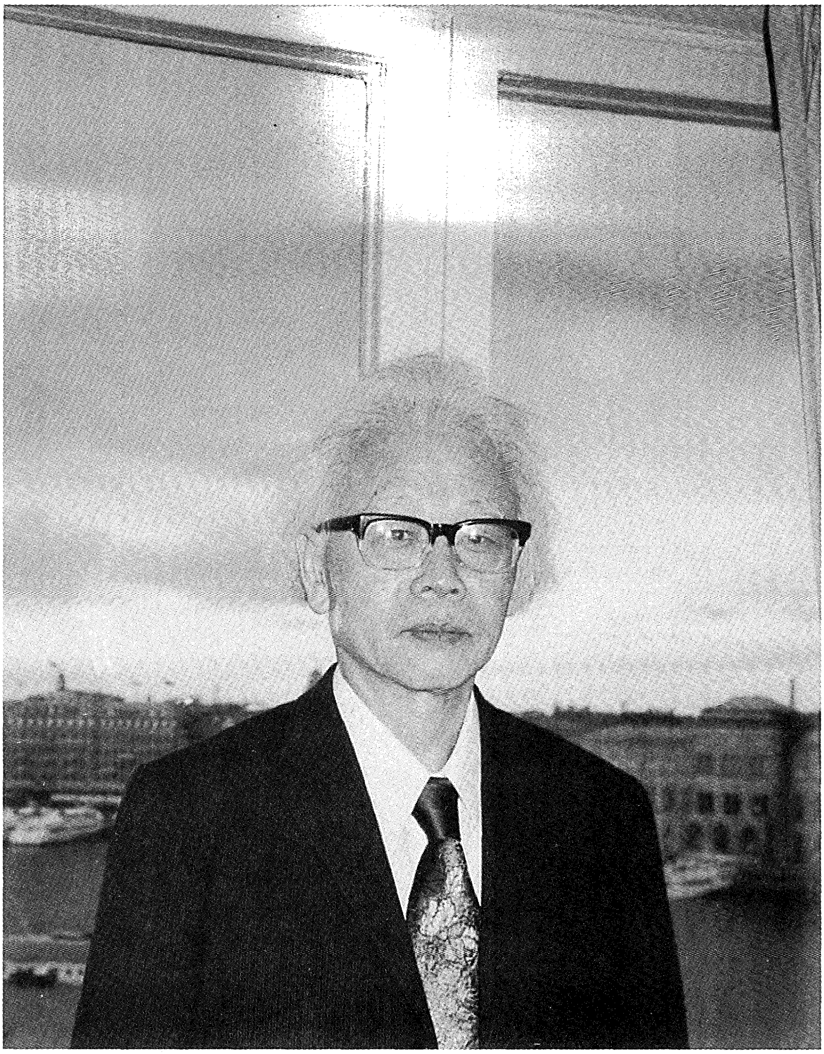
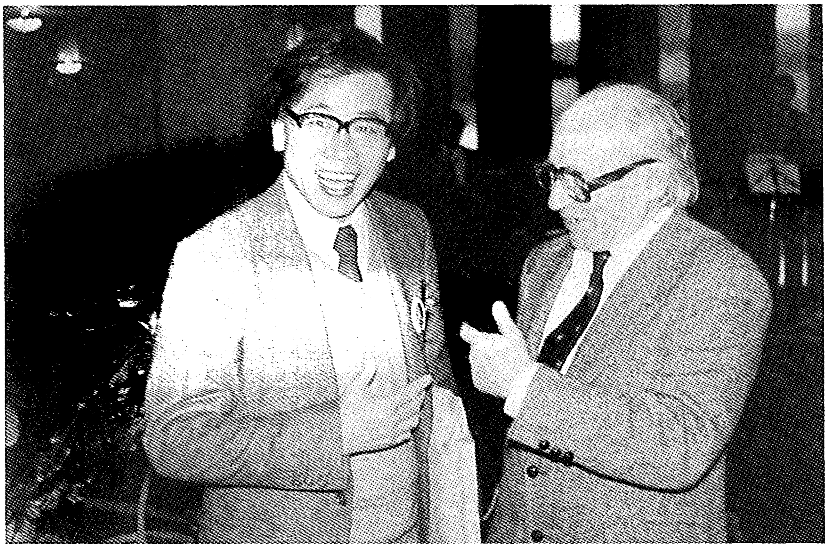


