Ideas and Methods Forged in Kyoto, Paris, and Harvard Lead to Solution of a Longstanding Conundrum in Algebraic Geometry (interviewed in 2009)

Interviewee

Heisuke Hironaka

Mathematician; President, Japan Association for Mathematical Sciences; Professor Emeritus, Kyoto University; Member of the Japan Academy

Born in Yamaguchi Prefecture in 1931. After graduating from the Faculty of Science, Kyoto University, he worked as assistant professor at Brandeis University, as professor at Columbia and Harvard Universities, as director of the Research Institute for Mathematical Sciences, Kyoto University, and as president of Yamaguchi University. He was awarded the Fields Medal in 1970.

Interviewers: S. Mori, m.j.a., M. Kashiwara, m.j.a.

An algebraic variety is, essentially, a geometric figure defined by a finite number of algebraic equations. Algebraic geometry developed with the aim of investigating the structure of these algebraic varieties. As a key pillar of modern mathematics, it is closely related to other branches of mathematics. In the early 1960s, the resolution of singularities in algebraic varieties, i.e., the proof that any given algebraic variety can be birationally transformed into an algebraic variety without singularities, was a fundamental challenge in algebraic geometry. In 1964, Dr. Heisuke Hironaka finally solved this thorny problem. (The problem had previously been solved for varieties of dimension up to 3, but not for 4 or higher.)

Interaction with Grothendieck

Mori: What year did you go to the US?

Hironaka: In 1957, after I completed my master's studies at Kyoto University, I went to Harvard University. Later, until half a year before writing my doctoral thesis in 1960, I spent six months at the Institut des Hautes Études Scientifiques (I.H.E.S.) in Paris, where Alexander Grothendieck was working.

Mori: You seemed to be involved in a lot of things around 1960.

Hironaka: Grothendieck was at Harvard for about a year, from 1958 to 1959. His theories were still not well known at the time, but there was no doubt about his excellence [as a mathematician], so Harvard invited him to give lectures. He had abstracted algebraic geometry and extensively rewritten it, but his work was not yet accepted. Half-jokingly, Oscar Zariski commented, "If he could generalize it that much, I'd be able to do some concrete calculations." When Grothendieck was leaving Harvard, he asked me to go back with him, so, a while later, at the end of 1959, I went to join him in Paris.

All the Parisian algebraic geometry crowd were at the Paris seminars, including Jean-Pierre Serre, Pierre Cartier, and Henri Cartan. It was around this time that Grothendieck said he was going to write "Éléments de Géométrie Algébrique [Elements of Algebraic Geometry]" in 13 volumes and began writing them while giving lectures. Within a short time, he defined Chern classes and generalized the Riemann-Roch theorem. This led to K-theory. He announced with deep pleasure, "It's done!" This was

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the time that Grothendieck began attracting serious attention. After this, he went on to develop étale cohomology, but I was back at Harvard by then.

There was a debate at that time about why étale cohomology was possible? In simple terms, the Zariski topology is very coarse. How was it possible to have a proper cohomology theory despite this fact? That was the problem. Then Serre proposed that when you take a point on an algebraic variety and take a suitable Zariski neighborhood, you need to prove that it is K(π,1), meaning that its universal cover is contractible. This was proved by Michael Artin. Later, Grothendieck called Artin and together they proceeded to develop étale cohomology.

I went to Paris again in 1965. Pierre Deligne was there that time. Deligne was a very good-natured character and Grothendieck liked him. Grothendieck's cohomology eventually led to Deligne's proof of the Weil conjectures.

Akizuki's Kyoto School of Algebraic Geometry

Mori: Did your direct experience and absorption of all this activity lead you to your resolution of singularities in some sense?

