

YUTAKA TANIYAMA AND HIS TIME

Very Personal Recollections

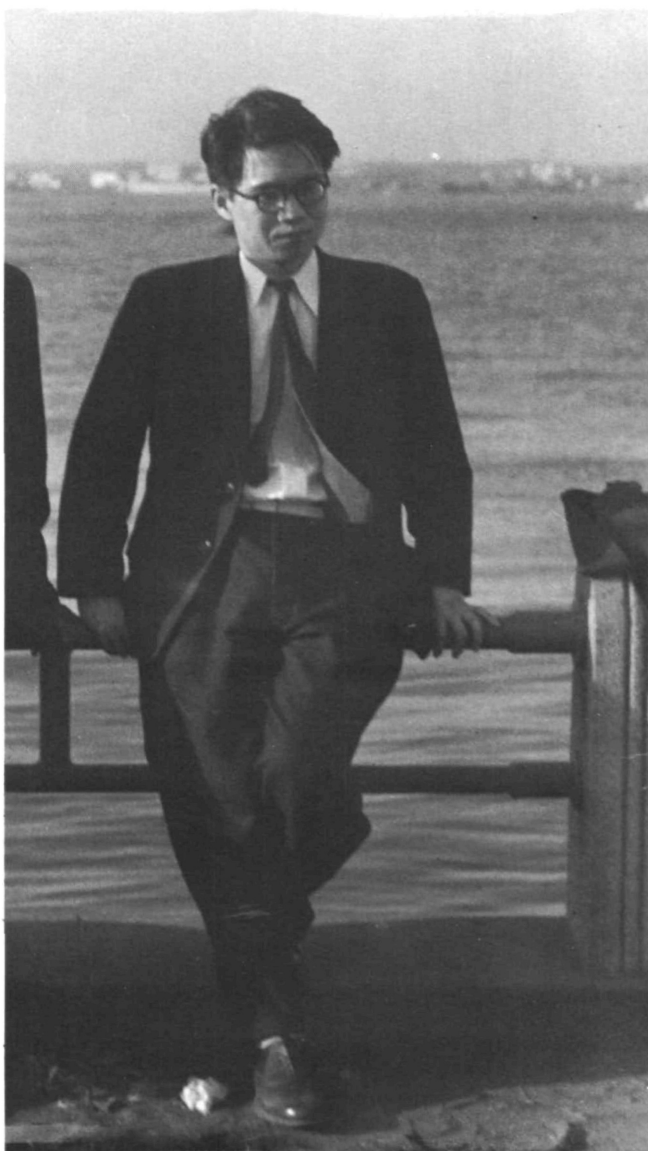
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To write about Taniyama's time, first I have to emphasize that it was the mid to late 1950s, and the situation was totally different from that of Japan today, to say nothing of the comparison with the United States or Europe, now or then. Pollution was not a household word in those days, and in fine weather one could see, from the center of Tokyo, Mt Fuji on the western horizon 70 miles away, with its snowed crown in the morning and silhouetted in the evening. The destruction and deprivation of wartime and the succeeding period were things of the past, but not forgotten. We were no longer hungry. The whole country was aspiring and hopeful, but still very poor. This was so in a collective sense and also on the individual level. Taniyama and his peers were no exception, though it may be said, in any country at any time, one is usually both ambitious and poor at the beginning of one's career.

He was not particularly poorer than others, and I think he never had any great financial difficulties, yet his life was anything but comfortable, as it was for most of us at that time. At least, he enjoyed a fair portion of the universal poverty of the period. For example, he lived in a one-room apartment which consisted of 81 square feet of living space, a sink, and a tiny unfloored part behind the door. Running water, gas and electricity were provided separately in each room, but there was only one toilet on each floor of the two-storey building, shared by all the occupants of the dozen or so rooms of the floor. I remember that his was No. 20 on the second floor, close to the last. Thus it was more like a dormitory than an apartment, but it was more or less typical of the time. To take a bath, he had to go to a public bathhouse, a few minutes' walk from his apartment. The building, a rather shabby wooden structure, was named poetically 'Villa Tranquil Mountains', but that expressed merely an unfulfilled desire, because it stood on a narrow but lively street closely lined by small retail shops, and besides, trains passed on the nearby railroad every few minutes. There was no central heating system; air conditioning was inconceivable at such a place. However, many of the innumerable coffee shops in Tokyo offered a certain luxury of coolness whenever needed, as well as a place for endless mathematical and nonmathematical conversation, with the price of 50 yen a cup of coffee. It was the time when one dollar was 360 yen, and his monthly salary as a lecturer at the University of Tokyo was less than ¥15,000.

