Memories of Goro Shimura

Don Blasius, Toni Bluher, Haruzo Hida, Kamal Khuri-Makdisi, Kenneth Ribet, Alice Silverberg, and Hiroyuki Yoshida

Goro Shimura, a mathematician who greatly influenced number theory in the second half of the twentieth century, was born in Japan on February 23, 1930. Over a career spanning six decades, he repeatedly made transformational discoveries that stimulated new lines of investigation and played a central role in the development of the field. Shimura earned his degrees at the University of Tokyo and held appointments at the University of Tokyo and Osaka University. He was a professor at Princeton University from 1964 until he retired in 1999. He authored numerous influential books and papers and was awarded a Guggenheim Fellowship in 1970, the Cole Prize in Number Theory in 1977, the Asahi Prize in 1991, and the Steele Prize for Lifetime Achievement in 1996. Goro Shimura passed away in Princeton on May 3, 2019, at the age of eighty-nine.

Don Blasius

Goro Shimura advised my 1981 Princeton thesis and he was, through his research and guidance, the central figure of my intellectual life in graduate school and for many years afterwards. His ideas about how to do mathematics have influenced me throughout my career.

Arriving at Princeton in fall 1977, I had no plan to study number theory, had never read a book about it, and had never heard of Shimura, Iwasawa, Dwork, Langlands, or even Weil. Most graduate courses had incomprehensible course descriptions. In this context Professor Shimura's offering of an introductory course in algebraic number theory stood out as a beacon of light. Algebra already had great appeal for me, and I was hooked right away by this perfect course. Later I took his courses on families of abelian varieties, special values of L-functions, period relations, the arithmetic theory of automorphic forms, Eisenstein series, theta functions, etc., all topics essential to his current research and, as it turned out, my initial research. He really taught for his students, not just to have a context to explore a topic of interest to himself. Each course started

Figure 1. Goro, Chikako, Haru, and Tomoko returning to the US from Haneda Airport in 1971.

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with minimal assumptions of background and went far into its subject with both clarity and an economy of means. He always had his lecture fully written out in a notebook, which would be on the table in front of him. He would lecture without it except for complex formulas. For them, he would pick it up to refresh his memory, always explaining clearly as he went. Shimura was always perfectly prepared and engaging.

By early in my second year, it was clear to me that I wanted Shimura to supervise my dissertation. When I queried him about it, he asked me to read his famous 1971 text *Introduction to the Arithmetic Theory of Automorphic Functions*. This book is a masterpiece of exposition, starting from scratch and giving, mostly with proofs, the key themes of his research up to that point. For me it was the perfect preparation for reading the articles on which it was based. It was a transformative undertaking. As an anecdote, I mention that when I told him I had finished Chapter 3, the one on Hecke algebras and the relation with L-functions, he asked me how long I had spent on it. I told him I had spent about a month, and he said that was "very fast" (meaning too fast). He was correct! Later that year, when I asked to be his student formally, he agreed but imposed the condition that I promise not to "fire" him. He explained that a student with whom he had been working for some time had just done that, and he was visibly hurt when he spoke about it. I told him that there was no chance of that.

Shimura did not tend, at least with me, to engage in lengthy, detailed discussion of mathematics. Usually, after a brief discussion of math, he would shift the conversation to something else, ranging from departmental gossip to Chinese stories. He did not give me a research problem right away. Instead, he asked me to read articles. The first was [3], mentioned below, and the second was [17]. Sometime while reading the second paper, he gave me the simple-sounding research problem concerning its main theme of relations between the periods of abelian varieties: "Find more precise results. Find natural fields of definition." This was a great problem because it connected with so many topics, including values of L-functions and the extension of the canonical models formalism from automorphic functions to automorphic forms, both new areas.

Before giving me this problem, Shimura had supported my desire to come up with my own. Several times I made suggestions to him, but he found compelling issues with them, such as being too hard or already done. When I finally found some results on his problem, he was happy and exclaimed "Isn't it nice to have some success?!" Later, I asked him about how he worked. He drew a really messy self-intersecting path on the blackboard and declared the end as his destination after the fact. In words, he told me he starts with a general idea but no fixed goal or conjecture.

Many people who did not know him have some imagination of Shimura as exclusively serious or severe. This is simply not true, and I mention two among many moments of wit that we both enjoyed. After I started a conversation with a remark about Hecke operators, he said, "The first thing you say to me is always interesting." And after I complained to him that I found his paper on confluent hypergeometric functions hard, he said, "You need to go see an analyst!"

About other mathematicians, he freely acknowledged his debts to Eichler, Hecke, and Weil, who was a friend for over four decades. He also had the highest respect for Siegel. Once he told me, "His proofs are correct and you can just use his theorems." He did not think that about many mathematicians. One day we discussed Weil's 1967 paper on the converse theorem. After a while he said, with intensity, "Weil is a genius." I never heard him say that about anyone else, even when discussing major works. About ideas, although he knew a great deal, he was something of a minimalist in his preferred way of writing. For example, when I referred to automorphic forms as sections of vector bundles, he queried me sharply as to whether this language added anything. I had to admit that, in the context, except for curb appeal to some, it did not. As a consequence, I avoided such usage in my dissertation.

I was asked to make a brief summary concerning Shimura's theory of canonical models and its antecedents. In 1953, at the start of his career, Shimura created ([1]) the first theory of reduction mod p of varieties in arbitrary dimensions. In a December 1953 letter to Shimura, Weil called it "a very important step forward" and emphasized its promise for the further development of complex multiplication. He also wrote that it was "just what is needed [to study] modular functions of several variables." The first direction became the famous collaboration of Shimura and Yutaka Taniyama, which was well underway by 1955. They defined and studied abelian (group) varieties of CM type and proved the Shimura–Taniyama reciprocity law, which describes explicitly the action of a Galois group on the points of finite order of the variety. The main underlying result here is a celebrated formula for the prime ideal decomposition of the endomorphism that reduces to the Frobenius morphism attached to a given prime of the field of definition. As a key application of this formula, they computed the Hasse–Weil zeta function at almost all places, thereby verifying Hasse's conjecture for such functions. Their research was summarized in the well-known 1961 monograph *Complex Multiplication of Abelian Varieties and Its Applications to Number Theory*, which Shimura wrote after Taniyama's death. In fact, Shimura wrote research articles about complex multiplication and abelian varieties over his career, even publishing in 1998 an expository monograph *Abelian Varieties with Complex Multiplication and Modular Functions*. This text includes more recent fundamental topics of his research, such as reciprocity laws for values of modular functions at CM points and the theory of period relations for abelian varieties of CM type.

