On the History of Fermat's Last Theorem: A Down-to-Earth Approach

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.1 Introduction .... - 1 -
2. Origins – in the margins .... - 5 -
3. Early stages – in the margins as well .... - 8 -
4. FLT between 1800 and 1855 – still in the margins .... - 13 -
5. Marginality within diversification in number theory (1855-1908) .... - 22 -
6. The Wolfskehl Prize and its aftermath .... - 34 -
7. FLT in the twentieth century: developing old ideas, discovering new connections .... - 44 -
8. Concluding Remarks: the Fermat-to-Wiles drama revisited .... - 56 -
9. REFERENCES .... - 60 -

1. Introduction

Andrew Wiles' proof of Fermat's Last Theorem (FLT), completed in 1994, was a landmark of late twentieth century mathematics. It also attracted a great deal of attention among both the media and the general public. Indeed, not every day a mathematical problem is solved more than 350 years after it was first posed, and not every day the work of a pure mathematician makes it to the front page of the New York Times.

On the margins of his own copy of Diophantus' *Arithmetic* Fermat had written down the result sometime after 1630: it is not possible to write a cube as the sum of two cubes or a bi-square as the sum of two bi-squares, and in general, it is not possible that a number that is a power greater than two be written as the sum of two powers of the same type. "I have discovered a truly marvelous proof, which this margin is too narrow to contain...", Fermat famously wrote. The proof of a result so easy to enunciate turned out to be enormously elusive. Wiles' was an extremely complex and lengthy proof that used sophisticated mathematical techniques drawing from a wide variety of mathematical domains. One can be certain that this is not the proof that Fermat thought to have had.

The media coverage of Wiles' work on FLT offered to the nonmathematical public an unprecedented opportunity to become acquainted with the arcane world of research in number theory. Doubtlessly, Simon Singh took the lead in this popularization effort when presenting his BBC TV program (in collaboration with John Lynch) and his best-seller *Fermat's Enigma*. The cover of this book described the history of FLT as "the epic quest to solve the world's greatest mathematical problem." The reader is told that the problem "tormented lives" and "obsessed minds" for over three centuries, thus constituting "one of the greatest stories imaginable". On the front flap we read that FLT became the Holy Grail of mathematics and that Euler, the most outstanding mathematician of century XVIII, "had to admit defeat" in his attempts to find a proof. Moreover:

> Whole and colorful lives were devoted, and even sacrified, to finding a proof. … Sophie Germain took on the identity of a man to do research in a field forbidden to females … The dashing Evariste Galois scribbled down the results of his research deep into the night before venturing out to die in a duel in 1832. Yutaka Taniyama … tragically killed himself in 1958. Paul Wolfskehl, a famous German industrialist, claimed Fermat had saved him from suicide.

"The Last Theorem", we read in the opening passage of the book, "is at the heart of an intriguing saga of courage, skullduggery, cunning, and tragedy, involving all the greatest heroes of mathematics."

Descriptions of this kind have done much to enhance the dramatic qualities of the story, and to help attract the attention of the lay public to the discreet charms of mathematics. Indeed, for his laudable effort in popularizing the story of FLT in 1999 Singh was bestowed a special award by the American Mathematical Society.[1] But as the above quoted passage suggests, even a cursory reading of the book shows that 350 years of history of FLT were enormously over-dramatized in this account. Of course, this over-dramatization may be easily explained away by alluding to its intended audience and aim. Still, I think that the historiographical approach underlying Singh's book actually helps identifying some shortcomings that have affected many previous,

[1] *Notices of the AMS* 46 (5), 568-569.

both scholarly and popular, accounts of the history FLT (some of which possibly provided Singh with sources of information). It is not only that suicides, transvestism, duels at dawn and deception were never part of this mathematical tale, and that the "great heroes of mathematics" actually devoted very little attention to FLT, if they did at all (Galois, to be sure, devoted none). It is that since the time of Euler, when this theorem aroused curiosity among prominent mathematicians, it was mainly in a passive, and typically ephemeral way that did not elicit their own active participation, and thus— after its formulation by Fermat and before the last stage that culminated in the work of Wiles—serious work was devoted to it only seldom.

In the present article I would like to outline the history of FLT from a perspective that seems to me to fit the historical record more adequately, by putting forward a down-to-earth, essentially non-dramatic account.[2] This is not intended as a comprehensive exposition of the main mathematical results that were accumulated throughout generations of research in number theory in either direct, or indirect, or merely incidental relation with FLT. Nor is this an attempt to offer a general account of the ideas necessary to understand Wiles' proof and the related mathematics.[3] Likewise, in this article I do not intend to come up with a personal evaluation of the intrinsic mathematical importance of FLT (that would be unimportant and irrelevant). Rather, this historical account analyzes the changing historical contexts within which mathematicians paid more or (typically) less attention to FLT as part of their professional activities. Some of the facts around which I build the story (especially in sections 2,3 and 4, covering the period between Fermat and Kummer) have been previously discussed in the secondary literature. Here I revisit those discussions with a somewhat different stress, while connecting them as part a specific historical thread that is visible from the perspective I suggest. Other facts that are part of this thread have been essentially overlooked thus far and I discuss them (especially in sections 5,6 and 7, covering activity in number theory in the nineteenth and twentieth century.)

[2] For a recent discussion of narrative strategies (and dramatic vs. non dramatic approaches) in the historiography of mathematics, see [Corry 2008].

[3] Interested readers can consult, among others, [Edwards 1977], [Hellegouarch 2001], [Ribenboim 1980, 1997], [Van der Poorten 1995].

In particular, section 6 discusses the surprising role of the Wolfskhel prize as a pivotal point in the story, while section 7 describes the way in which some deep changes that affected the discipline in number theory over the twentieth century offered new perspectives from which to consider FLT as part of more general families of problems to which increasingly significant research efforts were devoted.

As already suggested, the reader will not find here entire mathematical lives dedicated to the theorem, and only three cases (with varying degrees of intensity and length) of "partial-life" dedication: Sophie Germain, Harry Schultz Vandiver, and Wiles. Certainly, theorem-related suicides are not part of this story. Rather, we will come across ingenious ideas (some of them, but rather few, with broader, long-term repercussions on the theory of numbers), intermittent curiosity and, mainly, mathematicians occupied with all kinds of problems close to, or sometimes distant from, FLT, with occasional incursions and furtive attempts to solve the problem. As will be seen here, an important virtue of the history of FLT, simply because of the time span it covers, is that it affords a useful point of view from which to follow the deep changes undergone by the entire field of number theory from the time of Fermat to our days.

The inherent difficulty involved in a prospective proof of FLT is nowadays more evident than ever. This provides a possible argument to explain, in retrospect, the relative scarcity of evidence indicating *significant and lasting* activity directly related to FLT that I will discuss in what follows. Thus, one possible counterargument to be adduced is that mathematicians whose work is discussed here *did* consider FLT as a truly important problem and/or *did* devote at times much effort to attempt to solve it, but since this effort did not lead to any real breakthrough (and now we realize that it could not lead to a breakthrough at the time) nothing of this was published, in spite of the interest and effort devoted. It might also be claimed that in spite of the intrinsic importance conceded by many to the problem, prominent number theorists immediately sensed the real magnitude of the difficulty and only for this reason decided to redirect their time and efforts towards different, more potentially rewarding aims. Lack of evidence, under these arguments, does not indicate lack of interest or even lack of activity. Such arguments, I admit, are plausible and they are hard to refute. Indeed, I believe that they may perhaps hold true (at least to some extent) for some of the mathematicians discussed here or for some whom I did not discuss. And yet, I think that the positive evidence that I do present and the overall picture that arises from my account below produce a sufficiently coherent case that supports my interpretation that lack of evidence was related to lack of activity, or at least *serious* interest and activity.

Wiles' achievement involved a proof of the semi-stable case of the Taniyama-Shimura conjecture (TS) that by then was known to yield FLT as a corollary. This was a truly mathematical *tour of force*. The story of his own personal quest to prove FLT is indeed dramatic at two levels: his interest in the theorem ever since childhood that, after a long hiatus of decades, eventually culminated in the great proof, and the years of seclusion that led to a proof in which an error was eventually found that required extra work before completion several months later. But the earlier chapters of the story were much less heroic: the historical record shows that FLT was born on the physical margins of a book and that throughout more than 350 years it essentially remained at the margins of the theory of numbers, with occasional forays into the limelight and a truly amazing *grand finale* in the work of Wiles.

2. Origins – in the margins

Number-theoretical questions started to occupy the mind of Fermat sometime around 1635 under the influence of Marin Mersenne (1588-1648). While studying Diophantus' *Arithmetica*, in a Latin translation prepared in 1621 by Claude Bachet (1581-1638), Fermat came across the eighth problem of Book II, typical of those formulated and solved by Diophantus. It requires to write a given square number as a sum of two square numbers. On the margins of his copy, Fermat wrote down the famous claim about the impossibility of doing something similar for cubes, bi-squares, or any higher power, and about having a remarkable proof of this fact which, unfortunately, could not be written in the narrow margins of the book. This is the famous origin not only of what became a long mathematical quest, but also of the dramatic character often associated with the story. A less dramatic approach for the part of the story directly related to Fermat, however, arises from a close reading of several existing historical accounts of this mathematician's overall contribution to number theory [Goldstein 1995; Mahoney 1984; Scharlau & Opolka 1985, 5-14; Weil

1984, 37-124]. I rely here on these accounts to recount briefly the initial stages of the story of FLT within the more relaxed perspective I suggest for the entire narrative associated to the history of the theorem.

It was Fermat's habit to write on the margins of books, particularly his copy of the *Arithmetica*, unproven results, general ideas, and possible mathematical challenges. He also communicated ideas in letters to a group of correspondents with like interests, but he never kept copies of such letters. Except for one well-known case, mentioned right below, he never explained his method of proof for an arithmetical result. It is thus hard to determine with certainty in all cases which results he had really proved, and which he had asserted on the basis of accumulated numerical evidence. Most likely, in most cases, he did have a clear idea of a proof when he so declared, but since he seldom wrote them down, some of these may have been occasionally erroneous. The best known example of this is the one related with the so-called Fermat integers, of the form $2^{2^n}+1$. In letters written between 1640 and 1658, Fermat expressed his conviction that such numbers would be prime for any n, but at the same time he confessed not to have a proof of that fact. And yet in a letter sent in 1659 to Pierre de Carcavi (1600-1684) but actually intended for Huygens, Fermat suggested to have a proof of this conjecture based on the "method of infinite descent" [Fermat *Oeuvres*, Vol. 2, 431-436]. Eventually, however, in 1732 Euler famously showed that for $n = 5$, the corresponding Fermat number is not a prime.

In the case that concerns us here, the marginal note that later became known as FLT was written close to the awakening of Fermat's interest in this kind of questions, and there is no evidence that he ever repeated in writing the conjecture for the general case. In contrast, he explicitly challenged in 1636 a correspondent to prove (among other problems) cases $n = 3$ and case $n = 4$ of the conjecture [*Oeuvres*, Vol. 3, 286-292]. Also in the 1659 letter to Carcavi, Fermat claimed to have used the method of descent for proving several results, among which he mentioned the case $n = 3$ of his conjecture. It seems thus safe to assume that Fermat soon spotted an error in what he initially thought to be a correct proof for the general case, and there was no reason to publicly correct a statement advanced only privately. At any rate, this was only one of many other conjectures he proposed and discussed.

The method of infinite descent is applied in the one proof of an arithmetical result that Fermat indicated in detail, which is another result inspired by Diophantus. This time, the margins of the *Arithmetica* were wide enough to contain the argument [*Oeuvres*, Vol. I, 340-342]. The result states (symbolically expressed) that there are no three integers x,y,z that satisfy the formula

$$x^2 + y^2 = z^2$$

and for which, at the same time, $xy/2$ is a square number.[4] Interestingly, Fermat's proof of this fact also proves FLT for case $n = 4$, since it proves that $z^4 - x^4$ cannot be a square, and hence certainly not a bi-square [Edwards 1977, 10-14; Weil 1978, 77-79]. Fermat was certainly aware of this implication [*Oeuvres*, Vol. 1, 327]. Of course, the case $n = 4$ is important for a fundamental reason: if FLT is true for $n = 4$, then it is clear that in order to prove the general case it is enough that FLT be valid for prime numbers.

In Fermat's 1659 letter, the impossibility of writing a cube as a sum of two cubes appears as part of a list of several number-theoretical, general results that Fermat considered worth of attention, and that could be solved using infinite descent. Fermat stated that they summarized his "fantasies on the subject of numbers" but at the same time he made it clear that is just one of an infinity of questions of this kind. Among the most interesting and challenging of these, Fermat singled out the representability of any number as a sum of up to four squares, a problem that also Descartes considered one of the most beautiful of arithmetic and one whose apparent difficulty prevented him from even trying to search for a proof [Scharlau & Opolka 1985, 9]. The other items in Fermat's list were fundamental in leading to the eventual development of the theory of quadratic forms. The general case of the conjecture that became FLT does not appear in the list. Except for his circle of correspondents Fermat felt a total lack of interest on the side of his contemporaries (especially British mathematicians) on questions of this kind, and he decided to provide information about his method and this list of problems for fear that they be lost to later generations as he would not to find "the leisure to write out and expand properly all these proofs and methods" [Weil 1984, 118].

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[4] [Goldstein 1995] contains an intreresting and detailed account of the history of this result and how it was read, developed and transmitted by contemprary and successive generations of mathematicians.

This very brief account should suffice to provide a fair idea of the very limited interest that the marginal conjecture had within the overall picture of Fermat's arithmetic endeavors. Eventually, a large number of his achievements became publicly known thanks to his son, Samuel. In 1670 Samuel Fermat published an edited version of Bachet's Latin translation of Diophantus' *Arithmetica*, including comments and related letters by his father. Through this edition the world became aware not only of what became FLT, but also of many other ideas.

3. Early stages – in the margins as well

After Samuel Fermat had published in 1630 Diophantus' *Arithmetica* with his father's comments, and until the mid-eighteenth century, few mathematicians ever attempted to solve any of the problems proposed by Fermat, and possibly no one attempted to solve what became FLT. 1753 is the year when Leonhard Euler (1707-1783) enters the story. The amount and scope of Euler's scientific output is astonishing. He was involved in, and significantly contributed to, all fields of mathematics and physics in which research was conducted at that time. This includes, of course, what we know today as the theory of numbers. It is important to stress, however, that in Euler's time there was actually no mathematical field called "number theory". Rather, there was a heterogeneous collection of more or less complex problems and techniques that were never fully systematized, and that were associated with the field of "arithmetic". In fact, the term "higher arithmetic" remained in use until the nineteenth century (and even thereafter), which is the time when the term "number theory" started to be widely adopted. Moreover, the period of time between Fermat and Euler is precisely the time of the rise and early development of the calculus. It seems quite evident that during this time, it was the new calculus that attracted, above all, most of the energies of the leading mathematicians. Relatively little attention was devoted at the time to this higher arithmetic.

