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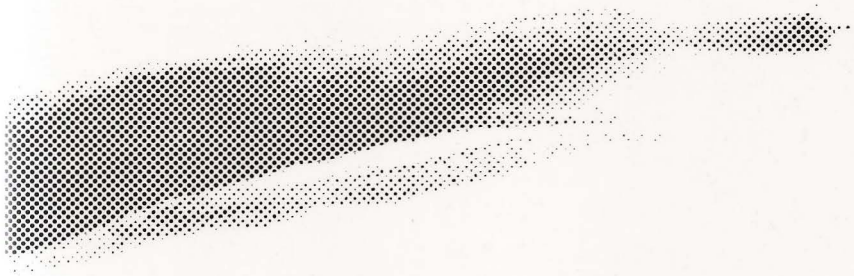

MATÉRIAUX POUR L'HISTOIRE DES MATHÉMATIQUES AU XX^{e} SIÈCLE

Actes du colloque à la mémoire de Jean Dieudonné (Nice, 1996)

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SÉMINAIRES ET CONGRÈS 3

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SOCIÉTÉ MATHÉMATIQUE DE FRANCE 1998

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On the History of Hilbert's Twelfth Problem

A Comedy of Errors

Norbert Schappacher*

Abstract

Hilbert's 12th problem conjectures that one might be able to generate all Abelian extensions of a given algebraic number field in a way that would generalize the so-called theorem of Kronecker and Weber (all Abelian extensions of $\mathbb{Q}$ can be generated by roots of unity) and the extensions of imaginary quadratic fields (which may be generated from values of modular and elliptic functions related to elliptic curves with complex multiplication). The first part of the lecture is devoted to the false conjecture that Hilbert made for imaginary quadratic fields. This is discussed both from a historical point of view (in that Hilbert's authority prevented this error from being corrected for 14 years) and in mathematical terms, analyzing the algebro-geometric interpretations of the different statements and their respesctive traditions. After this, higher-dimensional analogues are discussed. Recent developments in this field (motives, etc., also Heegner points) are mentioned at the end.

Résumé

Le douzième problème de Hilbert propose une façon conjecturale d'engendrer les extensions abéliennes d'un corps de nombres, en généralisant le théorème dit de Kronecker et Weber (toutes les extensions abéliennes de $\mathbb{Q}$ sont engendrées par des racines de

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> l'unité), ainsi que les extensions des corps quadratiques imaginaires (qui sont engendrées par des valeurs de fonctions modulaires et elliptiques liées aux courbes elliptiques à multiplication complexe). La première partie de l'exposé est centrée autour de la conjecture incorrecte de Hilbert dans le cas du corps quadratique imaginaire. Elle est discutée aussi bien du point de vue historique (pendant quatorze ans, l'autorité de Hilbert empêcha la découverte de cette erreur), que du point de vue mathématique, en analysant les interprétations algébro-géométriques des énoncés différents relatifs à ce cas et de leurs traditions. On discute ensuite des analogues en dimension supérieure. Les développements récents (motifs, etc., ainsi que les points de Heegner) sont mentionnés à la fin.

A good problem should be

- well motivated by already established theories or results,
- challenging by its scope and difficulty,
- sufficiently open or vague, to be able to fuel creative research for a long time to come, maybe for a whole century.

David Hilbert tried to follow these precepts in his celebrated lecture *Mathematische Probleme* at the Paris International Congress of Mathematicians in 1900.[1] He did not have time to actually present in his speech all 23 problems which appear in the published texts.[2] In particular, the 12th problem on the generalization of the Kronecker-Weber Theorem by the theory of Complex Multiplication did not make it into the talk. This may be due to the slight technicality of the statements involved. But Hilbert held this 12th problem in very high esteem. In fact, according to Olga Taussky's recollection, when he introduced Fueter's lecture "Idealtheorie und Funktionentheorie" at the 1932 International Congress at Zürich, Hilbert said that "the theory of complex multiplication (of elliptic modular functions) which forms a powerful link between number theory and analysis, is not only the most beautiful part of mathematics but also of all science."[3]

[1] [*ICM* 1900, pp. 58-114] (French translation by L. Laugel of an original German version), [Hilbert 1901] (definite German text), cf. [Alexandrov 1979].

[2] [Reid 1970, p. 81f]. See also *Enseign. Math.*, 2 (1900), pp. 349-355.

[3] Obituary Notice for Hilbert in *Nature*, 152 (1943), p. 183. I am grateful to J. Milne for giving me this reference. In [*ICM* 1932, p. 37], one reads about Hilbert presiding over this first general talk of the Zürich congress: "Der Kongress ehrt ihn, indem die Anwesenden sich von ihren Sitzen erheben."

The present article covers in detail a period where a number of initial mistakes by most mathematicians working on the problem were finally straightened out. At the end of the 1920's the explicit class field theory of imaginary quadratic fields was established and understood essentially the way we still see it today. However, the higher dimensional theory of singular values of Hilbert modular forms remained obscure. Later developments are briefly indicated in the final section of the paper.

What I describe here in detail *is* a comedy for us who look back. It is genuinely amusing to see quite a distinguished list of mathematicians pepper their contributions to Hilbert's research programmme with mistakes of all sorts, thus delaying considerably the destruction of Hilbert's original conjecture which happened to be not quite right. The comedy is at the same time a lesson on how, also in mathematics, personal authority influences the way research progresses — or is slowed down. It concerns the condition of the small group of researchers who worked on Hilbert's 12th problem. The errors made are either careless slips or delusions brought about by wishful thinking which was apparently guided by Hilbert's claim. The authors were just not careful enough when they set up a formalism which they controlled quite well in principle (a weakness in the formalism may, however, be behind the big error in Weber's false proof of the "Kronecker–Weber Theorem" — see section 2 below). Meanwhile Hilbert was conspicuously absent from the scene after 1900.[4] This is also not atypical for the comedy where the characters are mostly left to themselves when it comes to sorting out their complicated situation:

> "— Say, is your tardy master now at hand? ...
>
> — Ay, Ay, he told his mind upon mine ear.
> Beshrew his hand, I scarce could understand it.
>
> — Spake he so doubtfully, thou couldst not feel his meaning?
>
> — Nay, he struck so plainly, I could too well feel his blows; and withal so doubtfully, that I could scarce understand them."
>
> (Shakespeare, *The comedy of errors*, II-1)

The history of complex multiplication has already received a certain attention in the literature — see in particular the well-researched book [Vlăduţ 1991]. Apart from newly introducing a few details into the story, my main difference

[4]Hilbert did intervene indirectly, as thesis advisor. As such he should have been better placed han anybody else to see, for example, that Takagi's thesis of 1901 produced extensions that provided counterexamples to Fueter's thesis of 1903... See section 3 below.

with existing publications is the emphasis that I put on Hilbert's peculiar perspective of his problem, which is not only very much different from our current viewpoint, but seems also to be the very reason which led him to the slightly wrong conjecture for imaginary-quadratic base fields in the first place.

As for the style of exposition, I try to blend a general text which carries the overall story, with some more mathematical passages that should be understandable to any reader who knows the theories involved in their modern presentation.

I take the opportunity to thank the organizers of the Colloquium in honour of Jean Dieudonné, *Matériaux pour l'histoire des mathématiques au XX^e siècle*, at Nice in January 1996, for inviting me to contribute a talk. I also thank all those heartily who reacted to earlier versions of this article and made helpful remarks, in particular Jean-Pierre Serre and David Rowe.

1. Hilbert's statement of the Twelfth Problem

Coming back to the features of a good problem stated at the beginning, let us look at the motivation which Hilbert chose for his 12th problem. He quoted two results.

First, a statement "going back to Kronecker," as Hilbert says, and which is known today as the "Theorem of Kronecker and Weber." It says that every Galois extension of $\mathbb{Q}$ with Abelian Galois group is contained in a suitable cyclotomic field, *i.e.*, a field obtained from $\mathbb{Q}$ by adjoining suitable roots of unity. This was indeed a theorem at the time of the Paris Congress—although not proved by the person Hilbert quoted ... We will briefly review the history of this result in section 2 below.

