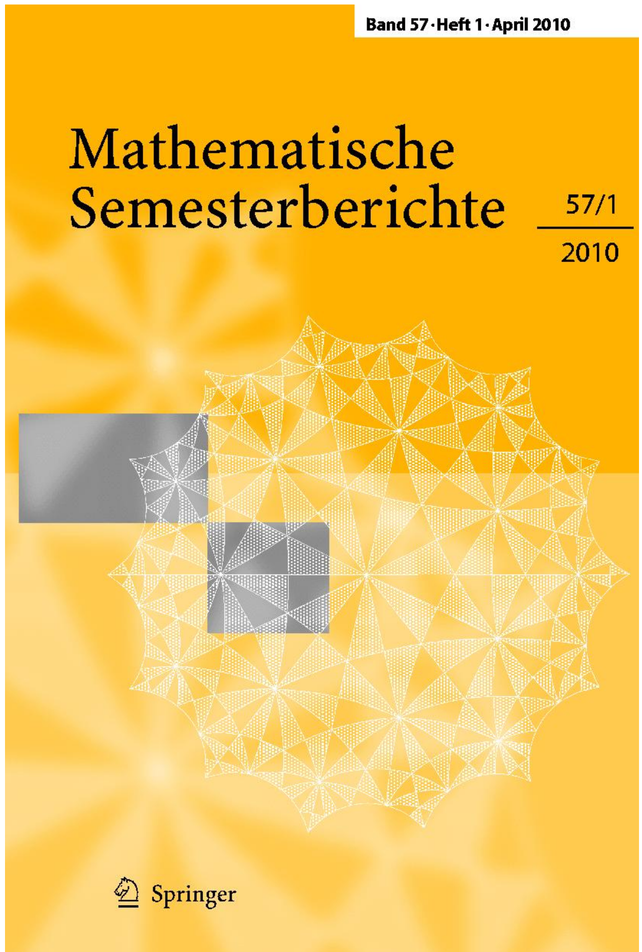
ISSN 0720-728X, Volume 57, Number 1



Math Semesterber (2010) 57: 12
DOI 10.1007/s00591-010-0068-4

On the history of Fermat's last theorem: fresh views on an old tale

Leo Corry

Received: 10 October 2008 / Accepted: 15 November 2009 / Published online: 3 February 2010

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*.

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." In the book one can read that the problem "tormented lives" and "obsessed minds" for over three centuries, thus constituting "one of the greatest stories imaginable". The front flap states that FLT became the Holy Grail of mathematics and that Euler, the most outstanding mathematician of the 18th century, "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."

L. Corry (✉)
Tel-Aviv University, 69978 Ramat Aviv, Israel
e-mail: corry@post.tau.ac.il

"The Last Theorem", one reads 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" [36, xv].

Descriptions of this kind have done much to enhance the dramatic qualities of the story, and thus 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] Still, from the point of view of a more sober, detached kind of historical perspective, not only the journalistic, over-dramatized style of his account leaves much to be desired, as do many similar ones published in the wake of Wiles' achievement. Indeed, the real and interesting problems with this account arise with some of its basic historiographical guidelines, since they also tacitly underlie many serious historical texts, and especially many historical sketches that accompany technical expositions of the mathematics associated with FLT. Such guidelines include, on the one hand, a clear tendency to overlook the proper historical context of some work that is now associated with FLT, and thus to misjudge – and typically to overstate, either implicitly or explicitly – the importance it was accorded by its authors and by their contemporaries. On the other hand, they also promote a marked disposition to underestimate or sometimes even ignore contributions that were explicitly conceived as attempts to prove the conjecture, but that eventually were abandoned because they were found unfruitful.[2] The quest to prove FLT may have been, in terms of the number of years it spanned, one of the *longest* pursuits in the history of modern mathematics (and indeed one with a most fascinating *dénouement*) but a different question is how *important* it really was in historical perspective, or, more precisely, in *what sense* was it important, for whom (and for whom not), and when?

In a series of recent articles I have tried to take a fresh look into the history of FLT and to offer some new vistas on this old and often told – but recurrently decontextualized – story. By setting some of the main historical facts straight and in their proper historical context, not only most of the dramatic tones of the story evaporate – suicides, transvestism, duels at dawn, and deception were never, of course, part of this mathematical tale – but it readily transpires that the "great heroes of mathematics" actually devoted very little attention to FLT, if they did at all. Indeed, after its formulation by Fermat and before the specific thread that culminated in the work of Wiles – a thread that was exceptional within the story, not only because the deep mathematics it involved, but also because of some truly dramatic moments it contained – prominent mathematicians devoted to FLT serious work only occasionally and then for brief periods of time. When FLT aroused their curiosity, it was mainly in a passive and typically ephemeral way. Thus in historically discussing any particular instance of a focused effort devoted to FLT, it is advisable to pay special attention to the question of the specific weight that the said effort carried within both the overall picture of the individual mathematician involved in it, and the overall picture of the discipline at the time.