From the late 1950s until the late 1960s Shimura made a continuing study, mostly published in the *Annals of Mathematics*, of the fields of definition for certain varieties defined by arithmetic quotients of bounded symmetric domains. For me, the start was the 1963 article "On analytic families of polarized abelian varieties and automorphic functions." In its first part, this highly readable paper showed that arithmetic quotients of three of the four classical domains arise, via normalizing period matrices, as analytic parameter spaces of the varieties of a given type. In 1966 he followed up with [7], which introduced the well-known notion of *PEL type*. He constructed a moduli space for each type as a model of an arithmetic quotient, thus providing a large supply of varieties whose points had definite meaning, and remarking of their fields of definition k_Ω: "In many cases we have verified that k_Ω is an abelian extension of K'." Here K' is a number field, frequently called the reflex field, which is central to the subject and which first arose in the Shimura–Taniyama theory. In 1964 in [4], he studied the varieties (quotients of products of upper half-planes) attached to quaternion algebras over a totally real field of arbitrary ramification behavior at infinite places. This paper introduced the cases where the fields of definition are abelian extensions of totally real fields. The algebraic varieties themselves had already been studied in [5] as moduli spaces associated to quaternion algebras over CM fields, in which case the canonical fields of definition are abelian extensions of the reflex field, a CM field. Thus in [4] it was a question of a further descent (Shimura used the term "bottom field"). In 1967, in [9] he considered further the cases of [5] where the dimension is one and computed, via a congruence relation analogous to Eichler's, the Hasse–Weil zeta function at almost all places, thereby proving Hasse's conjecture for the curves. These are the famous "Shimura curves." This article also introduces the notion of a canonical model as one uniquely characterized by an explicit description, obtained by virtue of the uniformization, of the Galois action over the reflex field on the images of CM points (see Main Theorem 1 of the article). In 1967 as well, a further paper [8] extended the canonical model theory of [9] to arithmetic quotients of higher-dimensional Siegel spaces. All these papers were written in the language of ideals. Finally, in 1970 this long development culminated with two articles, now famous: "On canonical models of arithmetic quotients of bounded symmetric domains I, II," both published in the *Annals*. In them, Shimura gave an adelic version of [8]. This viewpoint enabled him to define an action of the finite adeles (a way of introducing Hecke operators) of the associated reductive group on the system of models. He conjectured that such a theory would exist for any reductive group giving rise to a product of classical domains. Indeed he wrote, "The completion of this task does not seem so difficult."

Figure 2. Chikako and Goro in 1993 in Nagano prefecture, where they had a summer house.

Shimura's students Shih and Miyake each proved cases of Shimura's conjecture, and Deligne made major progress, as well as reformulating the theory in a general axiomatic way. In 1983 Borovoi and Milne constructed canonical models for all reductive groups of Hermitian symmetric type. They did this by proving a conjecture of Langlands that extended the reciprocity law at the fixed points to arbitrary automorphisms of Gal($\overline{\mathbf{Q}}/\mathbf{Q}$) instead of Gal($\overline{\mathbf{Q}}/K'$). This conjecture was based itself on Langlands's remarkable extension, later proven by Deligne, of the reciprocity law of Shimura and Taniyama for abelian varieties of CM type. Shimura himself did not really return to the theory after 1970, except, as mentioned, for extending it to automorphic forms in some special cases. Instead, the 1970s were for him a period of great and diverse achievements in new fields such as the theory of critical values of L-functions. Yoshida and Khuri-Makdisi mention these in their contributions, so I will stop here.

I miss Shimura deeply. Before beginning to write, I looked again at many of his articles that were so important to me. I fell again under the spell of my teacher and I found a problem to work on.

Toni Bluher

One of the reasons that Professor Shimura had so many graduate students was his extraordinary commitment to

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teaching. He gave masterful lectures that were very popular among the graduate students. We circulated among ourselves mimeographed copies of notes from prior years. Most influential to me was his course on Siegel modular forms, given during my first year of graduate school in 1984. Based on my experience in his course, I asked to be his graduate student and told him that I particularly enjoyed the material on theta functions. He remembered that comment and designed a thesis topic for me that included theta functions of half-integral weight. He prepared a series of readings that introduced all the concepts that I would need for my thesis topic, including some material on bounded symmetric domains, several of Shimura's articles, and Andre Weil's *Sur certain groups d'operateurs unitaires* (Acta Math **111**, 1964, 143–211). Everything fit together perfectly, and the thesis was progressing well. I got married in my second year of graduate school and had a son in September of my fourth year. Professor Shimura was supportive and nominated me for a Sloan Scholarship, which relieved me of teaching duties and made it possible to focus on writing my thesis. Being Shimura's student was akin to hiking Mount Everest with a skilled guide—he cleared the path so that we could reach the summit. I remember him saying that he would have preferred an approach that would give us more time to read and gain perspective, but he adopted his style because at that time it was expected that graduate students should finish in four years, something that is hard to do in such a technical field.

I have fond memories of when Professor Shimura invited me, my husband, and other graduate students to his house on a few occasions and also a dinner party where we met Andre Weil. At one of those occasions, I learned about his sense of humor. He said that puns are not part of Japanese culture, and he could not see how they were funny. "So what is funny to you?" I asked. He told the following joke. Some mosquitos were annoying the guests, so the host said he would take care of it. He put out a bowl of sake and many small pieces of tissue paper. "How will this help get rid of the mosquitos?" the guests asked. The host replied that the mosquitos would drink the sake and then fall asleep. "Ah, very clever," said the guests, "and what is the tissue for?" "It's so that the mosquitos will have a place to rest their heads after they drink the sake!"

Haruzo Hida

In the mid-1970s, I was a senior undergraduate at Kyoto University and had just started learning mathematics.

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Somehow I had met Professor Doi in Kyoto slightly earlier, and because of a friend of mine (an ardent mathematics addict) I had started reading mathematics books at college level and above. This day, following a suggestion of Doi, I planned to attend a lecture by Professor Shimura at Tokyo University of Education. I took a bullet train in the morning and reached Tokyo about an hour before the lecture. In the lecture, he talked about CM abelian varieties and their fields of moduli. I understood the content well, as I had already read his red book and the English version of the book he coauthored with Taniyama. Because of my late start, I was obsessively reading, as quickly as possible, many mathematics books. I did well, giving myself good background knowledge of analysis, algebraic and analytic number theory, and algebraic geometry, including the viewpoints of both Weil and Grothendieck. Prior to this point in my life, I had read a great deal but mostly to have fun. Reading books, including Chinese classics that Shimura loved, had been my way of life.

