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ANDRÉ WEIL

6 May 1906 — 6 August 1998

Elected For.Mem.R.S. 1966

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Introduction

André Weil died in Princeton in August 1998; he was ninety-two years old. His last years were saddened by the loss of his wife Eveline, and by the infirmities of old age; death was perhaps a relief for him.

The man

He was born in Paris in 1906, into a Jewish family. His father, a doctor, came from Alsace, his mother was of Austrian origin but was born in Russia. He had a sister, Simone, three years younger; the two children were very close, and remained so until Simone's death in 1943; thereafter, André Weil was much involved in publishing the many manuscripts that she had left.

His book *Souvenirs d'apprentissage* (47)* contains a charming account of the unorthodox but scholarly education that he received; the outcome was a lively taste for ancient languages (Latin, Greek, Sanskrit) and a firm vocation to be a mathematician. This led him to enter the École Normale Supérieure in 1922, when he was only sixteen (he was said to walk there in short trousers). He left in 1925, first in the class despite a blank paper on rational mechanics,

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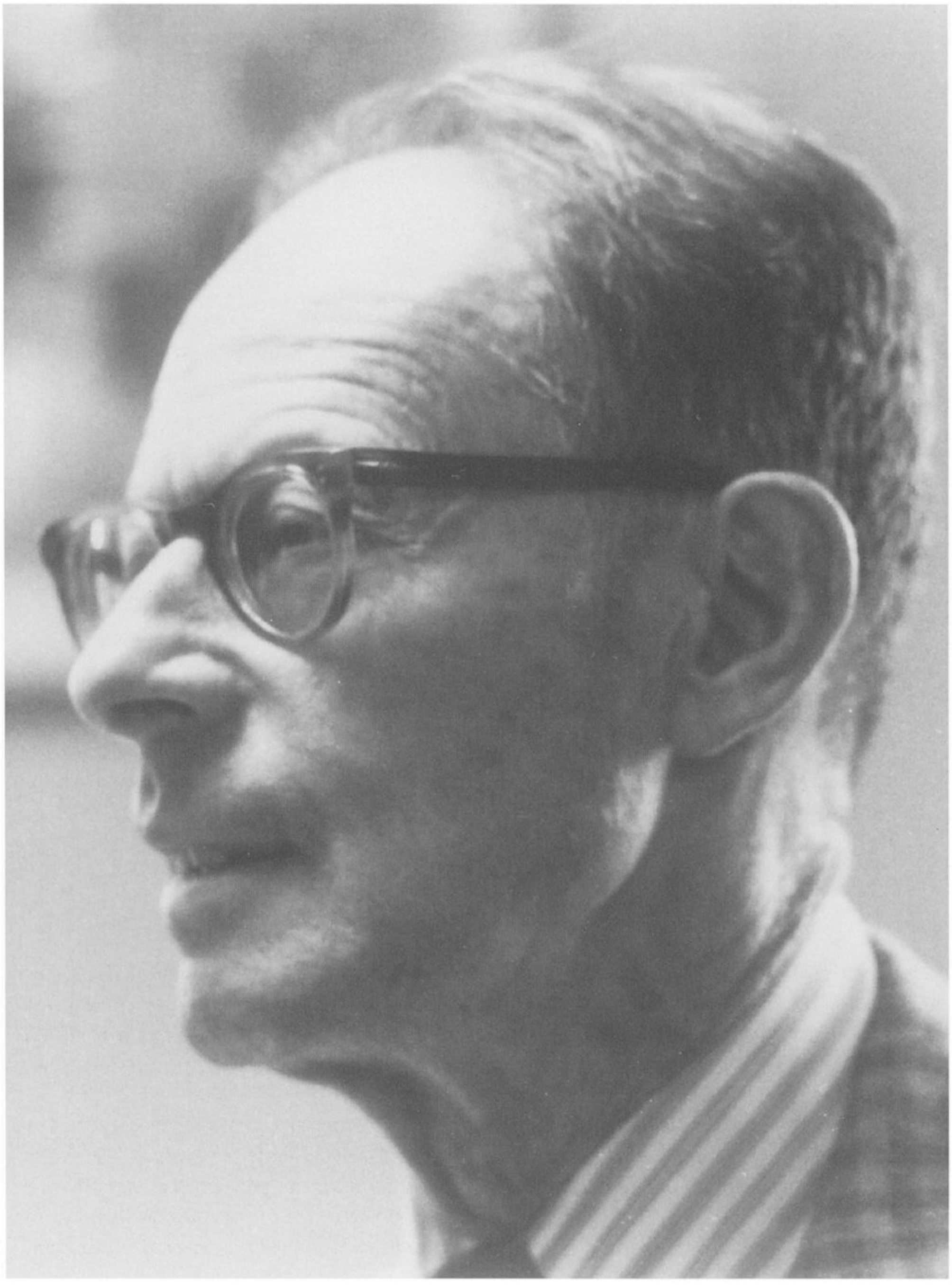
* Numbers in this form refer to the bibliography at the end of the text.