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

[2] The case of Harry Schultz Vandiver, typically ignored in existing accounts, and to be discussed below in some detail, offers a good example of this.

In the present paper I would like to summarize some of the main issues discussed in my published and forthcoming articles, while directing interested readers to the full texts for further details.[3] It may be useful to clarify in advance, that I did not attempt to provide a comprehensive exposition of the main mathematical results that accumulated throughout generations of research in number theory in either direct, or indirect, or merely incidental relation with FLT. Much less did I try to offer a general account of the ideas necessary to understand Wiles' proof and the related mathematics.[4] Finally, I did by no means intend to offer a personal evaluation of the intrinsic mathematical importance of FLT, or of any of the efforts directed at solving it. Rather, what I have attempted in these articles is an analysis of the changing historical contexts within which mathematicians paid more or (typically) less attention to FLT as part of their professional activities, and of the relative importance they accorded to the problem. I have also discussed the ways in which the changing conceptions of the aims and scope of the discipline of number theory affected the intensity and the perspective with which the problem was addressed (in those cases in which it was addressed), or from which it was ignored.

1 Lives partially devoted

As a starter, while lives were of course never sacrificed on the altar of FLT, one may still ask to what extent some professional lives were ever devoted to attempts to solving it. Very few names, it turns out, can be associated with various degrees of "partial-life dedication". Wiles is the most obvious one. The second one is Sophie Germain (1776–1831), the first person to devote sustained efforts to FLT and to come up with a well-conceived strategy to attack the problem in its generality. Her case helps illustrate the kind of analysis that I have pursued in my account. Germain's ideas are interesting and original, but of course, her strategy had intrinsic limitations which become evident to us with the benefit of hindsight. The point which is historically interesting, however, is that Germain's attraction to FLT covered only a delimited part of her career, and that even this partial attraction cannot be taken as evidence for a general, contemporary attitude towards the problem. To the contrary, whatever attention she devoted to FLT was both result and evidence of her relative institutional isolation. Let me explain this point is some detail.

Germain's correspondence on arithmetic with Carl Friedrich Gauss (1777–1855), which started in 1804, is often cited as evidence of the strong impression she made *because* of her ideas on FLT. It is a plain fact, however, that beginning in 1808 her scientific attention focused on a different field in which she gained recognition, even though in retrospective her work was essentially flawed: the theory of vibrations for elastic surfaces. The French Academy offered an important prize for original research in this very difficult problem, and Germain presented three memories between 1811

[3] [6–10]. The articles explore different aspects of the story, obviously with some overlapping among them. PDF versions of these texts can be found in my websiste: http://www.tau.ac.il/~corry/publications/articles.html.

[4] Such accounts appear, for instance, in [13, 14, 22, 34, 35, 39].

and 1816. For the last one she was awarded the prize. At the same time, in 1815 and then again in 1818 the French Academy 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 stressing that she had never stopped thinking about number theory. Gauss's replies to Germain's letters of 1804–1809 reflect indeed admiration and respect, and these only grew upon his realizing the gender of his correspondent, as the story goes. 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 too late.[5]

Now, in her letters of 1804 to 1809, Germain mentioned some ideas about possible ways to solve FLT but she wrote mainly about two central topics treated in Gauss's *Disquisitiones*, namely reciprocity and quadratic forms. In all of his replies to her, Gauss *never* ever referred to the points she raised in relation with FLT. Also in a letter of 1819 Germain stressed that she had used ideas taken from the *Disquisitiones* as the basis of her strategy to prove FLT. She explained her ideas in detail and specifically asked Gauss about their possible importance. Gauss never answered her. He was obviously busy at the time with other fields of interest and as a matter of fact he had already estranged himself for many years from direct involvement with number theory. But a more direct explanation for Gauss's lack of an answer is that, for him, FLT was an essentially uninteresting question. Indeed, in a well-known letter of 1816 to his friend, the astronomer Wilhelm Olbers (1758–1840) [19, Vol. 2, p. 629], Gauss confessed that FLT "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". Commenting on his own theory of complex integers, which he had developed as part of work on higher reciprocity, Gauss stated that a generalization of this theory would no doubt lead to important breakthroughs. FLT, he added in passing, "would appear only among one of the less interesting corollaries" of such a generalized theory.

It is clear, then, that Gauss highly regarded Germain's mathematical abilities but not always her choices, which developed in the framework of a self-imparted education. An equally ambiguous attitude towards the importance of her work is that of her second mathematical benefactor, Adrian Marie Legendre (1752–1833). 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. In a letter of 1819 Legendre wrote her discouragingly about her line of research [37, p. 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 considerably 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."

[5] For recent research on Germain, based on previously upublished material, see [11, 12].

