Interview

with Andrei Okounkov

Interviewer: Hiraku Nakajima

Studied Real Math in Special Courses in the Evening at Moscow State University

Nakajima: Thank you very much for making time for our conversation today. This is a nice opportunity for me to ask you some questions. I wanted to ask you them during your stay at our RIMS.

Okounkov: It's my pleasure. I also have some questions I want to ask you later.

Nakajima: Okay, let me start with hearing about your academic background. What did you study at Moscow University, especially in mathematics and physics? Your supervisors were Kirillov and Olshanski, so I guess you studied representation theory originally, but your current works are linked with many fields: algebraic geometry, probability, and also physics, gauge theory, string theory, integrable systems. Why do you have such a wide range of knowledge?

At the University of Tokyo, which I graduated from many years ago, I studied physics only in the 1.5 years of the undergraduate course. I heard only basic things (including experiments, which I did not like at all). Later I learned physics when Witten's paper on Chern-Simons appeared, but not from physics colleagues, but from Tsuchiya (who worked on CFT) and also the notes of an Oxford seminar on Jones-Witten theory. Then I heard physicists talks on Seiberg-Witten theory after 1994. It is not systematic, hence I cannot give advice to younger readers on my path.

Okounkov: I studied mathematics at Moscow State University from 1989 to 1993, so missed the golden age of mathematics there and met many of its heroes only in the West. When I was a student, there were two very different layers to our education. The regular curriculum was, I think, very much oriented towards employment in the space or defense industries, with a lot of numerical methods, mechanics, and core physics. I enjoyed many aspects of it, and later really liked teaching numerical methods myself, but for me the real math was in the special courses and the seminars that were happening in the evenings. I believe the only regular course that contained Lie groups was M. Zelikin's course

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on optimal control. But in the Kirillov seminar (often led by Olshanski when Kirillov was away), the Gelfand seminar (led by Rudakov after Gelfand left), as well as in the courses by Beilinson and Feigin, representation theory was certainly very much at the center of things.

My Ph.D. project, inspired by the work of Olshanski, was on representation theory of the infinite symmetric group. Other projects we worked on with Olshanski while I was a graduate student could perhaps be described as classical and combinatorial representation theory inspired by the infinite-dimensional and asymptotic points of view (that may be traced to the Gelfand school, and to the ideas of Vershik and Kerov, respectively). So, by training, this is my mathematical home.

I learned very early from Olshanski, and I now repeat to my students, that there is a huge difference between learning a subject abstractly, *in libro*, and learning to work with it *in vivo*. At Moscow state, we had a physics course based on Landau and Lifshitz, and also courses in probability and stochastic processes, which were certainly very solid and rigorous courses, but... When, by a very lucky chance, I got into Dobrushin's laboratory, and saw people really do mathematical physics, it was completely different, and very intuitive in its unpolished form. I think I've been very fortunate to be able to learn a lot of things practically, while working on projects and papers, including learning so much directly from my collaborators.

Nakajima: I have heard of the "two layer" system in Moscow several times. It does not exist in Japan and other countries. Could you explain it to readers? Does it still exist now?

Okounkov: I am surprised to hear this, in Russia there has been a strong tradition of what we call "circles" in schools after regular classes, continuing with various "schools," etc. where university students and faculty teach high-school students in the evenings, and then on to special courses and seminars outside of the regular curriculum at the university. I don't know the history, but maybe this enthusiasm for teaching has its roots in the effort of the intelligentsia to educate the masses in the 19th century? It is charming to see these traditions now blossoming on foreign soil, e.g., in some parts of the US.

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I think I learned more through such channels than through the regular ones, and this is also how I met many dear friends, including my wife Inna, when we were both at one such school called EMSch for Economics + Mathematics + School. In fact, when we first met, I was a student and she was already a teacher. EMSch is still going very strong, and just had its 50th anniversary.

