Interview with Mikio Sato

Mikio Sato is a mathematician of great depth and originality. He was born in Japan in 1928 and received his Ph.D. from the University of Tokyo in 1963. He was a professor at Osaka University and the University of Tokyo before moving to the Research Institute for Mathematical Sciences (RIMS) at Kyoto University in 1970. He served as the director of RIMS from 1987 to 1991. He is now a professor emeritus at Kyoto University. Among Sato's many honors are the Asahi Prize of Science (1969), the Japan Academy Prize (1976), the Person of Cultural Merit Award of the Japanese Education Ministry (1984), the Fujiwara Prize (1987), the Schock Prize of the Royal Swedish Academy of Sciences (1997), and the Wolf Prize (2003).

This interview was conducted in August 1990 by the late Emmanuel Andronikof; a brief account of his life appears in the sidebar. Sato's contributions to mathematics are described in the article "Mikio Sato, a visionary of mathematics" by Pierre Schapira, in this issue of the *Notices*.

Andronikof prepared the interview transcript, which was edited by Andrea D'Agnolo of the Università degli Studi di Padova. Masaki Kashiwara of RIMS and Tetsuji Miwa of Kyoto University helped in various ways, including checking the interview text and assembling the list of papers by Sato. The *Notices* gratefully acknowledges all of these contributions.

—Allyn Jackson

Learning Mathematics in Post-War Japan

Andronikof: *What was it like, learning mathematics in post-war Japan?*

Sato: You know, there is a saying that goes like this: in happy times lives are all the same, but sorrows bring each individual a different story. In other words, I can tell of my hardships, but this will not answer your general question. Besides, I think the reader's interest should lie in the formation of the ideas of hyperfunctions, microlocal analysis, and so forth. It is true that in my young age I encountered some difficulties, but I don't think I should put emphasis on such personal matters.

Andronikof: *Still, I think we could start from a personal level. We could mix up journalism with mathematics, and go from one to the other. After all, you might not have become a—I would say, such a—mathematician without the experience of these hard times.*

Sato: Let me tell you this. In pre-war Japan, school was organized like the old German system. Elementary school ranged from the age of six to twelve, then followed middle school from twelve to seventeen, then three years of high school before entering university, where you graduated after three years. After the War, the system was changed to the American one: the five years of middle school were replaced by three years of junior high school and three years of high school. In order to become a graduate student, one then has to attend university for four years.

When I entered the middle school in Tokyo in 1941, I was already lagging behind: in Japan, the school year starts in early April, and I was born in late April 1928. The system was rigid, and thus I had to wait one year before getting in. Actually, it did not really matter, since I was not a quick boy. On the contrary, when I was a child, say, four like my son is now[1], I was called *bonchan*, which means a boy who is very slow in responding, very inadequate. I think I am very much the same now, ha! ha! Anyway, I turned thirteen right after entering middle school. In December of that year, Japan entered the war against the allied forces: U.S., UK, Holland, and China.

Andronikof: *Hectic times?*

Sato: Not so much in the beginning, as Japan was in a winning position. After Pearl Harbor, the British fleet was destroyed in the Far East, Singapore was occupied, and so on. Things looked favorable for Japan. But soon after, a year or so later, things started changing.

This was the beginning of my hard experiences. My regular courses in middle school lasted for only two years, and the rest of my school life was total chaos. The war in the Pacific ended on May 15, 1945. The first atomic bomb was dropped on August 6, 1945, after which the USSR declared war on Japan in order to secure the Kurilsk and Sachalin islands. At that time I was fifteen. Being a teenager, I had to work in factories. From 1943 to 1945, I had to carry coal. Very hard work... bad food... In late 1944, the systematic bombings of

[1] *That is, in August 1990.*

civilian targets by the U.S. started, after the fall of Micronesia, which then served as a base. In early 1945, Tokyo was a target. The first attack on Tokyo was on March 10, 1945, and some 80,000 were killed that night, but my family was spared. This was a short respite, since a month later there was a second attack and another broad area of Tokyo was burned down, including our house. I narrowly escaped the fire. We lost everything, but the family was safe. Due to the smoke, I partially lost my eyesight for a couple of weeks. Who cares about such details, anyway? Well, Japan had been rough on some people elsewhere, like the occupation army in China, and now it was hard for Japan. It was hard for many.

But I didn't intend to get into such detail... Isn't it tiresome?

Andronikof: *The tape recorder has no opinion, besides, I am personally interested.*

Sato: General Tōjō, Japan's military strong man, had taken power in Japan and conducted the whole war. He remained as prime minister at the time, and he, and the government, decided to move the schools to the countryside—they were practically closed, anyway. We could not find a place or job outside Tokyo. In a way, our family collapsed. We did not have any relatives in the countryside we could stay with, and we had no house. Pupils with family or friends in the countryside were supposed to go there, and those without such advantages had to join a party led by a schoolteacher. My father was a lawyer, but in 1941 or 1942 he fell ill and could not work as such anymore. Still, he thought that he could provide for his family. But then there was a sharp devaluation of the yen, by a factor of 100 (a yen was nearly the equivalent to a dollar before the war). Soon, the money my father had left from his work was down to practically nothing. We could not live on that anymore, and we nearly starved.

But let me talk about my formation. In elementary school we learned some arithmetic, coming from traditional Japanese mathematics. This is arithmetic for small boys. A typical example is the "counting of tortoises and cranes". Say there are several tortoises and several cranes. The total number is 7 and the total number of legs is 18. Then, how many tortoises are there? We had to manage to follow the reasoning without using equations.

I became very interested in mathematics at the age of twelve, I guess. This is when I moved from elementary school to middle school, and I had my first experience with algebra. We learned how to handle x's and y's, so things were solved very systematically. I was charmed by the simplicity of it. I was amazed by algebra, and in two years I made very quick progress and learned, for example, about complex numbers and their use in trigonometrical formulas, like the Euler formula. Of course, this was not taught at school. I remember saving my money systematically, by walking for hours instead of taking buses and streetcars to central Tokyo, and then spending hours in the bookstore to find the book that would give me the best value for my money. I remember an expository book by Fujimori on the theory of complex numbers where power series expansion and Taylor series were touched upon. The conformal transformation for the airplane wings was given as an example of the usefulness of complex numbers in the war industry... Some kind of propaganda book! Another book I read at that age was a book by Iwata on projective geometry: Desargues' theorem, Pascal's theorem, Brianchon's theorem dual to Pascal's...

My teacher of mathematics at that time was Mr. Ohashi, who is still alive. I actually met him again six years ago, after more than forty years. He contacted me after seeing my name in a newspaper or on television. I was lucky to have a good teacher. Of course I don't mean that he taught me such mathematical things during my first year. At that time I was rather timid, not talkative at all, and I did not try to consult him on my choice of books. But still, he was very encouraging: he always pushed me and taught me. That gave me the feeling that indulging in mathematics was not a game. This was very important, because on many occasions—at least in the Japanese educational system—pupils are supposed to follow the lead of the teacher and should not get off the track which he sets, whereas I was running completely off the track of the educational system. So I was just feeling it as a gift, so to speak, I was enjoying a kind of permitted pleasure.

