Forum

Some History of the Shimura-Taniyama Conjecture

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I shall deal specifically with the history of the conjecture which asserts that every elliptic curve over **Q** (the field of rational numbers) is modular. In other words, it is a rational image of a modular curve $X_0(N)$, or equivalently of its Jacobian variety $J_0(N)$. This conjecture is one of the most important of the century. The connection of this conjecture with the Fermat problem is explained in the introduction to Wiles's paper (*Ann. of Math.* May 1995), and I shall not return here to this connection. However, over the last thirty years, there have been false attributions and misrepresentations of the history of this conjecture, which has received incomplete or incorrect accounts on several important occasions. For ten years, I have systematically gathered documentation which I have distributed as the "Taniyama-Shimura File". Ribet refers to this file and its availability in [Ri 95]. It is therefore appropriate to publish a summary of some relevant items from this file, as well as some more recent items, to document a more accurate history. I call the conjecture the Shimura-Taniyama conjecture for specific reasons which will be made explicit.

Serre's Bourbaki Seminar. To start, I quote from Serre's Bourbaki Seminar of June 1995, when Serre wrote:

> Une courbe elliptique sur **Q** pour laquelle la conjecture 1′ est vraie a été longtemps appelée une courbe "de Weil". On dit maintenant que c'est une courbe elliptique "modulaire".
>
> Le terme de "conjecture de Weil" a été d'abord utilisé pour désigner l'ensemble des conjectures du n^0 1.1; c'était un peu facheux, vu le risque de confusion avec d'autres conjectures de Weil. On est passé de là à "conjecture de Taniyama-Weil"; c'est la terminologie utilisée ici. Plus récemment, on trouve "conjecture de Shimura-Taniyama-Weil", ou même "conjecture de Shimura-Taniyama", le nom de Shimura étant ajouté en hommage à son étude des quotients de $J_0(N)$. Le lecteur choisira. L'essential est qu'il sache qu'il s'agit du même énoncé.

Serre's statement that "Shimura's name was added in homage to his study of the quotients of $J_0(N)$" is false. Serre misrepresents other people's reasons for associating Shimura's name to the conjecture, namely, that the conjecture is due principally to Shimura. An "Erratum" in the *Notices* (January 1994) corrected the previous use of the expression "Taniyama conjecture" in two previous articles (July/August 1993 and October 1993) and concluded that these articles "should have used the standard name, 'Taniyama-Shimura Conjecture'." Wiles in his e-net message of 4 December 1993 called it the Taniyama-Shimura conjecture. In the article [DDT 95] by Darmon, Diamond, and Taylor, it is called the Shimura-Taniyama conjecture. Faltings in his account of Wiles's proof in the *Notices* July 1995 refers to "the conjecture of Taniyama-Weil (which essentially is due to Shimura)." Thus Faltings points to a contradiction in the way some people have called the conjecture.

So what happened which led to such contradictions?

§ 0. Preliminaries: Hasse's Conjecture

In the 1920s and 1930s until about 1940–1941, zeta functions and L-functions had been extensively studied by Artin, Hasse, and Hecke from various points of view. There is no need to go extensively into this preliminary history here; but to understand the context of what follows, it is worth recalling that in the thirties, Hasse defined the zeta function of a variety over a number field by taking the prod-

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uct over all prime ideals of the zeta functions of this variety reduced modulo the primes. He conjectured that this product has a meromorphic continuation over the whole plane and a functional equation. In an influential address and paper, Weil brought the conjecture to the attention of the mathematical community at the International Congress in 1950. He attributed this "very interesting conjecture" to Hasse [We 1950b], cf. *Collected Papers* Vol. I, p. 451. Weil commented: "In a few simple cases, this function [previously defined by Hasse] can actually be computed; e.g., for the curve $Y^2 = X^3 - 1$ it can be expressed in terms of Hecke's L-functions for the field $k(\sqrt[3]{1})$; this example also shows that such functions have infinitely many poles, which is a clear indication of the very considerable difficulties that one may expect in their study." In 1950, as far as I know, Hasse had not published his conjecture, but he did publish it in 1954; see his comments on the first page of [Ha 54].