Happy Graduate Student Days with Family and Plenty of Free Time

Nakajima: You mentioned to me that you had plenty of time in your graduate course (and have two daughters). You also told me that you read textbooks in the Moscow subway. How was that possible? It is different from me, and probably many others. People study hard in graduate course in order to arrive at the cutting edge of the field. I was fortunate to get a permanent position after graduating from my master's course (that was popular in my day in Japan), but young people nowadays have only temporary postdoc positions for many years. They must study even harder than we did, I think.

Okounkov: Maybe my case is not so representative, because when I started graduate school in 1993, I already had a family, the country's economy was in total collapse, and traveling to the West was basically the only source of income for most Russian scientists. In my case, my wife Inna started a business and provided for our family, while I had plenty of time to think about math while looking for food to buy, cooking, and washing and ironing the home-made diapers, etc. We didn't have disposable diapers, nor a washing machine, and the iron was a combined wedding gift from all of our friends. In fact, on the day of my thesis defense I mishandled the boiling diapers, and so came to the defense with one arm bandaged. I don't recall people on the defense committee expressing any concern; nothing was out of the ordinary in those times. But all things considered, I think those were exceptionally happy years, as a family is certainly the greatest source of happiness in life, and the second largest source of happiness is to understand something new, of which there was also plenty.

Compared to the stress of being a junior faculty member on a short-term contract, I still think that graduate school is a local maximum of free time for a young researcher in mathematics and one should really make the best possible use of it. This means, of course, that one should study hard, but also take time to simply ponder or to consider examples, as well as to be curious about mathematics and science in general. It is true that many books that were formative for me I read on the subway rides to and from the University, and to this day I try to always have a book with me for the subway ride, also in NYC.

Collaboration with Rahul Pandharipande on Quantum Cohomology

Nakajima: Now I want to ask you about collaborations with Rahul (Rahul Pandharipande) on quantum cohomology. How did it start? You did know Hurwitz theory before, but Gromov-Witten invariants were new to you at that time. Did you have an outlook before you started the collaboration? I have several collaborations (Yoshioka, Goettsche, Braverman, Finkelberg, and others), but I needed several years of mutual understanding of respective work before we actually started collaboration. For you and Rahul, did you know each other's work well before you started? And how did it go after you started?

Okounkov: By another lucky chance, Rahul was my next-door office neighbor in Chicago, and I was coming to a very lively seminar that Fulton, Rahul, and others were running at the University of Chicago on quantum cohomology. Amusingly, in one talk by Rahul, Spencer Bloch pointed out that Bernoulli numbers are appearing in Faber-Pandharipande computations of Hodge integrals in exactly the same form as in Spencer's and my work [B. Spencer and A. Okounkov, The character of the infinite wedge representation, Adv. Math. 149 (2000), 1] on the character of the infinite wedge (with hindsight, this is the degree 0 term in the eventual theory of completed cycles of Rahul, Eskin, and I). So I had some familiarity with the subject, but of course no real technical knowledge.

And then, after moving to California, Rahul wrote a paper on various implications of the then conjectural Toda equations of Eguchi, Hori, and Yang for the Gromov-Witten theory of $\mathbf{P}^1$, including a conjecture about Hurwitz numbers. Hurwitz numbers, of course, I knew since first, this is just a different name for the characters of the symmetric groups, and so was something from the world that Olshanski and I spent a long time rethinking; and second, they were very much on my mind because at the time I had just proved the Baik-Deif-Johansson conjecture on increasing subsequences in random permutations precisely by a geometric argument with Hurwitz numbers. So, I thought it couldn't be such a hard conjecture to prove (which was indeed the case) and this was the first point of actual mathematical contact.

