

Abel interview 2025: Masaki Kashiwara

Bjørn Ian Dundas and Christian F. Skau

Bjørn Ian Dundas / Christian F. Skau: *Professor Kashiwara, we want to congratulate you for being awarded the Abel Prize for 2025.*

Masaki Kashiwara: Thank you very much.

[BID/CFS]: *The citation from the Abel committee reads as follows:*

“...for his fundamental contributions to algebraic analysis and representation theory; in particular the development of the theory of $\mathcal{D}$ -modules and the discovery of crystal graphs.”

You will receive the prize tomorrow from His Majesty, the King of Norway.

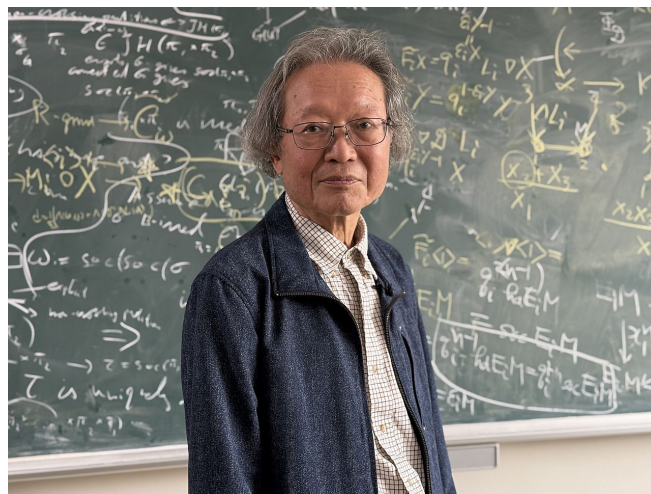
Early life

You are the first Japanese national – and the first person based outside North America, Europe or Israel – to win the Abel Prize. Our first question to you is: what kindled or sparked your interest in mathematics in the first place, and when did you discover that you had a special talent for mathematics?

MK: That is not so easy to answer. I always liked mathematics, also when I was a kid, and perhaps I was much better at it than other kids. That's true, but I was not a child prodigy, as many talented mathematicians are. I'm not like that. I started with mathematics because I think that mathematics is somewhat very logical, but also somewhat very appealing to human beings. So that's how I started to like mathematics.

[BID/CFS]: *We talked earlier about the “tsurukamezan,” or “cranes and turtles problem.” Describe the problem, and tell us when and how that problem caught your interest.*

MK: As you know, the problem is about turtles and cranes. Cranes have two legs, and turtles have four legs, and we know the total number of legs and the total number of cranes and turtles. What is



Masaki Kashiwara, Abel Prize laureate 2025.
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the number of cranes, and hence the number of turtles? I learned how to solve it, but it's very tricky, in fact. And when I learned about it first time I didn't like it at all, because it's too tricky.

Later I learned that you can solve it very easily using x and y , and then you don't need a trick. The solution comes about in a very natural way. I think that is in a way the aim of mathematics.

[BID/CFS]: *And you were fascinated by this?*

MK: Yes.

[BID/CFS]: *Were there persons that influenced you in your formative years, before you entered the university?*

MK: Not really. But some teacher made me aware of a book on projective geometry, which I could borrow from the library. I studied it and learned that in dimensions larger than three there is only one solution, while in dimension two there are several solutions to a certain problem. I remember this made a deep impression on me.



The Abel Prize Award Ceremony 2025.
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Early cooperation and the 1970 master's thesis

[BID/CFS]: *Your prospective advisor at the university, professor Mikio Sato (1928–2023), said of you that you learned Bourbaki, Grothendieck and these things when you were 18 or 19 years old. He said that you studied it by yourself, with no teachers. Why were you so fascinated early on by this type of abstract mathematics?*

MK: So what happened is that I was taking a course in mathematics. I think it was an algebra course. And the professor told me of the existence of EGA, *Éléments de géométrie algébrique*, and of Bourbaki. I started to read the EGA and found it very easy to read, in fact. I think that is a very well written book. And I was very happy to have read that.

[BID/CFS]: *And you didn't have any problem with the text being in French?*

MK: I had a problem, yes. I learned some French when I entered the university. Besides English, you have to learn another foreign language, and I chose French. But I'm not very good at French.

