The Mysteries of Adaequare: A Vindication of Fermat

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Summary

The commonly accepted interpretations of FERMAT's method of extreme values tell us that this is a curious method, based on an approximate equality and burdened with several contradictions within FERMAT's writings. In this article, both a philological approach taking into account that there is only one manuscript written in FERMAT's own handwriting and a mathematical approach taking into account that brilliant mathematicians usually are not so very confused when talking about their own central mathematical ideas are combined. A new hypothesis is put forward which renders the mathematics clear and coherent and which does not need the assumption that FERMAT was confused. Probably a number of words have been added by CARCAVY in two of FERMAT's papers.

1. The Dogma

A lot of excellent work has been done concerning FERMAT's method of extreme values and his method of tangents, and quite a lot of acumen has been spent on the interpretation of his method. Although there are considerable differences between various interpretations, there seems to be a common dogma, to which all interpreters, as far as I know, agree, namely: *Fermat uses the word "adaequare" in the sense of "to be approximately equal" or "to be pseudo-equal" or "to be counterfactually equal"*. The dogma is given support by the editors of the FERMAT Oeuvres who used in the third volume (FERMAT 1896) a special notation ($\backsim$) for adaequalitas which FERMAT quite surely never did. As for "pseudo-equality" and "counterfactual equality", I have to confess that I never could understand these notions, but in any case, these words indicate the lack of an equality. I want to challenge this common dogma of FERMAT interpretation because it seems to be incapable of yielding a consistent interpretation. As yet, this fact has been recognized only in disguise: Usually, the explicit or tacit assumption is made that FERMAT himself was somewhat confused. Having shown some of the oddities of the present state of our knowledge of FERMAT's method in the first part of my article, I would like to put forward a new hypothesis contradicting the dogma. There are several arguments in favour of my hypothesis, a new interpretation of the well-known passage in Diophantus being one of them. In the last part of this article, a number of counterarguments will be discussed. Although my hypothesis does not differ from other hypotheses in so far as no cogent proof can be given, I would like to stress that my hypothesis renders FERMAT's mathematics clear and intelligible, that the hypothesis is supported by several philological arguments, and that it does not need the assumption that FERMAT was confused.

2. Oddities

The first oddity of our present state of knowledge is the fact that the WIELEITNER-STRØMHOLM interpretation (WIELEITNER 1929; STRØMHOLM 1968/69) has not been refuted. According to them, there are two methods of extreme values in FERMAT's writings, both of them solving the same problems and using the same formulas, but one of them claiming the existence of an equality between two algebraic expressions and the other one claiming the existence of an approximate equality or a pseudo-equality between the very same expressions. This is a strange interpretation, but it is even stranger that it could not be refuted and that there is really some evidence in favour of it. WIELEITNER and STRØMHOLM tried to mitigate the strangeness by assigning different dates to the different methods: Whereas WIELEITNER argued that the inequality method was earlier, STRØMHOLM believed that the chronological order was the other way round. But neither dating is convincing, as can be shown by the following consideration. We know from CARCAVY's letters to HUYGENS (HUYGENS 1888, 432; HUYGENS 1889, 457, 534; HUYGENS 1890, 38) and from the Florence collection of manuscripts (FERMAT 1922, XVII–XIX, XXI–XXII) that CARCAVY intended to edit FERMAT's papers and that FERMAT looked through the final collection. The very two papers (FERMAT 1891, 140–147, 147–153) which give best support to the WIELEITNER–STRØMHOLM interpretation were part of this collection. Thus if FERMAT had changed his mind, he would, of course, have corrected the earlier manuscript in order to have a coherent edition. Although there are marginal notes by FERMAT giving advice for the editing, none of the passages in question was changed. If there were two methods, then, FERMAT had both at the same time. Still we are left with the impression that FERMAT was rather confused.

The second oddity is the chronology of FERMAT's use of the word "adaequare". Restricting the investigation to generally accepted dates, the following stages can be given:

(i) In a paper written at the end of 1637 (or earlier) and sent to DESCARTES, FERMAT writes: "Adaequentur, ut loquitur Diophantus, duo homogenea maximae aut minimae aequalia" (FERMAT 1891, 133; FERMAT 1894, 128). The reference to DIOPHANTUS will be discussed later, but whatever the result will be, two quantities both of them equal to the extreme value must of course be equal. Therefore, adaequare indicates an equality.

(ii) In a paper written after the paper just quoted but before the spring of 1643 (because there is a copy which VAN SCHOOTEN very probably made during his stay in Paris (FERMAT 1922, X)) the definite contention is made that adaequalitas is an inequality (FERMAT 1922, 74, 75; FERMAT 1891, 140, 141).

(iii) Using the word adaequare, FERMAT tells us in the very first sentence of his tract on rectification (written in 1659 and published in 1660) that hitherto no one established an equality between a straight line and a general curved line (FERMAT 1891, 211). No doubt, adaequare indicates an equality.

(iv) In the tract on quadrature, we read the remark "adaequetur, ut loquitur Diophantus, aut fere aequetur" (FERMAT 1891, 257). Whatever the result of the investigation of the passage in DIOPHANTUS will be, here adaequare evidently indicates an approximate equality. The tract was written in 1658 or 1659, but as there is explicit reference to the tract on rectification, there might have been a later reworking of the text. Anyway, both tracts were written more or less at the same time, but contradict each other plainly.

The texts quoted under (i) and (ii) are part of the Florence collection; thus we know that FERMAT looked through the final collection, but there was no correction of the passages in question.

The third oddity is to be found in the only extant manuscript on the method of extreme values and tangents written in FERMAT's handwriting. The facsimile (FERMAT 1891, after XVIII) gives the impression that the manuscript was not written in a hurry; there are few additions and deletions, and the handwriting is fair. In two passages of the manuscript, FERMAT expresses the same idea in nearly identical words, but in the first time he uses the word adaequalitas, whereas in the second passage the word aequalitas is used (FERMAT 1891, 159, 162, 426). If the dogma of the usual interpretation is assumed, then this is another example of FERMAT's supposed confusion. We are lucky to have FERMAT's own handwriting in this particular case, and we are lucky to have a critical edition of FERMAT's writings, because although the editors of the Oeuvres deemed it necessary to repair FERMAT's supposed confusion by changing the aequalitas into another adaequalitas, they nevertheless indicated their change in the critical apparatus (FERMAT 1891, 426). It is one of the advantages of a critical edition that it allows independene of the interpretation of the editors. By the way, the editors of (FERMAT 1891) were not the only ones to believe that there was an inconsistency in the manuscript written by FERMAT himself. The editors of (FERMAT 1679) present a version which has been changed the other way round: They give twice aequalitas, whereas (FERMAT 1891) gives twice adaequalitas.

3. A Warning

Looking at these oddities, we finally arrive at an alternative and at a warning. The alternative is: Either FERMAT was pretty confused – far more confused than a mathematician presenting one of his central ideas is expected to be – or something is fundamentally wrong with our understanding of FERMAT's method of extreme values. And the warning is: Perhaps something is wrong with the texts. Obviously, there is no proof that every word in the texts handed down to us really was written by FERMAT. FERMAT himself did not publish anything about the method of extreme values and the method of tangents, and there is only one manuscript concerning these methods written in his handwriting. Most of the texts concerning these methods are not only copies, but copies which were taken from copies which may have been taken from FERMAT's original. As for the authenticity of FERMAT's texts, we are in a singularly bad position. Some hard facts will be useful. Some of the copyists changed at least the notation: FERMAT used VIÈTE's notation, but some of the copies and some of the texts printed in (FERMAT 1679) use Cartesian notation. For convenience of the reader, the notation of these texts has been changed back in (FERMAT 1891–1922).

