Chapter 10. The Weil Conjectures

We shall now interrupt our biographical account in order to insert a section about the Weil conjectures. According to Grothendieck himself (see later on in this chapter), they represented an absolutely essential motivation for his own mathematical work. It therefore seems appropriate to make mention of them in a biography.

Perhaps the most naive idea that a layman might have of mathematics and of mathematicians is that mathematicians formulate equations and then solve them. With regard to algebraic geometry, this naive idea is quite close to the truth, and in fact it would be completely true if instead of saying “solve equations” one would say “to study the set of solutions of equations”.

Some will remember from school days "quadratic equations" such as

$$x^2 - 3x + 2 = 0,$$

and that such equations can have two solutions, but also exactly one solution or no solutions at all. Some will also remember that geometric figures can be described with equations - more exactly with algebraic equations. For example, the following equations

$$xy = 1, \qquad y = x^2, \qquad x^2 + y^2 = 1$$

describe respectively a hyperbola, a parabola and a circle in the x,y plane. (In this context the adjective “algebraic” means that one only makes use of ordinary numbers, unknowns x, y, z,... and the four basic arithmetic operations.)

If one now considers more complex equations; not just one equation but several together, with many unknowns, then determining the solution set of these equations becomes extraordinarily difficult. The solution sets are complex geometric objects in higher dimensional spaces. In almost all cases the solution set is so inaccessible that one has to be satisfied with partial results. Algebraic geometry is precisely the mathematical theory which studies these geometric objects, known as algebraic varieties, using algebraic methods. Speaking metaphorically, one attempts to “measure” or “describe” these algebraic figures. For example, the oft-mentioned Riemann-Roch theorem is a theorem in which algebraic varieties are measured in a certain sense, that is to say, certain characteristic data characterizing them can be established.

Before coming to the Weil conjectures, we must deal with a further essential part of the problem. Mathematicians know many “number systems” in which it is possible to calculate. Even the layman knows of three such number systems, namely the set of “whole numbers” .., -2, -1, 0, 1, 2, 3, ..., the set of “rational numbers” which is just the set of all ordinary fractions, and the set of “real numbers”, which is the set of infinite decimals. The choice of number system is decisive when seeking solutions of an equation. For instance, Fermat's famous equation

$$x^n + y^n = z^n$$

does not have any solution with positive whole numbers x, y, z for any exponent n higher than 2 (as proved by Andrew Wiles), but there are solutions to this equation with real numbers. Thus it is fundamental to decide within which number system one is seeking a solution.

As already said, mathematicians know of many number systems, including some which, contrary to the above-mentioned examples familiar from school, only contain finitely many numbers. Everyone has already heard about the fact that all computer technology is based on a binary number system, using only the two numbers 0 and 1, and in which the equation 1 + 1 = 10 holds. The Weil conjectures deal with algebraic varieties over these number systems with a finite number of “numbers”, the so-called finite fields.[1]

If we have an algebraic equation over such a finite field, one thing is a foregone conclusion: there is only going to be a finite number of possible solutions for the unknowns x, y, z, ..., since there are only a finite number of numbers in the number system we are using anyway. Whatever happens, the equation can thus only have a finite number of solutions, and possibly none at all.

The conjectures formulated by André Weil in 1949 make precise statements about the number of solutions to such equations. One can not write down an explicit formula for the number of solutions, but one can make some descriptive statements about it. For example, the statement that the number of solutions is even or odd could be an important finding. The formulation of these conjectures was a first-rate stroke of genius, the more so as Weil had few numeric examples with which to support and test his conjecture. Rather, he arrived at his conjecture through deep analogies to genuinely geometric theories. It is possible that Grothendieck first heard about the Weil conjectures at a Bourbaki lecture by Chevalley in the year 1949.

On the basis of the above mentioned analogies with geometry, a sketch was made (mostly by Serre) of a possible strategy to prove the Weil conjectures.

[1] The word “field” does not have any geometric meaning, but simply means that in this number system the four basic arithmetic operations can be carried out with no restrictions.