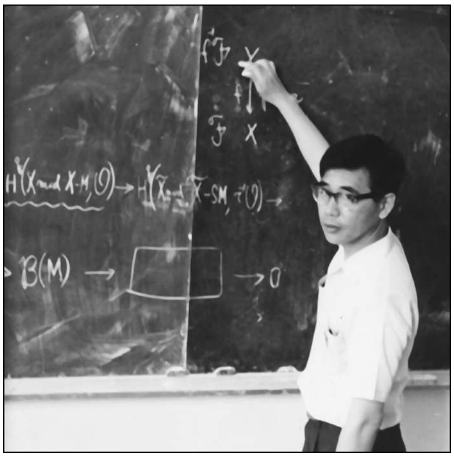
Andronikof: *Were the conditions such that you had plenty of time on your own?*

Sato: Well, in the daytime we were supposed to stay at school, whether we had any classes or not. Besides, the only mathematical library was in Tokyo, and not a big library. Unbelievable! It was the contrary of reality in Japan today.

Andronikof: *So, you have always been interested in mathematics.*

Sato: Since twelve years of age, yes, and also a little in physics. But you see, my interest in physics grew much later, around 1945, when I read a university textbook which I had the opportunity to borrow from some graduate student.

In a way, the first three years of middle school were very fruitful for me. Afterwards, I developed my way of mathematical thinking through reading books and making calculations, solving interesting problems and so on. But I did not receive any good education after that, ha! ha! I was lucky enough to have had a chance of awakening my ability in mathematics at an early age. For the rest, I was sort of a dull boy. In the Japanese school system there are such subjects as geography, where memorizing names and years and so on is important, and in this field my performance was extremely low. That gave me the feeling that studying at school was a kind of unpleasant job. Doing my own mathematics—that is, not school mathematics, but reading those books I mentioned—was like watching television would be for a present-day boy. See, I was probably indulging in such things to forget the unpleasant school courses. A way to escape the school system. During and after the war things became harder and harder and I went deeper and deeper into mathematics, so to speak, like another would dive into alcohol.


Sato at blackboard, around 1972.

After middle school—though we had not completed it... I was admitted to high school. At that time, high school was rather elitist, more like École Polytechnique or École Normale Supérieure in France. The high school that I entered was called the First High School, and was closely attached to Tōdai[2] (Imperial University at the time). Both were national, i.e., non-private. The First High School is considered to be the top of elite schools, and I was lucky enough to skip the entrance examinations, because of the war. Well, there was a kind of test, but just to check some ability in mathematics: if they had tested my knowledge, then I couldn't have entered. Today, there are entrance examinations at many universities, including Tokyo or Kyoto: a bad test... But this is not interesting.

After the war, chaos occurred again—or rather, persisted. As I said, because of the devaluation of the yen, my family was starving. My father was sick, and I had a younger sister (by nine years) and a younger brother (by five years). I had to support them, so, in 1948, after three years of the First High School, I immediately started to work as a full-time teacher at the *new* high school, just when the school system was changed and middle school was cut by half. Housing and food conditions were extremely bad at the time, as you can imagine: like in Eastern Europe or Southeast Asia now. I entered Tōdai in 1949, having failed to enter in 1948. I had very little time to get prepared, then.

These hard times as schoolteacher lasted ten years, from 1948 to 1958. In 1958 I published the theory of hyperfunctions, in order to get a job at the university. I was an old student at the time, but it was like today: finishing university is sort of automatic, provided you succeed in getting in, where the competition is very tough.

Andronikof: *I read in your CV that you got a BSc in physics after your BSc in mathematics at Tōdai.*

Sato: You see, in Japan teaching depends on each professor, and one of my professors was very strict. At the time I was to graduate, he called me up, and told me that my term paper was very good but I had not attended the mandatory exercise sessions—not even once. This was an obligation that I didn't know of. Remember, that's why I was called *bonchan* when I was a little boy, and I'm still very much that way now. So, he said: "I cannot give you the points, so you cannot graduate". Then, he remained silent and watched me for a good minute. He opened his mouth again and said: "Okay, I'll give you the lowest points, so you can just graduate. Your paper is the top one". But this barred me from getting a position at the university as an assistant, which is customary for top students. Being assistant in Japan is a tenured position. The second-best student may also get some special position, and hence is assured of some top financial support. Anyway, I lost that kind of chance then. Since at the time I had also become interested in theoretical physics, I just moved to physics for two years under the new, American-style university system. I was still teaching full time in high school, so in physics I ran into the same academic problems as in mathematics. After two years at the Tōdai Physics Department, I moved to the graduate school of another university, Tokyo School of Education, where Professor Tomonaga taught theoretical physics. I stayed there until 1958.

This was the end of my twenties. At the time I was undergoing some kind of crisis in physical strength. Since by then my younger brother and sister were able to support themselves, my duty to them was sort of accomplished. I was able to return to my own life, so to speak, and go back to mathematics.

The Birth of Hyperfunctions and Microfunctions

Andronikof: *So you decided to go back to mathematics, rather than physics?*

Sato: Yes, and it was a good decision since competition in physics seemed stiffer. See, after these tough years I was beginning to feel physically tired, and my youth was leaving me. Even if I wasn't a man of quick response, I nevertheless understood that I had to face real life, so to speak, and to try to show what I could do in mathematics.

[2] *University of Tokyo.*

Names Mentioned in the Interview

Below is a list of names of mathematicians mentioned during the interview.

Armand Borel
Élie Cartan
Etsurō Date
Pierre Deligne
Jean Ecalle
Leon Ehrenpreis
Daisuje Fujiwara
Roger Godement
Alexandre Grothendieck
Ryōgo Hirota
Sin Hitotumatu
Yasutaka Ihara
Kenkichi Iwasawa
Shōkichi Iyanaga
Michio Jimbo
Akira Kaneko
Masaki Kashiwara
Yukiyosi Kawada
Takahiro Kawai
Tatsuo Kimura
Hikosaburo Komatsu
Serge Lang
Jean Leray
André Martineau (1930–1972)
Yozō Matsushima
Barry M. McCoy
Tetsuji Miwa
Mitsuo Morimoto
Atsushi Nakayashiki
Yousuke Ohyama
Kiyoshi Oka
Frédéric Pham
Yasuko Sato
Pierre Schapira
Jean-Pierre Serre
Goro Shimura
Takuro Shintani (1943–1980)
Masuo Suzuki
Teiji Takagi
Kanehisa Takasaki
Shunichi Tanaka
Junichi Uchiyama
André Voros
Kōsaku Yosida
André Weil

My advisor at the mathematics department was Professor Iyanaga, and I wanted to show him what I was capable of. High school teachers had a forty-day vacation between the first and second semester. So, during the summer of 1957 I tried to prepare something that I could show him, and that was hyperfunctions. I worked out hyperfunction series and outlined the theory for several variables—though the complete theory was finished later, since it required a generalization of cohomology theory. In December of that year, I went to see Professor Iyanaga, after an interruption of some years, and told him about it. Professor Iyanaga showed interest in my work and persuaded Professor Kōsaku Yosida to offer me a position as an assistant.

Actually, it seems that Iyanaga was one of the professors who wanted me to get a position as an assistant already when I had graduated, but he was not a senior professor at the time, so his opinion did not prevail. I was glad that Professor Iyanaga showed interest in hyperfunction theory. I was lucky: if the professor had not been Iyanaga—if he were a specialist in analysis, for example—perhaps he would have told me: "You are doing nothing". Fortunately, Iyanaga was a generous and open-minded mathematician, open enough to appreciate what I was doing. He was a student of Professor Teiji Takagi, the founder of class field theory and number theory.

Of course, coming from ten years as a schoolteacher, this new position made me lose some financial advantages. But finally, after the hard times, I could have the pleasure of doing solely mathematics. What I wanted to do was to organize mathematics.