a subject that did not seem to him to be part of mathematics. He went to Italy and then Germany, which was then home to some of the finest mathematicians of the time, including Hilbert, Artin, von Neumann and Siegel. After submitting his thesis in 1928, when he was 22, he spent two years on the staff of Aligarh University in India, a post found for him by the Sanskrit scholar Sylvain Levi, whose course he had attended at the Collège de France. Thereafter, it was Marseille, then Strasbourg from 1933 to 1939. It was during his stay in Strasbourg that he and some friends from the École Normale (Henri Cartan, Jean Dieudonné, Jean Delsarte…) joined together to found the Bourbaki group. In 1939, he was in Finland when war was declared; after almost being shot as a Soviet spy, he returned to France and was imprisoned at Rouen, charged with 'insoumission'. He was soon freed, and after various adventures described in his *Souvenirs* he managed to leave for the USA in 1940. He stayed there for some years before spending two years in Brazil, but it was only in 1947 that he was given a post commensurate with his talents: he became professor at the University of Chicago and then in 1958 he moved to the Institute for Advanced Study at Princeton, where he remained for the last 40 years of his life. The Institute suited him very well, both in the freedom that he had to teach (or not to teach) as he pleased and in the high level of its professors and visitors. (His proper place, in his homeland, should have been the Collège de France; I often dreamed of a chair of mathematics that he would occupy, but alas it was not to be.)

To finish this account of Weil's *curriculum vitae*, I list some honours that he received (or rather, that he agreed to receive). He was a member of the US National Academy of Sciences, a Foreign Member of the Royal Society and of course a member of the Paris Academy of Sciences; he received the Wolf Prize in 1979 (the same year as Jean Leray, and a year before Henri Cartan) and the Kyoto Prize in 1994; this last prize gave him particular pleasure because of the excellent relations he had always had with Japanese mathematicians.

The work

Now I come to the essential part, that is, to his work. His first publication was a *Comptes Rendus* note in 1926 (1). In the 50 years that followed, he published a dozen books and more than a hundred papers, written in French, in English, and sometimes in German. His papers have been collected in the three volumes of his *Oeuvres mathématiques* published by Springer in 1979 (45). They include Weil's valuable Commentaires, which explain their motiviation.

It is not possible to classify these works by subject, because so many themes are combined. It is true that one might play the American game of listing keywords: zeta, Siegel, rational points, Abelian varieties, and so on; but that hardly makes sense. It seems to me that the only possibility is to follow the chronological order, as is done in his *Oeuvres*.

1. Let us begin with his thesis (2). It is concerned with Number Theory and more particularly with Diophantine equations, that is to say, with rational points on algebraic varieties. At that time, the only known method was Fermat's descent; very often, the application of this method depended on explicit calculations, so that a different little miracle seemed to happen in each particular case. Weil was the first to see that behind these computations there was a general principle, which he called the *théorème de décomposition*; this theorem allowed a sort of transfer between algebra (in principle easy) and arithmetic (harder). He deduced what we now call the Mordell–Weil theorem: given an Abelian variety A

and a number field K, the group $A(K)$ of points defined over K is *finitely generated.* The proof was far from easy: the algebraic geometry of the time had not yet developed the tools that were needed. Fortunately, Weil had read the works of Riemann at the École Normale, and he was able to replace the missing algebra by analysis: theta functions. So he was able to reach his goal.

However, this goal was by no means an end. As in almost all of Weil's works, it was much more a point of departure, from which one could attack other problems. In this instance, the problems were as follows.

- Prove the finiteness of the set of integer points of an affine curve of genus $g > 0$. This was done a year later by Siegel, who combined Weil's ideas with those from the theory of transcendental numbers.
- Prove the finiteness of the set of rational points of a curve of genus $g > 1$ (the Mordell conjecture). This was done 55 years later by Faltings.
- Make *effective* (that is to say, explicitly computable) the qualitative results of Mordell–Weil, of Siegel and of Faltings. This question is still open, and of great interest to arithmeticians.