After the lecture, senior PhD students were invited into a smaller room to pose questions to the speaker. Out of curiosity, I walked into the room also. As Shimura had a rare charisma, at first nobody dared to ask him questions. This seemed impolite to me, so I started asking some well-posed questions about CM abelian varieties. This went well, and he answered me, looking directly into my eyes, treating me correctly as a novice, and emphasizing the importance of studying periods of CM automorphic forms. Then there was another awkward silence, so I decided to ask a somewhat imprecise question about a minimal field of definition of a given CM abelian variety, as it seemed to be related to some results on the field of moduli that I had seen in a preprint of his that Doi had received ([15]). Once I had described the question (saying that Doi allowed me to see his new results), he got excited and gave me a terse reply, saying that you could ask questions about facts, but trying to get some "guess" or some "way" towards a new result from somebody else is not morally sound. You should think about them on your own and should find a way out. His last words were, "Do your own mathematics."

I am from a family belonging to a traditional commercial class of people in Osaka-Kyoto. My family had been successful in banking from the late shogunate era through the Meiji Restoration. In Japan, we had a "rice exchange," which was analogous to a European stock exchange, as early as the late seventeenth century. There were "rice stocks" (*kome-tegata*) that were traded at the exchange by banking officials, and the price of rice, including that collected as tax by each feudal province, was determined nationally at the exchange. Banking (called *Ryogae*, literally "money-exchange") was a prosperous business for three or four centuries. Even though Japan was governed by a combination of the shogun's samurai (principled and well literate in Chinese classics) and the emperor's aristocrats (often indulging in subtle poems with hidden meanings), the economy at the time was essentially an unofficial capitalism. This is one of the reasons Japan was able to modernize itself so quickly in the colonial period of other Asian nations. For people in banking, a deep understanding of the governing samurai class is fundamental for their business. So most children of either gender in such a family had a sort of private tutor/nurse/nanny to train them how to divine the undercurrent thinking of people. (I mean, training so that your front personality can make friendly contact while at the same time your rear personality can delve into the counterpart's intents, often by posing questions that appear innocuous.) I had learned this type of slightly schizophrenic approach to people. Thus, when Shimura made his reply to my question, I was calm, but inside I was quite amused by his excitement (as I was looking for a way to cope well with him). I decided to avoid henceforth indulging myself too much in unfounded thoughts with him and instead to focus on asking him well-posed, maybe conjectural, questions and to present him with new ideas, not necessarily in mathematics. I made a firm note in my mind that this should be my way to cope with his principled personality.

Immediately after his lecture, I stopped my indulgence of reading mathematics broadly and focused on books whose content I felt I really needed. This freed my time, and I started writing a research article that became my first paper, published in 1978. In March 1976, there was a Takagi anniversary conference at RIMS, and at it I had a short conversation with Shimura without much content (though he remembered me well). I felt shame that I could not produce something entertaining to him by this time, although I had the seed of an idea of creating complex multiplication on the complex torus spanned by CM theta series in the middle degree Jacobian of the Hilbert modular variety. I finished this project a year after the conference. Doi had left for the Max Planck Institute for Mathematics just after the conference, and in April 1976, I entered the graduate school of Kyoto University. I was next to meet Doi again in Sapporo only two years later, so I was alone. Fortunately, Hiroyuki Yoshida returned to Kyoto at this time after his PhD study with Shimura in Princeton, and he had a good understanding of Hilbert modular varieties. In fact, while working on this problem, I talked only to Yoshida. When I was ready, I wrote about my results to Shimura at Princeton and, surprisingly, this attracted him. Indeed, the work suggested that higher-dimensional periods of Hilbert modular CM theta series are somehow related to periods of CM elliptic curves, at least if the base totally real field has odd degree. Here I should mention that CM period relations were a main topic of Shimura's research at the time. I knew this conjecture, but I did not explicitly write it in the letter or in the published paper. In any case, I got a job at Hokkaido University with the help of Doi, who moved there after his trip to Germany. For my next project, at Hokkaido, I classified CM factors of Jacobians of Shimura curves, extending Shimura's work for the case of modular curves. I thus, with Shimura's support, was offered a one-year visit to the Institute for Advanced Study.

I arrived at Princeton in August 1979, and right away I called Shimura on the phone. He invited me and my wife to a dinner at his home. At the dinner, he asked us a funny question: Why do Osaka people use the plural form *oko-tachi* in referring to an only child? The part "tachi" is a plural indication, although the Japanese language does not have a systematic plural form. My answer was that for a family in commerce, having several children is more desirable than having one, and hence the talker is apparently showing friendship by way of courtesy. He was not at all convinced, giving me a couple of counterexamples from the usage found in Kyoto aristocracy. This was typical in his conversation. He would come up with totally unexpected questions, and if one's answer was off the mark, he used it as a seed-topic of often poignant stories he loved to talk about. My answer could be wrong but not bad either (at least not provable in either way). After this conversation, he started calling me *Haruzo-san* and told me to call him *Goro-san*, which I never did in our conversation. (I called him, to his dismay, always *Sensei*.) I was told at the dinner to come to see him in Fine Hall at tea time, that is, every Thursday at 3 pm.

I kept busy every week to concoct something new to tell Sensei on Thursday. If I had not done that, I would have had a hard time listening to his short stories. To cope with them I needed all my skill of conversation. Perhaps he was doing this intentionally to pull out the most from me. I was fairly successful in the first year, and I wrote three papers, later published in *Inventiones*. By these, I think, I earned an extension to stay at the IAS for a second year. This year was difficult, although I had a conjectural idea about p-adic deformation of modular forms and the use of the Hecke algebra to make something like a $GL(2)$-version of Iwasawa theory. They were fuzzy thoughts at the time and only once or twice useful in our conversation. Repetition of topics, without much progress, was not a useful strategy for conversation with Sensei. But if you are an entertainer hired by somebody, you need to produce an attraction every time you perform! Thus, I had to skip the meeting several times. Fortunately, I eventually came up with a use of the partial Fourier transform to compute q-expansions of orthogonal and unitary Eisenstein series, as well as some series that Shimura had invented (I call them Shimura series). Then I went to the tea. The first words he threw at me were "I thought you are dead." I replied to him, "Like the characters 'Yosaburo and Otomi' of a Kabuki play (Japanese opera), somebody's survival could not be known even to Buddha." This amused him. I recovered, and I was to find a place twenty-five years later for this computation, in the case of Siegel's theta series, in my article in the Coates volume of *Documenta Math*. dealing with the anti-cyclotomic main conjecture.