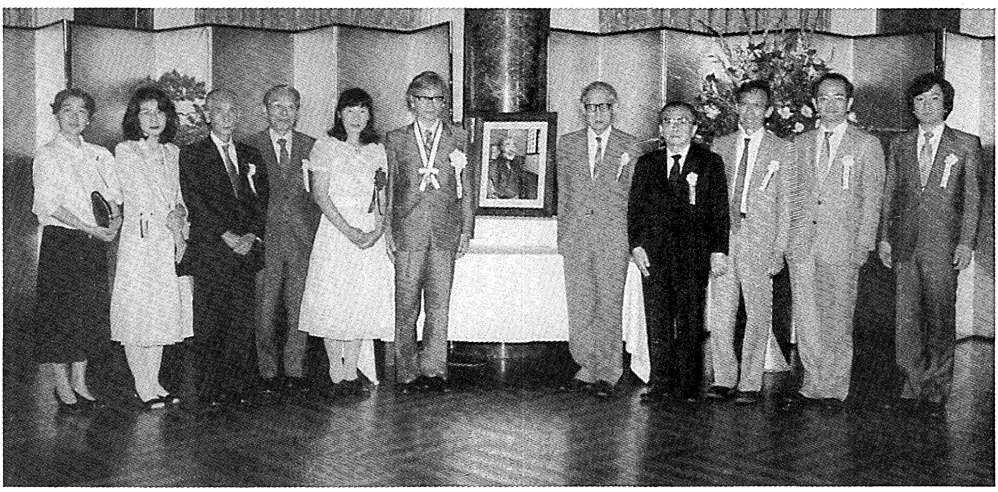
Mikio Sato



Prof. Sato with Prof. I.M. Gel'fand
Moscow, 1980
Photo taken by Prof. H. Komatsu

Top Row: Prof. Jimbo, Prof. Bros, Prof. Brezin, Prof. Sato and Prof. D. Iagolnitzer,
Bottom Row: Prof. M. Kashiwara , the son of Prof. Miwa, and Mrs. Sato

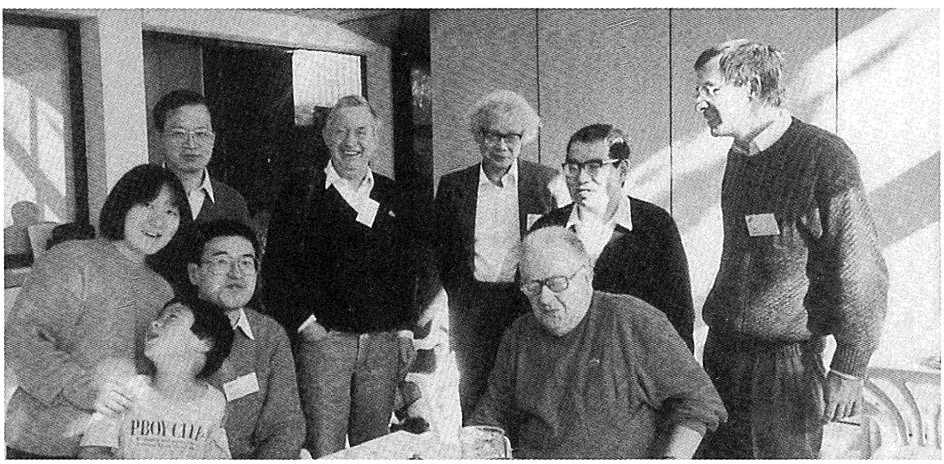
Prof. M. Jimbo, Prof. T. Miwa, and Prof. Sato
Celebrating the awarding of the prize of
the Math. Society of Japan Prize to Prof. Jimbo and Prof. Miwa
Kyoto, Fall, 1987



第28回　藤原賞贈呈式　S.62.6.17.　於 日本工業クラブ

On the Occasion of the Fujiwara Prize ceremony, June 17, 1987
Beginning with the fourth from the left, Prof. Toda, Mrs. Sato, Prof. Sato,
Prof. Iyanaga, Prof. K. Kodaira, Prof. R. Kubo, Prof. H. Komatsu, Prof. K. Minemura

The Satos with Prof. and Mrs. Leray, CIRM, 1991



Prof. and Mrs. Y. Takei, Prof. Sato's son, Prof. K. Uchiyama,
Prof. B. Malgrange, Prof. Sato, Prof. Lascous, Prof. Kawai, Prof. A. Voros, CIRM, 1991

Preface

This issue of the Asian Journal of Mathematics is dedicated to Professor Mikio SATO to celebrate his seventieth birthday.

Professor Sato, born in 1928 in Tokyo, Japan, graduated in 1952 from the Department of Mathematics of the University of Tokyo, having majored in number theory under the direction of Professor S. Iyanaga. After spending a few years of graduate study in physics with the late Professor S. Tomonaga as his adviser, he came back to the world of mathematics with the concept of hyperfunctions. The theory of hyperfunctions was the first visualization of his method of an algebraic approach to analysis, or algebraic analysis; an algebraic procedure which by considering relative cohomology groups with the sheaf of holomorphic functions as their coefficients produces generalized functions in a straightforward manner (in the sense that it is attained without using the duality of huge topological vector spaces); and, still more importantly, he discovered that the resulting sheaf of generalized functions, called hyperfunctions, enjoys the surprising property, "flabbiness". ([1]) The 1997 Rolf Schock prize was awarded to Professor Sato by the Royal Swedish Academy of Science for his creation of the theory of hyperfunctions. He held positions at the University of Tokyo, Tokyo Kyoiku University, the Institute for Advanced Study, Osaka University, and Columbia University, and in 1970 at the invitation of the late Professor K. Yosida, he settled at the Research Institute for Mathematical Sciences, Kyoto University, where he served as the director from 1987 through 1991, and stayed until his retirement in 1992. He is currently Professor Emeritus of Kyoto University.

