Interview with Joan Birman

Joan S. Birman is a leading topologist and one of the world's foremost experts in braid and knot theory. She was born on May 30, 1927, in New York City. She received a B.A. degree in mathematics in 1948 from Barnard College and an M.A. degree in physics two years later from Columbia University. She worked on mathematical problems in industry for several years, raised three children, and eventually returned to graduate school in mathematics. She received her Ph.D. in 1968 at the Courant Institute at New York University, under the direction of Wilhelm Magnus. She was on the faculty of the Stevens Institute of Technology (1968–1973), during which time she also held a visiting position at Princeton University. Her influential book *Braids, Links, and Mapping Class Groups* (Annals of Mathematics Studies, number 82, 1974) is based on a series of lectures she gave during her time at Princeton. In 1973 she joined the faculty of Barnard College, Columbia University, where she has remained ever since and where she is now Research Professor Emeritus.

Joan Birman

Birman's honors include a Sloan Foundation Fellowship (1974–1976), a Guggenheim Fellowship (1994–1995), and the Chauvenet Prize of the Mathematical Association of America (1996). She was a member of the Institute for Advanced Study, Princeton, in spring 1987. In 1997 she received an honorary doctorate from Technion Israel Institute of Technology. She received the New York City Mayor's Award for Excellence in Science and Technology in 2005.

Birman has had twenty-one doctoral students and numerous collaborators. She has served on the editorial boards of several journals and was among the founding editors of two journals, *Geometry and Topology* and *Algebraic and Geometric Topology*. Both journals are now published by the nonprofit Mathematical Sciences Publishing Company, for which Birman serves on the board of directors.

In 1990 Birman donated funds to the AMS for the establishment of a prize in memory of her sister, Ruth Lyttle Satter, who was a plant physiologist. The AMS Ruth Lyttle Satter Prize honors Satter's commitment to research and to encouraging women in science. It is awarded every other year to a woman who has made an outstanding contribution to mathematics research.

What follows is an edited version of an interview with Joan Birman, conducted in May 2006 by *Notices* Deputy Editor Allyn Jackson and Associate Editor Lisa Traynor.

Early Years

Notices: *Let's start at the beginning of your life. Were your parents American? Were they immigrants?*

Birman: My father was born in Russia. He grew up in Liverpool, England, and came to the United States when he was seventeen, to search for lost relatives and to seek a better life. My mother was born in New York, but her parents were immigrants from Russia-Poland.

Notices: *What did your father do?*

Birman: He started as a shipping clerk in the dress industry and worked his way up to become a successful dress manufacturer. He told his four daughters repeatedly that the U.S. was the best country in the world, a land of opportunity. Paradoxically, he also told them, "do anything but go into business." He wanted us all to study.

Notices: *Did your mother have a profession?*

Birman: No, she was a housewife. Neither of my parents finished high school.

Birman's parents, Lillian and George Lyttle.

Notices: *Why did they emphasize their four daughters getting an education?*

Birman: Jewish culture, as it was handed down to us, included the strong belief that Jews survived for so many years in the Diaspora because they were "the people of the book". The free translation, when I brought home an exam with a grade of 98, was "what happened to the other 2 points?" Becoming an educated person, and using that education to do something bigger than just to earn money, was set up to my generation as a very important goal.

Notices: *When you were a child, did you like mathematics?*

Birman: Yes, I liked math, from elementary school, and even earlier than that, although I did not know enough to pinpoint what I liked.

Notices: *Were there teachers in your early years who encouraged you in mathematics, or who were inspiring?*

Joan Birman, age 11.

Birman: In elementary school that's hard to say, although we certainly had challenging math. I went to an all-girls high school in New York, Julia Richmond High School. It was really a rough inner-city high school, but within it there was a small academic unit, a school within a school. We had some very good teachers. We had a course in Euclidean geometry, and every single night we would have telephone conversations and argue over the solutions to the geometry problems. That was my introduction to proof, and I just loved it, it was wonderful. When the course ended, I joined a small group of girls who campaigned for more geometry, but the teacher (her name was Miss Mahoney) was willing but perhaps not knowledgeable enough to know how to continue to challenge the intellectual interests of this eager group of girls! She taught us 3-dimensional Euclidean geometry, and that was a little dull. If she had taught us hyperbolic geometry, or group theory, where we would have encountered new ideas, we would have been in heaven!

Notices: *Usually high school girls are on the phone talking about their hair.*

Birman: We did that too! Actually I was in this little group, and we were definitely regarded as being nerds. Most of the girls in our selective school within a school worked hard and got good grades, but talked all the time about boys and clothes. I was a late developer and wasn't ready for that. I didn't date at all until I was in college. Still, at one point I was elected president of the class, so the other students could not have been really hostile. I felt accepted, and even liked. There was an atmosphere of tolerance.

