有朋自遠方來 (數學傳播 **174**, 2020)

A: You know what this series of interview, the title of it.

B: Yeah, Confucius. The friend comes from far. How it is fun, or maybe, isn't it fun? Something like that?

A: So let me start with an offbeat question. It is my impression, which must have some validity, that is Japanese have made a lot of contribution to number theory, and they seem to like number theory a lot. And of course there's Taniyama and so on. Is this the cultural thing you think?

B: I think so, because Japanese mathematics started very early, even in the 16th, 17th century. In some Shinto Shrines, there's a competition to solve, say Euclidean geometry problems or number theory diophantine problems. If you win, then you are recorded by a rectangle wooden plaque of winning person's name with a description of the proof and possibly a geometric picture called 算額 (pronounced "sangaku" in Japanese). The plaque is dedicated to the Shinto god. They are at many shinto shrines in Japan. It's a kind of competition. So some of the famous Edo period (the shogunate time) mathematicians regarded this winning record as a prize. There discussed a lot of number theoretic questions, Diophantine equations and Euclidean geometry questions in addition to computing numerically a special number, say, π. So it seems, Japanese loved hard thinking. It is a way to come closer to Shinto gods. That is why they did the competition in Shinto shrines. This is purely a Japanese habit. It is not a Buddhism influence. It is a part of Shintoism.

A: I see. When I see the Japanese cuisine for example, the presentation of the dish is very important, not just the material. How to arrange the sashimi, and so on. For my un-initiated mind, this seems like very discrete kind of thinking. For example, ramen is not the typical Japanese dish.

B: Ramen is imported from China.

A: Yeah, so this is not the typical Japanese. The presentation of Japanese foods is very discrete and has a pattern , the beauty of it, origami, and so on. It seems to be very Japanese.

B: It is a traditional Japanese culture. Because the Japanese islands are so isolated from the continent. Especially in the early times, say, in the tenth century. It's so far from the continent, it's not like England. You cannot reach the continent by swimming. Because the current is quite strong and the distance is prohibitive. Even by ship, it is very difficult to reach the continent from Japan. So that is why we developed a peculiar Japanese system. Japanese aristocrats and the emperors in the 9th, 10th, 11th centuries, they had, how I can say, garden poem parties "歌会"(pronounced either "kakai" or "uta-kai" in Japanese). We also call a banquet in old Japanese "Utage (うたげ)" literally "food with poem and song". Here "うた" means "song/poem" and "げ"means "food". At a garden party, participants exchanged poems. They compete quality of poems made at the party ad-hoc (possibly praising in appearance delicacy of season's flowers with hidden meaning of romantic feeling towards another participant). Creating a poem with double or triple meanings, enjoying sophisticated food and drinking sake in an aesthetic way. That, you know, is also another way of coming closer to Shinto gods. This is deeply ingrained in Japanese minds at the time. For example, Japanese way of presenting food you mentioned has its origin in this garden party banquet with some later influence of tea celemony from the culture of Samurai and also Japanese people in commerce class. It is aristocratic thing with a touch of Samurai. So it is different from ramen or Japanese curry rice, which was imported from England. According to my Indian students, Japanese curry is very far from Indian curry, it has now become a Japanese cuisine, but its presentation is different from the original Indian way.

C: It tastes different.

B: Yeah, totally different. But some may not come from this aristocratic type. Later in the peaceful shogunate era, you know the samurai also became aesthetic. Of course, samurai means originally some sort of well-trained warriors, but once we reached peaceful time, they become very much principled and well-learned people. They learned very well from this aristocratic tradition, but more discipline is there than aristocracy. So this samurai type of cuisine is slightly different from the earlier ones. It is more outstanding in Tokyo area compared to Kyoto-Osaka area. The sequence of dishes getting shorter. However, the amount of food got slightly bigger for each piece. Sashimi is exactly this type. To make good sashimi, you need to have very sharp knife (and mind). That comes from samurai culture. At the time of aristocracy, the 10th, 11th century, because you could not bring raw fish from the sea immediately, they mainly ate dried fish. Present raw sashimi and sushi are developed later in the samurai period. Sushi master often has difficult personality, very proud of himself. Often, at the backside of the sushi shelf, if he is proud of being himself, he has a Japanese sword presented, hung over the wall, and sometimes he shows the sword proudly to the customers. Since I am from the Kyoto-Osaka area, I do not like such behaviour, but for Tokyo people, it could be something attractive.

