundamental historical questions unanswered and, I think, it creates more historical problems than it solves. (Cf. my forthcoming paper »On the Need to Rewrite the History of Ancient Greek Mathematics« in the »Proceedings of the XIVth International Congress of the History of Science«, Tokyo and

Kyoto, 1974 and my forthcoming lengthy and detailed study with the same title).

Closely related to Mahoney's paradoxical and ambiguous attitude toward »geometrical algebra« is his equally ambiguous attitude toward the use of modern symbolism in explaining and interpreting Fermat's mathematics. Though he is aware of the great dangers involved in misapplying modern notation, he compromises too nonchalantly with this very condemnable procedure, and this necessarily leads him to anachronisms.

His anachronistic tendencies of transcription are exemplified by his statement of Fermat's »lesser« theorem: »... for mutually prime a und p, and p prime, $a^{p-1} \equiv 1$ (mod p)...« (p. 52). (Cf. also essentially the same statement on p. 283.) Actually what Fermat says is (and it is translated by Mahoney on p. 291): *Tout nombre premier mesure infailliblement une des puissances -1 de quelque progression que ce soit, et l'exposant de la dite puissance est sous-multiple du nombre premier donné -1; et, après qu'on a trouvé la première puissance qui satisfait à la question, toutes celles dont les exposants sont multiples de l'exposant de la première satisfont tout de même à la question* (Oeuvres, II, 209, in a letter to Frenicle of Oct. 18, 1640). Though mathematically Fermat's and Mahoney's statements are equivalent, Mahoney's way of putting it is certainly anachronistic! There are other examples of the same anachronistic transcription on the same page. For instance, »... Galileo's spiral ($\rho = \alpha^2$, generalized to spirals of the form $\rho = \alpha^n$)...«

Another instance of the same sin can be found on p. 285, where Mahoney says: »The problem of determining perfect numbers occupied the Pythagoreans and their successors, and its solution is recorded in Euclid's »Elements«: if $2^{n+1}-1$ is prime, then $2^n(2^{n+1}-1)$ is a perfect number«. This is most certainly not the way Euclid puts it (cf. prop. IX, 36). Instances of anachronistic transcriptions of rhetorical statements into symbolic formulae can also be found on p. 73, n. 3. On p. 61 (and also on p. 63), Mahoney refers to Fermat's »... solution of the equation $x^2 - py^2 = 1$ (p prime) in integers«. But, of course, this way of writing the so-called »Pell equation« is anachronistic, since Fermat put it always in words; for instance: *Tout nombre non quarré est de telle nature qu'on peut trouver infinis quarrés par lesquels si vous multipliez le nombre donné et si vous ajoutez l'unité au produit, vienne un quarré* (»Oeuvres«, II, 333).

This means that even when transcribed in modern symbolism Fermat's way of putting it becomes $Nx^2 + 1 = y^2$, where N is any non-square positive integer. (Cf. also: *Dato quovis numero non quadrato, dantur infinite quadrati qui, in datum numerum ducti, adscitâ unitate conficiant quadratum* (»Oeuvres«, II, 335); also, *Tout nombre non quarré est de telle nature qu'il y a infinis quarrés qui, multipliant ledit nombre, font un quarré moins 1* (ibid., 433), which transcribed in the same fashion becomes $Nx^2 = y^2 - 1$). Though Mahoney's modern transcription and Fermat's rhetorical enunciation are equivalent mathematically, Fermat's way of putting it is significant in obtaining the solution (and otherwise).

There are many other cases of anachronistic statements. »Where Descartes«, says Mahoney, »justifies in detail the use of line lengths as algebraic variables, *that is, by showing that geometric magnitudes constitute an algebraic field*[!] Fermat assumes it as part of the Viètan algebra he employs« (p. 80, my emphasis). Mahoney's unacceptable compromise in matters of symbolism is evident in n. 21 on p. 81, where he says, among other things, »The matter of symbolism creates some difficulties in this chapter. I resolve them by a compromise . . . When seeking merely to explicate the mathematics of the »Introduction«, or to discuss a logico-mathematical point, I use modern terminology and symbolism[!] . . . The expression f(A, E) is a h o p e l e s s [my emphasis] mixture of old and new . . . The mathematicians of the seventeenth century lacked means for expressing a general function of one or more variables, working rather from *instar omnium* or verbal expressions«. It seems to me that Mahoney's last statement is the crucial thing which should have prevented him from following the too easy route he chose to follow.

Indeed, sometimes Mahoney has to face problems of his own making, due to his use of modern notation. For example in 23, p. 83, he sys: *»The nonhomogeneity of the equation* $x^2+xy=ay^2$ *results from using the equation instead of the proportion by wich Fermat actually expresses the relationship*[!]« (my emphasis). Time and again, Mahoney is confronted by problems stemming from his conscious decision to compromise and use a foreign notation in discussing Fermat's mathematics: »At this point, the problem of notation again obtrudes on our narrative. Throughout his memoirs on maxima and minima and on the rule of tangents, Fermat remained loyal to the notation of Viète. But that notation hides more from the modern reader than it reveals. As always, a compromise is necessary« (p. 154, n. 24). »The equation in polar coordinates is, needless to say, anachronistic, as the equations for the spirals Fermat investigated. Both Archimedes and Fermat gave verbal definitions of their curves in terms of generation by a combination of rotational and rectilinear motions. Here and in what follows, however, use of the equations accurately[?] describes the content of their achievement while saving words[!], and the dangers of conceptual anachronism are slight« (p. 218, n. 9).