What was necessary was the "right cohomology theory". Mathematicians know many variants of cohomology theories; they were originally an aid to the study of geometric objects such as curved manifolds in higher dimensional planes. During the fifties, in the field of algebraic geometry, such cohomology theories were developed and refined, including by Grothendieck. They were needed, for instance, to prove the Riemann-Roch theorem, and in fact even just to formulate it. However, it was perfectly clear from the outset that with regard to the Weil conjectures, the existing cohomology theories ("sheaf cohomology") were useless. Better theories were needed, with the right formal properties, and from the end of the fifties the search for the "right" cohomology theory was the focus of research in the field of algebraic geometry, including Grothendieck's. In a certain sense this search is still not finished even today.[2]

There can be no doubt that proving the Weil conjectures was the ultimate goal that Grothendieck had in mind with his reconstruction of algebraic geometry. In *Récoltes et Semailles* he wrote that the Weil conjectures were the capital of an undiscovered land. And he had a clear (even dogmatic) idea about how this proof should be accomplished: one should construct the right cohomology theory; it would prove to be the "right" one if it could be shown to have certain formal properties, called the "standard conjectures", and the Weil conjectures would follow directly from the standard conjectures.

Several mathematicians from Grothendieck's circle relate that at the end of the sixties he made a last great effort to prove the Weil conjectures, that is, to construct the "right" cohomology theory. (For the sake of historical exactitude it must be said that a part of the conjectures had already been proven by Grothendieck and Bernard Dwork, indeed everything except for the very last and most difficult part.) This right theory is the theory of "motives"; to this day the standard conjectures have not been proved, and the elaboration of the theory remains incomplete due to hitherto insurmountable technical difficulties.

These mathematicians also relate that Grothendieck appeared to suspect that this last success would be denied him, and that perhaps his most brilliant student, Pierre Deligne, would be necessary to complete the program. Perhaps the exhaustion described in Chapter 2 was beginning to show, or perhaps it was "Nobel Prize Syndrome" after he received the Fields Medal, or perhaps the widespread and ominous notion that after age forty a mathematician's best work is behind him; in any case, Grothendieck did not really make progress.

But in the summer of 1973, the news spread that Deligne had succeeded in finding a proof for the final conjecture. Although not everything had been written up yet (large elaborations of whole sections of Grothendieck's SGA program used in the proof were missing, since at that point the proofs existed only as sketches), among experts there was soon no doubt at all that Deligne had completed the proof - and that one had "always" expected this of him.

[2] The theory of motives is its logical development.

Grothendieck visited the IHES with Justine (probably after his return from Buffalo), where Katz, Messing and Deligne in person gave him the details. He was both fascinated and disappointed. Deligne's proof was very different from what he had imagined. Deligne had succeeded in avoiding the complete construction of the "right" cohomology, and he had not proven the "standard conjectures". The key to Deligne's success was that he knew the methods of classical mathematics much better than the autodidact Grothendieck. According to Allyn Jackson, Deligne commented on Grothendieck's reaction in the following manner:

If I had done it using motives, he would have been very interested, because it would have meant that the theory of motives had been developed. Since the proof used a trick, he did not care.

Deligne is here doubtless overly modest when he speaks of just a "trick". It was far more; in fact he did not have the "blind spot" (as John Tate put it) that Grothendieck had.

Pierre Deligne, 1975

After 1973 Grothendieck did continue to engage in mathematics, but his grand goal no longer beckoned from the horizon, and he had neither the desire nor the strength to apply himself to the technical complications of the theory of motives. And thus from this point onwards a certain lack of purpose and direction is visible in his mathematical "meditations"; they spread out endlessly without really arriving anywhere.

Although this may digress a little from Grothendieck's biography of the years 1970 until 1991, it is perhaps interesting to read his view, at a much later date, of the significance of the Weil conjectures for his own mathematical work, and about his personal relationship with André Weil. In his letters he expressed himself on this subject several times, mostly in connection with the correspondence about *Récoltes et Semailles*. On 12 December 1986 he wrote to Bob Thomason among other things:

The more amazing thing though in your letter, is your referring to Weil's conjectures as being "boring". Without these conjectures, I wouldn't probably ever have dreamt of étale cohomology nor of motives, nor of weights in cohomology groups, and a good deal of what you and a number of other people did wouldn't exist.

