

# André Weil As I Knew Him

Goro Shimura

Bathed in the sunlight of late summer, I was walking a quiet street of Takanawa, a relatively fashionable district in southern Tokyo, toward the Prince Hotel Annex, where André Weil was staying. It was the afternoon on a warm day of early September in 1955. He was among the eight foreign participants of the International Symposium on Algebraic Number Theory, to be held in Tokyo and Nikko that month. The Korean War had ended two years earlier, and in the United States Eisenhower's first term had begun in the same year. Five years later, in 1960, his planned visit to Japan would be hindered by the almost riotous demonstrations of labor unions and students in the city, but nobody foresaw it in the peaceful atmosphere of the mid 1950s. While walking, I had a mildly uplifted feeling of expectation and curiosity about what would happen, the first of those I would experience many times later whenever I was going to see Weil.

My acquaintance with him began in 1953, when I sent my manuscript on "Reduction of algebraic varieties with respect to a discrete valuation of the basic field" to him in Chicago, asking his opinion. I told him my intention of applying the theory eventually to complex multiplication of abelian varieties. In his answer, dated December 23, 1953, he was quite favorable to the work and encouraged me to proceed in that direction; he also advised me to send the paper to the *American Journal of Mathematics*, which I did. By that time I had read his trilogy *Foundations*, *Courbes Algébriques*, and *Variétés*

---

*Goro Shimura is professor of mathematics at Princeton University. His e-mail address is goro@math.princeton.edu.*

*Abéliennes*, as well as his 1950 Congress lecture [50b]<sup>1</sup> and a few more papers of his. I was also aware of the existence of many of his other papers or had some vague ideas about them, [28], [35b]<sup>2</sup>, [49b], [51a], for example. But I don't think I had read all those before 1955. The article "L'avenir des mathématiques" [47a] and his review [51c] of Chevalley's book on algebraic functions were topics of conversation among young mathematicians in Tokyo. Later, while in Japan, when he was asked to offer his opinion on various things, he jokingly complained that he was being treated like a prophet, not a professor. But to some extent that was so even before his arrival.

In any case, when he accepted the invitation to the Tokyo-Nikko conference, we young mathematicians in Japan expected him with a sense of keen anticipation. I shook hands with him for the first time on August 18, in a room in the Mathematics Department, University of Tokyo. He looked gentler than the photo I had seen somewhere. He was forty-nine at that time. Our meeting was short, and there was not much mathematical discussion, nor did he make any strong impression on me that day. He was given, perhaps a few days later, a set of mimeographed preliminary drafts of papers of most Japanese participants, including my 49-page manuscript titled "On complex multiplications", which was never published in its original form.<sup>3</sup>

About two weeks later a message was forwarded to me: Professor Weil wishes to see me at his hotel. So I brought myself there at the appointed time. He appeared in the lobby wearing beige trousers with no jacket or tie. He had read my manuscript

by then, and, sitting on a patio chair in a small courtyard of the hotel, he asked many questions and made some comments. Then he started to talk about his ideas on polarization of an abelian variety and a Kummer variety. He scribbled various formulas on some hotel stationery, which I still keep in my possession. At some point he left his chair; pacing the courtyard from one end to the other, he impatiently tried to pour his ideas into my head. He treated me as if I was an expert who knew everything. I knew of course what a divisor meant and even the notion of linear and algebraic equivalence, as I had read his 1954 *Annalen* paper on that topic, but I lacked the true feeling of the matter, not to speak of the historical perspective. Therefore, though I tried hard to follow him, it is fair to say that I understood little of what he said. At the end we had tea, and he ate a rather large piece of cake, but I declined his offer of the same, perhaps because his grilling lessened my appetite.

During the conference and his stay in Tokyo afterward, I saw him many times. On each occasion he behaved very naturally, if in a stimulating way. It was as if that hotel encounter had the effect of immunization for me, and possibly for him too. I remember that I asked him about the nature of the periods of a differential form of the first kind on an abelian variety with complex multiplication. He said, "They are highly transcendental," which was not a satisfactory answer, but as good as anything under the circumstances. At least, and at long last, I found someone to whom I could ask such a question. Those several weeks were truly a memorable and exciting period. To make it more exciting, one of my colleagues would make a telephone call to the other, imitating Weil's voice and accent: "Hello, this is Weil. I didn't understand what you said the other day, so I'd like to discuss with you ..." Sometimes the prank worked. A few days before his leaving Tokyo for Chicago, I, together with three such naughty boys, visited him in another hotel in the same area. Taniyama promised to come, but didn't; apparently he overslept as usual. During our conversation, Weil advised us not to stick to a wrong idea too long: "At some point you must be able to tell whether your idea is right or wrong; then you must have the guts to throw away your wrong idea."