2. In the years after his thesis, Weil tried various paths that might lead to the Mordell conjecture. One of them led to his monograph *Généralisation des fonctions abéliennes* (8), a text presented as analysis, whose significance is essentially algebraic, but whose motivation is arithmetic! (And, one asks oneself, who in the world beside Weil and Siegel could have understood this text in 1938?) The success of his thesis arose from the use of Abelian varieties, and in particular of Jacobians; for the Mordell conjecture, Weil felt that it would be necessary to go beyond the Abelian setting. The Jacobian parametrizes line bundles of rank 1 (and degree 0); one has to parametrize fibre bundles of any rank (that is to say, to pass from GL_1 to GL_n—this was to be one of his favourite themes). However, in 1938 no one, not even he, knew what an analytic vector bundle was—still less an algebraic vector bundle: it was not till a decade later that such a notion would be introduced (by Weil himself). This little detail did not stop him. He introduced an equivalent notion, that of 'classe de diviseurs matriciels', and by analytic methods (following Riemann and Poincaré) he proved the Riemann–Roch formula and what we nowadays call the duality theorem (which he called the 'inhomogeneous Riemann–Roch theorem'). It was a real tour-de-force! Unhappily, to define vector bundles was not enough: one needed also their 'moduli varieties', to replace Jacobians. From the point of view of algebraic geometry, this is a very tough passage-to-the-quotient problem, which was solved only some 20 years later, by Grothendieck and Mumford. Weil had to be content with partial results, partly unproved but which would turn out to be essentially correct; *a fortiori*, he could make no arithmetic application. A set-back, perhaps? No, for his work on Riemann–Roch served as a model to others fifteen years later, and the moduli varieties that he tried to construct turned up again at the roots of other questions: in differential geometry, with Donaldson, and in characteristic $p > 0$, with Drinfeld.

3. During the period 1925–40, Weil was far from confining himself to Number Theory. Here are some of his achievements.

- In the analysis of several complex variables, he introduced a generalization of the Cauchy integral, which is nowadays known as the *Weil integral* (3, 4). He deduced a generalization of Runge's theorem: if D is a bounded domain defined by polynomial inequalities, then every holomorphic function on D is a limit of polynomials for the compact convergence topology.

- In the theory of compact Lie groups, he used topological methods (Lefschetz's formula) to prove the conjugacy of maximal tori (5).
- In non-Archimedean analysis, a subject then in its infancy, he defined p-adic elliptic functions (6).
- In topology, he gave the definition of *uniform* spaces (7).
- He published a book *L'intégration dans les groupes topologiques et ses applications* (10) in which he expounded, in a style at once elegant, concise and Bourbaki-like, the two aspects of the theory that were accessible at that time: the case of compact groups (orthogonality relations of characters) and that of commutative groups (Pontryagin duality and Fourier transform).

4. We return to number theory and algebraic geometry, with the celebrated note of 1940 (9). Between 1925 and 1940, the German school, led by Artin and Hasse, had found remarkable analogies between algebraic number fields and fields of functions of one variable over finite fields (in geometric language: curves over finite fields). Each of them has zeta functions, for which a Riemann hypothesis can be stated. In the function field case, Hasse was able to prove this hypothesis for genus 1. What about genus ≥ 2? During his stay in Rouen, Weil saw the answer: instead of working with curves, that is to say varieties of dimension 1, one should use varieties of higher dimension (surfaces, Abelian varieties) and adapt to them results proved (over the complex field) by topological or analytic methods. He sent to the *Comptes Rendus* a note (9) that begins as follows: 'Je vais résumer dans cette Note la solution des principaux problèmes de la théorie des fonctions algébriques à corps de constantes fini …'.

This note contains a sketch of proof, no more; everything depends on a 'crucial lemma' culled from Italian geometry. How should one prove this lemma? Weil soon realized that it is possible only if one completely reworks the definitions and results that form the foundations of algebraic geometry, in particular those involving intersection theory (so that one has a calculus of cycles that replaces the missing homology). He was thus led to write his *Foundations of algebraic geometry* (12), a massive and rather dry book of 300 pages, which was only replaced twenty years later by the no less massive and dry *Eléments de géometrie algébrique* of Grothendieck. Once the *Foundations* were in place, Weil could return to curves and their Riemann hypothesis. He published in quick succession two works: *Sur les courbes algébriques et les variétés qui s'en déduisent* (15) and *Variétés abéliennes et courbes algébriques* (16). After eight years and more than 500 pages, his Note of 1940 was at last justified!