A: I would like to go ask some questions on number theory. I'm not an expert, so my question is very superficial at best. Andrew Wiles came to Stanford and gave a general public talk for Fermat's Last Theorem starting with ancient Pythagorean triples and so on. Then there were some questions from the audience at the end. One of the questions is did Hardy make contribution to the ultimate resolution for Fermat's last theorem. And Andrew Wiles said no, but Ramanujan did. What is your take on this? Who are really most responsible for the Fermat's last theorem's final resolution? We all are told that Taniyama is important.

B: I think, of course, one of the key points is to relate elliptic curves to modular form, and this is the Shimura-Taniyama conjecture. But I think the most important thing is, not many people would say, that Frey found the meaning of each integer solution (if any) of Fermat's equation. You pick a solution A, B, C of Fermat's equation. Then out of that he created an elliptic curve with very funny property, making it probably non-existent. How you can prove non-existence? There the Shimura Taniyama conjecture comes in. You can relate the given solution to the Galois presentation made out of the elliptic curve Frey associated. It is then related to or is given by a modular form by the conjecture, and it is easy to show no such modular form exists. This is an obvious thing which Ken Ribet pointed out. Of course, Andrew's contribution is deep. But an initial input is perhaps from Frey and Ken Ribet.

A: I see.

B: When Ken Ribet found this after Frey's work, he gave a lecture in Paris in the 1980s all I could remember. I was sitting beside Jun-ichi Igusa in the lecture hall. Igusa was quite excited listening to Ribet's talk. "Oh good, if you can relate this elliptic curve with a modular form, you are done." Oh, "Shimura and Taniyama are great" he said. So I told him that you just reduce the difficult conjecture to another difficult one. Why you are so happy? He got angry at me. "A mathematician shouldn't have such an attitude", he said. Anyway, I was and am not so much interested in solving a hard question posed by somebody else. Therefore, my first impression was that I would never try. But Igusa is a different type of person somehow, and he was quite impressed by that.

A: Was Taniyama trying to relate, in a very vague sense, an algebraic thing with an analytic thing?

B: Yeah, sure. Because the number theory is the unique branch of mathematics, which has theory in the name. But actually, there is no theory in it in a literal sense. It is just a collection of hard problems. To solve them, a number-theorist just makes whatever efforts and use of whatever thing available, not necessarily analysis, not necessarily algebra. One of the Langland's contributions is bringing harmonic analysis (with representation theoretic techniques) into number theory. And Erdös's contribution is bringing into, perhaps, a statistical way of thinking. So the number theory encompasses everything. In other words, we don't have theory on its own. Therefore, we need to mix up. That is the main endeavour to solve a hard question.

A: Taniyama was quite young when he come up with his problem. Before that he met André Weil. And there was an article. I somehow came upon it. I think you must know this article.

B: Yeah, I know the Japanese version.

A: Yes, it was first published in Japanese.

B: This is in Sugaku, in one of the very early volumes, something like that.

A: For a young guy to write this article is quite amazing. He actually criticized André Weil in some sense. There was Shimura's account of Taniyama early on. He was very amazed how much mathematics Taniyama has digested at a very, very young age. Do you have any impression of Taniyama?

B: Well, I know Shimura well, but I never met Taniyama. He died young. But I got some story from Shimura. But anyway, you know these two mathematicians have a quite different personality. I think, Taniyama is quite strong in math, but rather living in dream. He sometimes had trouble of formulating things precisely. Shimura doesn't dream much, I guess. But once he got an input, he could go really. So that's a very good combination. And these two people are not just friends, also enemies. You know, if one is a really strong mathematician, a friend is possibly also an enemy at the same time. Obviously, you can't cope all the time well with the person so strong. I mean, you sometimes get angry about his dream, which you don't believe it in totality, and shut up your lips. Afterwards, it would sink into your mind deeper later. And then something pops in your mind and you want to talk to the person again. Then the person is of course happy. In this way they are friends. This is perhaps quite the same in the relation between Shimura and André Weil. Shimura had deep respect towards André Weil, but also often criticized him openly. And Weil also did the same. In Buddhism or Hinduism, one reincarnates into a new life. Somebody asked Weil, "what you want to be in a new life if this is true?" Weil replied he wanted to be a cat in a Chinese house. Why? Chinese house has so many things to read. Weil could manage many languages like Sanskrit or Portuguese, whatever. Possibly, his regret was that he couldn't manage Chinese well. On the other hand, Shimura can read Chinese, absolutely fast at will (and I can also read such fairly well). He once told me in Princeton University, in a library, there is a good collection of Chinese classic literature and a huge amount of Chinese books. He asked me, "have you ever read some of them?" I answered, perhaps I read a hundred or less while I was in Princeton. A hundred he said? Then he made a remark that he had read almost all of them three times. So this, perhaps he told André Weil. André Weil felt somewhat jealous. Of course this is my guess, so it is a sort of criticism I felt from Weil's answer, I would say. But that is my guess. I don't know really.