As he said in his *Collected Papers*, his stay in Japan was one of his most enjoyable and gratifying periods. He found an audience of young people who were not afraid of him and who were sophisticated enough to understand, or at least willing enough to try to understand, his mathematics; he certainly had an audience in the United States then, but apparently of a different kind.

More than two years passed before I saw him again, which was in Paris in November 1957. Henri Cartan, accepting his suggestion, had secured a position of chargé de recherches at CNRS for me. Weil

was on leave from Chicago for one year and sharing an office with Roger Godement at the Institute Henri Poincaré, but he occupied it alone for most of the time. In that period he was working on various problems on algebraic groups, the topics which can be seen from [57c], [58d], and [60b], for example. He was giving lectures on one such subject at the École Normale and regularly attended the Cartan seminar. He lived in an apartment at the southeast corner of the Luxembourg garden, with a fantastic view of Sacré Coeur to the far north and the Eiffel Tower to the west. One of his favorite restaurants was Au Vieux Paris, in the back of the Panthéon. A few days after my arrival, he invited me to have lunch there. I remember that he had *radis au beurre* (radish with butter) and *lapin sauté*, a fairly common affair in those days, but perhaps somewhat old-fashioned nowadays. I don't remember his choice of wine, but most likely a full-sized glass of red wine for each of us. To tell the truth, it was not rare to find him snoozing during the seminar. From his apartment, the Institut and the Panthéon could be reached in less than ten minutes on foot. Paris in the 1950s retained its legendary charm of an old city which had not changed much—he once told me—since the days of his childhood. It is sad to note that the city went through an inevitable and drastic transformation in the 1970s.

Though I was working on a topic different from his, he was earnestly interested in my progress, and so I would drop into his office whenever I had something to talk about. For instance, one day I showed him some of my latest results for which I employed Poincaré's theorem on the number of common zeros of theta functions. He smiled and said, "Oh, you use it, but it is not a rigorously proved theorem." Then he advised me to take a different route or to find a better proof; later he told me a recently proved result concerning divisors on an abelian variety, by which I was able to save my result, as well as Poincaré's theorem.

On another occasion, I heard some shouting in his office. As I had only a brief message for him, I knocked on the door. He opened it and introduced me to Friederich Mautner<sup>4</sup>), professor at Johns Hopkins, who was his shouting partner. After a minute or so I left. As soon as I closed the door, they started their shouting again. When I was walking through the corridor after spending half an hour in the library, the shouting match was still going on; I never knew when and how it began and ended, nor who won.

From time to time he fetched me for a walk in the city. The topics of our conversation during those walks were varied; he would suggest to me, for example, that I go to churches to listen to religious music; he said it was necessary for me only to stand up and sit down when others did. When asked about his faith, he said, "Pas du tout" ("Not

at all”). According to him, one of the best ways to learn French or any foreign language was to see the same movie in that language again and again, staying in the same seat in the same movie theatre, a piece of advice I followed perhaps too faithfully. It was the time when Brigitte Bardot and Zizi Jeanmaire were at their zenith. Another method he suggested was to read newspapers, but I was not so diligent in this task. Perhaps as he became impatient with my slow progress in French, he asked me whether I was doing my homework in that respect. I dodged the issue by mentioning an old Oriental saying: “He who runs after two rabbits will catch neither.” Maybe I subconsciously remembered the rabbit for his meal. “What’s your rabbit? Hecke operators?” he asked. Then we discussed the possible method or philosophy of how a Frobenius of a reduced variety can be lifted. A few days later he caught me in the library and asked again, “What about your rabbit?” He was an extremely sharp man, and clearly he sensed that I was up to something, which was true. In this article, however, I should leave the rabbit at large, merely mentioning that he would later say, “How is your rabbitry doing?”

Starting in the fall of 1958, he was at the Institute for Advanced Study permanently, and I was a member there for that academic year. So I practically followed him, and I had the same daily routine with him for another several months. Looking back on those days, I am filled with a sense of deep gratitude to him for paying such unusual and personal attention to me; also, I must note to my regret that, unaware of the real meaning of my situation at that time, I did not take full advantage of my fortunate privilege of being constantly with such an extraordinary man in his prime.