[BID/CFS]: *Professor Mikio Sato has been described as a visionary mathematician. Your master's thesis from 1970 – you were 23 years of age at the time – where you developed Sato's idea of $\mathcal{D}$ -modules was described as epoch-making. Please comment on this and also on how and why you got Sato as your advisor.*

MK: Professor Sato was in those days well known among mathematicians, and he was also a very charismatic mathematician. Hikosaburo Komatsu, a younger professor, came back from the United States, and I attended the seminar he and Sato organized. And Sato started to create so-called microlocal analysis.

Microlocal analysis is to study the functions not only on the space, but on the space with codirection; that is, on the cotangent bundle. So that was his idea. It was a very new idea. Sato thought that is possible to study functions microlocally. He started to create the theory, and, fortunately, I was there. I was participating in his seminar and started to collaborate with him. It was very lucky for me.

Together with Takahiro Kawai, who was also a student, we started to collaborate and to construct microlocal analysis. It was a very happy and propitious moment. Because of Sato I learned many valuable things, which in a way contributed to me being awarded the Abel Prize.

[BID/CFS]: *You once said that "Sato wanted to bring the equality world into analysis." This is the marriage between algebra and analysis?*

MK: Yes, you can say so. And also geometry. As you know, mathematics is often divided into geometry, algebra and analysis. Those are the three main divisions, and I think somehow the $\mathcal{D}$ -modules combine all these. Sato himself started to study $\mathcal{D}$ -modules because of that. So I was lucky to start my study with the theory of $\mathcal{D}$ -modules.

[BID/CFS]: *They were absolutely happy circumstances! You told us that it was a lecturer at the university who suggested to you that you should read EGA. Was that in order to prepare you for studying with Sato?*

MK: No, not at all. I didn't think about that at all. Sato developed homological algebra in his own particular way. Of course, homological algebra existed already, and was developed by Grothendieck and others. But I think that Sato didn't know about that, but that he independently discovered things.

[BID/CFS]: *Then you write your master's thesis in 1970, which contains so much fundamental stuff. For instance, you prove the Cauchy–Kovalevskaya theorem, which has to do with existence of local solutions. But you had your own version, which, of course, fits in the framework we're talking about now. Could you describe that to us?*

MK: Some of that is already covered by the idea of Sato. Sato initiated the theory of $\mathcal{D}$ -modules, but he didn't develop it, unfortunately. I think he was good to initiate a theory. But I think he never completed it himself. So I started from his idea, that's true, but once you know the idea, it's very easy to proceed. It's like climbing a mountain. You know the mountain, and you have to look for some route, and you can find it. I think that's what happened. Sato showed that there is a mountain over there. I think that up to that point, the influence of Sato is very big.

[BID/CFS]: *The statement, as you phrase it, is that “if the function is non-characteristic, then the associated inverse image functor commutes with the solution functor.”*

MK: Yes, that’s right.

[BID/CFS]: *We’ll come back to $\mathcal{D}$ -modules in more detail, but can you in more elementary terms connect your result back to the original Cauchy–Kovalevskaya theorem in any way, so that we see that there is a connection. Where are the differential equations and where are their solutions?*

MK: I think it’s difficult to say. We have a real world, and so there’s a real manifold. One of the basic ideas of Sato is that this is not a good point of view. A good point of view is that you have a real world, but that is surrounded by a complex world. And the real world is determined by this surrounding complex world. I think that is Sato’s main idea. And the complex world is, in fact, more important than the real world. If you know the complex world, then you know the real world. That was the starting point of the hyperfunction theory of Sato. And as for the Cauchy–Kovalevskaya theorem, I think the main part is concerned with the complex world.

And the next idea is that you can connect it to the real world. Sato, Kawai and I succeeded to construct microlocal analysis, and that was vital to how we understand the real world through the complex world.

Early career; SKK-paper and the index theorem

[BID/CFS]: *Which culminated in the groundbreaking “SKK” paper of 1973, co-authored with Sato and Kawai. The cotangent bundle – or, as the physicist would say, the phase space, which is position and momentum – has a prominent role in this theory. You talk about a wave front, and you just mentioned the tension between the real and complex worlds.*

Can you explain to us why the cotangent bundle should play such an important role?