This assertion is also true of Euler, but at the same time it is also true that he kept a lively interest in arithmetic in stark contrast to his predecessors and contemporaries, and that he achieved important results in this field. Fermat's legacy of unsolved problems was no doubt an important factor in directing Euler's interest to this field [Weil 1984, 160-274]. Euler solved many of these open problems, but some others he did not, or could not, solve. FLT was one of the latter class, but definitely not the only one. Thus, for instance, in 1770 Joseph Louis Lagrange (1736–1813) solved a different open problem proposed by Fermat, and that Euler had unsuccessfully attempted to prove, namely, that any integer can be written as the sum of no more than four square numbers.

It was mostly in the correspondence with his friend Christian Goldbach (1690-1764)—famous for his (still unproven) conjecture on the possibility of writing every even number as the sum of two prime numbers—that Euler discussed Fermat's ideas. This correspondence started in late 1729 and lasted thirty-five years. Euler and Goldbach discussed above all problems related with mathematical physics and infinite series and integration. They also discussed some higher arithmetic, especially concerning possible representations of numbers as specific kinds of sums of powers. Fermat's number-theoretical ideas do appear occasionally in these letters, but barely in connection with what became FLT. From the first letters, whenever the name Fermat appears, it is mainly in connection with the question of the Fermat numbers. And 1753, only twenty-four years after the correspondence started, is the first time when Fermat's marginal conjecture comes up, in the last half page of a letter where many other ideas were also discussed.

In that letter, Euler told Goldbach about this "very beautiful" theorem of Fermat. He claimed to have found proofs for the cases $n = 3$ and $n = 4$, and he added that the proofs of these two cases were so essentially different from each other, that he saw no possibility to derive from them a general proof. Moreover, it was clear to him that the higher one went in the powers, the more clearly impossible the validity of the formula $x^n + y^n = z^n$ would appear. Also, he had not been able so far to find the proof for $n = 5$ [Fuss 1843, Vol. 1, 618]. That's all he had to say about this problem. From a further letter of 1755, we learn that he was sure that Fermat did have the general proof, and stated again that his own efforts in this regard so far had been in vain [Fuss 1843, Vol. 1, 623]. In 1770 Euler published a textbook on algebra, and his proof for the case $n = 3$ appeared there. Although highly ingenious, it contained a non-trivial mistake [Euler 1770]. The idea needed in order to correct this proof does appear in other places in his work [Euler 1760], and thus, it has been common to claim, with some degree of justification but not with full historical precision, that Euler was able to solve the case $n = 3$ of FLT.

But there is another interesting point to look at in the last few sentences of Euler's letter of 1753. Euler added here his own idea, different to Fermat's, on how the original problem of Diophantus could be generalized. Since the equations $a^2 + b^2 = c^2$ and $a^3 + b^3 + c^3 = d^3$ do have integer solutions, then it would seem to appear that $a^4 + b^4 + c^4 + d^4 = e^4$ should also have such solutions. Euler had not yet found four integers that satisfy the equation, but he had found five bi-squares summing up to a bi-square. Then, in 1778 he referred once again to FLT and suggested a further generalization (quoted in [Dickson 1919, Vol. 2, 648]):

> It has seemed to many Geometers that this theorem may be generalized. Just as there do not exist two cubes whose sum or difference is a cube, it is certain that it is impossible to exhibit three biquadrates whose sum is a biquadrate, but that at least four biquadrates are needed if their sum is to be a biquadrate, although no one has been able up to the present to assign four such biquadrates. In the same manner it would seem to be impossible to exhibit four fifth powers whose sum is a fifth power, and similarly for higher powers.

Fermat's generalization was one way to go, and here Euler simply thought of a different possible one. Who could have told at the time which of all of these ideas would be the most interesting and fruitful one for mathematics? As a matter of fact, this latter conjecture by Euler, on the impossibility of finding three biquadrates whose sum is a biquadrate, had a beautiful history of itself, and it was only in 1988 that Noam Elkies at Harvard found the following, truly amazing counterexample to the conjecture [Elkies 1988]:

$$2{,}682{,}440^4 + 15{,}365{,}639^4 + 18{,}796{,}760^4 = 20{,}615{,}673^4.$$

A detached inspection of the historical record thus shows that while Euler may have certainly felt challenged by any problem in higher arithmetic that he was not able to solve, we can hardly think that FLT represented a pressing question of particular mathematical interest that called for an urgent solution on his side. Let us not imagine, then, of an Euler calling a press conference to "admit defeat" in proving FLT.

Nor had Euler many immediate successors that intensively pursued his interests in number theory in general. Of the great mathematicians of the following generation, only Lagrange, Euler's successor at the Berlin Academy of Sciences, can be singled out as having developed a real interest in the field. To be sure, however, Lagrange made no contribution to FLT nor expressed any interest in the problem. In 1786, Lagrange moved to Paris, and by that time he was no longer active in number theory.

One of the younger colleagues he met there was Adrian Marie Legendre (1752-1833), whose earlier work on ballistics Lagrange knew well. Legendre started now to publish interesting work on number theory, inspired by that of Euler and Lagrange [Weil 1984, 309-338]. In 1798 he published a comprehensive treatise on the field as known to him at the time, and this treatise would see several re-editions until 1830. The first edition included proofs of FLT for the exponents $n = 3$ and $n = 4$.

Legendre's treatise is an interesting source of information not just about the specific number theoretical results known at the time of publication of its various editions, but even more so about the evolving, overall image of the discipline with its main open problems, its internal organization into sub-topics, and its relevant techniques and tools along the years. The preface to the first edition [Legendre 1798], for instance, provides a clear roadmap of the field, updated to the end of the eighteenth century. Fermat appears here as a main hero, and Legendre stresses that if he had concealed proofs of the results he achieved, this was not only in order to secure his own personal successes, but also for that of his nation, because "there was a rivalry above all between the French and English geometers". Euler's contributions, of course, were also central, and especially since they led to the proof of two of Fermat's "principal theorems". By this Legendre did not mean FLT, but rather the following: "(1) that if a is a prime number, and x is any number not divisible by a, then the formula $x^{a-1} - 1$ is always divisible by a; (2) that every prime number of the form $4n + 1$ is the sum of two squares." Legendre also mentioned the other remarkable results found in Euler's works, among which the proofs of two cases of FLT are just one more item. In the entire list:

> One finds the theory of divisors of the quantity $a^n \pm b^n$; the treatise *Partitione numerorum* …; the use of imaginary and irrational factors in the solution of indeterminate equations; the general solution of indeterminate equations of second degree, under the assumption that a particular solution is known, the proof of various theorems about the powers of numbers and, in particular, of those negative propositions advanced by Fermat that the sum or difference of two cubes cannot be a cube and that the sum or difference of two bi-squares cannot be a bi-square.

Legendre also praised Lagrange for his achievements, and the only one that related Fermat's legacy was the proof of the solvability of the equation $x^2 - Ay^2 = 1$, and the techniques developed around it. In Legendre's view, the most remarkable of Lagrange's discoveries in this field was a general and "singularly fecund" method, from which "an infinity" of other results would follow, and that related the treatment of linear and quadratic forms as applied to questions of prime numbers.

As for his own works, Legendre mentioned various results concerning quadratic forms, the reciprocity law, and the application of these two to perfecting Lagrange's techniques. One application of his results that Legendre singled out as being particularly important was the solution of "the famous theorem by Fermat", that any number can be written as the sum of three triangular numbers, and another theorem by the same author stating that every prime number of the form $8n + 7$ is of the form $p^2 + q^2 + 2r^2$. Indeed, in the body of the book, when Legendre speaks about "Fermat's general theorem" he means the "polygonal number theorem", a polygonal number of order $m + 2$ being a number of the form $\frac{m}{2}(x^2 - x) + x$. Fermat had conjectured around 1638 that every integer n can be written as the sum of at most $m + 2$ polygonal numbers of order $m + 2$. This is the conjecture that Euler did not succeed in proving and for which Lagrange provided in 1770 a proof of the simplest case (i.e., that every non-negative integer is the sum of four squares). In 1816 Cauchy would prove the conjecture for $m > 2$, and Legendre would eventually include in the third edition of his treatise a refinement of Cauchy's proof [Nathanson 1987]. In the first edition Legendre just sketched a proof for triangular numbers.[5] Interestingly, he speaks in this section about Fermat's putative proof of this conjecture in a way similar to that used only much later to speak about FLT. Thus, Legendre explained why, in his view, it is clear that if Fermat were aware of ideas such as developed later by Lagrange, he would have most likely formulated his conjecture in a different way [Legendre 1830, 350]. Immediately after the section devoted to this theorem, which for Legendre was the most important one in Fermat's legacy, he presented the proof of FLT for $n = 3$ and did not add much comments to it.

It was only gradually, as more and more open problems related with Fermat's legacy were solved, and as FLT progressively became his "last" unproven conjecture, that this problem started to receive some specific attention, even though this attention did not derive from any special, intrinsic importance that could evidently be associated with the problem. Little by little, the fact that the problem had not been solved by

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[5] A full proof of this case appeared in 1801 in Gauss's *Disquisitiones Arithmeticae*.

others added increased mathematical charm to it. More than one hundred and fifty years after the famous marginal remark, then, very few efforts had been actually devoted to solve it. By the end of the eighteenth century little importance was yet attributed to FLT, even within the framework of a book like Legendre's that was entirely devoted to the field of number theory, this field being of relatively little importance within the overall economy of mathematics at the time.

4. FLT between 1800 and 1855 – still in the margins

The first great codification and systematization of number theory at the beginning of nineteenth century was the monumental *Disquisitiones Arithmeticae* published in 1801 by Carl Friedrich Gauss (1777-1855). This book presented for the first time in a truly systematic fashion a great amount of results that were theretofore seen (even in Legendre's presentation) as a somewhat haphazard collection of separate problems and diverse techniques. *Disquisitiones* soon rendered Legendre's book obsolete. It had a momentous, though far form straightforward, influence over what the discipline of number theory would become over the nineteenth century and beyond [Goldstein & Schappacher 2007]. It is for this reason natural that any account of FLT will raise the question of Gauss' attitude towards the problem. Gauss indeed gave some thought to the problem: in a letter posthumously published, he drafted a possible proof for the case $n = 5$, while stressing that the method he suggested might possibly be used also in the case $n = 7$ [Gauss *Werke*, Vol. 2, 390-391]. But at the same time, he was clear in his opinion about FLT, and this opinion he expressed, for instance, in a letter on 1816 to his friend, the astronomer Wilhelm Olbers (1758-1840) [Gauss *Werke*, Vol. 2, 629]: "I confess that Fermat's Last Theorem—he wrote—as an isolated proposition has very little interest for me, for I could easily lay down a multitude of such propositions, which one could neither prove nor disprove".

One of the main mathematical themes developed in the *Disquisitiones* pertains to the problem of "quadratic reciprocity", which for Gauss would provide a classical example of what is a truly important mathematical problem. Gauss devised no less than eight different proofs of the theorem of quadratic reciprocity [Lemmermeyer 2000, 9-11]. Indeed, there was an important reason for bothering that much with one and the same problem as in each of the proofs, Gauss hoped to find a way that will allow generalizing the problem to powers higher than two, and, indeed, he himself successfully solved the problem for the cubic and bi-quadratic cases [Collison 1977; Goldstein & Schappacher 2007].

Of special importance in his many attempts to deal with reciprocity was Gauss's introduction and study of a new kind of numbers, the so-called "Gaussian integers", namely, complex numbers of the form $a + ib$, where a, b are any two integers. In many respects, these Gaussian integers behave like standard integers (or "rational integers" as they became known starting with the work of Dedekind). While introducing this idea as a tool for investigating reciprocity properties, Gauss was also interested in investigating how far the analogy between rational integers and Gaussian ones could be extended. The first to use arguments involving complex numbers in order to prove theorems about integers was Euler, precisely in investigating the kind of questions mentioned above, and in particular in his alleged proof of case $n = 3$ of FTL. But it was Gauss who systematized it and who made this possibility widely known among mathematicians while developing the Gaussian integers. Further attempts to generalize this idea would play an important role in the later development of number theory, and in particular of FLT as we will see in what follows. In his already mentioned letter to Olbers, Gauss claimed that developing a theory that would generalize the idea of Gaussian integers would no doubt lead to important breakthroughs and that FLT "would appear only among one of the less interesting corollaries" of such a theory. Nevertheless, in the letter to Olbers, Gauss does say that FLT caused him "to return to some old ideas for a great extension of higher arithmetic." What he meant by this is not really clear, given that by this time he was not involved anymore in research on number theory. One may conjecture, though, that he was referring to his correspondence, especially between 1804 and 1809, with Sophie Germain (1776-1831), a remarkable figure in this story.

The limitations that prevented at the time a woman from pursuing her intellectual interests in the framework of the leading academic institutions did not deter Germain from valiantly educating herself at home, using his father's well-equipped library, in a broad range of scientific topics. Aware that she would not be accepted into the newly founded Ecole Polytechnique, Germain sent a paper on analysis to Lagrange using the name of an acquaintance who was a registered student at the institution, Antoine-August Le Blanc. Positively impressed by the work, Lagrange asked to meet M. Le

Blanc. The surprise upon meeting the person behind the name did not diminish Lagrange's enthusiasm for the author's talents, and he continued to support Germain for many years. This kind of surprise would repeat itself in the near future with two other important mathematicians, as Germain developed an interest in number theory and initiated a correspondence with both Legendre and Gauss after studying in detail their treatises and coming up with new and original ideas in the field [Bucciarelli & Dworsky 1980].