In the spring of 1961 he spent a few months in Japan with his wife, Eveline. Though they undoubtedly enjoyed their stay and I was happy to have a person at hand who really understood me—perhaps the only one at that time—I may be excused for saying that overall his presence was less than a pale revival of his former visit.<sup>5)</sup> As for myself, after spending three years in Japan, I came back to Princeton in September 1962, when I began a new and long chapter of my relationship with him. To continue my narrative, I will now present some interesting aspects of his words and deeds in this period, irrespective of the chronological order of the events.

As already mentioned, he liked to walk, partly for the purpose of physical exercise. In Princeton every Sunday he would walk one and a half miles from his home to buy the Sunday *New York Times*, and so, according to his daughters, his church denomination was pedestrian. At the Institute he would occasionally pick a walking partner among the members. He was not a good walker, however. Though he was physically fit and walked briskly,

he often fell on his face by tripping on something on the ground. That happened when I was with him in the Institute wood, but I pretended to have seen nothing, as he hated being helped on such an occasion. Though he was not injured then, he was not so lucky other times. During such a walk, he would answer my questions or would tell his stories. Here are some samples:

When he was twelve or thirteen,<sup>6)</sup> there was a magazine for elementary mathematics asking the reader to send in solutions to the problems; then they would print the best solutions. He contributed many, as he found great pleasure in seeing his name in the magazine, but he graduated from that level after about two years. Then he said, “Maybe I should have included some of the solutions in my oeuvres, he! he! he!”

Around the time when he was at Haverford, he asked Hermann Weyl to lend him some money. “How much?” asked Weyl. “Well, four or five hundred dollars.” Then Weyl brought out his checkbook, and, after thinking awhile, he signed a check for four hundred and fifty dollars.

When he was teaching at Lehigh, a student asked him for help in calculus. After they spent a lot of time struggling to find out what his problem was, the student finally said, “I don’t seem to understand this symbol  $x$ .”<sup>7)</sup>

A French gentleman’s ideal is to have three concurrent loves: the first one, whom he cares about at present; the second, a potential one, whom he has his eye on with the hope that she will eventually be his principal love; the third, the past one, with whom he hasn’t completely cut off his relations. Then he observed: “It’s a good idea for a mathematician to have three mathematical loves in the same sense.”

He would talk about Baudelaire, Proust, and Gide, their homosexuality in particular; Paul Claudel’s treatment of his sister Camille; and also about the letter exchange between Paul Claudel and Madeleine Gide. He amused himself by twisting each story in his own fashion to make it funny, often with a piquant effect.

I asked, for what reason I don’t remember, whether he read detective stories. “Yes, but only when I have a cold,” he said, and added, “You know, when you have a cold, there is nothing else to do but read detective stories.” He was rather apologetic, and so I asked, “How often do you catch cold?” “Very often” was his answer.

As to Fields medals, he said: “It’s a kind of lottery. There are so many eligible candidates, and the whole selection process is a matter of chance. Therefore, the prize could be given to *any of them*, as in a lottery.”<sup>8)</sup>

He used to say that a good mathematician must have two good ideas. “It is possible for someone to have a really good idea, but it may be just a fluke. Once the person has a second good idea, then

there is a good chance for him to develop into a better mathematician." He mentioned a well-known American as a prolific mathematician with a single idea. He also noted Mordell as a counterexample to his principle.

He could say something even harsher, but that was rare. In the summer of 1970, after the Nice Congress, I was talking with him somewhere in the Institute about French mathematicians. He observed that there were three young mathematicians in Paris who started brilliantly, and so there were high expectations for them. He mentioned three well-known names and said, "What happened to them? They utterly failed to produce anything great." That was more than a quarter century ago, but I cannot tell whether or not he changed his opinion, as we never talked on that matter again. Around 1975 he expressed, more than once, his pessimistic view that French mathematics had been declining for some time. Therefore, we should perhaps take his criticism in that context.