As a matter of course, a number of mistakes happen if a manuscript is copied. Most of these mistakes are not really worth mention or are of small importance, but we have to take into account changes that have been made on purpose. If such changes exist, their existence can be proved definitely only for the one text for which a manuscript in FERMAT's handwriting is extant. A comparison of this manuscript with other 17^{th} century sources of the same text leads to the following result: In (FERMAT 1679) the words adaequalitas, adaequalitatem, adaequari are five times changed into aequalitas, aequalitatem, aequari or a symbol with the meaning of equality (FERMAT 1891, 426); these changes have been made four times in a copy taken by VAN SCHOOTEN and three times (plus a passage with a meaningless word) in a manuscript at Florence written by an unknown copyist (FERMAT 1922, 167–168; FERMAT 1891, 159, 163, 164). If we stick to the dogma of FERMAT interpretation according to which there is a conceptual difference between aequare and adaequare, then it is hard to believe that these changes just happened by accident. Moreover, there are two changes which certainly have been made by purpose. The Florence manuscript (FERMAT 1922, 167; FERMAT 1891, 159) and (FERMAT 1679, 69) added the adjective "sufficiens", which is not to be found in the corresponding passage in FERMAT's original. In addition, someone else changed a passage in the manuscript written in FERMAT's handwriting (FERMAT 1891, 165, footnote). Thus we may take it for granted that changes have been made during the 17th century on purpose. A copyist who believed he was finding a mistake might have felt tempted to correct it, and someone intending to edit the papers would have felt that it was his duty to correct mistakes. I mention this consideration only as a general warning; it is not meant as an excuse to establish arbitrary hypotheses about possible changes in the text, whenever you perceive a difficulty.

The general warning can be made a bit more explicit. As is well known, FERMAT used to say that he would give a proof or publish this or that piece of his work, if only he had time (FERMAT 1894, 14, 99, 165, 196, 444). But meanwhile he had time to invent new theorems. Thus it seems that he did not like to write down all the necessary details and to check all calculations and every single letter in a drawing. He spoke about "la pente naturelle que j'ai vers la paresse" (FERMAT 1894, 461, compare also 488), and he even stated "je suis le plus paresseux de tous les hommes" (FERMAT 1894, 105). It is well-known that he did not give a final version even of his most important theorems (HENRY 1880, 19, 29). His remark in a letter to MERSENNE is quite typical: "S'il y a manque en la supputation, vous la corrigerez, car je n'ai pas seulement le loisir de relire ma lettre." (FERMAT 1894, 175).

MERSENNE was not the only one to receive full power to correct. As is well-known, CARCAVY had a large collection of FERMAT papers and desired to edit them (HENRY 1880, 29; FERMAT 1891, XIII, XVII; HUYGENS 1889, 534; HUYGENS 1890, 38). Apparently FERMAT did not even own a copy of quite a number of the tracts which were in the possession of CARCAVY (FERMAT 1894, 366, 407–408; HUYGENS 1889, 411). But as for the preparation of an edition, there was a problem: CARCAVY was well aware of the fact that he was not an expert mathematician: "Je ne scay pas beaucoup aux mathématiques, mais J'ay une grande passion pour cette science" (HUYGENS 1888, 418). Probably FERMAT (1894, 286) expected CARCAVY to have difficulties with the mathematics, but he believed that CARCAVY could overcome them. Then FERMAT wanted PASCAL to join in the editing; he wrote to CARCAVY, "je consens que vous soyez les maitres; vous pourriez éclaircir ou augmenter ce qui semble trop concis et me décharger d'un soin que mes occupations m'empêchent de prendre." (FERMAT 1894, 299; HENRY 1880, 28). PASCAL probably refused, but it is note-worthy that FERMAT agreed that some passages should be clarified or supplemented in the edition. CARCAVY's preparation of an edition will be discussed again later.

4. The Meaning of Adaequare

Having made these introductory remarks, I want to put forward my *hypothesis*: *Fermat used the word "adaequare" in the sense of "to put equal"*. Of course, some objections can be raised immediately against this hypothesis, but before I discuss them, I would like to give some arguments in favour of the hypothesis.

The first argument in favour of the hypothesis is simply taken from the Latin dictionary. The word "adaequare" was used quite frequently in classical Latin, for example by CAESAR, LIVIUS, TACITUS, PLINIUS, CICERO, SUETONIUS. The Thesaurus Linguae Latinae (1900) gives "re vera aequum reddere, aestimatione aequare" and simply "aequare" as synonyms. To avoid misunderstanding: "aestimatione aequare" does not mean that there is an approximate equality, but refers to the fact that things have to be quantified at first. Translations given by the Oxford Latin Dictionary (1982) include: to equal, to make equal in height, to make of equal duration, to put on equal footing, to assert to be equal, to achieve equality with, to be or show oneself equal in status, quality *etc.* In a mathematical context, the only difference between "aequare" and "adaequare" (if there is any) seems to be that the latter gives more stress on the fact that the equality is achieved. But this is only a slight difference, because the achievement of the equality is also implied in "aequare" (which makes it different from the simple "aequalis esse"). Therefore, "to put equal" or "to equal" seems to be the best translation into English as far as mathematics is concerned. There are well established Latin words to indicate an approximate equality, namely "approximare" or, more frequently used in classical Latin as well as in the 17th century, "appropinquare". It is well known that FERMAT's knowledge of Latin and Greek was excellent, and so if he wanted to tell us that there was an approximate equality, why should he use a word indicating an equality? As far as I know, this question has not been answered nor even discussed by any adherent of the dogma of FERMAT interpretation.

The reference to the Latin dictionary can easily be supported by reference to outstanding mathematical texts of the 17th century. First of all, there is abundant evidence that VIÈTE (1646, for example 80 (twice), 135, 137, 141, 143 (thrice)) used adaequare in the sense of "to put equal". The frequent use by VIÈTE is sufficient to prove that ITARD (1975, 117) was wrong in believing adaequare was a rare word in mathematical literature. ITARD drew attention to the fact that CAVALIERI and GULDIN (CAVALIERI 1635, liber II,17; CAVALIERI 1647, 23, 203) used the word without explaining it, but it seems to me that there was no need to explain the word because the dogma of FERMAT interpretation did not exist then. DESCARTES understood FERMAT's "adaequari" to indicate an equality; he translated the word with "étre posé égal" (FERMAT 1894, 128, 141, 142, 146; DESCARTES 1897, 488; DESCARTES 1898, 126, 127, 132–133), and we do not know of any protest by MERSENNE or anyone else participating in the debate. When FRANS VAN SCHOOTEN copied the first paper which had been given to DESCARTES (FERMAT 1891, 133–136), he changed three times "adaequabitur" into "aequabitur" or the Cartesian symbol ∞ for equality (FERMAT 1922, 164). Evidently, SCHOOTEN just replaced a word by another word or a symbol which he considered to be a synonym, in the same way as, in copying another passage (FERMAT 1891, 163; FERMAT 1922, 168) he replaced several times expressions like "AB vocetur C" by "AB ∞ C". In fact, SCHOOTEN (1659, 152) used "adaequare" in the sense of "to put equal", and the same is true for DEBEAUNE (1661, 70). HUYGENS (1940, 234, 235) used the word in a discussion of FERMAT's method in the sense of indicating an equality, and finally BARROW (1973, 252) may be mentioned.

Besides these mathematical texts, use of the word is frequent in philosophical texts concerning ideas and definitions. As this philosophical use might give rise to misunderstandings, some remarks may be useful. According to THOMAS OF AQUIN, "veritas est adaequatio rei et intellectus" (HOFFMEISTER 1955, 14). Truth is obtained if the human mind forms his ideas in such a way that a perfect congruence with the objects is achieved. As ideas and objects are different qualities, there is no real equality, but rather an isomorphism: Logical inference between ideas must be valid for corresponding objects. DESCARTES (1897, 233), SPINOZA (1925, II, 13, 15, 27, 28; IV, 270) and LEIBNIZ (1880, 423; 1890, 200, 295; BELAVAL 1978) used the word in this tradition. Although in this context we do not have quantities, the word has a precise meaning as long as ideas and definitions are discussed. But in later times, the word was used apart from this context, and so it acquired a somewhat vague meaning. The English "adequate" and similar words in other modern languages do not necessarily imply a strict equality. But we are not allowed to use this somewhat vague meaning in our interpretation of Latin texts of the 17th century, let alone of mathematical texts.

But wasn't there any Latin text in the early modern times, which used the word differently? As CIFOLETTI (1991, 16) reports, GOSSELIN (1583, 17 v°) used the word to indicate an approximate equality. He was not an influential author; in fact, I could not get his book in Germany and therefore I can only refer to CIFOLETTI. It is hard to believe that GOSSELIN should have influenced FERMAT more than VIÈTE did. GOSSELIN evidently had read XYLANDER's translation of DIOPHANTUS which had appeared in 1575, and, as will be shown, he had not succeeded in understanding DIOPHANTUS.

