

On the AMS *Notices* Publication of Krieger's Translation of Weil's 1940 Letter

Serge Lang

Generalities

The March 2005, *Notices* published Martin Krieger's translation of André Weil's 1940 letter to Simone Weil. Weil himself included this letter in his collected mathematical papers, thereby taking professional responsibility toward the mathematical community and giving the letter the professional standing which it would not have as a "private" letter to his sister. A translation in the AMS *Notices* going to 30,000 AMS members reinforces this professional responsibility. As Weil himself states in line five, his letter consists of two parts, the first one concerning the history of number theory. He writes explicitly to his sister (my translation): "Maybe you will believe you understand the beginning; you will understand nothing after that." Krieger's translation is not accurate in that he renders the first part of the sentence "you may be able to understand the beginning."

Weil, in the second paragraph of this letter, makes a disclaimer (my translation): "I warn you that everything concerning the history of mathematics, in what follows, relies on a greatly insufficient erudition, that it is in large part an a priori reconstitution, and that, even if that's the way things did happen (which is not proved), I couldn't certify that they actually happened that way." However, my comments below are concerned with items Weil knew quite well, so this general disclaimer does not apply to the specific cases I discuss.

Krieger gives Weil's letter his unqualified endorsement: "The Weil letter is a gem, of wider interest to the mathematical and philosophical

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community, concerned both with the actual mathematics and with how mathematicians describe their work." The *Notices* gave its imprimatur to Krieger's endorsement and to Weil's letter without warning to the reader. I don't find Weil's letter to be a gem in its usual praiseworthy sense. On the contrary, readers of the letter deserve being warned about the tendentious ways Weil gives some historical accounts. Furthermore, mathematicians do not usually publish (let alone as part of collected mathematical papers) purported historical accounts based on "insufficient erudition" and lack of scholarship. They don't call such publications "gems" either.

I thank the editors for publishing the present comments and my 2002 *Mitteilungen der Deutschen Mathematiker-Vereinigung* article in the current issue of the *Notices*. Among other things, that article contains a substantial analysis of some items in Weil's letter having to do with "nonreferences in Weil's works." See pp. 616-617.

Artin's Reciprocity Law

Here I wish to deal with one other specific item in Weil's letter, different from non-references. The item concerns Artin's reciprocity law. First, on page 244 of his *Collected Papers*, he makes a general statement concerning the history of number theory: "It is entirely dominated by the reciprocity law. It is the *theorema aureum* of Gauss..." Four pages later, he states, concerning abelian extensions of number fields and certain areas he mentions (my translation):

But these questions are well sorted out [débrouillées] and one can say that everything that has been done in arithmetic since Gauss up to these last few

years consists in variations on the reciprocity law: starting with the one of Gauss; ending up with that of Artin, and *it's the same one*. This is beautiful, but a bit “vexant”. We know a little more than Gauss, no doubt; but what we know in addition, is precisely (with a little leeway) [“ou peu s'en faut”] that we don't know more.

Some people may call this kind of general rhetoric “history” or a “gem, of wider interest in the mathematical and philosophical community, concerned both with the actual mathematics and with how mathematicians describe their work.” I don't, and I urge people to get a more accurate view of number theory up to 1940 elsewhere.

Going further into Artin's reciprocity law, Weil's letter reads:

...Artin arriva d'abord à formuler cette loi à titre de conjecture hardie (il paraît que Landau se moqua de lui), quelque temps avant de pouvoir la démontrer (chose curieuse, sa démonstration est une simple transposition de la démonstration d'un autre résultat, paru entre temps, par Tchebotareff, qu'il ne manque pas d'ailleurs de citer; et cependant c'est Artin, et à juste titre, qui a la gloire de la découverte).

The characterization “conjecture hardie” already contradicts the absurd rhetorical assertion that we don't know more than Gauss (ou peu s'en faut). Krieger's translation reads:

...and this is the way Artin first arrived at this law as a bold conjecture (it seems that Landau made fun of him), some time before being able to prove it (a curious fact, his proof is a simple translation of another result by Tchebotareff that had just been published, which he cited; however it is Artin, justly having it bear his name, who had the glory of discovering it).

First, both expressions “simple transposition” and “simple translation” are inappropriate, starting with the ambiguous use of the word “simple”. “Simple” to whom? Relative to what? Mathematicians are accustomed to making the distinction in situations when discovering some result, and making it simple was not a simple thing. It may become simple afterwards for some people to read the proof.

Second, the translation is defective, for instance, because Weil uses the word “transposition”, not “translation”. Artin's proof is not “a simple translation of another result by Tchebotarev.” In 1926,

Tchebotarev published a proof of a conjecture of Frobenius, giving the density of primes having a given associated Frobenius conjugacy class in the (nonabelian) Galois group of a normal extension of a number field. He used a new method, crossing the extension with a cyclotomic extension. Artin recognized the possibility of applying this method to prove his own reciprocity law conjectured in 1923 and credits Tchebotarev by stating: “Einen der Grundgedanken des Beweises, die Verwendung von Kreiskörpererweiterungen, verdanke ich der wichtigen Arbeit von Herrn Tschebotareff”. My translation: “I am indebted to the important work of Mr. Tchebotareff for one of the fundamental ideas [Grundgedanken] of the proof, the use of cyclotomic extensions.”

Third, the use of the expression “à juste titre” (“justly”) is tendentious in conjunction with the expression “simple translation” or “simple transposition”, suggesting that there could be or that there is or that there ever was a question about attributing the reciprocity law to Artin in light of the proof being a “simple transposition” (“simple translation”) of a proof by Tchebotarev. I have never seen any such suggestion from anyone else, and Hasse's account, which I reproduce below, is typical of the evaluations which I have heard throughout my life from other mathematicians.