MK: That idea, of course, had existed for a long time, for example, in Fourier analysis. We can say that is some kind of incarnation of the symplectic geometry, and physics also, as you mention. The cotangent bundle is important not in a global sense, but in a local sense. Before we thought the idea of “symplectic geometry” works only in a global sense. Sato’s big idea is that it works also in a local world, in the form of the cotangent bundle.

[BID/CFS]: *Could we interject with an interesting fact? We are talking about your master’s thesis, written when you were 23 years of age, and you wrote it by hand in Japanese. It took 25 years before*



Reception at the National Theatre 2025.
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this important manuscript was translated into English, which is remarkable.

MK: If I look at my master’s thesis now, I can see that there is a germ of an idea there, which was important for the subsequent theory.

[BID/CFS]: *These ideas became influential, and, given that they were only in handwritten Japanese, how did the ideas come out? Was that through the SKK paper from 1973?*

MK: In my master’s thesis, the microlocal point of view is very small. However, in the SKK paper the microlocal theory was developed. I worked with Sato and Kawai, and continued to do so for five years or so. During that period the microlocal point of view was fully developed.

[BID/CFS]: *In the paper from ’73, Sato, Kawai and you established the involutivity of characteristics of microdifferential systems, which, of course, had been touched upon by Guillemin, Quillen and Sternberg in 1970, and proved totally algebraically by Gabber in ’81. Explain to us: what is the involutivity of characteristics of microdifferential systems?*

MK: That part is very important. In the cotangent bundle you have some subspaces inside, but the important ones are not arbitrary subsets, just the involutive ones are important. These are the good ones with respect to the symplectic structure. Another word for this is integrability.

[BID/CFS]: *Also in 1973 – it must have been a very good year for you! – you proved an index theorem, which we understand you are especially proud of. You have revisited the index theorem*

several times, for instance in 1985 and in 2014 together with Pierre Schapira. Why do you like this result so much?

MK: It is better to go back a little bit. So, in my master's thesis I proved the index theorem for the one-dimensional case. For higher-dimensional cases, I didn't know how to do that, and that concerned me. The index theorem is a global theorem, you seemingly have to know the local information at every point. But my index theorem is not like that. It is enough to know some generic part – the generic part is enough. I was very surprised when I realized this and thus proved the theorem.

Of course, some of it can be expected because of involutivity. Arbitrary subsets do not appear. Only the involutive ones. There is no small subset which is involutive, which means that the generic part determines the whole thing. When I look back afterwards, then perhaps one can expect this, but it is still surprising.

The Riemann–Hilbert correspondence

[BID/CFS]: *Let's move to another of your major achievements. In 1980, you prove the Riemann–Hilbert correspondence, which in your framework is an equivalence between the derived categories of certain $\mathcal{D}$ -modules and constructible sheaves. The algebraic case had been done ten years before by Pierre Deligne; Zoghman Mebkhout did something very similar at the same time as you, and Alexander Beilinson and Joseph Bernstein should also be mentioned.*

Do we understand correctly that the hard part here is determining how to restrict so that your solution functor becomes fully faithful? Is that the key problem?

MK: There are many ways to look at it, but I think the most difficult part was regularity. So, the Riemann–Hilbert correspondence is a correspondence between two things, the topological part and the algebraic part: the algebraic part corresponds to the $\mathcal{D}$ -modules, and the topological part corresponds to the monodromy. The algebraic part is not about all $\mathcal{D}$ -modules, but more specifically about the so-called *regular* type $\mathcal{D}$ -modules, a very difficult concept. In fact, regularity is not easy to handle. That part is hard. In fact, it took a long time for me to define properly the regular holonomic $\mathcal{D}$ -modules.

[BID/CFS]: *Perhaps it's good to go back to Hilbert 21st problem and connect it to your result. Which part, in Hilbert 21st problem, corresponds to $\mathcal{D}$ -modules, and which one corresponds to the constructible sheaves; and what is Hilbert 21st problem?*

MK: The original problem has to do with the so-called monodromy. The solution to a linear ordinary differential equation is not univalent, it is multivalent. Consequently, if you go around a singularity

the so-called monodromy appears. The original problem is about the connection between the monodromy and the linear ordinary differential operators with regular singularities. The $\mathcal{D}$ -modules correspond to the differential equations, and the constructive sheaves to monodromy.