As a kind of completion of the idea of hyperfunctions, Professor

Sato introduced in 1969 the sheaf of microfunctions, which is defined on the cotangent bundle. ([2]) Sato's microfunctions, along with the discovery by Maslov and Egorov that one can find a transformation of pseudo-differential operators compatible with a contact transformation, opened a new era in analysis. The works of Sato-Kawai-Kashiwara ([3]) and (Duistermaat-) Hörmander (Acta Math. **127** (1971), **128** (1972)) established "Microlocal Analysis", local analysis on the cotangent bundle. One of the most important discoveries in [3], besides the sheaf of microfunctions, is the structure theorem for general systems of microdifferential equations. It is a very powerful result which settles, as corollaries, central issues in the theory of linear differential equations such as the propagation of singularities of solutions, and the (non-)existence of solutions of general overdetermined systems. This structure theorem richly illustrates Professor Sato's idea that the theory of linear differential equations should be most neatly and thoroughly studied with the machinery of cohomology (Colloquium talk at the University of Tokyo, 1960), and it also nicely exemplifies his philosophy that the structure of the solutions of differential equations could be deduced from the study of the structure of the differential equations themselves.

Around the time when the draft of [3] was completed, Professor Sato visited the University of Nice, where he had opportunities to discuss some problems related to physics with F. Pham, D. Iagolnitzer and D. Olive. Immediately, he recognized that the analysis of the S-matrix by Iagolnitzer-Stapp (Commun. math. Phys., **14** (1969)) could be regarded as a prototype of microfunction theory. Encouraged by that observation, he proposed a study of Green's function in quantum field theory and related functions with the aid of microlocal analysis. ([4]) In 1976, to implement this proposal, he began a study of the two-dimensional Ising model with T. Miwa and M. Jimbo. About the same time, Wu-McCoy-Tracy-Barouch (Phys. Rev. B **13** (1976)) made the surprising discovery that the two-point correlation function for the scaling limit of the two-dimensional Ising model can be explicitly written down with the help of the Painlevé transcendent. When he saw this result, Professor Sato immediately noticed the close connection between the Ising model and the classical theory of Schlesinger on the monodromy-preserving deformation of linear ordinary differential equations (J. Reine Angew. Math., **141** (1912)); and Sato, Miwa, and Jimbo were able to explicitly calculate general n-point functions by making use of solutions of nonlinear differential equations which arise as conditions to preserve the monodromy structure in deforming a system of linear differential equations. ([5]) It might be worth mentioning that Professor Sato once said that in recognizing the relevance of the Schlesinger theory, he greatly benefited from the atmosphere and tradition formed in Japan by Professor M. Hukuhara and his students.

Professor Sato then became interested in the relation between monodromy-preserving deformations and the spectrum-preserving deformations studied by P. Lax (Comm. Pure Appl. Math., **21** (1968)), and so he began to study nonlinear differential equations of soliton type. He particularly focused on Hirota's intriguing methods of constructing concrete solutions of soliton-type equations. Collaborating with Mrs. Sato in 1981, he obtained the following beautiful result ([6], [7]): a solution of the Kodomtsev-Petriashivili equations corresponds to a point of an infinite-dimensional Grassmann manifold, and (almost) all hitherto-known solutions of soliton-type equations can be obtained as an orbit of a subgroup of the transformation group $GL(\infty)$ of the infinite-dimensional Grassmann manifold. Further, Hirota's bilinear equations can be explained as Plücker relations defining the Grassmann manifold as a submanifold of projective space. Apparently, Professor Sato attempted to apply these results with the soliton-type equations to a systematic study of automorphic forms. (Colloquium talk at Kyoto University, 1992) We note that, although not much has been published, Professor Sato also made several contributions to number theory by the study of the relation between the Ramanujan conjecture and the Weil conjecture, conjectures on the distribution of zeros of congruence zeta functions, and the theory of prehomogeneous vector spaces and the associated zeta functions. ([8], [9], [10])

We think that this simple list of his scientific contributions may not sufficiently explain why he is so respected; perhaps the following suggestion which the late Professor T. Shintani gave to young students might illustrate: "How can you learn from Master Sato? Just bring one result to him. Then he will give you back ten results."

When some of Professor Sato's students dedicated a collection of papers to him on the occasion of his sixtieth birthday* (Algebraic Analysis, vol. I and II, ed. by M. Kashiwara and T. Kawai, Academic Press, 1988), Professor Sato and his collaborators learned a lot from the contribution of F. Pham on exact WKB analysis, which led them to examine the epoch-making result of Bender-Wu (Phys. Rev. **184** (1969)). We sincerely wish similar lucky encounters to occur this time. Have we succeeded in bringing "one result" to you, Professor Sato?

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* A list of those talks is appended at the end of this preface.

Congratulations on your seventieth birthday, Professor Sato.