A: Wonderful. Someone in number theory told me that Shimura also considered a particular thing, and that it was not clear why Shimura considered this particular thing. But it turns out to be a central thing, such as the Shimura variety and so on. Do you have this kind of impression? Why Shimura consider this particular thing?

B: Well Shimura said that he intended to create something. So he shot an arrow. Obviously missed it, he said. Then what you could do? Make the target bigger, so that your arrow was already in the center. That is the Shimura variety Shimura described, I believe. Perhaps he intended to solve something that I don't know what it is, but at the beginning, it was far off. So he just made things bigger so that the arrow actually hit.

C: So Professor Hida, when did you get interested in mathematics?

B: I was…I didn't have any interest in math until I was in my junior year of undergraduate study. First of all, I never had any interest in anything up to that time. Although, I had been fascinated in reading Japanese classics, Chinese classics, and all over. I could read Japanese, French, German, English at very early age. So I read a huge amount of these. That was fun, like re-live the life of somebody-else, but that was not the purpose of the life. Somehow I was very clever, whatever an entrance exam given, I passed, usually barely passed, but anyway passed, I almost never failed. Though I did no training for up-coming exams, but naturally I already knew a lot of things. The university I entered (Kyoto University) was closed for my first two years, because of the Maoist movement. Kyoto University was really radical at the time. So the first two years, no education, nothing. So what I did? I just taught many high school students to be successful in the entrance exam of Kyoto University (strangely they kept offering their entrance examination). I got a lot of salary by doing this, so I was quite affluent (and my family was also affluent). So I had a lot of money and spent also for a lot of things, did nothing meaningful. I was a chemistry major. That means you need to do a lot of lab work to graduate. Then third year, my junior year, the university suddenly opened (after expelling radical students). Somehow the university decided to give the two years credit to everybody. Then I needed to graduate within two years in a decent way. What you can do? Chemistry? No way, I thought. At the time, I had a friend who was a kind of a math addict. He attended some of the private seminars for graduate students. One of them was on partial differential equation. He himself wanted to be a topologist. Anyway, he suggested me to come to this seminar about partial differential equation. They were reading a book of Hörmander. I didn't know anything. But my friend asked me the meaning of some theorems in the book. "You ask me this? I don't know math". "Well, you are far cleverer than me, so you can". So I started studying analysis. Within weeks, reading L. Schwartz's "Cours d'analyse" and some algebra out of Bourbaki. I studied and studied (or read and read). Then this Hörmander's book has became very interesting to me, so I started presenting some proofs in the seminar. The only math book I read at that time was Bourbaki's "Algebre" and "Algebre commutative" and Hörmander 's book on linear partial differential equations. And that was the start. Then somehow I met Doi, and he was impressively funny. First of all, he is so incredibly number theoretic. And when he finds something numerically, his joy is overwhelming. He could then start drinking sake and continued for the entire night, talking about his mathematics. How happy he was. I never met this kind of person earlier. Math is so addictive, I thought, I might try. That was the start of my study of number theory.

C: So you pursue your PhD with Doi? Or?