§ 1. The Situation in 1955

The Taniyama problems. Renewed interest in modular curves in the post-WW II period of mathematics occurred in the fifties as a result of work of Taniyama and Shimura. Taniyama at the conference on number theory in Tokyo-Nikko in 1955 was interested in obtaining various zeta functions and L-series as Mellin transforms of some type of automorphic forms. He formulated four problems along these lines—problems 10, 11, 12, 13—in a collection of 36 problems passed out in English at this conference, which was attended by both Serre and Weil. Although these problems were published in Japanese in Taniyama's collected works, they were not, unfortunately, published in English. However, many people, including Serre, had copies. Serre drew attention to these problems in the early 1970s. Taniyama's problem 10 was concerned with Dedekind zeta functions and Hecke L-series, as follows (incorrect English being reproduced as in the original here and subsequently):

> 10. Let k be a totally real number field, and $F(\tau)$ be a Hilbert modular form to the field k. Then, choosing $F(\tau)$ in a suitable manner, we can obtain a system of Hecke's L-series with "Grössencharaktere" λ, which corresponds one-to-one to this $F(\tau)$ by the process of Mellin-transformation. This can be proved by a generalization of the theory of operator T of Hecke to Hilbert modular functions (cf. Herrmann).
>
> The problem is to generalize this theory in the case where k is a general (not necessarily totally real) number field. Namely, to find an automorphic form of several variables from which L-series with "Grössencharaktere" λ of k may be obtained, and then to generalize Hecke's theory of operator T to this automorphic form.
>
> One of the aim of this problem is to characterize L-series with "Grössen oder Klassencharaktere" of k; especially to characterize the Dedekind zeta function of k in this method, which is not yet done even if k is totally real.

Problem 11 shifts to elliptic curves with complex multiplication, but is less relevant to the questions considered here. Then Taniyama formulates two problems which begin the process of identifying the zeta function of an elliptic curve with the Mellin transform of some automorphic form, namely, problems 12 and 13, which we quote in full.

> 12. Let C be an elliptic curve defined over an algebraic number field k, and $L_C(s)$ denote the L-function of C over k. Namely,
>
> $$\zeta_C(s) = \frac{\zeta_k(s)\zeta_k(s-1)}{L_C(s)}$$
>
> is the zeta function of C over k. If a conjecture of Hasse is true for $\zeta_C(s)$, then the Fourier series obtained from $L_C(s)$ by the inverse Mellin transformation must be an automorphic form of dimension -2, of some special type (cf. Hecke). If so, it is very plausible that this form is an elliptic differential of the field of that automorphic functions. The problem is to ask if it is possible to prove Hasse's conjecture for C, by going back this considerations, and by finding a suitable automorphic form from which $L_C(s)$ may be obtained.

> 13. Concerning the above problem, our new problem is to characterize the field of elliptic modular functions of "Stufe" N, and especially, to decompose the Jacobian variety J of this function field into simple factors, in the sense of isogeneity.
>
> It is well known, that, in case $N = q$ is a prime number, satisfying $q \equiv 3 \pmod 4$, J contains elliptic curves with complex multiplication. Is this true for general N?

As Shimura has pointed out, there were some questionable aspects to the Taniyama formulation in problem 12. First, the simple Mellin transform procedure would make sense only for elliptic curves defined over the rationals; the situation over number fields is much more complicated and is not properly understood today, even conjecturally. Second, Taniyama had in mind automorphic forms much more general than what are now called "modular forms" which belong to the modular curves $X_0(N)$.[1]

[1]At my request to get clarification, Shimura wrote me on 22 September 1986:

> I think Taniyama wasn't very careful when he stated his problem No. 12. He referred to Hecke and as I