Masaki KASHIWARA (RIMS, Kyoto University)
Takahiro KAWAI (RIMS, Kyoto University)
Shing-Tung YAU (Harvard University)

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[8] On zeta functions associated with prehomogeneous vector spaces, *Ann. of Math.*, **100** (1974) 131-170 (with T. Shintani).

[9] A classification of irreducible prehomogeneous vector spaces and their relative invariants, *Nagoya Math. J.*, **65** (1977) 1-155 (with T. Kimura).

[10] Micro-local analysis of prehomogeneous vector spaces, *Inventiones Math.*, **62** (1980) 117-179 (with M. Kashiwara, T. Kimura and T. Oshima).

Program of the Conference in Prof. Sato's Honor, 1992
Algebraic Analysis and Number Theory
(March 23 – March 28, 1992 at RIMS, Kyoto University)

Grigis, A. (Ecole Normal Superieur, Paris) Quantization on the sphere and applications

Ooguri, H. (RIMS) On mirror manifolds

Jimbo, M. (Kyoto University) Quantum group symmetry and lattice correlation functions

Nakayashiki, A. (Kobe University) Crystals and vertex models

Ueno, K. (Waseda University) and Shibukawa, Y. (Waseda University) Completely Z symmetric R matrix

Honda, N. (Hokkaido University) On the solvability of completely integrable systems for distributions

Uchida, M. (Osaka University) Continuation of real analytic solutions of partial differential equations up to convex conical singularities

Kataoka, K. (University of Tokyo) and Takeuchi, K. (University of Tokyo) Isotropic 2nd microlocalization due to Lebeau and Γ-analytic microfunctions

Yoshino, M. (Chuo University) and Gramchev, T. (Bulgarian Academy Sciences) WKB analysis to global solvability and hypoellipticity

Takasaki, K. (Kyoto University) W algebra, twistor and nonlinear integrable systems

Iwasaki, K. (University of Tokyo) A Hamiltonian system on the Fuchsian moduli space

Sekiguchi, J. (University of Electro-Communications) Some topics related with discriminant polynomials

Saito, M. (RIMS) On the b-function of nonisolated hypersurface singularities

Aoki, T. (Kinki University), Kawai, T. (RIMS) and Takei, Y. (Kyoto University) WKB analysis, period integrals, deformations, ...

Saito, K. (RIMS) The Teichmüller space from a view point of group representations

Sato, M. (RIMS) Algebraic analysis and I

Yoshida, H. (Kyoto University) On periods of automorphic forms

Kato, K. (Tokyo Institute of Technology) Explicit reciprocity law and values of zeta functions

Sato, F. (Rikkyo University) Zeta functions of prehomogeneous vector spaces with coefficients related to periods of automorphic forms

Kurokawa, N. (University of Tokyo) Determinant expression of zeta functions and their tensor products

Oda, T. (RIMS) Galois action on the pro-nilpotent completion of the fundamental groups of algebraic curves

Ihara, Y. (RIMS) On the Fermat quotient and "the differential of a number"

What I could do for the Mathematician Mikio Sato

S. Iyanaga

The other day, I had the great honor of receiving a letter from Professor S.-T. Yau of Harvard University asking me to contribute an article to the forthcoming issue of the *Asian Journal of Mathematics* dedicated to Mikio Sato. I thanked him for his letter and reminded him of a book entitled *Algebraic Analysis* (in two volumes) published by Academic Press, a collection of papers dedicated to Mikio Sato on the occasion of his sixtieth birthday in 1988, compiled by his friends and colleagues, to which I had contributed a note: *"Three personal reminiscences."* In my answer to Professor Yau, I wrote that in case the Asian Journal urgently needed my article, it could reproduce my old note; I could try to write a new note, but I was afraid that I would not have very much to add.

Professor Yau kindly replied that the Asian Journal appreciated my offer concerning the old note, but it would prefer to have a fresh article. Besides, Prof. Kawai sent a message to me from Kyoto to encourage my new contribution, including a copy of the manuscript of the preface to this issue, together with the notes taken by Dr. Ohyama of Osaka University from a talk by Sato in 1992 at the RIMS (Research Institute for Mathematical Sciences.)

I should be happy, if the following lines which I have written in these circumstances, could interest the reader of this journal.

*

I have noticed that the preface to this issue begins by mentioning that Professor Sato graduated from the Department of Mathematics of the University of Tokyo in 1952, majoring in number theory under my direction. I see that this statement describes, indeed, the fact; but I have felt myself both honored and embarrassed by it: honored by being reminded that I once directed the study of Professor Sato, one of today's leading mathematicians; embarrassed because I suspect that this could arouse questions such as: what kind of advice could I have given him when he studied with me, which enabled him to become such a powerful mathematician. I would be obligated to answer honestly that I could give him no useful advice in his student days. I have reflected upon what I could do for him later on, and would like to report here the result of these reflections.

*

First, I should ask for the tolerance of the reader to speak for a while of my own personal history.