[BID/CFS]: *So let's see, if X is your manifold, $\mathcal{D}_X$ is the algebra of differential operators, that explains the $\mathcal{D}$ -modules, which then control the differential equations. But how do constructible sheaves at all look like monodromy?*

MK: They're exactly the same! Locally the structure is simple, and sheaves are exactly what connects the local simple structure to form the global picture. I think nowadays people understand monodromy in terms of sheaves. The sheaves patch things together so that you actually have the loop around.

[BID/CFS]: *The Riemann–Hilbert correspondence has been called a parallel, or perhaps, a generalization of the equivalence between the de Rham cohomology and Betti, or singular, cohomology. Is that a valid point of view?*

MK: No, no, no, I don't think so. A bit different point of view is better, I think. The de Rham sheaf figures prominently, but the setting is different. While you have the correspondence between de Rham cohomology and Betti cohomology, the $\mathcal{D}$ -modules are so different, and the correspondence itself is important.

Grothendieck wanted to unify this in a theory of motives. So, there is something called motives, and one aspect is Betti cohomology, and another aspect is de Rham cohomology. You can view them this way or you can view them another way, and you can see two kinds of things. But I think it's a little bit different from what you suggested.

[BID/CFS]: *But, like the correspondence between de Rham and singular cohomology, the Riemann–Hilbert correspondence is between, on one hand, differentials and, on the other hand, topology in the form of constructible sheaves?*

MK: Somehow, what you're saying is right, but somehow it's not. Of course, nowadays they are mixed, for example, in the work of Morihiko Saito. They connect two things, and then you know more information.

[BID/CFS]: *Later on – and published in 2016 – you and Andrea D'Agnolo were actually able to remove the regularity condition at the price of replacing the sheaves with Ind-sheaves. So, how does Ind-sheaves help you “resolve” – if we may – the singularity?*

MK: So, now it concerns not only the regular, but the irregular case also. In the regular case the behaviour of the solution at a singular



Andrea D'Agnolo, professor at the University of Padova, Italy and Masaki Kashiwara enjoy the Abel Lectures at the University of Oslo.
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point is rather easy to understand, but in the irregular case it is more complicated, and it takes a long time to understand how to control the singularities of functions in the irregular case. It's a rather tricky part – it took 20 years to reach a solution.

Ind-sheaves have an origin already in my paper on the Riemann–Hilbert correspondence in the regular case. In that paper, I introduced some operation to construct regular holonomic $\mathcal{D}$ -modules starting from constructible sheaves. Schapira noticed that we can develop this operation further. We have reached the notion of Ind-sheaves to capture a phenomenon we cannot capture by means of “sheaves.” To solve the irregular Riemann–Hilbert correspondence we needed an additional idea: to add one variable.

[BID/CFS]: *Replacing sheaves with Ind-sheaves doesn't seem such a dramatic thing, but it seems to us very drastic to remove the regularity condition. Was it surprising to you that such a “slight” extension actually would resolve the problem?*

MK: Yes, but it's a long history, because in the beginning, in co-operation with Schapira, we did it to control the singularity of functions. So, that is a starting point of Ind-sheaves, but gradually it developed, and the history is rather long.

[BID/CFS]: *On the other hand, the Riemann–Hilbert correspondence and $\mathcal{D}$ -modules figure prominently in the geometric Langlands conjecture; most recently, in the proposed proof by Gaitsgory and his collaborators. What are your thoughts on this? It sits so centrally in there.*

MK: For the geometric Langlands, of course you need $\mathcal{D}$ -modules. It's a very basic tool, but to solve the geometric Langlands it's not enough. You need much more.