In the work on BDJ, I observed a connection with Kontsevich's combinatorial formula for Witten's intersection numbers on the moduli spaces of curves, a very fashionable topic at the time. One could see how through Hurwitz theory and the ELSV formula (which, at the time, was fresh from the oven) one could reach an independent and, all things considered, transparent proof of that combinatorial formula. I was very curious to learn more algebraic geometry, and I hope Rahul was equally happy to listen to my explanations of other relevant ingredients, so this was the start. From the beginning, it was clear that this project was going to work; sometimes collaborations just have such a lucky beginning. (Later, for example, when we proved those Toda equations, or in some of the GW=DT

(Gromov-Witten/Donaldson-Thomas correspondence) papers, we really had to try many different things before we found the right mix of ideas.) Now, I don't think anybody reads that first paper of ours, even though Rahul's part contains an excellent introduction to virtual classes and virtual localization, among other things. But this is okay, the subject has reached much higher heights since then.

Collaboration with Nikita Nekrasov

Nakajima: Next, collaboration with Nikita (Nikita A. Nekrasov). Nikita understand mathematics very well, but he is still a physicist. I do not know many examples of mathematicians, other than you, who have joint papers with physicists. Many mathematicians are interested in physics these days (and it is the reason why we have IPMU), but it is not that easy to overcome the communication barrier yet. In particular, I do not have many examples of collaboration between physicists and mathematicians. How do you communicate with Nikita? More concretely how did you get your result on instanton counting?

I have a personal interest, as I had a different proof of the same result with Yoshioka. In my case, we did not originally intend to give a proof. We had an unsuccessful joint project on instantons on blowup before, and we wanted to correct it by using instanton counting. Quite unexpectedly we were able to find a proof. Nevertheless, it took us some time to understand the meaning of Seiberg-Witten curves. It seems that you had a good understanding from the beginning.

Okounkov: I think mathematics and theoretical physics grow very differently. In math, we spend a lot of time rethinking our foundations and count it as progress when a phenomenon is presented in its most general form, with all essential details highlighted and accidental features removed. As a result, our subject is solid, not just in the sense of rigor, but also in the sense of our knowledge filling a certain volume, with a well-defined boundary, beyond which lies the unknown. In physics, it seems to me, it is very important to be the first to add some new key bit to the tip of the current research focus, a bit like a DLA (Diffusion-limited aggregation) growth, or a discussion in a social network, may my physics colleagues forgive the comparison. As we know, this grows faster, but also results in very fractal dendriform structures, in which, for a mathematician, it is very hard to trace the boundaries or to navigate the continuum of literature on any given popular topic. Very fortunately, there are people like Nikita, in whose head it is all ordered and all the voids are filled in. I never had any problems understanding him.

Sometime in the spring of 2002, I was in Paris and Nikita gave me a draft of his "Seiberg-Witten Prepotential from Instanton Counting" that contained the Nekrasov partition function Z, as we know it now. Among other things, Z is a sum over partitions, and I admit I like partitions and find their elementary geometry very comforting. Nikita knew that. To get to the SW prepotential, one has to take a certain limit in Z, and at first I was sure that it had to be something very subtle since, after all, we are talking some of the deepest structures known to man at that time. So I didn't give it much thought at first until Nikita came to visit Princeton the following winter. It is remarkable how much in science depends on social aspects, because humans are social animals and our brains are wired for interaction with others. Anyway, once the right neurons fired, it was obvious that the SW limit is just about the law of large numbers for partitions and the SW curve is just the associated limit shape (which is a Vershik-Kerov-style math that I knew really well), and it became a purely mathematical problem, as a physicist would say, to work out the details.

You see, a power series is just an integral over natural numbers $\mathbf{N}$, or over the reals $\mathbf{R}$ with respect to a measure μ supported on the natural numbers. Sometimes, the asymptotics of the series can be computed by the Laplace method, i.e. by looking at the largest terms, which means the law of large numbers for suitably rescaled μ. This works for much more general spaces, e.g., for the space of Lipschitz functions on $\mathbf{R}$ instead of $\mathbf{R}$ with the diagrams of partitions instead of $\mathbf{N}$. (Following Vershik and Kerov, Russians draw partitions with 45° axes, which saves space in the paper by the factor of $\sqrt{2}$ and also makes the boundary of a diagram a function with Lipschitz constant 1.) The largest term, that is, the limit shape, is determined by a certain variational problem, which in this instance is very elegantly solved by the SW curve. This is all really basic probability, except for the part in which one can actually solve the variational problem explicitly using algebraic geometry. With Rick Kenyon, we later developed some general theory about this. One of the simplest among these limit shapes, the limit shape for a uniformly random 3-dimensional partition, was at some serendipitous moment recognized as identical to the Hori-Vafa mirror of $\mathbf{C}^3$. That was the start of whole GW=DT story...