The "mysterious" elliptic curves over Q. In any case, at the time the matter was an enigma. In a letter to me dated 13 August 1986, Shimura brought to my attention notes taken by Taniyama of an informal discussion session held 12 September 1955, 7:30–9:30 p.m. These notes were published in Japanese in *Sugaku*, May 1956, pp. 227–231, giving the following exchange between Taniyama and Weil (Shimura's translation):

> Weil asks Taniyama: Do you think all elliptic functions are uniformized by modular functions?
>
> Taniyama: Modular functions alone will not be enough. I think other special types of automorphic functions are necessary.
>
> Weil: Of course some of them can probably be handled that way. But in the general case, they look completely different and mysterious. But, for the moment, it seems effective to use Hecke operators. Eichler employed the Hecke theory, and certain elliptic curves with no complex multiplication are contained (in his results). Infinitely many such elliptic curves…
>
> Deuring: No, only a finite number of such curves are known.

In this letter of 13 August 1986, Shimura also wrote me:

> The same issue [of *Sugaku*] contains also Taniyama's problem No. 12 (p. 269), in which he says that the Mellin transform of the zeta function of an elliptic curve must be an automorphic form of weight 2 of a special type (cf. Hecke). He doesn't say modular form. That explains why he said other automorphic functions where necessary. I am sure he was thinking of Hecke's paper No. 33 (1936) which involves some Fuchsian groups not necessarily commensurable with $SL_2(Z)$. Of course, in 1955, our understanding of the subject was incomplete, and he wasn't bold enough to speculate that modular functions were enough.
>
> As for Weil, he was far from the conjecture. (It seems that strictly speaking, Weil has never made the conjecture; see item 4 below.) Indeed, in his lecture titled "On the breeding of bigger and better zeta functions" at the University of Tokyo sometime in August or September 1955, he mentions Eichler's result and adds: "But already in the next simplest case, that is, the case of an elliptic curve which cannot be connected with modular functions in Eichler's fashion, the properties of its zeta function are completely mysterious…." (loc. cit. p. 199)

wrote, he was thinking about Hecke's paper No. 33 (1936) which concerns automorphic function fields of dimension one. The functional equation treated there involves only one $\Gamma(s)$, so that the problem doesn't make sense unless the curve is defined over Q.

Also, he specifically speaks of a form of weight 2, and of an elliptic differential of the function field. These make sense only in the one-dimensional case for the following reasons.

First of all, you have to remember, in 1955, the results of Hecke, Maass, and Hermann were the only relevant things. Obviously the Maass theory can be eliminated, because it doesn't produce function fields nor elliptic differentials. In the Hilbert modular case, if an elliptic differential means a holomorphic 1-form, then its weight must be $(2, 0, \dots, 0)$, $(0, 2, 0, \dots, 0)$, or $(0, \dots, 0, 2)$, but such a form cannot be called a form of weight 2. In fact, such a non-vanishing form doesn't exist. If it is a form of weight $(2, \dots, 2)$, then it defines a differential form of highest degree, so that you cannot call it an elliptic differential.

For these reasons, I think he was not completely careful, and if someone had pointed out this, he would have agreed that the problem would have to be revised accordingly.

§ 2. The Sixties

Shimura's conjecture. Shimura himself in the late fifties and sixties extended Eichler's results and proved that elliptic curves which are modular have zeta functions which have an analytic continuation. (Cf. the three papers [Sh 58], [Sh 61], and [Sh 67].) But except for Shimura, it was universally accepted in the early sixties that most elliptic curves over the rationals are not modular. In a letter to Freydoon Shahidi (16 September 1986), Shimura gave evidence to this effect when he wrote:

> At a party given by a member of the Institute in 1962–64, Serre came to me and said that my results on modular curves (see below) were not so good since they didn't apply to an arbitrary elliptic curve over **Q**. I responded by saying that I believed such a curve should always be a quotient of the Jacobian of a modular curve. Serre mentioned this to Weil who was not there. After a few days, Weil asked me whether I really made that statement. I said: "Yes, don't you think it plausible?"