Here is how Hasse described Artin's work in his “Bericht über neuere Untersuchungen und Probleme aus der Theorie der algebraischen Zahlkörper” (*Jahresbericht der Deutschen Mathematiker-Vereinigung*, 1930), page 1, (my translation):

Since the appearance of the first part of this Bericht, the theory of abelian extensions of number fields has made an advance of the very greatest significance, which concerns precisely the main theoretical topic with which this second part [of Hasse's Bericht] is concerned, the reciprocity law. Namely, Artin succeeded in proving his group-theoretic formulation of the reciprocity law, which he had already conjectured in 1923 and previously proved in special cases. In what follows, I call it after him, the Artin reciprocity law.

Original: Seit dem Erscheinen des ersten Teils dieses Berichts hat die Theorie der relativ-Abelschen Zahlkörper einen Fortschritt von der allergrössten Bedeutung gemacht, der gerade die für diesen zweiten Teil in Aussicht genommene Seite der Theorie, das Reziprozitätsgesetz, betrifft. Es gelang nämlich Artin, den allgemeinen Beweis für seine schon 1923 vermutete und in speziellen Fällen bewiesene gruppentheoretische

Formulierung des Reziprozitätsgesetzes zu geben, die ich im folgenden nach ihm das Artinsche Reziprozitätsgesetz nenne.

Later in the Bericht, Hasse deals with Tchebotarev's density theorem, and repeats the credit Artin gave to Tchebotarev when he states on page 133:

Tchebotarev erkannte, dass eine Idealklasseneinteilung in k , die einem geeigneten Kreiskörper K' über k entspricht, bei der Durchkreuzung mit einer gegebenen Primidealabteilung^{**}) aus dieser gerade die einzelnen Primidealklassen heraushebt. Auf diesem wichtigen Grundgedanken fussend konnte dann Artin die Auflösung der Abteilungen auch bei seinem neuen Problem, dem Reziprozitätsgesetz in gruppentheoretischer Formulierung, bewerkstelligen.

In my book *Algebraic Number Theory*, I gave Artin's own simplification of his proof in courses from the late forties. I also reproduced Deuring's subsequent proof, showing how the Tchebotarev density theorem follows in half a page from Artin's reciprocity law (*Math. Ann.* **110** (1934)).

In any case, Artin did not only "discover" his reciprocity law in 1923; he proved it and published it in 1927, one year after Tchebotarev proved his density theorem. The "glory" did not pertain to just "discovering" the law but also to proving it. Both Weil's original account and the translation are tendentious, because of the way the "justly" clause is used to counterbalance the "simple translation" or even "simple transposition" expression, as if there were any question about who proved what, about the merit of finding a proof, "simple" or not, and about the greatness of finding a proof, not just making the conjecture.

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Comments on Nonreferences in Weil's Works

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The following article does not claim to give a history of algebraic geometry and its connections with number theory as it developed in the 1920s, 1930s and 1940s. I need only provide enough documentation to substantiate its title. I make no claim to completeness. I had no responsibility to mention unpublished letters, whose existence is not generally known in the mathematical community. I am thankful to Schappacher, who made valuable comments as referee for Mitteilungen der DMV. In particular, to take his comments and questions into account, I had to expand my analysis and references.

A "history" would require much more extensive work, and in any case cannot be written with a claim of relative completeness until the Hasse-Weil correspondence from the thirties is published. In addition, there is an extensive correspondence with Deuring which deserves further study and description. I am grateful to Schappacher for bringing their existence to my attention.

I also add here an important mathematical point. Independently of history, endomorphisms and correspondences are mathematically united today, and have been for quite a while. Today, mathematically, one learns at once the fact that the ring of (equivalence classes of) correspondences is isomorphic to the ring of endomorphisms of the Jacobian (Albanese-Picard) varieties, not to speak of (co)homology rings. Historically, different parties arrived at this unification at different times, with different perspectives and different goals. One has to distinguish at least:

- Hurwitz;
- Castelnuovo's articles, especially those of 1905, 1906, 1921 (*Memorie Scelte* 1937);
- Severi and his *Trattato* (1926);
- The Hasse-Deuring development of the thirties;
- Weil's publications in 1940, 1941, 1946, 1948;
- Thereafter.

The paths and concerns of the above mentioned mathematicians crossed at different times, but are not identical. I am still unable to read the Italians, although when someone (like Kani) tells me what to look for and where, I can check it out to a large and hopefully sufficient extent. As far as I can make out, Castelnuovo links correspondences and endomorphisms without making any fuss about it. Anyhow, I don't have, and do not want to go into, a general commitment toward this history and the evolving mathematical viewpoint. As I wrote above, my commitment is simply to provide documentation to substantiate the title. I hope others will do a fuller historical job.

—S.L.

In 1999, the *Notices* of the American Mathematical Society published several articles on André Weil's works (April, June–July, September), especially one by Raynaud on Weil's contributions to algebraic geometry. These were complemented in the April 1999 *Notices* with an editorial on Weil by the *Notices* editor in chief Anthony Knapp. Concerning a comment at some Weil talk that proper credit was not given by Weil for some theorem, Knapp quoted Weil's answer: "I am not interested in priorities", and added his own comment: "This was the quintessential Weil. Mathematics to him was a collective enterprise." I object. Knapp created a reality which is askew from documentable facts. In the sense that mathematics progresses by using results of others, Knapp's assertion is tautologically true, and mathematics is a collective enterprise not only to Weil but to every mathematician.