Those were demanding but happy days for me at IAS. Shimura was able to squeeze out of me every bit of mathematics I potentially had. I still have a good stock of usable results from the notes at the time. He did not teach me much mathematics, but he guided me how to pull out something useful from my own mind, not from books or articles somebody else wrote. I am grateful for his unusual effort for my development. Farewell to his existence, which was so richly difficult, rewarding, and fun for me. *Sayou-nara*! (literally, "if things have gone that way, we part").

Kamal Khuri-Makdisi

Personal memories. Goro Shimura was my thesis advisor in the early 1990s, and the relationship developed into friendship over the subsequent years. While I was his graduate student, he gave me full support and mentorship, patiently guiding me through first reading a number of his articles in preparation for my thesis, then the actual thesis work. Our weekly meetings would usually last one to one and a half hours, during which he was unstintingly generous with his attention and advice. When, as a result of my youthful inexperience, I made a naive mathematical speculation, or was mistaken about a certain point, he would diplomatically correct me with the phrase, "That is completely correct, but...." He also regularly exhorted me during my thesis and subsequent career choices to be "practical," especially in terms of finding research subjects to work on that were both attainable and interesting. He was so aware of my progress that when I stalled for a while on my thesis, he was able to diagnose the problem without my having told him precisely what I had been stuck on. He simply presented me one day with a few pages of notes where he explained what I had most probably overlooked (an issue where Maass-type Hilbert modular forms could be either odd or even at each archimedean place, which led to different constructions). His advice was of course right on the mark, and he included in his notes suggestions on

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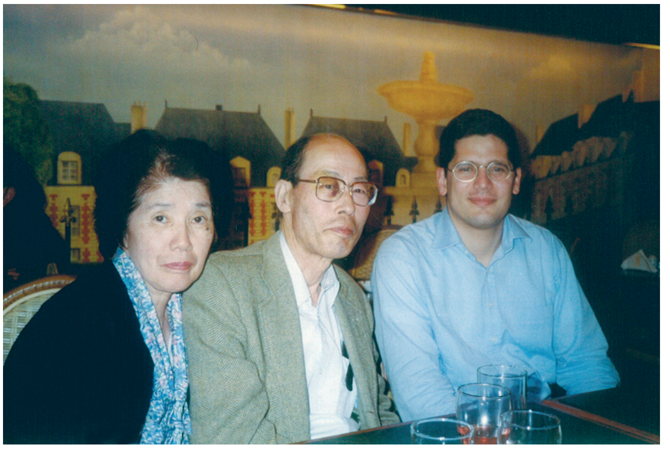

Figure 3. Chikako and Goro with Kamal Khuri-Makdisi in Beirut in May 2000.

how to overcome the blockage.

During my time at Princeton, Shimura's graduate courses alternated between lectures at the introductory or more advanced level and seminars where students had to present material out of his 1971 book *Introduction to the Arithmetic Theory...* or from terse notes of his on L-functions of modular forms and Artin L-functions. I have kept these notes preciously over the years. At the end of the semester, particularly with a seminar, he would invite the students over to his house for a "dinner in lieu of final exam," an occasion to be more informal than in class.

Shimura had a real interest in art, with an impressive collection of both prints and porcelain, the latter not only from East Asia but also the Middle East; his nonmathematical writings include a book on Imari porcelain. He and his wife Chikako traveled several times to the Middle East, visiting Turkey a few times (K. Ilhan Ikeda and I did our PhDs with him at the same time), Iran for a conference, and Lebanon on two occasions, when it was a real pleasure to be able to host Goro and Chikako. He managed to combine the mathematical aspect of his trips with visits to museums and archaeological sites, plus the inevitable antique shops.

Some aspects of his mathematical legacy. Shimura's mathematical contributions are so fundamental and wide-ranging that no one person can write about them all. I will go over many important topics too quickly and will skip others altogether. I hope that this discussion can at least do justice to some fraction of his work.

Shimura knew thoroughly the earlier work on modular forms by Hecke, Siegel, Maass, Petersson, Fricke, and Weber, among others. He had also carefully studied Lie (and algebraic) groups from Chevalley's book, as well as algebraic geometry in the language of Weil's *Foundations*. By the late 1970s, though, his articles tended to contain less algebraic geometry and more analysis. He never worked explicitly in the language of automorphic representations but was very happy to move between the adelic language and explicit computations with real or Hermitian symmetric spaces and arithmetic groups. In general, his articles are complete, thorough, and very demanding to read in terms of the intricacy of the calculation. However, all the material is there, with very few errors (which usually get corrected in errata at the end of a subsequent paper). So a patient and determined reader can make it to the end but must just be prepared for a slow rate of progress per page.

Readers in number theory will be familiar with Shimura's foundational contributions to the arithmetic of modular curves and abelian varieties, such as the decomposition of the Jacobian of a modular curve. The section by Blasius in this memorial article summarizes his introduction of what are now known as Shimura varieties and their canonical models. I will only mention Shimura's important insight that in the non-PEL case one can use the CM-points to pin down the rationality and produce a canonical model over the "correct" number field.

Another famous contribution by Shimura is in the area of modular forms of half-integral weight, beginning with [11]. Shimura studied many aspects, not just over $\mathrm{SL}(2,\mathbf{Q})$ as in the first paper above, but also over symplectic groups over totally real fields, so in the Siegel–Hilbert case. Over $\mathrm{SL}(2,\mathbf{Q})$, as is well known, Shimura showed that to a Hecke eigenform f of half-integral weight $k = m + 1/2$ there corresponds a Hecke eigenform g of (even) integral weight $2m$, so on $P\mathrm{GL}(2,\mathbf{Q})$, with matching Hecke eigenvalues. Shimura's original proof of this went via constructing the L-functions of twists of g by Dirichlet characters and then invoking Weil's converse theorem. Later, after work of Shintani and Niwa and with further hindsight, this "Shimura correspondence" was recognized as an early example of a theta-correspondence, here between the double cover of SL(2) and $O(2,1)$, which is essentially the same as PGL(2). Shimura revisited his correspondence from this point of view in [21] and subsequent articles for the Hilbert modular case, and describes the theta-correspondence viewpoint over $\mathbf{Q}$ in a readable account for students in his last book, *Modular Forms: Basics and Beyond*, published in 2012. As another result using half-integral weight on SL(2), Shimura was the first to prove the remarkable result [13] that the symmetric square L-function of a classical modular form has an analytic continuation to $\mathbf{C}$. Prior to that, one had only a meromorphic continuation with possible poles at all the zeros of the Riemann zeta function or a Dirichlet L-function. This proof used a careful analysis of Eisenstein series of half-integral weight. Shimura of course studied many other aspects of half-integral weight on larger groups, including but also going well beyond questions about the behavior of Eisenstein series, as part of his large program on arithmeticity, which was a large focus of his work from the mid-1970s through the late 1990s.