I was born in 1906, 22 years before Sato, and was lucky to study, like Sato, at the University of Tokyo. This was the first University created by the Meiji government which was responsible for modernizing our country. I entered this University in 1926, studied there for three years in the undergraduate course and one and a half more years in the graduate course. I was luckier than Sato, I believe, in that my study was directed by Professor Teiji Takagi, who after having studied in Germany toward the turn of the century founded the class field theory. He reported these results at the ICM in Strassburg in 1920 without finding, however, an immediate response. It was just after World War I, and German mathematicians were not invited there. The importance of this theory was soon recognized, however, and in 1926, the year in which I entered the University, the celebrated report of the Class Field Theory by Hasse began to appear in the *"Jahresbericht"* of the German Mathematical Society, and in the next year, 1927, Artin in Hamburg succeeded in proving his "general reciprocity law" to complete this theory. I was fortunate to be admitted to Takagi's seminar in 1928-29, and Prof. Takagi kindly indicated to me some problems on which I could begin to publish research papers.

In 1931, I left Japan to pursue advanced studies in Europe, first in Hamburg with Artin where I stayed for two semesters. I was very lucky to meet Claude Chevalley, who came from Paris to study there during the same period. In 1932, the ICM took place in Zürich presided over by Fueter, a pioneer in the theory of complex multiplication, to which the class field theory brought a decisive result. Professor Takagi was invited to this Congress as one of the vice-presidents, together with mathematicians like Hilbert and Hadamard. I was happy to participate in this Congress and see Professor Takagi again, now surrounded by such respect and honor.

After Zürich, I moved to Paris where I stayed two more years. I was fortunate to be introduced by Chevalley to friends such as André Weil, Henri Cartan, and Jean Dieudonné who (in 1936, after I left Europe to return home) took the famous pen name of Bourbaki and started their well-known activities. I shall not speak in detail about what I did, what they did, nor what kind of contact I had with them in those days. I shall merely note that I somehow acquired during my stay in Europe, a kind of instinct for feeling the presence of true mathematicians around me.

I returned to Tokyo in 1934, and early the next year, I was admitted to the teaching staff of Tokyo University, where I remained until I attained the retirement age of 60 in 1967. I felt very happy to work there, as I often had excellent students in my classes who later become famous mathematicians; one of whom was, of course, Sato.

I was not happy, however, on the other hand, about World War II, which devastated our country and culminated with the tragedies of Hiroshima and Nagasaki in 1945! During the War and also for several years afterwards, we were obliged to do many things other than studying mathematics, and although I am not by nature suited for administrative work, I had to concern myself with it, particularly after the War. Japan was under occupation until 1952, but I felt that the policy of the occupying authorities tended in general toward democratization and internationalization, thus toward the benefit of our country. To handle scientific administration, a group called the Science Council of Japan was created in 1948 under the influence of the occupation authorities. It consisted of scientists elected from scientific societies and was endowed with the authority to advise the government on such subjects as (1) the organization of international scientific meetings (aided by governmental budgets), and (2) the creation of scientific research institutes.

I was elected a member of this Council by the Mathematical Society of Japan in 1948, and reelected several times so that I remained on this Council for some ten years, during which time I made special efforts to realize (1) the International Symposium on Algebraic Number Theory in Tokyo-Nikko, 1955 and (2) establish the Research Institute for Mathematical Sciences (RIMS). The latter project was proposed by the Council in 1958 and was established as an institute attached to Kyoto University in 1963.

Before the Symposium in 1955, I was able to attend the ICM at Harvard in 1950 where I had the pleasure of renewing my friendship with mathematicians like Artin, Chevalley, and Weil with whom I had become acquainted in Europe before the War. On the same occasion, I also was able to attend the preparatory meeting for the creation of the International Mathematical Union at Columbia University, which was convoked by Marshall Stone just before the Congress. In this meeting it was decided that the new union would be established once 20 countries expressed their intention to join it, which occurred two years later. At the inaugural meeting in Italy, I was named a member of the executive committee. This committee, presided over by Stone, met for the first time in Paris in 1954, where my proposal to organize a symposium in Japan in 1955 was warmly considered. I quickly made the same proposal to the Science Council, and to my great relief, its agreement was obtained. We were able to invite Artin, Chevalley, Weil, and others; and Professor Takagi, then at the age of 80, attended as honorary chairman. More time and efforts were needed to establish RIMS. Of course, I made these efforts without any idea that Sato and his school would later find their place at that institute.

*

I return to Sato with apologies for the long digression. As mentioned above, he joined my seminar in his last student year at Tokyo University in 1952. It was seven years after the end of the War, but the general situation of Japan had not yet quite recovered.

I do not remember exactly how many members were in my seminar that year; but I do remember that Goro Shimura and Mikio Sato were among them, and we used the Foundations of Algebraic Geometry of André Weil as our text. I particularly admired the precision and clarity of the exposé of Shimura. I also found Sato's exposé remarkable, but everyone felt that Shimura was more deeply interested in the subject and that we could count on his progress. In fact, he soon began to write papers. I helped correct his English in the beginning, but soon I found that he was doing everything perfectly, and I suggested he send his manuscripts directly to André Weil, which he did. We can read his article, "André Weil as I knew him" in the recent special issue of the Notices of the American Mathematical Society in memory of André Weil, and see that Shimura and Weil remained good friends until the latter's death last August. Shimura also distinguished himself at the Symposium of 1955.