Second, passing to Abelian extensions of an imaginary quadratic field, Hilbert recalled the Theory of Complex Multiplication. As Hilbert puts it:

"Kronecker himself has made the assertion that the Abelian equations in the domain of an imaginary quadratic field are given by the transformation equations of the elliptic functions [*sic!*] with singular moduli so that, according to this, the elliptic function [*sic!*] takes on the role of the exponential function in the case considered before."[5] The slight incoherence of this sentence, which goes from certain "elliptic functions" (plural—as in Kro-

[5] "Kronecker selbst hat die Behauptung ausgesprochen, daß die Abelschen Gleichungen im Bereiche eines imaginären quadratischen Körpers durch die Transformationsgleichungen der elliptischen Funktionen mit singulären Moduln gegeben werden, so daß hiernach die elliptische Funktion die Rolle der Exponentialfunktion im vorigen Falle übernimmt." [Hilbert 1901, p. 311].

necker's[6] standard usage in this context) to "the elliptic function" (definite singular), is not a slip.[7] In fact, it gives the key to Hilbert's interpretation of Kronecker, and to his way of thinking of the 12th problem. What Hilbert actually means here becomes crystal clear in the final sentence on the 12th problem, because there he expands the singular "the elliptic function" into "the elliptic modular function."[8] So Hilbert was prepared, at least on this occasion, to use the term "elliptic function" also to refer to (elliptic) modular functions, *i.e.*, to (holomorphic, or meromorphic) functions $f : \mathcal{H} \longrightarrow \mathbb{C}$, where $\mathcal{H} = \{\tau \in \mathbb{C} \mid \Im(\tau) > 0\}$ denotes the complex upper half plane, such that

$$f(\frac{a\tau + b}{c\tau + d}) = f(\tau), \qquad \text{for all } \tau \in \mathcal{H}, \begin{pmatrix} a & b \\ c & d \end{pmatrix} \in \mathrm{SL}_2(\mathbb{Z}).$$

And Hilbert's definite singular, "the elliptic (modular) function," refers undoubtedly to the distinguished holomorphic modular function $j : \mathcal{H} \longrightarrow \mathbb{C}$ which extends to a meromorphic function $j : \mathcal{H} \cup \{i\infty\} \longrightarrow \mathbb{C}$ with a simple pole at $i\infty$, where it is given (up to possible renormalization by some rational factor, in the case of some authors) by the well-known Fourier development in $q = e^{2\pi i\tau}$:

$$j(q) = \frac{1}{q} + 744q + 196884q + 21493760q^2 + \ldots$$

See for instance [Weber 1891, § 41] who calls this function simply "die Invariante," and cf. [Fueter 1905, p. 197], a publication on this problem which arose from a thesis under Hilbert's guidance.

To be sure, this was and is not at all the standard usage of the term "elliptic function." Rather, following Jacobi—despite original criticism from Legendre who had used the term to denote what we call today elliptic integrals—it was customary as of the middle of the 19th century to call elliptic functions the functions that result from the inversion of elliptic integrals, *i.e.*, the (meromorphic) doubly periodic functions with respect to some lattice. If one takes the lattice to be of the form $\mathbb{Z} + \mathbb{Z}\tau$, for $\tau \in \mathcal{H}$, then a typical example of such an elliptic function is Weierstrass's well-known $\wp$-function

$$\wp(z, \tau) = \frac{1}{z^2} + \sum_{m,n \in \mathbb{Z}}{}' \Big(\frac{1}{(z - m\tau - n)^2} - \frac{1}{(m\tau + n)^2} \Big),$$

[6] For instance [Kronecker 1877, p. 70], [Kronecker 1880, p. 453]. Cf. section 4 below.

[7] Laugel missed this in his French translation of the text [*ICM* 1900, p. 88f], and thereby blurred the meaning of the sentence.

[8] " ... diejenigen Funktionen ..., die für einen beliebigen algebraischen Zahlkörper die entsprechende Rolle spielen, wie die Exponentialfunktion für den Körper der rationalen Zahlen und die elliptische Modulfunktion für den imaginären quadratischen Zahlkörper." [Hilbert 1901, § 313].

where the prime restricts the summation to pairs $(m,n) \neq (0,0)$.

Also Kronecker seems to have reserved the term "elliptic function" for these doubly periodic functions which depend on two parameters: the lattice (or the "modulus," in a terminology going back to Legendre) τ and a complex number z modulo the lattice. His frame of reference for the theory of these functions was Jacobi's formalism, not Weierstrass's, but since the translation back and forth between these two formalisms was routine by the end of the 19th century, we do not elaborate on this here.

However, when Kronecker speaks of "transformation equations of elliptic functions" —as he does in the very passage that Hilbert picked up—, this may be ambiguous in that the transformations affect in general both parameters. So as an extreme case these transformation equations might describe functions which no longer depend on the point variable z at all, and behave with respect to the lattice-variable like a modular function. As a matter of fact, in another key passage where Kronecker states his *Jugendtraum*, he mentions two different sorts of algebraic numbers to be used to generate the Abelian extensions of an imaginary quadratic field: the "singular moduli" of elliptic functions, and those values of elliptic functions with a "singular modulus" where the complex argument (*i.e.*, z, in our notation) is rationally related to the periods.[9]

Today, one calls "singular moduli" the values $j(\tau)$ for those $\tau \in \mathcal{H}$ which satisfy a (necessarily imaginary) quadratic equation over $\mathbb{Q}$. In Kronecker, "modulus" has to be understood as alluding to the quantity k or κ in Legendre's normal form of the elliptic integrals, or in Jacobi's formalism. Once the Weierstrass formalism is set up, $j(\tau)$ may be rationally expressed in k^2. Regardless of the formalism, the term 'singular modulus' always characterizes the cases with an imaginary quadratic ratio τ between the basic periods.

We will review in section 4 below the arguments about what Kronecker actually conjectured concerning the explicit generation of all Abelian extensions of an imaginary quadratic number field. For the time being, we continue to discuss Hilbert's presentation of his 12th problem.

A comparison between both cases that Hilbert chose as motivation brings out very clearly the picture he had in mind—and which he also attributed to Kronecker:

> *If the ground field is $\mathbb{Q}$, there is the analytic function $x \mapsto e^{\pi i x}$ which has the property that, if we substitute elements x of the*

[9] " ... Gleichungen ... , deren Wurzeln singuläre Moduln von elliptischen Functionen oder elliptische Functionen selbst sind, deren Moduln singulär und deren Argumente in rationalem Verhältnis zu den Perioden stehen." [Kronecker 1877, p. 70].

given field $\mathbb{Q}$ into it, the values $e^{\pi i x}$ generate all Abelian extensions of $\mathbb{Q}$.

If the ground field K is imaginary quadratic, then there is the analytic function $\tau \mapsto j(\tau)$ which has the property that, if we substitute elements τ of the given field K into it, the values $j(\tau)$ generate all Abelian extensions of K.

The first statement is the Kronecker-Weber theorem. The second statement is false. First of all, it is false for the trivial reason that roots of unity generate Abelian extensions of K which cannot in general be obtained from singular j-values. Since Hilbert's prose is not very formal, and since roots of unity were already brought into the game in the first step, to generate the Abelian extensions of $\mathbb{Q}$, we may naturally correct the second statement to mean that *all Abelian extensions of K can be generated by roots of unity and singular values $j(\tau), \tau \in K$.* This is how Hilbert's claim was understood by those who worked on the problem: Fueter, Weber, Hecke, Takagi, Hasse. But this statement is still wrong, as we know today: one does need other functions, for instance, suitable values $\wp(z,\tau)$, for $\tau \in K$ and rational z, to get all Abelian extensions of K.