B: No, because for example, Shimura perhaps doesn't have PhD, and Taniyama perhaps not. And this was not very rare in Japan at the time. Because these two people so advanced out of the time, under the Japanese education system so rigid, they did not fit well with old-fashioned professors at the time. They perhaps fought with them, two of them. Both of them is possibly out of elite education system and had to teach a lot of low level undergraduate courses in liberal art colleges (University of Tokyo having two campuses at the time, one for liberal arts and another for advanced study). Then Shimura had to move to Osaka University. In my case, I did not fight, but I was a total outsider anyway. I talked to Doi, usually in a bar, not really in the math department. So after that time, I drank a lot. Doi could drink one big bottle of Japanese sake. I could also do that at that time. So that made us very much friends each other rather in a non-traditional way (not like a professor and a student). Then he left for the Max Planck Institute (for Mathematics in Bonn) for a couple of years when I was admitted to the graduate school (again I barely passed the exam). I found that I was an isolated outsider because all the math department graduate students were math majors graduated from the mathematics department of Kyoto University. I was graduated from Kyoto university as an undergraduate, but I rarely talked with math students, except perhaps Mori a couple of times. Then Hiroyuki Yoshida came back from Princeton as a postdoc. He was a student of Shimura. And he was perhaps the unique person at the time who understood well Shimura variety, and I already read most of Shimura's papers up to Shimura variety by the time. So I talked to him. After two years in the master course, without Ph D, I got an offer of a postdoc job at Hokkaido University (helped by Doi who moved to Hokkaido from Germany). If you get an offer so early within two years from the undergraduate time, you take it, right? I realized I needed PhD to be promoted. So Doi and I asked Professor Masayoshi Nagata, he was a head of the algebra group at Kyoto University, and he made a Ph D committee chaired by Professor Hiroaki Hijikata for me. By the time I wrote a couple of papers. So I got a PhD. So I don't have any advisor.

C: But in your early stage, you studied adjoint L-values and congruence number, but at that time Doi also studied that kind of thing.

B: Yes, but that's not my thesis. My thesis is on the classification of CM factors of the Jacobians of Shimura curves. And my Master thesis was also about CM factors. Around that time, Doi discovered congruence primes numerically, so as usual, he was wonderfully excited. He asked me, "if one numerically finds congruence by a prime computing first several Hecke eigenvalues, how you can say that congruence will go on to all Hecke eigenvalues". Of course, for weight 2-forms, as Ohta proved, using Rieman-Roch, one can find a bound for how many first eigen values are necessary to guarantee the congruence for all. I told him that there is a computationally easier way to show the existence of congruence valid for all weight. You just write down some integral form into the linear combination of Hecke eigenforms. You find the denominator of the linear combination coefficient of the starting form. Then that is the congruence number, and that all you need for the validity of congruence for all Hecke eigenvalues. He's absolutely happy, and then he asked me. "if there is a way to move the denominator into a numerator of some value". I answered him that this denominator is the adjoint L-value (or equivalently the algebraic part of the self-Petersson inner product of the starting form). This comment made him to start having respect towards me.

C: Oh, I see. Okay. But later you keep studying the adjoint L-values very seriously until now. Still adjoint values seem one of the main themes in your research I guess.

B: I think it's the easiest among L-values. That's why.

C: Because you wrote many papers about this kind of thing. And so in your 1968 paper, you discover this Hida family of big Galois representation.

B: 1986.

C: 86, yeah, 86. And in the end of paper, you applied this big Galois representation to this adjoint values and congruence number. So how did you find this big Galois representation? How did you know this big Galois representation could have some application to this kind of problem in the beginning?

B: So to make a family of modular forms and Galois representations?

C: Yeah, so how did you arrive at the idea of constructing this family of Galois representation? Because at that time, there was no such kind of…

B: I was at the Institute of Advanced Studies from 79 to 81. Why? The reason is that I went to Shimura's lecture when I was in the junior year December or so. AMS notice would publish in May my memorial essay about Shimura. I described my first encounter with him. Anyway I attended Shimura's lecture in Tokyo University of Education that is now called Tsukuba university. I understood very well about his topic on fields of moduli of CM abelian variety. After the lecture, senior PhD students were invited to another room, a smaller room to pose him a question. Though I was an undergraduate, anyway I walked into the room. Shimura is kind of, sort of, a samurai type of principled person. So you could be a little afraid of him. Especially Japanese students in the room perhaps felt that way. So nobody asked him a question at the beginning. That's no good I thought. So I started asking questions. Then he answered me very well. And then again an awkward silence. I decided to ask again a question. This one is not so much well done. He got somehow excited (perhaps not angry). And he told me that you have posed some questions. Somehow trying to get some information of your conjectural thought or maybe dreamy way of thinking, to get something good for you. It is something like stealing. It is morally not sound. At the end, he said, "Do your own mathematics." I was impressed. Oh, god. Oh, do my own mathematics (I never thought of mathematics in this way before). So I started doing my own mathematics. Actually, Shimura might be perhaps a little impressed by my questions. After a couple of years from this first enounter, I wrote my master's thesis, classifying the CM factor of the intermediate Jacobian of Hilbert modular varieties. This tells you some period relation, of course. This I didn't write in the paper, but Shimura was working at that time in a more general setting about such relation of periods. Shimura was perhaps impressed by that because I wrote him about my results. Because of that, he supported my application to visit IAS, and I got the institute membership without PhD. I went to Princeton.