5. The Diophantus Passage

In the secondary literature on FERMAT, FERMAT's reference to DIOPHANTUS usually is considered to be decisive evidence in favour of the dogma, although it is generally believed that this reference is superfluous and without a real connection to FERMAT's method. But first of all, it is usually not mentioned that the references to DIOPHANTUS in FERMAT's writings plainly contradict each other. There are four references to DIOPHANTUS. As was mentioned above in the discussion of the second oddity, two references (FERMAT 1891, 133, 153) give support to my hypothesis, whereas the two others (FERMAT 1891, 140, 257; FERMAT 1922, 74) give support to the dogma (these two will be discussed in the last part of my article). In any case, a closer look at the DIOPHANTUS passage will be useful and will provide the second and the third argument (dealing with the Greek word in question respectively with the mathematics of the passage) in favour of my hypothesis.

Thus my second argument is just based on the Greek dictionary. DIOPHANTUS V,11 uses the Greek word *παρισότης*. The best Greek dictionary, Thesaurus Graecae Linguae (1842–1847), gives "aequalitas" as the translation into Latin and refers to the Diophantus passage in question. The word is derived from *παρισόω* which is translated by the Thesaurus Graecae Linguae into "adaequare", "aequale reddere". Therefore XYLANDER and BACHET were right in using the word "adaequalitas" in their translations of DIOPHANTUS (1575, 131; 1621, 309), although that does not imply that they had understood the mathematics of the passage. In any case, there is no problem with my hypothesis as far as language is concerned, but as the authors of the Thesaurus Graecae Linguae were only experts in Greek language, the mathematical aspect has to be checked separately.

The third argument in favour of the hypothesis is based on the mathematics of the Diophantus passage. DIOPHANTUS applies his method in V,9 and V,11. In order to facilitate understanding, I will use algebraic symbolism. In V,9 the problem is posed: Given a positive rational number a, find positive rational numbers u, v, x, y with the property

$$u + v = 1$$

$$u + a = x^2$$

$$v + a = y^2$$

Then DIOPHANTUS himself gives the following reformulation: Given a, find x, y so that

$$x^2 + y^2 = 2a + 1$$

$$|x^2 - y^2| < 1$$

As the second condition of the reformulation ensures $u = x^2 - a$ and $v = y^2 - a$ to be positive, both formulations are in fact equivalent. The solution of the problem as given by DIOPHANTUS supposes a condition on a, namely the existence of positive rational p, q with the property $2a + 1 = p^2 + q^2$. Although DIOPHANTUS uses this supposition, it is not explicitly stated in the formulation of the problem. Therefore it is generally accepted that DIOPHANTUS's text is corrupt. It was FERMAT who made an ingenious repair of the text (FERMAT 1891, 312–314; FERMAT 1894, 203–204). I need not go into this, but undoubtedly FERMAT was very familiar with this particular problem of DIOPHANTUS. DIOPHANTUS's solution proceeds as follows: At the first step – and this is the relevant step characterizing the method of παρισότης – he finds a positive rational z with the property

$$2z^2 \approx 2a + 1 \ .$$

If $p = q$, then $x = p$ and $y = q$ is the solution. Therefore we may assume $p > q$, then n, m, r are calculated from

$$p - z = \frac{n}{r} \ ,$$

$$z - q = \frac{m}{r} \ .$$

Finally s is calculated from

$$\left(p - \frac{n}{s}\right)^2 + \left(q + \frac{m}{s}\right)^2 = 2a + 1 \ .$$

Then the two expressions in the brackets are the solutions x and y.

Looking back on the solution, it is useful to repeat the problem: Two numbers p, q with the property that the sum of their squares equals $2a + 1$ are given, and two numbers x, y with the same property and the additional property of being close enough to each other (that is $|x^2 - y^2| < 1$) are sought. Reinterpreting the problem in terms of variables and curves, the idea of DIOPHANTUS as seen by FERMAT would be: Go to the minimum of the curve $|x^2 - y^2|$, there in a neighbourhood you will find an appropriate solution, that is two points which are close enough to each other. The minimum evidently is achieved by putting x equal to y, and that is why the method received its name "method of $\pi\alpha\rho\iota\sigma\acute{o}\tau\eta\varsigma$ or putting equal". As there is a minimum idea in the DIOPHANTUS passage, FERMAT's reference to DIOPHANTUS becomes intelligible. But in fact, DIOPHANTUS is not really interested in the exact minimum, but rather in a rational solution; therefore putting x equal to y leads to

$$2z^2 \approx 2a + 1$$

instead of $2x^2 = 2y^2 = 2a + 1$.

In V,11 the problem is a bit more complicated, but there is the same idea. The problem is: Given a with $3a + 1 = p^2 + q^2$, find u, v, w, x, y, z with

$$u + v + w = 1$$

$$u + a = x^2$$

$$v + a = y^2$$

$$w + a = z^2$$

DIOPHANTUS's reformulation is: Given a with $3a + 1 = p^2 + q^2$, find x, y, z with

$$x^2 + y^2 + z^2 = 3a + 1$$

and x^2 as well as y^2 as well as z^2 greater than a.

Although DIOPHANTUS does not say so, the last line may easily be replaced by $x \leqq y \leqq z$ and

$$|x^2 - y^2| + |x^2 - z^2| < 1 \ .$$

Furthermore, from the supposition on a, DIOPHANTUS concludes

$$p^2 + \frac{9q^2}{25} + \frac{16q^2}{25} = 3a + 1$$

Thus three squares with the required property are given, and three squares with the same property and the additional property of being close enough to each other are sought. Thus again, there is a minimum idea, and again, the minimum is achieved by putting $x = y = z$, but again DIOPHANTUS is not interested in the exact minimum, but in rational solutions, and so he uses $3x^2 \approx 3a + 1$.

DIOPHANTUS not only calls his method "method of putting equal", but the method is in fact based on an act of putting equal, and therefore it seems that the usual translations of DIOPHANTUS's Arithmetic should be corrected: TANNERY (DIOPHANTUS 1893, 345) translated into Latin as "processus appropinquationis", WERTHEIM (DIOPHANTUS 1890, 214) translated into German as "Methode der Beinahegleichheit", and HEATH (1885, 117, 214; 1910, 95, 207), too, considered an approximation "as closely as possible" to be the essence of the method. The wrong understanding of DIOPHANTUS might perhaps be caused or maintained by the wrong understanding of FERMAT, and *vice versa.* As for FERMAT, it fits quite well in his general way of thought that he saw a geometrical idea (namely the minimum idea) in formulas of number theory. FERMAT invented analytical geometry, and moreover, his method of infinite descent is the application of a topological concept (namely the order relation) in number theory. Thus we need not be surprised by FERMAT's seeing a curve, where DIOPHANTUS only talked about number theory. Furthermore, the approximate equality which in fact occurs in DIOPHANTUS's Arithmetic is only due to the fact that DIOPHANTUS seeks rational solutions. In FERMAT's application of the idea, the exact minimum or maximum is wanted, no matter whether it is rational or irrational. Therefore, if FERMAT's reference to DIOPHANTUS is to make sense, it cannot be meant as a reference to an approximate equality.

6. Some Mathematical Remarks

Before I proceed to develop another argument in favour of my hypothesis in the next section, some mathematical items should be discussed.