You see, being a high school teacher then was not like today. We had to work very hard. So, I couldn't do difficult things, but only general things like sheaf theory, category theory, and so on. I just tried to organize my own mathematics and studied a lot, always keeping my interest alive. During that period, I had the opportunity to see many papers published in *Sūgaku*[3]. In particular, my construction of relative cohomology was inspired by a report on complex variables by Hitotumatu, which contained a short account on sheaf theory and cohomology of sheaves, perhaps in three pages. *Sūgaku* was a very useful journal, which has been central to me: a compact publication covering every branch of mathematics. There were many journals that I could not get, so *Sūgaku* was essentially the only publication I was reading. And this, usually, when I was commuting on crowded public transportation, during rush hour. Well, some people read newspapers, anyway.

I believe that after ten years I had quite good ideas about theoretical physics and diverse branches of mathematics. Like a spider, I went on spinning my web for ten years, extending it by attaching it to different places. Some things get stuck in it, some go through. You go to some

[3] *A journal published in Japanese by the Mathematical Society of Japan. Recent volumes are translated into English and published by the AMS under the title* Sugaku Expositions.

things, keeping some others for later. But you don't let go.

Anyway, I was assistant to Professor Yosida in Tōdai for two years, and in 1960 I moved to Tokyo University of Education as a lecturer—a higher position than assistant. Professor Iyanaga sent a copy of my work to André Weil who showed some interest, and suggested that I come to the Institute for Advanced Study (IAS). I visited the IAS from the fall of 1960 to 1962. But before leaving for Princeton, in June 1960, I gave a talk at the Extended Colloquium of Tōdai. This was a periodic meeting, organized by the Tōdai Mathematics Department twice a year. There, I had the opportunity to present my program in analysis. I explained how a manifold is the geometric counterpart of a commutative ring, and vector bundles are the counterpart of modules over that ring, and if you go to the non-commutative case you can treat linear and nonlinear differential equations. From this point of view, linear equations are defined to be $\mathcal{D}$-modules, and if you write $\mathcal{D}$ in a more general form, you can consider nonlinear systems.

At that time, that method was only built to establish the algebraic theory of Picard-Vessiot. This has not much contact with pure analysis, like the study of hyperbolic equations. In that field, the more geometrical methods of Élie Cartan, based on the theory of differential systems, were considered more effective. Nonetheless, as I have told several people, like Masaki Kashiwara or Pierre Schapira, I already had the feeling that Cartan's methods were not the right ones to build a general nonlinear theory. But it was only after 1970 that I—how should I say—I became determined to throw away exterior differential methods and stick to this new point of view.

Anyway, in my 1960 talk I clearly stated the setting of $\mathcal{D}$-module theory in the linear case, the notion of maximally overdetermined systems (a name that we later changed to *holonomic systems*), and the important role that holonomic systems play even in the study of overdetermined systems, through elementary solutions. For example, the Riemann function of a hyperbolic equation usually satisfies a holonomic system. I also explained my program using homological methods: $\mathcal{H}om$ describing the homogeneous solutions to the system, $\mathcal{E}xt^1$ the obstruction to solvability, and things like that.

Andronikof: *Did that talk contain maximally overdetermined systems? That is, did you have an idea of the involutivity theorem*[4]*?*

[4] *This theorem is a kind of analogue in the theory of differential equations to the uncertainty principle in quantum mechanics. It asserts that characteristic (co)directions constitute a variety of low codimension in the phase space.*

Sato: Mmm… Not exactly. It was only ten years later, after the establishment of microlocal analysis, that everything was clarified. Actually, I already had some vague ideas before 1960, but then I had only spent two years on these problems. I then moved to Princeton, and there I had to change my subject. I'll tell you about that later.

The first half of my talk at the colloquium in June 1960 was recorded in the notes by Hikosaburo Komatsu.

Andronikof: *What happened to the second half?*

Sato: Well, I kept going on and went into the nonlinear systems, and so on and so on… and he just couldn't keep up, ha! ha! You don't have to tell him this. Anyway, I am certainly indebted to Komatsu because this is a very rare case of a record of my talks. He was introduced to me in 1958 by Professor Yosida, of whom he was the best student. Komatsu was the first man who really understood hyperfunction theory, and he was just a graduate student!

At that time I gave a lot of talks at different occasions: for example, I gave a talk about derived categories. I needed that kind of theory because the notion of spectral sequence by Jean Leray was inconvenient in my theory. So I wanted to improve it and produced derived categories between 1958 and 1960.

Andronikof: *Were derived categories included in your 1960 colloquium talk?*

Sato: No, not at the colloquium talk. Professor Kawada, a number theorist at Tōdai, was very kind to me and organized some sort of seminar in 1959—small periodic meetings—to offer me the chance to expose my theory in a systematic way. There were a few lectures in his office, for a limited audience. But I found that no one understood me. So after a few times, I don't remember the exact number, this collapsed. Actually, Professor Kawada himself wondered daily how long it would last, ha! ha! He couldn't get the participants to understand.

This was before the summer vacation of 1959/1960. During the vacation, I had to attend some English training, before moving to Princeton, to be able to get a Fulbright grant. This is the kind of competition I'm not too good at… Then, I remember crossing the ocean on a 10,000-ton ship and crossing the USA by train: a very interesting trip. The first one for me.

Andronikof: *So you left behind some people pondering over your theories?*

Sato: Mmm… Well, I do a poor expository job in general. Most of the audience gets lost on the way.

Andronikof: *That might be due to the contents, which are always new.*

Sato: You see, it seems I cannot adapt to the audience. I just expose my ideas according to my way of thinking and pay little attention to how the audience receives them.

Let me tell you one more thing that happened before leaving for Princeton. It concerns the birth of microfunctions. On one occasion, a summer school I think, Professor Hitotumatu—who was just four years my senior—talked about the edge-of-the-wedge theorem in the theory of several complex variables. Of course, this was not done in the framework of hyperfunctions. So, I just mentioned that that concept could be better handled in the hyperfunction category: I had the cotangent nature of boundary values in mind.

Andronikof: *You knew at once that it should be the cotangent bundle*[5] *and not the tangent bundle?*

Sato: Yes, because of Cousin-like theorems: a holomorphic function can be decomposed into greater domains. At that time, I clearly felt the existence of some kind of microlocal structure underlying hyperfunctions, but I didn't consider it seriously. Anyway, that impression didn't leave me for a long time. It stayed with me.

For nine years, after my active period of 1958-1960, there was a long intermission for hyperfunctions. From 1960 to 1969 I didn't work in that field at all. Then, in April 1969, Professor Yosida held an international symposium on functional analysis at RIMS[6], and I was asked to give a talk. I think I was told this in December 1968. So I tried to organize those ideas I had ten years before and started to compute the theory. I found that my thought was correct. This time I wanted to show that the decomposition of hyperfunctions into cotangent components is actually important in analysis. I wanted to show that it would be useful in order to establish some results in mathematical physics. I wrote this down in less than a month, and this was the start of microfunction theory.

Andronikof: *The interruption in hyperfunction theory was due to your stay in Princeton?*

Sato: Yes. It seemed to me that even André Weil did not like my way of putting things in terms of cohomology very much. In fact, I learned that he was very much against cohomology. I got the idea that hyperfunctions were not taken very seriously, and since I was sort of a little boy (though I was 32!) I just wanted to show the usefulness of my ideas. My general program, which I had expanded at the colloquium talk of 1960, was just a very formal kind of general nonsense. I then wanted to give some concrete example of it in the analysis of differential equations. But my knowledge of that field was very poor. I'm not a reader of big books and specialized papers. I live for practical examples and, just as now, I was very slow in developing my deeper thoughts.

But let me go back to the talk I had occasion to give in 1969. I found that microlocal analysis could explain several interactions with classical analysis. This time I wanted to show that my theory was not just nonsense, but could be applied to explicit problems. I was confident that microlocal analysis, once organized, could persuade people of the usefulness of hyperfunction theory.