If Germain and Wiles count as two who devoted several years of serious, sustained research focused on an attempt to proving FLT (with obvious differences, of course), there is only one mathematician whose *entire* professional life may be said to have been devoted, more than anything else, to systematically studying FLT: Harry Schultz Vandiver (1882–1973). A glance at his life and work provides a good example of the kind of new historical insights that may arise from the perspective I have followed in my account. In the first place, it is very symptomatic that Vandiver, an interesting and rather forgotten figure, is mentioned marginally, if at all, in most of the existing historical accounts of the problem (in Singh's book, for one, one would look in vain for his name). It is also symptomatic that, like Germain before him, his mathematical education was self-imparted outside formal academic frameworks. He was as a high-school dropout who never received a complete and systematic college-level mathematical training, and who, as part of a small and relatively isolated community of mathematicians interested number theory in the USA in the early twentieth century, developed a self-styled career where he chose as his main research a problem that ranked low in the research agendas of the leading European number-theorists.

In his life-long quest to deal with FLT Vandiver did not come up with new general concepts or with overarching theories that would afford completely novel perspectives to number theory. Rather, he approached this problem as a meticulous technician who is willing to explore and exhaust the unexploited potential of existing theories, while refining them where necessary. He was undaunted by even the most demanding computations, aiding himself with any available tools and persons. Vandiver's work on FLT is a direct continuation of the pioneering work of Ernst Eduard Kummer (1810–1893), dating back to the mid-nineteenth century. It is well-known that Kummer developed some ideas that provided an important breakthrough and around which some of the scarce research on FLT was conducted thereafter. This refers in particular to the concept of regular and irregular prime numbers, and their relationship to Bernoulli numbers, about which I would like to say some words here before further elaborating on Vandiver.

Kummer's ideas are interestingly related to a discussion on FLT held in 1847 at the Paris Academy among prominent mathematicians that included Augustin Louis Cauchy (1789–1857), Gabriel Lamé (1795–1870), and Joseph Liouville (1809–1882).[6] A possible proof of the theorem was suggested, based on representing a sum of integers as a product of certain complex numbers, as follows:

$$x^p + y^p = (x+y)(x+\zeta y)(x+\zeta^2 y)\ldots(x+\zeta^{p-1} y) \qquad (1)$$

Here p is an odd prime number, and ζ is a complex number called a primitive p^{th} root of unity, namely, a number that satisfies the condition: $\zeta^p = 1$ and $\zeta \neq 1$. A domain of complex numbers generated by a p^{th} root of unity, such as appear on the right-hand side of the Eq. 1 is called a "cyclotomic field", $k(\zeta_p)$. The Paris mathematicians suggested to prove FLT by starting from Eq. 1, and by applying the method of "infinite descent" in order to lead to a contradiction, in ways similar to those that had been used for proving the theorem in specific cases with low exponents. An implicit assumption behind this suggestion was that the product in the right-hand side of Eq. 1

[6] A classical exposition of this well-known historical episode is found in [14, pp. 59–75].

is unique. Of course, this assumption would be a natural extension to $k(\zeta_p)$ of the fundamental theorem of arithmetic as traditionally known for the domain of integers (with the additional requirement that the factors in the right-hand side of Eq. 1 can count as "prime" in some well-defined sense). Several years prior to 1847, however, as part of his research on higher reciprocity, Kummer had investigated the behavior of cyclotomic fields and he knew well that this assumption of unique factorization is not generally valid for such domains. On hearing about their intended proof, he wrote to Liouville informing that in 1844 he had already published a counterexample to that assumption. He also wrote that his new theory of "ideal complex numbers" that restored a somewhat different kind of unique prime factorization into these fields. While working on his theory, Kummer came up with the idea of the regular primes.

The basic definition of a regular prime uses the concept of "class number" h_p of a cyclotomic field $k(\zeta_p)$. h_p is a positive integer that characterizes factorization properties of the ring of integers in the field; in particular, $h_p = 1$ *iff* the ring is a Unique Factorization Domain. Now, the prime number p is said to be regular whenever p does not divide h_p. This basic definition, however, does not provide an easily implementable procedure for identifying a given prime as regular or irregular. Kummer very soon found a more operational criterion for allowing this identification, based on the use of so-called "Bernoulli numbers".

The Bernoulli numbers appeared for the first time in 1713 in the pioneering work of Jakob Bernoulli (1654–1705) on probabilities, and thereafter in several other contexts. Euler, for instance, realized that they appear as coefficients B_n of the following Taylor expansion:

$$\frac{x}{e^x - 1} = \sum_{n=0}^{\infty} \frac{B_n x^n}{n!}.$$

Euler was also the first to calculate actual values of the coefficients. With time, also several, well-know recursion formulas to calculate these numbers were developed. Given that for all odd indexes n greater than 1, $B_n = 0$, some mathematicians followed the simplifying convention of considering only even indexes. In these terms, the first few values of B_n are:

$$\begin{aligned} B_1 &= 1/6 \\ B_2 &= -1/30 \\ B_3 &= 1/42 \\ B_4 &= -1/30 \\ B_5 &= 5/66 \\ B_6 &= -691/273\,. \end{aligned}$$

Kummer showed that a prime p is regular *iff* it does not divide the numerators of any of the Bernoulli numbers B_1, ..., $B_{(p-3)/2}$. Already in the lower cases one sees that $B_6 = -691/2730$, which shows directly that 691 is an irregular prime.