We will discuss Hilbert's wrong conjecture and its influence on the work in the area in section 3 below. We will review the argument against Hilbert's historic claim (to the effect that Kronecker had had the same conjecture in mind) in section 4. For now, let us just try to understand the beautifully simple image that Hilbert is trying to convey to us—never mind that it is mathematically incorrect and probably also not what Kronecker conjectured. If what Hilbert claims were true, this would indicate a marvellous economy of nature, which provided just one function for all imaginary quadratic fields at once, giving all Abelian extensions by simply evaluating it at the elements of the base field in question.

Hilbert assumed that what he saw as Kronecker's conjecture would be proved without much trouble by a slight refinement of the already existing elements of class field theory.[10] It is with this optimistic picture in mind that he then formulated the general problem (cf. [Fueter 1905, p. 197]): Given a field K of finite degree over $\mathbb{Q}$, to find analytic functions whose values at suitable algebraic numbers generate all Abelian extensions of K. Here Hilbert had actually more up his sleeves than one can guess from the rather general

[10] "Der Beweis der Kroneckerschen Vermutung ist bisher noch nicht erbracht worden; doch glaube ich, daß derselbe auf Grund der von H. Weber entwickelten Theorie der komplexen Multiplikation unter Hinzuziehung der von mir aufgestellten rein arithmetischen Sätze über Klassenkörper ohne erhebliche Schwierigkeiten gelingen muß." [Hilbert 1901, p. 311f].

discussion of the analogies between function theory and algebraic number theory which he inserts into the text of the 12th problem. We will briefly discuss his research programme in section 5 below.

Even today, as we are approaching the centenary of Hilbert's lecture, we are still waiting to see these analytic functions and their special values in general. Meanwhile, it seems clear that generalizing the theory of complex multiplication is not going to do this job for us.

2. The "Theorem of Kronecker and Weber"

In [Kronecker 1853, p. 10] we read:

> " ... We obtain the remarkable result: 'that the root of every Abelian equation with integer coefficients can be represented as a rational function of roots of unity' ... "[11]

Thus Kronecker seems to claim that he has established the theorem which today goes by the name of Kronecker and Weber. But in fact, in 1853, his terminology of "Abelian equations" only referred to equations with cyclic Galois group. This is of course the crucial case of the theorem, and the reduction to it of the general case is indicated for instance in [Kronecker 1877, p. 69]. Another problem with the above quote is that in [Kronecker 1853, p. 8] he indicates that he has not been able to deal with the case of cyclic extensions of degree 2^ν, with ν at least 3.

Kronecker's contemporaries apparently did not think he had a valid proof of the result. Hilbert for instance, in [Hilbert 1896, p. 53], distinguishes between Kronecker who "stated" (*aufgestellt*) the theorem, and Weber who gave a "complete and general proof" of it. I happily go along with Olaf Neumann saying: "Nowadays it is hard to estimate to what extent Kronecker really could prove his theorem."[12] Still, it is conceivable that new light might be shed on this and other questions by a perusal of the handwritten notes of Kronecker's Berlin courses of which a remarkably rich collection, from between 1872 and 1891, is one of the historical treasures of the library of the Strasbourg Mathematical Institute.[13]

[11] " ... ergiebt nämlich das bemerkenswerthe ... Resultat: 'daß die Wurzel *jeder Abel*schen Gleichung mit ganzzahligen Coëffizienten als rationale Function von Wurzeln der Einheit dargestellt werden kann' ... "

[12] [Neumann 1981, p. 120]. Much of the present section owes to this careful article.

[13] There are 27 bound volumes of handwritten notes. They belonged to Kurt Hensel. After Hensel's death, in the Summer of 1942, several hundred items of his personal mathematical library were sold by his daughter-in-law to the (Nazi) Reichs-Universität Straßburg.

Kronecker was very pleased with the theorem.[14] He proudly emphasized [Kronecker 1856, p. 37] the novelty that it does not reduce certain algebraic numbers to others of smaller degree, but rather elucidates their nature by linking them with cyclotomy.

It is astonishing how comparatively little attention Heinrich Weber (5 March 1842,[15] 17 May 1913) and his work have received so far among historians of mathematics and among mathematicians.[16] He is remembered for having been the nineteenth century German mathematician who acccepted the greatest number of job offers from different universities. Thus he held positions at Heidelberg, Zürich, Königsberg, Berlin, Marburg, and Göttingen (chair of Gauss - Dirichlet - Riemann - Clebsch - Fuchs - Schwarz),... before he finally moved from there to Strasbourg in 1895. David Hilbert was Weber's successor in Göttingen; he had been Weber's student back in Königsberg, along with Hermann Minkowski.

Weber moved from mathematical physics to algebra and number theory. His achievements that are remembered include the following.

- The fundamental paper [Dedekind and Weber 1882] where the notion of point on an abstract algebraic curve is defined for the first time in history, thus taking a decisive step towards the creation of modern algebraic geometry. Looking up "H. Weber" in the index of [Bourbaki 1984] leads one only to numerous allusions to this one article.

- His *Lehrbuch der Algebra* in three volumes: [Weber 1894, 1896, 1908]. Suffice it to say here that this work marks the transition from the late 19th century treatment of algebra[17] to the "modern algebra" whose first full-fledged textbook treatment was going to be van der Waerden's well-known treatise of 1930–31.[18] The third volume [Weber 1908] would not

M. Kneser kindly found out the correspondence between Hasse and Marieluise Hensel concerning this transaction in NSUG, *Nachlaß* Hasse, 24, p. 3.

[14] See for instance [Kronecker 1877, p. 69], where he adds the comment: "Dieser Satz giebt, wie mir scheint, einen werthvollen Einblick in die Theorie der algebraischen Zahlen; denn er enthält einen ersten Fortschritt in Beziehung auf die naturgemässe Classification derselben, welcher über die bisher allein beachtete Zusammenfassung in Gattungen hinausführt."

[15] In [Voss 1914] the 5th of May is given as the day of birth. This mistake is repeated quite often in the literature.

[16] Published exceptions are [Frei 1989, 1995], cf. also [Katsuya 1994]. For Weber's administrative role in Strasbourg, see [Manegold 1970, p. 195ff] and [Craig 1984, pp. 141-145]. Cf. [Wollmershäuser 1981]. See also the preprint [Schappacher and Volkert 1998].

[17] As represented for instance by the famous book by Camille Jordan, *Traité des substitutions et des équations algébriques*, recently re-edited by Éditions Jacques Gabay, Paris, 1989. Weber's Algebra resembles Jordan's treatise in many respects.

[18] Recently re-edited as *Algebra*, Springer, 1993.

be called algebra today. It is in fact the second, thoroughly reworked edition of [Weber 1891], and contains a classical treatment of elliptic functions, especially their arithmetic theory, along with parts of algebraic number theory and class field theory, as well as a small chapter on differentials of curves in the higher rank case including Riemann-Roch.

- Generalizing slightly from a lecture of Dedekind's of 1856/57, Weber was the first to define our abstract notion of group in print: [Weber 1893]. This made it into the *Lehrbuch der Algebra*, see the beginning of [Weber 1896]. See also [Franci 1992, p. 263] for a few details and relevant references.

- Weber had a leading role in the edition of Riemann's Collected Papers which is particularly remarkable for making important parts of Riemann's *Nachlaß* available as well.

- Weber developed a notion of class field in [Weber 1897-98]; see also [Weber 1908, p. 164]. Cf. [Frei 1989], [Katsuya 1994, 1.3]. He emphasized the decomposition behaviour, as opposed to Hilbert's chief interest in the unramifiedness of the (Hilbert) class field. More precisely, we read in [Weber 1908, p. 164]: "Definition of the class field. The prime ideals $\mathfrak{p}_i$ of degree one in the principal class $\overline{A_1}$, and only these, are to split in the field $\mathfrak{K}(A)$ again into factors of degree 1."[19] This definition enables the argument (which follows our quote) that was to remain the essence of the "analytic part of class field theory" for almost half a century: the deduction of the inequality "$n \geq h$" from the analysis near $s = 1$ of partial zeta-functions of the ground field and the class field.[20]

Weber's numerous contributions to elementary mathematics (partly in joint work with Wellstein) are all but forgotten, and so are many of his widespread interests, which are however well reflected in the *Festschrift* for his 70th birthday.[21] Klein portrayed Weber as a particularly flexible mind.[22]

[19] "Definition des Klassenkörpers. Die Primideale $\mathfrak{p}_i$ ersten Grades der Hauptklasse $\overline{A_1}$, und nur diese, sollen im Körper $\mathfrak{K}(A)$ wieder in Primideale ersten Grades zerfallen."