The Le Blanc episode has repeatedly been rehearsed as part of the dramatizing trends associated with the FLT story. Singh, for instance, typically points out that Germain "had to take on the identity of a man to conduct research in a field forbidden to females". He does not qualify this statement by explaining that the field "forbidden to females" at the time was science in general, rather than anything specific connected with FLT. Another way in which the Germain story is seldom qualified is by duly pointing out that—for all the admiration she deserves as a woman who challenged contemporary social conventions in relation to gender, science and learning—when compared to other great feminine figures in the history of mathematics, her biography lacks the social or political interest of a Sofia Kovalevskaia (1850-1891) and by all means the mathematical brilliance and impact of an Emmy Noether (1882-1935) [Dauben 1985]. Still, she is certainly central to the FLT story as will be seen now. Indeed, she was the first person to devote sustained efforts to FLT and to come up with a reasonably elaborated strategy to attack the problem in its generality. This situation, however, only stresses the point I am trying to pursue in this article, and not because the intrinsic limitations of her strategy that become evident with the benefit of hindsight. Rather, my point is that Germain's focus on FLT is both a result of her institutional isolation and a strong evidence of the lack of systematic training and interaction with the main topics of contemporary research in higher arithmetic. Let me explain this point is greater detail.

As already said, a first stage of her correspondence on arithmetic with Gauss took place between 1804 and 1809. In 1808 Germain's scientific attention was diverted to a different field in which she gained recognition, even if in retrospective her work was essentially flawed: the theory of vibrations for elastic surfaces. The French Academy offered a prize for original research in this very difficult problem, and Germain presented three memories between 1811 and 1816. For the last one, in spite of some mistakes affecting it, she was awarded the prize. At the same time, in 1815 and then again in 1818 the French Academy in Paris offered a minor prize for the proof of FLT. This prize was never awarded, but apparently it brought Germain again to research in number theory. After more than ten years of silence she wrote to Gauss again in 1819 presenting some new ideas on FLT, and explaining that she had never stopped to think about number theory. Actually, she stated, she had been thinking on FLT long before the Academy established this new prize [del Centina 2005].

Gauss's replies to Germain's letters of 1804-1809 reflect admiration and respect, and these only grew upon his realizing the gender of his correspondent. We also know that he spoke to others about her with great esteem. Indeed, shortly before her death in 1831, Gauss convinced the University of Göttingen to award her an honorary degree, but unfortunately it turned out to be late. In her letters of 1804 to 1909, Germain mentioned some ideas about possible ways to solve FLT but she wrote mainly about the two main topics treated in *Disquisitiones*, namely reciprocity and quadratic as well as higher forms. In all of his replies to her, Gauss never ever referred to the points she raised in relation with FLT (see [Boncompagni 1880]). In her detailed letter of 1819 Germain stressed how she had used ideas taken from the *Disquisitiones* as the basis of her strategy to prove FLT. As soon as she had read this book, she wrote to him now, the connection between the Fermat equation and the theory of residues *vaguely* appeared to her. She also pointed out that she had already mentioned this idea to him a long time ago. Germain explained her ideas in detail in the letter and specifically asked Gauss about their possible importance. She was obviously anxious to hear Gauss's reply soon, when she entrusted this letter to his friend Schumacher who had come to visit. And yet, Gauss never answered her letter [del Centina 2008]. There may be many explanations for this, including lack of time and the fact that Gauss had estranged himself for many years from direct involvement with number theory. It seems to me, however, that Gauss' specific lack of interest towards FLT must have played a significant role as well.

Her interaction with Legendre was also rather complex and indicative of an ambiguous evaluation of the importance of her work. On the one hand, it was Legendre who published and stressed the value of some of her ideas, and thanks to his action her results sensibly influenced subsequent research on FLT. On the other hand, much of Germain's work remained unpublished and it is possible that even Legendre did not read it or at least did not attribute sufficient interest to them.[6] It is remarkable that in a letter of 1819 Legendre, possibly as a reply to a letter by her at that time, also wrote discouragingly [Stupuy 1896, 311]:

> I warn you that since I spoke to you for the first time about your approach, the opinion I had about its chances of success has now considerable weakened, and that, all things considered, I believe that it will be as sterile as many others. This is why I think you will make very well not to occupy yourself more with this, out of fear of wasting time which can be employed much more usefully with other research.

The central point in Germain's line of attack on FLT that turned out to be of signal importance for many subsequent attempts to prove the conjecture until the 1980s (though not for Wiles' eventual strategy) is related to the theorem that bears her name and the related, well-know separation of the problem into two cases. As already pointed out, a direct consequence of the proof of FLT for $n = 4$, is that the conjecture is proved once it is proved for all odd prime exponents. Also, it is easily seen that, without loss of generality, one may assume that x, y, and z are relatively prime. Germain divided the possible solutions to be investigated into two separate cases that were to become standard in the treatment of the problem thereafter, namely:

Case I – there are no three positive integer numbers x,y,z that satisfy $x^n + y^n = z^n$, and such that no one of them is divisible by n.

Case II – there are no three positive integer numbers x,y,z that satisfy $x^n + y^n = z^n$, and such that one and only one of them is divisible by n.

Germain's theorem can be formulated as follows:

Case I of FLT is true for an exponent n, if there is an auxiliary odd prime p for which the following two conditions hold:

(1) $x^n + y^n + z^n \equiv 0 \pmod p$ implies either $x \equiv 0 \pmod p$, or $y \equiv 0 \pmod p$, or $z \equiv 0 \pmod p$)

(1.2) $x^n \equiv n \pmod p$ is impossible for any value of x

Based on this result, Germain proved that Case I holds whenever n and $2n+1$ are both prime. She also proved additional conditions involving congruences among primes of

[6] Forthcoming - HM.

various forms and, based on these, performed detailed calculations, generating among other things values of the auxiliary prime l. She was thus able to prove that case I of FLT is valid for all prime exponents p smaller than 197.[7]

Case II turned out to be much more difficult. For $n = 5$, case II was proved only in 1825 in separate, complementary proofs of Legendre and Peter Lejeune Dirichlet (1805-1859). Dirichlet also proved in 1832 case II for $n = 14$, and he did so while trying to prove it for $n = 7$. This latter case turned out to be especially difficult, and it was finally proved in 1839 by Gabriel Lamé (1795-1870).

At this point in the story, we find ourselves two hundred years after Fermat wrote his marginal remark. Scattered efforts have been made to solve the problem. Sophie Germain is the only one to have devoted focused efforts, and some work has been done in the wake of her theorem leading to interesting results. Some new techniques were developed for dealing with problems similar to FLT but not FLT itself, and there is a growing realization that there may be some real mathematical challenge in this problem, after all, if it has not been solved so far. In 1815 and then again in 1818 the French Academy in Paris offered a minor prize for the proof, which was not awarded [Legendre 1823, 2], but FLT was included in the Academy's *Grand Prix* only in 1849 (more on this below). Lamé's proof for $n = 7$ was a difficult one, and it was perhaps the first one that comprised the development of new techniques specifically devoted to solving the problem. The same Lamé was involved in the most important

[7] Since only a part of Germain's work became public through Legendre's book, it was common until now to attribute her only with the proof for $p < 100$, whereas Legendre was attributed with its extension for all values up to $p < 197$. [Del Centina 2008] is based on a detailed analysis of many of her unpublished manuscripts, and it convincingly shows that Germain actually proved case I of FLT for all values of to $p < 197$.

nineteenth-century mathematical crossroad directly related with FTL, that took place in Paris in 1847, and that deserves a broader discussion here.[8]

In 1847 we find the first instance of a group of leading mathematicians devoting serious, focused discussions to the possibility of proving this by now well-known conjecture. The group, gathered at the Paris Academy, included such stars as Augustin Louis Cauchy (1789-1857) and Joseph Liouville (1809-1882). In order to get a correct picture of the situation, however, one should constantly keep in mind that while discussing FLT in the Academy and trying new ideas on this question, these mathematicians were simultaneously devoting their efforts to several other mathematical problems, especially those related with analysis and mathematical physics.

On March 1, 1847, Lamé presented his colleagues with what he thought to be a possible way to solve the general case. Lamé used an idea originally suggested to him by Liouville, which involved a factorization of a sum of integers into linear complex factors, as follows:

$$x^n + y^n = (x + y)\,(x + ry)\,(x + r^2y)\,\dots\,(x + r^{n-1}y)$$

Here n is an odd natural number, and r is a primitive root of unity, namely, a complex number ($r \neq 1$) satisfying the condition $r^n = 1$ (n being the lowest such power). Starting from this factorization, Lamé would apply an argument based on the method of infinite descent in order to lead to a contradiction that would prove FLT. Thus, combining ideas of several mathematicians like Fermat himself, Gauss, and Liouville, Lamé thought to be on the right track to solve the problem.

There were from the beginning some doubts about the logical validity of Lamé's argument, and Liouville himself was among those who manifested such doubts. Interesting and sometimes personally loaded discussion followed this presentation. Two main issues were at stake. On the one hand, the discussants addressed the very pressing question of who was the first to introduce what idea which was decisive for the putative proof. On the other hand, there was a more technical, and more crucial

[8] In this section I followed the presentation of [Edwards 1977, 59-75]. This seminal book can be consulted for details about the theorems and proofs menioned here.

matter, namely the question whether the factorization on the right-hand side of the above equation is unique, as in the case of decomposition into prime factors for rational integers. As already mentioned, the introduction of Gaussian integers immediately raised the question of how far the properties of rational integers might extend to this new kind of numbers, and it turned out that, essentially, most known properties would indeed apply. A similar question was at stake here for this more general domain, and it is here that the disappointing news came in, on May 24. That day Liouville read to his friends a letter sent from Germany by Kummer, who had also sent an article published in 1844 and that retrospectively invalidated Lamé's alleged proof in spite of the brilliant idea it involved. Kummer's article directly showed that the factorization in question was not unique, as tacitly assumed by Lamé.

Kummer also informed that he had developed a theory of "ideal complex numbers" meant to restore a kind of unique prime factorization in domains of numbers in which, as he had noticed, uniqueness may fail. The theory also led to the definition of a certain class of prime numbers (within the domain of the rational integers), later to be called "regular primes". Kummer noticed the relevance of this kind of primes to FLT and proved that the theorem is valid for all regular primes. Later on he found an operational criterion for telling whether or not a given prime is regular, using the so-called "Bernoulli numbers". It turned out, for instance, that the only irregular primes under 164 are 37, 59, 67, 101, 103, 131, 149, and 157. After 164, the calculations became prohibitively complex. Further, Kummer developed criteria to prove, given an irregular prime, if FLT is valid for it. In this way he proved FLT separately for 37, 59 and 67, thus achieving the very impressive result that FLT is valid for all exponents under 100 [Kummer 1857].[9]

Kummer's results opened a broad and clearly defined avenue for a possible, continued investigation of FLT. All what was needed now was to continue the search for irregular primes, for which separate proofs could then be worked out, according to Kummer's criteria. These criteria, moreover, could be further elaborated and refined in order to enable more efficient and precise procedures of verification that FLT was

[9] Kummer's proof contained some mistakes that were later corrected by successive mathematicians, culminating in [Vandiver 1926].

valid for the individual cases in question. Apparently, Kummer assumed but did not prove that there are infinitely many regular primes and that there would be a very limited proportion of irregular primes. These assumptions continued to be shared by many. But the interesting point, from the perspective of the present account, is that this broad avenue was followed by very few number theorist in the following decades and relatively little work was done on FLT until the early twentieth century along these lines.

This is not to say that Kummer's ideas were completely abandoned. On the contrary, they did lead to important mathematical developments, but following a somewhat different direction wherein attempts to prove FLT played no role whatsoever. Through the combined efforts of prominent number-theorists like Richard Dedekind (1831-1916) and Leopold Kronecker (1823-1891) the full elaboration of the important insights contained in Kummer's theory of "ideal complex numbers" led to a complete redefinition of how factorization properties are to be investigated (and even to a redefinition of "integers" in the more general domains that were now investigated). The mathematical discipline that in the early twentieth century became known as "algebraic number theory" actually arose from the works of Dedekind and Kronecker, and eventually, especially under the influence of Dedekind's approach, gave rise to what became known as modern commutative algebra of pervasive impact in twentieth-century mathematics [Corry 2004, 129-136].

The important work of Kummer and the influence it had in leading to the development of so many central ideas in modern algebra is often mentioned when explaining the importance of FLT for the history of mathematics. Kummer's attempt to solve FLT—so the typical argument goes—motivated him to launch a significant line of research that yield seminal ideas of lasting and pervasive importance in mathematics.[10] This would indeed be a strong argument supporting the view of FLT as a "historically important mathematical problem", were it not for the fact that the historical record leads us to add important qualifications to it.

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[10] Two prominent places where this opinion is presented are the introduction to [Hilbert 1902] and [Hensel 1910]. The latter became an often quoted source for stressing the historical importance of FLT. See below note 14.

In his 1847 letter to Liouville Kummer stated, indeed, that the application of the theory of ideal complex numbers to the proof of FTL had occupied him for some time now. On the other hand, he clearly stated his opinion that FLT was "a curiosity in number theory, rather than a major item." In fact, very much like Gauss, Kummer declared that the problem of higher reciprocity was the "central task and the pinnacle of achievement in number-theoretical research." He conducted important research in this field, following on the footsteps of Carl Gustav Jacobi (1804-1851). In his research Kummer even adopted the notation originally used by Jacobi when dealing with reciprocity. It was only after many efforts, especially after having performed lengthy and detailed calculations with numbers in domains that generalized the idea of the Gaussian integers, that Kummer realized that the tacit assumption of unique factorization would break down. He published only two, relatively short, articles where ideal complex numbers were applied to FLT: the first in 1847 and the second, containing his interesting proof of FLT for regular primes, as late as 1858. At the same time, between 1844 and 1859 he published a long list of dense, significant papers on higher reciprocity. Higher reciprocity, rather than FLT, was Kummer's true motivation for developing the theory of ideal complex numbers [Edwards 1977, 1977a].