The Kazhdan–Lusztig conjecture (and the role of co-operation)

[BID/CFS]: *You have authored more than 250 papers with about 50 co-authors. This is an unusually large number for a mathematician. Could you comment on this, and, in particular, on your cooperation with Jean-Luc Brylinski in 1979 which led to a proof of the Kazhdan–Lusztig conjecture?*

MK: Although the theory of $\mathcal{D}$ -modules was constructed in the 1970s, I did not encounter a chance for its direct application. While in Paris in 1979, the mathematician Jean-Luc Brylinski called me and proposed for us to meet, since he had an idea pertaining to $\mathcal{D}$ -modules. We met, and he talked about the Kazhdan–Lusztig conjecture which had been posed the year before. Brylinski suggested a possible application of $\mathcal{D}$ -modules to prove the conjecture.

This initial suggestion was extremely fruitful, and $\mathcal{D}$ -modules came to play an essential role in our proof of the Kazhdan–Lusztig conjecture. It is also noteworthy that a striking application of the Riemann–Hilbert correspondence is used in the proof. An independent proof was obtained by Beilinson and Bernstein using different methods.

The conjecture itself states that the so-called Kazhdan–Lusztig polynomials give complete information on how a canonical representation splits into irreducible ones.

As for your more general question about having many collaborators, I consider myself fortunate to have comparatively many joint papers. Through these collaborators I have learned a lot about other fields of mathematics.

Crystal bases and quantum groups

[BID/CFS]: *Around 1990, the name Lusztig appears again. He introduced his canonical basis for representations of quantum groups on a vector space over the field $\mathbb{Q}(q)$ of rational functions, and you introduced the notion of the crystal basis, which is a basis as the temperature q goes to zero. That results in a combinatorial description, by means of what you call a “crystal graph.” This was an extraordinary combinatorial tour-de-force which has been dubbed the “the grand loop argument,” involving about 20 interlocked steps. Can you tell us about that?*

MK: I worked with Sato on microlocal analysis, and he started to study so-called exactly solvable models in mathematical physics. He worked with Tetsuji Miwa and Michio Jimbo. I knew them both very well and collaborated with them sometimes. Jimbo introduced so-called quantum groups, independently of Drinfeld. The theory of quantum groups concerns itself with a parameter q , which corresponds to the temperature. I thought that at q equals zero things should simplify. That is what happened: As q goes to zero



Masaki Kashiwara lecture at University of Oslo, 21 May 2025.
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something good happens. I spent at least several months to get it to work.

What comes out of this is the notion of a “crystal basis” – by analogy with physics, where matter crystallizes at low temperature. We can analyse it totally combinatorially, and by a combinatorial method, we can describe the representation itself. The representation is rather difficult to analyse, but combinatorially it is not very difficult.

[BID/CFS]: *How does this propagate from q , the temperature, being equal to zero? Does that lift to bases with positive temperature?*

MK: Afterwards, yes. So, in my case, I started at zero and at zero there is a good basis. And after that, I considered the extended case.

[BID/CFS]: *You’ve returned to this topic several times. For instance, in 2018 together with Seok-Jin Kang and Myungho Kim, you discovered a duality relating quantum affine algebras to certain quiver Hecke algebras. Do you foresee that you will continue working on this?*

MK: That’s right. I’m still working on this.

[BID/CFS]: *Is this the main thing you’re working on, or are there other ideas that you are pursuing?*

MK: Yes, I’m working on that, but also on so-called cluster algebras. Cluster algebras were introduced by Sergey Fomin and Andrei Zelevinsky. That is what I’m most interested in and working on right now.

Actually, that is what I will explain in my Abel lecture on Wednesday.

[BID/CFS]: *We’re looking forward to that!*

Work style, outlook and legacy

[BID/CFS]: *We know it’s not completely fair. But if we were to challenge you, which of your mathematical achievements are you most proud of?*