[20] The terminology of "ray classes" etc., if not the corresponding concepts, seem to be due to Fueter; see [Fueter 1903, 1905]. Fueter appears to give insufficient credit to [Weber 1897-98]. Fueter's works are not mentioned in [Frei 1989].

[21] *Festschrift Heinrich Weber zu seinem siebzigsten Geburtstag am 5. März 1912 gewidmet von Feunden und Schülern, mit dem Bildnis von H. Weber in Heliogravüre und Figuren im Text*, Leipzig und Berlin: Teubner, 1912.

[22] "H. Weber ist 1842 in Heidelberg geboren, wo er auch seine Studien beginnt und bei Helmholtz und Kirchhoff hört. Von 1873–83 wirkt er in Königsberg, 1892–95 ist er Ordinar-

Given this somewhat eclectic appreciation of Weber's achievements today it is maybe not surprising that, in spite of some similar criticism by Frobenius of Weber's proof of the Kronecker-Weber theorem in [Weber 1909],[23] it seems to have gone unnoticed until 1979 that the 'proofs' of the Kronecker-Weber theorem proposed in [Weber 1886, 1896], and [Weber 1908] were also not valid, due to a basic miscalculation of the Galois action on certain complicated Lagrange resolvents at the very beginning of the argument.[24] For the details we refer to the concluding comments in [Neumann 1981, pp. 124–125]. So it was in fact Hilbert himself who gave the first valid proof of the result, in [Hilbert 1896]. Weber published his first correct proof at age 69, two years before his death, in [Weber 1911]. As Olaf Neumann suggests, it would be fitting to refer to the result as the theorem of Kronecker-Weber-Hilbert.

One may speculate [Neumann 1981, p. 124] that Weber was in fact misled by Kronecker's composition of Abelian equations. If so, this would provide a beginning of an explanation of this error within the historical context. Such an explanation seems desirable because otherwise it is all too uncanny to see the author of the *Lehrbuch der Algebra* deceiving himself at an essential place about the Galois action in the composite of two normal extensions.

Today it is common to deduce the theorem from the existence theorem of class field theory. But there are also a number of direct proofs in the literature: [Speiser 1919], [Čebotarev 1924], [Šafarevič 1951], [Zassenhaus 1968-69], [Greenberg 1974-75] and [Washington 1982, chap. 14].

---

ius in Göttingen; dann geht er nach Straßburg, wo er 1913 stirbt. Er ist eine schmiegsame und doch wieder energische Natur und besitzt eine wunderbare Fähigkeit, leicht in ihm zunächst fremde Auffassungen einzudringen, so z.B. in die Riemannsche Funktionentheorie und die Dedekindsche Zahlentheorie. Diese seine Anpassungsfähigkeit hat es ihm ermöglicht, auf fast allen Gebieten unserer Wissenschaft in den letzten Dezennien mitzuarbeiten und die umfassenden Lehrbücher, den Weber-Wellstein, den Riemann-Weber, die Algebra zu schaffen, die wir alle kennen und benutzt haben. Seiner Mitwirkung an der Herausgabe von Riemanns Werken 1876 wurde bereits gedacht; die zweite Auflage 1892 hat Weber allein besorgt." [Klein 1926, p. 275].

[23] See the excerpt [Frobenius 1911] from the letter of Frobenius to Weber, 19 June 1909, in NSUG 8° Cod. Ms. philos. 205, which corrects some flaws in Weber's preceding proof [Weber 1909], and suggests the simpler arguments for the following paper [Weber 1911]. Cf. the surrounding letters by Frobenius in NSUG, *loc. cit.*

[24] See for instance [Weber 1896, p. 209, formula (7)]. This formula is incorrect as soon as the radicals and the roots of unity entering into the resolvent form extensions of $\mathbb{Q}$ which are not linearly disjoint. Personally, I hit upon this problem when I proposed to Mlle A. Rauch a *mémoire de maîtrise* with a view to rewriting Weber's proof in modern notation.

3. Work on Hilbert's claim for imaginary quadratic fields

Around the turn of the century a number of Hilbert's students were involved in a research programme one of the centres of which was Hilbert's 12th problem. For the more arithmetic development of class field theory, one has to mention in particular Ph. Furtwängler and F. Bernstein—see the 1903 volume of the *Göttinger Nachrichten*. On what was then seen as the function theoretic side of the problem, there was O. Blumenthal, and later E. Hecke—see section 5 below. But it was the Swiss mathematician Rudolf Fueter who attacked the 12th problem head-on in [Fueter 1903, 1905], adopting the following philosophy which, one may assume, was inspired by Hilbert.

Suppose that, for a given number field K — say, Galois over $\mathbb{Q}$, as Fueter always assumes —, analytic functions have been constructed certain "singular" values of which generate a lot of Abelian extensions of K. We would then like to have a general class field theoretic method to prove that these values suffice to generate *all* Abelian extensions of K. The method proposed by Fueter comes down to the observation that we are done if we can show that all ray class fields are contained in what the special values give us. Indeed, it would follow from the *Hauptsatz* of chapter IV [Fueter 1905, p. 232] that every Abelian extension of K is contained in a suitable ray class field. The execution of this strategy in [Fueter 1905] is, however, invalidated in the case of Abelian extensions of even degree by a group theoretical mistake in the reduction steps of the first chapter [Fueter 1905, p. 207].[25]

Still, Fueter's strategy could have very well led to a timely *destruction* of Hilbert's overly optimistic claim. For the convenience of the reader, let us explain this in the classical ideal theoretic language of class field theory, say, like in [Hasse 1926a]. A comparison with [Fueter 1905], and in particular with [Weber 1908] shows that such a refutation of Hilbert's claim would have been well within the reach of these authors at the beginning of the century.[26]

Let K be an imaginary quadratic number field, and $\mathfrak{o}_K$ its ring of integers. The values $j(\tau)$, $\tau \in K \cap \mathcal{H}$, are precisely the j-invariants of lattices $\mathfrak{a} \subset \mathbb{C}$ such that the ring of multipliers of the lattice, $\mathfrak{o}_\mathfrak{a} := \{\alpha \in \mathbb{C} \mid \alpha\mathfrak{a} \subset \mathfrak{a}\}$, is an order in K, *i.e.*, is of the form $\mathfrak{o}_\mathfrak{a} = \mathfrak{o}_f = \mathbb{Z} + f \cdot \mathfrak{o}_K$, for some integer $f \geq 1$. Now, given such an order $\mathfrak{o}_f$, the extension $K_f = K(j(\mathfrak{a}))$ does not depend on the lattice $\mathfrak{a}$ such that $\mathfrak{o}_\mathfrak{a} = \mathfrak{o}_f$. In fact, all of these values $j(\mathfrak{a})$ are conjugate over K, and their number equals the class number of proper

[25] See [Fueter 1914, p. 177f, note **].

[26] A modern, extremely concise justification of the claims which we will use can be obtained from [Serre 1967].

$\mathfrak{o}_f$-ideals. The field thus obtained is an Abelian extension of K which Weber called *Ordnungskörper* (for the conductor f, which Weber calls Q), and which he recognized as the class field associated with the group of ideals prime to f, modulo principal ideals generated by elements $\alpha \in K^*$ satisfying

$$\alpha \equiv r \ (\mathrm{mod} f), \quad \gcd(\alpha, f) = 1$$

for some rational number r depending on α—see [Weber 1908, § 124]. Today this field is called the *ring class field* of K modulo f, a terminology going back to Hilbert.