It is worth noticing that the lowest case for which unique factorization fails in the cyclotomic fields $k(\zeta_p)$, is $p = 23$. Kummer obviously had made extensive and difficult calculations with numbers of all kinds before coming to realize that a case like this one may arise at all. Also in the case of the irregular primes, Kummer made extensive calculations, and the importance of some of these became clear only much later on. He showed that the only non-regular primes under 164 are 37, 59, 67, 101, 103, 131, 149, and 157. After 164, the computations became prohibitively complex. Kummer's computations also showed that for $p = 157$, the class number h_p is divisible by 157^2 but not by 157^3. For all the other irregular primes under 157, the class number was divisible by p only. Also $p = 157$ had the special property that it divides both the numerators of B_{31} and of B_{55}.

Kummer made clear his opinion that FLT was "rather more of a curiosity, than a high point in the discipline." Still, he announced in 1847 a proof that FLT is valid for all regular primes, since he considered that the methods he had elaborated and the partial results he had obtained were of greater value than the theorem itself [27, p. 139]. Later on he developed three criteria that provide sufficient conditions for proving the validity of FLT for exponents p which are irregular primes. Kummer's three criteria involve several divisibility relations connecting the class number h_p with certain Bernoulli numbers. They are not easy to apply but they give definite answers. With the help of these criteria he examined separately the three cases below 100 (37, 59, and 67), and he was thus able to publish in 1857 a remarkable proof that FLT is true for all prime exponents up to one hundred.

Neither Kummer nor (with isolated exceptions) anyone before Vandiver really undertook to expand the 1857 result beyond the limit of 100, as a possible path to a general proof, or at least to a better understanding of the behavior of the conjecture with higher exponents. Vandiver's initial efforts consisted in applying Kummer's criteria to higher instances of irregular prime exponents and at the same time refining the criteria and adding new ones, so that further cases could be elucidated. One of his earlier breakthroughs came in 1929, when he developed a criterion that allowed him to prove the validity of FLT for the elusive case $p = 159$, and, very soon thereafter, for all values of p, $p < 269$. Further calculations with increasingly higher values were soon to follow.

Another important insight of Vandiver refers to general properties of the class number. Kummer had initially introduced h_p as a product of two integers h_1, h_2 (commonly known nowadays as the first and second factors of the class number), each of which involved a rather complex expression of its own. Kummer proved that a necessary, but not sufficient condition for p to divide h_2 is that p divides h_1. The great amount of examples he worked out in his research led Vandiver to "the possible conclusion" that if p does not divide h_2 then FLT would be true. The so-called Vandiver conjecture (or sometimes Kummer–Vandiver conjecture, since Kummer had essentially suggested the idea back in 1847) is the general idea related to the statement that p never divides h_2 regardless of whether p is regular or not. Its validity is an open question that continues to attract some research interest up to this day.

Calculating the class numbers of the cyclotomic fields of the relevant indexes, and checking the divisibility properties embodied in the criteria, became an increasingly intricate task. Vandiver, working at Austin, Texas, recruited in the 1930s the assistance of several local graduate students who were assigned well-defined calculation tasks with individual cases. The larger the particular numbers investigated grew, the more intensive the calculations became and the more sophisticate the tools needed to support them. Mechanical and electro-mechanical computing machines of various kinds were harnessed to the task. But very soon the technical limitations of these tools were reached, and it took more than a decade before the real leap forward could be taken with the adoption of digital electronic computers, an adoption that proved to be far from smooth and self-evident. A high point in the history of calculations related to individual cases of FLT was embodied in the fruitful collaboration between Vandiver and the young couple Emma (1906–2007) and Derrick Henry (Dick) Lehmer (1905–1991).

The story of this unique collaboration, which started in the more pedestrian stages of the calculations in the early 1930s and culminated in the electronic era after 1945, is a most fascinating, and rather forgotten, episode in the history of FLT. A fair historical assessment requires that it be discussed within a broader historical context that considers, among other things, the slow and hesitant incursion of computer-assisted methods into the mainstream of research in pure mathematics in general (and particularly in number theory), and the enormous changes undergone by the USA community of number theorist from the beginning of the twentieth century and up to the years following World War II [7, 8, pp. 428–446, 4]. The intriguing personalities of the Emma and Dick Lehmer were fundamental to both processes, and they do deserve separate attention. Their story in this context is all the more interesting because their involvement with electronic digital computers, and the ability to devote expensive computational resources to esoteric questions in number-theory at a very early historical stage, was a result of a series of unique personal, historical, professional, institutional, and political circumstances. Were it not for these unique circumstances, the use of digital computers for number theoretical computations, especially those related with FLT and with Mersenne Primes, would surely have come much later than they actually did. Another interesting issue that arose in this context concerns the unwillingness of mainstream mathematical journals to publish the results achieved by Vandiver and the Lehmers in their work. The correspondence between the Lehmers and Vandiver, and between them and the editors of various leading journals is very revealing as to existing attitudes towards the relative importance conceded to research programs of various kinds in number theory and, particularly, to partial progress related with FLT. For lack of space I cannot go into this in any detail here [7].