Speaking about domestic affairs, he was a lazy type, and so he rarely cooked, and he ate out often at small restaurants. One of his favorite dishes available at several

Editors' note. This personal tribute to Y. Taniyama by his friend is published some thirty years after his death in recognition of his outstanding qualities as a man and because of his influence on the development of number theory and algebraic geometry. He was not a member of the London Mathematical Society, but perhaps might have become one in due time. The notes on Taniyama's problems, which have had a profound influence on work on elliptic curves and automorphic functions, were specially translated from the article in *Sūgaku* by G. Shimura, who also added the comments on them for this Notice.



YUTAKA TANIYAMA 1927–1958



A tram journey to Nikko at the 1955 conference on algebraic number theory. Shown are (left to right) T. Tamagawa, J.-P. Serre, Y. Taniyama and A. Weil.

western style restaurants was tongue stew, ¥250 à la carte, another of the few items of modest luxury in which he could indulge occasionally. He almost always wore—he exclusively wore, I am tempted to say—except in the summer, a blue-green suit with a strange metallic sheen. Once he explained to me how he got hold of the suit. His father bought the material very cheap from a peddler. Because of the luster, however, nobody in his family dared to wear it, and finally he volunteered to have it tailored for himself, as he didn't care much about how he looked. His shoelaces were always loose, and he often dragged them on the ground; since he was incapable of keeping them securely tied all the time, he decided not to concern himself about tying them again when they got loose.

Such was the mathematician who departed from this life so quickly, leaving an everlasting source of inspiration to his generation and also to the coming ones.

Yutaka Taniyama was born November 12, 1927, the third son and sixth child of Sahei and Kaku Taniyama. He had three brothers and four sisters. Both his parents lived beyond ninety. His first name, expressed by a single Chinese character, can be pronounced 'Toyo', and he once told me, if my recollection is correct, that was the originally intended pronunciation. But as he grew up, most people, especially those outside his family, read the character 'Yutaka', and so he accepted it at some point, and he was Taniyama Yutaka since then. At least all his papers were written under that name, if in reverse order. I have no information about his childhood, nor his pre-college years, except that he suffered from tuberculosis while attending a senior high school, and had to be out of school for two years. As far as I can remember, he was coughing every ten to fifteen minutes.

His father was a locally well known country doctor practising pediatrics, and often medicine in general, as this type of profession ordinarily demanded. I met him only once. He was a vigorous man in his early eighties, and seemed to belong to the so-called self-made type. Shortly after our encounter, he sent a letter to one of my colleagues at the University of Tokyo, who met him at the same time. Somehow the old man formed an idea that the young scholar was not doing well academically, and advised him to take food rich in vitamin B (or C or possibly calcium) in order to make his brain work more efficiently. Since this was after Yutaka's passing away, I was never able to check whether the father gave a similar piece of advice to the son.

Taniyama graduated from the University of Tokyo in March 1953, I in 1952, though he was older than I; the delay was caused by his illness. I knew him since 1950, but our first serious mathematical contact was in early 1954, when I wrote him a letter requesting that he return the copy of *Mathematische Annalen*, Vol. 124, which contained the paper by Deuring on his algebraic theory of complex multiplication. Taniyama had checked it out from the library some weeks ago. In December of the previous year, I had sent my manuscript on reduction modulo p of algebraic varieties to André Weil, who was at Chicago, and I was intending to apply the theory to abelian varieties, in particular to elliptic curves. In his reply to me, Taniyama wrote that he had the same intention, and politely asked me to explain my theory to him some time. In retrospect, I think he was, with a wider knowledge and a better perspective, more mature than I mathematically, though I didn't know it at that time.

I still keep the postcard, postmarked January 23, 1954. After more than thirty years, it naturally aged, but clearly shows his handwriting. It bears the address of his parents' home, where he was temporarily staying. It was in a small undistinguished town called Kisai, one of those half rural and half urban types, about 30 miles north

of the University. Incidentally, he was born and raised there. Only God knew, a mere five and a half years later, I would be standing in front of his grave in the cemetery behind a temple in the town.