However, there is also another sense. Mathematics is often a lonely business. Public recognition of the better mathematicians is a fact. Mathematicians are made aware early in their career of the need to attribute results properly. Weil transgressed certain standards of attribution several times throughout his life in significant ways. I documented at least one of these ways in my *Notices* Forum piece on the Shimura-Taniyama conjecture [La 95b]. In this piece, I reproduced a letter from Weil to me (3 December 1986), ending with Weil's own peremptory conclusion: "Concerning the controversy which you have found fit to raise, Shimura's letters seem to me to put an end to it, once and for all." A year after Knapp's editorial, Rosen returned to the Shimura-Taniyama conjecture with some comments [Ro 00] p. 476, where he did not accept Weil's own conclusion.

The *Gazette des Mathématiciens* also published a number of comments on Weil's works in 1999, Supplément au Numéro 80. The AMS *Notices* states editorially that the article [Ra 99a] which Raynaud wrote for the *Notices* is "expanded and translated from" the article [Ra 99b] which he wrote for the *Gazette*. We shall see that neither gave a proper account of Weil's contributions in relation to his predecessors. Unless otherwise specified, the passages I quote from Raynaud occur in both the *Notices* and *Gazette*. I quote the English version.

The questionable accounts in the *Notices* or *Gazette*, the Knapp editorial, and Rosen's comments, prompted me to complement my *Notices* Forum piece [La 95b] by further documentation showing how Weil several times throughout his life did not properly refer to his predecessors, but was "interested in priorities". These constitute significant examples when Weil does not regard mathematics as a "collective enterprise" in the sense that he hides the extent to which he uses previous work, and sets up or pokes fun at some of his predecessors, as we shall now document.

On Hasse's and Deuring's Work Concerning Endomorphisms and Correspondences

It was Hasse who uncovered the source of proof for the Riemann hypothesis in function fields (Artin's conjecture from his thesis), on curves of genus 1. Hasse dealt with the ring of endomorphisms of such a curve—an elliptic curve. He not only proved the theorem, but he uncovered the relation between characteristic 0 and characteristic p via reduction mod p [Ha 34]. After breaking open the whole question as above, Hasse [Ha 36] in three *Crelle* papers developed the theory purely algebraically on elliptic curves in characteristic > 0 , independently of reduction mod p . These works were followed by those of Deuring [De 37], [De 41a], [De 41b], who saw the connection with the theory of correspondences, as we shall summarize in greater detail below.

There are two articles and three books of Weil relevant to his continuation of Hasse's and Deuring's work and its interrelation with algebraic geometry stemming from Castelnuovo and Severi in the classical case: [We 40b], [We 41] where he announces his results, and [We 46], [We 48a], [We 48b] where he carries out complete proofs. Of these, only [We 41] contains bibliographical references (with the exception of one footnote in [We 48a]) as we shall see below in greater detail.

At the start of his announcement of a proof of the Riemann Hypothesis for function fields of genus > 1 over finite fields [We 40b], Weil writes (my translation):¹

I shall summarize in this Note the solution of the main problems of the theory of algebraic functions over a finite constant field; one knows that this theory has been the object of numerous works, and more specially, during these last years, those of Hasse and his students; as they have glimpsed, the theory of correspondences gives the key to these problems;...

There are no bibliographical references in Weil's 1940 paper to accompany the above comment. Weil's books on curves and abelian varieties [We 48a], [We 48b] published in the late forties do

¹ *The original few lines of Weil's paper read: "Je vais résumer dans cette Note la solution des principaux problèmes de la théorie des fonctions algébriques à corps de constantes finis; on sait que celle-ci fait l'objet de nombreux travaux, et plus particulièrement, dans les dernières années, de ceux de Hasse et de ses élèves; comme ils l'ont entrevu, la théorie des correspondances donne la clef de ces problèmes; mais la théorie algébrique des correspondances, qui est due à Severi, n'y suffit point, et il faut étendre à ces fonctions la théorie transcendante de Hurwitz."*

See below and footnote 5 for more precise information concerning the contributions of Hasse and Deuring.

not mention Hasse's and Deuring's contributions at all. Furthermore, Weil was indeed interested in priorities, as when he wrote at various times that some results of Severi were "rediscovered by Deuring", thereby minimizing his predecessors' discoveries, and misrepresenting the context in which they were made. For example, in his collected papers [We 79], Weil published a 1940 letter of his to Simone Weil [We 40a]. He calls this letter a sketch of a history of number theory ("esquisse d'histoire de la théorie des nombre") in the appended comments. At the beginning of this letter, Weil emphasizes its function by repeating twice that it is going to deal with the history of number theory. In that letter, Weil wrote (my translation):²

...it is incredible the extent to which people as distinguished as Hasse and his students, who gave their most serious thoughts to this subject for years, have not only neglected, but deliberately disdained the riemannian direction: it's to the point where they can't read works written in Riemannian (Siegel once poked fun at Hasse who had told him about not being able to read my paper in the Liouville journal), and that they rediscovered sometimes with considerable pain, in their dialect, important results which were already known, such as those of Severi on the ring of correspondences, rediscovered by Deuring.³

² "...il est incroyable à quel point des gens aussi distingués que Hasse et ses élèves, et qui ont fait de ce sujet la matière de leurs plus sérieuses réflexions pendant des années, ont, non seulement négligé, mais dédaigné de parti pris la voie riemannienne: c'est au point qu'ils ne savent plus lire les travaux rédigés en Riemannien (Siegel se moquait un jour de Hasse qui lui avait déclaré être incapable de lire mon mémoire de Liouville), et qu'ils ont retrouvé quelques fois avec beaucoup de peine, en leur dialecte, des résultats importants déjà connus, comme ceux de Severi sur l'anneau des correspondances, retrouvés par Deuring."