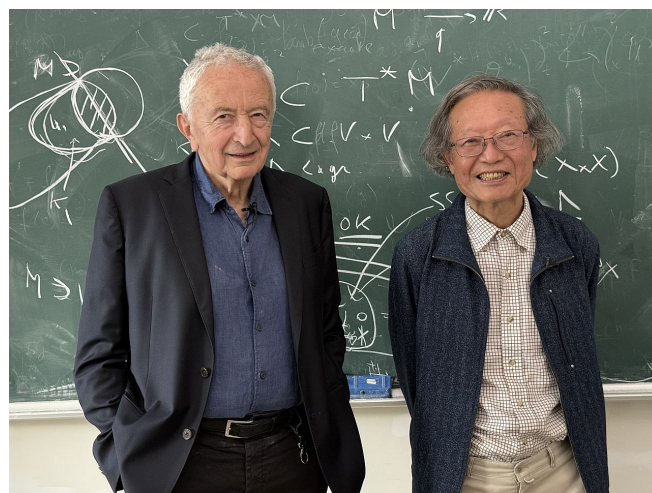
MK: I think it might be the Riemann–Hilbert correspondence and the crystal basis result.

[BID/CFS]: *Another question: working in isolation or working in collaboration, what do you prefer?*

MK: The short answer is: both. However, the meaning of collaboration is different now from when I was young. Today I am working with some younger mathematicians, and I get some energy from working with them.

[BID/CFS]: *Lennart Carleson, the Abel Prize Laureate in 2006, said the following during the Abel Prize interview with him: to prove something hard, it is extremely important to be convinced of what is right and what is wrong. You could never do it by alternating between the one and the other, because the conviction somehow has to be there. Do you agree with that, or...?*

MK: Partially, I agree. I can give you an example. I was working with Sato on microlocal analysis, and after that, with Pierre Schapira, I worked on the microlocal aspect of sheaf theory. When I was working with Sato, I thought that microlocal analysis can only be applied to the singularity of functions. That presumption was not good, and it stifled the imagination. After that I worked with Schapira, and we discovered that microlocal analysis can apply to other things as well.



With Pierre Schapira. (© Liwlig Norway AS / The Abel Prize)

Beyond mathematics

[BID/CFS]: *At the end of these interviews we generally ask: Do you have any special interests besides mathematics?*

MK: Not many, but I like music.

[BID/CFS]: *What sort of music do you enjoy?*

MK: Nowadays, I like Indian music very much. There are basically two types of Indian music, Carnatic and Hindustani. The Hindustani music is from the north, and Carnatic is from the south. The sitar is much used in Hindustani music. Carnatic music doesn't use sitar that much. I started to like Indian music when I was in Chennai.

In Chennai, there is a music season, a music month. All December was one festival, and every night I went to various theatres to listen to music (together with a lot of mosquitoes, I must add!).

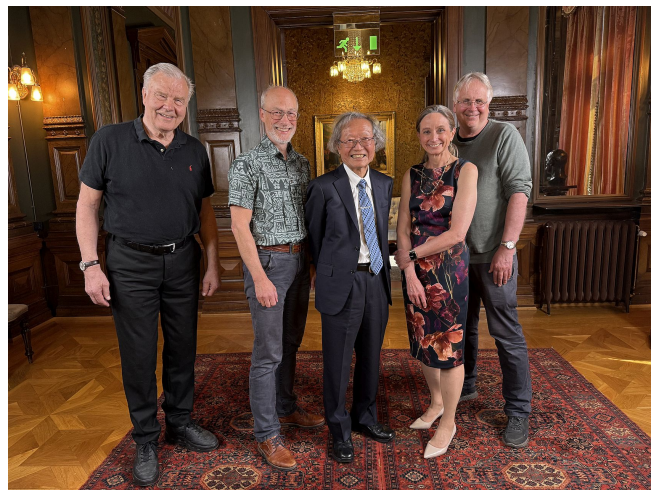
[BID/CFS]: *There is also a rumour that you played table tennis rather well. Do you play table tennis now?*

MK: No, not anymore. I have a bad knee.

[BID/CFS]: *But you were rather actively playing table tennis for a while, right?*

MK: Yes, for a while. Incidentally, I played with Jean-Pierre Serre. He was good; I was beaten by him.

[BID/CFS]: *On behalf of the Norwegian Mathematical Society, the European Mathematical Society, and both of us, we'd like to thank you for this very interesting interview.*



With current and future Abel Prize interviewers. From left to right: Christian Skau, Bjørn Ian Dundas, Masaki Kashiwara, Kathryn Hess and Tom Lindstrøm. (© Liwlig Norway AS / The Abel Prize)

MK: Thank you very much.

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