Before I mention Shimura's work on arithmeticity, however, I will briefly mention his significant production of books and articles from the mid-1990s until 2012, during his retirement ("only from teaching," he once told me). In a 1997 monograph, *Euler Products and Eisenstein Series*, he broke new ground in the explicit construction of L-functions using essentially the "doubling method" of Gelbart, Piatetski-Shapiro, and Shalika, obtaining all Euler factors and gamma factors. Also, in two monographs, *Arithmetic and Analytic Theories of Quadratic Forms and Clifford Groups* (2004) and *Arithmetic of Quadratic Forms* (2010), Shimura obtained new results in the theory of quadratic forms and new explicit forms of the celebrated Siegel mass formula. He further summed up and refined in monograph form many strands of his earlier work that had previously appeared in articles: his work with Taniyama from the 1950s on complex multiplication in *Abelian Varieties with Complex Multiplication and Modular Functions* (1998); elementary and less elementary topics in modular forms, including the Shimura correspondence and the simplest cases of arithmeticity in the books *Elementary Dirichlet Series and Modular Forms* (2007) and *Modular Forms: Basics and Beyond* (2012); and a more comprehensive treatment of his program of arithmeticity in *Arithmeticity in the Theory of Automorphic Forms* (2000).

Shimura's program on arithmeticity, a large focus of his work from the mid-1970s onwards, can be viewed as a very large and elaborate outgrowth of the two seminal articles [12], [14]. I shall single out two themes: from the first article, the arithmeticity of the values of nearly holomorphic modular forms at CM points, and from the second, the arithmeticity of special values of L-functions of modular forms, and the relation of these special values to more fundamental periods attached to the forms.

The first theme above generalizes Shimura's reciprocity law for holomorphic modular functions at CM points to certain nonholomorphic functions, where the relation to algebraic geometry is less direct. In the classical context, a nearly holomorphic form is a function $f : \mathcal{H} \to \mathbf{C}$ on the usual upper half-plane which transforms as expected under a congruence subgroup of $\mathrm{SL}(2,\mathbf{Z})$. Instead of requiring f to be holomorphic, we require $f = \sum_{n=0}^{N} f_n(z)y^{-n}$, where $y = \mathrm{Im}(z)$ and the f_n are holomorphic. (Actually, for arithmeticity reasons, it is better to use $(\pi y)^{-n}$.) A typical example is the Eisenstein series $E_2 = (8\pi y)^{-1} - (1/24) + \sum_{n\geq 1} \sigma_1(n)q^n$. One can also obtain nearly holomorphic forms by applying certain differential operators to holomorphic forms. Shimura introduced an ingenious way to evaluate such an f at a CM-point $z_0 = (a + b\sqrt{-D})/c$, by comparing f and $f|\alpha$ for an element α that stabilizes z_0, and combining this with taking various derivatives. The generalization of this to larger groups is of course more involved.

The second theme, in the setting of the arithmeticity of the special values of standard L-functions of classical modular forms, can be studied in terms of the Eichler–Shimura cohomology groups or (as formulated by Manin) in terms of modular symbols. The new approach in [14] is quite different and allows for a generalization to many other groups and L-functions. In the classical setting, let f be a newform (in Shimura's terminology, a primitive form). Instead of considering a single (twisted) special L-value $L(k, \chi, f)$ for a Dirichlet character χ, Shimura considers products of two such special values, which he obtains via an integral of Rankin–Selberg type as $L(k_1, \chi_1, f)L(k_2, \chi_2, f) = \langle f, G\rangle$ for an explicit modular form G. Here G is a product of two Eisenstein series and can be expanded as $G = c_E E + \sum_i c_i g_i$, where E itself is an Eisenstein series, and the g_i are cuspidal Hecke eigenforms (not necessarily newforms; one can have $g_i(z) = h_i(N_i z)$ for a newform h_i). Since G, E, and the h_i have algebraic Fourier coefficients, the c_i and c_E are themselves algebraic, and then one obtains (after some more work) an algebraic expression for $L(k_1, \chi_1, f)L(k_2, \chi_2, f)$ in terms of those c_i where $h_i = f$. This is the heart of the idea in the classical case, and it generalizes somewhat directly to Hilbert modular forms. However, for the generalization to larger groups and other L-functions, one requires two significant inputs: first, a good understanding of the analytic (not just meromorphic) continuation of Eisenstein series on larger groups, with a precise proof of arithmeticity of their Fourier expansions, and, second, once again a thorough understanding for higher groups of the differential operators that already appeared for evaluation at CM-points. (The differential operators are needed even in the classical case but are more tractable there.) Tackling both of these questions in more general settings involved a large body of work by Shimura (and his students, in their theses) over some twenty-five years, and the computations are quite intricate. Besides the books mentioned above, *Euler Products and Eisenstein Series* (1997) and *Arithmeticity in the Theory of Automorphic Forms* (2000) and their references, I will single out the articles [19], [20] as a memorable illustration of the careful study that Shimura was able to undertake of the analytic continuation and Fourier expansions of Eisenstein series in integral and half-integral weight. The difficulty resides largely in the number theory, but there are also genuine analytic challenges in terms of special (confluent hypergeometric) functions on the symmetric spaces of these higher-rank groups.

Figure 4. Goro Shimura and Kenneth Ribet in the summer of 1973.

Kenneth Ribet

I first saw Goro Shimura at the Antwerp conference Modular Functions of One Variable in 1972. I was a graduate student at the time and was shy around senior mathematicians. Some of the faculty members at that conference encouraged informal contact with graduate students, but Shimura's body language did not convey any encouragement. I was further intimidated by a comment that was made by one of the Princeton graduate students: "Many mathematicians have knowledge. Shimura has wisdom."

Twelve months later, I had finished my dissertation work and was about to begin a lecturer position at Princeton. Shimura welcomed me when I arrived in the Princeton math department and gave me tips for dealing with students. In particular, he recommended that I respond to students' questions by writing down answers in their notebooks so that they would have easy access to my comments even after their memories of my remarks had faded.

Shimura then asked me a mathematical question that turned out to be extremely fruitful. Specifically, let N be a prime number and let J be the Jacobian of the modular curve $X_0(N)$ that classifies degree N isogenies between elliptic curves. Then J is an abelian variety over the field $\mathbf{Q}$ of rational numbers. Moreover, J has endomorphisms T_n ($n \geq 1$) that are geometric versions of the Hecke operators on modular forms that had been studied by Hecke and Shimura. These endomorphisms generate a subring $\mathbf{T}$ of the ring of endomorphisms of J over $\mathbf{Q}$. Shimura asked me whether $\mathbf{T} \otimes \mathbf{Q}$ is the full endomorphism algebra of J.