At this point, Weil replied: "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." (For a confirmation by Weil of this conversation, see below.)[2]

[2]The rationale for Shimura's conjecture was precisely the conjectured functional equation (Hasse), along the lines indicated in Taniyama's problem 12, suitably corrected. Shimura's bolder insight was that the ordinary modular functions for a congruence subgroup of $SL_2(\mathbf{Z})$ suffice to uniformize elliptic curves defined over the rationals.

In the middle sixties, Shimura was giving lectures on the arithmetic theory of modular forms. Especially, he gave a version of the functional equation satisfied by a modular elliptic curve, which he communicated to Weil in 1964–65. This version was extended to higher dimensional factors in his book *Introduction to the Arithmetic Theory of Automorphic Functions*, Theorems 7.14 and 7.15.

Weil's 1967 paper. No attribution of the conjecture to Shimura. After thinking about the conjecture told to him by Shimura, Weil published his 1967 paper "Über die Bestimmung Dirichletscher Reihen durch Funktionalgleichungen" [We 1967a] in which he proved that if the zeta function of an elliptic curve and sufficiently many "twists" have a functional equation, then it is the Mellin transform of a modular form. However, nowhere in this paper does Weil mention Taniyama's or Shimura's role in the conjecture. In the letter to Shahidi, Shimura also stated that he explained to Weil "perhaps in 1965" how the zeta function of a modular elliptic curve has an analytic continuation. At the end of his 1967 paper, Weil acknowledges this ("nach eine mitteilung von G. Shimura…"). But Shimura added:

> I even told him [Weil] at that time that the zeta function of the curve C' mentioned there is the Mellin transform of the cusp form in question, but he spared that statement. Eventually I published a more general result in my paper in *J. Math. Soc. Japan* 25 (1973), as well as in my book (Theorem 7.14 and Theorem 7.15).
>
> 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 **Q**.

Weil calls the modularity "still problematic". Actually, at the very end of his 1967 paper, written in German, Weil concludes: "Ob die Dinge immer, d.h. für jede über **Q** definierte Kurve C, sich so verhalten, scheint im Moment noch problematisch zu sein und mag dem interessierten Leser als Übungsaufgabe empfohlen werden." By "sich so verhalten", Weil meant whether every elliptic curve over **Q** is modular, and so even then, he did not outright make the conjecture; he called it "at the moment still problematic" and left it as an "exercise for the interested reader"!

Weil's 1979 account of the conversation with Shimura. A decade later, Weil gave an account of previous work on the subject, and he translated into French the answer he gave to Shimura when Shimura expressed the conjecture to him. I reproduce here these historical comments from *Weil's Collected Papers* Vol. III (1979), p. 450.

> …D'autre part, Eichler en 1954, puis Shimura en 1958 dans des cas plus généraux, avaient déterminé les fonctions zêta de courbes définies par des sous-groupes de congruence du groupe modulaire; la célèbre courbe de Fricke définie par le group $\Gamma_0(11)$ (cf. [1971a], pp. 143–144) en était un exemple typique.
>
> Dans les cas traités par Eichler et par Shimura, on savait d'avance que la fonction zêta de la courbe est transformée de Mellin d'une forme modulaire. Déja en 1955, au colloque de Tokyo-Nikko, Taniyama avait proposé de montrer que la fonction zêta de toute courbe elliptique définie sur un corps de nombres algébriques est la transformée de Mellin d'une forme automorphe d'un type approprié; c'est le contenu du problème 12 de la collection de problèmes déja cité (v. [1959a]*). Quelques années plus tard, à Princeton, Shimura me demanda si je trouvais plausible que toute courbe elliptique sur **Q** fût contenue dans la jacobienne d'une courbe définie par un sous-groupe de congruence du groupe modulaire; je lui répondis, il me semble, que je n'y voyais pas d'empêchement, puisque l'un et l'autre ensemble est dénombrable, mais que je ne voyais rien non plus qui parlât en faveur de cette hypothèse.