Notices: *Were your sisters also interested in math?*

The Lyttle sisters (left to right), Ada, Ruth, Helen, and Joan.

Birman: Yes. My oldest sister, Helen, was a math major at Barnard, and the next one, Ruth, was a physics major. Ruth ultimately became a plant physiologist. She was Ruth Satter of the Satter Prize. She had a fine academic career, before her untimely death from leukemia. Helen is independently wealthy and is a philanthropist, with very special interests of her own. My younger sister, Ada, became a kindergarten teacher. She was less oriented toward academics.

Notices: *Did you like math when you went to college?*

Birman: Two things changed. First, the college math course that I was advised to take at Swarthmore was a cookbook calculus course, and it was both boring and unconvincing. So I looked around and found other things that appealed to me (astronomy, literature, psychology), although I did major in math. Then I transferred to Barnard College, in order to be able to live in New York. At Barnard, the math offerings were all low-level. When you got to the point where you were ready for serious math, you were directed to courses at Columbia, which at that time was an all-male school. That was the first time that I hit a situation where I was one of a very small number of girls. Most of the Barnard women were cowed by it and gave up. Eventually I was the only girl in my classes, and I caught the idea that maybe math was not for girls.

From Bachelor's Degree to Industry

Notices: *But then you did get a bachelor's degree.*

With husband Joe in 1954.

Birman: Yes. But there was a long gap before I went on to graduate school. The social atmosphere had presented unexpected difficulties. My parents not only expected their four daughters to get married, but we were to get married in order! There was all kinds of nonsense like that. But on the other hand, the only way that a respectable girl could get out from under her parents control was to marry, so I was not averse to the idea. But I did not want to make a mistake in my choice, and that took attention. I *did* think about going to graduate school, but I understood how hard math was. I thought it would take lots of concentrated effort, as it must for any serious student. I was afraid that I would wreck my life if I gave math that kind of attention at that time. (I think I was right. As we talk, Joe and I have been married for fifty-six years, and he has been my biggest supporter.) Actually, I didn't really decide *not* to go to graduate school, but when the opportunity arose to put it off and accept an interesting job, the job was appealing.

The job was very nice. I was extremely lucky. It was at an engineering firm that made microwave frequency meters. These meters were cylindrical cans with two parameters, the radius of the base and the depth (or height). The radius was fixed, but the depth could be changed with a plunger, changing the resonant frequency. The (depth-to-resonant frequency) curve was nonlinear, and the problem was that they had a hard time calibrating the dials, putting the notches on to indicate what the frequency would be as you pushed the plunger in. They hired me because they had the idea that they could sell more meters if they could push in the plunger in a novel way that would yield an approximately linear response curve. In calculus I had learned about ladders sliding against a wall, and in the job interview the idea came up that the curve that gave the height of the ladder as a function of its distance from the wall might be a curve that could be fitted to the experimental data. The idea worked very well. For about eight months I computed the parameters, and they constructed meters of all sizes with plungers that pushed in along an axis orthogonal to the axis of the can. The dials were for all practical purposes linear. I was very happy!

But when that project ended, they set me to work taking measurements on an oscilloscope, and that was pretty dull. One day I happened to run into my old physics professor from Barnard, and he offered me a position as the physics lab assistant at Barnard. I took the position and applied to graduate school in physics. I realized that my job possibilities would improve if I had a physics degree.

I did get a master's degree in physics, but I do not have good intuition for the subject. I felt they could just tell me anything, and I would have to believe it. I am astonished these days at the way in which physics has fed into math. Physicists do seem to have an intuition that goes beyond what mathematicians very often see, and they have different tests of truth. I just didn't have that intuition. Yet I really enjoyed the physics lab, because when I saw things in the lab, I knew they were true. But I didn't always trust the laws of physics that we learned. On the other hand, I got an MA, and then I got a better job.

Notices: *This was in the aircraft industry?*

Birman: Yes. It was in the days of analog computers. I worked on a navigation computer. The pilot would be flying a plane, and the computer would send a radar signal to the ground. The signal would be bounced back to the plane. The computer measured the Doppler shift and used it to compute air speed and altitude. My part of the whole thing was error analysis—to figure out the errors when the plane was being bounced around by changes in air pressure. A second problem was that of maximizing aircraft range for a fixed amount of fuel. A third was the design of a collision avoidance system.

Notices: *Were there many women?*

Birman: I worked at three different engineering firms. At one of them there were several women, but at the others I don't recall any other women.