Firstly, there is the obvious question whether there is a connection between the DIOPHANTUS passage and FERMAT's method. Roughly speaking, the essence of the DIOPHANTUS passage is that there is a function $g(x, y) = x^2 + y^2 - 2a - 1 = 0$ with $g(x,y) = g(y,x)$ and another function $f(x) = |2x^2 - 2a - 1|$ with $f(x) = f(y)$ if only $g(x, y) = 0$. Then the minimum x_0 of f is found by putting x equal to y, that is, by solving the equation $g(x_0, x_0) = 0$. The modern reader, being used to look for a germ of the derivative, will be disappointed because the relation between f and g has nothing to do with the relation between a function and its derivative. It seems to be lucky coincidence to have a suitable polynomial expression g at one's disposal. But FERMAT found a generalization; his study of VIÈTE's theory of equations (MAHONEY 1973, 147–160) drew his attention to the division of a polynomial by one of its roots. This helps to find for any polynomial f a suitable g, namely

$$f(x) - f(y) = (x - y)\,g(x, y) \ .$$

Now FERMAT's method is: In order to find a possible maximum or minimum x_0 of f, solve $g(x_0, x_0) = 0$. For proof, we look at the definition of g and find the following two equations for $x < x_0 < y$:

$$f(x) - f(x_0) = (x - x_0)\,g(x, x_0) \ ,$$

$$f(x_0) - f(y) = (x_0 - y)\,g(x_0, y) \ .$$

Without loss of generality, let x_0 be a maximum. Then $f(x) - f(x_0) < 0$ and $f(x_0) - f(y) > 0$. As $x - x_0 < 0$ and $x_0 - y < 0$, we conclude $g(x, x_0) > 0$ and $g(x_0, y) < 0$. Then the theorem of intermediate value for polynomials implies $g(x_0, x_0) = 0$.

I do not claim to give a reconstruction of FERMAT's true thoughts. The problem with FERMAT is that he evidently thought on a more abstract level than he could write down on paper with the notation available at his time. It is difficult or even impossible to discuss his ideas (the general validity of which he claims) in full generality without the notion of function or at least of "polynomial in general" or without index notation. As it is useless to speculate about the ideas which might have been in FERMAT's mind, I used functional notation, and I claim only to give a rough sketch of a possible generalization of the passage in DIOPHANTUS.

It is not the purpose of this article to discuss all details of FERMAT's method (like distinction between maximum and minimum, points of inflection *etc.*), but only to discuss the arguments and counterarguments concerning my hypothesis. Therefore, as for his extension of the method from polynomials to algebraic curves, a brief remark will be sufficient. Let p, q be polynomials and let

$$r(x) = p(x) + \sqrt{q(x)} \ .$$

Let x_0 be a maximum or a minimum of r. Then FERMAT (1891, 153–155) applies his method to the polynomial f defined by

$$f(x) = q(x) + 2r(x_0)p(x) - p^2(x) \ .$$

It is easy to deduce that

$$f(x) - f(x_0) = (r(x) - r(x_0))(r(x) + r(x_0) - 2p(x)) \ .$$

Now, if $q(x_0) = 0$, then

$$f(x) - f(x_0) = q(x) - (p(x) - p(x_0))^2 \ ,$$

x_0 is a minimum of q (because $q(x) \geqq 0$), therefore $x - x_0$ is a double root of $f(x) - f(x_0)$. But if $q(x_0) > 0$, then there is a neighbourhood of x_0 with

$$r(x) + r(x_0) - 2p(x) = \sqrt{q(x)} + \sqrt{q(x_0)} + p(x_0) - p(x) > 0 \ .$$

Therefore x_0 is a maximum or minimum of f. I do not claim that this was the reasoning of FERMAT, but I do claim that the extension of the method was not based on an approximate equality.

In secondary literature, FERMAT sometimes is treated a little bit as a brilliant boy in high school, not having learned his lesson properly. Sure enough, the justification of a step is often missing, but as far as I can see, it can always be added in a few lines. According to FERMAT (1894, 317), it was not necessary to write down all the details. Even in the tract on rectification, which was published, he asks the reader to supplement the passages which seem to be too concise (FERMAT 1891, 238). None of his writings on the method of extreme values and tangents was written to be published as it was; all these papers were just meant to be given to colleagues with a particular mathematical ability.

Matters of course are not written down in such circles. Sometimes FERMAT is reproached for not having made the explicit restriction that everything is to take place in a sufficiently small neighbourhood. But FERMAT certainly knew about curves with a maximum and a minimum or with two maxima and a minimum. Very probably he could have been more precise about "sufficiently small", because he knew about points of inflection.

Another and more interesting example of criticism in secondary literature concerns FERMAT's repeated remark about a possible division by a higher power of E (FERMAT 1891, 133, 141; FERMAT 1922, 124). This is usually regarded to be a mistake or an absent-mindedness of FERMAT (WIELEITNER 1929, 25), as there always seemed to be only a division by the first power of E. An exception is (ANDERSEN 1980, 24) where the claim is made that FERMAT's remark refers only to algebraic curves. But the reproach to FERMAT is due to a misunderstanding of his method. In FERMAT's notation, the vowels are used to denote the variables as well as the unknown constant quantities. In the method of tangents and in his determination of the centre of gravity of the parabolic conoid (FERMAT 1891, 136–139) as well as in the deduction of the law of refraction (FERMAT 1891, 170–172), A is an unknown constant quantity. It is strongly misleading to mix FERMAT's notation with our own and to describe his method in these cases by something like "$f(A)$ adaequatur $f(A-E)$", as is often done. This description of his method invariably leads to the subsequent complaint that FERMAT did not really apply his own method. But FERMAT was thoroughly coherent. If you describe his method by "$f(A)$ adaequatur $f(A-E)$", then you have to take into account in the cases mentioned that the coefficients of the polynomial f may depend on A. This is strange, but it is only strange because modern notation was mixed with FERMAT's notation. It seems to be easier and more appropriate to understand his method in the cases mentioned as follows: The method consists in finding a polynomial in E which takes a minimum if $E = 0$. Of course, A and E have to be cleverly chosen according to the nature of the problem; then the application of the method leads to an equation in A which solves the problem. As the coefficients of the polynomial the minimum of which is considered depend on A, it may well happen that some powers of E do not appear in the resulting equation. Thus FERMAT's repeated remark as to the possible division by some higher power of E is completely justified. If, for example, $E = 0$ is a quadruple root instead of a double root, then you have to divide by E^3.

Another complication arises from the fact that FERMAT's method consists of two steps, the calculation of the function g (which is based on $x - x_0 \neq 0$) and then the calculation of x_0 (which is based on $x = x_0$). The method being routine, FERMAT sometimes takes both steps at the same time, thereby giving the impression that there was a division by zero. But these calculations are only the analysis (MAHONEY 1973, 28–71); the synthesis, namely the proof that the calculated value of x_0 really is a maximum or a minimum, is always omitted, because it is trivial (just divide the polynomial $f(x) - f(x_0)$ by $(x - x_0)^2$ and then look at the signs on both sides). The synthesis necessarily has to be a formal deduction, but not the analysis.

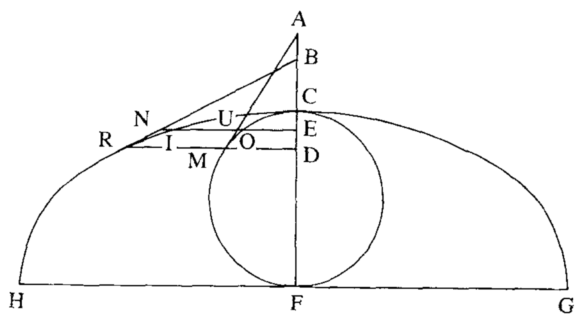
Sometimes (especially for the method of tangents) FERMAT applies his method in a slightly different way: He looks only at those x with $x < x_0$ (compare FERMAT 1894, 152), and the auxiliary function g is defined to be the unique polynomial satisfying

$$f(x) - f(x_0) = (x - x_0)g(x) \; .$$

In this second version, FERMAT's method is solving the equation $g(x_0) = 0$. Whereas the first version is backed by the intuitive idea that in a sufficiently small neighbourhood of the maximum or minimum x_0 for every $x < x_0$ there is a $y > x_0$ with $f(x) = f(y)$, this intuitive idea does not work for the second version. But of course, both versions are essentially the same. Again, FERMAT sometimes abbreviates the tedious calculations by calculating g and x_0 from $g(x_0) = 0$ at the same time. To discuss the complications involved, let us look at an example, namely the determination of the tangent to the parabola (FERMAT 1891, 135–136). FERMAT finds two rational functions, one of them always taking a greater value than the other one. In order to get polynomials, FERMAT multiplies; thus he arrives at $\alpha(x) > \beta(x)$ with two polynomials α and β (as mentioned above, in FERMAT's notation in this case A is an unknown constant and E is the variable). Then he proceeds (FERMAT 1891, 135, last line) by bringing $\alpha(x)$ and $\beta(x)$ into the relation of "adaequare". Can this be interpreted as an approximate equality? FERMAT explicitly tells us that E (in our notation: the variable x) can be chosen arbitrarily, and so there need not be any approximate equality. FERMAT rather puts $\alpha(x)$ equal to $\beta(x)$, thereby imposing a condition on x: The equality holds only for some x. In fact, it holds only for the extreme value x_0 (in this particular example $x_0 = 0$). Two lines later on, FERMAT divides by x, although the equation holds only for $x = 0$. But we know already that the essence of the method is the double root idea, so it is not really a division by zero. To reduce this example to the formulas given above, we might put

$$f(x) = \alpha(x) - \beta(x) \; .$$

In conclusion, the mathematics of FERMAT's method seems to be clear: There is no approximate equality, but as the exposition lacks functional notation and gives only the analysis in an abbreviated form, FERMAT was misunderstood.