He held Riemann and Poincaré in high esteem, which was more than natural; Hecke was also a favorite. He rarely talked about Hilbert in our conversation. He didn't think much of Klein, which is not surprising. Picard was depicted by him as formal and stiff. Among his contemporaries, he thought highly of Siegel and spoke of Chevalley in amicable terms, but not so with Weyl, about whom he seemed to have a kind of ambivalence. He recognized the unusual talent of Eichler.<sup>9)</sup> Hadamard was his teacher, and their relationship is well documented in his autobiography. He paid due respect to Hasse, though he remembered the fact that Hasse wore a Nazi uniform at some point.<sup>10)</sup> He told me several anecdotes about Hardy, but he presented each story in a sarcastic tone. "Hardy's opinion that mathematics is a young man's game is nonsense," he said.

It may be too optimistic a view to say that most people mellow with advancing age. At least many do, and there are those who don't. It is told, for example, that Saint Saëns achieved an ever-increasing reputation as a man of bad temper through his long life of eighty-six years. Weil did mellow, but even after the age of seventy he was capable, if rarely, of being childishly irritable, as can be seen from the following episode. But first let me note: Around 1976 or 1977 he declared, "I am no longer a mathematician; I am a mathematical historian." Apparently he realized that there were no more subjects he could handle better than the younger generation. Coming to my story: In my teens I somehow got hold of a copy of a pirate edition, which was being called the Shanghai edition, of *Eindeutige Analytische Funktionen* by Rolf Nevanlinna. I enjoyed reading the first one-third of the book, but gave up on the rest. Still, my reading of the book remains as one of my fond memories. When I recognized Nevanlinna in a lecture hall at

the 1978 Helsinki Congress, I introduced myself and shook hands with him, an incident which in my youth I never imagined would happen. He was eighty-three then. Weil gave a lecture titled "History of Mathematics: Why and How" there.

After the Congress I spent a week in Paris, and one day I was sipping coffee with Weil in a café near his apartment. I told him about that happy experience of mine at Helsinki. But he was much displeased with my story. He said with a grimace that Nevanlinna was not such a good mathematician worthy of my esteem, and so on. I was dumbfounded; I never idolized Nevanlinna, whose name I knew before acquainting myself with any of Weil's works, simply because the book was accidentally available. That must have been clear to him. After all, it was none other than Nevanlinna who saved him from being executed by the Finnish police, a fact he told me some years earlier and narrated in his autobiography, which also includes a passage on the Weil couple's happy stay in Nevanlinna's villa in 1939.

I should add, however, that he could be found on the other side of the world. When there was a discussion of a new appointment at the Institute, Morton White, professor of the school of history, was fiercely against the proposition, and at the faculty meeting he expressed his opinion in a heated fashion. Then Weil, sitting next to him, said, "Calm down, please, calm down." White later told me that he thought the scene rather funny in view of the normal temperament of Weil.

After Eveline's passing away in May 1986 at the age of seventy-five, his daughter Nicolette bought a microwave oven for him. However, saying that he didn't like to "push the button," he never touched it, and so the oven was returned to the dealer. The Weils had been our regular dinner guests, but since then, naturally he alone was with us, which happened not infrequently. It was sometime in December 1986. Weil, Hervé Jacquet, Karl Rubin, Alice Silverberg, my wife Chikako, and I had dinner at a Chinese restaurant and were having dessert at our place. When I prodded the guests to tell their ambitions in their next lives, Jacquet said he would like to be an opera singer, and that was not a joke for him. In fact, opera singing was his first love, mathematics being merely the second. Next, "I want to be a Chinese scholar studying Chinese poems," said Weil. After visiting China twice, he had been reading English translations of Chinese standard literature like *The Dream of the Red Chamber*. "That may be a rather dull life, and I don't think a person like you can stand it," said I. "All right then, I will be a house cat. The life of a house cat is very comfortable." Pointing to our neighbor's female white cat, who was also a guest, he said, "Maybe she will be my mother." Then Rubin said, "Perhaps a Chinese cat is a good solution." With laughter, everybody accepted it. That

was about a week or two before Christmas, and so after a few days Chikako brought him a stuffed cat as a Christmas present, which pleased him greatly. In fact, the Weil family used to have a cat, and once he defended himself for having a Christmas tree in his house by saying that they had it because their cat loved it.

He was conscious of his old age, particularly after he became a widower. According to what he said, Eveline was afraid of becoming senile. But she was not at all senile when she died. A famous French mathematician who lived beyond eighty was senile in his last two years, but he knew it himself. So when he had visitors, he held a newspaper to show that he was at least able to read, but the paper was often upside down. Another, who lived longer, was not like that; even so, when Weil visited him, he brought out and showed him, one after another, the diploma of each of the many honorary degrees he had received.