Once I reached Princeton in 1979, Shimura told me that "you come to Department tea every week, we chat". "Chat" means what really? He did not seem to me to teach me anything. I thought perhaps, "it then means I need to present something interesting to him." At the tea, he just listened to me and then possibly criticized me or told me whatever thing he came up. So I needed to create something, or I needed to say something. From time to time, some Chinese literature or some Japanese literature or some Mathematics. Pre-concoct some sort of new thought and tell him. Of course he could say "my idea doesn't work". In any case, I needed to produce something every week. Out of these conversations, I wrote three papers, the 81 paper about congruence number and so on. The first year, somehow I survived the meetings well. Second year, I got the extension to stay longer at IAS because these three papers were well received by the people there. Well, the second year was hard. I started a more systematical thinking about this congruence number. I talked with Langlands. In his and Harish-Chandra's theory, the cuspidal spectrum of the Hilbert space of automorphic forms is discrete. I asked myself, why discrete? If discrete, you can't go by one place to another. Somehow you need to change the base topology if one wants continuity to get an information of other automorphic forms from a starting one, that was the first impression I got talking with Langlands. More precisely, why Archimedean case is discrete? Because our world is arc-wise-connected. The spectrum becomes discrete because the inner product exists. Therefore, the base topology has to be ultra-metric to have a continuous spectrum, I thought. So I started trying deformation of a given modular form (i.e., continuously moving modular form under an ultra-metric topology). I talked about this idea to Shimura, that this would probably produce something like GL(2)-type version of Iwasawa theory. Shimura was happy first time. Next week, I told him similar things. He asked me "Have you ever invented anything else in the last week?" Well, first of all, this is a big project you can't do it just by a week. "Sure, you can't do that within a week. But you should have another idea. Then you would have a variety of ideas and by these, you can survive longer". That is somehow his way of doing things. If you shoot something and you miss it, make it bigger. That type of philosophy. In any case, it was a difficult time. However, I gradually developed the theory. And after getting back to Japan, I proved in December 1981 the result published in 1986. I sent the first draft to Shimura, Coates , Mazur and so on whom I met at the institute. But not many people seemed to believe my result as the results were totally new. So it took four to five years to be published. We did a seminar on the result in Paris including myself, Richard Taylor, from time to time, Andrew Wiles, and some other young students of Coates. We did read the draft. These seminar participants are perhaps the referees, though I do not have any real evidence.

C: It's a secret.

A: But the story should be in the record.

B: So in this way, it was published. I was in Paris from 83 to 86, and for a while we did the seminars. And this is the main purpose and is the main reason, I guess, I was invited to Paris for such a long span of time. Shimura's contribution is in some sense great. He never taught me any mathematics, but really pushed me to do something. What he said at the beginning, "do your own mathematics", it is his principle anyway. He somehow enforced it on me.

A: But he has a sense that you could do it. He was testing you. Yeah, but he would not say that to anyone, I would think so.

B: Well, at our first meeting at Tokyo university of education, nobody-else asked him serious questions if I remember correctly. Only I did perhaps.

C: You were the only one. So Mazur's Theory on Galois deformation theory was after your paper.

B: Yeah. So when I was in Paris, at the very beginning, I think, in summer 84, he came to IHES. I was just arrived from Japan. I once went to Paris in 1983, but I returned to Japan and coming back to Paris again, and stayed at IHES in 84 until 86. It was June or July, I don't remember when exactly. Somebody in the next morning knocking at the door of my IHES apartment. "Well, who he/she is?" I opened the door. Mazur stood there. I told Mazur that I just arrived and I would see him the next day. Next morning, I went to his office to chat. I was so sleepy, but he explained his deformation theory. He said "because of your stuff, I got it". He was rather excited. I told him that he just gave a natural ordering on all the Galois representations congruent to the starting one and then he took a projective limit, getting the universal deformation, is this correct? He replied, if I remember correctly, "to a good extent". After all, this worked so well. Those were crazy days.

A: He wrote a paper on your work, right?

C: You went there. Okay, next. So this question is about theory of p-adic modular forms. So theory of p-adic modular forms was developed by Serre and Katz in early 70s. And later not so much progress was done during 80s I guess. It seems during that time, no one really tried to generalize Katz's ideas until your paper on this control theorem for coherent cohomology. So how did you decide to do this through coherent cohomology, because I think before you were working on control theorem for topological…for Betti cohomology

B: No, it's reverse.