Wandering toward Graduate School

Notices: *You got married when you were studying physics in graduate school. Did you stop working then?*

Birman: No, I continued to work until I had a child, five years later. When my first child was born, I planned to go back to work because I really liked what I was doing. But that posed a problem. In those days, there was no day care. Unless you had a family member to take care of your children (and my mother and mother-in-law were unable to do that), it was almost impossible. My husband and I had thought, very unrealistically, we will put an ad in the paper and hire somebody. But then, I had this huge responsibility for our baby, and I just couldn't see leaving him with somebody about whom I knew very little. My husband was very encouraging about my going back to work. I did work a few days a week. First I worked two days a week, then one day a week after we had a second child. Just before our third child was born, my husband had been invited to teach in a distant city. He had been in industry and was thinking about a switch to academia. During that year, I had to stop my part-time job, but it had already dwindled down to one day a week. When I came back I knew I couldn't work that way anymore. So I went to graduate school with the idea of learning some new things for when I'd go back to work. You can see that I led a very wandering and undirected life! It amazes me that I got a career out of it—and it has been a really good career!

Notices: *When did you then decide that you would get a Ph.D.?*

Birman: I started grad school in math right after my younger son was born, on January 12, 1961. I went to New York University, where my husband was on the faculty, so that my tuition was free. NYU's Courant Institute had an excellent part-time program, with evening courses that were essentially open admissions. I took linear algebra the first semester, and then real and complex analysis the following year. And then I decided I could handle two courses a year, and did.

One of the first courses I took was complex analysis, with Louis Nirenberg. In the first lecture he said, "A complex number is a pair of real numbers, with the following rules for adding and multiplying them." I certainly knew about "imaginary numbers", but he put them into a framework that was sound mathematics. It sounds like a trivial change, but it was not. Eventually, I also had a course in topology, which I loved, with Jack Schwartz. He was not a topologist, and when I go back and look at my notes, I see it was a weird topology course! He was somebody who liked to try new things. He taught us cohomology in a beginning topology course—not homology, not even the fundamental group! But I really loved that course. It really grabbed me, although the approach had its down side, as I knew almost no examples. I had started studying at Courant with the intention of learning some applied mathematics. But everything I learned pushed me toward pure mathematics.

At Courant I was starting to pile up enough courses for an MA, and there was a required master's final exam. When I took the exam, I didn't realize it was also the Ph.D. qualifying exam. I was surprised when I passed it for the Ph.D. That's when I applied for financial assistance, but to get it I had to be a full-time student. So that's really when I started on a Ph.D. track. There were not many women around. The people in the department were very nice to me—they realized that I had three children, and they did not give me heavy TA assignments. Karen Uhlenbeck was one of the students there, but she transferred out. Cathleen Morawetz was on the faculty, and I took one course from her.

Notices: *Your adviser was Wilhelm Magnus. How did he end up being your adviser?*

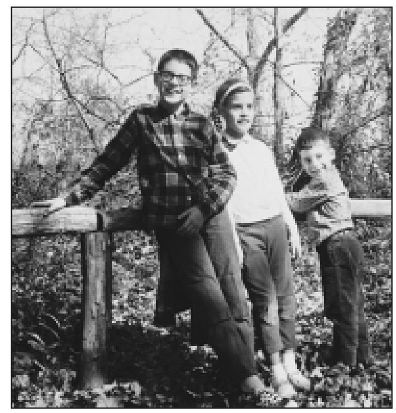
Birman: After passing the qualifying exams, one had to take a series of more specialized exams for admission to research. My husband was on the NYU faculty, and the first question I was asked in one of the exams was, "Who is smarter, you or your husband?"

Notices: *That was the first question?*

Birman: Yes, it's ludicrous, in 2006. Later on when I became a mathematician, I met the person who asked this question and reminded him of it, and he said, "Oh no, not me! I didn't say that!"

Notices: *How did you answer the question?*

Birman: I laughed. It was the only thing to do. Afterwards I started to get really angry about it. It was a stupid question!



The Birman children (left to right), Kenneth, Deborah, and Carl David, around 1968.

Anyway, I passed that exam too and went looking for an advisor. The first person I approached was the topologist Michel Kervaire, but he wasn't interested. He said, "You're too old and you don't know enough topology." He was right, I didn't know enough topology. And I can understand why he would be skeptical of a person my age. You have to be convinced when you see someone who is outside of the usual framework that the person is a serious student, and he had never been my classroom teacher.

I went to speak to Nirenberg. He was very helpful to me. I read the *Notices* interview with him, and he had told you that he loved inequalities. That's funny, because I remember he asked me, "Do you like inequalities?" And I said, "No, I don't like inequalities!" He said, "Then you don't want to study applied math." And he was right!

Notices: *That was a good question to ask!*

Birman: It was an excellent question. After that I went to talk to Wilhelm Magnus. He had noticed me, because I had done some grading for him. He was an algebraist, but he had noticed that I loved topology, and so he met me halfway and gave me a paper to read about braids. That showed great sensitivity on his part. It was a terrific topic. He later told me of his habit of picking up strays, and in some way I was a stray.