Hironaka: No, my work on the resolution of singularities started somehow when I was at Kyoto University. At that time, there was an enthusiastic and confident mathematics professor named Yasuo Akizuki at the university. He contacted Shokichi Iyanaga at The University of Tokyo to bring Jun-ichi Igusa to Kyoto to work under him as an assistant professor, even though his favorite student was Teruhisa Matsusaka. He also brought in Masayoshi Nagata, an outstanding student of Tadashi Nakayama at Nagoya University, as a lecturer. Some of the other people on the scene were Mieo Nishi, Hideyuki Matsumura, Shigeo Nakano, Yoshikazu Nakai, Kotaro Okugawa, Satoshi Suzuki, and Kayo Otsuka, as well as Nobuo Yoneda, who was a local.

I was invited to join this unique group when I was in my third year at Kyoto University. As Igusa-san was impressed, whenever anyone in the group submitted a paper on algebraic geometry, Prof. Akizuki would read it all carefully and comment on it. I especially admired him for inviting in people from outside. At that time, Kyoto University was still

Heisuke Hironaka (left), Masaki Kashiwara (center), and Shigefumi Mori (right).

a rather insular institution, but Prof. Akizuki was determined to bring people together to form a movement. He was also quick to provide us with and get us to read manuscripts on topics such as Zariski geometry and Abelian varieties. I was the youngest, so I wasn't assigned any work, but Shigeo Nakano, Masayoshi Nagata, and Yoshikazu Nakai had a tough time. When Serre published a new paper, for example, they were everyone. If they said something hard to understand, Prof. Aki zuki would yell at them. Mieo Nishi did his best to explain Zariski's resolution of singularities. That was the first asked to explain the content of his preprint to time I heard about it, in my third year of university.

Kashiwara: Are you referring to the case of dimension 3?

Hironaka: I think that's what it was. Later, Prof. Akizuki invited Zariski to Kyoto University for three months in 1956. When Zariski was leaving, he asked me, "Why don't you take the Fulbright examination and come to study at Harvard?" That's one of the reasons that I got interested in Zariski's work on resolution of singularities.

Earning a Ph.D.

Mori: You earned your Ph.D. in 1960 for your research on cones.

Hironaka: At Harvard, I got interested in singularities and studied the subject quite intensively. The problem of resolution of singularities is interesting because it is higher dimensional. That is, it needs to be a general theory to some degree. Zariski was busy, so David Mumford, Artin, and I ran our own seminars. I was trying to develop a general theory

Michita Sakata, Minister of Education; executives of the Japan Academy; and the 1970 Japan Academy Prize winners (at the then Japan Academy Hall). Dr. Hironaka is second from the right in the back row. His award title was "Study of Algebraic Varieties."

of rational transformations (operations that change the shape of a figure only slightly) by creating an abstract definition of the technique of blowup (a mathematical operation in which a part of a figure, e.g., a point, is removed and a larger figure, e.g., a curve, is inserted in its place). Nagata-san, who was also in the US at that time, was trying to create a foundation for abstract algebraic geometry. He really helped me in various ways.

So, after I created my generalization, I was speaking about it in one of our little seminars when Zariski suddenly arrived. After listening, he scolded me, "What is that kind of generalization good for?" Aside from that, when a blowup is performed with a rational transformation, all kinds of exceptional sets emerge. But they also have rigidity. This means that if you make something like a cone out of these rigid objects, the result will be even more general. In other words, I thought it would be possible to properly express all the regular blowups. That was the focus of my doctoral thesis.