Mahoney's »paraphrase« (p. 90) of Fermat's solution to the *difficilima omnium aequalitatum* (»Oeuvres», I, 100) – i. e., in Fermat's notation, *Bq-Aq bis aequetur A in E bis + Eq* (translated by Mahoney correctly as $b^2-2x^2=2xy+y^2$) is another case of anachronistic symbolic transcription of Fermat's Viètan symbolism which is richly dipped in rhetorical formulations. (Cf. »Oeuvres«, I, 101–102 vs. Mahoney's discussion on pp. 89–90). Mahoney's presentation of Apollonius's propositions 11, 12, and 13 of Book I of the »Conics« (containing the determination of the *symptomata* of the three conic sections) is couched in unacceptable, barely hidden, algebraic form. (Cf. J. L. HEIBERG, *Apollonii Pergaei Quae Graece Exstant*, Leipzig: Teubner, 1891, vol. 1, pp. 37–53 and Paul VER EECKE, ed. and transl., *Les Coniques d'Apollonius de Perge*, Bruges: Desclée, de Brouwer et C[ie], 1924, pp. 21–31).

Throughout the book, I find too many clear-cut instances of anachronistic interpretations (cf. pp. 209–210, 222, 247, 321, etc.). Mahoney, though in principle fully aware of the dangers involved, is willing to compromise and pay the heavy historical price required by the practical application of his conciliatory views in matters of symbolism: »It does no violence to Fermat's patterns of thought here to take advantage of the economy of modern notation, although doing so of course masks the genius that the lack of such notation demanded of Fermat [So why do it?]. Nonetheless, his method was algebraic, even if his language was not« (p. 230). The question is: Since even in those cases when the thinking of Fermat was undoubtedly algebraic, transcribing his rhetorical analysis into modern symbolism necessarily leads (according to Mahoney's own statement) to a very serious loss (namely, the possibility of assessing properly the greatness of Fermat's genius), why should one use such a transcription? In this specific case, reading Fermat's own words in his two letters, one to Mersenne (»Oeuvres«, II, 63–71) and the other to Roberval (ibid., 83–87), and comparing Fermat's rhetorical style with Mahoney's dexterous manipulation of symbols, the historically minded reader cannot but feel that much too much of Fermat's great genius is lost in Mahoney's transcription for it to be acceptable, let alone welcome.

Describing in avowedly anachronistic symbolism Diophantus's method of solution of indeterminate quadratic equations in two unknowns (p. 299), Mahoney remarks that Diophantus's choice of substitution expressions is guided by (among other requirements) the necessity of a positive solution. He goes on to say: ». . . he [i. e., Diophantus] does not move beyond the single solution . . . His failure to do so is characteristic of the »Arithmetic's« overwhelming orientation toward problem-solving . . . though it probably [my emphasis] also reflects the lack of a symbolism sophisticated enough to capture a general solution algorithm in visual format« (p. 300). Now, I think that the use of the term »probably« by Mahoney in this case (where »Certainly« is called for) is an illustration of Mahoney's very ambiguous relationship toward the very important historical problem of algebraic symbolism.

Mahoney's tendency to express Fermatian ideas in avowedly anachronistic terms comes to the fore in the statement: »Is $(m+1)^2 - N = y^2$ for some integer y? Often the last digits of the remainder will show immediately that it is not, since they are not quadratic residues of 10 (only the language here is anachronistic; the idea is not . . .)« (p. 321). The point is, however, that anachronistic language almost necessarily means anachronistic ideas!

V.

Though it may seem to the reader that the tenor of my judgements is on the whole rather critical, I do find it necessary to point out emphatically that I consider Mahoney's book the best detailed study of Fermat's mathematical career available to the interested scholar. It is precisely because I deem Mahoney's book a very valuable study that I found it right to include in this article a detailed criticism of its drawbacks. Less good a book would not have warranted such close scrutiny. Mahoney set himself a very ambitious goal – a thorough study of Fermat and his mathematics – and I think he has largely achieved his goal. Indeed his book presents us with a full account of the life and work of Fermat, and it represents a serious contribution to a more profound and adequate understanding of 17th century mathematics.

If not everything is perfect, this should not surprise us, especially if we take into account the magnitude, variety, and depth of Mahoney's topic. It is only because Mahoney's scholarly efforts strive to enable one to begin approaching Fermat's mathematics in a sympathetic fashion, and on its own grounds, that my numerous criticisms of Mahoney's unwarrantedly conciliatory steps in matters of notation and algebraic manipulations have been emphasized. This should not surprise Mahoney, who is, after all, himself aware of the serious dangers involved in modern algebraic transcriptions of pre-modern and proto-modern mathematical texts.

»Il y a deux sortes d'hommes«, said Jean Monnet, »ceux qui veulent être quelqu'un, et ceux qui veulent accomplir quelque chose«. Mahoney belongs to the latter category. He has indeed achieved something. His book is an important book, written with style, penetration, and insight. It makes us see all the main problems of Fermat's mathematics in a new and very valuable perspective. Every chapter of the book will repay close study by anyone interested in Fermat and the mathematics of the 17th century. Fermat studies can never be quite the same again after this book. This is why I strongly hope that its readership will be broad enough to warrant a second edition of the book, in which Mahoney will, I hope, correct what needs correcting and (is it expecting too much?) get rid of all his concessions to anachronisms of transcription and interpretation.