Since roots of unity generate the ray class fields of $\mathbb{Q}$, the Abelian extension of K generated by K_f and by the f-th roots of unity corresponds to the group of principal ideals generated by elements

$$\alpha \equiv r \ (\mathrm{mod} f), \quad r^2 \equiv 1 \ (\mathrm{mod} f), \quad \gcd(\alpha, f) = 1$$

for some rational number r depending on α. These conditions do not in general imply that $\alpha \equiv \pm 1 \ (\mathrm{mod} f)$. But it is this latter condition that describes the ray class field of conductor f of K, because K being totally imaginary there is no real place to distinguish between the two units ± 1.[27] The essential gap between the two conditions is that one may have different signs at different prime divisors of f. Thus, if we call K' the union of the fields K_f, for all f, and K'' the union of all ray class fields of K, then $\mathrm{Gal}(K''/K')$ is an infinite product of groups of order 2. Therefore, even independently of the existence theorem of class field theory, which says that $K'' = K^{ab}$, the field K' proposed by Hilbert in his 12th problem is not big enough to contain all Abelian extensions of K.

On the 4th of July, 1903, Heinrich Weber wrote to his former student and friend David Hilbert to tell him that now, after the end of the teaching term, he felt free to embark again on some serious work, and asked him for information about works of Hilbert's students on Complex Multiplication. He explained that he had been out of touch with this theory for a while and had to start by learning the new developments. He mentioned that he had just received Fueter's thesis [Fueter 1903] which "looks very promising, judging from its title and the table of contents."[28]

[27] As Takagi points out nicely in [Takagi 1920, p. 103ff], the ray class fields of K are analogous to the maximal totally real subfields of the cyclotomic fields. He had himself overlooked this point in his work on extensions of $\mathbb{Q}(i)$, see [Takagi 1903, p. 28]; cf. footnote 34 below.

[28] NSUG, 8° Cod. Ms. philos. 205, sheets 39–40. Unfortunately the letters from Hilbert to Weber do not seem to have survived ...

What he did not mention in this letter was the work of his own student Daniel Bauer at Strasbourg who submitted his dissertation [Bauer 1903] that same year. There Bauer studies the following conjecture which Weber had made in a vague form—in agreement with Hilbert's conjecture, although Weber probably wrote this down before Hilbert's lecture at the Paris *ICM*—in his encyclopedia article [Weber 1900, end of §11, p. 731]. Let $\mathfrak{a} \subset \mathbb{C}$ be as above a lattice such that $\mathfrak{o}_\mathfrak{a}$ is an order of the imaginary quadratic field K. (Bauer's thesis excludes the cases where $K = \mathbb{Q}(\sqrt{-3}), \mathbb{Q}(\sqrt{-4})$, *i.e.*, where $\mathfrak{o}_K$ has extra units besides ± 1). Let $\mathfrak{m}$ be any $\mathfrak{o}_K$-ideal prime to the conductor of $\mathfrak{o}_\mathfrak{a}$. Define the $\mathfrak{m}$-th *Teilungskörper* $\mathfrak{T}_\mathfrak{m}$ to be the extension of $K(j(\mathfrak{a}))$ generated by the $\mathfrak{m}$-division points of Weber's τ-function associated to the lattice $\mathfrak{a}$. In the cases without extra units (the only ones that Bauer considers), this is just a weight zero variant of the Weierstrass $\wp$-function: up to a rational factor, $\tau(z)$ equals $\frac{g_2(\mathfrak{a})g_3(\mathfrak{a})}{\Delta(\mathfrak{a})}\wp(z;\mathfrak{a})$. Today we may say that $\mathfrak{T}_\mathfrak{m}$ is the field generated over $K(j(\mathfrak{a}))$ by the x-coordinates of the points annihilated by all elements of $\mathfrak{m}$, on a model defined over $K(j(\mathfrak{a}))$ of the elliptic curve $\mathbb{C}/\mathfrak{a}$. $\mathfrak{T}_\mathfrak{m}$ is certainly Abelian over $K(j(\mathfrak{a}))$. Weber suggests [*loc. cit.*] that these *Teilungskörper* are always contained in suitable composites of ring class fields of K and cyclotomic fields.

Bauer purports to prove that, if $\mathfrak{m} = p \cdot \mathfrak{o}_K$, for an odd prime number p, then the field generated over K by K_p and the p-th roots of unity coincides with $\mathfrak{T}_\mathfrak{m}$ [Bauer 1903, p. 4 and p. 32f]. This cannot be quite right in the case where p splits into the product of two prime ideals in $\mathfrak{o}_K$, because then we may choose, in the class field theoretic analysis of the fields in question, different signs at the prime divisors of p. I have not traced down Bauer's arguments. They are coached in terms of Jacobi's elliptic function sn rather than Weber's τ.

In the third volume of his *Lehrbuch der Algebra*, Weber [1908] discusses fields called *Teilungskörper* at various places, first in §154. There he considers the fields $\mathfrak{T}_\mathfrak{m}$ defined above, under the additional assumption that $\mathfrak{o}_\mathfrak{a} = \mathfrak{o}_K$, so that $K(j(\mathfrak{a}))$ is the Hilbert class field K_1 of K. Taking division values of the τ-function, rather than the field generated by both coordinates of the $\mathfrak{m}$-torsion points of an elliptic curve isomorphic to $\mathbb{C}/\mathfrak{o}_K$ defined over K_1, can be seen today to be the geometric analogue of the fact that we cannot distinguish between ± 1 in the ray condition. Note in passing that adjoining all the coordinates of torsion points does not in general give Abelian extensions of K.[29]

[29] This is related to a condition introduced by Shimura into the theory of Abelian varieties with complex multiplication. For the case of elliptic curves, see for instance [Schappacher 1982].

Hasse in his particularly tidy work [Hasse 1927] showed how to construct the ray class fields of K directly from these *Teilungskörper* $\mathfrak{T}_{\mathfrak{m}}$. Weber however, for technical reasons, was led, in the third part of [Weber 1897-98] as well as in [Weber 1908], to work with more complicated fields, replacing the τ-function by certain quotients of theta series. These fields he still calls *Teilungskörper*, and denotes them by the same symbol $\mathfrak{T}_{\mathfrak{m}}$ [Weber 1908, § 158, end]. As Hasse points out in [Hasse 1926a, p. 55], Weber even gets caught up in a confusion between the two sorts of fields in [Weber 1908, § 167, (5)]. Let us gloss over this additional problem here. Then Weber finally derives for his *Teilungskörper* $\mathfrak{T}_{\mathfrak{m}}$ in [Weber 1908, § 167] a class field theoretic description which in our language pins them down as the ray class fields of K, modulo given ideals $\mathfrak{m}$ of $\mathfrak{o}_K$.[30]

Then he sets out in [Weber 1908, § 169] to show that the ray class fields can be indeed generated over K by singular moduli and roots of unity. If $\mathfrak{m}$ is an ideal of $\mathfrak{o}_K$ dividing the rational integer f, Weber wants to conclude the congruence $\alpha \equiv \pm 1 \pmod{\mathfrak{m}}$ from the conditions $\alpha \equiv r \pmod{f}$, $r^2 \equiv 1 \pmod{f}$. Now, this is alright if $\mathfrak{m}$ is the power of a prime ideal of $\mathfrak{o}_K$ not dividing 2. But Weber thinks he can always reduce to this case without loss of generality. In fact, at the end of [Weber 1908, § 158], he had claimed that any *Teilungskörper* $\mathfrak{T}_{\mathfrak{m}}$ was the composite of various *Teilungskörper* $\mathfrak{T}_{\mathfrak{n}}$ with $\mathfrak{n}$ equal to powers of prime ideals. This were true if he had adjoined all the coordinates of torsion points, not just division values of particular functions. Translating back to the characterization by ray class groups, Weber overlooked precisely the possibility of choosing different signs in ± 1 modulo different prime factors of $\mathfrak{m}$.