5. Marginality within diversification in number theory (1855-1908)

The story of the development of number theory in the second part of the nineteenth century is a complex one. On the one hand, there are important achievements of long-standing impact such as embodied in the works of Dedekind and Kronecker, or such as those derived from the use of analytic approaches introduced in the work of Dirichlet. On the other hand, actual interest in the field on the side of broader audiences of mathematicians (and especially of prominent mathematicians) was definitely reduced. It is well know, for instance, that Dedekind's theory of ideals was hardly read at the time of its publication, both in its German original and then in its French translation of 1876-77.[11] This is not to say, however, that there was no actual

[11] See [Goldstein & Schappacher 2007a, 68-70].

research being done in number theory. Rather, there was a reorganization of the field into several clusters of interest. These clusters grouped mathematicians who shared common interests within number theory, used similar techniques and pursued similar objectives. They published in similar journals and quoted each other. As a rule, they represented self-contained fields of research that hardly intercommunicated with each other.[12]

During this period, the cluster where most of the actual activity in number theory developed was not connected to those trends that eventually led to the main foci of number theoretical research at the turn of the twentieth century, such as the "algebraic" and the "analytic" traditions. Rather, this cluster focused on questions directly connected with some of the basic topics discussed in Gauss's *Disquisitiones*, such as reciprocity, and cyclotomic and Diophantine equations. It explicitly avoided the use of techniques involving complex numbers and analysis. At the same time, it showed a clear inclination towards historical accounts of the discipline. Contributors to this cluster included in a visible way not only mathematicians, but also engineers, high-school teachers and university professors from other disciplines. They came from various countries including places without well-developed research traditions in the field. Remarkably, very few Germans were among them. More than any other cluster or sub-discipline in number theory, works belonging to this cluster as a rule did not involve highly sophisticate mathematical knowledge. Still, some of them comprised very ingenuous and innovative ideas, appearing mostly in the work of the more prominent mathematicians that contributed here. The latter included James Joseph Sylvester (1814-1897), Angelo Gennochi (1817-1889), and Edouard Lucas (1842-1891). This cluster did not evolve into a full-fledged, school of mathematical research that trained students and was systematically thought. Most research on FLT during this period can be easily associated with this cluster of activity in number theory, and indeed, even here it counted as a rather marginal trend in terms of attention devoted to it, as we will see in this section.

[12] Detailed analyses of these processes appear in [Goldstein 1994], [Goldstein 2007a, 71-74].

But even before discussing some of the relevant research in this period it is interesting to asses the status of FLT within some contemporary expository presentations of the field. Indeed, textbooks on the theory of numbers written after 1855 do mention FLT in various ways, usually with some didactic message in mind, but they never describe the problem as an outstandingly important one. In more research-oriented texts, FLT is hardly mentioned. I illustrate this point by looking at three prominent texts of this kind.

I start with the influential and comprehensive *Report on the Theory of Numbers* presented between 1859 and 1866 to the British Association for the Advancement of Science by Henry J.S. Smith (1826-1883). Smith, Savillian professor of geometry at Oxford since 1860, was the foremost, perhaps the only, mathematician of his time in the English-speaking world truly knowledgeable with current developments in number theory. He also made important contributions of his own to the discipline [Macfarlane 1916, 58-68]. The *Report* aimed at bringing to his fellow mathematicians in the English-speaking world an updated picture of the field, which he described as rather neglected in the British context. This updated picture would comprise a systematic presentation of the contributions in the field as it had been established by Gauss, and then further developed by Jacobi, Eisenstein, Legendre and Dirichlet, and also more recently by the contributions of Kummer, Kronecker and Dedekind. Following along the lines of Gauss' *Disquisitiones*, Smith described the field as composed by two main branches, and to them he devoted the bulk of his book: the theory of congruences and the theory of homogeneous forms. He described the reasons for the immediate appeal of this part of mathematics, quoting directly from Gauss, in the following terms [Smith 1894, Vol. 1, 38]:

> The higher arithmetic presents us with an inexhaustible storehouse of interesting truths – of truths, too, which are not isolated, but stand in the closest relation to one another, and between which, with each successive advance of the science, we continually discover new and sometimes wholly unexpected points of contact. A great part of the theories of Arithmetic derive an additional charm from the particularity that we easily arrive by induction at important propositions, which have the simplicity upon them, but the demonstration of which lies so deep as not to be discovered until after many fruitless efforts; and even then it is obtained by some tedious and artificial process, while the simpler methods of proof long remain hidden for us.

One might think that FLT could be a very good example that fitted this description and that it might used by Smith to encourage the interest of fellow mathematicians not working in the field. Smith describes FLT as a "celebrated proposition", and he devoted lengthy footnotes to describe its history. But in terms of mathematical ideas, he devoted very little space to it. In a few pages he explained the principles of Kummer's criteria for proving the validity of FLT for any given irregular prime. Indeed, Smith stated his belief that it would probably be difficult to find an irregular prime for which Kummer's criteria would not hold [Smith 1894, Vol. I, 131-137]. Fermat's result that Smith did discuss in detail and that he consistently used throughout what he called in his book simply "Fermat's theorem", which is what we usually call nowadays "Fermat's little theorem".

In November 1876, Smith delivered his retiring presidential address to the London Mathematical Society, under the title "On the Present State and Prospects of Some Branches of Pure Mathematics". Once again he intended to present the theory of numbers (apparently still neglected among his colleagues) and to encourage young mathematicians to undertake research in the field. He repeated the basic ideas and quotations appearing in his *Report* and this time he also added what he considered to be a third branch of the discipline not previously mentioned, namely the analytic approach to number theory (which he called here, "the determination of mean or asymptotic values of arithmetical functions"). He also described the current, active efforts, originating with an idea of Liouville, to investigate whether numbers like e and π are algebraic or transcendental. The problems and achievements that Smith described in his speech should in his view convince about the importance and deep interest that research in the field would offer to aspiring mathematicians. He thus said [Smith 1876, 167]:

> I do not know that the great achievements of such men as Tchebychef and Riemann can fairly be cited to encourage less highly gifted investigators; but at least they may serve to show two things – first, that nature has placed no insuperable barrier against the further advance of mathematical science in this direction; and that the boundaries of our present knowledge lie so close at hand that the inquirer has no very long journey to take before he finds himself in the unknown land. It is this peculiarity, perhaps, which gives such perpetual freshness to higher arithmetic.

In this festive occasion, however, when he used this important podium with the explicitly purpose in mind of promoting research in numbers theory, it is remarkable that Smith did not even mention FLT.

The second prominent text worthy of consideration here is the *Zahlbericht*, published in 1897 by David Hilbert (1862-1943), then one of the foremost number theorists of the world. The *Zahlbericht* was initially commissioned by the Association of German Mathematicians as an up-to-date report on the state of the art in the discipline. Hilbert indeed summarized the work of his predecessors but he also added many new results and sophisticate techniques and opened new avenues for research in the fields that many would indeed follow in the decades to come. It was the most comprehensive and systematic compendium of the theory of algebraic number fields composed at the end of the nineteenth century. The role played by the *Zahlbericht* for the discipline is very similar to that played by *Disquisitiones* one hundred years earlier. However, the impact of this book goes well beyond the specific significance of this or that result that it presents, and relates to broader trends of development in mathematics at the time, and these merit a brief discussion here.

In developing the theory of algebraic number fields on the wake of Kummer's work on ideal complex numbers, Kronecker and Dedekind had mutually complemented the theorems, proofs and techniques elaborated by each other. Nevertheless, they represented two very different, and in some sense opposed, approaches to the very essence of mathematical practice. Kronecker represented what may be called a more "algorithmic" approach, whereas Dedekind was the quintessential representative of the so-called "conceptual" approach. This is not intended to mean that Kronecker introduced no new, abstract and general concepts or that he derived no results from an adequate use of them. Nor do I mean to say that one finds no computations in Dedekind. Rather, the point is that Dedekind's perspective allowed for the indiscriminate use of infinite collections of numbers defined by general abstract properties, whereas Kronecker insisted on the need to prescribe the specific procedures needed to generate the elements of such collections and to determine whether or not two given elements were one and the same. Dedekind did not seek or require such procedures and Kronecker did not consider it legitimate to ignore them.

The choices made by Hilbert in preparing the *Zahlbericht* gave a clear emphasis to the "conceptual" perspective embodied in Dedekind's work, as opposed to the "calculational" one of Kronecker. The *Zahlbericht* thus became a decisive factor in transforming Dedekind's approach into the dominant one in algebraic number theory and related fields. Eventually it spread to all of algebra, via the influential work of Emmy Noether (1882-1935), and from there to many other mathematical domains. Indeed it became a mainstream characteristic of the entire discipline of mathematics throughout the twentieth century.

In the introduction to his compendium Hilbert stated his views on what is the most important task of his field of enquiry. Like Kummer, he stressed the primacy of the problem of reciprocity. Unlike Kummer, however, Hilbert suggested to avoid the complex, straightforward calculation of the kind that had originally led to his discoveries. Hilbert thus wrote [Hilbert 1998, ix]:

> It is clear that the theory of these Kummer fields represents the highest peak reached on the mountain of today's knowledge of arithmetic; from it we look out on the wide panorama of the whole explored domain since almost all essential ideas and concepts of field theory, at least in a special setting, find an application in the proof of the higher reciprocity laws. I have tried to avoid Kummer's elaborate computational machinery, so that here too Riemann's principle may be realised and the proof completed not by calculations but purely by ideas.

Hermann Minkowski (1864-1909) – who was Hilbert's close friend and no less prominent number-theorist than him – systematically promoted a similar perspective in his work. He spoke of "the other Dirichlet principle", embodying the view that in mathematics "problems should be solved through a minimum of blind calculations and through a maximum of forethought" [Minkowski 1905]. The deep influence of approaches such as espoused by Hilbert and by Minkowski—against blind and extensive calculations—helps explain why further calculations with individual cases was not favorably encouraged within the mainstream of number theory in the decades that followed, as will be seen below.

Lengthy section of the *Zahlbericht* discuss cyclotomic and quadratic fields, as well as topics related with reciprocity. Under the kind of conceptual point of view derived from Dedekind, extending the results of Kummer on FLT by calculation of additional, individual cases was indeed of little interest. Hilbert devoted a few pages in the last section of the book to FLT, and there he examined the Diophantine equation $\alpha^l + \beta^l + \gamma^l = 0$ for exponents l that are regular primes, while correcting a mistake of Kummer (who thought to have proved the impossibility of the equation for cyclotomic integers in general, rather than just for rational integers). Hilbert's proof, like most of the material presented in the *Zahlbericht* reformulated Kummer's ideas in terms of concepts and techniques introduced by Dedekind for dealing with algebraic number fields. But if he devoted this kind of special attention in some pages to the equation $x^n + y^n = z^n$, at the same time he also looked at a related one, showing that $x^4 + y^4 = z^2$ has no solutions in Gaussian integers.

It may be worth pointing out at this point that while Hilbert was teaching in Göttingen for the first time a seminar on number theory along the lines pursued in his report, Felix Klein (1849-1925) taught, in parallel, another seminar in the same topic, a somewhat unusual choice for him. His course amounted to a discussion of the theory of binary quadratic forms seen from a geometric perspective. He stressed that the visual (*anschauulich*) perspective he was following and the "logical treatment" presented in Hilbert's seminar were complementary and not mutually exclusive [Klein 1896-97]. At any rate, FLT finds no place in the syllabus of Klein's seminar. Nor is it mentioned, many years later, when Klein published his classical *Lectures on the Development of Mathematics in the Nineteenth Century* [Klein 1926] (though it must be stressed that this is far from being a balanced and comprehensive account of the discipline of mathematics in the period covered, anyway. Rather, it is the author's own, broad perspective on it. Number theory at large, to be sure, receives only very little attention in these lectures.)

Not long after the publication of the *Zahlbericht* Hilbert had another remarkable opportunity to express his views about important problems in the theory of numbers, and in mathematics in general. This was at the second International Congress of Mathematicians held in 1900 in Paris, where Hilbert gave the now very famous talk presenting a list of problems that in his opinion should and would occupy the efforts of mathematicians in the new century. In the introductory section of the talk, Hilbert gave some general criteria for identifying important mathematical problems (and by the way, most of the problems in his list do not easily fit into these criteria). He also mentioned three exemplary problems from recent times: the problem of the path of quickest descent, the three body-problem and FLT. Concerning the latter Hilbert said [Hilbert 1902, 440]:

> The attempt to prove this impossibility offers a striking example of the inspiring effect which such a very special and apparently unimportant problem may have upon science. For Kummer, incited by Fermat's problem, was led to the introduction of ideal numbers and to the discovery of the law of the unique decomposition of the numbers of a cyclotomic field

> into ideal prime factors—a law which today, in its generalization to any algebraic field by Dedekind and Kronecker, stands at the center of the modern theory of numbers and whose significance extends far beyond the boundaries of number theory into the realm of algebra and the theory of functions.

As already pointed out, this was indeed the prevailing view then and for a long time thereafter, that attributed historical importance to FLT because of its contribution, via the work of Kummer, to the rise of modern algebra. However, as we have already seen, higher reciprocity, and not FLT, was the real, main motivation behind Kummer's efforts. But the point I want to stress here is that in spite of this statement and of the discussion of FLT in the introductory section, the problem was *not* included in the list of problems itself. Hilbert included no less than six problems directly related with number theory, but he did not consider that FLT should be among them. This was a clearly conceived choice. Among the problems included were the Riemann conjecture (which was by no means an obvious choice at the time), and, not surprisingly, higher reciprocity. Hilbert explicitly stated that the reason to include the Riemann conjecture was that its solution would lead to clarify many other problems, such as for instance—so he thought—the Goldbach conjecture. Hilbert did not single out FLT as holding a promise similar to that. At best, a roundabout reference to FLT (but in no way a direct mention of it) was implicitly comprised in the famous tenth problem of the list in which, given a Diophantine equation with any number of unknowns and with rational integer coefficients, the challenge is to devise a process, which could determine by a finite number of operations whether the equation is solvable in rational integers. Equation (1) would be an example of such an equation, and a possible solution of the tenth problem would indicate if the equation has solutions. If Hilbert had in mind this connection, this would only place the problem in a similar status to that formulated one hundred years earlier by Gauss, namely as a very particular corollary of a much broader and interesting problem.

A third book to be considered here relates to a very helpful source of additional information on the status of FLT after Kummer. This is the well-known, three-volume *History of the Theory of Numbers* published between 1919 and 1923 by Leonard Eugene Dickson (1875-1954). Out of close to eight hundred pages and thirty seven chapters that compose this book, Dickson devoted his last one, of forty-five pages, to list all published mathematical works bearing some relation with FLT.[13] Dickson pronounced himself clearly with respect to the status FLT as a separate mathematical problem: “Fermat’s Last Theorem—he wrote in the introduction to the relevant chapter—is devoid of special intrinsic importance, and if a full proof of it is ever published it will loose its main source of attraction”.[14]

Dickson’s list comprises about 240 items published after Kummer. In order to properly evaluate its historical import, however, it is important to stress that almost all of these works belong to the above mentioned cluster of research in the discipline that distanced itself from the use of analytic methods or of techniques such as developed by Dedekind and Kronecker in their theories. Indeed, most items listed are very short (typically between one and three pages) and involve essentially unsophisticated mathematics. Several are just summaries of the current state of research on the problem, and they are typically directed to an audience of non-experts. They embody the typical text stressing the outstanding importance of the problem, mainly in order to enhance the virtues of their own intended contribution (which in general is either erroneous or trivial).[15] These publications usually appear in obscure journals or in

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[13] In order to present the full picture it is important to stress that the three volumes of Dickson’s book do not include even a chapter on the law of quadratic reciprocity. This omission was not due, however, to Dickson’s opinion on the importance of this problem, or to a neglect on his side. Rather, he intended to write a fourth volume exclusively devoted to it, but for several technical reasons this plan did not materialize. See [Fenster 1999].