The situation with Sato was not the same. He came to me just before his graduation to express his desire to turn to the subject of theoretical physics, and I only had to encourage him in his study of the new field. He came to see me now and then to tell me what interested him at each period– which was often beyond my comprehension.

I received a telephone call from him on a winter day in 1957. (In my old note I wrote 1957 or 58, but now I am sure that it was in 1957.) He told me that he had a new idea about which he would like to speak to me. I was, of course, happy to see him again. It was some time after the symposium in Tokyo-Nikko, which had been prominently reported in the Japanese newspapers. I wondered if this had prompted him to return to algebraic geometry. What he told me in my study that evening, however, was a very different kind of thing. It concerned an interpretation of generalized functions such as Dirac's delta function as boundary values of analytic functions. I knew that Laurent Schwartz had received a Fields medal in 1950 for his idea of distributions. Of course, Sato also knew this, but thought that his entirely different idea could be used more effectively in certain cases. His account fascinated me, and I felt myself in the presence of a true mathematician! I asked him if he wouldn't like to return to mathematics, and was delighted to hear his affirmative answer.

The following day, I enthusiastically told my colleague Kosaku Yosida about Sato. Yoshida was in charge of analysis in our department, and he agreed to invite Sato to take a position as his assistant, which was fortunately vacant at that time. I have written in detail what I remember of that time in my old note, which I shall not repeat here.

In my old note, I spoke of two more reminiscences: one at the Institute of Princeton in 1960, when Sato arrived from Japan for the first time. I happened to be there at the same time with Weil and Schwartz. I had looked forward to introducing Sato to them, expecting that they would immediately understand the importance of Sato's "theory of hyperfunctions." Unfortunately, this hope was not realized, but Sato began his research at the institute on prehomogeneous vector spaces, an interesting new subject in number theory, and also on other subjects. The other encounter was at the ceremony to mark his winning the Fujiwara prize in 1987, when I met him and his wife.

I believe I don't have to repeat what I have written in my old note, nor what is explained in other parts of this issue. I shall, however, tell how delighted I was on two occasions: first, when I welcomed him to my home when he came from Kyoto to tell me that he had just received the honor of being named officially as a person who had performed distinguished service in a cultural field; and second, when I read in the notes of his talk in 1992 that he retains pleasant memories of the day when he visited my study in 1957.

The early days of the theory of hyperfunctions and differential equations

Hikosaburo Komatsu

I have never worked with Professor Mikio Sato. For him I should have been rather a critic than a collaborator. I have tried, however, to propagate his mathematics as far as I can understand, and I am proud of my role in this respect.

Sato graduated from the Department of Mathematics, University of Tokyo in 1952. Goro Shimura was his classmate. Ichiro Satake and Michio Kuga are approximately of the same age, and Yutaka Taniyama graduated a year later. All of them studied number theory and/or algebraic geometry with Professor Iyanaga. The University of Tokyo had a strong tradition of number theory created by Professor Takagi. A. Weil had solved in 1948 the Riemann hypothesis for congruence zeta functions for curves, and his conjecture for the higher dimensional case fascinated those people.

After graduation, however, he reentered the Department of Physics, and moved in 1954 to the Graduate School of Tokyo University of Education (the original of the present University of Tsukuba) to work with Professor Tomonaga. I don't know the reason. He might merely have followed Professor Kodaira's example of a double major, or for him physics might have looked easier to study with his limited time, because he had to work to support his family. Japan was very poor at that time. (My first monthly salary in 1960 as an assistant at University of Tokyo was an equivalent of only 37 US dollars.) Moreover, Japan was not yet fully accepted in international society except in the area of physics. H. Furuhashi was breaking many world records in swimming, but he was not allowed to take part in the Olympic Games in London in 1948. On the other hand, Professor Yukawa was awarded the Nobel Prize in physics in 1949. In mathematics, Professor Kodaira received the Fields

Medal in 1954, but that was mainly for his achievements in Princeton. Japan was able to host its first international conference of mathematics only in 1955. (Shimura and Taniyama told their conjecture to Weil at this conference on Algebraic Number Theory.)

In 1958, with his theory of hyperfunctions, Sato came back to the Department of Mathematics as an assistant of Professor Yosida. That was the first time I met him. I was a graduate student of the first grade in Yosida's seminar.

Sato doesn't mention it explicitly, but there is a long history of the complex method of real analysis. Special functions are treated from this point of view, in particular, in the text books of applied mathematics. Whittaker-Watson [1] was his favorite book. Nobody, however, had ever considered the whole "boundary values" of holomorphic functions. I imagine that in extending them to higher dimensional cases, he was motivated by the method of dispersion relations, which was going to be in fashion in quantum field theory. He uses the word "thresholds" to mean singularities such as poles and branching points of defining holomorphic functions in the same way as physicists.