Resolving a Difficulty for Kunihiko Kodaira

Hironaka: This became useful after I received my Ph.D. At that time, Kunihiko Kodaira was at Princeton, and he often came to Harvard. He was struggling with a problem. It had been proven that the Kähler property of Kodaira-Spencer deformations is preserved for small deformations. However, one scholar then wrote a paper saying that Kählerness is preserved even for long deformations, as long as they are non-singular.
Mori: That's quite strange.
Hironaka: The scholar would write a rough draft and send it to Kodaira-san and his team. It was a long paper. Kodaira-san would point out problems and report them back to the author. This went on and on. Since he couldn't say for sure that the claim was wrong, he felt very frustrated. He then asked me, "Hironaka-san, you know blowups, so can't you use them to make a counterexample? If you change the order, maybe the Kählerness won't be preserved, so perhaps you can try that?" But it couldn't be done like that, I concluded. I needed a slightly different form, so I spent a night thinking it over. Soon, I struck on a counterexample. I was able to do this because I had partially classified the blowups for conical forms. Kodaira-san was very pleased, saying, "That's a weight off my shoulders."

Kodaira-san's own interest in the topic made him engage in the mistake-filled paper. This interest of his led me to have many opportunities to talk with him.

Locked in Battle on the Resolution of Singularities

Mori: You received your Ph.D. in 1960 and by 1962 you submitted your paper on the resolution of singularities, which was published in 1964. How were you able to get so much done in such a short time?
Hironaka: One reason that I managed to complete my work in such a short time was that, thanks to Masayoshi Nagata, my grasp of commutative ring theory was probably stronger than that of anyone else at Harvard at that time. Even Mumford, who was a real genius, approached me to ask about commutative rings, which did my confidence and spirits a lot of good.
Mori: I think I understand.
Hironaka: Another factor behind my progress was the influence of Grothendieck. For example, I thought at first that it was unacceptable to use the Weierstrass preparation theorem, because it was too much of a departure from algebra. But for Grothendieck, there was no problem at all. He had no qualms about mixing up analytic geometry and

Fields Medal award ceremony (September 1, 1965, Nice, France). From left, A. Baker, Dr. Hironaka, and J. G. Thompson.

algebraic geometry. In Paris, it occurred to me that even if you limit discussion to the vicinity of points and perform your operations to completion, you can still arrive at global definitions if you incorporate inductive reasoning in a proper manner. By the time I was writing my doctoral thesis, I already felt that I would be able to somehow achieve the resolution of singularities.

Mori: So, you felt the time ripening even before you received your Ph.D.

Hironaka: However, you can never know how it will turn out until you write everything down formally. In about my second year at Brandeis University, I felt that I had nearly all the tools I needed to achieve a generalized solution. A key point was that, thanks to Grothendieck, I could introduce and use a notion of half-complete schemes.

We made quite substantial use of commutative ring theory. That's because we didn't really know what a singularity in the general dimension looks like geometrically. But when you apply commutative algebra, you can come to conclusions even if you don't know what you are doing at each step along the way. This demonstrates algebra's power of abstraction.

Translate a problem into an abstract form and solve it. Then, after solving it, you can understand the geometric meaning. It was all thanks to Nagata-san, who taught me commutative rings.

Mori: So, you were able to submit your paper within two years of getting your Ph.D.

Hironaka: In my second year at Brandeis, I became fully convinced that the solution was within reach, even though it was only in my head. I called Zariski to tell him. He responded kindly, advising me to proceed with due caution and suggesting that we hold a seminar.

After he approached the relevant people at Harvard and MIT, the seminar began. I started preparing my induction and went as far as formularization. I made some quite ridiculous stumbles, though. For example, when defining a normal crossing (intuitively, a right-angle intersection) in a set of hypersurfaces, two elements may globally become one, so at the stage of definition, I had to assume that the two may be identical. Otherwise, there would be problems of inconsistency later. In generalizing a theory, if you don't think carefully enough when creating your definitions, you will run into problems with your own definitions down the track.

For example, if you only assume that the center of the blowup is a variety with no singularities, you will run into very serious problems later. This is, in essence, the problem of determining which subvariety to be allowed as a center of blowup. In the end, what I initially defined didn't quite work. So, I asked Zariski to suspend the seminar and then I reconstructed the theory, which took me about a month.

Mori: Your research was completely different from previous approaches to the problem. You created your entire theory from scratch. Your story is an episode during the process, isn't it? And the theory that you created then is still going strong 50 years later. A wide range of mathematical work has been built on it.