At about the same time, on July 9, 1986, he wrote to his Japanese friend Yamashita:[3]

As for the Weil conjectures, I never got to the point where I would really try seriously to actually prove them - I mean the last and crucial point, the absolute values of the proper values of the cohomological Frohenius operations. Too many more basic things had to be elucidated first. If I hadn't quit mathematics by the end of the sixties, but had gone on steadily doing mathematics the way I feel them, I would probably never have hit upon Deligne's proof - but instead, I am rather confident, I would have solved some variant or other of the "standard conjectures", including the basic Hodge-type general "index theorem" (positivity of certain "traces"), which would yield the Weil conjectures as just by-product (of basic significance, sure enough), of a geometrical theory of much wider sweeping. At least, this was on my short-range program, before I quit. The long prevailing stagnation in the cohomological theory of schemes is due to a large extent, in my eyes, to the neglect of such basic questions, for the "benefit" of singling out and solving just the "challenge question", namely the celebrated Weil conjectures (namely what was still left to prove ...) Or more accurately, while the solution should have been taken as an encouragement for establishing the wider view of things they had become part of, this success has become a kind of justification, for sneering on a wider view as well as on the workman who had shaped the basic tools ...

Of course, as witnessed by your citation from the introduction to EGA, the influence of Weil's conjectures, in my work between 1958 and 1969 (when I quit), was quite a deep one. But it never has been the *challenge*: "I will be the one who 'cracks' them" - but just an *inspiration* for some of the main themes in the vision of a new geometry, developing between 1955 (or 1958) and 1969. As a matter of fact, in the late sixties, as I was quite busy with a number of foundational matters, I used to think that probably one of my students, presumably Deligne, would solve the Weil conjectures (and the standard conjectures too) within the very next years.

We end this chapter with another letter to Yamashita, dated September 16, 1986, in which Grothendieck expressed himself in a very balanced manner

[3] This passage and the others quoted from Grothendieck's letters are in his original English, left untouched.

about his relationship to Weil:

I was very surprised to hear you say that “it is very famous that Weil is not friendly with me”. I didn't get aware of a possible “unfriendliness” of Weil towards me, until writing “Promenade” - and more specifically, I believe, till the the moment I came to writing that footnote you are referring to. I met Weil for the first time in Cartan's seminar, in 1948 I think - and the general impressions about the Bourbaki group, which I describe in ReS I, apply to him equally well. I came to know him a little more, and his particular authoritative role in Bourbaki, while I was a member of the Bourbaki group, after 1955. I got angry at him once or twice (once I quit a Bourbaki congress because of this, and even thought of quitting Bourbaki altogether), but it didn't seem to me this left any unfriendly feelings between us. Of course I admired him a great deal as a mathematician, and his opinion had a lot of weight for me - but not on those matters I was most closely involved with, which he generally frankly disliked (like topological vector spaces and functional analysis I was involved in till 1955, or cohomological machinery after 1955), or appeared not to be really interested in (as my foundational work on algebraic geometry after 1958). However, it was quite clear to me that Weil knew well that I did math without being just kidding and pretending to, and that some of it was relevant for his own work (although I don't remember Weil ever saying explicitly anything of this kind). I never had the feeling of anything like “unfriendliness” of Weil towards me, nor did anybody ever report to me anything, which I could interpret as unfriendliness,

[...] When Weil told me he was working on reediting his Foundations, I had been pondering and working already for quite a while (two or three years) on the new approach through schemes, and I knew perfectly well (and I was convinced he himself knew so also) that his Foundations were wholly out of date. This was for me a matter of fact, I don't believe it had anything to do with my opinion on Weil himself or on me. After all, a science progresses through old approaches bring superseded by new ones, which doesn't mean in the least that those people who developed the old ones, were any less smart than the newcomers, whose work would equally be superseded by later work. This was very clear in my mind, by a kind of elementary instinct, even though I had only the faintest notion of (say) the history of mathematics. Thus I felt perfectly free and at ease, when jokingly replying to Weil that when working at reediting his Foundations, he must have felt like the chap who is putting fresh paint upon pretty old walls (this image had flashed quite spontaneously to my mind). As a matter of fact, this remark was probably a way of expressing my surprise, that Weil should lose his precious time for a kind of work which, to me, looked quite evidently unproductive. I don't remember Weil's reply - at any rate, if he had been hurt by my remark |(and I certainly didn't dream someone like Weil could possibly be hurt by it), he didn't show it. (And sure enough, as I know him, he wouldn't ever admit being hurt by anyone - because admitting being hurt means also, renouncing an attitude of superiority over the ordinary human beings.)…