What are the rewards? First, the Riemann hypothesis has down-to-earth applications. It gives upper bounds for trigonometric sums of one variable (17), for example the following (useful in the theory of modular forms):

$$\left|\sum \cos\left(\frac{2\pi\,(x + x')}{p}\right)\right| \leq 2\sqrt{p} \quad \text{for } p \text{ prime},$$

where the sum is over integers x with $0 < x < p$, and x' denotes the inverse of x modulo p.

What is more, Weil was led not only to provide a solid foundation for algebraic geometry, but also to develop an algebro-geometric theory of Abelian varieties, parallel to the analytic theory based on theta functions. Abelian varieties long remained one of his favourite themes

(23, 24, 41, 42), including notably the theory of complex multiplication (25, 26) that was done simultaneously and independently by Taniyama and Shimura.

5. Guided by the case of curves, and also by explicit computations for hypersurfaces defined by diagonal equations, Weil (18) formulated what were immediately known as the *Weil conjectures*. These conjectures were about (projective non-singular) varieties over a finite field. They amount to supposing that the topological methods of Riemann, Lefschetz, Hodge, and others, can be adapted to work in characteristic $p > 0$; from this point of view, the number of solutions of an equation (mod p) appears as a number of fixed points, and one can compute it by the Lefschetz trace formula. This truly revolutionary idea thrilled the mathematicians of the time, as I can testify at first hand; it has been the origin of a major part of the progress in algebraic geometry since that date. The objective was reached only after about twenty-five years, and then not by Weil himself but (principally) by Grothendieck and Deligne. The methods that they were led to develop remain among the most powerful of present-day algebraic geometry; they have had applications in varied fields including the theory of modular forms (as Weil had foreseen) and the determination of the characters of the 'algebraic' finite groups (Deligne–Lusztig).

6. Weil returned to arithmetic in 1951 with his work on class field theory (20). This theory had attained an apparently definitive form in 1927, when Artin had proved the general reciprocity law. In the language introduced by Chevalley, the main result states that the Galois group of the maximal Abelian extension of a number field K is isomorphic to the quotient C_K/D_K, where C_K is the idèle class group of K and D_K is the connected component of C_K. (Accordingly, one describes what happens above K in terms of objects taken from K itself, just as a topologist describes the coverings of a space in terms of its classes of loops.) However, a disagreeable feature of this statement is that it is not C_K itself that is a Galois group, but only its quotient C_K/D_K. Weil started with the idea that C_K itself should be a Galois group in a suitable sense (in what sense we still cannot say). It this were true, it would imply remarkable functorial properties of the groups C_K (for instance, if L/K is a finite Galois extension there should be a canonical extension of $\mathrm{Gal}(L/K)$ by C_L). One could try to prove these properties directly, and that is what Weil did. Here again, there were important consequences, as follows.

- One was led to the study of the cohomology groups of the groups C_L; it is the origin of the cohomological methods in class field theory as developed by Nakayama, Hochschild, Artin, Tate, and others.
- The new 'Weil groups' so defined allow one to define new types of L-functions, containing as particular cases both Artin's non-Abelian L-functions and Hecke's L-functions with 'Grössencharacter'. As Weil said, one has thus wedded Artin to Hecke!

7. A little later, Weil published a study (22) (completed later (37)) on *explicit formulae* in the theory of numbers; these formulae (essentially known to specialists, it seems) related sums taken over prime numbers to sums taken over the zeros of zeta functions. Weil rewrote them in a very suggestive manner (for example, by highlighting the analogy between finite and infinite places—another of his favourite themes). The most interesting result is a translation of the Riemann hypothesis in terms of the positivity of a certain distribution. Might this translation help to prove the Riemann hypothesis? It is too soon to say.

8. Various works of Weil between 1940 and 1965 were concerned with differential geometry. They include the following.

- With Allendörfer, a Gauss–Bonnet formula for Riemannian polyhedra (11).
- Proof of the theorems of de Rham in a letter to Cartan in 1947 (see (21)). This letter greatly influenced Cartan in his formulation of the theory of sheaves (due initially to Leray).
- Harmonic forms and Kähler theory (14, 27); these were the basic tools for the application of analytic methods to algebraic geometry.
- Theory of connections and introduction of the Weil algebra (19).
- Deformations of locally homogeneous spaces and discrete groups (28, 31, 32); rigidity theorems are proved for co-compact discrete subgroups of simple Lie groups of rank > 1.