[14] We find exactly the same assessment also in [Dickson 1917, 161], but there he added that if a proof was found the problem would not loose its *historical* importance. He was citing here Hensel’s opinion (see above note 10).

[15] [Calzolari 1855], [Thomas 1859, Ch. 10], [Laporte 1874], [Martone 1887], [Martone 1888], [Barbette 1910] , [Gérardin 1910]. The same Gérardin edited between 1906 and 1912 in Nancy a journal entitled “*Sphinx-Oedipe. Journal mensuel*

booklets published by obscure private publishers, and one can also count among them the only doctoral dissertation appearing in the list.[16] Moreover, the vast majority of the works appearing in Dickson's list comprise slight improvements over previously proven results about specific properties of numbers that have some kind relevance to an existing argument related with FLT. As a rule, the authors of these short pieces do not even care to mention that their article had anything to do with FLT. Even in cases where the equation $x^n + y^n = z^n$ is at the focus of the article, the new result is occasionally presented as improving on Kummer's results, rather than as a new solution to an open famous problem formulated two hundred earlier by Fermat (whose name is not even mentioned in many cases).[17] In fact, in many cases Dickson's decision to include a certain item seems to be justified only retrospectively and indirectly, as it is clear that the author in question was *not* thinking of FLT when he published the piece. In such cases, we see that only a small section of the article contains a specific result that might be used in dealing with FLT, even if this is not the

de la curiosité, des concours et de mathématiques". The journal published several reviews and reprints of works related to FLT, inlcuding a series of articles published between 1853 and 1862 by one Fortuné Landry (1798-?), who also found a factorization of $2^{64} + 1$, published in [Landry 1880].

[16] [Rothholz 1892] was written in Giessen under the direction of Eugen Netto (1848-1919). It consists mainly of a review of existing results, and an elaboration of one of Kummer's result, leading to the conclusion that the FLT is valid whenever x,y,z are less than 202.

[17] Thus for instance [Mirimanoff 1892, 1893], both of which deal with the case of the irregular exponent p = 37. Also other mathematicians dicussed this case with special interest after Kummer.

main issue discussed there.[18] Of these, some are no more than conjectures or even "beliefs".[19]

A considerable part of the mathematicians mentioned in Dickson's account of FLT are far from prominent and very often they are absolutely obscure. For some of them one can find no entry under their names in the main review journal of the time, the *Jahrbuch über die Fortschritte der Mathematik*. On the other hand, whenever renowned mathematicians do appear in the list, they appear with very minor, and indeed marginal works, or with reformulations of old results in more modern guise. An interesting case in point is that of Ferdinand Lindemann (1852-1939), who had been Hilbert's doctoral advisor, and became a very famous and well-connected mathematician after the publication, in 1882, of his proof of the transcendence of π. After this important proof, however, Lindemann never published again significant mathematical works. One place where he did try his forces was precisely in FLT, of which he published four attempted proofs between 1901 and 1909, all of them mistaken, as it turned out. Euler, Lagrange, Legendre, Lamé, Cauchy, Dirichlet and Kummer are of course included in the first part of Dickson's list of contributors to FLT, but most of the prominent German number theorists after Kummer do not even appear: Dedekind and Kronecker, Minkowski, Kurt Hensel (1861-1914), Alexander Ostrowski (1893-1986), Emil Artin (1898-1962), or Carl Ludwig Siegel (1896-1981). If we look at the period before Kummer, the two most important contributors to the

[18] Thus for instance [Jacobi 1846]. This is an article enitrely devoted to the problem of quadratic and higher reciprocity and FLT is not mentioned at all. In the last two pages, Jacobi presented a table of certain values of integers m', that statify a property indirectly related to a proposed solution of FLT, that Dickson quotes earlier. Dickson was explicit in giving only these last two pages as reference here, and not the entire article.

[19] Thus, the famous conjecture presented in a very short note of eight lines in [Catalan 1844] as the "belief" that $x^m - y^n = 1$ holds only for $3^2 – 2^3 = 1$.

theory of higher reciprocity were Jacobi and Gothold Eisenstein (1823-1852), none of whom devoted any attention to FLT.[20]

Of the mathematicians mentioned by Dickson, those with the most significant contributions to FLT after Kummer were Dimitry Mirimanoff (1861-1945) and Arthur Wieferich (1884-1954). Wieferich proved in 1909, for instance, that if three integers x, y, z satisfy $x^p + y^p = z^p$ and the three and p are relatively prime, then $2^{p-1} \equiv 1 \pmod{p^2}$. Mirimanoff then showed that under the same conditions, $3^{p-1} \equiv 1 \pmod{p^2}$. These two results, and some similar ones that were sporadically added later on,[21] were considered to imply significant progress in proving the theorem since they helped determine a lower bound for the value of integers for which the Diophantine equation associated with case I of FLT could be satisfied (and this, moreover, only by considering p, and irrespective of the values of x,y,z that may satisfy the equation).

And yet, even in the case of these two mathematicians it is important to set their works the proper historical context. Wieferich, for instance, published only nine papers during his lifetime. They deal with number-theoretic questions of diverse nature, but only four of them go beyond what seem to be minor remarks. Only one among the latter deals with FLT [Wieferich 1909], and it establishes indeed an important result that had a significant follow-up. Mirimanoff, in turn, was a versatile mathematician with important contributions to set theory, probabilities and number theory. Of about 60 published articles, about twelve deal with matters related with FLT and only six contain significant contributions. In an article published in 1904 [Mirimanoff 1904], he pointed out with some puzzlement, that some important criteria developed by Kummer concerning case I of FLT had received very little attention so far and had not been duly appreciated. Indeed, only one article written between 1857 and 1904 [Cellérier 1894-97] had dealt with the question addressed here by Mirimanoff, without however leading to any significant result.[22]

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[20] To this one may also add the entire Russian tradition in which no attention whatsoever seems to have been devoted to FLT. See, for instance, [Delone 2005].

[21] Some references appear in [Ribenboim 1999, 362].

[22] See [Vandiver 1952].

We thus see that the information contained in Dickson's section on FLT, when set in the proper historical context, indicates the rather low degree of direct mathematical interest aroused by FLT in the decades following Kummer's contributions, and the relatively little, real effort devoted by mathematicians to the problem.

6. The Wolfskehl Prize and its aftermath

An interesting turn of events concerning FLT and the legend around it took place in 1908, when the Göttingen Royal Society of Sciences instituted a prize of 100,000 marks to the first person to publish a complete and correct proof of the theorem. The fund was bequeathed as part of the will of Paul Wolfskehl (1856-1906) the son of a family of wealthy Jewish bankers. According to the legend, repeated in many sources (including Singh), Wolfskehl was depressed because of the unreciprocated love of a "mysterious woman", whose "identity has never been established". Wolfskehl decided to commit suicide, but his decision did not materialize, according to the legend, because in his last hours he started to browse through Kummer's work on FLT. He became so absorbed in his reading and in the thought that he might finally contribute to the efforts to prove FLT, that the carefully planned time for carrying out his tragic project—midnight, what else?—passed without notice, and "his despair and sorrow evaporated". The money he decided to devote to the prize was a token of recognition to the value of mathematics and, more particularly, to the theorem that "renewed [Wolfskehl's] desire for life" [Singh 1997, 122-129].

In 1997 the Kassel mathematician Klaus Barner decided to find out some solid facts about the most famous philanthropist in the history of mathematics. The facts pertaining to the life of Wolfskehl sensibly differ from the legend [Barner 1997]. Wolfskehl graduated in medicine in 1880, apparently with a dissertation in ophthalmology. As a student, early symptoms of multiple sclerosis started to appear, and Wolfskehl realized that a future as a physician was rather uncertain for him. He thus decided to switch to mathematics. Between 1881 and 1883 he studied at Berlin, where he attended Kummer's lectures. Wolfskehl's interest in, and knowledge of, FLT date back to those years. He even published some work in algebraic number theory. In 1890 he completely lost his mobility and the family convinced him to marry, so that someone would continue to take care of him. Unfortunately, the choice of the bride seems to have been unsuccessful and, according to Barner's research, Wolfskehl's life became rather miserable after marriage in 1903. And then, in 1905, he indeed changed his will on behalf of his life's only true love, the theory of numbers, which gave some meaning to his last, apparently unfortunate years. Perhaps the wish to reduce to some extent the capital bestowed to his future widow played a significant role in the decision. At any rate, if Wolfskehl ever considered committing suicide the reason behind such a decision was the deep depression that affected him following his disease, and not because a broken heart caused by an unknown lady. FLT did not save his life even though, as part of his interest in the theory of numbers, it may have given him some comfort.

A rather surprising, and totally overlooked, fact related with the establishment of the Wolfskehl prize is that it had an almost immediate effect on the amount of work devoted to FLT, and not only by amateurs (as it is well-known) but also among professional, and sometimes prominent, mathematicians. Before discussing this interesting point in greater detail, however, it is relevant to comment on previous prizes that were offered in relation with FLT, as the importance attributed to these earlier prizes has often been overstated. Also in this case, some historical context helps clarifying the correct proportions.

It seems that the first time a prize was offered for would-be solvers of the problem was on December 1815. The prize, which apparently rekindled Sophie Germain's interest in the problem after having devoted some years to elasticity theory, comprised a golden medal valued in three thousand francs. The announcement extolled the great progress undergone by the theory of numbers since the time of Fermat, but also referred to two of the "main theorems" due to "this illustrious sage" that had remained unproved in the general case. Cauchy, they indicated, had just proved the one concerning the polynomial numbers, and his proof had elicited the praises of the geometers. Only one result, then, remained now to be proved, and that was FLT. The prize was being offered as homage to "the memory of one of the sages who had honored France the most", and also as an attempt to offer the geometers the occasion to perfect this part of science. The expected general proof should be delivered before

January of 1818, which did not materialize.[23] Indeed, we do not know of any mathematician, other than Germain, who devoted some efforts to prove the theorem at this time, on the wake of the offer.

FLT was included in the *Grand Prix* of mathematics only in 1849, and it was postponed several times. Five competing memoirs were presented to the Academy in 1850 and eleven in 1853, but none of these was deemed worthy of the prize. Finally, in 1856 it was decided that the prize would go to Kummer – who had not presented himself as a competitor – for his "beautiful" works on the roots of unity and complex numbers.[24] In explaining why this problem was chosen, the proceedings of the Academy used a rather moderate formulation:

> As recent works by several geometers have directed attention to Fermat's last theorem, and have notably advanced the question, even for the general case, the Academy proposes to remove the last difficulties that still remain in this subject.

In both the announcements of the prizes (1815 and 1849) the message is that the prize might encourage work that would lead to complete a task that was long overdue and thus finally put this enticing and by now somewhat mysterious riddle to rest.

Also the Royal Belgian Academy devoted in 1883 its annual contest to a proof of FLT. The committee comprised only one relatively well known mathematician, Eugène Catalan (1814-1894), who indeed had done some work in number theory and even on FLT. The other two, Paul Mansion (1844-1919) and Joseph de Tilly (1837-1906), were much less so. Only two memoirs were presented to this competition and they were not considered to be worthy of the prize.[25]

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[23] See *Académie des sciences (France). Procès-verbaux des séances de l'Académie*, Vol. 5. 1812-1815, p. 596.

[24] The Grand Prix was usually announced in the *Comptes rendus hebdomadaires des séances de l'Académie des sciences*. See Vol. 29 (1849), 23; Vol. 30 (1850), 263-64; Vol. 35 (1852), 919-20; Vol. 44 (1857), 158.

[25] Reports appear in *Bulletin Acad. R. Belgique* (3), 6 (1883), 814-19, 820-23, 823-32.

The Wolfskehl prize became much more famous than the previous ones devoted to FLT and indeed it gave rise to a flurry of works. The considerable amount of money promised to the would-be successful solver seems to have been more persuasive about the importance of devoting attention to the problem, than any other kind of purely mathematical consideration. Even before the prize was established, the one aspect in which FLT could safely claim precedence over any other mathematical problem concerned the amount of false proofs of it that had been published. The announcement of the prize boosted this tendency to new, previously unimaginable dimensions. More than thousand false proofs were published between 1908 and 1912 alone.[26] The journal *Archiv der Mathematik und Physik*, devoted above all to high-school teachers, initiated a special section for FLT and until 1911 it had already published more than 111 proofs acknowledged to be wrong.

Interestingly, however, not only amateurs or obscure mathematicians were led to increased activity around FLT. Wieferich, as already mentioned, published his contributions in 1909, and indeed, the Wolfskehl fund paid him an amount of 100 marks in recognition for the progress achieved in the solution of the problem. Also Mirimanoff published six articles on FLT after 1909, about six years after not having published anything on this question. It is hard to determine if, and to what extent, the works of these two mathematicians were directly motivated by the prize. But we also find top-rate mathematicians like Philip Furtwängler (1869-1940), Erich Hecke (1887-1947), Felix Bernstein (1878-1956), and Ferdinand Georg Frobenius (1849-1917) who published works on FLT for the first time around these years [Bernstein 1910, 1910a; Frobenius 1909, 1910; Furtwängler 1910, 1912; Hecke 1910]. All of these works are connected to Kummer's criteria in various ways as well as with the recent advances by Wieferich and Mirimanoff. A direct sequel of this thread can be found as late as 1922 in an article by Rudolf Fueter (1880-1950), a distinguished number-theorist pupil of Hilbert [Fueter 1922].

[26] See [Lietzmann 1912, 63]. [Ribenboim 1999, 381-388] adds information about false or insufficient proofs published throughout the years. The amount of unpublished wrong proofs that ever reached the desks of mathematicians who were publicly known to be associated with FLT cannot even be estimated.