This is how Weber missed his chance to disprove Hilbert's claim in the third volume of his *Lehrbuch der Algebra* [Weber 1908, § 169].[31]

As late as 1912 Erich Hecke, another thesis student of Hilbert's, assures us in the preface to his thesis [Hecke 1912] that Fueter has proved Hilbert's claim in [Fueter 1905, 1907]. He is careful to add, however, a footnote to the effect that Fueter will fill a few gaps in his proof in a book soon to be published. As a matter of fact, this book was to appear only in 1924, more than 20 years after Fueter had begun working on the problem under Hilbert's guidance (and then it was promptly mauled by Hasse in his merciless review [Hasse 1926b] ...). Ten years before the book, one year after Heinrich Weber's death, the general agreement on Hilbert's claim had finally come to an end in [Fueter 1914].

[30] For details see [Hasse 1926a, p. 43f]. Even though this is not at all recalled in the later sections of *Viertes Buch* of [Weber 1908], it seems that Weber actually restricts attention to ideals $\mathfrak{m}$ prime to 2 all along.

[31] One more incorrectness in this part of [Weber 1908] is mentioned in [Hasse 1926a, p. 55].

This long article shows a rather hapless Fueter. He now has a counterexample to Hilbert's claim: for $K = \mathbb{Q}(i)$, the field $K(\sqrt[4]{(1+2i)})$ cannot be generated by singular moduli and roots of unity. He has also understood the group theory mistake he had made in [Fueter 1903]. Furthermore, he guesses what the correct picture is going to be: the *Teilungskörper* will do the job, and in general they are strictly bigger than the fields considered by Hilbert. He formulates this as the *Hauptsatz* [Fueter 1914, p. 253] and claims it explicitly (*"Dagegen gilt der Hauptsatz ... "*). Then he talks about what one has to do to prove this. His problem is precisely the one that Hasse solved in [Hasse 1927]: to work with Weber's original definition of the Teilungskörper and see its relation to the ray class fields. Since he does not know how to do this, he explains that "the investigation necessitates a discussion of the function theoretic side of the problem. I have not yet executed these considerations, and they would have actually led too far astray. I will cover this problem in its full context in a Teubner textbook. But I do believe that I have made sufficient progress on the number theoretic side."[32]

It was Teiji Takagi who got there first. In the final chapter V of his momentous paper [Takagi 1920] — which he wrote up when the end of the War and the upcoming first postwar *ICM* (Strasbourg 1920) promised the renewal of contact with European colleagues [Iyanaga 1990, p. 360f] — the author does what Weber should have done in the third volume of his *Lehrbuch der Algebra*. In fact, Takagi follows Weber as closely as he can, working with the modified, more complicated *Teilungskörper*, but getting things right. To be sure, the crucial thing that Weber could not have done easily 15 years before Takagi is the proof of the fact that every Abelian extension of K is contained in a suitable ray class field. Takagi, in [Takagi 1920, p. 90, Satz 28], deduces this in complete generality as the key result of his tremendous development of general class field theory, which occupies the bulk of the article [Takagi 1920] and which in turn was made possible also by prior work of the Hilbert school, in particular Ph. Furtwängler. Cf. [Katsuya 1994, § 3].

Believing his own account [Iyanaga 1990, p. 360], one concludes that Takagi had "started his own serious investigations on class fields in 1914 when World War I began ... because he could not expect the flow of academic

[32] "Ist dagegen die Körperklassenzahl von 1 verschieden, so verlangt die Untersuchung ein Eingehen auf die funktionentheoretische Seite des Problems. Diese Betrachtungen habe ich noch nicht durchgeführt, sie würden auch zu weit abseits führen. Ich werde dieses Problem in einem Teubnerschen Lehrbuche im Zusammenhange darstellen. Doch glaube ich, daß die zahlentheoretische Seite durch meine Entwicklungen ausreichend gefördert ist." [Fueter 1914, p. 255].

books and journals from Germany anymore." [Katsuya 1994, p. 116] But at least in some ways Takagi's fine article of 1920 was the culmination of almost 20 years of work and calls for a flashback. In fact, Takagi had been, so to say, a 'member of the club' all along—yet remained an outsider at the same time. He had come to Germany in 1898 to study, first with Frobenius in Berlin, and as of Spring 1900 with Hilbert in Göttingen. It was Hilbert who supervised his thesis [Takagi 1903] which Takagi finished writing in the Spring of 1901 and submitted to the Imperial University of Tokyo.

Even if Takagi's anecdotal account diminishes Hilbert's direct guidance of the thesis [Iyanaga 1990, p. 357], the influence of the master is evident throughout the thesis: The short introduction, which the author (humbly?) calls "almost superfluous"[33], uses close reformulations of sentences from Hilbert's text on the twelfth problem. In particular, Takagi also states Kronecker's conjecture quoting the ambiguous "transformation equations of the elliptic functions with singular moduli." He does not elaborate at all on the meaning of this. What he does in his dissertation is actually quite different in spirit from Hilbert's version of Kronecker's conjecture, although inspired by another work of Hilbert's in the area:

Fixing the base field $K = \mathbb{Q}(i)$, Takagi shows that all Abelian extensions of K are contained in the extensions of K generated by division values of the lemniscatic elliptic function, *i.e.*, essentially of the Weierstrass $\wp$-function associated to the elliptic curve $y^2 = x^3 - x$. The method is to transfer Hilbert's proof of the Kronecker-Weber theorem [Hilbert 1896] to the lemniscatic case.[34]

So from his very first exposure to the problem Takagi was oriented towards division fields rather than general ring class fields. This orientation can be clearly traced through his subsequent publications on complex multiplication.[35] His decisive contribution [Takagi 1920] is therefore also the fruit

[33] "Diese fast überflüssigen Einleitungsworte schliesse ich mit dem Ausdruck herzlichsten Dankes an den Herrn Prof. Hilbert in Göttingen, dessen Anregung diese Erstlingsarbeit ihr Entstehen verdankt" [Takagi 1903, p. 13]. This sentence seems to contradict the above-mentioned anecdote according to which Takagi simply told Hilbert what he was working on and Hilbert accepted ... It is presumably because he did not get his doctorate in Göttingen that Takagi is missing from the "Verzeichnis der bei Hilbert angefertigten Dissertationen" in the third volume of Hilbert's *Gesammelte Abhandlungen*, 1970, pp. 431–433.

[34] Takagi himself points out in [Takagi 1920, p. 145, footnote 3] a mistake in [Takagi 1903, p. 28]. Cf. our footnote 27 above. Another mistake, concerning [Takagi 1903, p. 29, Hülfssatz 1], is noted and briefly discussed by Iwasawa in [1990, p. 343, footnote 2]. Note that the lemniscatic analogue of the Kronecker-Weber theorem is already claimed, at least vaguely, in [Kronecker 1853, p. 11]. The article [Masahito 1994] (which is not always easy to follow, but certainly insists on the importance of the lemniscatic case for the prehistory of complex multiplication in the 19th century) does not mention Takagi's thesis.

[35] See Nos 7, 9, and 10 of Teiji Takagi [*Papers*, pp. 342–351].

of a line of thought independent of the main intention of Hilbert's twelfth problem, yet still suggested by Hilbert, in the very special and concrete case of lemniscatomy.

4. "Kronecker's Jugendtraum"

Kronecker's letter to Dedekind dated 15 March 1880 begins:

> "Thank you very much for your kind lines of the 12th. I believe they are to give me a welcome occasion to let you know that I believe to have overcome today the last of many difficulties that were still withstanding the completion of an investigation which I had taken up again more intensely in the last few months. It concerns the dearest dream of my youth, to wit, the proof that the Abelian equations with square roots of rational numbers are exhausted by the transformation equations of elliptic functions with singular moduli exactly in the same way as the rational integral Abelian equations by the cyclotomic equations."[36]

In section 1 above we have discussed the possible ambiguity of these "transformation equations of elliptic functions with singular moduli." We quoted a passage from [Kronecker 1877, p. 70] (footnote 9 above), where Kronecker mentions in a row "equations the roots of which are singular modules of elliptic functions or elliptic functions themselves the modules of which are singular and the arguments of which have a rational ratio with the periods." In that same passage Kronecker goes on to conjecture that all equations Abelian over quadratic fields "are exhausted by those which come from the theory of elliptic functions."