9. During the 1950s and 1960s, Weil devoted a series of articles to themes inspired by Siegel. He writes in his *Oeuvres* (vol 2, p. 544): 'To comment on the works of Siegel has always appeared to me to be one of the tasks that a present-day mathematician may most usefully undertake.'

Note the lovely understatement of the word 'comment'! Weil did much more:

- In (29) and (30) he developed systematically the adélic methods introduced by Kuga and Tamagawa. These not only give us again Siegel's theorems on quadratic forms, but suggest new problems, for instance to show that the Tamagawa number of any simply connected group is 1 (we now know that this is so, thanks to the work of Langlands, Lai and Kottwitz).
- In two *Acta Mathematica* memoirs (33, 34) he returned to quadratic forms and Siegel's formula from an entirely different point of view. He introduced and studied a new group, the *metaplectic* group, as well as a representation of this group (nowadays called the *Weil representation*). The Siegel formula appeared as the equality of two distributions, one of them a sort of Eisenstein series, the other an average of theta functions. This result is not limited to quadratic forms: Weil showed that it applies to all classical groups, and it implies local-to-global theorems (Hasse principle) as well as the determination of Tamagawa numbers.

10. Hecke's work too was an inspiration for Weil. In 'L'avenir des mathématiques' (13) he was already writing about Euler products 'whose extreme importance for the theory of numbers and the theory of functions have only just been revealed to us by Hecke's work'. Twenty years later (35), he made a decisive contribution to Hecke's theory, by showing that the validity of certain functional equations for a Dirichlet series and its twists by characters is equivalent to the fact that the series comes from a modular form. One thus obtains something very precious in mathematics, namely a *dictionary*

$$\text{modular forms} \iff \text{Dirichlet series.}$$

The implication $\Rightarrow$ was due to Hecke, who had also proved the reverse implication in the particular case of level 1; Weil's new idea was to use *twisting*. One of the more interesting aspects of his theory is the way in which the constants in the functional equations vary by twisting (i.e. by tensor product).

This work led to several developments, including some by Weil himself (36). It fits now into the so-called 'Langlands philosophy'. One of its consequences was a precise formulation of a slightly vague conjecture made by Taniyama in 1955, according to which every elliptic curve over $\boldsymbol{Q}$ is 'modular'. The work of Weil suggested that the appropriate modular 'level' should be the same as the 'conductor' of the elliptic curve, that is to say, it is determined by the primes and types of bad reduction of the curve. This allowed a great deal of numerical verification, before the result was finally proved (subject to some technical restrictions) by Wiles in 1995.

11. The last publications of Weil were concerned with the history of mathematics. It is a subject that had interested him for a long time, as certain of the *Notes historiques* in Bourbaki show (in particular, those on the differential and integral calculus in *Fonctions d'une variable réelle*, chapters I–III). He began with a short booklet, at once mathematical and historical: *Elliptic functions according to Eisenstein and Kronecker* (40); he says he enjoyed writing it, and his enjoyment communicates itself to the reader. The following works are more clearly historical. Above all, one must cite his *Number Theory—an approach through history from Hammurabi to Legendre* (46) in which he describes the history of the theory of numbers up to 1800, stopping just before the *Disquisitiones arithmeticae* (his readers would have liked him to go further and to write on Gauss, Jacobi, Eisenstein, Riemann, and others—but he did not.) As expected from him, it is mathematics that is the subject of these books, and not the private lives or the social relationships of the mathematicians. Only the history of ideas matters; what a refreshing point of view! It is not easy to write such books. One needs linguistic and literary gifts, and Weil did not lack them. One must also, above all, be able to judge when an idea is truly new and when it is standard technique (he discussed this in 'History of mathematics: why and how' (44)); that is certainly the most difficult part for a historian of mathematics who is not a mathematician (see (38, 39, 43)).

I end this description, too superficial I fear, of what André Weil did. What makes his work unique in the mathematics of the twentieth century is its prophetic aspect (Weil 'sees the future') combined with utmost classical precision. To read and study this work, and to discuss it with him, has been among the greatest joys of my mathematical life.

Acknowledgement

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