Andronikof: *Microfunctions were devised to help hyperfunction theory—so to speak!*

Sato: Yes, so that hyperfunction theory could at least be perceived as a method in analysis. $\mathcal{D}$-modules and things like that were not accepted at the time. The only one who really appreciated $\mathcal{D}$-modules was Masaki Kashiwara, who became interested in developing a survey of the theory. It was in 1969 or in 1970 that he did it, to get his master's degree. In Japan, this degree is considered of primary importance in order to obtain a position. So he published a very nice paper on $\mathcal{D}$-modules.

Andronikof: *You mean Kashiwara's master's thesis, handwritten in pencil, that can be found in Tōdai library*[7].

Sato: Yes, precisely. That was completely his own work. He had a very good background in general nonsense. In the lectures before the colloquium, I already developed as much general nonsense as I thought was needed for hyperfunction and $\mathcal{D}$-module theory. But it was rather informal, because I didn't do it in a systematic way, whereas Kashiwara started his mathematical career in a clear way from the very beginning. To perceive some of the difficulties of his task, recall that it was a kind of hunting age for such general nonsense. He learned Bourbaki, Grothendieck, and these things when he was eighteen or nineteen years old. He studied it by himself, with no teacher, when he was only—what do you call it?—a senior mathematics student. Yes, he is very ingenious. The best young boy I ever met.

Andronikof: *But who put him onto these subjects?*

Sato: After the first two years at Tōdai, he moved to Hongo campus. There, he first learned of hyperfunction theory in 1968, at a lecture by Komatsu. I think this lecture is fundamental in the history of algebraic analysis. It was published in the Seminar Notes of the University of Tokyo after notes taken by student participants like Kawai, Uchiyama, . . .

Andronikof: *How did $\mathcal{D}$-modules come to Kashiwara?*

Sato: In 1968 Komatsu and I organized a weekly seminar on algebraic analysis at the Tōdai Mathematics Department, where I gave several talks—very disorganized, as my talks always are. It wasn't

[5] *The phase space of classical mechanics.*

[6] *Research Institute for the Mathematical Sciences, which is part of Kyoto University.*

[7] *Kashiwara's thesis has been translated into English by Andrea D'Agnolo and Jean-Pierre Schneiders: Masaki Kashiwara,* Algebraic study of systems of partial differential equations, *Mém. Soc. Math. France (N.S.) (1995), no. 63, xiv+72.*

an official seminar in Tōdai, but rather a kind of "Jacobin Club". Among the participants, there were many very eager young students, including Kawai and Kashiwara. I met them there for the first time, and the group of Kawai, Kashiwara, and myself was formed that year.

In spring 1969 some old friends of mine in Komaba, which is part of the Faculty of General Education of the University of Tokyo, arranged to have me go there as a professor. I stayed in Komaba for two years.

Andronikof: *And when did you come to RIMS?*

Sato and Emmanuel Andronikof, 1990.

Sato: It was in June 1970. Actually, in Tokyo I had a great understanding with Komatsu and other seniors, as well as with many young mathematicians who gathered at our seminar. Among the participants, besides Kashiwara and Kawai who were extremely active, there were Morimoto, Kaneko, Fujiwara, Shintani, Uchiyama, and some others. So, I could supervise a lot of people who were very eager to study mathematics with me, and I thought I should better stay at Tōdai than come to RIMS. Anyway, Professor Kôsaku Yosida, who was director of the Institute from 1969 to 1972, and of whom I was once an assistant, put great pressure on me to come to RIMS. He had already asked on the occasion of the seminar he had organized in 1969. But since I had a position at Komaba, that was delayed until 1970. I was unhappy when I had to move to Kyoto because it meant I would be separated from this group: I could bring Kawai and Kashiwara to Kyoto, but I had to leave others behind.

The Katata Conference and S-K-K

Andronikof: *As for the "milestones" in the birth of hyperfunction and microfunction theory, can you comment on the famous Katata conference in fall 1971?*

Sato: Actually, what I said at that conference was sort of completed quite early, just after 1969. I have already told you how microfunctions originated in preparing the talk I gave at the international symposium at RIMS in April 1969. I had planned to present some of the things I had in mind, like the cotangential decomposition of hyperfunctions, so I had to check whether my ideas were working or not. I started to check this in the three-hour *shinkansen*[8] trip, commuting from Tokyo to Kyoto (or vice versa, I don't remember) to attend a pre-symposium meeting at RIMS. You could say that the basic part of the theory was conceived during these three hours. But later I checked it in detail, and it was completed at the international symposium. The final touch was a proof of microlocal regularity for elliptic systems[9]. At the time, I employed Fritz John's method of plane wave decomposition. Of course, the idea went back to 1960, when I attended Professor Hitotumatu's talk on the edge-of-the-wedge theorem.

Andronikof: *When were the famous S-K-K*[10] *proceedings written?*

Sato: The basic structure of the paper hinges on my talk at the Katata conference, but the manuscript was completely prepared by Kawai and Kashiwara. Let us say I presented the whole story, but did not prove every detail. For example, concerning the notion of microdifferential operators, I worked out some cohomological constructions, but then Kawai and Kashiwara gave a better, more direct presentation, by which the proof of the invertibility for microelliptic operators, instead of using Fritz John's plane wave method, reduced to a kind of abstract nonsense. Kawai and Kashiwara must have taken a lot of effort to complete every detail.

The work was done between 1969 and 1971: surely the golden age of microfunctions. At the time, the three of us were working together, in the same places. In 1969 we were in Tokyo, then we moved to RIMS in 1970. Kawai came here as an assistant, while Kashiwara had only a kind of grant since he was very young at the time. He became assistant in 1971. I think the main part of the job was finished prior to the Katata conference, and was already presented in my talk at the Nice Congress [International Congress of Mathematicians] in 1970. To be precise, in the Nice talk the structure theorem for microdifferential systems was not yet finished. It was presented at the summer school on partial differential equations at Berkeley in 1971. I also prepared a kind of preprint, which did not appear in the proceedings of the Berkeley summer school, though it was distributed. There, I stated the structure theorem, asserting that all microdifferential systems are—at least generically—classified into three categories, the most important being what we called Lewy-Mizohata type system. The proof of this reduced to some simple nonlinear equations

[8] *A high-speed Japanese train.*

[9] *Now known as Sato's theorem.*

[10] *M. Sato, T. Kawai, and M. Kashiwara,* Microfunctions and pseudo-differential equations, *In Komatsu (ed.),* Hyperfunctions and pseudo-differential equations, *Proceedings Katata 1971, Lecture Notes in Mathematics, no. 287, Springer, 1973, pp. 265–529.*

describing some geometrical transformations[11]. I clearly remember discussing it with Kawai and Kashiwara. Actually, at Berkeley this was stated only for the simply characteristic case. The multiple characteristic case was done the following month, and all this was finished for the Katata conference. I must also say that at Katata I was given plenty of time, while at Berkeley my work was not taken into great consideration. I was sort of neglected there.

Andronikof: *So, when was the S-K-K manuscript ready?*

Sato: It was ready in 1971, but then Komatsu asked me to write a preface. I kept putting it off, so it was my fault that the publication was delayed by more than a year. I always behave in that silly way.