Notices: *What paper was it that he gave you?*

Birman: It was a paper by Fadell and Neuwirth [1]. The braid groups were defined in that paper as the fundamental group of a certain configuration space. Magnus said that he didn't understand the definition, and it took me a long time to understand it. Finally I did, and I was very happy. Magnus had worked on the mapping class group of a twice-punctured torus, and he had suggested that I could extend this work to a torus with 3 or 4 punctures. My thesis ended up being about the mapping class group of surfaces of any genus with any number of punctures. He thought that was a real achievement. As soon as I understood the problem well enough, I solved it. It was both fun and very encouraging.

Around this time there was a very different paper by Garside on braids that interested me greatly [2]. I was aware of the fact that there was a scheme for classifying knots with braids. When I saw that Garside had solved the conjugacy problem in the braid group, I thought that was going to solve the knot problem. I couldn't have been more mistaken, but still, it grabbed my interest. I am still working on it—right now I am trying to show that Garside's algorithm can be made into a polynomial algorithm. This is important in complexity theory. So my interest in that problem dates back to graduate school.

Moving Into Research

Notices: *After you got your Ph.D., you got a job at Stevens Institute of Technology.*

Birman: I had not done a thorough job on applications and was not offered any job until late August 1968, when Stevens Institute had some unexpected departures. The first year I was there I started working with Hugh M. Hilden (who is known as Mike). We solved a neat problem that year and wrote several really good papers. The one I like best is the first in the series [3].

The work with Hilden was very rewarding. My thesis had been on the mapping class group of a punctured surface. I showed there is a homomorphism from the mapping class group of a punctured surface to that of a closed surface, induced by filling in the punctures. I worked out the exact sequence that identified the kernel of that homomorphism, but I didn't know a presentation for the cokernel, the mapping class group of a closed surface, and realized that was a problem that I would like to solve. The whole year I talked about it to Mike, whose office was next to mine, and finally we solved the problem for the special case of genus 2. As it turned out, our solution had many generalizations, but the key case was a closed surface Σ of genus 2. In that case, the mapping class group has a center, and the center is generated by the class of an involution that I'll call $\mathcal{I}$. The orbit space $\Sigma/\mathcal{I}$ is a 2-sphere S^2, and the orbit space projection $\Sigma \to \Sigma/\mathcal{I} = S^2$ gives it the structure of a branched covering space, the branch points being the images on S^2 of the 6 fixed points of $\mathcal{I}$. We were able to use the fact that the mapping class group of S^2_6 of S^2 minus those 6 points was a known group (related to the braid group), to find a presentation for the mapping class group $\mathcal{M}(\Sigma)$ of Σ. The difficulty we had to overcome was that mapping classes are well-defined only up to isotopy. We knew that in genus 2, every mapping class was represented by a map that commuted with $\mathcal{I}$, but we did not know whether every *isotopy* could be deformed to a new isotopy that commuted with $\mathcal{I}$. We felt it had to be true, but we couldn't see how to prove it. One day Mike and I had the key idea, together. The idea was to look at the path traversed on Σ by one of the 6 fixed points, say p, under the given isotopy. This path is a closed curve on Σ based at p. Could that closed curve represent a nontrivial element in $\pi_1(\Sigma, p)$? It was a key question. Once we asked the right question, it was easy to prove that the answer was no, and as a consequence our given isotopy could be deformed to one that projected to an isotopy on S^2_6. As a consequence, there is a homomorphism $\mathcal{M}(\Sigma) \to \mathcal{M}(S^2_6)$, with kernel $\mathcal{I}$. Our hoped-for presentation followed immediately. It was a very fine experience to work with Mike, to get to know him as a person via shared mathematics. It was the first time I had done joint work, and I enjoyed it so much that ever since I have been alert to new collaborations. They are different each time, but have almost all been rewarding.

At that point I was thoroughly involved in mathematics. But my husband had a sabbatical, and I had promised him that I would take a year off so that he could spend his sabbatical with collaborators in France. So I took a leave of absence from my job and found myself in Paris, and in principle it should have been a lovely year. But we had three children, and once again I had lots of home responsibilities! Moreover, I didn't know any of the French mathematicians, because I had come to France without any real introductions, and nobody was interested in braids. French mathematics at that time was heavily influenced by the Bourbaki school. I found myself very isolated and discouraged. Looking for a problem that I could handle alone, I decided to do a calculation.