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Figure 5. Goro, Chikako, and Haru at the Ukiyo-e Museum in Matsumoto, Japan.

I found first that $\mathbf{T} \otimes \mathbf{Q}$ is the algebra of all endomorphisms of J that are defined over $\mathbf{Q}$, but this was not news to Shimura. He asked me whether or not there were endomorphisms of J over the algebraic closure of $\mathbf{Q}$ beyond the ones that are already defined over $\mathbf{Q}$. Using results that had been obtained by Deligne and Rapoport a year or two before, I proved that all endomorphisms of J are defined over $\mathbf{Q}$. Technically, I used the theorem of Deligne–Rapoport to the effect that J has semistable reduction at the prime N (and thus at all primes, because J has good reduction outside N); my result was really about abelian varieties with semistable reduction.

Shimura was delighted by my result and asked me to explain my proof to him in detail. In response, he wrote down a polynomial identity that simplified the main computation that I had presented to him. Shimura then suggested that we write a joint article with the result. Perhaps selfishly, I told him that I was reluctant to write a joint paper because I had never yet published any mathematical article. Shimura accepted my answer and encouraged me to publish the result on my own. I did so—and credited Shimura for posing the original problem and for the simplification that he made to my argument. I was grateful to him for this act of generosity, but now worry that I was wrong to decline his offer.

By the way, my theorem shows that $(\operatorname{End} J)/\mathbf{T}$ is a torsion abelian group. Here, $\operatorname{End} J$ is the full ring of endomorphisms of J, and $\mathbf{T}$ again is the subring of those endomorphisms that come from Hecke operators. In 1977, Barry Mazur proved that the quotient $(\operatorname{End} J)/\mathbf{T}$ is *torsion free* in his "Eisenstein ideal" article. Our results together imply that the quotient is *trivial*, i.e., that $\mathbf{T}$ is the full ring of endomorphisms of J.

Alice Silverberg

The way I became a PhD student of Goro Shimura was a bit unusual. Sometime in my first year, I asked Nick Katz to be my thesis advisor. He told me to talk to him after I passed my General Exam in the spring. After the exam, Katz went away for the summer. At a conference early that summer, I ran into John Coates, who asked me whom I planned to work with. When I said Katz, he exclaimed, "But Alice, you can't possibly work with Nick Katz! He won't be at Princeton. He's accepted a job at Berkeley."

I had no way to contact Katz to check this (and to find out that it wasn't correct). So I decided that I had better have a back-up plan. Former students of Professor Shimura had told me that the first thing he tells prospective students is to read the red book, *Introduction to the Arithmetic Theory of Automorphic Functions*. That summer, I started to read the book and do the exercises.

In the fall, I needed to talk to the director of graduate studies about an administrative matter. That happened to be Katz. One day after tea I followed him to the elevator and asked to talk with him. As we waited for the elevator he turned to me and said, "So are you my student or aren't you?" Based on his tone of voice, I reflexively responded, "No, I'm not." Then he asked me who my advisor was. Without thinking I blurted out "Shimura." Then, horrified at the thought that he might ask Shimura and find out it wasn't true, I hurriedly added, "But he doesn't know it yet!"

Luckily, Shimura agreed to be my advisor.

A former student told me that his experience was that he would bring a notebook to his meetings with Shimura. Shimura would write a problem in the notebook and ask the student to solve it for their next meeting. If the student didn't solve it, Shimura wrote the solution in the student's notebook. My experience was very different; I worked very independently.

Shimura suggested a nice thesis problem, and I went away and worked on it. He then took the problem away from me. I heard through the grapevine that Shimura had given the problem to a former student to do for his thesis, but the student hadn't solved the problem then and now wanted it back.

Next, perhaps to make up for the time I had spent on the first problem, Shimura gave me a choice of three problems. When I chose the one furthest from his interests at the time, he was pleased with my choice.

Shimura gave me a first step to solve and told me to come back in about two weeks to report on my progress.

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By the end of those two weeks I managed to understand the question, but I hadn't made progress towards a solution. I didn't feel that was good enough, so I didn't arrange to see him and kept working. After another two weeks I had answered the question, but I didn't feel I could show up after four weeks having only accomplished what I should have done in two. So I kept working and made more progress, but it never seemed like enough, given the time I had spent on it. After a few months, I realized that I needed to let my advisor know I was still alive, so I met with Shimura and told him what I had accomplished. He could have been angry that I hadn't kept him informed. Instead, and luckily for me, he was pleased with both the work I had accomplished and my independence.

If I were restricted to one word to describe Shimura as a thesis advisor, I would say that he was "responsible." That's higher praise than it sounds; conscientiousness seemed like a rare and unusual trait among thesis advisors when I was a Princeton graduate student.

As my interests moved away from automorphic forms and into cryptography, I found that I continued to use Goro's work, especially the theory of complex multiplication, which is useful for modern-day cryptography. I also used Shimura reciprocity to help construct an algorithm related to point counting on elliptic curves over finite fields.

Goro Shimura had very high standards. I do best when the standards for me are high, so I am very grateful to Goro for having high standards for me, for telling me that a mathematician *must* be an optimist, and for believing in me as a mathematician. While he didn't often communicate that he thought highly of me, he did it enough (to both me and mathematicians who made decisions about me) to have a positive effect on my life and career. I will cherish the memories of our mathematical father-daughter relationship.

Hiroyuki Yoshida

The impact of student protests in Paris in May 1968 spread to the world, and Kyoto University in Japan was swallowed up in big waves from the beginning of 1969. No courses were offered to students for one year. In the autumn of 1969, I visited, with classmates, Professor Hiroaki Hijikata, who was then a young associate professor, in his office to ask him to give us a seminar on number theory. I was a senior mathematics major at Kyoto University. Hijikata asked me what I was interested in. I responded, "Complex multiplication." I knew at that time, without precise understanding, the legend of Shimura–Taniyama–Weil on complex multiplication and Shimura's work on Shimura curves. Hijikata selected some suitable literature for a seminar, and I was able to proceed rather quickly and learned that Shimura was developing the theory of higher-dimensional arithmetic quotients of bounded symmetric domains. Hiroshi Saito also attended Hijikata's seminar.