Andronikof: *Concerning another important result in S-K-K, could you comment on what is now called the* Fourier-Sato transformation*? When was it cooked up?*

Sato: As I told you, during my years as a schoolteacher I began to develop my own ideas in mathematics. Since the beginning, I had chosen the subject of generalized functions: both as one of the building blocks of my mathematical world and as a way to present some of my mathematical results, in order to get a job. It seemed to me that this subject could be quite easily appreciated. I estimated that the classical functional analytic approach, considering dual spaces to Banach spaces or to locally convex spaces, was not satisfactory at all. I was determined to throw away all these things, and to construct a theory which completely relied upon algebraic methods, like cohomology. What I wanted was to build something in the spirit of Cartan-Serre's presentation of Oka's theory of complex variables, or of Godement's work on sheaf theory. That was the idea of algebraic analysis, around 1960. You see, in this context the Fourier transform was not such a drastic idea to me, but rather a natural consequence of my kind of thinking. Of course, the actual theory of the Fourier transform was made later, in the course of systematizing microfunction theory, after 1969. At first, I constructed it using a quite straightforward method, considering the edge-of-the-wedge and the microlocal decomposition. But then I tried to describe it in a more algebraic way. That is when I arrived at the formulation of the Fourier transform as it appears in the S-K-K proceedings, with the three exact sequences: one on the base space, and two in the tangent and cotangent bundles.

[11]*What they called* quantized contact transformations.

The Theory of Prehomogeneous Vector Spaces

Andronikof: *Between 1960 and 1967, what happened to the broad program in mathematics that you devised? You said that you had stopped working in microfunction theory.*

Sato: Things are not so linear, of course. Let us say that I was working on microfunction theory in the background, while considering several other problems in parallel. In my twenties, my thoughts were scattered. While trying to organize analysis on an algebraic basis, I was also very much interested in subjects such as special functions via concrete examples, class field theory, automorphic forms, and of course quantum field theory. As I told you, after mathematics I had switched to the physics department for six years, from 1952 to 1958. Since I had to work as a high school teacher, I couldn't participate in research activities at the university, but nevertheless physics was one of my major subjects of interest.

Anyway, when I moved to Princeton in 1960, I wasn't acquainted with the mathematical society. I always pursued mathematics my own way, which was not the academic one. I was a kind of amateur in mathematics, certainly not a professional. As a consequence, whenever I tried to explain my ideas, I did not know which parts were well known or which were new. Most of the time people did not understand me. In Princeton there were a number of eminent and active mathematicians, but to them I just seemed a strange man with some very strange ideas. So I decided that in order to connect with some audience, I should devise some applications of my theory to differential equations. Of course, I then thought of developing the theory of differential equations from scratch. In my talk in Tokyo in 1960, I had already pointed out how holonomic systems are a powerful tool in understanding other differential equations, and how they give an algebraic way of defining special functions. But gradually, I understood that fundamental solutions may not satisfy holonomic systems in the usual sense: to understand the situation better you have to go to some wider class of equations: nonlinear, of infinite degree, or with an infinite number of independent variables.

To develop the theory of differential equations from the beginning, I thought I had to get acquainted with differential equations through concrete examples. As you know, the differential equations one usually encounters are mainly of second order, like the Laplace equation, the wave equation, the heat equation, or some modifications of these where the coefficients are variable. So, I tried to get a good example that was beyond the second order but that was still manageable through a special function. That is how, in spring

1961 I think, I devised the theory of prehomogeneous vector spaces (PHVS). Though the name is not a good one, I wanted it to mean a vector space that is not exactly homogeneous, but homogeneous except for a rather inessential subspace. PHVS gave a good generalization of the Laplacian, whose principal part is a quadratic form. A quadratic form is connected with orthogonal transformations, and PHVS generalizes this to the case of arbitrary groups. In this framework, instead of a quadratic form we can consider other interesting polynomials, associated to differential equations of higher order, whose principal part is very symmetrical, like a determinant or a discriminant. Moreover, I immediately realized that PHVS could also be useful in the theory of the Fourier transform, or to get generalized ζ-functions, and so on. Throughout this month of 1961, I was engaged in this theory, and in connection with this concept I introduced the notion of the a-function and b-function. The most important is the b-function[12], since it makes it possible to define a generalization of hypergeometric functions, and the a-function is nothing but the principal symbol of the b-function. The reason for "a" and "b" is that a is easier but b can also be developed from this concept. In late 1961, I explained these things at a seminar at Princeton conducted by Armand Borel. I gave two talks there, but again I think the subject was not fashionable at the time. It was something people were quite unfamiliar with, out of the focus of contemporary mathematicians, and so they just ignored or forgot it.

Because of this situation, the following year I tried to do something fashionable. So, I took up something else which I had in mind, about number theory. I was quite well acquainted with algebraic number theory and especially class-field theory and ζ-functions or automorphic forms. You know, Japan had a strong tradition in algebraic number theory, since Takagi and its school: Iwasawa, Shimura… It was one of my favorite subjects, and I started investigating it more seriously. I was interested in the Ramanujan conjecture, and I began working on it during the summer of 1962. Actually, Shimura was also working very actively in that field at that time. My term in Princeton expired that summer, and just before my departure I succeeded in establishing the relation between Ramanujan-type functions and the Dirichlet series. I won't go into detail about this, since I think we should stay within microlocal analysis. We cannot focus on too many subjects at a time.

In August 1962 I returned to Japan. At that time, mathematics departments were very small. Including all universities like Tokyo, Osaka, and Kyoto, there were only five positions for professors. A professor of analysis from Osaka University was retiring, and Matsushima invited me very strongly to go there. So I moved to Osaka University in spring 1963. There, I had a couple of good young students working with me. Once again, what I then did remained unpublished, with the possible exception of a talk that I gave at Tōdai, and of which Ihara wrote a report: some record of it exists in the form of handwritten notes in Japanese, in a journal for young mathematicians. Later, Ihara wrote a paper about this work of mine, but the story is rather complicated since there is a relation with later work by Deligne, and if I present it briefly I risk being imprecise. So just forget about it.

The following year, in 1964, I went to Columbia University, where I stayed as a visiting professor for two years. Serge Lang invited me there. You see, he came to Japan and was impressed by my work: that is, my work in number theory! Ha!, ha!

Andronikof: *Let me return to PHVS. You said the theory came about through your interest in high order PDEs.*

Sato: Yes, but at the same time it has a relation with algebra, geometry, number theory, θ-functions, generalized ζ-functions, and so on. It is a very interesting subject, related to later developments of microlocal calculus. But again, this is another subject, and so I won't go into detail. Let me just mention that a very good mathematician, Takuro Shintani, who unfortunately died some years ago, wrote a paper which is the basis for the theory of PHVS applied to ζ-functions. After a seminar he gave in Tokyo, a number of mathematicians from Japan, the USA, and Europe began to work on that subject. So, although I am not active in that field at the moment, the theory of PHVS is far from being buried, and instead is a rapidly developing subject.

This is more or less how my time was spent from 1960 to 1967. In 1967 I really started considering PDEs through algebraic analysis, i.e., in the category of hyperfunctions and microfunctions.

Andronikof: *What happened just after 1966, when you came back from Columbia?*

Sato: I resigned from Osaka in fall 1966 and was out of the university for a year and a half, when I became professor at Komaba. In fact, I had been living in Tokyo since early 1967. There, I met Komatsu, and we started the Tokyo seminar on algebraic analysis I told you about. It lasted until Kawai, Kashiwara, and I left for Kyoto in 1970.

Toward Mathematical Physics

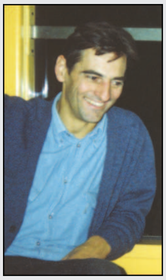
Andronikof: *And after the congress in Nice, you went to France.*

Sato: Yes, Kawai, Kashiwara, and I stayed in Nice from September 1972 until the next aca-

[12] *Now called* Bernstein-Sato polynomial.