In [We 60], Weil wrote another similar put down of his predecessors, without citing them by name, stating that "les meilleurs spécialistes des théories arithmétiques 'galoisiennes' ne savaient plus lire le riemannien, ni à plus forte raison l'italien..." Collected Works Vol. II, p. 412, [My translation: "the best specialists of arithmetic and 'galois' theories didn't know any more how to read riemannian, let alone italian..."]

³ As Schappacher pointed out to me, in Weil's review of Chevalley's book on function fields in one variable [We 51], Weil confirms what he wrote previously: "...The algebraic method begins to show its weaknesses when it comes to dealing with extensions of the field of constants. Here also a new language and technique had to be invented by the author [Chevalley]...; in his introduction he acknowledges the considerable effort which this has cost him, and strangely enough, finds no better justification for it than a reference to a notably unsuccessful paper of Deuring on correspondences, where the latter rediscovered rather clumsily a few of Severi's more elementary results on the same subject."

This quote may be "quintessential Weil", but it shows something other than "mathematics to him was a collective enterprise." It is actually a tendentious presentation on several counts, passed off as history. To substantiate:

(a) Hasse and Deuring did not merely "rediscover ... in their dialect" results already known to Severi. Notably Hasse, who had just written major papers on complex multiplication (1927-1931), saw first the connection with the Riemann hypothesis in function fields of genus 1 [Ha 34], and he uncovered the connection between the existing problem of the Riemann hypothesis on elliptic curves and the theory of endomorphisms. Before Hasse, mathematicians had no inkling where a proof would come from. Thus Hasse made a fantastic step forward in connecting the complex theory with the purely algebraic theory in characteristic p , by showing how reduction mod p merges with complex multiplication in the theory of endomorphisms.⁴ Readers cannot get an inkling of the origins of such fundamental insights either from Weil's own works or from the accounts of Weil's works in the *Notices* (1999). Raynaud's account [Ra 99a] refers to Hasse in just one sentence: "[The Riemann hypothesis in the case of curves over finite fields] was first proved by Hasse [4] in the case of elliptic curves ($g = 1$)." There isn't even a reference to Hasse in the *Gazette* [Ra 99b].

Deuring's published papers deal with the theory of correspondences and endomorphisms algebraically in characteristic > 0 for higher genus as well as genus 1.⁵ In a first paper [De 37], Deuring not only gives an algebraic version of certain results, but he points to the connection with the transcendental theory citing Hurwitz's work (p. 190).

⁴Essentially, in [Ha 34], Hasse gives a one-line proof for the Riemann hypothesis on elliptic curves, assuming appropriate foundations. Indeed, he argues as follows. Lift the curve from characteristic p to characteristic 0, and also lift the Frobenius endomorphism to a complex endomorphism μ . The degree of Frobenius is q . Hence $\mu\bar{\mu} = q$, so $|\mu| = q^{1/2}$, which is one formulation of what one is after.

⁵I am indebted to Schappacher for informing me precisely that it was Deuring who, in 1936, first had the idea to generalize Hasse's proof by replacing endomorphisms by correspondences. According to Schappacher, Deuring communicated this to Hasse in a letter dated 9 May 1936, and an extensive correspondence ensued. Again according to Schappacher: "After Deuring's first announcement and first version, and before the Oslo International Congress in 1936, Hasse wrote a long letter to Weil telling him of Deuring's idea, and developing quite explicitly why and how he thinks that this idea is going to give a proof of the Riemann hypothesis in general..." The exchange of letters between Hasse and Weil during this period is not yet publicly available.

The present article only provides what I hope is enough documentation to substantiate its title. It is based entirely on the published record, but I look forward to the unveiling of the complete Hasse-Weil correspondence. I also look forward to a further study of Deuring's correspondence.

Then Deuring determined the structure of the group of points of finite order for elliptic curves ([De 41a], p. 36, submitted in June 1939), and started the l -adic representation of the endomorphisms of the curve on the group of points of l -power order, especially with $l \neq p$ (the characteristic) for the purpose of determining the structure of this ring [De 41a,b]. He also saw that this provided an algebraization of the complex representation.

Raynaud in the *Notices* and the *Gazette* does not mention these fundamental contributions when he attributes to Weil the introduction of l -adic representations in algebraic geometry. Of course, Weil went beyond Hasse and Deuring, ultimately giving a complete proof for the higher genus case, and establishing systematically a completely algebraic theory of abelian varieties.

(b) The phrase “not only neglected but deliberately disdained” (“non seulement négligé, mais dédaigné de parti pris”) is an example of Weil’s tendentious attributions. Artin, Davenport, Hasse, Mordell, Siegel, Weil had limitations, like all of us, including me. One of Hasse’s limitations was that he was not able to read the classical transcendental versions of the theory of abelian functions, as in Poincaré, Castelnuovo, or Weil’s paper [We 38], and was not able to read the Italian geometers as well as Weil; but it was not a question of “disdain” or “neglect”.

I don’t know how justified Weil is in attributing to Siegel the reaction toward Hasse as Weil describes it. But Siegel and Weil had no reason to ridicule or poke fun at (“se moquait de”) Hasse for his limitation in not understanding Weil’s transcendental approach to abelian functions. Although Siegel himself understood and handled this type of analysis, Siegel’s limitations were evidenced later by his inability to understand much of the mathematics and especially algebraic geometry developed in the fifties and sixties, as partly described in my article concerning Siegel’s letter to Mordell [La 95a].