Relation between Quiver Varieties and Quantum Integrable Systems

Nakajima: You explained the relation between quiver varieties and quantum integrable systems in your lectures for students. They were very interesting, and I was impressed that you understood the works of Jimbo-Miwa and others very well. How did you learn quantum integrable systems?

Okounkov: I am very glad you liked it, even though I am surely still very much behind people like Jimbo and Miwa. I find our brains really repel some parts of math while very easily absorbing others. This depends maybe on some inborn qualities, but also very much on what you already know and understand. I always tell my students to go with what comes naturally... Maybe this is what Kirillov meant by his saying that "math can be only learned adiabatically." Anyway, I find the Jimbo-Miwa-Faddeev-Reshetikhin-... style math really easy to absorb, because, first, it is, fundamentally, representation theory with a mix of statistical mechanics, two subjects to which I can relate really well. But maybe more importantly, it both answers some fundamental enumerative geometry questions and also is illuminated in a new and, I think, simplifying light by these geometric considerations.

I've spent a very long time doing and analyzing various enumerative computations, which is really a very hard subject. It may be a fashionable topic to discuss, but to actually work out a solution to a modern enumerative question is a different matter. There may be a trivial case you can do from the definition, a couple more with some cleverness and tricks, perhaps a bunch more with the help of a computer, maybe a lucky guess, and then at some point some miracle needs to happen. So any time there is a framework to explain a certain totality of answers, your brain is already prepared to flesh it out with concrete data and features. I certainly felt that immediately after Nikita and Samson (Samson L. Shatashvili) had their vision for how curve counts in Nakajima varieties and related geometries should be tied with the quantum integrable systems. Things that I sort of understood from one side suddenly shone in a new light from the other—a great feeling.

Andrei Asks Hiraku Questions

Okounkov: Now, let me ask you some questions. As someone who has studied Nakajima quiver varieties for many years, I am naturally very curious about their origin and early history. How did it happen?

Nakajima: I was impressed by Mukai's work on moduli spaces of holomorphic vector bundles on K3 surfaces in 1988, and started to study similar problems for ALE spaces, which are noncompact version of K3 surfaces. Then I collaborated with Kronheimer to give the ADHM description (or quiver description) of holomorphic vector bundles, or instantons on ALE spaces. This happened in summer of 1989 at Berkeley. Kronheimer changed his interests to applications of the gauge theory to topology, but I continued to study these particular moduli spaces. In 1990 at ICM (International Congress of Mathematicians) Kyoto, I heard Lusztig's plenary talk and knew that quivers appeared in his works. I started to study his works, but it was hard as they were very far away from my background at that time. Meanwhile, I found that Slodowy slices appear as moduli spaces, and knew that Hotta-Springer (and also Hotta-Shimomura) computed their Betti numbers in the context of Springer representations. In 1991, I found that their computation in top degrees gave weight multiplicities of irreducible representations in type A, and it gave a link to Lusztig's works. This observation occurred when I read a paper by Kashiwara-Nakashima, and I still remember that I was very excited at that time. Thus I understood how moduli spaces (called quiver varieties after that) were related to representation theory, and study went on smoothly afterwards.

Okounkov: You worked in both differential and algebraic geometry; which one did you enjoy more? How do these two different kinds of geometry compare for you?

Nakajima: I studied differential geometry, especially nonlinear PDE on manifolds, when I was a student. Since Kobayashi-Hitchin correspondence was one of the hot topics in the area at that time, I had seminars with algebraic geometers. I was also interested in Kaehler-Einstein metrics on Fano manifolds. Since these problems were related to geometric invariant theory, I gradually learned it. On the other hand, the minimal model program was the central topic for most Japanese algebraic geometers. It looked difficult to me, and I classified myself as a differential geometer at that time.