The kind of mathematical project in which Vandiver and the Lehmers were involved also leads to a broader historical discussion, namely that of the dynamic interaction within number theory between the development of broad, abstract theories, on the one hand, and computations with individual cases, on the other hand. The ways in which this interaction, in its different manifestations, impinged upon the efforts (or lack thereof) to address FLT is a subject of considerable historical interest. Thus, for instance, the very idea of which prime numbers are within computational range, and for which FLT can or cannot be separately verified with the existing mathematical tools at a given point in time presents us with a mathematical concept that is, inherently, historically conditioned. This being the case, it is relevant to discuss the kind of computational tools (both conceptual and material) available at any given point in time and the changing historical contexts in which these tools developed, were used, and affected work on FLT. Likewise, the changing conceptions about the role of computations within the overall disciplinary picture of number theory influenced the ways in which specific cases of the theorem were pursued, and the kinds of general insights thus achieved. Thus, a fair historical account of FLT must also engage a systematic overview of the role of mathematical table-making since the time of Jacobi and Kummer and up until the adoption of electronic computers, and their influence on number theory.[7] This part of the story leads to interesting territories that are apparently distant from number theory and its history, but from the perspective of my account they become actually very close and highly relevant. Of special interest in this context are a series of government initiatives, first in the British Isles in the last third of the nineteenth century, and then in the USA in the first third of the twentieth, devoted to create mathematical tables for very practical purposes such as navigation and astronomy. These initiatives involved the coordinated work of large numbers of human computers and they also led to the introduction of mechanization processes at various levels (not only computation, but also reproduction, distribution, etc.). Because of the personal interests of some of the leading persons involved in these projects, also computations related with number theory were produced here, and this provided additional impulse to the works of a few mathematicians involved in computations with individual cases in relation with a possible proof of FLT.

2 Textbooks, reviews, awards

The cases of Germain, Vandiver and Wiles, and in a different sense, that of Kummer, are the exceptions in the long story of FLT. Much more typical of the overall account are the cases of mathematicians occupied with all kinds of problems close to, or sometimes distant from, FLT, who were involved in furtive attempts to solving the problem, or who, more commonly, expressed intermittent, if vivid, curiosity about the state of the art. Occasional incursions sometimes yield ingenious ideas, a few of which had broader, long-term repercussions on the theory of numbers. It is historically relevant to provide a clear account of the mathematical content of these kinds of contributions to the evolving views on the problem, but no less important and relevant is to sort out the contextual, historical circumstances of these incursions. Besides the more purely internal developments of ideas directly connected with FLT it is also very instructive to look at the status of the problem from more "institutional" perspectives, such as for example the changing attitudes and comments devoted to it in textbooks, review articles, encyclopedias, prize announcements, etc. If we consider mainstream, as well as more esoteric textbooks on number theory, from the early nineteenth century on, we will soon realize that FLT typically occupied a marginal position within the overall picture of the discipline as presented in them (sometimes it was simply absent).

The famous *Zahlbericht* published in 1897 by David Hilbert (1862–1943) affords a good illustration of this point. This book was not intended as a comprehensive inventory of all theorems accumulated thus far in the theory of algebraic number fields, but rather as Hilbert's own thorough, systematic view of the main threads and

[7] [8, pp. 406–435]. This interesting topic has only recently received serious attention by historians. See [5].

main basic results of the discipline, including possible, important directions for future research. It became the main textbook in the discipline for at least three decades to come, and it shaped the main dominant perceptions within the relevant community. Hilbert devoted the last six pages of the book to FLT, mainly to explain how Kummer's ideas (which he discussed in greater detail in previous chapters) had been of help in addressing it. In the preface to the book he had addressed his potential readers as travelers touring an attractive landscape, in which the lemmas discussed represent "wayside halts", whereas the theorems provide "larger stations signaled in advance so that the activity of the mind can rest" in them. Moreover, he specifically indicated a list of some among the theorems which, because of their "fundamental significance", provide the "main destinations", or, alternatively, the "departure points for further advances into as yet undiscovered countries". Kummer's proof for regular primes (or FLT for that matter) was not among these [24, x–xi].