In the secondary literature the dogma was also used for the interpretation of FERMAT's determination of tangents to transcendental curves. Restricting attention to a few remarks, I wish to show how easy FERMAT's idea in fact is. During his determination of the tangent to the cycloid, FERMAT (1891, 163 last paragraph) looks at the expression NE – OE – CM + MO (CM and MO being arcs of the circle). This expression takes a minimum, namely zero, if the point E coincides with the point D. Then FERMAT replaces MO – OE by MU – EU. Now if the point E is not too far away of the point D, then

$$\mathrm{MO} - \mathrm{OE} \geqq -\mathrm{MD}$$

as well as

$$\mathrm{MU} - \mathrm{UE} \geqq -\mathrm{MD}\ ,$$

and equality holds if and only if the points E and D coincide. Therefore, NE–UE–CM+MU also has a minimum, namely zero, if the points E and D coincide. Thus FERMAT may apply his method, and he does not make use of an approximate equality between an arc length of the circle and a segment of the tangent. The latter interpretation usually is made, but it plainly contradicts the text: FERMAT explicitly calls the straight line DE "recta utcumque assumpta" (FERMAT 1891, 163), that is, DE is not infinitely small or "very small", and so if we try to grasp FERMAT's reasoning with a mathematical concept of a later period, neither the notion of an infinitesimal nor the notion of a limit are appropriate, but rather the notion of function: FERMAT considers a function of DE, taking a minimum for DE = 0. It is hard for the modern reader to get rid of the limit ideas in his mind, and so he considers the replacement of a very small arc length of the circle by a very small segment of the tangent to be quite natural. But this is not the way of FERMAT.

7. The Best Texts

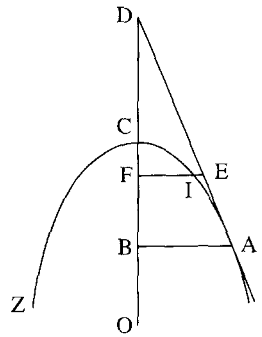
Having made these mathematical remarks, I would like to put foward the fourth argument in favour of my hypothesis: The hypothesis is in full agreement with the best texts in the philological sense, that is, the texts published by FERMAT himself and manuscripts written in his own handwriting. As for the printed texts, the tract on rectification was already mentioned: The only appearance of the word adaequare in this tract (FERMAT 1660a, 3; FERMAT 1891, 211) is undoubtedly in favour of my hypothesis. There is only one more text (FERMAT 1660b; FERMAT 1891, 199–210) which FERMAT published himself, but there neither adaequare nor adaequalitas is to be found. As for the manuscripts, reference has already been made to the only relevant manuscript written in FERMAT's handwriting. There are two passages evidently using aequalitas and adaequalitas interchangeably (FERMAT 1891, 159, 162, 426). Furthermore, adaequalitas and adaequari are mentioned in eight more passages in this manuscript, but there the use of the word might be considered to be compatible with the hypothesis as well as with the dogma. Thus if we restrict attention to the most reliable texts, there are two passages contradicting the dogma, but none contradicting my hypothesis. As we know already that changes in the texts have been made, this result is significant.

The first paper given to DESCARTES (FERMAT 1891, 133–136) (which supports the hypothesis as was explained above in the second oddity) was published by CLERSELIER only shortly after FERMAT's death, and so it is of no use in this section. During FERMAT's life-time, WALLIS (1658) published some letters written by FERMAT, but these letters do not deal with the method of extreme values. Then there are two relevant texts written by other authors and published during FERMAT's life-time, and we might ask whether FERMAT knew them and whether he protested or agreed. Firstly, there is HÉRIGONE's (1644, 59–69) exposition of the method. FERMAT (1891, 171; 1894, 463, 487) repeatedly referred to HÉRIGONE's account, and as long as the authenticity of this references is not questioned, it is obvious that he approved it. Thus HÉRIGONE's text should be discussed briefly, although he clearly gave an exposition of his own. HÉRIGONE does not use the word adaequare, and he simply considers FERMAT's variable E to be the constant zero. This is strange, because then there is a division by zero, and, of course, extreme values and tangents cannot be determined, if only one single point is regarded. But HÉRIGONE calls his exposition an analysis; *i.e.*, he consciously does not give a synthesis, and FERMAT, too, calls his method in his reference to HÉRIGONE an analysis. Thus HÉRIGONE does not claim to give a proof or a logical deduction, he just gives an instruction to perform certain operations (like a cooking recipe). If the instruction is understood properly, it always will lead to correct results, the correctness of which has to be proved in every particular case. Therefore FERMAT's approval is quite intelligible. HÉRIGONE's exposition is not based on an approximate equality but rather on the double root idea. Nevertheless, I do not wish to claim HÉRIGONE's account of the method as an argument in favour of my hypothesis.

Secondly, SCHOOTEN (1659, 253–255) gave an account of FERMAT's method. There is no evidence that FERMAT knew this, let alone that he approved the account. SCHOOTEN refers to HÉRIGONE, but there was an independent source of his knowledge of the method, as is evidenced by the Groningen manuscripts (FERMAT 1922, IX–X). In fact, SCHOOTEN seems to allude to (FERMAT 1891, 133, line 13) when he writes that there are two expressions for the minimum and that there is an equality between them (SCHOOTEN 1659, 254). In this exposition, SCHOOTEN does not use the word adaequare; as mentioned earlier, adaequare and aequare are synonyms for SCHOOTEN. SCHOOTEN states that the calculation can be shortened if terms with E^2 or E^3 are neglected. Evidently, SCHOOTEN thinks of an analysis; he does not aim at correctly deduced steps, but as a recipe. Finally SCHOOTEN refers to HUYGENS, but his considerations were not published at that time (HUYGENS 1908, 19, 46–49; 1910, 60–68). In conclusion, the accounts of HÉRIGONE, SCHOOTEN and HUYGENS do not help in this section on best texts. Now I turn to the discussion of four counterarguments.

8. The First Counterargument

The first counterargument is based on two papers which FERMAT wrote during his debate with DESCARTES on the method. Both passages (FERMAT 1894, 137, 155) deal with the method of tangents and use nearly the same words, and



so it is sufficient to quote one of them: "Quoique la ligne FE soit inégale à l'appliquée tirée du point F à la courbe, je la considère néanmoins comme si en effet elle étoit égale à l'appliquée, et en suite la compare par *adéquation* avec la ligne FI, suivant la propriété spécifique de la courbe." (FERMAT 1894, 155). This seems to imply that there are two expressions, which are not equal, the relation between them being called adaequalitas.

The mathematical idea of this passage was already noted above, when the determination of the tangent to the parabola was discussed. FERMAT considers a certain expression based on the ordinate of the curve and another expression based on the ordinate of the tangent, and he puts them equal. Of course, both expressions are not equal in general, but in putting them equal, a certain condition is imposed on the variable, which thereby becomes a constant. (The mathematical background of the procedure is the fact that the difference of the two expressions mentioned takes a minimum in the abscissa of the point of intersection of tangent and curve). To understand the passage quoted above, we have to note that neither FERMAT nor his correspondents have the notion of function. We have not only this notion, but also a standard vocabulary for these situations: For two different functions α, β, we may write $\alpha(x) \neq \beta(x)$, and we might add "in general", but nevertheless $\alpha(x) = \beta(x)$, and if necessary, we add "for some particular x". As FERMAT did not have this standard vocabulary, he expressed the situation a little bit paradoxically. But the apparent paradox is quite intelligible if you really look at the mathematics of FERMAT's examples.