At the time of our correspondence, he was a 'special research student' and I an assistant, but there was not much difference in substance. If there were, that would have been like the distinction of stipend from salary. He was at the Department of Mathematics where professors taught juniors and seniors, while I belonged to another department, which took care of calculus courses for freshmen and sophomores, and which was located in a different campus called College of General Education. This separation was the main reason why I had little contact with him before the above exchange; a secondary one may have been the shyness on either side. Eventually both of us became lecturers in the latter department. He was an associate professor when he died.

Whatever positions we held, our real status in 1954–55 was, in all practical senses, that of a graduate student with no advisor, but with a certain teaching load, which was, at least in my case, equivalent to two undergraduate courses in an American University. This observation applies to nearly all Japanese mathematicians of my generation. The only significant point was that most of us had tenure even as assistants! At any rate, no senior professors were capable of advising students. Even so some of them occasionally offered unsolicited pieces of advice. Once one of us met accidentally on a train a professor who was about fifty at that time. The latter asked the young man what his research interest was. Hearing that he was studying Siegel's theory of quadratic forms, the old man said, 'Ah, quadratic forms. You, as young as you are, may not know it, but it's Minkowski's work that matters.' My colleague complained of this to me afterward. Mocking the professor's pompous air, he said, 'Of course I knew that Minkowski mattered, but did he add anything to Siegel's work?' I too received similar useless pieces of advice or comments.

I always wondered if those professors were trying to imitate their elders, in particular a very worshipped figure among them, who must have made many such comments, most of which, I am inclined to think, were similarly meaningless. Or maybe they were trying to be useful in their style, without realizing that the younger generation, Taniyama for example, had already leapfrogged them, evidence of which the reader will find below. I should note here that Taniyama himself never made pretentious comments; his advice to his juniors was always practical and professional.

Anyway we ignored such almost comical sayings, but accepted them as a kind of reminder that we could not rely on anybody but ourselves. There were of course some Japanese mathematicians in the generation in between who were already prominent or on the way to becoming so. But practically all of them were either abroad or to leave in a short time. For instance, Kodaira and Iwasawa were in the States, soon to be followed by Igusa and Matsusaka.

Around 1950, Hilbert's fifth problem was a topic much mentioned, and the arithmetization of class field theory, or even lattice theory, was being talked about. As none of these was attractive, not a few chose algebraic geometry. At that time, perhaps Chevalley's *Theory of Lie groups* and Weil's *Foundations of algebraic geometry* were the two most widely read books, the former usually read till the end, and the latter given up after the first twenty pages or so in most cases.

In his undergraduate years, Taniyama read both, as well as the two succeeding books by Weil on algebraic curves and abelian varieties. He once wrote that he was led to number theory under the influence of Masao Sugawara, whose course on

algebra he had taken. Sugawara was a senior professor in my department, and published some papers on complex multiplication and also on discontinuous groups in the higher-dimensional spaces. However, I have been puzzled by this acknowledgment by Taniyama, as I found Sugawara uninspiring, though I liked him and even respected him as a man. Speaking for myself, during this period and on a personal level, I was influenced exclusively by the people of my generation, above all by Taniyama, and by none of those above the age of thirty. I think this applies in essence to him too.

Indeed, his training ground was many seminars organized by the students themselves. He was the driving force of such activities, and furiously acquiring mathematical knowledge as much as possible. He must have studied Hecke's papers Nos. 33, 35, 36, and 38 on Dirichlet series and modular forms at some point, perhaps somewhat later. While in the same department, he would kindly lend me his notes on this topic when I was unable to secure the library copy of the journal in question.

His first nonelementary work is titled 'On n -division of abelian function fields', and may be termed his senior thesis, though such was not required. As I have no intention of intermingling detailed expositions of his mathematical works with these personal recollections of mine that are my main objective here, let me just say that this paper gave a proof of the Mordell–Weil theorem, based on an idea of Hasse and the results of Weil's paper (*Ann. of Math.* 1951) and that in 1953 he was the only person in Japan who had any working knowledge of this topic. I remember vividly his presentation of this work in a few lectures in the seminar Chevalley held at the University of Tokyo in the spring of 1954.