C: Oh, later.

B: Publication of the topological paper was earlier. Springer is more speedy in publishing than Annales Ecole Normale Scientifiques. So this Springer paper in 86 is the one you know, and the Ecole Normale paper dealing with coherent cohomology took longer time to be published, even though it was accepted perhaps earlier. The Springer published faster, so it's reverse.

C: So in the 80s, not so many people were studying this p-adic modular forms for higher rank group , right? My impression, that's my impression, but I am not so sure.

B: I am not so sure.

C: Because I only read your papers. JIMJ paper.

B: Of course, you know Serre did it and Katz created this theory.

C: Right, but Gouvêa , too.

B: Yeah, too. And for higher group, once your initial input is given, it is not something so difficult. I needed my theory to be extended to the general reductive group with Shimura variety. The only way is through coherent cohomology as topological cohomology groups get fuzzy because of torsion. So the only way is using this Katz's strategy. That is not so hard. Because nobody except me had incentive to do that. Obviously I had the incentive. And I had sufficient skill in doing this. And that's it. Later, I wrote a book and so on.

C: You mean yellow book? It's a yellow book?

B: The first yellow book.

C: Oh so at that time, so you said you had some motivation to generalize things to higher rank groups. So do you have application of these theorems in mind at that time?

B: At that time, I made a lot of p-adic L-functions. Like Fabian is doing now, once you can prove a good control theory, I might be able to do that for higher rank groups. But actually, I did not do that. A reason for not persuing this direction is that first of all, my many variable p-adic L-function was so unpopular at that time, and nobody cared about them. For traditional Iwasawa theorists, the traditional cyclotomic variable is sufficient. And that is somehow god-given by Iwasawa. So why you add more variables? So I really tired of it. So I stopped making p-adic L-functions at some point. Effort was made, but then I found that I could prove other things, say, q-expansion principle. Other things, they were not intended, so I became very busy, and I didn't try too much going for p-adic L-functions. Now some young people are trying to make p-adic L-functions of many variable out of bigger reductive groups (going the path I planned once).

C: So later you wrote papers on the nonvanishing of L-values. So it's 2004 to work about the paper, and 2010, Annals paper. It seems to me, you changed the style to these nonvanishing problems.

B: No, the point here is that I prove a oneside anticyclotomic divisibility with J. Tilouine in the 1990s, i.e., Katz anticyclotomic L divides the corresponding Iwasawa power series. And to prove the anti-cyclotomic main conjecture, i.e., the equality, I needed nonvanishing.

C: Oh, at that time you prove inequality up to μ-invariant.

B: We proved that L-function divides the characteristic power series up to p-power. As for the reverse identity, I could not do at the time first of all. And also this problem of μ. I changed the course, and first I tried to show μ=0.

C: But it was 1993, and you came back until after almost 10 years.

B: No no. The main point is I studied all the time this kind of integrality of automorphic forms and something you know, the integral model of Shimura variety is necessary to do that. So that's why I wrote the first yellow book (and so on), and made a p-adic integral theory about it, not just making p-adic L-functions. That is also one of my purposes. So this I really did. Though, determing μ-invariant of Katz p-adic L was very difficult, actually. This vanishing of μ uses in depth the q-expansion principle Ribet proved and I reproved in the book. In any case, this μ-invariant paper is so difficult, it takes about ten years to be published. I made a lot of errors, and Ching-Li Chai corrected some of them very well. And I also corrected some others, so it took time to be published. Then in the meantime, this work is so tough I wrote that paper on non-vanishing modulo a prime of Hecke L-values assuming this μ-invariant paper's construction. It requires quite a large amount of arithmetic geometry input there. And so Ching-Li's help is essential.

C: I think I read the version of 2003 and now the published version is quite different from the original.

B: I gave a lecture at the mathematics institute in Hsinchu, in 2001. And Ching-Li was there. He pointed me out a mistake. We tried to correct it. And that was corrected. And then I found some other errors, which I corrected. And so on so much time. But on the other hand, the earlier referees seemed ineffective. And one of the referees did not understand well, even the strategy, so it took so much time. At the very end, the last referee understood the paper very well.

C: In the beginning, probably not.