When I first read Weil's answer about "one and the other set being denumerable", I characterized it as "stupid". I have since also called it inane. But actually, Weil's answer gives further evidence that he did not think of the conjecture himself. Indeed, as a result of his conversations with Serre and Weil, Shimura was directly responsible for changing the prevailing psychology about elliptic curves over **Q**. Weil's account of the conversation with Shimura in his collected papers as quoted above confirms in the published record Shimura's report of the conversation.

Thus a major source of confusion and contradictions in the way the conjecture has been reported for three decades lies in the fact that the above historical comments were not made in [We 1967a], let alone in the introduction to that paper, but were made only in 1979 in Weil's collected papers.

§ 3. The Seventies: Weil Inveighs against Conjectures

As a result of a wider distribution of Taniyama's problems in the early seventies, the terminology of "Weil curves" shifted to "modular curves", and the conjecture that all elliptic curves over the rationals are modular became the "Taniyama-Weil conjecture" rather than the "Weil conjecture". Throughout the sixties and seventies, there was still incomplete knowledge of Shimura's role, partly because he himself did not have anything in print about the conjecture. For Shimura's explanation, see § 4 below. However, when others brought out the role of Taniyama and Shimura, Weil started inveighing against conjectures in general, especially in two instances in 1974 and 1979. He did it first in his "Two lectures on number theory, past and present" [We 74], when he wrote:

> For instance, the so-called "Mordell conjecture" [*why "so-called"? S.L.*] on Diophantine equations

> says that a curve of genus at least two with rational coefficients has at most finitely many rational points. It would be nice if this were so, and I would rather bet for it than against it. But it is no more than wishful thinking because there is not a shred of evidence for it, and also none against it.

Weil picked up the same theme against conjectures in 1979 comments about [We 1967a], *Collected Papers* Vol. III, p. 453, when he wrote specifically about the way he avoided mentioning conjectures when he lectured about the results of his 1967 paper [We 1967a]:

> Néanmoins, dans l'exposé que je fis de mes résultats à Munich en Juin 1965, à Berkeley en Février 1966, puis dans [1967a], j'évitais de parler de "conjectures". Ceci me donne l'occasion de dire mon sentiment sur ce mot dont on a tant usé et abusé…

Weil went on, mentioning first Burnside's conjecture, which turned out to be false, and then inveighing against Mordell's conjecture (as documented further below). Thus Weil had a quite different attitude from the one he had when he brought Hasse's conjecture to the attention of the mathematical community in 1950. In writing this way, Weil conveniently (consciously or not) set a self-justifying stage for avoiding to mention the Shimura-Taniyama conjecture as such. About Weil being against the idea of making conjectures as on page 454 of his collected works Vol. III, Shimura wrote in his letter to Shahidi: "For this reason, I think, he avoided to say in a straightforward way that I stated the conjecture."

The extent to which Weil went out on a limb in 1979, going one better than in 1974, is shown by the following passage:

> Nous sommes moins avancés à l'égard de la "conjecture de Mordell". Il s'agit la d'une question qu'un arithméticien ne peut guère manquer de se poser; on n'aperçoit d'ailleurs aucun motif sérieux de parler pour ou contre.

First, concerning Weil's statement that "Mordell's conjecture" is 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. Indeed, in his thesis [We 28] (see also *Collected Papers* Vol. I, p. 45) Weil wrote quite differently (my translation):

> This conjecture, already stated by Mordell (loc. cit. note [4]) seems to be confirmed in some measure by an important result recently proved, and which I am happy to be able to cite here thanks to the kind permission of its author: "On every curve of genus $p > 0$, and for any number field k of rationality, there can be only a finite number of points whose coordinates are integers of k."

The above important result is of course Siegel's theorem on the finiteness of integral points. Weil made a similar evaluation in [We 36] (*Collected Papers* Vol. I, p. 126) without reference to Mordell:

> 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.*
> This seems extremely plausible, but undoubtedly we are still far from a proof. Perhaps one will have to apply here the method of infinite descent directly to the curve itself rather than to associated algebraic varieties. But, first of all, it will be necessary to extend the theory of abelian functions to nonabelian extensions of fields of algebraic functions. As I hope to show, such an extension is indeed possible. In any case, we face here a series of important and difficult problems, whose solution will perhaps require the efforts of more than one generation.