Emmanuel Andronikof

Pierre Schapira



Emmanuel Andronikof passed away on September 15, 1995, from a brain tumor discovered two years earlier, just after he had been named professor at the University of Nantes. He faced his illness with exceptional courage, never complaining about his misfortune or about the unfairness of this world. Perhaps his character was a product of his strict orthodox and aristocratic Georgian education. An excellent sportsman, especially in boxing and swimming, he had a unique vision of life and was able to spend long days in a forest with nothing but a book of Russian poetry. Needless to say, he was not interested in the games of power, not even in his own career. While he was devoted to science, he looked at the scientific world as a theater with very few actors and many jokers, considering himself to be an extra.

Nevertheless, his contribution to mathematics is highly significant. In his thesis, he succeeded in making a synthesis of the microlocalization functor of M. Sato and the temperate cohomology of M. Kashiwara, defining the functor of temperate microlocalization. This opened the way to the linear analysts to study microlocally (i.e., in the cotangent bundle) distributions, or more generally temperate cohomology classes, with the tools of homological algebra, sheaf theory, and $\mathcal{D}$-module theory. In other words, this was the first step towards a "temperate microlocal algebraic analysis", and Emmanuel Andronikof began to apply his functor to handle various problems. He provided an illuminating proof of the Nilsson theorem on the integrals of holomorphic functions of "Nilsson class", he proved that the C^∞-wave front set of a distribution solution of a regular holonomic system coincides with its analytic wave front set, and he was the first to give a microlocal version of the Riemann-Hilbert correspondence, this last work being at the origin of further important developments (see [7] and [6]). He also had many projects that he was unfortunately unable to carry out to completion.

The departing of Emmanuel Andronikof leaves us with the memory of an excellent man, both rigorous and open, tough and gentle.

References

[1] E. Andronikof, Intégrales de Nilsson et faisceaux constructibles, *Bull. Soc. Math. France* **120** (1992), p. 51–85.

[2] ______, On the C^∞-singularities of regular holonomic distributions, *Ann. Inst. Fourier (Grenoble)* **42** (1992), p. 695–705.

[3] ______, The Kashiwara conjugation and wave front set of regular holonomic distributions on complex manifolds, *Invent. Math.* **111** (1993), p. 35–49.

[4] ______, Microlocalisation tempérée, *Mémoires Soc. Math. France* **57** (1994), 176 pp.

[5] ______, A microlocal version of the Riemann-Hilbert correspondence, *Topol. Methods Nonlinear Anal.* **4** (1994), p. 417–425.

[6] S. Gelfand, R. MacPherson, and K. Vilonen, Microlocal perverse sheaves, arXiv math.AG/0509440.

[7] I. Waschkies, Microlocal Riemann-Hilbert correspondence, *Publ. Res. Inst. Math. Sci.* **41** (2005), p. 37-72.

Pierre Schapira is professor of mathematics at the Université Pierre et Marie Curie, Institut de Mathématiques, Paris, France. His contact addresses are: schapira@math.jussieu.fr *and* http://www.math.jussieu.fr/~schapira/. *Extracted and adapted from* La Gazette des Mathématiciens **66** *(1995).*

demic year. There, I became acquainted with a number of mathematical physicists who Frédéric Pham had invited from different parts of Europe. Moreover, Pham stimulated my interest in studying a program in mathematical physics. He introduced me to the momentum space structure of an S-matrix and Green functions in quantum field theory. This was the first time I became interested in applying microlocal analysis to such subjects. Later, Pham worked in that field for quite a long time.

Ten Papers by Mikio Sato

Theory of hyperfunctions. I and II, *J. Fac. Sci. Univ. Tokyo. Sect. I*, **8**, 1959 and 1960, 139–193 and 387–437.

Hyperfunctions and partial differential equations, *Proc. Internat. Conf. on Functional Analysis and Related Topics, 1969*, Univ. Tokyo Press, Tokyo, 1970, 91–94.

Regularity of hyperfunctions solutions of partial differential equations, *Actes Congrès Internat. Math., 1970,* Gauthier-Villars, Paris, 1971, **2**, 785–794.

(with T. Kawai and M. Kashiwara), Microfunctions and pseudo-differential equations, *Hyperfunctions and pseudo-differential equations, Proc. Conf., Katata, 1971*, 265–529, Lecture Notes in Math., Vol. 287, Springer, 1973.

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

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

(with T. Miwa and M. Jimbo), *Holonomic quantum fields. I–V*, Publ. RIMS, Kyoto Univ., **14** (1978) 223–267 (I), **15** (1979) 201–278 (II), 577-629 (III), 871–972 (IV), **16** (1980) 531–584 (V), 137–151 (IV Suppl.), **17** (1981).

(with T. Miwa, M. Jimbo, and Y. Mōri), Density matrix of an impenetrable Bose gas and the fifth Painlevé transcendent, *Phys. D*, **1**, 1980, no. 1, 80–158.

(with M. Kashiwara, T. Kimura, and T. Oshima), Microlocal analysis of prehomogeneous vector spaces, *Invent. Math.*, **62**, 1980, no. 1, 117–179.

(with Yasuko Sato), Soliton equations as dynamical systems on infinite-dimensional Grassmann manifold, *Nonlinear partial differential equations in applied science (Tokyo, 1982),* North-Holland, Amsterdam, 1983, pp. 259–271.

Then I returned to Japan in the spring of 1973. In April of that year, Tetsuji Miwa came to Kyoto and obtained a position as an assistant at RIMS. He was a very good student of Komatsu at Tōdai and had studied hyperbolic equations with him. The following year, in 1974, Michio Jimbo had just finished his undergraduate courses and was preparing to enter the graduate school of Kyoto. He spent two years there and then got a position as an assistant at RIMS in 1976.

Miwa, Jimbo, and I worked on the subject in mathematical physics in which I'd become interested through Pham and his friends. We began studying the momentum space structure. In order to have a clearer idea than from the general quantum field theory, I intended to use some very concrete examples. Working in that direction, I just took up the Ising model. In fact, when I was a student in physics I was very much interested in quantum field theory, of course, but also in statistical physics and statistical mechanics. At that time, Dr. K. Ito, a theoretical physicist who was then a graduate student, mentioned a paper by a young Russian mathematician on correlation functions in the Ising model that we found very interesting. Later, Professor Masuo Suzuki from Tōdai mentioned the work of Wu-McCoy-Tracy-Barouch ([1]) on the two-point function of the Ising model. It was probably published in 1976, but I learned later from Barry McCoy that their work had been conceived much earlier, around 1973. The remarkable point is that their work contained a correct expression by means of Painlevé transcendents. It was a pleasant surprise to me that such special functions actually appeared in concrete problems of theoretical physics, especially in one of my favorite subjects, the Ising model. So, we developed this much further with Miwa and Jimbo. We started doing this around 1976, having spent the previous three years doing much more elementary things, in order to get acquainted with the subject. Everything worked very effectively, and very quickly, in 1977, we finished working in that direction. 1976–1978 was a very fruitful time. Since then, Jimbo and Miwa have done quite a lot of things.