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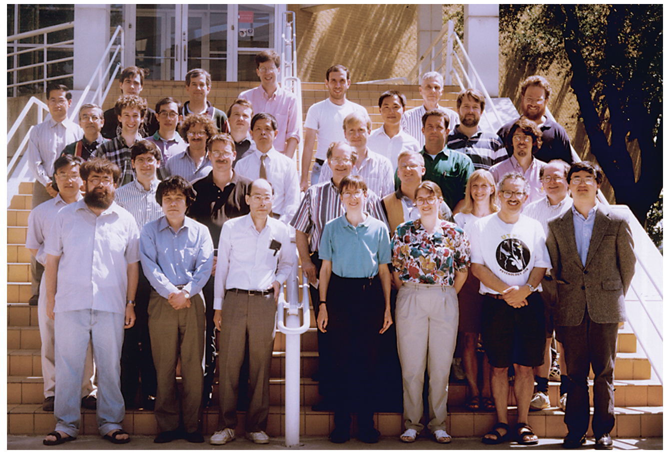

Figure 6. Group photograph taken at the NSF-CBMS conference at Texas Christian University in honor of Shimura in May 1996.

Next summer, Hijikata, through his friend Professor Yasutaka Ihara, introduced me to Shimura. In the spring of 1971, I was admitted to graduate school in mathematics at Princeton University with a scholarship, to start in the autumn. That spring, Professors Koji Doi and Hidehisa Naganuma came back to Kyoto from IAS in Princeton. I first met Professor Goro Shimura on July 14, 1971, on the Ishibashi campus of Osaka University. Shimura was invited by Professor Taira Honda to give a lecture in Osaka. My first impression was that he resembled the famous philosopher Kitaro Nishida of the Kyoto school.

Talks with Shimura in person. I arrived at Princeton on September 13, 1971, and met Shimura the next day in his office, and he kindly took me to his home. Shimura had just published his now standard textbook *Introduction to the Arithmetic Theory of Automorphic Functions*. I asked him, "What is your present interest?" He replied that he was studying modular forms of half integral weight. He hinted that he had discovered a relation between modular forms of half integral weight and modular forms of integral weight. A preprint became available only the next spring, and now this relation between modular forms of half integral weight $(2k+1)/2$ and modular forms of integral weight $2k$ is called the *Shimura correspondence* ([11]). A technical core of the proof is an ingenious application of the Rankin–Selberg convolution and Weil's converse theorem.

We also talked about complex multiplication of abelian varieties and construction of class fields. Though an abelian variety A has (sufficiently many) complex multiplications by an algebraic number field K, the class field is obtained over the reflex field K', which is different from K in general. Shimura explained this curious phenomenon, first discovered by Hecke in a simple case, as follows. Suppose that A is defined over a subfield k of $\mathbf{C}$. Then the field generated by the division points of A is determined as a subfield of $\mathbf{C}$. But K is not determined as a subfield of $\mathbf{C}$; only its isomorphism class has a definitive meaning. In contrast to this, the reflex field is determined as a subfield of $\mathbf{C}$ from K and the CM-type of A. I felt a revelation and got deeply interested in this "thought experiment."

In the summer of 1972, Shimura took me to the Antwerp conference on modular functions of one variable. One afternoon, we went out for sightseeing in the city. I was impressed that Shimura very efficiently found a route to visit museums and places of interest with a handy map. This experience turned out to be very useful for my travels in later years. He went back to his hotel after buying some paper of good quality for his calculations.

I received a PhD in 1973 under Shimura's guidance and stayed in Princeton as a postdoc until August of 1975. In 1975, when I was preparing to leave Princeton for Kyoto, Shimura was studying critical values of L-functions associated with modular forms. This time the Rankin–Selberg method was employed again. The basic formula is

$$(4\pi)^{-s}\Gamma(s)D(s,f,g) = \int_{\Gamma_0(N)\backslash H} \overline{f(z)}g(z)E_{k-l}(z,s+1-k)y^{s-1}dxdy. \quad (1)$$

Here s is a complex variable, H is the complex upper half-plane, and $z \in H$ is written as $z = x+iy$, $y>0$, $x \in \mathbf{R}$; $f(z) = \sum_{n=1}^{\infty} a_n q^n$ and $g(z) = \sum_{n=0}^{\infty} b_n q^n$, with $q = e^{2\pi i z}$, are holomorphic modular forms with respect to $\Gamma_0(N)$ of weights k and l, respectively, with $k > l$. For simplicity, we assume that the characters of f and g are trivial (Haupttypus). For $0 \le \lambda \in \mathbf{Z}$, $E_\lambda(z,s)$ is an Eisenstein series defined by

$$E_\lambda(z,s) = \sum_{\gamma \in \Gamma_\infty\backslash\Gamma_0(N)} (cz+d)^{-\lambda}|cz+d|^{-2s},$$

where

$$\gamma = \begin{pmatrix} a & b \\ c & d \end{pmatrix}$$

and

$$\Gamma_\infty = \left\{ \pm\begin{pmatrix} 1 & m \\ 0 & 1 \end{pmatrix} \middle| \, m \in \mathbf{Z} \right\}.$$

Our first objective is to study special values of $L(s,f) = \sum_{n=1}^{\infty} a_n n^{-s}$. Suppose that f is a Hecke eigen cusp form. Then there exist periods $u^{\pm}(f)$ such that $L(m,f)/\pi^m u^{\pm}(f)$ is algebraic for $1 \le m \le k-1$, $(-1)^m = \pm 1$, $m \in \mathbf{Z}$. (A cohomological approach was discovered by Shimura fifteen years earlier ([2]).)

Moreover, $u^+(f)u^-(f) = i^{1-k}\pi\langle f,f\rangle$, where $\langle f,f\rangle$ is the normalized Petersson norm of f. Shimura stressed the importance of $D(s,f,g)$ and not only $L(s,f)$. Suppose that g is also a Hecke eigen cusp form. Then $\zeta(2s+2-k-l)D(s,f,g)$ has an Euler product of degree 4 and may be denoted as $L(s,f\otimes g)$. Then $(2\pi)^{l-1-2m}L(m,f\otimes g)/u^+(f)u^-(f)$ is algebraic for $l \le m < k$, $m \in \mathbf{Z}$ ([14], [16]). It is very interesting that the form of higher weight gives the dominant contribution. Shimura discovered an ingenious but now standard way to deduce these results from (1).

Shimura was a great master of using the Rankin–Selberg convolution to squeeze arithmetical information from it. In the half integral weight case mentioned above, f is a form of half integral weight, g is a classical theta function, and E is an Eisenstein series of half integral weight in (1). In the later years, Shimura generalized the method to higher-dimensional cases.