After I had analyzed quiver varieties for several years, I needed algebraic geometry more and more. For example, I wrote differential geometric aspects in the quiver variety paper written in 1994, but not in the paper in 1998. This shows a shift in my interest. Finally, I found smooth quiver varieties were best understood as moduli spaces of sheaves and Hilbert schemes of points, rather than moduli of holomorphic vector bundles and instantons. This was the time when I stopped my interest in differential geometry.

Nevertheless, I feel that

my differential geometric background is useful when I read physics papers. I like joint works with Yoshioka on instanton counting on blow-up. He is an actual algebraic geometer and very strong in moduli theory. Thus I concentrated on looking for relevant physics literature, and found the paper on the RG equation.

Okounkov: You are a frequent visitor to Moscow now, but what things surprised you the most at first? Do you see many similarities and differences between mathematics in Moscow and in Japan?

Nakajima: When I was a student, we did not have many chances to hear talks by foreign mathematicians. Since Japanese professors could cover limited areas in mathematics, we learned many things from written texts and papers. We were encouraged to read many papers in detail. There were also many expository talks where new preprints sent from abroad (by ordinary mail) were introduced. The situation might be different in other fields, where more Japanese mathematicians were working, like number theory and algebraic geometry. But I got my basic knowledge through papers, rather than direct communications from professors when I was young. When I met many people who learn many things from talks in US and other places, I was surprised.

Feigin visited Kyoto every summer starting around 1990. Opportunities to hear talks by foreign people had already drastically increased at that time (partly because of ICM 90), but his talks were very different from anyone else's. He usually started with easy examples, and gave some computation, but suddenly said something very mysterious but interesting towards the end of lectures. They were very hard to follow as I was not used to hear un-organized talks like his. Also it was impossible for me to understand from where he got his ideas. His thinking looked very mysterious.

I had an idea for a long time that all Russian mathematicians gave talks like him, and Russian students were accustomed to learn things from such talks. I met other Russian mathematicians, and gradually understood that Feigin is unique even among Russians, and most people are not so different from us. Therefore, when I first visited Moscow in 2013, I was not surprised at all. My first encounter with Feigin was a much bigger surprise.

Okounkov: Okay, this is my last question. In Japan, many things are very carefully preserved, while many other things are very dynamic. What is your sense of the balance of tradition and innovation between the generations in Japanese mathematics?

Nakajima: Last year Takeuchi published a text book on D-modules (in Japanese), and he wrote that he regrets that D-modules theory did not become popular in Japan despite the fact that it was born in this country. Also as you observed, quantum integrable systems are not popular in Kyoto any more since Miwa retired. (It is partly because researchers in integrable systems spread outside Kyoto.) And we do not have classes teaching integrable systems. A similar thing happened on algebraic topology, where it was popular in Kyoto at some period, but only a very few people remain now. On the other hand, algebraic geometry, number theory, probability, and many other areas are taught in regular classes. Their research groups keep the same size, or even grow. There was no symplectic geometry when I was a student. But we have a strong group in Tokyo and Kyoto.

As far as I understand, these changes and preservations did not happen by plan. The number of faculty member is fixed (in fact, decreasing recently), and we need to hire good researchers in newly born fields, like symplectic geometry, as we cannot keep up the level of research otherwise. Hence some fields shrink in turn. Another factor is the availability of textbooks. There are many good Japanese textbooks from the undergraduate level to advanced ones in algebra. We have a few good books on integrable systems, but certainly not enough. It is difficult for students to learn integrable systems.

Since I have successfully changed my fields in my career, I enjoy discussion with people with a different background. So I like the dynamic changes of my surroundings. On the other hand, I understand that I should write textbooks for future generations, but it is not easy for various reasons. I promised to write three books, but I cannot finish them many years...

Okay, I really enjoyed talking with you. Thank you.

Okounkov: Thank you.