Andronikof: *What about the KP hierarchy?*

Sato: We became acquainted with many people in soliton physics. Date was a student of Professor Tanaka, who was in Osaka at that period and then moved to Kyushu University. Tanaka and Date were pioneer mathematicians, who introduced soliton theory in Japan, including Krichever's work, Jacobian variety, θ-function, KdV, and KP equations. We learned about these things through them during that period. Of course, there were many people in physics departments or engineering departments who were interested in soliton theory, but in the world of pure mathematics in Japan, it was Tanaka who introduced such modern developments in connection with soliton theory. I also learned a lot from Hirota. He is a very important name in soliton theory, although he is not a mathematician in the strict sense. He is very unique, in that he has his own system of mathematics, so to speak, and has devised quite a unique method of analysis of soliton theory. And this inspired me. You see, most of the time, when I meet some new subject in mathematics, I don't find it very strange. I'm thinking about the traditional systems of mathematics, like those of Newton, Leibniz, or even Descartes, Gauss, Riemann, and so on. But Hirota's mathematics seemed to me quite strange, so I just wanted to understand it in the language of modern mathematics. In this way I finally arrived at the concept of infinite-dimensional Grassmannian manifold around Christmas 1980, in collaboration with my wife. We performed lots of computations, using BASIC programs. Looking at them day after day, I finally realized that in the background of the whole story stood a Grassmannian manifold of infinite dimension. Again, once I arrived at this concept of Grassmannian manifold and Plücker coordinates, things developed quite quickly, and in a very short period—less than a month—everything was almost finished.

Andronikof: *Who works on KP nowadays here?*

Sato: I don't think that anyone is working seriously on it now. But at the time Miwa and Jimbo and others developed the theory in connection with Kac-Moody algebras. I worked to generalize this to higher dimensions. You see, KP is a kind of one-dimensional theory, using microdifferential operators in one variable, so we tried to generalize it to higher dimensions and did it in part with my graduate students Ohyama and Nakayashiki. But I am not yet satisfied with this. I gave some talks on this subject at the AMS summer institute organized by Ehrenpreis in 1988. They have been written down by Takasaki([4]).

Andronikof: *It seems that many subjects have been lying dormant inside like still waters, like a volcano waiting to erupt. Where did you turn your attention to after KP theory?*

Sato: Life is not limited to mathematics, and one is forced to engage in other things. So, unwillingly of course, at some times I was prevented from focusing in mathematics. Starting from about 1980, I was not able to concentrate on mathematics for several years, and I was rather unhappy. As I told you, mathematics give me relief from everyday life, so to speak. Anyway, it has only been in the last two or three years that I have been able to work again. I'm now trying to reorganize my own mathematics these days.

Sato's School

Andronikof: *Which mathematicians played a role as supporters of your ideas, as propagators of the theory?*

Sato: At the early stage the most important names were Iyanaga, Komatsu, Martineau and, in a sense, André Weil. After 1970 I became acquainted with a lot of French people, like Schapira, who is a student of Martineau, or Pham, as I told you, and Jean Leray.

Andronikof: *Did you create a school?*

Sato: This may be partly true, but in fact many of the active people were students of Komatsu at Tōdai. Kawai graduated with him in 1968 and came to RIMS in 1970 as an assistant. In 1968 I also met Kashiwara, who was only a junior student at Tōdai. Since I was at Komaba at the time, he formally registered with Kodaira, but in practice he was with me. Miwa was also a student of Komatsu and came to RIMS in 1973 as an assistant. Jimbo was my only graduate student among these people. As concerns PHVS, Shintani was the most important person. He was not a permanent member of the Tokyo seminar, because Komatsu was not interested in PHVS. He was a very talented mathematician, and if he were alive he would now be a major figure in the world of mathematics, like Kashiwara. Kimura was in the same generation as Miwa. Though not in a formal sense, he was also my student and often came to Kyoto to study PHVS—in which he is an important figure.

Andronikof: *For nonlinear equations you had Takasaki, Ohyama, Nakayashiki?*

Sato: Yes. Actually, Takasaki was a student of Komatsu, and he joined me rather late. After finishing his course at Tōdai, he formally stayed in Tokyo for three more years, but actually came to Kyoto. Ohyama and Nakayashiki are from Kyoto University and came as graduate students to RIMS, so they are really my students.

Andronikof: *Do you have any students working on your ideas on number theory?*

Sato: Mmmhh… Not many.

Andronikof: *Tōdai has been a good source of students for you.*

Five Papers About Mikio Sato

S. Iyanaga, Three personal reminiscences, *Algebraic analysis*, Vol. I, Academic Press, Boston, MA, 1988, 9–11.

Masaki Kashiwara and Takahiro Kawai, The award to Mikio Sato of a medal for distinguished services to culture, *Sūgaku* **37**, 1985, no. 2, 161–163.

______, Introduction: Professor Mikio Sato and his work, *Algebraic analysis*, Vol. I, Academic Press, Boston, MA, 1988, 1–7.

Gian-Carlo Rota, Mikio Sato: impressions, *Algebraic analysis*, Vol. I, Academic Press, Boston, MA, 1988, 13–15.

Kōsaku Yosida, Sato, a perfectionist, *Algebraic analysis*, Vol. I, Academic Press, Boston, MA, 1988, 17–18.

Sato: True. Remember that when I came here twenty years ago I didn't want to leave Tokyo because I didn't want to sever ties with the young mathematicians who were eager to work with me, and it was with some regret that I came to Kyoto. In spring 1992 I'll be retiring from the University of Kyoto, and I will have no other choice but to work here, ha! ha!

Andronikof: *Is there such a thing as retirement in mathematics?*

Sato: Well, I'm not retiring from mathematics at all, but I have less time to work now. I am not young anymore, and my brain is not as fast...

Research Now and Future[13]

Andronikof: *What is left of your colonies of ideas? What has not been exploited yet of the program you exposed at the colloquium in 1960?*

Sato: In that sense: nonlinear equations. I have been keeping it in mind since that time, and it's still not worked out very well. I now want to include singular perturbation in my framework, but all this is not enough.

Andronikof: *So you are not yet satisfied with the form you gave to nonlinear equations?*

Sato: It is just a starting point, as you see... In 1960 I proposed the $\mathcal{D}$-module concept as the most natural way to deal with linear equations, so everything can be worked out by means of homological algebra, and I also introduced what are now called holonomic systems. But in the case of nonlinear equations, the only application of differential algebra was to a kind of Galois theory for equations of the Picard-Vessiot type. It is a rather minor branch in analysis, the major stream in analysis being, say, Hadamard's theory of hyperbolic equations.

At the time, the major battlefield in nonlinear PDEs was mathematical physics, as described for example in Courant-Hilbert's famous book, and differential geometry, with the Monge-Ampère, Hamilton-Jacobi, and Einstein equations, and contact transformation theories. These concepts naturally fitted in Élie Cartan's scheme, and I was not sure that my approach by means of the nonlinear filtered differential algebra $\mathcal{D}$ could work as effectively... But later I became more and more confident that this algebraic method was the only natural way to develop a general theory, because it represents the structure of differential equations without depending on how you write down the equations. Anyway, in my 1960 talk I just touched upon two main methods to develop the general theory of nonlinear equations, without sorting out either of them.

Andronikof: *In the one-dimensional case what do you think of Jean Ecalle's approach?*

Sato: My way of understanding it is that this kind of problem is close to what Pham noticed at a meeting in Greece three years ago and is related to my oldest problem in connection with PDEs. In my talk of 1960 I explained that linear nonholonomic systems can be related to holonomic ones by considering elementary solutions: what Riemann called singular functions. Suppose you have a PDE on a manifold X. Then its fundamental solution can be expected to satisfy some holonomic system, not on X but on $X \times X$. In that way, one could hope to control the whole theory of PDEs through holonomic systems.