Everybody welcomed his theory in the one-dimensional case, but analysts were rather cool towards his theory in the multi-dimensional case. They were not familiar with the language of cohomology theory, and were embarrassed by the fact that the space of hyperfunctions had no natural topologies.

Japan was not retarded in accepting the sheaf theory. People knew that H. Cartan and J.-P. Serre simplified Oka's theory by introducing sheaves, and that Kodaira was obtaining remarkable results on algebraic geometry by the sheaf theory and analysis. As early as the years 1955-56, Professor Akizuki of Kyoto University published a book on harmonic integrals and expounded the cohomology theory of sheaves. Yet, the relative theory for open pairs was new, and the flabby (or hyperfine in Sato's terminology) and injective resolutions were about to be introduced by R. Godement [2] and A. Grothendieck [3], respectively.

Sato's research announcements [4, 5, 6] published in 1958 are all written well, and so is the first part [7] of his full paper. All definitions, including that of relative cohomology modules, are stated without ambiguity, and the main properties of hyperfunctions are fully and clearly explained. There are also many interesting examples of hyperfunctions of one variable. This is not the case, however, with the second part [8]. Too many proofs are omitted to follow the paper. Rumor had it that since Sato was too slow in writing the paper, Professor Iyanaga invited Sato to his house every day to write and did not allow Sato to take written sheets out of his house. Sato announced in the paper that the proofs would be published in a subsequent paper. Unfortunately, the promise was not fulfilled.

Essentially he had to work by himself. In Japan mathematics was thought to be a personal matter at that time. Some changes were going on, however. His classmates mentioned above, had, in the mean time, positions in the Department of Mathematics at Komaba Campus for general education, and they started a seminar named Seminar on Topology together with topologists and differential geometers. I don't think Sato had time to attend it, but the lecture notes they published under the leadership of Professor Y. Kawada should have benefited him. He also says that he learned mathematics from the Encyclopedic Dictionary of Mathematics [9], edited by the Mathematical Society of Japan.

Another activity, which resulted in the creation in 1963 of the Research Institute for Mathematical Sciences at Kyoto University, started around the same time. The institute is originally modeled after the Courant Institute of Mathematical Sciences. In order to obtain the support of neighboring disciplines, Professor Iyanaga and others organized a big research group of more than 500 members, including not only applied mathematicians and statisticians but also physicists and computer scientists. Since 1959, they organized many joint meetings and initiated some joint projects. Sato served as the first secretary of the group.

In 1960 Sato was promoted to a lecturer at the Tokyo University of Education, and in 1960-62 he visited Princeton. At the farewell colloquium at the University of Tokyo, he gave a talk on "Linear partial differential equations". After a brief review of E. Cartan's theory of exterior differential systems and Cauchy's characteristics, he exhibited his view of regarding systems of linear partial differential equations as coherent left modules over the noncommutative ring of linear partial differential operators, and solutions as homomorphisms of the modules into function spaces. Then he claimed that the solutions are more or less characterized by the module in the maximally overdetermined case and that elementary solutions (of single equations) should be computed in this way.

As I mentioned above, Japanese analysts were cool towards Sato's theory. That was also the reaction of analysists in the world. A. Martineau was the only exception. In 1960, he introduced Sato's hyperfunctions at the Bourbaki Seminar [10]. Then, he reconstructed the theory of distributions, looking on them as cohomology classes with bounds of holomorphic functions, and proved among others a generalized edge-of-the-wedge theorem for holomorphic functions with distributional boundary values [11]. He reported the results in 1964 at a conference in Lisbon. Unfortunately, the proceedings were published much later.

In 1964-66 I was visiting Stanford University. I started there my work on fractional powers of operators in Banach spaces. In my first paper [12] I quoted Sato's paper [7]. For me, Sato's interpretation of power distributions x_+^α was more natural than Gel'fand's [13]. In the second year, I was asked by Reese Harvey to become his thesis advisor. I suggested that he reconstruct the theory of hyperfunctions of several variables along Martineau's sketch, and then discuss linear partial differential equations with constant coefficients. Until the summer of 1964, Hörmander was a professor at Stanford, and gave a course on several complex variables. His book [14] is its lecture notes. D. C. Spencer moved to Stanford in 1964, and Kodaira in 1965. Therefore, Harvey had enough background.

In a very short time, Harvey completed his thesis [15] in which he proved the existence of hyperfunction solutions of single equations on every domain, and the analyticity of hyperfunction solutions to elliptic equations. I thought these were the first results on hyperfunctions, and wrote this to Y. and T. Kōmura who were working with Professor Köthe in Frankfurt. I was planning to visit Frankfurt and Heidelberg on my way to attend Moscow Congress. To my astonishment, they replied that the same results were obtained by G. Bengel in Münster. I learned later that Bengel wrote his thesis [16] in Montpellier with Martineau. During my stay in Heidelberg/Frankfurt, I extended their results to systems and showed, in particular, that the Köthe-Martineau duality theorem and the Alexander-Pontrjagin duality theorem were easy consequences [17, 18]. The former was the starting point of the Martineau-Harvey theory of hyperfunctions, and the latter implies the Jordan-Brouwer theorem. In Moscow I presented it in a short communication. I still meet people today who remember this talk.