In many other important texts on number theory that I could mention here FLT also appears essentially as a curiosity. But there are other, less obvious, yet no less illuminating sources that can be explored in order to gain additional insights into the perceived status of the problem within the mathematical communities at various times. These include, as already indicated, review journals, prizes and awards, topics of doctoral dissertations, and some others. An examination of the *Jahrbuch über die Fortschritte der Mathematik*, for instance, allows an informative overview of the amount of published work on the problem and the kind of mathematicians involved in such work. A curious, and revealing point that one comes across as part of such an examination concerns the creation of the Wolfskehl prize in 1908. A couple of prizes had been offered in the preceding centuries by the French and the Belgian academies to would-be solvers of the problem, but the Wolfskehl is the one that became, by far, the most closely identified with FLT and its long history. It was also the prize that elicited the greatest amounts of attempted proofs, giving rise to a flurry of works. As a matter of fact, 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 solutions of it that had been published. The announcement of the prize boosted this tendency to new, previously unimaginable dimensions. More than a thousand false proofs were published between 1908 and 1912 alone.[8] 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. These proofs were proposed, as it is well known, mostly by amateurs with very little knowledge of advanced number theory. The considerably high value of the prize was no doubt a main motivation behind this flurry. The truly interesting, and much-less known point, however, is that by this time also some prominent mathematicians, who never before and never thereafter (or almost never) had any connection with FLT, tried their luck with attempted proofs, apparently under the direct motivation provided by the prospects of wining the prize. Thus one can see that mathematicians like Philip

[8] See [28, p. 63]. Ribenboim [35, pp. 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.

Furtwängler (1869–1940), Erich Hecke (1887–1947), Felix Bernstein (1878–1956), and Ferdinand Georg Frobenius (1849–1917), published works on FLT for the first time around these years [2, 3, 15–18, 20]. Furtwängler's work, for instance, had the important virtue of having introduced methods of class field theory into the study of FLT.[9]

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. 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 [21], 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 [17, 18]. 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) was published more than a decade before his first article on FLT. The impression created by all these examples is strengthened by an explicit assertion found in an appendix written by Théophile Got to the 1913 French translation of *Zahlbericht*. 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" [23, p. 325].

At the same time, there is also interesting evidence indicating that the situation brought about by the creation of the Wolfskhel prize produced discomfort among some other 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 [33, p. 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

[9] See [34, pp. 165–178].

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."

Similar was the opinion expressed in 1912 by Walther Lietzmann (1880–1959), a former doctoral student of Hilbert who, as high school teacher and then principal of the *Felix Klein Gymnasium* in Göttingen between 1919 and 1946, published extensively on didactics of mathematics. "Fermat's problem would not be on everyone's mouth", Lietzmann wrote in a booklet devoted to the Pythagorean theorem, were it not for the Wolfkshel prize. In his opinion, the consequences of the creation of the prize were dreadful (*fürchterlich*). He thus wrote, in a spirit similar to Perron [28, p. 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. With the years, as the Wolfskehl 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 [28, p. 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."

3 Fields medals

Additional historical insights arise from examining the ways in which FLT and related work has appeared in the history of other, general-purpose mathematical prizes and awards. One interesting example that I would like to mention here relates to the Fields Medal. As is well-known, a non-trivial error found in Wiles' original proof required about eight months of extra work and the collaboration of Richard Taylor before it could be corrected. Because of this delay Wiles was not awarded the Fields medal at the 1994 International Congress of Mathematicians in Zürich. At the following ICM, held in 1998 in Berlin, he was already above forty thus precluding him from being a candidate. Still, Wiles was duly recognized at that opportunity with a special award by the Fields medal committee, obviously expressing an overall consensus in the international mathematical community about the value of his work. 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. Each case in its turn elicited absolute consensus that the mathematicians involved, both Perelman and Wiles, were clearly in the category of Fields Medal recipients. Significantly, however, *before* these great works, FLT and PC featured very differently within the history of the award. Some details, I think, are revealing.

Two mathematicians were awarded the medal for work directly connected with attempts to prove PC, namely, Stephen Smale in 1966 and Michael Freedman in 1986, for their proofs in the cases of dimension 5 or greater and of dimension 4, respectively [32, 38]. Other topologists who received the Fields Medal did work that attracted attention in a prominent – if not exclusive – manner in connection with PC, such as classifications of manifolds and cobordism. These include René Thom (1958), John Milnor (1962), Serge Novikov (1970), and William Thurston (1982) [1, 25, 40, 41].