As for Weil himself, he showed no such sign, as far as I remember. I talked with him sometime in November 1995 for half an hour or so in his office. He was alert and able to make a reasonable judgment on the matter for which I went to see him. There was a lunch party for his ninetieth birthday in May 1996 at a restaurant in Princeton; though he didn't talk much, he was in a good mood. Before and after that, Chikako had lunch at the Institute cafeteria several times; she would find him eating mostly alone, sometimes with his daughters. She would say hello to him, to which he would reply, "Is Goro here?" So she was relieved to find that he at least remembered her as someone related to me.

I saw him for the last time on December 19, 1996. For some reason he phoned me the day before. Since he had hearing difficulties, he finally suggested that I see him at the Institute. I proposed some date, but he said, "No, why don't you come tomorrow; otherwise I won't remember." So I had lunch with him there that day. From the previous night it had been drizzling endlessly. When I met him in the common room of Fuld Hall, he didn't have his hearing aid, and he asked me to drive him home to get it. After getting it, we went into the dining hall. He used to eat well, and almost twice as much as I. Around 1980 André, Eveline, Chikako, and I had lunch together at a restaurant in New Hope, Pennsylvania. That was a buffet style affair, and he was in high spirits. I remember that his appetite impressed the remaining three. Incidentally, he was not fussy about wine. Not that he didn't care, but it is my impression that Eveline cared more.

I was curious how he would eat this time. Not surprisingly, compared with what he ate sixteen years ago, the quantity he took was modest, less than half of the previous meal. Since he had hearing problems, it was difficult to conduct our con-

versation smoothly, and I often had to write words and sentences on a piece of paper. Unlike the occasion forty-one years ago, this time it was I who was writing. I was working on the Siegel mass formula<sup>11)</sup> with a new idea at that time, and that was one of his favorite topics. So I asked him about the history of that subject. For example, I asked him whether or how he studied the works of Eisenstein, Minkowski, and Hardy. He said he didn't remember about Eisenstein,<sup>12)</sup> but he had studied a little, but not much, of Minkowski's work; he never studied Hardy. He kept saying that it was a long time ago, and so he didn't remember, which must be true, and so we should not accept what he said at face value. In fact, to check that point, I asked him whether Minkowski was reliable. He said, "I think so." At that point I realized that his recollection was faulty, since Minkowski gave an incorrect formula, as Siegel pointed out, and that was known to most experts. If I was asking questions on what he did in his twenties or thirties, he might have remembered things better, but at that time I didn't take into account the fact that he worked on the Siegel formula in his fifties.

I asked him whether he was writing something on a historical topic. He said, "I cannot write anymore." To cheer him up, I then said, "That's why I told you long ago to get a computer." He also said he was half blind. Toward the end of the meal he said, "I'd like to see the Riemann hypothesis settled before I die, but that is unlikely."

That reminded me of a party at Borel's place in the 1970s. Wei-Liang Chow was the guest of honor. I was talking with Chow and Borel about a passage in Charlie Chaplin's autobiography. In it Chaplin in his twenties met a fortuneteller in San Francisco who told him that he would make a tremendous fortune, would be married so many times with so many children, and would die of bronchial pneumonia at the age of eighty-two. Hearing this story, Weil said, "Well, in my autobiography I might write that in my youth I was told by a fortuneteller that I would never be able to solve the Riemann hypothesis."

When we left the dining hall and were walking to the parking lot, he said, "You are certainly disappointed, but I am disappointed too," and added, after a few seconds, "with myself." He knew that I was expecting him to say something about Siegel's work. He again said, "I cannot write anymore." I drove him home and left. He was able to walk slowly, but I couldn't say he was in good shape; still, he was not in terrible shape, and so I had a sense of relief. While driving home alone under still drizzling rain, I could not help but recall our hotel encounter in 1955 and the lunch in 1957, though I did not think much about the possibility that I would never see him again.