In addition, in 1979 Weil made comments on page 525 of Vol. I of his collected works concerning his papers [1927c] and [1928], the latter being his thesis [We 28], and he makes similar comments on pages 528–529 about [1932c], showing clearly the influence Mordell had in making the conjecture.[3]

Second, the statements in 1974 and 1979 that there is no "shred of evidence" or "motif sérieux" for Mordell's conjecture went not only against Weil's own evaluations in earlier decades (1928, 1936), but they were made after Manin proved the function field analogue in 1963 [Man 63]; after Grauert gave his other proof in 1965 [Gr 65]; after Parshin gave his other proof in 1968 [Par 68], 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; and within four years

[3] I quote from these pages:

> **p. 525**: Mon ambition avait été de prouver aussi que, sur une courbe de genre > 1, les points rationnels sont en nombre fini; c'est la "conjecture de Mordell". Je le dis à Hadamard. "Travaillez-y encore," me dit-il; "vous vous devez de ne pas publier un demi-résultat." Apres quelques nouvelles tentatives, je décidai de ne pas suivre son conseil.
>
> **pp. 528–529**: Je n'avais pas encore renoncé à démontrer un jour la "conjecture de Mordell"; je ne désespérais même pas de pouvoir me rapprocher de ce but (lointain encore aujourd'hui) par une analyse attentive et un approfondissement des moyens mis in oeuvre dans ma thèse…
>
> Mon espoir secret, bien entendu, était qu'il permettrait d'avancer en direction de la conjecture de Mordell; il n'en a pas été ainsi, que je sache, jusqu'à présent.

of Falting's proof of Mordell's conjecture. In addition to that, some mathematicians thought there was experimental evidence for Mordell's conjecture, as when Parshin wrote in [Pa 68]: "Finally when $g > 1$ numerous examples provide a basis for Mordell's conjecture…" Thus, as I stated in a letter dated 7 December 1985 (reproduced in the Taniyama-Shimura file), when Weil wrote that "one sees no reason to be for or against" Mordell's conjecture, all he showed was that he was completely out of it in 1979.

§ 4. The Spread of Improper Attributions

Weil's 1967 paper received considerable attention. At the time, Shimura's book including Theorems 7.14 and 7.15 was not yet available. Weil's formulation went beyond Shimura's in that when the differential of first kind on the elliptic curve corresponds to a modular form of level N, Weil made explicit that this level is also the conductor of the elliptic curve, namely, a certain a priori definable integer divisible precisely by the primes dividing the discriminant, but usually to much smaller powers. This connection with the conductor suggested explicit computations to those working in the field, and these computations (in addition to the more structural evidence provided by the conjectured functional equation) in turn led them to believe the conjecture. Modular elliptic curves over **Q** were then called "Weil curves". The idea that all elliptic curves over **Q** are modular was generally attributed to him, for example when Tate referred to it as "Weil's astounding idea" in [Ta 74]. I myself for a decade used the terminology "Weil curve" and "Taniyama-Weil conjecture", before I learned more information.

For instance, Barry Mazur told me in 1986 that he heard Weil give credit to Shimura verbally (offhand) in a colloquium talk in the early sixties. However, in 1986, when I discussed these matters with Serre (publicly, in front of others at a party in Berkeley), he claimed that Weil had reported a conversation with Shimura as follows:

> Weil: Why did Taniyama think that all elliptic curves are modular?
>
> Shimura: You told him that, and you have forgotten.