The case with FLT was very different. Gerd Faltings is the only recipient (1986) whose work is directly related to the history of problem. The works of other number theorist among the recipients, Atle Selberg (1950), Klaus Roth (1958) Alan Baker (1970), and Enrico Bombieri (1974), went in completely different directions anyway, certainly at the time of their receiving the prize. The research agenda of Pierre Deligne and the works for which he was awarded the medal in 1974 might perhaps be more easily associated with earlier or subsequent attempts to prove FLT, but the fact is that in Nicolas Katz's presentation of Deligne's work for the medal [26] FLT was not mentioned at all. The dedication speech for Faltings was pronounced by Barry Mazur who explained the importance of Faltings' proof of the Mordell conjecture [29]. Indeed, the importance of the unique role that certain "structural conjectures" play in twentieth century mathematics has been a topic that Mazur has stressed at various opportunities [31]. Also on the occasion of Faltings being awarded the medal, Mazur drew attention to the intimate connection of Faltings' work with some of those "outstanding conjectures – fundamental to arithmetic and to arithmetic algebraic geometry." And the outstanding conjectures mentioned by Mazur were the Shafarevich conjecture for curves, the Birch and Swinnerton-Dyer conjecture, and the Grothendieck semisimplicity conjecture. Mazur stressed that based on the ideas developed by Faltings we could "expect similarly wonderful things in the future", and these wonderful things would relate to topics such as moduli spaces of Abelian varieties, Riemann–Roch theorem for arithmetic surfaces, or p-adic Hodge theory.

Mazur did not mention FLT at all in relation with Faltings work. More surprisingly perhaps, the Taniyama–Shimura–Weil conjecture (TSW) was also absent from the general panorama of fundamental conjectures described by Mazur in this 1986 overview. I find this highly revealing in terms of the history of FLT. As most of the readers can surely recall, Wiles proof of FLT involved the proof of a particular case of TSW, the so-called semi-stable case. TSW establishes a surprising link between two apparently distant kinds of mathematical entities: "elliptic curves" and "modular forms". The road that finally led to Wiles' proof via a possible link between TSW and FLT derived from ideas elaborated in the separate works of Yves Hellegrouach and Gerhard Frey starting in the 1970s, in relation with the possible association of elliptic curves to hypothetical integer solutions of $x^n + y^n + z^n = 0$. One such hypothetical solution would imply the existence of a non-modular elliptical curve, and the idea behind the non-modularity of such curves was eventually formulated by Jean Pierre Serre in precise terms with the help of Galois representations. This formulation 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 essentially coincided in considering such a task to be inaccessible, but not so Wiles. Immediately upon hearing about Ribet's proof of the epsilon conjecture he secretly decided that it was time to return to his old mathematical love, FLT, and to prove it by providing a proof of TSW. This part of the story is well-known by now. Just three years before Wiles presented in public for the first time his proof of FLT (via TSW), Mazur 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." This impinged on the status of FLT as an open problem of number theory is a novel way that Mazur formulated as follows [30, p. 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 within 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 now to Mazur as a problem with deep yet *purely tangential* importance, not because of some kind of intrinsic mathematical interest and consequences, but rather because what it added to the deeper significance of this kind of "structural", "unifying conjecture" of mathematics, TSW. And in spite of all of this, only five years earlier, when presenting Faltings' work for the Field Medal, Mazur not only did not mention FLT as such, but even TSW was not in the list of "outstanding" number theoretical conjectures that Faltings' work should be related to (perhaps because of the perceived difficulty in coming to terms with it).

My point in referring here to all this evidence related to the Fields Medal is not to express any mathematical judgment about the intrinsic, relative importance of the two problems, FLT and PC, and certainly not that of the works of Perelman and Wiles, but merely to add yet another perspective from which to consider the ways in which this importance was perceived before their achievements, and in particular to stress the marginal consideration accorded to FLT as part of the great research programs in twentieth-century number theory. 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.

An important virtue of the *history* of FLT, that I hope to have made clear in my account, is that, merely because of the time span covered by the problem since it was first formulated, it affords a useful vantage point of view from which one can follow the deep changes undergone by the entire field of number theory from the time of Fermat to our days. As already said, the interested reader may find detailed elaboration of these, and other related topics, in the articles listed in the bibliography.