However, it soon appeared that this program could not be carried out, because in certain cases the fundamental solution may not be holonomic in a strict sense. For example, some particular fundamental solutions may contain a factor of the form x^y, with both x and y being variables (of course, x^α, α constant, is a solution to a holonomic system). This happens quite often, and this means that you have to further extend the concept of holonomic system by considering equations of infinite order, or by involving an infinite number of independent variables. This is now a quite natural thing in modern mathematical physics. The work of Pham, Ecalle, André Voros, as well as some earlier works by mathematical physicists, seem to have close connections with such difficulties. This is also related to singular perturbation theory. There, an equation involves a parameter and at some value—which could be a coupling constant, or Planck's constant—the system becomes singular, so convergence is lost, although it is summable by some kind of Borel resummation. This kind of resummation is not unique—contrary to the convergent case—but depends on the chosen direction. If you move the direction it may suddenly change, yielding a kind of Stokes phenomenon. And this again is related to concepts like instantons or tunneling, which are also quite common in mathematical physics and are related to monodromy groups and so on. It is clear that such quite general concepts should be handled systematically in analysis.

In my talk at Okayama[14], I explained that singular perturbation can be dealt with by considering solutions of nonlinear equations which are neither holomorphic nor hyperfunctions but rather a kind of modified microfunctions, living in a filtered formal function space. For example, microfunctions supported at the origin are generated by the Dirac δ function together with its derivatives. If you apply microdifferential operators of order zero you get something like a Heaviside function times analytic functions, and this is the kind of special microfunction whose

[13] *That is, in August 1990.*

[14] *On the occasion of ICM-90 Satellite Conference on Special Functions.*

support is the origin. In several variables solutions have to be holomorphic in some variables but in the remaining ones it can be like a δ function or Heaviside function. Such mixed solutions can give a singular perturbation situation. This is what I tried to explain in Okayama—though I did not develop my talk in a very organized way. Anyway, this is how I see the work of Ecalle, Voros, or others. I think these things are very important and very natural in many problems of mathematical physics.

Andronikof: *Like shock waves?*

Sato: Yes, shock waves or diffraction, which are described by subdominant terms. The magnitude of the backward wave is exponentially small, so that the C^∞-theory cannot take it into account. But the analytic theory can single it out. This subdominant effect is related to Ecalle's notion of resurgency. In quantum physics, tunneling is also related to subdominant or Stokes phenomena. This aspect of analysis should be exploited to reach a consistent understanding, a correct general theory of such phenomena. I think this should be done in coming years.

During the congress [15] it should have clearly appeared that mathematical physics is very important, not only in applied, but also pure mathematics, including analysis, geometry, algebraic geometry, and number theory. But there are actually deeper connections with mathematical physics. This is not fully exploited yet. Ecalle's, Voros's, or earlier works, like Balian-Bloch ([2]) and Bender-Wu ([3]), are instances of applications of analysis to mathematical physics. But, in my opinion, this is still very primitive. There is work for the next decade in order to settle things so that analysis can really be applied to mathematical physics. While methods of mathematical physics in quantum field theory have profited various branches of mathematics (topology, braid theory, number theory, geometry), the converse is not necessarily true. Today, mathematical physicists mostly use number theory or algebraic geometry. Mathematical physics is receptive only to higher developed areas of mathematics, some of which are exploited in superstring theory, though not to its full extent. Mathematics has not succeeded in providing a more effective way of computation than perturbation expansions. Of course, there are some primitive methods of computation, like the Monte-Carlo method. All these are kind of brute force computations, not refined mathematics, surely not refined enough for the problems physics is now confronted with, like determining the mass of particles or quarks. All these things are discussed on a very abstract level, not on a quantitative level. So I think that mathematical analysis should be developed much further to match the reality of physics.

Andronikof: *What could remain of these problems in the twenty-first century?*

Sato: I cannot say: a good mathematician can appear in each branch. If one of these branches attracts a good young mathematician, this branch might develop quite rapidly. Look for instance at the past: if there were no Grothendieck or Deligne, things in algebraic geometry would have developed in a different way. But you can imagine how it is going to evolve in the next few years, because a number of very active mathematicians at this congress gave a number of very interesting talks and would give you an idea of what mathematicians in the 1990s will look like.

Andronikof: *My last question is about Japanese mathematics—the fact that in modern mathematics Japan has risen from nowhere to its current outstanding place.*

Sato: I'm not a specialist in Japanese mathematical history, but as I have explained it, Japan has a good background for modern mathematics. About two or three hundred years ago, at the time of Leibniz and Newton, we had Seki, one of the founders of Japanese mathematics. He and some of his great followers developed a kind of algebra. Some elementary mathematics, related to counting, was introduced from China. But Seki developed a major system of algebra and even infinitesimal calculus. He developed a theory for algebraic equations, with Chinese characters instead of x and y. He dealt with linear equations in several variables, with higher-order algebraic equations, though he did nothing like Galois theory or Cardano formulas for cubic equations: instead he developed approximate solutions akin to Newton-Horner methods. Also, he knew how to solve linear equations using the Cramer method, since he had the notion of determinant, earlier than in Europe. He used derivatives to determine maxima and minima of a polynomial in several variables. But the biggest defect of his system was that they did not see at the time that integration is the converse of differentiation. So they computed integrals the way Archimedes did, obtaining volumes and areas using a kind of slicing procedure. One of the typical integrals appearing in connection with computing area or volume for which they had such a formula, is $\int_0^1 (\sqrt{1-x^2})^n dx$, for n odd. This is just one type of accomplishment of Japanese mathematics in the seventeenth and eighteenth century.

Andronikof: *Did it go on?*

Sato: Although we had this tradition, mathematics declined to a kind of hobby. The government in the Meiji era decided to adopt Western mathematics, because Japanese mathematics is just a kind of empirical thing. They did not pay

[15] *Kyoto, ICM 1990.*

much attention to logic. So even in geometry, they did not develop a demonstration. Just a discussion is given, but not a completely logical inference.

Andronikof: *What about RIMS*[16]*?*

Sato: When the Russians sent the Sputnik into orbit in 1957, it was a big shock in the USA. This certainly motivated them to develop their science. At that time, the number of professors in scientific branches, including mathematics, simply doubled. The same thing took place in Japan, a little later. Scientists had a good excuse to get more money. A number of institutes of technology and physics, but also of pure sciences, were built at that time. Many senior mathematicians, like Yosida and Iyanaga, persuaded our government to found a new institute for mathematics. The Institute was created in 1959, when I had just started my career.

Tōdai had already too many research institutes independent of the departments and was reluctant to have a new one. So it was finally decided that the new mathematical institute should be opened within Kyoto University, which supported the idea.

Andronikof: *And from the start it was devoted to pure mathematics?*

Sato: It was devoted to mathematical sciences. The fact is that its emphasis is on pure mathematics, but it also includes a few mathematical physicists and a few computer scientists. We are even trying to put emphasis on applied mathematics. This balance has now changed. We have to recover this. The importance of computer science in mathematics is recognized, as we can see by the Nevanlinna Prize, dedicated to that discipline.

References

[1] T. T. Wu, B. M. McCoy, C. A. Tracy, and E. Barouch, Spin-spin correlation functions for the two dimensional Ising model: Exact results in the scaling region, *Phys. Rev. B* **13** (1976), 316–374.

[2] R. Balian and C. Bloch, Solutions of the Schrödinger equation in terms of classical paths, *Ann. Phys.* **85** (1974), 514-545.

[3] C. M. Bender and T. T. Wu, Anharmonic oscillator, *Phys. Rev.* **184** (1969), 1231–1260.

[4] M. Sato, The KP hierarchy and infinite-dimensional Grassmann manifolds, *Theta Functions, Bowdoin 1987, Proceedings of Symposia in Pure Mathematics*, vol. 49, Part 1, Amer. Math. Soc., Providence, RI, 1989, pp. 51–66,

[16]*Sato was the director of RIMS at the time of this interview.*