In the autumn of 1966, I gave a series of lectures at the University of Paris IV [19]. The audience diminished rapidly, but I remember that Professor Schwartz, F. Trèves, P. Schapira, K. Aomoto, and M. Morimoto remained. Morimoto went to Nice the next year to work with Martineau, and completed the edge-of-the-wedge theorem to be employed in the theory of hyperfunctions and microfunctions [20, 21]. In 1967-68 I gave a course in Tokyo [22]. T. Kawai, A. Kaneko, and M. Kashiwara were among the attendants. Professor Iyanaga was also present.

Sato came back from Princeton in 1962, but he went to Osaka the next year to become a professor. Then he visited Columbia University in 1964-66. Therefore, I had not met him for a long time when I saw him in New York on my way to Europe. He was interested in Harvey's work and asked why he had to restrict himself to the constant coefficient case. In his proof Harvey employed Ehrenpreis' fundamental principle [23]. There was also another technical difficulty. Sato himself had been working on prehomogeneous spaces and powers of relative invariants.

In April, 1968, he came back to Tokyo to take the position of professor at Komaba. Japan was going to host the second international conference in mathematics after the war, this time on Functional Analysis and Related Topics, in April, 1969. With this conference in view, Sato and I started a seminar. About 20 members met at three o'clock in the afternoon every Saturday! Often the meeting lasted until after seven o'clock. This enthusiasm continued until Sato moved to the Research Institute for Mathematical Sciences in the summer of 1970 in spite of the surge of student rioting at that time (in which I was busy defending the faculty and some members of the seminar were active on the student side).

Microlocal Analysis was born at the conference in 1969. Sato [24] introduced the sheaf $\mathcal{C}$ of microfunctions on the cotangent sphere bundle S^*M, which describes the singularities of hyperfunctions on the manifold M modulo the real-analytic functions, and claimed that the action of a linear differential operator on $\mathcal{C}$ is injective at non-characteristic points in S^*M. This was his formulation of Weyl's lemma in the variable coefficient case. On the other hand, Hörmander talked of his ideas of Fourier integral operators not at the conference, but at a private seminar. This meeting triggered an intense competition between two schools of microlocal analysis. The monumental papers [25], [26] and [27] are its outcome.

Sato's first idea seems to modify F. John's construction of elementary solutions [28] microlocally, and for microlocal decomposition of singularities of hyperfunctions to use the integral decomposition of the δ function into curvilinear waves. The published paper [25] employed instead the analytic pseudo-differential operators of Kashiwara-Kawai [29].

Whenever we read Sato's paper, we are overwhelmed by his too huge and too abstract settings. If you attend his lectures, however, you will have a quite different impression. He spends most of his time in explaining historical backgrounds and examples, and his theory appears only at the last moment. I believe that that is his way of research.

After he finds a target from history, he spends most of his efforts in computing examples and then formulating the problem most naturally.

I appreciated Sato's theory of hyperfunctions for his interpretation of power functions x_+^α. I think this was a right attitude. He might have introduced the hyperfunctions to give the most natural definition of x_+^α. They are used in Riemann's proof of the functional equation of ζ function [1], and in fractional integration and differentiation. In several variables, powers of quadratic forms appear in elementary solutions of the Laplacian, and the wave operator. These properties are equivalent to the fact that the Fourier transforms of those functions have similar forms. M. Riesz [30], Gel'fand [13] and many others contributed in this direction. Sato was not satisfied with either of them, and constructed the theory of prehomogeneous spaces.

Much later in December 1980, Sato found that soliton equations are described by the infinite-dimensional Grassmannian manifolds. The next February he gave a series of lectures in Tokyo, in which he spent most of the time showing how the products of irreducible characters of symmetric groups are decomposed into the sums of irreducible characters. I am convinced that this was exactly the way that he unveiled the unexpected relation.

This reminds me of a Japanese mathematician Katahiro Takebe in the early 18th century. He succeeded his teacher Takakazu Seki working on the problem of finding the formula for arc length 2θ of a section of the unit circle from its height $h = 1 - \cos\theta$. Given a height h, they were able to compute the half arc length θ as accurately as they wished by a tedious method of bisection similar to Archimedes'. Seki determined an accurate approximate polynomial by interpolation. Takebe was not satisfied with Seki's result and after a long trial detected that the right formula was a power series from the value of

$$\begin{aligned}\theta^2/4 &= 0.0000010000\ 0033333351\ 1111225396\ 9066667282\\ &\quad 3477694795\ 9587535726\ 7148043103\ 832\ldots\end{aligned}$$

for $h = 2 \cdot 10^{-6}$. Thus, power series were discovered independently in Japan in 1722.

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