I myself have had my own limitation in that I was not (and still am not) able to read the papers of the Italian algebraic geometers. I needed the algebraic versions by van der Waerden, Chevalley, Zariski and Weil himself to get into the subject. It was not at all the case that I “not only neglected but deliberately disdained” the works of the Italian geometers.

(c) Whatever individual limitations existed, certain previous results of algebraic geometry, some coming from the more algebraic methods of Severi and others from more transcendental methods of Hurwitz and mixed transcendental-algebraic methods of Castelnuovo (see below), needed to be algebraicized completely because they were relevant in this generality for the applications to the Riemann Hypothesis on higher genus curves in

characteristic p . In footnote 1 of [We 41], referring to the theory of correspondences in Severi’s *Trattato*, Weil himself makes the point precisely: “It should be observed that Severi’s treatment, although undoubtedly containing all the essential elements for the solution of the problems it purports to solve, is meant to cover only the classical case where the field of constants is that of complex numbers, and doubts may be raised as to its applicability to more general cases, especially to characteristic $p \neq 0$. A rewriting of the whole theory, covering such cases, is therefore a necessary preliminary to the applications we have in view.” This “rewriting” is not a matter of “dialect”. Deuring (following Hasse) established the connection between more general algebraic geometry and the main problem of concern to Hasse and to him, showing what direction to take; and he started the process of developing parts of algebraic geometry relevant to this concern in a way sufficient to include characteristic > 0 .

On van der Waerden

Van der Waerden’s series of papers *Zur Algebraischen Geometrie* in *Math. Ann.* (see [vdW 83]) and his book *Einführung in die algebraische Geometrie* [vdW 39] both contributed to providing completely algebraic versions of some results known over the complex numbers, and went beyond. For example, van der Waerden introduced generic points, among other basic and important contributions to algebraic geometry, including the laying of algebraic foundations. These were basic to Weil’s book *Foundations of Algebraic Geometry* [We 46]. In [We 41] Weil himself refers to them in an appropriate manner, “in the precise sense defined by van der Waerden”. Weil reproduces the definition in the accompanying footnote, which refers to van der Waerden’s *Einführung in die algebraische Geometrie*. Weil also references two papers by van der Waerden for some questions of intersection theory, including the definition of intersection numbers and the application to the theory of correspondences. In addition, in the Introduction to *Foundations*, Weil states very appropriately:

...there is no doubt that, in this field [algebraic geometry], the work of consolidation has so long been overdue that the delay is now seriously hampering progress in this and other branches of mathematics. To take only one instance, a personal one, this book has arisen from the necessity of giving a firm basis to Severi’s theory of correspondences on algebraic curves, especially in the case of characteristic $p \neq 0$ (in which there is no transcendental method to guarantee the correctness

of the results obtained by algebraic means), this being required for the solution of a long outstanding problem, the proof of the Riemann hypothesis in function-fields. The need to remedy such defects has been widely felt for some time; and, during the last twenty years, various authors, among whom it will be enough to mention F. Severi, B. L. van der Waerden, and more recently O. Zariski, have made important contributions towards this end. To them the present book owes of course a great deal;

...As for my debt to my immediate predecessors, it will be obvious to any moderately well informed reader that I have greatly profited from van der Waerden's well-known series of papers¹, where, among other results, the intersection-product has for the first time been defined (not locally, however, but only under conditions which ensure its existence 'in the large'); from Severi's sketchy but suggestive treatment of the same subject, in his answer to van der Waerden's criticism of the work of the Italian school²;...

The notion of specialization, the properties of which are the main subject of Chap. II, and (in a form adapted to our language and purpose) the theorem on the extension of a specialization... (th. 6 of Chap. II, §2) will of course be recognized as coming from van der Waerden...

Thus Raynaud's assertion in [Ra 99a], [Ra 99b] that the book *Foundations* "marks a break ("rupture" in the French version) with respect to the works of his predecessors—B. L. van der Waerden [10] and the German school..." is not correct.⁶ It goes against Weil's own specific references in [We 41] and the expression of indebtedness expressed in the above Introduction. Weil went beyond van der Waerden in significant ways, but it was not a "break" or "rupture".

Raynaud goes on about what he calls the break resp. rupture: "To signal this clearly, the book [Foundations] contains no bibliography." The first

⁶ Incidentally, Raynaud adds: "The emphasis is systematically put on fields: fields of definition of varieties, fields of rational functions. Fifteen years later, Grothendieck [3], in developing the language of schemes, would bring out the role of rings." Actually, Chow and Igusa had already brought out the role of rings in several important papers dating back to 1957, 1958, 1959, cf. [La 96], §5.

part of this statement goes into intent, and is open to different interpretations which we shall leave to the reader. The second part is correct but somewhat misleading because it does not take into account Weil's substantial specific attributions in the Introduction to *Foundations*. It is in this context that Weil makes the assertion: "Our method of exposition will be dogmatic and unhistorical throughout, formal proofs without references, being given at every step."

On Castelnuovo's Work

The situation is very different with respect to Castelnuovo's work. Weil did not regard mathematics as a collective enterprise with Castelnuovo, by leaving out of his references throughout his life the extent to which he used Castelnuovo's ideas concerning the equivalence defect, the characteristic polynomial, and the Jacobian of a curve.

In the brief announcement [We 40b], Weil like Deuring only mentions Hurwitz when he states: "...but the algebraic theory of correspondences, which is due to Severi, does not suffice, and it is necessary to extend to these [algebraic] functions the transcendental theory of Hurwitz." There is no mention of Castelnuovo in [We 40b] or [We 41].