In the summer of 1985, Shimura suddenly called me. (In the meantime, I was communicating with Shimura by occasional letters.) He told me he had come to Kyoto with his wife but with no business. His wife was visiting her friend. So we went to Kurama temple for sightseeing. In the bus from his hotel to the local train station, I talked about the then-popular movie *Amadeus*. I talked about the sad destiny of Salieri's music. Then he said, "I am Mozart" immediately. Shimura also declared at this time that the modularity conjecture for elliptic curves over $\mathbf{Q}$ was his. Shimura explained to me some interesting work of his recent PhD students. The name of Don Blasius was among them. I think Shimura wished to give me some stimulation. We parted this time after he promised to visit Kyoto University in the summer two years later.

Shimura had a summer villa in Tateshina, Nagano prefecture, Japan. After 2011, Shimura invited me three times to visit his villa. The villa was acquired around the time when Shimura moved to Princeton from Osaka. An old, rather small building was standing in a large site. Tateshina is very nice to stay in during the summertime. Nearby are villas of celebrities. Shimura spent summers here with his family every couple of years during his professorship in Princeton. His study was small, less than 5m^2. It is amazing that monumental papers were written in this place. As I was accompanying a young mathematician, I asked Shimura what would you do in mathematics if you were young. It was around 2013. He responded that he would study Siegel. In fact, volume V of his collected papers contains several important papers on quadratic forms, and the influence of Siegel and Eichler was manifest.

Shimura sometimes told me that he loved "tricks" in mathematics. The trace formula and the Rankin–Selberg convolution are examples to him. A long winding road of reasoning leads to a simple theorem, illuminated by concrete (sometimes numerical) examples that can be seen by everybody. This is a tradition among number theorists from ancient times. For a layman, this may look like magic. I mention [2], [6], [11], [14], and [25].

On a few occasions, we talked about big problems such as the Hodge conjecture and the Riemann hypothesis. Shimura was negative about attacking a big problem without having good ideas. But he had the opinion that for the Riemann hypothesis, differential operators will play a crucial role. An interested reader may consult [23], [24].

In Shimura's class. Shimura's students know that the master has excellent skills of exposition and is also an entertainer.

In a lecture around 1972 at Princeton University, he explained the theory of canonical models, now called Shimura varieties. To study the model further, he said, "To desingularize or not to desingularize: that is the question." Everybody, knowing *Hamlet*, enjoyed the performance and laughed. (This incident is recorded in his book written in Japanese *How to Teach Mathematics*, p. 20. This book is the last item of the bibliography of volume V of his collected papers.) Also in another lecture around the same time, he talked about exotic ℓ-adic representations constructed using the theory of canonical models ([10], and §8 of "On canonical models of arithmetic quotients of bounded symmetric domains. I"). He said that the eigenvalues of the Frobenius automorphism have the property of "Riemann–Ramanujan–Weil type." Everybody enjoyed it and laughed.

In a lecture around 1989 at Kyoto University, he talked about the critical values of Dirichlet series and periods of automorphic forms ([22]). Automorphic forms are of Hilbert modular type, and L-functions are of standard type or of Rankin–Selberg type. But he considers all configurations of weights including the half integral weight case. Shimura's exposition was clear, but the situation is complicated and divided into several cases. For the half integral case, the period to be considered is the minus period of the integral weight form that corresponds to the half integral weight form by the Shimura correspondence, while it is the product of plus and minus periods when the form is integral weight. He explained this by saying, "The period becomes the half because the weight is half." Everybody laughed but this time with some feeling of relief. (To catch the point quickly, the reader is advised to see the introduction of [18].)

In Paris in 2000, after finishing a talk at a conference, Shimura was asked to give advice for a young audience by the chairman. He replied, "Don't prove anybody's conjecture," and everybody laughed. This episode is recorded in *How to Teach Mathematics*, p. 33. In this book, Shimura explains this advice in some depth.

I stayed at IAS in Princeton for 1990–91. In the spring of 1991, Shimura showed me reports of senior students evaluating Shimura's course. Most students evaluated the course highly; a few said that they never attended such splendid lectures during their student time in Princeton. Shimura told me that they were not mathematics majors; he lectured on number theory with an emphasis on historical perspectives. He was very proud of the students' evaluations and saw some of them in his Princeton home for decades.

Shimura published two autobiographies; one in English (*The Map of My Life*), one in Japanese. The contents are basically the same, of course, but they are independently written. In the book, he wrote that when he was young (1952–56), he taught at the University of Tokyo and also in a preparatory school because the salary was so low. Hijikata told me that he was then in preparatory school and impressed by Shimura's lecture; that experience led him to study mathematics.

Perhaps I should comment briefly on the historical facts concerning the Shimura–Taniyama conjecture, i.e., the modularity conjecture of elliptic curves over $\mathbf{Q}$. The issue is analyzed in detail in his autobiographies. The English version gives a more detailed account. He understood well that the issue was quite social. I quote one paragraph from Shimura's book: "The reader may ask why there were so many people who called the conjecture in various strange ways. I cannot answer that question except to say that many of them had no moral sense and most were incapable of having their own opinions."

As a man of culture. We had several interests in common, so it was not difficult for me to start a conversation with Shimura. One interest is Japanese chess (*Shogi*). The chess board is 9×9, and we can use captured pieces again. The other rules are basically the same as the western chess game. When I visited him in his Princeton home, he showed me a Japanese chess problem (*Tsume-Shogi*) composed by him. When I showed the correct answer of 71 moves written on a paper in my next visit, Shimura was very pleased. I still keep this problem and another problem of 169 moves. Shimura said he composed chess problems when he was very young, spending considerable time.

As my family served as Buddhist priests for centuries, I had some training to read Buddhist scriptures in Chinese translation from my childhood. Shimura liked to read Chinese classics and published two books. He also had some original perspectives on Buddhism.

Shimura loved music very much. His writings about music are scattered in many places in his books. In my first visit to his home, he played the well-tempered clavier on a record player. In later years, he regularly visited the Metropolitan Opera with his wife.

Shimura had an interest in antiques. He published a book on Imari porcelains. He had this interest since he was young; he wrote so in his book written in Japanese. But it was enhanced by a famous professor of oriental art at Princeton University, I think. Shimura's wife told me that her husband had a very strong memory of shapes, perhaps stronger than that professor.

Final recollections. Writing this article, I felt strongly the coherency of Shimura's character. Time has passed, and I am now much older than Shimura was when we first met. Nobody can stop aging and change their ultimate destiny. What I can be proud of, if anything, is that, after 1987, I invited Shimura about ten times to Kyoto University to give intensive courses, which must benefit students and young researchers. I also helped Shimura to edit his collected papers I–V with Alice Silverberg and Doi. But compared to what Shimura gave me to live as a mathematician, they are very small contributions. I thank Professor Goro Shimura again, this time with the deepest feeling of loss. Shimura is not among us anymore and will not respond to our emails. But his mathematics will live like the music of Mozart.

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Credits

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