As explained earlier, he had been interested in complex multiplication of abelian varieties for some time. He took up first the case of the jacobian variety of a hyperelliptic curve, and eventually that of more general abelian varieties. Since not much had been known in this field, the task was a 'hard fighting' against difficulties and a 'bitter struggle' of trial and error. He used to express any substantial undertaking of a mathematician in those four words (strictly speaking, in four corresponding Chinese characters). 'Effortless' was a word alien to his mathematics at least from his viewpoint, though it may have looked differently to others, and he must have found immense delight in such 'fighting and struggle.' He presented his results at the symposium on algebraic number theory held in Tokyo–Nikko, September, 1955. He met Weil there, and incorporating some of Weil's ideas into his, he later published an improved version of his theory on the relationship between abelian varieties and certain Hecke L -functions, a supreme achievement of the time (' L -functions of number fields and zeta functions of abelian varieties', [3]).

As for the part not included in this paper, the collaboration with me was then planned, as I had some results on the subject on my own. We set out in this task in a fashion which may be called leisurely according to today's standard, as we were living in a relaxed, perhaps too relaxed, atmosphere of no competition, which might be envied by the young mathematicians of the 1980s. I have to thank Yasuo Akizuki for hastening our project by inducing us to write a volume in the series of mathematical monographs of which he was an editor.

During this period of collaboration, I often visited his 'Villa' to discuss the matter, as the place was appreciably closer to our school than my home. He always worked late at night. My diary of 1957 tells that, on the afternoon of Thursday, April 4, 2:20 p.m. to be exact, I visited his apartment where he was still asleep. He said he went to bed at 6:00 a.m. On another occasion, perhaps in the late morning, as my

knock at the door was not answered, I went to our department office, half an hour train ride from his residence. Finding him there, I said to him, 'I've just stopped by your place before coming here', to which he answered: 'Hum, was I there then?' Immediately realizing his blunder and much embarrassed, he defended his position: 'But you know I am often asleep at that time of the day'.

I discovered that he was different from me in many ways. For one thing, I was, and still am, an early riser. At that time, I thought he was more rational and I more whimsical, but I might have been wrong. We had something in common however: each of us was a late child in a big family; I am the fifth and last child. I say this because I used to resent the egocentricity of those first sons in Japanese families. Though he was by no means a sloppy type, he was gifted with the special capability of making many mistakes, mostly in the right direction. I envied him for this, and tried in vain to imitate him, but found it quite difficult to make good mistakes.

Our joint work in Japanese entitled *Modern number theory* was published in July, 1957. Our next task was of course to make an English version, desirably in a better form, but somehow we lost enthusiasm. The first obvious reason was that we were relieved by the fact that at least it had been written, if in Japanese. There was another practical side of the matter: I was going to leave for France in the fall of that year, which made me restless in a certain way. However, a more fundamental reason can be given by quoting a passage from the preface of the book:

'We find it difficult to claim that the theory is presented in a completely satisfactory form. In any case, it may be said, we are allowed in the course of progress to climb to a certain height in order to look back at our tracks, and then to take a view of our destination'.

Said more prosaically, it was necessary to search for a better formulation and to refine the results. In that year, we were already thinking about the adelization of the whole theory, and perhaps we should have pursued that direction, but didn't. Also, as a matter of psychological reaction, once one proves something, one is more interested in obtaining more new theorems than polishing old ones. Indeed, both of us were interested in modular forms of various types, and that course looked more exciting. Thus our correspondence between Tokyo and Paris was always on that subject. In the spring of 1958, as he informed me of the news, Tokyo greeted Siegel and Eichler, who gave series of lectures, the former on reduction theory of quadratic forms and the latter on his recent results, in particular his trace formula. Meanwhile, in Paris, the topics of the Cartan Seminar were centering around Siegel modular forms.

I wrote more often than he, who wrote to me only twice in this period. In his second letter dated September 22, 1958, which is the very last of all his letters in existence, he mentions that the correspondence of Hecke's type between Hilbert modular forms and certain Dirichlet series could be formulated on the adèle group of $GL(2)$. However, as indicated in the tone of the letter, his enthusiasm was rather restrained. He knew that the mere feasibility of such a formulation was not enough, and a real breakthrough was lacking. Obviously more work was necessary; in fact he wrote: 'Because of the heat, I've laid aside the work for one month, but will start thinking about it soon.' Given a sufficient time of concentration, he would have succeeded, but he had to leave the work unfinished forever, since he was destined to die within two months, the remotest thing both sender and recipient of the letter could imagine at that point.