Another objection could be made: The word adégalité is printed in italics in (FERMAT 1894), and so evidently it is underlined in the manuscript. One might argue that this underlining indicates adégalité being a technical term with a specific meaning; this would contradict my hypothesis. I am not really convinced of this. FERMAT could have underlined the word in order to make the apparent paradox more obvious. But even if the supposition of the objection is accepted, there is no proof that it was FERMAT who underlined the word. The underlined word is to be found five times in total in the two passages in question (FERMAT 1894, 137, 155, 156); the printing was made according to copies taken by ARBOGAST, who saw copies taken by MERSENNE. MERSENNE's copies are not known to us, nor do we know whether MERSENNE saw FERMAT's original or for example a copy taken by CARCAVY. In any case, the two passages are also to be found in a manuscript which was discovered after (FERMAT 1891–1922) were finished and which is now in the possession of the Bibliothèque Municipale in Toulouse (MAHONEY 1973, 404). This copy was taken by MICHELANGELO RICCI when MERSENNE was in Italy (FERMAT 1922, XII–XV). RICCI made an Italian translation of the two papers in question, and there the five words in questions are four times definitely not underlined (Toulouse, fol. 16r°, 17r°). As for the fifth time, the word had been forgotten at first, and then added in the margin. Below the last letters of the word, there is a line connecting the end of the word with the text (Toulouse, fol. 17v°). I do not think that this can be considered as an underlining of the word. There is no proof that the words were already underlined in MERSENNE's copies, let alone in FERMAT's original.

9. The Second Counterargument

The second counterargument is based on the paper "Je veux par ma méthode" (FERMAT 1922, 74–83; FERMAT 1891, 140–147). The paper starts with an easy example: The extreme value of $A^2B - A^3$ (I use Cartesian notation) is to be calculated. FERMAT calculates $(A + E)^2B - (A + E)^3$ and then continues: Both expressions are compared "comme s'ils estoient esgaux, bien qu'en effect ils ne le soient pas, et i'ay appelé en mon escrit latin cette sorte de comparaison *adaequalitatem* comme Diophant l'appelle, car le mot grec *παρισότης* dont il se sert, peut estre ainsy traduit." (FERMAT 1922, 74). And in the end of this calculation, two expressions are found, "entre lesquels il ne faut plus faire, comme auparavant, des comparaisons feintes et *adaequales*, mais une vraye équation." (FERMAT 1922, 75). This, of course, plainly contradicts my hypothesis.

The text in question is the only one dealing with the method of extreme values and tangents, where adaequare undoubtedly is used as a technical term, indicating an approximate equality or a "pseudo-equality". In the quoted passages, adaequalitatem and adaequales are printed in italics, furthermore, the word adaequalitatem is five more times printed in italics in this text, that is, these words are underlined in the manuscript. With the doubtful exception of the two passages in the preceding section, FERMAT did not underline these words in any other text on the method of extreme values and tangents. It seems to be worth mentioning that the only text which undoubtedly uses adaequalitas as a technical term is strictly consequent in underlining *every* adaequalitas. There is a striking contrast between this habit and the manuscript on the method of tangents written in FERMAT's handwritting (FERMAT 1891, 158–167) where adaequare and adaequalitas are never underlined.

The editors of (FERMAT 1891–1912) who were adherents of the dogma as their change of an aequalitas into adaequalitas (FERMAT 1891, 162 line 22, 426) sufficiently proves, noticed the existence of a stylistic oddity in the text: Twice (FERMAT 1891, 141; FERMAT 1922, 75) the expression "comparison adaequalitatem" is to be found. As it appears twice, it does not seem to be a mistake, but rather done by purpose. But this use of the words is, according to the editors, not compatible with FERMAT's usual habit, because he uses "comparatio" and "adaequalitas" as synonyms. As the editors continue, one of the words is superfluous and has probably been added by someone else. I agree with the editors in this reasoning, although I would like to add that FERMAT not only used "comparatio" and "adaequalitas" as synonyms, but also as is to be expected from VIÉTE's use of both words, "comparare" and "aequare" (FERMAT 1891, 148 line 19, 149 line 17–18). By the way, this is another argument in favour of my hypothesis. Anyway, the case for a change in the text has already been made by the editors, and this change seems to have been made by someone who considered adaequare to be a technical term, because "comparaison adaequalitatem" implies that there are various kinds of "comparaison", one of them being the "comparaison" by adaequalitas. Then a conclusion is to be drawn which has not been drawn by the editors: If "comparaison" and adaequalitas have in general the same meaning in FERMAT's writings, and if a change in the text has been made by someone else violating this rule, then doubt has to be cast on the two passages quoted, namely "i'ay appelé . . . cette sorte de comparaison *adaequalitatem*" (FERMAT 1922, 74) and "des comparaisons feintes et *adaequales*" (FERMAT 1922, 75), because here we are explicitly told that there are different kinds of "comparaisons".

Furthermore, one of the passages quoted refers to DIOPHANTUS, and this reference is not only wrong, but wrong in two different aspects. Firstly, the reference contradicts plainly the mathematical interpretation given above of the passage by DIOPHANTUS and its relevance for FERMAT. There is no approximate equality involved in FERMAT's insight and in his application of the DIOPHANTUS passage. Thus I would like to conclude that these words were written by someone who did not really understand the meaning of FERMAT's reference to DIOPHANTUS. Secondly, the passage quoted refers to the first paper (FERMAT 1891, 133) on the method given to DESCARTES, but nevertheless it contradicts this paper, because there without any doubt adaequare is used in the sense of putting equal. It seems hard to believe that FERMAT could have been so confused as to write this rubbish.

Finally, the same example, namely find the minimum of $A^2B - A^3$, is calculated in (FERMAT 1891, 149). There it is clearly stated that there is an equality. This seems to be irrefutable evidence that either FERMAT was very confused or someone else who did not really understand the method changed the text. I mentioned already in the discussion of the first oddity that the interpretation of FERMAT's having changed his mind during his life time does not work.

If changes in the text were made, who could have done it? As the passages in question are to be found in the Groningen copy (FERMAT 1922, 174) and as VAN SCHOOTEN left Paris in the spring of 1643 (FERMAT 1922, X), the changes must have been made before that time. We know for certain that FERMAT communicated his method to quite a number of people (FERMAT 1894, 71; FERMAT 1922, 73, 98–101). PAUL TANNERY and CHARLES HENRY tell us (FERMAT 1894, X note 3) that there are a few pecularities of FERMAT's French orthography, one of them being that the letter z never occurs at the end of a word. But in the text in question we find "marquéz" instead of "marqués" (FERMAT 1922, 75, twice) and "costéz" instead of "costés" (FERMAT 1922, 75) and "quarréz" instead of "quarrés" (FERMAT 1922, 83). If we feel justified in concluding that these pecularities, being found in both the Groningen and the Florence copies, were probably already present in the manuscript in the CARCAVY collection, then we might look for someone with this peculiar orthography. But this line of thought is pointless (so I need not discuss possible objections), because MERSENNE (1955, IV, 278–281) as well as CARCAVY (HUYGENS 1889, 535–538) and BOUILLAU (HUYGENS 1889, 28, 287, 332, 376) show the same pecularity in their French orthography.