Mentioning both kinds of functions and special values at the same time makes good sense for many reasons. Helmut Hasse, in his painstaking discussion of what Kronecker's "Jugendtraum" really consisted in, noted that the orientation of Kronecker's research in this area actually moved from singular

[36] "Meinen besten Dank für Ihre freundlichen Zeilen vom 12.c.! Ich glaube darin einen willkommenen Anlass finden zu sollen, Ihnen mitzutheilen, dass ich heute die letzte von vielen Schwierigkeiten besiegt zu haben glaube, die dem Abschlusse einer Untersuchung, mit der ich mich in den letzten Monaten wieder eingehender beschäftigt habe, noch entgegenstanden. Es handelt sich um meinen liebsten Jugendtraum, nämlich um den Nachweis, dass die *Abel*'schen Gleichungen mit Quadratwurzeln rationaler Zahlen durch die Transformations-Gleichungen elliptischer Functionen mit singulären Moduln grade so erschöpft werden, wie die ganzzahligen *Abel*'schen Gleichungen durch die Kreistheilungsgleichungen." [Kronecker 1880, p. 453].

moduli to division values [Hasse 1930, p. 514] — which is another major argument to show that Hilbert's interpretation of the "Jugendtraum" was not that intended by Kronecker.

A mathematical reason for coupling both kinds of functions, which is very close to the way we view things today, is that division values make (geometric) sense only over a field of definition of the corresponding (geometric) object, which in the case at hand is the field generated by the corresponding singular modulus. It seems hard to decide how much of this "geometric" perspective may have been present already in Kronecker or Weber.[37] It yields an understanding of the analogy between the Kronecker-Weber theorem and the Jugendtraum which is completely different from Hilbert's point of view in his 12th problem. See section 6 below.

Hasse [1930] wrote his thorough philological analysis as a kind of penitence. For he had never cared before to check Hilbert's historical claim (repeated in particular by Fueter, see for instance [Fueter 1905]) that Kronecker's "Jugendtraum" was precisely what Hilbert expected: the generation of all Abelian extensions of an imaginary-quadratic field by singular moduli and roots of unity—this is what is called interpretation (a) of the Jugendtraum in [Hasse 1930]. Thus in [Hasse 1926a, p. 41], he had still written that "Kronecker's conjecture ... turns out to be only partially correct." Now, in [Hasse 1930, p. 515], he went so far as to conclude that "if Kronecker had any precise formulation of his Jugendtraum-theorem in mind at all, then it can only be" what is called interpretation (b) in [Hasse 1930], *i.e.*, the generation of all Abelian extensions of an imaginary-quadratic field by singular moduli and division values.

I find little to add to Hasse's study of this historical issue, if one accepts the question the way he poses it. In particular, Hasse shows convincingly by quoting from other places in Kronecker why the term 'transformation equations' appearing in the Jugendtraum quote in [Kronecker 1880] introduces an ambiguity of meaning, and he argues carefully to show that Kronecker was indeed envisaging to use both kinds of algebraic quantities to generate all Abelian extensions of imaginary-quadratic fields: singular moduli as well as division values of corresponding elliptic functions.

On the other hand, it seems only fair to say that a casual reading of [Kronecker 1880], especially from the middle of page 456 on where Kronecker mentions only "singular moduli" explicitly, can easily create the impression that Kronecker did want to do without the division values, which would amount to Hilbert's claim. Adding to this Hilbert's optimistic conviction that this

[37] For the same reason we do not think that Vlăduţ's remark [1991, p. 79, last paragraph] concerning interpretation (c), of the Jugendtraum in [Hasse 1930] is historically sensible.

claim was correct, and fit into a beautiful general picture, Hilbert's double error—mathematical and historical—reduces to a minor slip. What we have shown is how long this double slip could survive, carried as it were by Hilbert's tremendous authority.

But when we look at this story, we have to be careful not to forget how differently we are programmed today in these matters: For us, moduli tend to be points on a moduli scheme and thus represent algebro-geometric objects as such, whereas division values suggest Galois representations, which will be Abelian in the presence of complex multiplication—see section 6 below. Such a conceptual separation of the two kinds of singular values that Kronecker brought into play did not exist at the turn of the century. For instance, the chapter "Multiplication und Theilung der elliptischen Funktionen" in [Weber 1891] culminates in a §68 about "Reduction of the division equation to transformation equations." And Kronecker himself once stated this continuity very forcefully that he saw between the two notions in the case of complex multiplication.[38]

5. Hilbert Modular Forms

In the introduction to Otto Blumenthal's *Habilitationsschrift* [Blumenthal 1903b] (submitted at Göttingen in 1901) we read: "In the years 1893–94 Herr Hilbert investigated a way to generalize modular functions to several independent variables. ... Herr Hilbert has most kindly given me these notes for elaboration."[39] I do not know whether Hilbert's original notes on what was to become the theory of *Hilbert Modular Forms* still exist.

Blumenthal was the first student to whom Hilbert gave an aspect of this research programme. He was to develop the analytic theory, relative to an arbitrary totally real field—see [Blumenthal 1903a,b, 1904a,b,c]. Today it is part of the folklore of this subject[40] that Blumenthal's works contain in particular the mistake that he thinks he needs only one cusp to compactify the fundamental domain for the full Hilbert modular group, whereas h are

[38] "Während für die Kreisfunctionen nur Multiplication, für die allgemeinen elliptischen Functionen aber Multiplication und Transformation stattfindet, verliert die Transformation bei jener besonderen Gattung elliptischer Functionen [*sc.* für welche complexe Multiplication stattfindet] zum Theil ihren eigenthümlichen Charakter und wird selbst eine Art von Multiplication, indem sie gewissermaaßen die Multiplication mit idealen Zahlen darstellt ... "[Kronecker 1857, p. 181].

[39] "In den Jahren 1893–94 beschäftigte sich Herr *Hilbert* mit einer *Verallgemeinerung der Modulfunktionen* auf mehrere unabhängige Variable. ... ... Herr Hilbert hat mir diese Notizen zur Ausarbeitung freundlichst überlassen."

[40] Cf. Schoeneberg's notes to [Hecke 1912] in his edition of Hecke's *Mathematische Werke.*

needed (h the class number of the field in question). This error was passed on to the second student that Hilbert sent into this field, Erich Hecke. He was to explore the application of Hilbert modular forms to the 12th problem in the case of a real quadratic field in his thesis [Hecke 1912]. Exploiting a relation with theta functions which was found by Hilbert,[41] Hecke has at his disposal a Hilbert modular function analogous to the j-function of the elliptic case (but not holomorphic in the fundamental domain), and he wants to generate interesting Abelian extensions of a totally imaginary quadratic extension of the given real quadratic field by suitable special ('singular') values of this Hilbert modular function. He does obtain a statement in this direction in his dissertation [Hecke 1912, p. 57], but the result is far from satisfactory, as Hecke is the first to point out.

In his *Habilitationsschrift* [Hecke 1913], he then tries to go further by taking a Hilbert modular function which is regular everywhere in the fundamental domain. Since such a function has to be constant, this work is strictly speaking empty. To get some impression of what Hecke does manage to understand in spite of his impossible function, one may take a modern point of view, and say that he is developing part of the theory of Abelian surfaces with complex multiplication. In this language, one of the surprising features of the theory that Hecke discovers is the fact that CM-field and reflex field are in general different—see for instance [Hecke 1913, p. 70].

It is with reference to this that André Weil speaks of Hecke's *audace stupéfiante* to tackle a theory for which the time was clearly not yet ripe [Weil *Œuvres* II, art. 1955 c, d]. This critical compliment should be transferred at least partly to Hilbert who had become convinced, with his tremendous mathematical optimism, of the sweeping perspective which he wrote into his 12th problem.