As for our collaborative work, the situation was changed completely by his death,

which I will describe later. Having been left alone, I felt it was my duty to finish it as quickly as possible, though I was not completely satisfied with the formulation I had. Eventually 'Complex multiplication of abelian varieties and its applications to number theory' was published in the spring of 1961. The title had been suggested by him in one of his letters. It took me ten more years to be able to set things in a better perspective, and still another five years or so in order to formulate the theory in terms of theta functions as he might have wished, but alas, the man who would have been pleased by these had been long gone.

To write about the private aspect of his life and his last days, first I have to go back a few years to 1955. Though he and I had been members of the same seminar for some time, our relationship became closer after his joining my department in December that year, which naturally made us engage in the same activities of various kinds. For instance, as our official duties would require, we would be confined together in a department office to grade examination papers for admission to the University, more than 5000 sheets for each of us. Fortunately for us but unfortunately for the examinees, many of them were blank.

On a more agreeable note, we would enjoy, together with some other friends, relaxed times in those coffee shops, and spend a Saturday afternoon at a botanical garden in the city, or at a park in the outskirts. In the evenings, we would eat at a restaurant specializing in whale meat, not a particular delicacy in those days but perhaps unthinkable nowadays. We would also take a long walk after a day of work at our school, visiting a Shinto shrine, where we would purchase 'oracles' printed on small pieces of papers to amuse ourselves; they were supposed to tell our fortunes.

While riding in a train together, he asked me the name of the next station, to which I answered, 'The next stop will be *Station*; and then the next stop will be *Next Station*'. This entertained him very much, as he heard it for the first time, and I had to explain to him that I merely mimicked a line of a comedian who was popular on the radio at that time. Immediately afterward, he bought a radio set, and eventually acquired a record player and a sizeable record collection. In his last letter mentioned above, he wrote, 'Lately I am listening repeatedly to Beethoven's No. 8'. I would think these and movie-going were practically all the entertainments he had alone. One of the movies he enjoyed was 'The King and I'. I don't think he ever played any musical instrument. He was no athlete. He never drank, nor smoked, and had no hobby whatsoever. He was not fond of travelling; rather, it seemed to me, he avoided it whenever he could, perhaps on account of his delicate health. I would think Kyoto was the farthest place he reached in his whole life. As an educated man, he must have read standard classical literature, but I am inclined to think he was not an avid reader of novels by modern writers, Japanese or foreign. Nor was he much interested in history, except in that of mathematics.

There is one thing, however, on which he spent a considerable amount of time and energy throughout his prime years, that is, a kind of journalistic writing on academic matters. The topics are varied: how researchers should be trained, how a new institute for mathematical sciences should be organized, criticism on previous articles by others, book reviews, etc. He wrote these articles rather quickly, and made few revisions after finishing them. Probably he was organizing his thoughts by expressing them in writing. He was an articulate writer, more so than in his speech. Incidentally, he was more cheerful in his letters than in his conversation. To tell the truth, I found this 'hobby' of his regrettable, as I thought he was simply wasting much of his precious time, and the cause in each case was not important enough to justify his

expending much effort, though I never told him so explicitly. But on one occasion, a few days after hearing my opinion of *laissez-faire*, he showed me his rough draft on the issue, in which he caricatured my manner of speaking. Naturally I protested and he dropped my part.

He was always kind to his colleagues, especially to his juniors, and he genuinely cared about their welfare. However, in retrospect, I can surmise, without fearing blame for cynicism, that the cause aside he derived much enjoyment from his writing activities. If so, perhaps there was not much point in my regretting.

I wish to close this rather discursive description of his life by telling about his last months. In those days, we were naturally full of youthful ardor and yearning; this could be said on all matters, academic or nonacademic. Speaking now in the latter sense, I can express the predominant mood at that time by a single sentence: nobody believed in arranged marriages—well, almost nobody. Maybe some of us thought, if jokingly, the institution was for the bourgeoisie, and we the proletariat should denounce it as an evil practice, but of course that was an exaggeration. In fact, when I, together with some of my friends, made a call of condolence at his family's home on a hot summer day of 1959 some eight months after his death, his eldest brother, or it may have been his father, suggested to me the daughter of a well known painter as a possible mate. Embarrassed, I asked a female companion in the party how I should respond. She said that the etiquette book would advise me to say such and such. So I repeated the suggested words mechanically, which caused laughter. And that was that.

I used to fancy the idea that the girl might have originally been picked as a candidate for Yutaka. If that were so—my wife would tease me some years later—I should have married her for that reason. Whatever wishes his family may have had, he chose someone on his own, with the eventual consent of the parents of both sides. Her name was Misako Suzuki. He refers to her as M. S. in his will, to which I will now turn. But first the background.