There is a homomorphism from the mapping class group of a surface to the symplectic group. People knew defining relations for the symplectic group, but not for the mapping class group, unless the genus is ≤ 2. I was interested in the kernel of that homomorphism, which is called the Torelli group. It was an immense calculation. I finished it, and I did get an answer [4], which was later improved with the help of a Columbia graduate student, Jerome Powell. In 2006 a graduate student at the University of Chicago, Andy Putman, constructed the first conceptual proof of the theorem that Powell and I had proved. Putman's proof finally verifies the calculation I did that year in France!

When I returned from France I was invited to give a talk at Princeton on the work that Hilden and I had done together. That was when my career really began to get going, because people were interested in what we had done. I was invited to visit Princeton the following year. I did that, commuting from my home in New Rochelle, New York, to Princeton, New Jersey. That was a very long commute.

Notices: *Was it around this time that you gave the lectures that became your book* Braids, Links, and Mapping Class Groups *[5] ?*

Birman: Exactly. The lectures were attended by a small but interested group, including Ralph Fox and Kunio Murasugi, and James Cannon, at that time a postdoc. Dmitry Papakyriakopolous was also at Princeton, and he was very welcoming to me.

Braids had not been fashionable mathematics, and their role in knot theory had been largely undeveloped. Three topics that I developed in the lectures and put into the book were: (1) Alexander's theorem that every link type could be represented, nonuniquely, by a closed braid, (2) Markov's theorem, which described the precise way in which two distinct braid representatives of the same link type were related, one of those moves being conjugacy in the braid group, and (3) Garside's solution to the problem of deciding whether two different braids belonged to the same conjugacy class. I had chosen those topics because I was interested in studying knots via closed braids, and together (1), (2), and (3) yielded a new set of tools.

When I had planned the lectures at Princeton, to my dismay I learned that there was no known proof of Markov's theorem! Markov had announced it in 1935, and he had sketched a proof but did not give details, and the devil is always in the details. When I told my former thesis advisor, Wilhelm Magnus, he remarked that the sketched proof was very likely wrong! But luckily, I was able to follow Markov's sketch, with the help of some notes that Ralph Fox had taken at a seminar lecture given by a former Princeton grad student (his name vanished when he dropped out of grad school). After some number of 2:00 a.m. bedtimes I was able to present a proof. There are now some six or seven conceptually different proofs of this theorem, but the one in my 1974 book was the first.

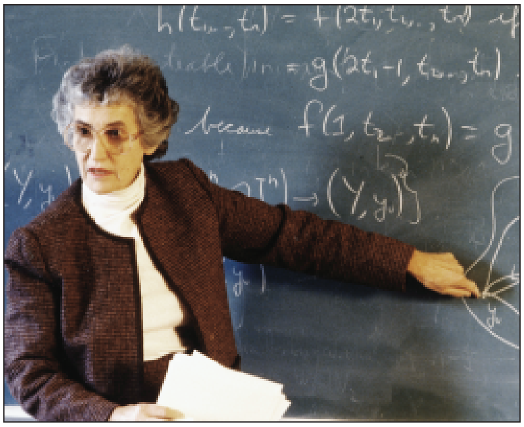
Knot Polynomials and Invariants

Notices: *Can you tell us about your interaction with Vaughan Jones, when he was getting his ideas about his knot polynomial?*

Birman: One day in early May 1984, Vaughan Jones called to ask whether we could get together to talk about mathematics. He contacted me because he had discovered certain representations of the braid group and what he called a "very special" trace function on them, and people had told him that I was the braid expert and might have some ideas about its usefulness. He was living in New Jersey at the time, so he was in the area, and we agreed to meet in my office. We worked in very different parts of mathematics and we had the expected difficulties in understanding each other's languages. His trace arose in his work on von Neumann algebras, and it was related to the index of a type II_1 subfactor in a factor. All that was far away from braids and links. When we met, I told him about Alexander's theorem, and Markov's theorem, and Garside's work. He told me about his representations and about his trace function. Of course, his explanations were given in the context of operator algebras. I recall that I said to him at one point, Is your trace a matrix trace? And he said no, it was not. Well, that answer was correct, but he did *not* say that his trace was a weighted sum of matrix traces, and so I did not realize that, if one fixed the braid index, the trace was a class invariant in the braid group. He understood that very well and did not understand what I had missed. He would willingly have said more, if he had, because

Left to right: Vaughan Jones, Bill Menasco, Joan Birman.

he is super-generous and truly decent. In between our meetings he gave the matter much thought (which I did not!), and one night he had the key idea that by a simple rescaling of his trace, it would in fact become invariant under all the moves of Markov's theorem, and so become a link invariant. He told me all this, in great excitement, on the telephone. The proof that his normalized trace was a link invariant was immediate and crystal clear. After all, a good part of my book had been written with the goal of making the Alexander and Markov theorems into useful tools in knot theory, and Vaughan had used them in a very straightforward way.