B: At that time, Katz was not I think the editor. Andrew Wiles was the editor. And Andrew was not so strong in this type of arithmetic geometry. And thus he could not find a good referee. Later Andrew told me he was sorry about that. But there is no reason for him to apologize me as I made a lot of errors any way. Most errors were not found by the earlier referees, but by Ching-Li and myself.

C: Nowadays, it seems your result and Cornut-Vatsal's result are the only examples for which we can prove this kind of nonvanishing. So, do you think your method can be pushed to slightly bigger groups or is this still difficult?

B: I think that, in the Hilbert modular case, Hilbert modular variety has dimension equal to the number of variables of the anticyclotomic Katz p-adic L-function. This coincidence is very important. It could go farther if one has such coincidence, for example, some unitary groups with a specific signature.

C: Oh higher orthogonal groups.

B: Doubling method. The U(n,n) Shimura variety has dimension n^2 while the corresponding p-adic L would have n variables. So perhaps it doesn't work. However, U(n,1) is probably good. I told this idea to many p-adic analylists, but it seems that they are afraid of arithmetic geometry. I told this to many arithmetic geometers, they are afraid of this huge group and Langlands theory. I could try if my brain survives well (as I have some other prioritized projects).

C: So no one really tried to really work on this.

B: As far as I know of. I could possibly do, but nowadays, I hate laborious work, and it is certainly a hard one. I love GL(2) and still I can do a lot of things for GL(2). And if I survive another ten years, I might try it. But for the moment, I have so many things to do, so I don't do it.

C: For U(n,1) it is okay I can try it. Because that's also one of the goal for next year's conference. I try to…because it is so important.

B: U(n,1) is the only choice, I think. U(n,1) is so good somehow.

C: This is clearly very important nonvanishing. Nowadays, people construct a lot of p-adic L-functions related to cycles, but they cannot prove these cycles are non-zero. This is the only way to prove it.

B: Those related 0-cycles, this is just a variation of CM points. But the higher cohomology would have more difficulty, that there's no real known way of doing that. And that's the main point that the study of the cyclotomic μ-invariant has difficulty, (i.e., modular symbol has higher dimension). It is therefore difficult.

C: That's another difficulty.

D: So it is somehow reported that Poincaré spent only two hours doing math per day. Although, he is a pretty impressive mathematician.

B: Sure sure.

D: So you wrote so many important papers. And also you published 7 textbooks so far. And I'm sure you're writing more. So how do you arrange your life around mathematics?

B: I don't know, but when I was young, I could do math like Grothendieck 24 hours a day even. After I found math was fun, I was totally addicted, so day and night I did. But I didn't write many papers, because I had a permanent position in the Japanese university. At the time, at any Japanese university, promotion is basically automatic, and you don't need to produce anything. Also, I had a lot of things for fun, poem and so on, and drinking sake having fun with Doi. So I didn't write much. Then I came to UCLA, and I found that people compete writing a paper. Sure, I can do that, I thought. So I wrote, I had a good stock of new results by the time. While I was in Princeton, I started to make quite a detailed note of anything I found interesting. That Shimura suggested me to do. Every week, I met him. Then somehow he remembered everything I told him one week earlier. If I say something different (if some discrepancy there), he pointed them out. He asked me "Are you really making a note?" No, I am just pulling them out of my head. He suggested me to write every proof, every detail in the note. He told me, "at some point, you would appreciate that". So I started doing that. Writing paper is easy out of all the details anyway given. So it was fast.

D: Very efficient Japanese way.

B: Shimura had also another point. Somebody told me that he had two desks. That is true in his office. A big office. Far bigger than the one we are in. The front big desk and the back big desk. And he's usually sitting there behind the back desk. And when somebody come, he comes to the front desk greeting the guest. On this back desk, are his manuscripts (with some of his writing). So first of all, the visitor can't see anything of his writing (so that he can keep them secret). I am told that he also have two big desks at his home. That I don't know true or not. I never entered in his office at his home. His house is also a big one. So he wrote many difficult papers. And at that time, it is physically typed. Once finished typing a manuscript, he throw his thick paper towards the back desk, forget about it one year. And then after one year, he started reading it. In this way he finds errors. In the second reading, he finds he has forgoten many things. Therefore, he reads it just as a novice. In this way, he finds errors. And that's the main reason for his not making many errors. And that I do not do. I just write. I just publish it. So I make many errors. If I had done like Shimura did, then I could have avoided errors. In his red book, this thick book, there is only one error. That is sort of unavoidable because he doesn't use scheme theory.