I think he met her as a friend of a friend of a friend in a small and loosely defined social circle around us. I clearly remember the dinner party she gave at her home, with the help of her mother, for Taniyama, K. Yamazaki (one of the colleagues at our school), his fiancée, and myself, shortly before my departure for France in November 1957. The gathering, nominally a farewell party for me, was rather a quiet one, unlike those on other occasions. Indeed, I remember that she was making fun of his reticence during the meal. The same five had spent an evening together in April of that year, which could have been approximately the first encounter of the two. There were many such evenings in those days, with varied members depending on circumstances.

Misako was, relatively speaking, a newcomer to my social circle, and I never got to know her well, but she appeared to be a typically pleasant girl from a typically upper middle class family, and spoke the standard Tokyo dialect with few inhibitions. She was an only child and about five years younger than he. When informed of their engagement, I was somewhat surprised, since I had vaguely thought she was not his type, but I felt no misgivings.

I was told afterward that they had signed a lease for an apartment, apparently a better one, for their new home, had bought some kitchenware together, and had been preparing for their wedding. Everything looked promising for them and their friends. Then the catastrophe befell them. On the morning of Monday, November 17, 1958, the superintendent of his apartment (the one mentioned earlier) found him dead in his room with a note left on a desk. It was written on three pages of a notebook of the

type he had been using for his scholastic work; its first paragraph read like this:

‘Until yesterday, I had no definite intention of killing myself. But more than a few must have noticed that lately I have been tired both physically and mentally. As to the cause of my suicide, I don’t quite understand it myself, but it is not the result of a particular incident, nor of a specific matter. Merely may I say, I am in the frame of mind that I lost confidence in my future. There may be someone to whom my suicide will be troubling or a blow to a certain degree. I sincerely hope that this incident will cast no dark shadow over the future of that person. At any rate, I cannot deny that this is a kind of betrayal, but please excuse it as my last act in my own way, as I have been doing my own way all my life’.

He went on to describe, quite methodically, his wish of how his belongings should be disposed of, and which books and records were the ones he had borrowed from the library or from his friends, and so on. Specifically he says: ‘I would like to leave the records and the player to M. S. provided she will not be upset [by my leaving them to her]’. Also he explains how far he reached in the undergraduate courses on calculus and linear algebra he was teaching, and concludes the note with an apology to his colleagues for all the inconveniences this act would cause.

Thus one of the most brilliant and pioneering minds of the time ended his life by his own will. He had attained the age of thirty-one only five days earlier.

There was an inevitable turmoil, and then a funeral, followed by a gathering of his friends and colleagues in his memory. They were utterly perplexed. Naturally they asked among themselves why he had to kill himself, but no convincing answer was available. According to his fiancée, he was to see her within a few days of that fatal Monday morning. It was as if God designed him to be a virgin mathematician and not a family man. I eventually reconciled myself to that view, but that was much later.

Anyway, after a few weeks, people seemed to have recovered somewhat, if slowly, from the shock and sorrow, and things were going back to routine. Then, on a chilly day of early December, Misako killed herself in the apartment which had been intended for their new home. She left a note, which was never made public. I only heard that it contained a passage to the following effect: ‘We promised each other that no matter where we went, we would never be separated. Now that he is gone, I must go too in order to join him.’

When these misfortunes occurred, I was staying in Princeton as a member of the Institute for Advanced Study, and so the details of all the events were told to me by Kuga and Yamazaki after my return to Tokyo in the spring of 1959. Taniyama himself was supposed to be at the Institute in the fall of that year, and I could have spent my second year there, but decided to leave.

By the time I came home, the cherry trees had shed their blossoms, and dark green leaves were already dominating the view. Though a hackneyed expression, spring was swiftly passing. After my absence of one year and a half, the streets of Tokyo with their vitality and vulgarity showed little change. But people changed. So did I. A further period of transformation was still ahead of me, but in those late spring days, I could not but keep reflecting vainly on a simple fact: the type of party we had only two years ago was no longer possible. The years of turbulence had passed.