In my book on abelian varieties, I systematically gave Weil credit for his ability to make the contributions of Severi and Castelnuovo available to the postwar period of algebraic geometry, and to go beyond. In fact, in historical comments concerning Castelnuovo's equivalence defect, I stated that Weil "was the first to recognize that Castelnuovo's theorem on the equivalence defect of correspondences on a curve could be expressed as a theorem on abelian varieties." It turns out that I was wrong. I was taken to task for this erroneous attribution by Kani [Ka 84], see especially p. 27, footnote 12. Indeed, Weil makes only one reference to Castelnuovo in his book on abelian varieties [We 48b], for some of the basic theorems on abelian varieties. Referring to the principle that a rational map of a variety into an abelian variety is always defined at a simple point, and that if both varieties are abelian, then the map is a homomorphism, up to a translation, Weil states in the introduction to the book (my translation):⁷ "...already Castelnuovo had recognized how to use the latter⁽³⁾, although it is not easy to find in his works a formulation or even less a precise justification... The proof of Poincaré's theorem from the above principle, which one will

⁷ "...déjà Castelnuovo avait reconnu le parti qu'on peut tirer de ce dernier⁽³⁾, sans qu'il soit pourtant facile d'en trouver chez lui une formulation ni encore moins une justification précise... La démonstration du théorème de Poincaré à partir du principe en question, qu'on trouvera au n° 51 du présent travail, est par exemple substantiellement identique à celle qu'en donne Castelnuovo, pour le cas classique, au n° 9 de ce mémoire."

find in No. 51 of the present work, is for instance substantially the same as the proof given by Castelnuovo in the classical case, in No. 9 of his memoir." Weil's footnote (3) refers to "the beautiful paper" ("le beau mémoire") [Ca 05], specifying that it is reproduced as No. XXVI in the volume *Memorie Scelte* (*Selected Papers*), published in 1937 [Ca 37]. In particular, Weil was fully aware of the *Memorie Scelte* when he made that reference in [We 48b].

Weil's books [We 48a], [We 48b] contain no other bibliographical references besides the footnote (3) just mentioned. In [We 48a] p. 28, Weil only writes (my translation): "As will be recognized without pain, the present memoir is directly inspired by the works of Castelnuovo and Severi on the same subject."⁸ What does "directly inspired" mean? Weil does not refer to any other paper by Castelnuovo, and he omitted far more important and relevant references to at least two other Castelnuovo papers, namely the paper on the "positivity of the trace" [Ca 06], reproduced in *Memorie Scelte* [Ca 37] No. XXVIII, and the paper *Sulle funzione abeliane* [Ca 21], reproduced in *Memorie Scelte* No. XXX.

I learned of this second paper and of Castelnuovo's fundamental contributions from Kani [Ka 84]. In the complex case, the relation between Castelnuovo's equivalence defect and an intersection number on the Jacobian is clearly established in the paper [Ca 21] = [Ca 37] No. XXX. Furthermore, Castelnuovo defines the characteristic polynomial of an endomorphism of the Jacobian (determinant of the pfaffian of the complex representation) expressing it as an intersection power, pp. 536–537. He thus merges the complex analytic theory and the algebraic intersection theory. He develops systematically the theory of this characteristic polynomial. He thereby shows that the equivalence defect occurs as the penultimate coefficient of the characteristic polynomial, i.e. the trace, as on pp. 536, 538, and 541, and that all these coefficients can be expressed as intersection numbers. Castelnuovo also gives the intersection formulas of the sum of the curve with itself r times and the theta divisor, as well as powers of the theta divisor. See pp. 547–548. In the fifties, I learned such results from Weil's book and lectures on abelian varieties. Weil in his book [We 48b] gives Castelnuovo's formalism and generalizes it. Compare [We 48b] pp. 73, 74, 132 with Castelnuovo's paper pp. 537–547. But there are no references to this paper in Weil's works which deal with these matters, nor were there in his courses, nor are there in the *AMS Notices* or *Gazette* articles by

⁸ "Comme on le reconnaîtra sans peine, le présent mémoire est directement inspiré des travaux de Castelnuovo et Severi sur le même sujet." I myself in [La 95a] was still misled as to what this phrase meant, and I still attributed the trace and its positivity to Weil.

Raynaud. In [Ra 99a,b] Raynaud attributes to Weil Castelnuovo's algebraic definition of the characteristic polynomial via intersection theory. Whatever "directly inspired" means, Raynaud did not give a proper account of Weil's contribution to the subject in relation to Castelnuovo's.

At the end of his article [Ra 99b] Raynaud states (my translation): "Let us mention that Weil, who was very reserved with respect to the rigor of 'Italian geometry', nevertheless attributes to Castelnuovo the discovery of the positivity of the trace, in the theory of correspondences."⁹ In [Ra 99a], Raynaud states differently: "Castelnuovo had proved this in the complex case." Raynaud does not indicate any specific reference where Weil makes the attribution claimed in [Ra 99b]. There is no such attribution in the papers [We 40b], [We 41] where a proof of RH for curves over finite fields is first announced, nor in the books [We 48a], [We 48b] where a complete proof is given. To my knowledge, Weil made such an attribution only decades later, as a comment in his *Collected Papers*, Vol. I, (1979) p. 557. There, Weil calls it "one of the most beautiful discoveries of Castelnuovo", and refers to Castelnuovo's *Memorie Scelte* No. XXVIII, pp. 509–517. In whatever references he does make at different times, Weil gives no evidence of being "reserved" (let alone "very reserved") with respect to the "rigor" of Italian geometry, whether comparing results of Deuring to those of Severi (see footnote 3, "rediscovered rather clumsily"), or mentioning Castelnuovo's trace in 1979. Furthermore, Weil's specific references to items in the *Memorie Scelte* (XXVI in 1948 and XXVIII in 1979) document his awareness of this volume.