References

1. Atiyah, M.: The Work of Serge Novikov. Actes du Congrès International des Mathématiciens, 1970, pp. 11–13. Paris, Dunod (1971)
2. Bernstein, F.: Über den letzten Fermat'schen Satz. Göttingen Nachr. 482–488 (1910)
3. Bernstein, F.: Ueber den zweiten Fall des letzten Fermat'schen Satz. Göttingen Nachr. 507–516 (1910)
4. Bullynck, M.: Reading Gauss in the Computer Age: On the U.S. Reception of Gauss's Number Theoretical Work (1938–1989). Arch. Hist. Exact Sci. **63**(5), 553–580 (2009)
5. Campbell-Kelly, M. et al. (eds): The History of Mathematical Tables. From Sumer to Spreadsheets. Oxford University Press, Oxford (2003)
6. Corry, L.: Fermat Comes to America: Harry Schultz Vandiver and FLT (1914–1963). Math. Int. **9**(3), 30–40 (2007)
7. Corry, L.: FLT Meets SWAC: Vandiver, the Lehmers, Computers and Number Theory. IEEE Ann. Hist. Comput. **30**(1), 38–49 (2008)
8. Corry, L.: Number Crunching vs. Number Theory: Computers and FLT, from Kummer to SWAC (1850–1960), and beyond. Arch. Hist. Exact Sci. **62**(4), 393–455 (2008)
9. Corry, L.: Hunting Prime Numbers from Human to Electronic Computers. Rutherford J., Vol. 3 forthcoming (2010)
10. Corry, L.: On the History of Fermat's Last Theorem: A Down-to-Earth Approach, unpublished manuscript
11. Del Centina, A.: Letters of Sophie Germain preserved in Florence. Hist. Math. **32**, 60–75 (2005)
12. Del Centina, A.: Unpublished Manuscripts of Sophie Germain and a Reevaluation of her Work on Fermat's Last Theorem. Arch. Hist. Exact Sci. **62**(4), 349–392 (2008)
13. Dickson, L. E.: History of the Theory of Numbers, 3 Vols. New York, Chelsea (1919)
14. Edwards, H. M.: Fermat's Last Theorem. A Genetic Introduction to Algebraic Number Theory. Springer, New York (1974)
15. Frobenius, F. G.: Über den Fermat'schen Satz. Berlin Ber. 1222–1224 (1909) (reprint: Jour. R. Ang. Math. **137**, 314–316 (1910))
16. Frobenius, G.: Über den Fermat'schen Satz. II. Berlin Ber. 200–208 (1910)
17. Furtwängler, P.: Untersuchungen über die Kreisteilungskörper und den letzten Fermat'schen Satz. Göttingen Nachr. 554–562 (1910)
18. Furtwängler, P.: Letzter Fermatscher Satz und Eisensteinsches Reziprozitätsprinzip. Wien. Ber. **121**, 589–592 (1912)
19. Gauss, C. F.: *Werke*, 12 Vols. Dieterich, Göttingen (1863–1929)
20. Hecke, E.: Über nicht-reguläre Primzahlen und den Fermat'schen Satz. Göttingen Nachr. 420–424 (1910)
21. Hecke, E.: Vorlesungen über die Theorie der algebraischen Zahlen. Akademie Verlagsgesellschaft, Leipzig (1923)
22. Hellegouarch, Y.: Invitation to the Mathematics of Fermat-Wiles. Academic Press, London (2001)
23. Hilbert, D.: Théorie des corps de nombres algébriques (French translation by A. Lévy), Notes de G. Humbert et Th. Got, préface de G. Humbert. Hermann, Paris (1913)
24. Hilbert, D.: The Theory of Algebraic Number Fields. Springer, Berlin (1998) (English translation by F. Lemmermeyer and N. Schappacher)

25. Hopf, H.: The Work of R. Thom, Proc. ICM Edinburgh, 1958, pp. LX–LXIV. Cambridge University Press, Cambridge (1960)
26. Katz, N. M.: The Work of Pierre Deligne, Proc. ICM Helsinki, 1978. Acad. Sci. Fenica 47–52 (1980)
27. Kummer, E. E.: Beweis des Fermat'schen Satzes ... Monatsberichte Ber. Ak. 132–141, 305–319 (1847)
28. Lietzmann, W.: Der Pythagoraeische Lehrsatz; mit einem Ausblick auf das Fermatsche Problem. Teubner, Leipzig (1912) (7th rev. edn.: 1965)
29. Mazur, B.: On some of the mathematical contributions of Gerd Faltings, Proc. ICM, Berkeley 1986, pp. 7–12. AMS, Providence (1987)
30. Mazur, B.: Number Theory as Gadfly. Amer. Math. Mo. **98**, 593–610 (1991)
31. Mazur, B.: Conjecture. Synthese **111**, 197–210 (1997)
32. Milnor, J.: The work of M. H. Freedman. Proc. ICM, Berkeley 1986, pp. 13–15. AMS, Providence (1987)
33. Perron, O.: Über Wahrheit und Irrtum in der Mathematik. Jahresb. DMV **20**, 196–211 (1911)
34. Ribenboim, P.: 13 Lectures on Fermat's Last Theorem. Springer, New York (1979)
35. Ribenboim, P.: Fermat's Last Theorem for Amateurs. Springer, New York (1999)
36. Singh, S.: Fermat's Enigma. Walker and Company, New York (1997)
37. Stupuy, H.: Sophie Germain, Œuvres philosophiques. Nouvelle ed. Ritti, Paris (1896)
38. Thom, R.: Sur les travaux de Stephen Smale, Proc. ICM Moscow, 1966, pp. 25–28. Mir, Moscow (1968)
39. Van der Poorten, A.: Notes on Fermat's Last Theorem. Wiley, New York (1995)
40. Wall, C. T. C.: On the work of W. Thurston. Proc. ICM Warsaw, 1983, pp. 11–14, Warsaw (1984)
41. Whitney, H.: The Work of John Milnor, Proc. ICM Stockholm, 1962, pp. 1–5. Institute Mittag-Leffler, Stockholm (1963)