I am reporting here the gist of the exchange. Weil may have asked, "Why did Taniyama make the conjecture?" I immediately wrote to both Shimura and Weil to ask them whether such a conversation took place and to verify what Serre had attributed to them. In his letter to me of 13 August 1986, already cited in § 1, Shimura answered categorically:

> Such a conversation never took place…
>
> 1. It doesn't fit, or rather contradicts, the actual conversation between Weil and myself: "Did you make the statement that every Q-rational elliptic curve is modular?" "Yes, don't you think it plausible?" etc.
>
> 2. It would have been stupid of Weil to have asked why Taniyama made the conjecture. Once the statement is given, it makes sense, and it shouldn't have occurred to Weil to pose such a question. Indeed, Serre never asked the reason behind my statement.
>
> …
>
> 4. Knowing the above passage and Taniyama's problem, and having stated the conjecture in my own way, I couldn't and wouldn't have attributed the origin of the conjecture to Weil. Besides, there is one point which almost all people seem to have forgotten. In his paper [1967a], Weil views the statement problematic. In other words, he was not completely for it, and so he didn't have to attribute it to me. Thus there is nothing for which you can take him to task. Anyway, for these reasons I have consistently and consciously avoided speaking of the Weil conjecture. For example, in my Nagoya paper (vol. 43, 1971), I proved that the conjecture was true for every elliptic curve with complex multiplication. Other authors treating the same problem would very naturally have mentioned Weil. But I didn't. Also I always thought the reader of my book (*Introduction to the Arithmetic Theory* [of automorphic functions]) would wonder why the conjecture was not mentioned. The fact is, I was unable, or rather did not try very hard, to find a presentation of the topic in a way agreeable to everybody, including myself.

I then wrote both to Serre and Weil to ask them to comment on Shimura's reply. Of course I also sent them the Taniyama-Shimura file as it developed. Serre wrote me back two letters. The first dated 16 August 1986 criticized my attempt to verify what he told me, and thus gave rise to a side exchange. (Among other things, in this exchange, I asked Serre to correct his false reporting of the conversation between Shimura and Weil and to stop spreading false stories.) Serre's second letter to me dated 11 September 1986 stated briefly, in full, "Merci pour tes lettres, ainsi que la copie de celle de Shimura. Je les ai trouvées très instructives."

§ 5. Weil's Letter

Somewhat later on 3 December 1986, Weil wrote me back a longer letter which also contained comments concerning other items in the file, such as those mentioned in § 2 and § 3 above.

> Dear Lang,
>
> I do not recall when and where your letter of August 9 first reached me. When it did, I had (and still have) far more serious matters to think about.
>
> I cannot but resent strongly any suggestion that I ever sought to diminish the credit due to

Taniyama and to Shimura. I am glad to see that you admire them. So do I.

Reports of conversations held long ago are open to misunderstandings. You choose to regard them as "history"; they are not. At best they are anecdotes. 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.

As to attaching names to concepts, theorems or (?) conjectures, I have often said: (a) that, when a proper name gets attached to (say) a concept, this should never be taken as a sign that the author in question had anything to do with the concept; more often than not, the opposite is true. Pythagoras had nothing to do with "his" theorem, nor Fuchs with the fonctions fuchsiennes, any more than Auguste Comte with rue Auguste-Comte; (b) proper names tend, quite properly, to get replaced by more appropriate ones; the Leray-Koszul sequence is now a spectral sequence (and, as Siegel once told Erdös, abelian is now written with a small a).

Why shouldn't I have made "stupid" remarks sometimes, as you are pleased to say? But indeed, I was "out of it" in 1979 when expressing some skepticism about Mordell's conjecture, since at that time I was totally ignorant of the work of the Russians (Parshin, etc.) in that direction. My excuse, if it is one, is that I had had long conversations with Shafarevich in 1972, and he never mentioned any of that work.

Sincerely,

A. Weil

AW:ig

P.S. Should you wish to run this letter through your Xerox machine, do feel free to do so. I wonder what the Xerox Co. would do without you and the like of you.

Da capo. I have no explanation why Serre's Bourbaki Seminar talk took no account of Serre's past letter where he found Shimura's and my letters "instructive", of Weil's own historical comments in his *Collected Papers* Vol. III, p. 450, or of Weil's warning about "when a proper name gets attached to (say) a concept," as well as Weil's clear-cut statement: "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."

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