Birman teaching at Columbia, 1985.

Was his new invariant really new, or a new way to look at something known? He did not know. Examples were needed, and a few days later we met again, in my office, to work some out. That was probably May 22, 1984. The new link invariant was a Laurent polynomial. My first thought was: it must be the Alexander polynomial. So I said, "Here are two knots (the trefoil and its mirror image) that have the same Alexander polynomial. Let's see if your polynomial can distinguish them." To my astonishment, it did! Well, we checked that calculation very carefully, on lots more examples, because the implications were hard to believe. By pure accident, I had recently worked out a closed braid representative of the Kinoshita-Terasake 11-crossing knot, whose Alexander polynomial was zero. Fishing it out of my file cabinet we learned very quickly, that same day, that the new polynomial was nonzero on it. So in just that one afternoon, we knew that he not only had a knot invariant, but even more it was brand new. I remember crossing Broadway on my way home that night and thinking that nobody else knows this thing exists! It was an amazing discovery. Very quickly, other parts of the new machinery came to bear, and the world of knot theory experienced an earthquake. There was not just the Jones polynomial, but also its cousins, the HOMFLY and the Kaufman polynomials, and lots more. And some of the stuff in my book about mapping class groups was relevant too. Much later, Garside's machinery appeared too, in a particular irreducible representation of the braid group that arose via the same circle of ideas. Garside's solution to the word problem was used by Daan Krammer to prove that braid groups are linear.

There was another related part to this story. In 1991 Vladimir Arnold came to the United States to visit Columbia for a semester. I knew Arnold and met him in the lobby as he arrived, in September, with his suitcase. He is a very excitable and enthusiastic man. He put down his suitcase right then and there and opened it on the floor next to the elevator to get out a paper he had brought for me. It was by his former student Viktor Vassiliev. He said, "You have to read this paper, it's wonderful, it contains new knot invariants, and they come from singularity theory, and it's fine work, and I would like your help in publicizing it!" Of course I looked at the paper. At that point there had been an explosion in new knot invariants, and the open question was what they meant geometrically. And here Arnold was, with more invariants! The old ones were polynomials, the new ones were integers (lots of integers!). Arnold asked me to copy and distribute the paper in the United States. So one afternoon shortly after his arrival I made lots of photocopies, and sent them out to everyone I could think of who seemed appropriate. But even as I did it I suspected the knot theory community might not be so overjoyed to have yet more knot invariants coming unexpectedly out of left field! There is resistance to learning new things. We had just learned about operator algebras, and suddenly we had to learn about singularity theory! But Arnold kept after me, at tea every day.

Xiao-Song Lin was an assistant professor in the department, and his field is knot theory. We ran a seminar together and talked every day. We were good friends, and he was always ready to talk about math. I told him about the paper of Vassiliev. We read it together, and we finally understood most of it. We said, here are the Vassiliev invariants, and there are the knot polynomials—and they must be related in some way. But how? For a fixed knot or link, its Jones polynomial was a one-variable Laurent polynomial with integer coefficients, whereas its Vassiliev invariants were an infinite sequence of integers, or possibly of rational numbers.

Birman with Xiao-Song Lin, March 1998.

We had an idea that perhaps we should, for the moment, set aside the fact that the Vassiliev invariants came from the machinery of singularity theory, and try to construct them from their properties. We did that because we knew that the Jones polynomial (the simplest of the knot polynomials) could be constructed from its properties. We thought that might be a way for us see a connection. That had good and bad consequences. The bad one was that later, Vassiliev invariants were renamed "finite type invariants", and were defined via our axioms. In the process their origins in singularity theory were lost and remain underdeveloped to this day.

Soon Lin and I realized how to make the connection we had been seeking. We had the idea of making a change of variables in the Jones polynomial, changing its variable from x to t, with $x = e^t$.

The Jones polynomial was a Laurent polynomial in x, and e^{kt} has an expansion in positive powers of t for every positive and negative integer k. This change in variables changes the Jones polynomial to an infinite series in powers of t. We were able to prove that the coefficients in that infinite series satisfied all of our axioms for Vassiliev invariants, and so were Vassiliev invariants [6]. Everything went quickly with that idea—eventually all the knot polynomials were related to Vassiliev invariants in this way. They are generating functions for particular infinite sequences of FT invariants. But in fact the set of FT invariants is larger than those coming from knot polynomials. They are more fundamental objects.