D: Now it can be spelled out that he made an error. So somehow in the recent, but not so recent time, also because of the proof of Fermat's last Theorem, number theory somehow has become more popular among students. Do you have any advice for young students?

B: Ah, do your own mathematics! Then your advisor are very happy if the student does their own mathematics, get PhD and graduate. You don't need to advise anything.

D: Yeah, I think you already finished that one, right? That one is already done also.

C: Yeah, this is already finished. But I have one extra question. So nowadays, it seems in Japan or, I know in Japan or in Taiwan, it's pretty difficult for fresh PhD, especially in number theory, to get a job. So do you have any advice for fresh PhD? How to deal with this kind of situation. Because I think in Japan, maybe it's even more serious, it's even…they cannot, at least, I know many people. They cannot even find a postdoc job in the beginning. They need to survive without salary for the first year. So do you have any advice? How to deal with this kind of situation?

B: No solution, I think, in these advanced countries, you know, in the early days they exhausted resources out of the entire world. If your country developed earlier, the country survives longer. That is somehow why the developped countries are quite rich now. The situation has changed, and many new countries enter in the developed stage. Also the number of students used to be not large. So they could supply them the job, and also population was younger, so teacher's job was growing. Now, things are reverse. I mean, those advanced countries exploited developing countries to some extent. And these developing countries now developed well. On the other hand, these advanced countries get naturally somewhat decadent. Because so many older people, especially Japan you know. If you just ride on the metro or the bus, most people are like me. So there are many special seat for old men and women. So I would be sitting. There could be somebody look far older than me. Somehow, you know, younger people perhaps need to find a job in the developing countries, right? India or China, wherever. Now, mainland China is very well developed. But still there seems to be a very good amount of jobs. Perhaps salary is lower, and living standard maybe lower, but you need to pay something to get something. Perhaps that's the only way, I think. In this way, the developing country get nicer, having better teachers. Then they can develop faster. On the other hand, developed countries may further go down.

C: So it's similar. In Taiwan, many people in algebra, they find jobs in China or even in Malaysia.

B: Sure, I met many young Japanese people when I visited Chinese places. And they are doing postdocs or something, and that is fine.

C: For a long time.

B: Getting job outside Japan makes Japanese more cosmopolitan, I think. Japanese are not at all cosmopolitan, because they are so isolated mentally. And that makes their culture very unique and perhaps interesting as we talked at the beginning. But also, there's a kind of price you need to pay. And they are therefore too Japanese, and they can't cope well with other type of people. So just leaving their own country would be perhaps very good for them, I think. Especially talented people. Then they can speak other languages as well, I think.

D: Yeah, so would you agree with observation that if you think about science as a hierarchy, like you have students, you have grad students, you have postdocs, you have assistant professors, and associate professors, and full professors. My impression is that one main problem in this picture is that the pyramid gets flatter and flatter. So I think it would be more reasonable to have a higher slope, you see what I'm saying?

B: Right. But this hierarchy is basically German-made, no?

C: Germany is the most serious.

B: In Japan, they imported the system from Germany, so one full professor in the number theory, then two assistant/associate professors, then perhaps three of four assistant, and that's it.

D: But now, things changed in Japan.

B: Sure, sure. No other way one can hire younger people.

D: Yeah, sure, but the question is…

B: So a full professor is having a huge office.

D: Do you think we need so many mathematicians as we have students?

C: So many number theorists.

B: Oh, so many…

D: So many PhD students, especially in mathematics in this world than we have actually.

B: I think that something like bio-chemistry kind of things, the number of students…of course, they have a lot of jobs. Most of PhD students of pure math or number theory know that they can't make money. And therefore, it's not about money. Therefore, still we are small, right?

D: Yeah, the job perspective is very small. And there's not so many students, but compared to the positions. And also the professor, there's a huge number. So I have the impression that the number of PhD students is constantly increasing.

C: True, this is in Taiwan. In Japan, I don't know.

D: The number of professors is almost stable. Maybe even decreasing in number theory, because mathematics itself is more diverse than…

B: Because the number of teachers, anyway, has to be bounded, because number of younger people is declining. In Japan now, absolutely. So there's no way out for that. So maybe prosperous days are actually over. Now, even in China, the number of younger people go down because of the one child policy. Korean women produce 0.9 child per…

C: Taiwan the same, 0.9.

B: Yeah, even Japan. It's 1.2 or something like that. First of all, you need to do something for this problem, right? Then the entire planet becomes old men and women's world.

C,D: Okay, thank you very much.