Hironaka: Simply speaking, Zariski proposed a method of resolving the singularities of a figure for each direction at each point. I avoided that impasse, however. Even reading it now, I think that Zariski did an amazing job with his proposal. The best techniques for each direction are written neatly on the pages. However, since the necessary techniques differ from one direction to another, the solution is extremely microlocal. There was no way to globalize it later. My approach, utilizing the Weierstrass preparation theorem and Tschirnhausen transformation, was very natural. The framework is still fundamentally the same as when I formulated it.

Sprinter or Marathon Runner?

Hironaka: I am now 78 years old, and although I cannot do it as well as I did when I was younger, I am very glad that I did mathematics. I genuinely enjoy it. I remember a Fields Medal winner commenting that some people are good at running 100 meters, while others are good at marathons. When I worked alongside Mumford and Artin, I realized that I would surely lose the race if I tried sprinting.

Kashiwara: Yet, you were very quick to solve the resolution of singularities problem.

Hironaka: In a sense, the singularities problem was rather easy, although it required great perseverance. It would have been impossible without patience and tenacity. But the solution did not require any great iconoclastic idea. In contrast, Dr. Mori's breakthrough is truly prodigious. You noticed things that no one else had seen.

The work of resolving singularities is not like that. It is a task that necessitates the accumulation of many small minute items of work, little by little. Then, by carefully surveying the whole picture, you build some skillful theory. That's the kind of challenge it was. It's not something that you can solve with an explosive burst of inspired work. In any case, the mathematicians who are "sprinters" typically avoid this kind of problem, because it is too tedious.

Mori: Your foresight was good.

Hironaka: I was lucky too. If I had not learned how to use commutative ring theory from Masayoshi Nagata, I would not have been able to manipulate local rings so freely. I was also fortunate to associate with Zariski. If I had only been assisted by Nagata-san, I might only have become an expert in commutative rings. With Zariski, I started to think that the resolution of singularities might be possible. And then there was Grothendieck.

Mori: Do you have any memories of the Japan Academy Prize?

Hironaka: My father, who was born in the Meiji period and suffered a lot after the war, was very pleased that I had won the prize. After his two eldest sons were killed in the war, he was still left with 13 children, including me. He supported us by selling kimono fabrics itinerantly. He was particularly thrilled about the opportunity to see the Emperor Showa at the award ceremony. He even made his own business card, declaring, "Father of Japan Academy Prize winner Heisuke Hironaka." I asked him not to do such things. In the summer of the same year, 1970, I was awarded the Fields Medal, but he died in an accident before learning the news. I had already received notification of the award, but it was confidential, so I hadn't told my father. But since he was so happy about the Japan Academy Prize, I felt satisfied knowing that I had made him proud.

Commemorative photo at the Japan Academy Assembly Hall, September 13, 2009; Masaki Kashiwara (left), Heisuke Hironaka (center), and Shigefumi Mori (right).

Interviewer:
Shigefumi MORI (Mathematician)

Born in Aichi Prefecture in 1951. Graduated from the Faculty of Science, Kyoto University. After serving as assistant at the same university and as professor at the School of Science, Nagoya University, he worked as professor at the Research Institute for Mathematical Sciences, Kyoto University. He also became a member of the Japan Academy. In 1990 he was awarded both the Japan Academy Prize and the Fields Medal.

Interviewer:
Masaki KASHIWARA (Mathematician)

Born in Ibaraki Prefecture in 1947. Graduated from the Faculty of Science, University of Tokyo. After working as assistant professor at the School of Science, Nagoya University, he became professor at the Research Institute for Mathematical Sciences, Kyoto University, and a member of the Japan Academy. He was awarded the Japan Academy Prize in 1988.

(The original interview was conducted on Sept.13, 2009, and the titles at the time are used.)