It seems much more than pure coincidence that all these works were published on the wake of the announcement of the prize. In his highly productive career, Bernstein published important works on set theory and on statistics. He completed a *Habilitation* thesis under Hilbert on class field theory and published two short articles related to this [Bernstein 1903, 1904]. But then, the only time he ever returned to number theory was in 1910, when he published this article on FLT. Similar is the case of Hecke, a distinguished number theorist whose only contribution to FLT was this one. In a well-known textbook published in 1923 [Hecke 1923], he did not even mention FLT. No less prominent was Furtwängler, who published the most important contributions to the question of higher reciprocity some years later. In his career he published only two articles on FLT, both of them soon after the prize was established [Furtwängler 1910, 1912]. Furtwängler explicitly indicated in a footnote to his 1910 article that the recent awakening of interest in FLT created by the announcement of the prize was the direct motivation for publishing his results without further delay, even though the current state of his research on the topic did not really satisfy him. Finally, Frobenius – the dominant figure of Berlin mathematics between 1892 and 1917 – published significant research in an enormous variety of fields, and only very little on number theory. His only important contribution to the discipline (the so-called Frobenius density theorem [Frobenius 1896]) was published more than a decade before his first article on FLT. The latter consisted in a refinement of Wieferich proofs and results. A couple of years later, he proved a congruence similar to Wieferich's, for the bases 11 and 17, as well as for bases 7, 13, and 19, provided $p = 6n - 1$ [Frobenius 1914]. The impression created by all these examples is strengthened by an explicit assertion found in an appendix to the 1913 French translation of *Zahlbericht* written by Théophile Got. This appendix was devoted to presenting a summary of recent work on FLT up to 1911, and the reason that justified its inclusion in this edition was the "revival of interest in the question" of FLT aroused by the "recent creation of the Wolfskehl prize" [Hilbert 1913, 325].

There is interesting evidence indicating that the situation brought about by the Wolfskhel prize produced discomfort among many mathematicians. That is for instance the case of Oskar Perron (1880-1975), a versatile mathematician with a very long career and contributions to many fields, including number theory. Among other things, he had been in charge, with two other colleagues, of reporting in the *Archiv* *der Mathematik und Physik* on putative proofs of FLT. In his 1911 inaugural lecture on taking a chair of mathematics at Tübingen ("On truth and error in mathematics"), he interestingly referred to his recent experience as a reader of so many failed attempts by amateurs. He compared the status of mathematics with that of other sciences in terms of public awareness to the current state of research, while lamenting the disadvantageous situation of mathematics in this regard. Among other things he also said the following [Perron 1911, 197]:

> Strangely enough, however, there are a few problems in mathematics that have always aroused the interest of the laymen, and remarkably, it has always been the least competent who have wasted their time in vain upon them. I am reminded of the trisection of the angle and of the quadrature of the circle. However, those who have dealt with these problems and still deal with them nowadays, even though they have been settled for a long time, are likely to be utterly unable to indicate adequately what is at stake. And recently also Fermat's theorem has joined the category of popular problems, after the prize of 100,000 marks for its solution has awaken a previously unsuspected "scientific" eagerness in much too many persons.

One can only wonder to what extent his views were shared by others, but there are two interesting testimonies that point to the same direction. The first appears in Klein's well-known *Elementary Mathematics from an Advanced Standpoint* [Klein 1939]. Like others before him, Klein stressed that the interest in this problem laid in the fact that attempts to solve it had been in vain thus far. For Klein, it was likely that the expected proof would come from the direction opened by Kummer, and at the same time it was unlikely that Fermat could have had in mind a proof related with that conceptual direction. Thus, he wrote, "we must indeed believe that he succeeded in his proof by virtue of an especially fortunate simple idea" (p. 48). He then referred to the dramatic change undergone by the status of the problem after the Wolfskehl prize was established, and pointed out that, while mathematicians typically understood the potential difficulty involved in solving the problem, "the great public thinks otherwise". He went on to describe with open disgust the "prodigious heap of alleged 'proofs" that were received since the price was announced. These were sent by people from all walks of life, including "engineers, schoolteachers, clergymen, one banker, many women", and what was common to all of them was that they had "no idea of the serious mathematical nature of the problem … with the inevitable result that their work is nonsense" (p. 49).

The second testimony comes from a text by Walther Lietzmann (1880-1959). Lietzmann completed in 1904 his dissertation in Göttingen under Hilbert, with a thesis on biquadratic reciprocity. Later on he continued to do some research on higher reciprocity laws [Lietzmann 1904, 1905]. He became a high-school teacher and principal, from 1919 to 1946, of the *Felix Klein Gymnasium* in Göttingen. He published extensively on didactics of mathematics. Throughout the years Lietzmann remained in close contact with Hilbert.[27] Therefore, it does not seem farfetched to assume that views expressed by Lietzmann agreed to a large extent with those of Hilbert. At any rate, it is illuminating to see what he has to say about FLT in a booklet devoted to the Pythagorean theorem, first published in 1912 and republished in successive new editions until 1965.

"Fermat's problem would not be on everyone's mouth", Lietzmann said, were it not for the Wolfkshel prize. Lietzmann indicated with satisfaction that while no one had yet been awarded the money of the prize (except, as already said, a small amount to Wieferich), Hilbert, as head of the prize committee, was making a very good use of the interests yield by the fund. Indeed, throughout the years Hilbert organized a series of Wolfskehl lectures in Göttingen, where the most prominent physicists of the time were invited to discuss pressing, open issues of the discipline. Some of these meetings became real milestones in the history of physics, such as a series of talks delivered by in 1922 Niels Bohr (1885-1962), where he presented for the first time to a relevant audience, his groundbreaking ideas about the structure of the atom.[28] Lietzmann also quoted a famous Hilbert comment regarding FLT: "Fortunately, there is no mathematician except me who is in a position to solve this question. However, I do

[27] See [Fladt 1960]; [Reid 1969, 89; 212-213].

[28] The various Wolfskehl lectures are described *passim* in [Corry 2004a]. For Einstein's lecture, see pp. 321-326. For Bohr, see pp. 411-414. In an unintended sense, FLT led, through the Wolfskehl prize, the Wolfskehl lectures and Hilbert's intiative, to the most direct, significant, and unlikely contribution of number theory to theoretical phsyics.

not intend to slaughter myself the goose that lays the golden eggs".[29] FLT may have been considered perhaps difficult, but not important enough so as to be worth the time and efforts of a Hilbert, and so that it would justify losing this money. In Lietzmann's opinion, moreover, the consequences of the creation of the prize were dreadful (*fürchterlich*). He thus wrote, in a spirit similar to Perron [Lietzmann 1912, 63]:

> In the past, well-known mathematicians, but above all editors of mathematical journals, received every now and then an attempted solution to the problem of the quadrature of the circle and the trisection of the angle, even though the impossibility of such constructions with straightedge and compass has been fully demonstrated. Nowadays the Fermat problem takes the place of these constructions, since here also resonant coins lure side by side with fame.

Faced with mostly trivially mistaken proofs, Lietzmann commented (partly amused and partly annoyed) that the senders must believe that mathematicians are truly stupid people if they were offering such a high amount of money for a question that can be solved in two lines of elementary calculations. Lietzmann complained that even the first edition of his own booklet, in spite of his warnings, had led to additional letters with attempted proofs. On the other hand, as with the years the fund lost most of its value, Lietzmann remarked in later editions of his book that the continued flow of attempts had receded and that this was perhaps the only positive consequence of interwar hyperinflation. He concluded with the following comment [Lietzmann 1965, 96]:

> For whom this is a matter of money, he will now have to do without. But whoever deals with this problem out of love for mathematics, to him I can advise to divert his urge to mathematical activity into other directions, in which the prospects for satisfaction, and perhaps even for some research results, are granted.

Another Göttingen mathematician whose name became associated with FLT and the Wolfskehl prize, and not precisely in a positive sense, was the leading number theorist Edmund Landau (1877-1938). Landau was a distinguished Berlin mathematician who in 1909 was appointed to Göttingen following the sudden death of Minkowski. Soon

[29] This qoutation appears differently formulated in various contexts, but always as hearsay. Lietzmann did not quote it in the first edition of his book, in 1912. It does appear in the seventh edition of 1965.

after his arrival he was commissioned with handling the Wolfskhel prize, and the dreary, unending correspondence associated with it. In order to cope with this rather unpleasant task, it is well known that Landau printed a standard answer in the following terms,

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Dear ........,

  Thank you for your manuscript on the proof of Fermat's Last
Theorem.
  The first mistake is on:
  Page ...... Line .....
  This invalidates the proof.

  Professor E. M. Landau
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He would then ask a student to read any manuscript sent and to fill in the missing details. For this reason alone, but possibly because his lack of direct interest in the problem, Landau was never enthusiastic about FLT and there are two interesting pieces of evidence for that. At the International Congress of Mathematicians held in 1912 at Cambridge, UK, Landau was invited to give a plenary lecture. The topic of his talk echoed to some extent that of Hilbert in 1900 and its title was “Solved and Unsolved Problems in the Theory of Numbers” [Landau 1912]. About two thirds of the problems and results discussed belonged to Landau’s own work, and for the rest he used this opportunity also to add his own considerations about what others had achieved in the field. The main topic was the distribution of primes and the Riemann Zeta-function. The talk also discussed several other topics of analytic number theory and the special functions defined in it. Like Smith in 1867 before a purely British audience, Landau still remarked in 1912 that his field was essentially unknown among mathematicians in general. The difficulty of its methods, he stressed, has not helped attracting many colleagues to become acquainted with the beautiful results of this theory. He used this opportunity to present the main challenges and important open problems of the field. As one may already expect, FLT was not even mentioned.

The second piece of evidence comes from a rather obscure, but indeed unique source. In 1927 Landau accepted the challenge to become the first professor of mathematics in the newly established Hebrew University in Jerusalem. He moved to Mandatory Palestine in 1927 with his family, intent on establishing residence in Jerusalem, a rather remote and provincial town at the time. Things did not develop as smoothly as he would have wished, and the Landau family returned to Germany one and a half year later. Still, the Landau spirit left a strong imprint in the incipient mathematical community that would eventually turn into the prestigious Jerusalem school of mathematics. In 1925, two years before his move to Jerusalem, Landau had been invited to give an official speech during the act of inauguration of the Hebrew University. The text of this speech is perhaps the first text dealing with a subject of advanced mathematics in modern Hebrew [Katz 2004]. Landau used once again the title "Solved and Unsolved Questions in Elementary Number Theory", but this time he aimed to an audience of laymen. In fact, Landau's list contains exactly 23 problems like Hilbert's, and Landau found it adequate to explain why he chose precisely this number, namely, because 23 is a prime number, which befits very much the topic of the problems covered by the list. One can hardly imagine anyone in the audience who may have ever heard about Hilbert's list, a list that was surely present in Landau's mind when preparing his speech.

In this very festive occasion we cannot imagine that Landau—an extremely meticulous mathematician anyway, who was careful about every word he ever chose for a text—prepared his list other than with great care and without any rush. After all, he was about to present the respectable audience of gathered in Mount Scopus to inaugurate the new university of the Jewish people in the Land of Israel with their first glimpse after two-thousand years into the current status of the classical discipline of arithmetic. A full description of the list is well beyond the scope of this article. Still, it is important to stress that in spite of the word 'elementary' appearing in the title, the list also included some rather advanced problems associated with both the analytic and the algebraic tradition in number theory. FLT, to be sure, did not appear there. In fact, also the Riemann conjecture that was at the focus of his 1912 lecture was left out of this list. But taking into consideration the kind of audience addressed here and the context of the talk, this decision may seem justified by the apparent difficulty in explaining the conjecture. This difficulty, of course, does not exist in the case of FLT. Its absence can only be taken as an indication of Landau's lack of attraction, or perhaps even negative attitude, towards it.

7. FLT in the twentieth century: developing old ideas, discovering new connections

Side by side with the negative reactions discussed above, one can also indicate after 1915 a gradually increasing tide of texts that reached a relatively broad circle of readers and that consistently stressed the historical importance of FLT while explaining the development of the main, related ideas. As already mentioned, in his three-volume book Dickson stated very clearly his opinion about the lack of *mathematical* importance of FLT. But in a 1917 article that is often cited in contemporary accounts, he stressed that the *historical* importance of FLT would never diminish even if a complete proof was found. This importance stemmed, of course, from the rise of the algebraic theory of numbers in the wake of Kummer's work, which Dickson described, like others before him, as having been created in order to deal with FLT. Dickson's 1917 paper gave a systematic and very sympathetic account of the relevant ideas, especially concerning Kummer's contributions.

Another interesting example appears in the work of Paul Bachmann (1837-1920). Bachmann had studied with Dirichlet and Kummer, and was a lifelong close friend of Dedekind. He published several comprehensive textbooks on the theory of numbers that were widely circulated and used in the German-speaking world [Bachmann 1892-1923; 1902-1910]. In all these textbooks, FLT received very little attention. In the historical introduction to one of his books, Bachman wrote at length about the decisive influence of Fermat in shaping the future development of the discipline [Bachman 1892]. He mentioned FLT, but only in passing, as one among a long list of problems that Fermat bequeathed to coming generations. Its peculiarity was that even prominent mathematicians had failed to solve it. In the various chapters of the book, however, the problem was not even discussed. His book of 1902 was an elementary introduction to the theory of numbers, and FLT is discussed there towards the end [Bachman 1902, 458-476]. Bachman described the most elementary techniques associated with some known proofs. The presentation was rather superficial to the extent that Bachmann claimed wrongly that all primes under 100 are regular (p. 461, but he corrected this in a Errata on p. 480). But then in 1919 Bachmann published a general account of FLT and its history that became a very popular and frequently quoted source [Bachmann 1919]. Bachmann suggested with evident disgust that following the creation of the Wolfskehl prize an immense amount of non-professional dilettantes had inundated the mathematical literature with their failed attempts. Still, he found that many ideas that had developed in relation with FLT were of enduring mathematical importance, and his book was an attempt to present those ideas to non-specialist in number theory.

A third and important example is that of Louis Mordell (1888-1972). Mordell was a leading authority in the field of Diophantine equations. Early in his career he had studied the solutions of $y^2 = x^3 + k$, an equation to which also Fermat had devoted some attention. In combination with a result previously obtained by Axel Thue (1863–1922), but unknown to Mordell at the time, his work implied that this equation had only finitely many solutions. In 1912 he was awarded the Smith Prize for his work on the equation that now is associated with his name [Davenport 1964]. Mordell also made important contributions to the theory of elliptic curves, the theory within which Wiles' proof would be formulated many years later. A famous conjecture he advanced in 1922 about rational solutions of certain Diophantine equations implied that for $n > 3$, $x^n + y^n = z^n$ could have at most a finite number of primitive integer solutions. The conjecture was proved in 1983 by Gerd Faltings. This result signified, on the face of it, a very meaningful advance towards a possible general proof of FLT, but no further progress was made on it, and eventually Wiles' proof came from a different direction.