Rich Problems, Rich Collaborations

Notices: *Can you tell us about your recent work with Menasco that involved the Markov theorem?*

Birman: That is another aspect of the same underlying project, to understand knots through braids. In 1990 at the International Congress in Kyoto, when Vaughan Jones got the Fields Medal, I gave a talk on his work. Afterward Bill Menasco invited me to give a colloquium based on it in the math department in Buffalo. So I gave a talk there about Vaughan Jones's work, and I stayed at Bill's house that night. We started to talk, and he said, "What problems are you working on? What's your dream?" I told him my dream is to classify knots by braids. I had an idea about how you could avoid the "stabilization" move in Markov's theorem. Then about three weeks later, I got a letter from him saying "I have an idea how we might try to prove the 'Markov theorem without stabilization' (MTWS)." And that's when our collaboration began. Of course, my original conjecture was much too simple. We kept solving little pieces of the sought-for theorem. We wrote eight papers together. The last one stated and proved the MTWS [7]. There was also an application to contact topology [8].

I like to collaborate. My collaborators are also my best friends. Bill Menasco and I are very good friends. We have had such a long collaboration. But we have very different styles. He can sit in a chair and stare at the ceiling as he works on mathematics, but I like to talk about it all the time.

Notices: *Why do you do mathematics?*

Birman: To put it simply, I love it. I'm retired right now, I don't have any obligations, and I keep right on working on math. Sometimes mathematics can be frustrating, and often I feel as if I'll never do another thing again, and I often feel stupid because there are always people around me who seem to understand things faster than I do. Yet, when I learn something new it feels so good! Also, if I work with somebody else, and it's a good piece of mathematics, we get to know each other on a level that is very hard to come by in other friendships.

Kirbyfest, MSRI, February 1998. Joan Birman in front row, fifth from right, with Robion Kirby on her right.

I learn things about how people think, and I find it very moving and interesting. Mathematics puts me in touch with people on a deep level. It's the creativity that other people express that touches me so much. I find that, and the mathematics, very beautiful. There is something very lasting about it also.

Notices: *Let's go back to the connections between your work and complexity theory. Did you come up with an algorithm that can tell whether a knot is the trivial knot?*

Birman: Yes. But the algorithm that Hirsch and I discovered [9] is slow on simple examples, and it is slow as the complexity of the example grows. Yet it has the potential to be a polynomial algorithm, and I don't think that's the case for the more fashionable algorithms coming from normal surface theory. There is a misunderstanding of our paper. Readers who did not read carefully saw that we used normal surfaces in our paper (in a somewhat tangential manner). They dismissed our paper as being derivative, but it was not. There are ideas in our work that were ignored and not developed.

However, at the present moment it seems most likely that the problem of algorithmically recognizing the unknot will be solved via Heegaard Floer knot homology. That is a very beautiful new approach, and fortunately there is an army of graduate students working on it and making rapid progress. It was, somehow, fashionable from day one and received lots of attention. That can make a big difference in mathematics.

Notices: *Are there connections between this and the P versus NP problem?*

Birman: Yes, there are connections, but they are not directly related to the unknot algorithm. A problem that has been shown to be NP-complete is "non-shortest words in the standard generators of the braid group". If you had an algorithm to show that a word in the standard generators of the braid group is not the shortest representative of the element it defines, and could do that in polynomial

Birman with some of her former graduate students.

time, then you would have proved that P is equal to NP. Of course, if you are given any word in the generators of the braid group and want to know whether it is shortest or not, all you have to do is try all the words that are shorter than it—and since there is a polynomial solution to the word problem, you can test quickly whether any fixed word that's shorter than the one that you started with represents the same element. However, the collection of all words that are shorter than the given one is exponential, so that solution to the non-shortest word problem is exponential. But the normal forms that I am working on in the braid group are such that if you could understand them better, you might learn how to improve this test. But I am not holding that up as a goal. At the moment it seems like a question that is out of reach.

I have been working on a related question: the conjugacy search problem in the braid group. It's complicated and difficult, but I believe strongly that it won't be long before someone proves that it has a solution that's polynomial in both braid index and word length. It's a matter of understanding the combinatorics well enough. It is related to (but considerably weaker than) the P versus NP problem. I am working on that problem right now with two young mathematicians, Juan González-Meneses from Seville, Spain, and Volker Gebhardt from Sydney, Australia.

Notices: *It's amazing that knot theory and braids are connected to so many things.*

Birman: I think I was very lucky because my Ph.D. thesis led me to many different parts of mathematics. The particular problems that are suggested by braids have led me to knot theory, to operator algebras, to mapping class groups, to singularity theory, to contact topology, to complexity theory and even to ODE [ordinary differential equations] and chaos. I'm working in a lot of different fields, and in most cases the braid group had led me there and played a role, in some way.

Notices: *Why do braids have all these different connections?*

Birman: Braiding and knotting are very fundamental in nature, even if the connections do not jump out at you. They can be subtle.