After the comment in Weil's *Collected Papers*, p. 557, which we have just cited, Weil adds (my translation): "But I read Castelnuovo only in 1945 in Brasil; I realized then that Severi, in his *Trattato* (pp. 286–287) had not credited his elder [predecessor?] to the extent he deserved."¹⁰ [sic!] Thus on the one hand, Weil knew the works of the Italians well enough to chide Hasse and Deuring for having "not only neglected but deliberately disdained the riemannian direction" and "rediscovered, sometimes with considerable pain, in their dialect, important results which were already known, such as those of Severi on the ring of correspondences, rediscovered by Deuring." On the other hand, Weil gives no references to Castelnuovo for the positivity of the trace or the theory of the characteristic polynomial in [We 40b], [We 41], [We 48a],

⁹ "Signalons que Weil, qui était très réservé à l'égard de la rigueur de la 'géométrie italienne', attribue néanmoins à Castelnuovo la découverte de la positivité de la trace, en théorie des correspondance."

¹⁰ "Mais je ne lus Castelnuovo qu'en 1945 au Brésil; je constatai alors que Severi, dans le *Trattato* (pp. 286–287) n'avait guère fait à son ancien la part qu'il méritait."

Letter to the Editors

I regard it as unfortunate when addressing issues of professional or institutional responsibility is interpreted in terms of personal animosities. For instance, in his article “Adieu à un ami” (*Gazette des Mathématiciens*, 1999), Cartier writes: “Quel fut bien le déclencheur de la rupture entre Weil et son ancien disciple Serge Lang? Il est vrai que les deux parties étaient expertes en récriminations.” Such a version is highly tendentious, and I reject it.

My documentation of certain aspects of mathematical history implies nothing concerning personal relationship, one way or another. I take this opportunity to put in the record some information concerning Hasse’s behavior after France’s defeat in 1940. In fall 1940, Hasse went to meet Elie Cartan at his home in Paris. Hasse was dressed in a German uniform. The only other person present was Elie Cartan’s son, Henri Cartan, whom I heard personally report the encounter publicly in the late fifties, as follows. Hasse acted in a very friendly way, and proposed to Elie Cartan that French and German mathematicians should cooperate, independently of the circumstances which were otherwise occurring. Elie Cartan answered in an equally friendly fashion that it was an excellent idea, but that the Poles should also take part. Hasse then answered no, that the Polish people was a separate people with whom it was not possible to collaborate. Elie Cartan then answered that under these conditions, it was impossible to start a French-German mathematical cooperation.

Some 40 years later, in 2000–2001, at the Max-Planck Institut in Bonn, I heard for the first time an account from the Norwegian mathematician Arnfinn Laudal, of a similar visit that Hasse made to Thoralf Skolem in Oslo. Laudal got the story from Skolem himself, and the story was confirmed recently by Skolem’s children. Hasse had shown up at Skolem’s home dressed in German navy uniform (“Kommandeur-Kapitain-der See” uniform), but was refused entrance by Skolem, on the doorsteps. Hasse had come with a proposition like the one he had made to Elie Cartan. There occurred a vigorous and high-voiced exchange between Skolem and Hasse.

Thus Hasse’s visit to Elie Cartan was not an isolated event. Different people react differently about recalling the painful past of Nazism, and the role of individual mathematicians during that period. We make ad hoc decisions about what to recall, and when, depending on circumstances. My current decision is represented by this letter and the accompanying article on some mathematical history.

—Serge Lang
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[We 48b]; nor does he refer to Severi’s *Trattato* in [We 40b], [We 48a], [We 48b].

The extent to which Severi himself did not properly credit Castelnuovo in the *Trattato* remains to be analyzed separately.

To summarize: In the thirties and forties, the main motivation for Hasse, Deuring, and Weil was to carry out the program of developing enough algebraic geometry (geometric or transcendental) purely algebraically, and with complete proofs, in order to reach a proof of the Riemann hypothesis for curves of higher genus. What Weil did in the forties was to algebraicize completely Castelnuovo’s theory [Ca05], [Ca 06], [Ca 21], as well as parts of Severi’s *Trattato*, and extend them, following Hasse’s and Deuring’s fundamental discoveries and works on the subject, as described above. Of course, to carry out this plan was a first rate mathematical achievement. For two decades, Weil was the only one in the world capable of pulling it off, in large part because he was able to read Castelnuovo, Severi, and Hurwitz. I have the highest regard for his mathematics. But being a great mathematician is not a license for obscuring and misrepresenting works and original ideas of others who opened up the field, and for poking fun at them.

On Mordell’s Conjecture

Weil correctly referred to Mordell’s conjecture in his thesis [We 28], when he stated that (my translation)¹¹ “...this conjecture, already stated by Mordell (loc. cit. note 4) seems confirmed to some extent by an important result recently proved...”, and then cites Siegel’s theorem on the finiteness of integral points on curves of genus at least 1. Weil made a similar evaluation in *Arithmetic on algebraic varieties* [We 36], but without reference to Mordell, namely: “On the other hand, Siegel’s theorem, for curves of genus > 1 , is only the first step in the direction of the following statement: On every curve of genus > 1 , there are only finitely many rational points.”

Subsequently, Weil explicitly denigrated Mordell’s contribution. In his *Two lectures on number theory, past and present* [We 74a], he wrote: “For instance, the so-called Mordell conjecture on Diophantine equations says that a curve of genus at least two with rational coefficients has at most finitely many rational points.” Why “so-called”? Weil goes on: “It would be nice if this were so, and I would rather bet for it than against. But it is no more than wishful thinking

¹¹ “...cette conjecture, déjà énoncée par Mordell (loc. cit. note 4) semble confirmée en quelque mesure par un important résultat démontré récemment...”

because there is not a shred of evidence for it, and also none against.” In his *Collected Papers* Vol III, p. 454, he goes one better (my translation):¹² “We are less advanced with respect to ‘Mordell’s conjecture’. This is a question which an arithmetician can hardly fail to raise; in any case, one sees no serious reason to bet for or against it.”