Mordell was among the first prominent number theorists to speak consistently about the historical importance of FLT and to do so while addressing various kinds of audiences. In March 1920 Mordell gave a series of three public lectures at Birkbeck College, London, fully devoted to FLT, and to explain the methods that had been developed throughout the years in the attempt to prove it. These lectures were intended for persons with general mathematical background, but not necessarily in number theory [Mordell 1921]. In November 1927 he lectured at the Manchester Literary and Philosophical Society on recent advances in number theory trying to convey to a lay audience the excitement of his field of research [Mordell 1928]. FLT was one of the six problems he chose, and so was the already mentioned Euler conjecture (for which Elkies gave a counterexample in 1988). Like Dickson before him, Mordell stressed above all the historical, as opposed to intrinsic mathematical, importance of the problem. In fact, Mordell prepared his lectures on the basis of the information contained in Dickson's and Bachmann's texts, and following a similarly sympathetic approach. Dickson, Bachmann and Mordell together established the kind of discourse about FLT and its history that would dominate many later presentations of the subject.

Mordell was Sadleirian professor of mathematics at Cambridge between 1945 and 1953. In his inaugural lecture, he discussed again the Diophantine equation with which he opened his career, $y^2 = x^3 + k$. Mordell found it necessary to justify before his own mathematical colleagues, who "are familiar with obviously important and abstruse branches of mathematics", why in such festive an occasion he had chosen a question that may seem "insignificant and remote" to them. That he felt the need for such a justification says much about the status of this kind of questions within the profession. His explanation put the stress on the surprisingly long period of time that these problems had stood unsolved. He thus said [Mordell 1947, 5-6]:

> Most of the mathematical knowledge of the period 1600-1700, as far as algebra, geometry and calculus are concerned, would be described now as being thoroughly elementary and completely absorbed in the body of knowledge, with all its possibilities exhausted long since. This, however, does not apply to the questions in the Theory of Numbers discussed at that time, and there are several of permanent interest which have not sunk into anonymous obscurity.

He mentioned three examples of this: the Pell equation (actually solved in the seventeenth century), FLT and the equation $y^2 = x^3 + k$. In his talk, like in his entire career, Mordell focused on the latter equation rather than on FLT. Interestingly, Mordell explained how Fermat had posed this question as a challenge to English mathematicians, following the work of Bachet, who was the first to deal with it. Fermat had asked if there is another square, other than 25, that when added to 2 makes a cube. Fermat declared to have discovered "an exceedingly beautiful and subtle method which enables me to solve such questions in integers and which was not known to Bachet not to any other with whose writings" he was familiar. But Mordell did not believe that Fermat had indeed discovered such a method.

Like FLT, also the Diophantine equation that attracted Mordell's interest had been given some attention by Euler, Dirichlet, Legendre and others. Mordell's own work, however, opened a new direction that considered rational points of cubic curves as a main tool for finding all integer solutions of the equation. Indeed, this can be seen as part of a broader train of ideas in which significant applications of algebraic geometry and its methods were applied to problems related with Diophantine equations. Among its early contributions one finds works by Henri Poincaré (1854-1912) at the turn of the twentieth century [Poincaré 1901], and it is remarkable that also André Weil (1906-1998) (on whose work more is said below) participated in it with his earlier works [Weil 1928, Hindry 1999]. “I must emphasize – Mordell wrote when explaining the approach followed in previous works – that I am not underestimating the possibilities of elementary mathematics by which difficult results may often be proved.” But he was aware of pursuing now a totally different point of view, when trying to find certain rational solutions for the cubic curves that were related with his equation. He wrote [Mordell 1947, 28]:

> I was rash enough to write in 1921 that the prospects of any immediate solution of such problems appeared then almost as remote as that of discovering any knowledge concerning the chemical constitution of the stars appeared in say, 1800. This was not altogether an unjustifiable view, since a mathematician of course never knows whether the solution of a difficult problem requires principles or ideas which may not be discovered for hundreds of years to come.

In the specific context of his problem, Mordell discovered that all the rational points of the general cubic curve could be generated from a finite number of them. His work opened the way for additional, important investigation by others, but more generally, it indicated that the solution of some seemingly elementary problems of number theory would need to be solved by new methods that could hardly be considered “elementary” in the same sense. This was to be the case with FLT, even though Mordell was not thinking about that neither in 1912 nor in 1945. Still, the train of ideas leading from Mordell’s conjecture to Faltings’ proof is one of the remarkable and well-known advances related to FLT in the twentieth century. In the end, however, it did not lead to a complete solution of FLT. It suggested that perhaps a proof of FLT might be closer than ever, but it also made clear that the idea of an *elementary* proof would have to be relinquished, like Mordell had suggested in 1945 for his equation.

Additional attempts and results continued to support this possibility. For instance, the joint contributions of Len Adleman, Roger Heath-Brown and Étienne Fouvry in 1985 implied that there exist infinitely many primes for which case I of Fermat's last theorem holds [Fouvry 1985; Adleman & Heath-Brown 1985]. These works drew together ideas taken from an astounding amount of diverse mathematical sources.[30] There was also an attempt in 1988 by Yoichi Miyaoka that elicited much interest because the methods he developed in arithmetical algebraic geometry seemed to indicate that a wealth of difficult mathematical problems might perhaps be solved following a similar approach. In 1987 A.N. Parshin developed an arithmetic analogue of an important inequality on topological invariants that Miyaoka himself, working on problems of differential geometry, had proved in 1974 [Parshin 1989]. Parshin suggested that if this arithmetic analogue were true, then FLT would also be true. Miyaoka then announced, a year later, a proof of this analogue to his own inequality. Quite soon, however, it turned out that Miyaoka's proposed proof contained several flaws [Cipra 1988]. Interestingly, flaws encountered in the proof, did not in themselves diminish the kind of enthusiasm surrounding Miyaoka's attempt, and in a certain sense they actually increased it. By this time, also the line of attack that would finally lead to Wiles' proof had already been initiated, starting from a totally different direction.

What all these important developments highlight is that FLT came gradually be seen within the framework of an increasingly evident trend in number theory. Problems involving Diophantine equations, some of which can be formulated in very elementary terms and which had been seen in the past within a much more reduced perspective, started to appear now as intimately connected with deep and intricate mathematical topics pertaining to broad range of fields of enquiry such as algebraic geometry or differential geometry. This trend was eliciting a new kind of interest in both older and more recent problems in number theory – FLT included – and many mathematicians were eager to stress it [Lang 1990].

Parallel to this, another trend with far reaching consequences had also been developing in number theory, whereby an increasingly important role was accorded to unproven, overarching conjectures, as a focus of attention around which new and interesting mathematics is produced. Of course, unproven conjectures had always been part and parcel of number theoretical research (FLT being a case in point). But now, when considerable evidence existed for the plausibility of such conjectures they were often being taken as hypothetical starting points for the development of

[30] See *Math. Rev.* MR 87d: 11020.

elaborated theories. This trend was singled out by Barry Mazur as a unique, central characteristic of twentieth-century mathematics [Mazur 1997]. At the confluence of these two interesting and interrelated trends, FLT became a focus of special attention in a sense that differed from the traditional one. Indeed, Mazur explicitly stressed this situation in 1991, three years before Wiles presented in public for the first time his proof. He spoke then about TSW as a foremost example of a "profoundly unifying conjecture" that "plays a structural and deeply influential role in much of our thinking and our expectations in Arithmetic." The status of FLT could thus be formulated in this context as follows [Mazur 1991, 594]:

> Fermat's Last Theorem has always been the darling of the amateur mathematician and as things have progressed, it seems that they are right to be enamored of it: Despite the fact that it resists solution, it has inspired a prodigious amount of first-rate mathematics. Despite the fact that its truth hasn't a *single* direct application (even without number theory!) it has, nevertheless, an interesting *oblique* contribution to make to number theory; its truth would follow from some of the most vital and central conjectures in the field. Although others are to be found, Fermat's Last Theorem presents an unusually interesting "test" for these conjectures.

Thus, after close to 350 years of history, FLT appeared as a problem with deep yet *purely tangential* importance (even though, one might perhaps qualify the claims about the prodigious amount of first-rate mathematics inspired by FLT). FLT suddenly appeared as more important that it could have ever been considered along the years not because of intrinsic mathematical interest and consequences, but rather because what it does to the status of one specific, deep and "structural", "unifying conjecture" of mathematics.

It would be beyond the scope of the present article to describe the story of Wiles' breakthrough in detail. Still, it is important to briefly consider it within the framework discussed thus far in this article. The train of ideas more directly leading to Wiles' proof of FLT may be traced back to 1955. At that time, in a symposium held in Tokyo, Yutaka Taniyama (1927-1958) presented two problems on the basis of which a conjecture was formulated somewhat later. The conjecture establishes a surprising link between two (theretofore) apparently distant kinds of mathematical entities: "elliptic curves" and "fields of modular forms". Goro Shimura (1930-) and Weil took part in refining the conjecture over the following years and formulating it in a very precise fashion. Eventually it came to be known as the "Taniyama-Shimura", or

"Taniyama-Shimura-Weil" conjecture (TSW). Only many years later its possible connection with FLT became apparent. The fact that Taniyama committed suicide in a tragic and enigmatic manner has also been harnessed to enhance the dramatic qualities of the story of FLT. Many years after it saved Wolfskehl from suicide, one may be led to believe from some accounts, it did the opposite to a despaired Taniyama. Of course, Taniyama himself had no clue about the connections that would arise between the problems he suggested and FLT, neither in 1955 nor at the time of his death. The reason for his suicide in 1958 has remained unclear to this day, but one thing is sure: it had no connection whatsoever with TS and much less so with FLT.

The road that finally led to Wiles' proof via a possible link between TSW and FLT derived from ideas found in the separate works of Yves Hellegrouach and Gerhard Frey. In his doctoral dissertation, [Hellegouarch 1972] as part of an attempt to show the inexistence of points of certain orders in elliptic curves, Hellegouarch associated an elliptic curve to a hypothetical integer solution of equation (1) with exponent $2p^n$, with n ≥ 1 and p prime. In the mid-eighties, Frey came up with a similar association and constructed a certain curve based on a supposed integer solution of $x^n + y^n + z^n = 0$. This curve appeared to be non-modular. This meant that the falsity of FLT seemed to imply the falsity of TSW. The idea behind the non-modularity of Frey's curve was precisely formulated by Jean Pierre Serre in terms of Galois representations, in what became known as the epsilon conjecture, which was proved in 1986 by Ken Ribet. At this point, a full proof of FLT appeared to be at hand, albeit just in theory, for the first time. It was necessary, though, to complete a clearly definable, but very difficult task: proving TSW. Experts at the time, however, essentially coincided in considering such a task to be inaccessible. Not so Wiles, though. Immediately upon hearing about Ribet's proof of the epsilon conjecture he decided that it was time to return to his old mathematical love, FLT, and to prove it by providing a proof of TSW. The rest of the story is well known by now.

This chapter in the history of FLT, involving the final stages leading to Wiles' proof as delineated above, is an inspiring story of uncommon mathematical determination that was crowned with success. The amount of unexpected mathematical difficulty that appeared at the end of an unusually long quest amply justifies the excitement with which this achievement has been saluted. From a historical point of view, however, it one should not let this excitement make us forget other developments related with FLT that took place independently of those mentioned above and that evolved along a completely different path of events. This alternative path did not lead to a general proof of FLT and perhaps for this reason they have been now somewhat forgotten. Still, some of its main milestones are openly documented in accounts written in the late 1970s on the state of the art on FLT (e.g., [Ribenboim 1977, 1980], [Edwards 1977]). In these accounts, none of the names that came to be famously associated with the theorem after Wiles' proof, such as Taniyama or Shimura, appear. By contrast, many do appear in those accounts who were not mentioned, or were barely so, in Dickson's list of 1919, and that in spite of their involvement with FLT after 1920, were often forgotten in accounts written after 1994. I would like to devote the remainder of this section to discuss some twentieth century works on FLT that did not lead to Wiles.

Quantitatively speaking, most efforts devoted to FLT in the twentieth century represented a continued exploration of ideas related with Kummer's methods, albeit with some historically interesting twists. Indeed, as already seen in relation with the works of Mirimanoff and Wieferich, some progress was done along these lines in the first decade of the twentieth century, even if limited and slow. In the following decades FLT continued to be a problem to which the mainstream of number theory paid little attention. The first mathematician whose work must be mentioned here was the Danish Kaj Løchte Jensen (not to be mistaken for Johan Ludvig Jensen (1859-1925)). Very little is known about Jensen. Apparently he was a student of Niels Nielsen (1865-1931), a versatile mathematician who became interested in Bernoulli numbers around 1913 [Nielsen 1913-14]. It seems that Jensen could not complete a dissertation due to some kind of mental breakdown, but still as a student in 1915 he published in a remote Danish journal a proof of the existence of infinitely many irregular primes of the form $4k + 3$ [Jensen 1915].[31]

[31] I thank Jesper Lützen and Christian U. Jensen in Copenhagen for information on Jensen (for which unfortunately I do not have direct evidence). This information originated with Thøger Bang who presumably heard it from Harald Bohr.

Kummer had initially assumed that in proving FLT for regular primes, he was proving it for an infinite number of cases. Nowadays, there are heuristic arguments to support such an assumption, but no definite proof of it [Washington 1997, 63]. In spite of the fact that this question arises naturally when following the line of attack derived from Kummer, it was not until 1915 that someone devoted some systematic thought to it. This was an unknown student whose teacher happened at the time to be looking at Bernoulli numbers, and what he did was to prove a result about irregular primes, rather than about the regular ones. His proof was rather straightforward and did not require any special idea or technique that was not known to number theorists since the time of Kummer. Jensen asserted that the result he had proved was then commonly assumed. This may have indeed been the case, and this is in any case what Jensen heard this from his teachers. I have found no written evidence of discussions pertaining to the question at the time. At any rate, Jensen did connect it, but only in passing, with FLT. He wrote:

> That there exist infinitely many irregular primes has been conjectured for a long time but as far as I know it has not been proved. It is well known that it has a certain importance for the evaluation of Kummer's investigations of Fermat's last theorem.