Notices: *Which result of yours gave you particular pleasure?*

Birman: There are many ways to answer that question. I have had much pleasure from discovering new mathematics. That happened, for example, when I was working on my thesis. The area was rich for the discovery of new structure, and (unlike most students) I experienced very little of the usual suffering, to bring me down from that high. I have also gotten much pleasure from collaborations and the friendships they brought with them. I would probably single out my good friend Bill Menasco as one of the best of my collaborators. It has been a particular pleasure to me when others have built on my ideas, and I see them grow into something that will be there forever, for others to enjoy. In that regard, I would single out the work that was done by Dennis Johnson in the 1980s, which built in part on the calculation I had done alone in Paris in 1971 and in another part on my joint work with Robert Craggs [10]. In a related way, I get great pleasure when I understand an idea that came from way back. An example was when I read several papers of J. Nielsen from the 1930s on mapping class groups. (I had to cut open the pages in the library, they had been overlooked for a long time.) Nielsen's great patience and care in explaining his ideas, and their originality and beauty, reached out over the years. I also feel privileged to have worked as an advisor of very talented young people and to have been a participant in the process by which they found their own creative voices.

It would be dishonest not to add that the competitive aspect of math is something I dislike. I also find that the pleasure in various honors that have come to me is not so lasting and have the disagreeable aspect of making me feel undeserving. The pleasure in ideas and in work well done is, on the other hand, lasting. But it's easy to forget that.

Women in Mathematics

Notices: *The situation for women in mathematics has changed greatly. Have all the problems been solved?*

Birman: No, of course not. The disparity in the numbers of men and women at the most prestigious universities (and I include Columbia in that) is striking. Anyone who enters a room in the math building at Columbia when a seminar is in progress can see it.

Notices: *Do you think attitudes toward women in mathematics have improved?*

Birman: Enormously, in my lifetime. On the whole, I think the profession is now very accepting of women. When I took my first job I was the first woman faculty member at Stevens Institute of Technology. A few years later, I was the only woman faculty member (and I was a visitor) in the Princeton math department. Now one sees ever-increasing numbers of women faculty members, although the numbers in the top research faculties are still very small. That is certainly the case at Columbia, but this year for the first time, Columbia's freshman class of graduate students was half men, half women. Just six years ago it was all men, no women.

Recently several young people I know who are husband-and-wife mathematicians have gotten jobs in the same department. There used to be nepotism rules against that. It's such a big effort for a department to make, to hire two people at the same time, in whatever fields they happen to be in, sometimes the same field. It's impressive that departments care enough about doing right by women to do it. So yes, I think things are changing.

But there are serious issues regarding women in research. At the moment there are a very small number of women at the top of the profession. This is the very thing that Lawrence Summers [former Harvard University president] pointed out. What are the reasons for it, and what can we do about it? It would be good to try to understand why, and if we don't admit all possibilities, then we may never find out. So I was rather shocked that women on the whole did not want to look at that problem openly.

Notices: *He offended a lot of women when he speculated that there might be a biological difference between men and women that accounts for the difference of performance.*

Birman: Yes, he offended, but the reaction "stop, don't ask that question" was not a good response. Women in math have done so much to help other women, and the issues are so complex, that I was distressed that political correctness overshadowed the need to understand things better. The truth may not always be pleasant, but let's find out what it is. If women mathematicians refuse to face the issue openly, then who will do it for them? The sociologists? I hope not. However, that kind of discussion is not my strong point. I am too opinionated and tactless to say what needs to be said. Ralph Fox gave me tongue-in-cheek advice long ago: "Speak often and not to the point, and soon they will drop you from all the committees."

I did, however, wonder for many years whether there was a way for me to help other women. Rather early in my career I began to work with male graduate students, and I enjoyed that very much. Yet the first time a Columbia woman graduate student (Pei-Jun Xu, Ph.D. Columbia 1987) asked whether she could work with me, my private reaction was "together we will probably make a total mess of it!". We did not, and she wrote a fine thesis, and on the way I understood that I could help her in more ways than math just because we were both women and I sensed some of her unspoken concerns. Ever since then I realized that was the unique way that I could help other women—simply by taking an interest, working with them when it was appropriate, and being open to their conflicts and sensitive to their concerns.

Notices: *That's what it comes down to, the women actually doing mathematics.*

Birman: Yes, of course it does.

I have heard some women who are bitter because they feel the rewards of research don't seem big enough for the sacrifice. Of course there are men who feel that way too. Fritz John, a very fine research mathematician, once said to me that at the end of the day the reward was "the grudging admiration of a few colleagues". Well, if what you are looking for is admiration because you have done a great piece of work, admiration is often not there (and maybe the work isn't so great either). What is much more important, to me, is when somebody has really read and understood what I have done, and moved on to do the next thing. I am thrilled by that. Sure, it's nice to get a generous acknowledgment, but that is a bonus. The real pleasure is to be found in the mathematics.

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