I have several objections to Weil’s tendentious evaluation (“quintessential Weil”). First, Weil puts Mordell’s conjecture in quotes, as if there was some question about Mordell’s famous insight.

Second, concerning a “question which an arithmetician can hardly fail to raise”, I would ask when? It is quite a different matter to raise the question in 1921, as did Mordell, or decades later, especially following Mordell’s insight. Furthermore, Weil here goes against the evaluations which he himself made in the two papers mentioned above, dating back to 1928 and 1936. Weil at the end of his 1928 thesis even proposed a generalization of Mordell’s conjecture as follows (my translation):¹³ “The most important problem of the theory is no doubt precisely to know if, among all virtual systems of degree $\leq p - 1$ arising from a finite set of generators, there are infinitely many effective ones; if this question has a negative answer, it would follow in particular that on a curve of genus $p > 1$ there is only a finite number of rational points, whatever be the domain of rationality (for example, Fermat’s equation $x^n + y^n = z^n$, would have only a finite number of solutions for each value of $n > 2$.)” However, when I learned abelian varieties (from Weil’s books and his course in Chicago in 1954), I observed that Weil’s proposed generalization for effective $(p - 1)$ -cycles on curves was false because the theta divisor could contain an elliptic curve. At the time, I made my general conjecture that a subvariety of an abelian variety is Mordellian if (and only if) it does not contain the translation of a non-trivial abelian subvariety. My conjecture was proved by Faltings three decades later.¹⁴

¹² “Nous sommes moins avancés à l’égard de ‘conjecture de Mordell’. Il s’agit là d’une question qu’un arithméticien ne peut guère manquer de se poser; on n’aperçoit d’ailleurs aucun motif sérieux de parier pour ou contre.”

¹³ “Le problème le plus important de la théorie est sans doute précisément de savoir si, parmi tous les systèmes virtuels de degré $\leq p - 1$ qui se déduisent d’une base finie, il peut s’en trouver une infinité d’effectifs; si la question devait être résolue par la négative, il s’ensuivrait en particulier que sur une courbe de genre $p > 1$ il n’y a qu’un nombre fini de points rationnels quelque soit le domaine de rationalité (par exemple l’équation de Fermat, $x^n + y^n = z^n$, n’aurait qu’un nombre fini de solutions pour chaque valeur de $n > 2$.)”

¹⁴ In his article [Fa 91] p. 549, Faltings states that the conjecture was made “by A. Weil and also by S. Lang”; (p. 549) later in [Fa 94] p. 175, it’s “by A. Weil (as well as apparently independently by S. Lang).” (p. 175) I objected to Faltings about the attribution to Weil, which is incorrect. Cf. the quotes from Weil I give in the above text.

Third, concerning Weil’s statements in 1974 and 1979 that there is no “shred of evidence” or “motif sérieux” [serious reason] for Mordell’s conjecture, they not only went against his own evaluations in earlier decades, and similar evaluations by others since,¹⁵ but they were made after Manin proved the function field analogue in 1963; after Grauert gave his other proof in 1965; after Parshin gave his other proof in 1968, while indicating that Mordell’s conjecture follows from Shafarevich’s conjecture (which Shafarevich himself had proved for curves of genus 1); at the same time that Arakelov theory was being developed and that Zarhin was working actively on the net of conjectures in those directions (Shafarevich conjecture, Tate conjecture, isogeny conjecture, etc.); and within four years of Faltings’ proof.

On the Shimura-Taniyama Conjecture

I gave a systematic account of this item in my *Notices* Forum article [La 95b], which I now urge readers to look at again in the present broader context. Weil’s first reaction when Shimura told him the conjecture was to make the comment: “I don’t see any reason against it, since one and the other of these sets are denumerable, but I don’t see any reason either for this hypothesis.” [We 79], Vol. III, p. 450. When others brought out the role of Shimura and Taniyama, Weil started inveighing against conjectures, and kept it up for the next decade. In my article, I quote from a letter where Shimura writes: “For this reason, I think, he [Weil] avoided to say in a straightforward way that I stated the conjecture... Of course Weil made a contribution to this subject on his own, but he is not responsible for the result on the zeta functions of modular elliptic curves, nor for the basic idea that such curves will exhaust all elliptic curves over \mathbb{Q} .” If Weil had started his 1967 paper with a couple of sentences stating that Shimura told him this basic idea, and that the paper was the result of his thinking about the idea, then there would be evidence in this instance for Knapp’s purported description of Weil’s motivation. As it is, Weil’s suppression of Shimura’s role in making the conjecture was evidence of something opposite to viewing mathematics as a “collective enterprise”. It is unfortunate that the accumulated evidence was not taken into account by some people to follow Weil’s own conclusion in his letter to me, already quoted in the introduction: “Concerning the controversy which you have found fit to raise, Shimura’s letters seem to me to put an end to it, once and for all.”

¹⁵ For instance, Parshin in 1968 [Pa 68] wrote: “Finally when $g > 1$, numerous examples provide a basis for Mordell’s conjecture that in this case $X(\mathbb{Q})$ is always finite. The one general result in line with this conjecture is the proof by Siegel that the number of integral points (i.e. points whose affine coordinates belong to the ring Z of integers) is finite.”

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