6. Outlook on later developments, and another historical tradition

The focus of this paper was on the "comedy of errors" which arose from Hilbert's formulation of *Kronecker's Jugendtraum*. This story may leave the

[41] "Die interessanteste Analogie mit den Modulfunktionen aber bezieht sich auf den Zusammenhang der neuen Funktionen mit dem *Transformationsproblem der ϑ-Funktionen* mehrerer Veränderlicher. Herr Hilbert zeigt hier, daß seine Funktionen bei diesem Problem eine ganz ähnliche Rolle spielen, wie die Modulfunktionen in Bezug auf die elliptischen Funktionen. Er leitet insbesondere eine Formel ab, aus der sich schließen läßt, daß man zu Funktionen des Fundamentalbereichs gelangen kann, indem man Quotienten von Theta-Nullwerten bildet." [Blumenthal 1903b, p. 510]; see also [Blumenthal 1904b].

somewhat stale aftertaste of being a series of unnecessary mistakes bearing no serious relation with the mathematical substance involved. Considering more recent developments around Hilbert's 12th problem reveals quite a different aspect. Roughly from the end of the twenties or the beginning thirties on, the point of view of Arithmetic Algebraic Geometry began to set in and dominate more and more the domain of complex multiplication.

Arithmetic Algebraic Geometry was explicitly initiated by Poincaré in his seminal research programme [Poincaré 1901] on the arithmetic of algebraic curves. Still, its connection with the theory of complex multiplication had to wait for about half a century, until several background theories had reached the necessary maturity. In particular, the reduction of elliptic curves modulo primes, the L-function of a curve over a finite field (*Kongruenzfunktionenkörper*, in the German school), the global L-function of a curve over a number field, ... — all these notions that began to crystallize in the twenties and thirties, finally come together in the beginning fifties to shape what is still today our basic understanding of the arithmetic theory of Complex Multiplication.

So talking about Hilbert's 12th problem from this point of view is similar to Bourbaki's approach to history in his *Eléments d'histoire des mathématiques*: we place ourselves in today's mathematical context and try to recognize what we know, in documents which cannot be said to really possess this knowledge. Thus the Kronecker-Weber theorem, looked at from the point of view of arithmetic algebraic geometry, provides an example of the generation of Abelian extensions of a field of definition K from one-dimensional ℓ-adic representations of some group variety defined over K (or, more generally, of a motive of rank 1). More precisely, the Abelian extensions of $\mathbb{Q}$ are generated by the torsion points of the multiplicative group $\mathbf{G}_m$ over $\mathbb{Q}$. Similarly, departing from Hilbert's narrow (and probably incorrect) interpretation of *Kronecker's Jugendtraum*, the coordinates of the torsion points of an elliptic curve with complex multiplication by K, which is defined over the Hilbert class field K_1 of K, do suffice to generate (over K_1) all Abelian extensions of K.

In this perspective, the plethora of singular j-values which Hilbert proposed are really uncalled for. They have no analogue at all in the Kronecker-Weber theorem because $\mathbf{G}_m$ is already defined over $\mathbb{Q}$, and they should, seen from this new vantage point, enter into the theory only as generators of fields of definition for the given objects of arithmetic algebraic geometry, *i.e.*, for a given elliptic curve with complex multiplication.

This analysis motivates the generalization of both classical results: the Kronecker-Weber Theorem and CM elliptic curves, in the arithmetic theory of CM Abelian varieties of any dimension. And it is in this interpretation that

Hecke's dissertation and *Habilitationsschrift* do appear as a first step in this direction, *i.e.*, as an attempt at a theory of Abelian surfaces with complex multiplication.

The thirties and forties were characterized by a mutual fertilization of the theory of complex multiplication with other developments in the domain of arithmetic geometry (Hasse and his school, the Weil conjectures, Hecke characters). These developments all express the general tendency to place individual geometric objects, and the study of their arithmetic properties, at the centre of the theories. Deuring's theory of the L-function of a CM elliptic curve [Deuring 1953-57] is one of the most visible consequences of this trend within the traditional domain of the one-dimensional theory.

The higher dimensional theory was developed very quickly in the early fifties by Shimura, Taniyama, and Weil — see [Shimura and Taniyama 1961]. It turned out to be quite a bit more complicated than the one-dimensional case. New features include the distinction between CM-field and reflex field, the non reducibility of the class equation in general, the problem (solved explicitly only in 1980, by Tate and Deligne) of describing the action of all of $\mathrm{Aut}(\mathbf{C})$ on a CM Abelian variety, etc. And what is more relevant to Hilbert's 12th problem: in higher dimensions the theory systematically fails to provide enough elements to generate all Abelian extensions of the reflex field.

This point of view is modern in that the objects dealt with—elliptic curves, group varieties—do not show up as such in the arithmetic investigations of the 19th and early 20th century related to our subject. There one mainly talked about special values of modular or elliptic functions. But the modern point of view also has its own historical roots. In fact, there is a strong tradition which goes back to Gauss's remark at the end of the introduction of Chapter VII on cyclotomy of his *Disquisitiones Arithmeticae*,[42] where he suggests that it is possible to complement the cyclotomic theory which he is about to develop in the book by an analogous lemniscatic theory. This clue was taken up in particular by Eisenstein in [1850], and from there it entered into Kronecker's seminal papers of the 1880ies, and further into Weber's work.[43] A relatively modern version, but presented with a view to simplifying certain formulas in

[42] "Ceterum principia theoriæ, quam exponere aggredimur, multo latius patent, quam hic extenduntur. Namque non solum ad functiones circulares, sed pari successu ad multas alias functiones transcendentes applicari possunt, e.g. ad eas quæ ab integrali $\int \frac{dx}{\sqrt{(1-x^4)}}$ pendent, prætereaque etiam ad varia congruentiarum genera ... "; see [Schappacher 1997].

[43] See [Schappacher 1997], cf. [Vlăduţ 1991, chap. 3 and chap. 4, in particular pp. 74–76]. Note that Eisenstein's special case can be conveniently used to settle the normalizing property needed in the identification of the Taniyama group, and the simultaneous proof of the generalization due to Deligne and Tate of the Shimura-Taniyama reciprocity law; see [Schappacher 1994, 4.4.4].

Weber's Algebra can be found in [Deuring 1954].

The key result established in this tradition is a prototype of what is known today as the Shimura-Taniyama congruence relation, and thus of one of the central theorems of CM arithmetic, and the key for the computation of the Hasse-Weil L-function of the curve. Hilbert was surely aware of this tradition and its potential arithmetic relevance, in particular to higher reciprocity laws. He does not, however, make an explicit connection between his 12th problem and the ninth on general higher reciprocity laws. Only his comments in the middle passage of the 12th problem, on the analogies between the theory of algebraic functions of one variable and number theory, might conceivably be understood as hints in this direction. Still, it is striking that Hilbert does not seem to want to build this aspect of Kronecker's work into the research programme he proposes. He rather appears to have had a definite project in mind, and rewrote the history of complex multiplication in the 19th century accordingly.

The higher-dimensional theory was linearized in the book [Serre 1968] and succesfully integrated into a theory motives by Deligne, with Langlands's *Märchen* leading the way—cf. [Schappacher 1994] and the literature cited there.

But the history of ideas is not a one-way street, and the tradition of looking at the theory of complex multiplication mainly as a source of singular values of modular forms or functions and a tool for working with them, not only kept very much alive thoughout our century—from Hilbert's 12th problem, to the thesis under Emil Artin's supervision [Söhngen 1935], all the way to Shimura's reciprocity law for singular values of Hilbert modular forms—, but was finally turned even to diophantine problems following Kurt Heegner's seminal ideas and their development by Birch.

With motives and Heegner points—which both can only be fully appreciated today against the background of the theory of Shimura varieties—we have reached active current research. I hope to be able to come back on a later occasion to a more detailed historic analysis of these developments of the past fifty years.

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