If orthography does not help, we might look for someone with a motive. It is hard to see that anyone could have had a motive to change the text, with the exception of an editor who had got the commission to clarify wherever clarifications were needed. In fact, if the sentence with the reference to DIOPHANTUS was added by someone else, then that someone used the phrase "i'ay appelé en mon escrit latin", thereby alluding to a paper (FERMAT 1891, 133–136) which the contemporaries knew to be written by FERMAT. That seems to exclude the possibility that someone like BEAUGRAND who pretended that he himself had invented the method (FERMAT 1922, 114) made the change. As generally accepted (BAILLET 1691, 325; HENRY 1884, 319–320), it was CARCAVY who gave FERMAT's "escrit latin" to DESCARTES. Presumably he also saw the two papers on the method of tangents which were discussed in the section on the first counter-argument. If he saw these papers, and, given his limited mathematical abilities, did not really understand the passages discussed above, then he might have got the impression that there was an inequality. Furthermore, if he followed the reference to DIOPHANTUS given in (FERMAT 1891, 133), and, as is probable, did not really understand the passage in DIOPHANTUS, then again he would believe that the method was based on an inequality. Now imagine CARCAVY preparing the paper "Je veux par ma méthode" (FERMAT 1922, 74–83) for the edition. CARCAVY probably neither saw the double root idea nor its intuitive preparation (x_0 being the maximum or minimum, then for every $x < x_0$ there is a $y > x_0$ with the property $f(x) = f(y)$). In CARCAVY's opinion the text seemed to need a clarification. As it was his duty to make the text intelligible, he might have added two sentences and underlined some words. But then, why did he not continue and change all papers? As for the first paper given to DESCARTES (FERMAT 1891, 133–136), he could hardly do so, because this paper was already more or less made public. The same might perhaps have been true for the paper given to ROBERVAL (FERMAT 1891, 136–139). Thus he might have turned to the paper "Dum syncriseos et anastrophes Vietaeae" (FERMAT 1891, 147–153) in order to prepare it for the edition. But then certainly he would have been at a loss, because this paper is strongly based on the intuitive preparation of the double root idea, which was just mentioned. It is definitely impossible to change some words or to add one or two sentences in order to make it fit with the idea that the method is based on an approximate equality. Thus in 1650 (FERMAT 1894, 287; FERMAT 1922, XXII), he gave all papers, those which he had changed as well as those which he had left as they were, to FERMAT (HUYGENS 1889, 457). But as we know already, FERMAT just gave these papers back as they were, although they contradicted each other (we will deal with this strange fact in the next section). In 1659, CARCAVY tried again: He gave all the papers to FERMAT, asking him to look them through (HUYGENS 1889, 534; HUYGENS 1890, 38), but again FERMAT returned the papers without commenting on the contradictions. Thus there was no help to be expected from FERMAT. There may have been several reasons why CARCAVY never succeeded in editing FERMAT's papers, although he intended to do so for about thirty years, but one of the reasons for this delay presumably was that he did not dare to edit papers which he could neither understand nor clarify. The CARCAVY story is to be continued later.

10. Fermat's Inspection

Now the third counterargument suggests itself immediately: As FERMAT inspected the papers prepared by CARCAVY, he should have noticed that the text was changed, and as he did not correct it, he seems to have approved it. At first glance, this seems to be a decisive argument against my hypothesis, but a closer look will prove to be useful.

In "Je veux par ma méthode" as edited in (FERMAT 1922), we read "la moindre proportion" (FERMAT 1922, 76 line 23), and correspondingly in the Latin translation of it "proportionem omnium quae proponi possunt minimam" (FERMAT 1891, 142 line 11). But this is not what the sources give: The Groningen as well as the Florence manuscript have "question" instead of "proportion" (FERMAT 1922, 174), and the Latin translation printed in (FERMAT 1679) has "quaestionem" instead of "proportionem" (FERMAT 1891, 424). This of course is nonsense; it is just the kind of mistake that occurs if a manuscript is copied: The copyist (in this case the person who wrote the manuscript in the CARCAVY collection) just looked into the wrong line (compare (FERMAT 1922, 76 line 19–24)), and then the mistake was copied by those who made the Groningen and the Florence copies. Finally the mistake was translated into Latin and then printed in (FERMAT 1679). Before the Florence manuscript was written, FERMAT had at least inspected the papers prepared by CARCAVY for edition (FERMAT 1922, XVIII–XIX, XXI–XXII). Thus if he really had read the manuscript word for word, he necessarily would have noticed this obvious mistake. We are led, therefore, to conclude that FERMAT just glanced at the manuscript, and that he did not read it carefully.

This can be confirmed by another example. In (FERMAT 1922, 80 line 5–6) we read "entre le point O et le concours de la tangente". But again, this is not what the sources give. The Groningen as well as the Florence manuscript have instead "entre le point V pris a discretion entre le point O et le concours de la tangente" (FERMAT 1922, 175). This is nonsense, and again it is easily seen that the mistake was made because the copyist (the one who wrote the manuscript in the CARCAVY collection) had looked at the wrong line (compare (FERMAT 1922, 80 line 8)). Later this mistake was translated into Latin and then printed in (FERMAT 1679) (compare (FERMAT 1891, 145, 424)). Hence this is another obvious mistake which FERMAT did not correct during his first inspection.

What about the second inspection? As the mistakes mentioned are printed in (FERMAT 1679), we may suspect that the second inspection was just as superficial as the first one. In any case, there are striking resemblances (FERMAT 1891, 423–427; FERMAT 1922, 164–168, 174–175) in the mistakes in copying between the Groningen collection, the Florence collection (with the exception of the VIVIANI copies) and (FERMAT 1679). Thus we may take it for granted that these three sets of papers derive from a common source, and this common source evidently is the CARCAVY collection (FERMAT 1922, XVII–XIX, XXI–XXII). It seems to be probable that SAMUEL DE FERMAT, the editor of (FERMAT 1679), had access to the CARCAVY collection (FERMAT 1891, XVII). If the second inspection had been more careful, then there should have been corrections in the CARCAVY collection and therefore also in (FERMAT 1679).

In conclusion, the third counterargument is refuted. I would like to add that my previous argument given in the discussion of the first oddity is still valid: If FERMAT had changed his mind, then of course we would expect him to have corrected his earlier papers in order to achieve a coherent edition. But if someone else had changed the text, then FERMAT would not know about it.

11. The Tract on Quadrature

The fourth and last counterargument refers to the tract on quadrature in the beginning of which there is an explicit definition: "adaequetur, ut loquitur Diophantus, aut fere aequetur" (FERMAT 1891, 257). This plainly contradicts my hypothesis. I wish to argue that someone else made changes in the text, for he did not understand the text and desired to clarify it.

First of all, the use of adaequare in the tract on quadrature is incoherent. The word is used four times to indicate an approximate equality (FERMAT 1891, 257 line 9, 258 line 23, 259 last line, 263 line 1), but once it is used (FERMAT 1891, 259 line 2) to indicate an equality. This fact has to be explained; up to now, the only explanation given just is that FERMAT was careless.

The tract on quadrature is known to us only by the printed version in (FERMAT 1679, 44–57). There are several indications that the editor SAMUEL DE FERMAT did not have a manuscript written in FERMAT's handwriting at his disposal but rather a copy. There are several mistakes which seem not to be a printer's error but rather due to a copyist's error (FERMAT 1679, 46, 50, 51; FERMAT 1891, 432–433, 259 line 22, 272 line 32, 273 line 1, 273 footnote). Furthermore, Cartesian notation for equality is used (FERMAT 1891, 432, 269 line 14) and there are other indications for a change in the notation (FERMAT 1891, 283 footnotes). Notation is an argument, because SAMUEL DE FERMAT did not do much in preparing the edition. Some papers are printed in VIÉTE's notation, some in Cartesian notation. Sometimes the notation even changes within the same paper: We find for example within two lines ^2E as well $''E$ for the product of 2 and E (FERMAT 1679, 63; FERMAT 1891, 423, 134 line 13, line

17). A very rare symbol for equality (CAJORI 1928, 301, 304) is to be found occasionally (FERMAT 1679, 2, 72; FERMAT 1891, 419, 92 line 22, 432, 164 line 12, line 14, line 16), but then on the same and the next page, respectively, we find Cartesian notation for equality as well as "aequabitur". These details are also to be found in the Florence and partly in the Groningen copies (FERMAT 1922, 161, 164, 168). The general impression is that SAMUEL DE FERMAT just gave to the printer what had been given to him. Therefore if a paper in (FERMAT 1679) is not given in VIÈTE's notation, it is very probable that SAMUEL DE FERMAT did not have a manuscript written in FERMAT's handwriting.

Leaving philological reasoning, let us turn to mathematics. The definition of adaequare quoted above refers to DIOPHANTUS (FERMAT 1891, 257), but this is evidently wrong, if my interpretation of the passages in DIOPHANTUS is correct. Furthermore, this reference to DIOPHANTUS plainly contradicts the reference to DIOPHANTUS in the first paper given to DESCARTES (FERMAT 1891, 133). But we are not left with no more than that; there is more evidence to raise suspicions.