André Weil as a mathematician will of course be remembered by his colossal accomplishments,

witnessed by the three volumes of his *Collected Papers* and several books, the trilogy mentioned at the beginning in particular. In my mind, however, he will remain chiefly as the figure with two mutually related characteristics: First, he was flexible and receptive to new ideas of others and new directions, quite unlike many of the younger people these days who can work only within a well-established framework. Second, more importantly and in a similar vein, he had a deep and penetrating understanding of mathematics, or, rather, he strived tirelessly to understand the real meaning of every basic mathematical phenomenon and to present it in a clearer form and in a better perspective. He did so by endowing each subject with new concepts and setting up new frameworks, always in a fresh and fundamental way. In other words, he was not a mere problem solver. Clearly, his death marked the end of an era and at the same time left a large vacuum which will not easily be filled for a long time to come.

## Endnotes

1) Each number in brackets refers to the article designated by that number in his *Collected Papers*, with "19" omitted.

2) It seems that [35b] is the first paper which mentions the fact that the coordinate ring of a variety is integral over a subring obtained by considering suitable hyperplanes (see *Collected Papers*, vol. I, p. 89). Zariski attributed it to E. Noether. It is my impression that she considered generic hyperplane sections, but not the fact of elements being integral. Weil agreed with me on this and said, "Perhaps Zariski didn't like to refer to the work of a younger colleague, a common psychological phenomenon." On the other hand, though he must have had his own citation policy, frankly I had difficulty in accepting it occasionally. See 9) below.

3) As to my paper on "Reduction of algebraic varieties, etc." he said, "Il (Shimura) me dit, il eût plutôt eu en vue d'autres applications" (*Collected Papers*, vol. II, p. 542). This is not correct. Probably he misunderstood me when I told him that I was interested in Brauer's modular representations at one time. Brauer was also a participant of the conference.

4) Mautner was responsible for introducing Weil to Tamagawa's idea; see Weil's comments on [59a].

5) In his *Collected Papers* he says practically nothing about his second visit, though he mentions it; see vol. II, p. 551.

6) This is what he told me. In his autobiography, however, the story is assigned to an earlier period, which may be true.

7) This is also what he told me. A somewhat different version is given in his autobiography. He referred to his Lehigh days as his period of "overemployment".

8) There is a big difference. In order to win a lottery, we have to buy a ticket, but by doing so we put our trust in the fairness of the system.

9) Whenever he spoke of strong approximation in algebraic groups, he always referred to Kneser's theorem. That is so in [65], for example, which is understandable. But that was always so, even in his lectures in the 1960s, though in [62b] Eichler is mentioned in connection with the fact that the spinor genus of an indefinite quadratic form consists of a single class. However strange it may sound, it is possible, and even likely, that he was unable to recognize Eichler's fundamental idea and decisive result on strong approximation for simple algebras and orthogonal groups, and he knew only its consequence about the spinor genus. In his *Collected Papers* he candidly admits his ignorance in his youth. Though he had wide knowledge, his ignorance of certain well-known facts, even in his later years, surprised me occasionally. He knew Hecke's papers to the extent he quoted them in his own papers. It would be wrong, however, to assume that he was familiar with most of Hecke's papers. Besides, his comments in his *Collected Papers* include many insignificant references. For these reasons, the reader of those comments may be warned of their incompleteness and partiality.

10) According to Weil, Hasse, in such a uniform, once visited Julia, who became anxious about the possibility that he would be viewed as a collaborator.

11) In [65] he says, "On a ainsi retrouvé, quelque peu généralisées, tous les résultats démontrés par Siegel au cours de ses travaux sur les formes quadratiques, ainsi que ceux énoncés à la fin de [12] (Siegel's *Annalen* paper in 1952) à l'exception des suivants. Tout d'abord, ..." (*Collected Papers*, vol. III, p. 154). I think this is misleading, since the list of exceptions does not include the case of inhomogeneous forms, which Siegel investigated. It is true that Siegel's product formula for an inhomogeneous form in general can be obtained from the "formule de Siegel" (in Weil's generalized form, combined with some nontrivial calculations of the Fourier coefficients of Eisenstein series), and one might say that that is not so important. Still, it should be mentioned at least that the inhomogeneous case is not just the matter of the Tamagawa number and that nobody has ever made such explicit calculations in general, even in the orthogonal case. In the mid 1980s I asked Weil about this point, but he just said, "I don't remember."

12) In [76c] he reviews the complete works of Eisenstein; also the title of [76a] is *Elliptic Functions according to Eisenstein and Kronecker*. It is believable, however, that he didn't study Eisenstein's papers on quadratic forms in detail, though he must have been aware of them.