To conclude this article, I may ask somewhat rhetorically: Who was Yutaka Taniyama? This is not asked about his stature in the history of mathematics. My concern here is what his existence meant to his generation and especially to me. What

I have written may naturally be viewed as a lengthy answer to that question, but to sum up, I should state more clearly one point to which my writing so far has only vaguely alluded: that he was the moral support of many of those who came into mathematical contact with him, including of course myself. Probably he was never conscious of this role he was playing. But I feel his noble generosity in this respect even more strongly now than when he was alive. And yet nobody was able to give him any support when he desperately needed it. Reflecting on this, I am overwhelmed by the bitterest grief.

ACKNOWLEDGMENT. I wish to express my sincere gratitude to Dr Seiji Taniyama and Professor Mitsuo Sugiura for providing me with essential biographical data. Also I wish to thank heartily some of my friends who read the first draft and contributed valuable suggestions, which have been incorporated into this article.

Taniyama's problems

At the International Symposium on Algebraic Number Theory held in Tokyo and Nikko in September, 1955, mimeographed copies of a collection of 36 mathematical problems were distributed to the participants. Two of the problems posed by Taniyama concern the possibility that a certain elliptic curve is a factor of the Jacobian of an automorphic function field. These may be viewed as the origin of the now widely accepted conjecture: every elliptic curve defined over the rational number field is a factor of the Jacobian of a modular function field. Instead of reproducing the problems in their original forms, we present here English translations of their Japanese versions published in *Sūgaku*, Vol. 7 (1956), p. 269. This choice was made for minor technical reasons. At any rate, there is little difference in substance between the English and Japanese versions.

PROBLEM 12. Let C be an elliptic curve defined over an algebraic number field k , and $L_C(s)$ the L -function of C over k in the sense that

$$\zeta_C(s) = \zeta_k(s) \zeta_k(s-1) / L_C(s)$$

is the zeta function of C over k . If Hasse's conjecture is true for $\zeta_C(s)$, then the Fourier series obtained from $L_C(s)$ by the inverse Mellin transformation must be an automorphic form of dimension -2 of a special type (see Hecke). If so, it is very plausible that this form is an elliptic differential of the field of associated automorphic functions. Now, going through these observations backward, is it possible to prove Hasse's conjecture by finding a suitable automorphic form from which $L_C(s)$ can be obtained?

PROBLEM 13. In connection with Problem 12, the following may be set as a problem: to characterize the field of elliptic modular functions of 'Stufe' N , and especially to decompose the Jacobian variety J of this function field into simple factors up to isogeny. Also, it is well known that if $N = q$, a prime, and $q \equiv 3 \pmod{4}$, then J contains elliptic curves with complex multiplication. What can one say for general N ?

A few comments may be in order as to exactly what Taniyama meant or what he was thinking. An insight into this matter can be gained from a passage in the record

of an informal discussion session on September 12, 1955, attended by many of the participants of the Symposium mentioned above. The notes were taken by Taniyama, and published in Japanese in the same issue of *Sūgaku*, p. 228. The relevant part may be translated as follows.

Weil asks Taniyama: Do you think all elliptic functions are uniformized by modular functions?

Taniyama: Modular functions alone will not be enough. I think other special types of automorphic functions are necessary.

In these notes, he refers to Hecke's papers $\langle 3, 4, 5 \rangle$, and we can safely assume that the reference to Hecke in Problem 12 was to $\langle 4 \rangle$ which involves not only congruence subgroups of $SL_2(\mathbb{Z})$ but also some Fuchsian groups not commensurable with it. That explains his answer in the above exchange. The 'well known' fact in Problem 13 obviously refers to $\langle 3 \rangle$.

I should add that he was familiar with the results of $\langle 1, I; 2 \rangle$, and $\langle 6 \rangle$, which were the only papers concerning this topic in 1955.

It should also be noted that, strictly speaking, Problem 12 makes sense only when $k = \mathbb{Q}$, because $\langle 4 \rangle$ deals with the functional equation with a single $\Gamma(s)$ and so doesn't apply to the case where $k \neq \mathbb{Q}$. However, the reason he formulated the problem for a general number field may be given in the following way. If C has complex multiplication, then, with a suitable choice of k , L_C is the L -function of a certain Hecke character of k , or the product of two such L -functions, according to the choice of k , as proved by Deuring (see $\langle 1 \rangle$). If $k \neq \mathbb{Q}$, this type of Hecke's L -function itself is not the Mellin transform of a modular form, but can be the product of several such transforms in certain cases. As can be seen from his second problem, he was conscious of this connection of modular forms with the case of complex multiplication, and it is my guess that he therefore considered C over a general k so that this case could be included.

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