FERMAT's general idea in the tract on quadrature is derived from the Archimedean method of the quadrature of the parabola. For a full understanding of the sketchy tract on quadrature, it is necessary to compare this tract with the mathematical style of reasoning given in the tract on rectification (FERMAT 1660a), which was written more or less at the same time (MAHONEY 1973, 409–410) and, as it was published by FERMAT himself, is more polished than the tract on quadrature. I agree with ZEUTHEN (1903, 295) in the statement that the tract on rectification is to be used as an example of how FERMAT meant that proofs should be done. To understand FERMAT, it is useful to note that he gave at first an abbreviated argument and then added a complete proof in the manner of ARCHIMEDES for those who considered the abbreviated version to be too concise (FERMAT 1891, 222–225). This apagogical proof is printed in italics in (FERMAT 1660a, 12–15), although neither in (FERMAT 1679) nor in (FERMAT 1891). Evidently FERMAT accepted the Archimedean standard for mathematical rigour, but on the other hand, he considered an explicit apagogical proof to be rather superfluous for an able mathematician. Thus an expert might skip the routine argument given in italics.

In the tract on quadrature, the abscissa axis is divided in geometrical progression. That leads to circumscribed and inscribed rectangles, which themselves make up another geometrical progression. Finally an apagogical proof in the manner of ARCHIMEDES would show the equality between the area under a curve (namely a higher parabola or a higher hyperbola) and a certain rectangle. FERMAT states this once and for all: "quod semel monuisse sufficiat" (FERMAT 1891, 257). With the exception of this passage, FERMAT in the tract never mentions again inscribed rectangles. It would have been awkward to write down all necessary details, particularly as there was no index notation at FERMAT's disposal. In any case, it is easy to see for the expert mathematician which analogous relations hold for the inscribed rectangles. Moreover, FERMAT skipped the final step, the apagogical proof, completely. If someone who did not understand the Archimedean method read the text (the question whether such a person existed will be discussed later), he would have got the impression that the sum of the circumscribed rectangles was nearly equal to the area under the curve, and that this was sufficient to establish the quadrature of the curve. It is absolutely certain that FERMAT never thought this. His tract on rectification (FERMAT 1660a) and a comment on a proof given by PASCAL (FERMAT 1894, 438–440) provide sufficient evidence for that, if evidence is needed at all. Nevertheless, there is a passage in the tract on quadrature (FERMAT 1891, 258 line 23–24) which exactly claims that the sum of the circumscribed rectangles can be replaced by the area under the curve, this being valid "ex adaequatione Archimedea". Had we not found this contention in a tract written by FERMAT, we would reject it at once as a silly remark. Moreover, given the definition of adaequare in the beginning of the tract, it is particularly strange to be told here that the method of ARCHIMEDES is based on an approximate equality. It seems that these two lines were not written by FERMAT. This conclusion is supported by the fact that the tract proceeds as if these two lines had not been written, and after some more lines the result, namely the equality between the area under the curve and a certain rectangle, is stated again (FERMAT 1891, 259, line 1–7), but this time in a more appropriate way, and with a reasonable argumentation replacing or abbreviating the apagogical proof. Moreover, this passage uses the word adaequare also, but it is interesting to see that adaequare this time indicates an equality. Thus the statement of the correct result plainly contradicts the definition of adaequare as "to be nearly equal" (FERMAT 1891, 257) as well as the strange statement "ex adaequatione Archimedea" (FERMAT 1891, 258).

There are two more passages in the tract on quadrature using the word adaequare. It is contended (FERMAT 1891, 259 last line) that the ratio $(1 + q):q$ of two line segments on the axis may be replaced by $2:1$ "ex adaequatione". The reason is given that the line segments are nearly equal. Evidently, a somewhat more complicated reasoning should be given, although the result is correct. It seems hard to believe that FERMAT could have stated the reason which is to be found in the text, although it would fit perfectly well if FERMAT had given no argument at all, the idea being obvious for an expert mathematician. Some pages later (FERMAT 1891, 263 line 1), the same thing occurred again. The claim is made that the ratio $(q(q + 1) + 1):q(q + 1)$ between two line segments equals $3:2$ "propter adaequalitatem et sectiones minutissimas". In addition the reason is given that the intervals x and xq on the axis are nearly equal. Again, this is a curious way to express things, particularly for FERMAT, his mathematical style being quite different in the tract on rectification, which was written at the same time.

Now the story on CARCAVY is to be continued. We know for certain that CARCAVY did not understand the method of ARCHIMEDES. In 1659, CARCAVY sent PASCAL's proof about the equality of the arc lengths of an Archimedean spiral and a parabola to HUYGENS (PASCAL 1658; HUYGENS 1889, 365). HUYGENS expressed some doubts (HUYGENS 1889, 412, 458, 474), and CARCAVY communicated these doubts to FERMAT, because PASCAL's health was bad. FERMAT (1894, 438–440) supplemented the apagogical proof which PASCAL had skipped because he considered it to be a routine argument. In sending this supplement to

HUYGENS, CARCAVY added, that he perhaps had believed too quickly in the correctness of PASCAL's result. This is strange enough, but moreover, he added: "Et Je ne croyois pas qu'il fallut tant de discours pour en faire uoir l'Euidence." (HUYGENS 1889, 534). Evidently CARCAVY did not understand the proof technique of ARCHIMEDES. (By the way, HUYGENS's problem was different (HUYGENS 1890, 26–28).)

Probably CARCAVY had given the tract on quadrature to SAMUEL DE FERMAT (FERMAT 1891, XVII, 360); therefore the text known to us probably derives from a copy in the possession of CARCAVY. CARCAVY just saw that the sum of the circumscribed rectangles and the area under the curve were nearly equal and that some commentary might be useful. To CARCAVY, adaequare was the perfectly appropriate magic word which was needed in order to transform an approximate equality somehow into an equality. After all, FERMAT had not criticised CARCAVY's additions in the paper "Je veux par ma méthode". CARCAVY might have felt justified in adding another reference to DIOPHANTUS and some more words (all passages containing the word adaequare with the exception of (FERMAT 1891, 259 line 1–2)). If he did so, he probably believed that he had done a good job in clarifying the tract on quadrature.

Another historical fact should be reported. CHRISTIAAN HUYGENS (1693, 326–355; 1901, 95; 1908, 19, 46–49; 1910, 60–68; 1940, 228–255) dealt several times with FERMAT's method, he even gave a lecture on FERMAT's method in the Académie des Sciences two years after FERMAT's death. Even if CARCAVY was not present, he could have read the text afterwards in the Registres, and he certainly would have done so, because FERMAT had been his friend and he was still charged with editing FERMAT's papers (FERMAT 1891, I, XIII, 359–361). In this lecture, HUYGENS not only used infinitesimal quantities, but also he claimed that this was the method of FERMAT (HUYGENS 1940, 232). The statement of the famous mathematician HUYGENS would probably impress CARCAVY strongly. If he had already changed some passages in the tract on quadrature, he would have felt justified; if not, he would have felt encouraged.

12. Conclusion

If we allow for changes having been made in only two papers (to be more precise: the addition of three sentences and some more words), a clear and coherent understanding of FERMAT's papers on his method of extreme values and FERMAT's tract on quadrature is possible. The mysteries of adaequare are not due to FERMAT's supposed confusion, but to the traditional dogma of FERMAT interpretation, which I believe, should be abandoned. As FERMAT (1894, 56) claims, the method of extreme values is based on an equality and not on an approximate equality.

Finally I should like to mention some interesting and important problems the solution of which was not included in the aim of this article. FERMAT (1894, 72, 176; MAHONEY 1973, 286) repeatedly claims that his method is also useful in number theory, but up to now, no suggestion has been made about this.

Another problem is the interpretation of FERMAT's letter to BRÛLART (FERMAT 1922, 120–125); the letter poses some difficulties the solution of which would be helpful in order to find out whether FERMAT possessed a proof of his method. Furthermore, a general discussion of the authenticity of the FERMAT papers would be valuable. For example, such a discussion would presumably lead to the conclusion that changes have been made in the two texts considered in the section on the first counterargument. My discussion of these two papers did not aim at the question of authenticity, but only whether these papers are compatible with my hypothesis. Finally, it is an interesting question why some very important papers (FERMAT 1891, 147–153, 153–158; FERMAT 1922, 120–125) were not printed in (FERMAT 1679). Did someone make a choice? In any case, there still is much to be done concerning the FERMAT papers.

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