Jensen's result was not mentioned in Dickson's 1919 account, but only in a follow-up published several years later [Vandiver & Wahlin 1928, 182]. Harry Schultz Vandiver (1882-1973) published Jensen's argument in English for the first time only in 1955 [Vandiver 1955], and he stressed that by that time it was not yet well-known. One year earlier, Leonard Carlitz (1932-1977) proved a similar, but more general result without the limitation $4k + 3$ [Carlitz 1954].

The same Vandiver is the second mathematician I want to mention here, and he is also some kind of outsider, though in a very different sense that Jensen was. Vandiver did never go the usual training of number theorists, and when he did gain a more institutionalized status as a mathematician, he continued to follow a self-fashioned, original path. He never completed high-school, and the little college-level mathematics he studied, he studied in rather haphazard, non-systematic way. In 1900 he started publishing original research work in various journals, some of it in collaboration with George David Birkhoff (1884-1944), the most influential mathematician of the time in the USA. Later on, in 1919, with Birkhoff's endorsement, Vandiver got a position at Cornell. That year he collaborated with

Dickson in the preparation of the latter's book on the history of the theory of numbers, especially the chapter on FLT. In 1927 he published a follow-up of that chapter. After Cornell, Vandiver moved to the University of Texas, Austin, where he obtained a permanent appointment.[32]

Vandiver is the only person in this story, other than Wiles, to have truly devoted a considerable part of his professional life to work on FLT. Most possibly he was never "tormented" by the theorem and probably not even "obsessed" with FLT, but he certainly did a great amount of work specifically devoted to proving it. This choice was definitely unusual in the American, as well as in the international mathematical community. In 1914 he extended the Wieferich-Mirimanoff type of criterion for case I to basis 5 [Vandiver 1914], (i.e., he proved that if the equation $x^p + y^p + z^p = 0$ is solved in integers relatively prime to an odd prime p then $5^{p-1} - 1$ is divisible by p^2).

Beginning in 1920, Vandiver published a long series of articles on FLT. Among other things, Vandiver was the first to make significant progress since Kummer concerning case II. He also corrected some of Kummer's proofs [Vandiver 1920]. Of special importance was a paper published in 1929 where he summarized some of his early work and proved the validity of FLT, for both case I and case II, for exponents up to 269 [Vandiver 1929]. For this article he was awarded by the AMS the first Cole Prize for an outstanding work in number theory.

At the center of Vandiver's work was a sustained attempt to refine Kummer's criteria for proving FLT for irregular primes. He added theoretical insight into the properties of the Bernoulli numbers and developed algorithms for facilitating their calculation [Vandiver 1926-27]. More interestingly, he was the first to conduct this kind of research in number theory while systematically applying two strategies not commonly found theretofore in the discipline. The first one was, simply, division of labor. In his 1929 article, for instance, he was assisted by young members of the faculty in Austin, such as Samuel Wilks (1906-1964) and Elizabeth Stafford (1902-2002). Each collaborator was assigned a certain part of the range of numbers to be investigated.

[32] Very little historical research has been devoted to Vandiver, to his work as mentioned here, and to his interesting collaboration with younger colleagues. An initial attempt appears in [Corry (forthcoming)].

The second strategy was the massive use of number-crunching devices for support. In this case, he used electro-mechanical Monroe and Marchant calculators then available, but in subsequent research he also tried additional possibilities.

It is remarkable that results similar to those of Vandiver were achieved at roughly the same time by another person, and again this was a complete mathematical outsider at the time: Otto Grün (1888-1974). When he was over forty, and without any formal background in mathematics, Grün initiated a correspondence with Helmut Hasse (1898-1979), then at Marburg. Hasse was not directly involved in research on FLT and indeed at that time he was deeply immersed in his own attempts to prove the Riemann conjecture for curves. But he was definitely impressed by Grün's ideas, even though he was not fully aware that Grün then made his life in commercial occupations rather than in mathematics. As he did in many similar occasions Hasse devoted considerable energies to react to Grün's letters and to correct many mistakes he found in the work of a man who could not previously enjoy of any serious feedback for his ideas. Hasse highly encouraged Grün to continue with his original efforts. As he had been previously in contact with Vandiver, Hasse also facilitated the communication between the latter and Grün, and Vandiver explained in letters the specific points in which their results coincided or differed. Very soon, however, as Grün's mathematical activities and knowledge became more and more articulated and well-grounded, he abandoned his interest on FLT and his focus moved to class field theory and, above all, to group theory. He went on to make highly interesting contributions to this latter field but, unfortunately, his mathematical career never reached institutional stability in spite of efforts by many to help him.[33]

Going back to Vandiver, the most important of his collaborations was the one held with the young couple from Berkeley, Emma (1906 -2007) and Derrick (1905-1991) Lehmer. In 1937 they were able to prove FLT for prime exponents smaller than 619, including 36 cases of irregular primes [Vandiver 1937]. The technical limitations imposed by the machine used at that opportunity did not allow them going beyond that limit. But the truly decisive step forward was taken in 1954 when Vandiver

[33] The correspondence between Hasse and Grün was uncovered and published only recently in [Roquette 2005].

continued his collaboration with the Lehmers, using for the first time a general purpose, programmable electronic computer, SWAC, for a problem of this kind. The Lehmers had been previously involved in developing "standard" proofs of FLT, as well as in the application of analog computers to the calculations related with prime numbers. Specifically, they made significant calculations related with the classical problem of finding Mersenne primes. In 1941, for instance, the couple published a joint paper which presented a synthesis of various methods applied then to FLT, and that allowed them proving case I of FLT for all exponents smaller than 253,747,899 [Lehmer & Lehmer 1941]. In their joint paper of 1954 using the electronic computer, they showed that almost half of the primes smaller than 2500 are irregular, and they proved the theorem up to this value [Lehmer, Lehmer & Vandiver 1954].

This is not the place to describe the details of this interesting collaboration [see Corry 2008a, 2008b]. What was said is enough to indicate that it proceeded along a path that was, and in many sense remained, outside the mainstream. Still, the kind of work initiated by Vandiver and his collaborators opened a new direction of research, which is still alive and well until this very day. Calculation techniques with digital computers were strongly developed after 1951, but their use in mathematics in general and in particular for finding proofs for various questions related with number theory evolved in a very slow and hesitant manner. It has become increasingly common over the last decades, but it is yet far from having become mainstream. Within this trend, additional proofs of FLT along similar lines continued to appear up to exponents over one billion, and case I of FLT up to values much higher than that [e.g., Wagstaff 1978; Granville & Monagan 1978; Buhler, Crandall & Sompolski 1992]. In fact, articles of this kind continued to be published even after Wiles completely general proof [Buhler et al. 2000]. One should not be surprised to see additional articles of this kind appear in the near future, and, perhaps, more general attempts at computer-assisted "elementary" proofs of FLT.

In 1959, the mathematician Heinrich Tietze (1880-1964) summarized the current status of FLT, as he saw it, in a book devoted to presenting "Famous Problems of Mathematics". Mentioning the fact that although it had been mostly addressed by "flood of amateurs", also some professionals had occasionally returned to it, mostly on the wake of the Wolfskehl Prize. These professionals, he said, were aware of the difficulty of the problem, and of "the profound theories which must be mastered" before one can appreciate the work already done in the field, and he concluded [Tietze 1965, 286]:

> Perhaps some day Fermat's problem will be completely solved. It will be a real achievement and if the solution yields new methods of research and new problems, it will be a new milestone. If not, our desire of knowledge will have been gratified, but Fermat's problem in itself would then occupy no more than a modest place in the history of mathematics, and this principally because it stimulated Kummer's work.

Faced with some further historical evidence, I believe that Tietze would even agree to temper his claim in relation with Kummer, but at any rate this seems to me a sober and balanced historical appreciation of FLT that had the additional virtue of foreseeing how a groundbreaking work, like Wile's would eventually turn out to be, would completely change the minor historical importance of this problem.

8. *Concluding Remarks: the Fermat-to-Wiles drama revisited*

Within the entire story of FLT, the episode involving Wiles and his contribution to finally proving the theorem is no doubt the one that comes closer to a real drama of the kind that some accounts have tried to create around this mathematical tale. Perhaps the word "obsession" can be used here with some degree of justification, especially to describe the years of self-imposed seclusion during which Wiles worked out his proof and the unexpected discovery of a non-trivial mistake that took about eight months, and the collaboration of Richard Taylor, before it could be corrected. Because of this delay Wiles could not be awarded the Fields medal at the 1994 International Congress of Mathematicians in Zürich, but then he was duly recognized in 1998 in Berlin with a special award by the Fields medal committee, obviously expressing an overall consensus in the international mathematical community. Also the recent 2006 ICM at Madrid was marked by the sensational solution of another, long standing, famous open problem, the Poincaré conjecture (PC), by Grigori Perelman. Also here a somewhat dramatic tale surrounded the solution and the award, thus giving rise to its own share of media coverage and popularization efforts of various kinds. It seems tempting to me, therefore, to make a brief comparison, in this concluding section, of the different ways in which these two problems appear in the history of the Field medals. It definitely appears as natural, and even self-evident, that both Perelman and Wiles were considered to be in the category of Fields medal recipients for their work. There is little to be said in this respect. But, interestingly enough, *previous* to them, FLT and PC featured very differently in terms of the history of this award.

At least three mathematicians were awarded the medal for work directly connected with PC: Stephen Smale (1966), William Thurston (1982), and Michael Freedman (1986). In the cases of Smale and Freedman this connection was explicitly mentioned as the main reason for the medal in the presentation at the respective ICMs [Thom 1968, Milnor 1987]. In the case of Thurston's presentation PC was not mentioned but the connection was by all means implicitly clear [Wall 1984]. Also René Thom (1958), John Milnor (1962) and Serge Novikov (1970) received the prize for work related to classifications of manifolds and cobordism, within which PC always attracted prominent (though not exclusive) attention [Hopf 1960, Whitney 1963, Atiyah 1971].

The case for FLT was very different. Gerd Faltings is the only Fields medal recipient (1986) whose work can is directly related to the history of problem. The works of other number theorist among the recipients, Klaus Roth (1958) and Alan Baker (1970), went in completely different directions anyway, certainly at the time of their receiving the prize.[34] The dedication speech for Faltings was pronounced by Barry Mazur who already advanced here some of his ideas, mentioned above, on the unique role that certain "structural conjectures" play in twentieth century mathematics. The importance of Faltings' proof of the Mordell conjecture derived, in his view, precisely from the fact that it was intimately connected with some of those "outstanding conjectures – fundamental to arithmetic and to arithmetic algebraic geometry." And the outstanding conjectures mentioned by Mazur were Shafarevich's conjecture for curves, the conjecture of Birch & Swinnerton-Dyer, and the semisimplicity conjecture of Grothendieck. Moreover, Mazur stressed that based on the ideas developed by Faltings we could "expect similarly wonderful things in the future", and these would relate to topics such as moduli spaces of Abelian varieties, Riemann-Roch theorem for arithmetic surfaces, or p-adic Hodge theory. Not only FLT was not mentioned here specifically in relation with Faltings proof, but, more surprisingly perhaps, TSW

[34] But see [Baker 1982].

was also absent from the general panorama of fundamental conjectures described by Mazur, and this in spite of his following, very clear pertinent statement:

> Nowadays the analogy between number fields and fields of rational functions on algebraic curves (over finite fields) is so well imprinted upon our view of both number theory and the theory of algebraic curves that it is hard to imagine how we might deal with either theory, if deprived of the analogy. [Mazur 1997, 8]

Here, like throughout the entire article, my point in referring to this evidence related to the Fields medal is not to express any mathematical judgment about the intrinsic, relative importance of the two problems but merely to add another interesting perspective from which to consider the ways in which this importance was perceived *before* the impressive, respective achievements of Wiles and Perelman. Contrary to what is the case with PC, the medal dedications during these years never presented FLT at the center of an acknowledged, great plan of research that attracted continued attention and efforts meriting the institutionalized, top formal recognition of the international mathematical community.

Wiles' fascination with FLT, I would like to say to conclude on a different note, reportedly started in his childhood when he read Eric Temple Bell's *The Last Problem*. On looking at the example provided by his career, and in spite of the tone of my account thus far, I must acknowledge at this point that over-dramatized accounts of mathematical tales have undeniable virtues. Indeed, over-dramatizing and telling the history of mathematics as a series of essentially undocumented legends about mathematical heroes is the main narrative strategy famously followed by E.T. Bell in his book about FLT as well as in his better-known and indeed legendary *Men of Mathematics*. This approach is what caught the imagination of many young readers (some of which became mathematicians), apparently including that of young Andrew Wiles. It does not seems risky to conjecture that, had a young, mathematically sensible child like Wiles, read a moderate account of the kind I have pursued here—for all the historiographical and scholarly qualities I hope it has—he was much less likely to become fascinated with the problem and to be led to imagine himself as pursuing a mathematical career with the hope of solving it.

On becoming a professional mathematician, Wiles realized that existing methods to address FLT seemed to be thoroughly exhausted. Focusing on an attempt to prove the theorem did not appear to be a reasonable way to build up a mathematical career. Without thereby loosing his “emotional attachment” to the problem, Wiles nevertheless built a distinguished career while working on other fields, especially elliptic curves. Upon hearing in 1986 about Ribet’s proof of the epsilon conjecture, his old interest on FLT was rekindled. Wiles had not previously believed that the epsilon conjecture would be correct. But now, upon hearing of Ribet’s proof, several significant factors converged into a situation where proving FLT had become a mathematical task to which he could devote all of his energies: FLT, the problem for which he was passionate for so many years, was to be derived from a mathematical conjecture that evidently posed a difficult professional challenge, that was considered to be highly important and that, at the same time, he now believed to be true. His strong background on elliptic curves only made the achievement of the task appear as even more plausible. But without the direct, emotionally-laden motivation to prove FLT, TSW alone, for all of its acknowledged mathematical importance, would probably not have provided Wiles with enough fuel to undertake this long and difficult quest. This is the crossroads at which he reached the decision to devote his time now exclusively to proving TSW, so that FLT could be finally established. Bell’s kind of dramatic approach to the history of mathematics is indeed problematic for the professional historian, but one must acknowledge that it did play a direct role in triggering the series of events that would eventually lead to the solution of FLT (and, as a bonus on passing, TSW as well).

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