Interview with Srinivasa Varadhan

Martin Raussen and Christian Skau

S. R. S. Varadhan is the recipient of the 2007 Abel Prize of the Norwegian Academy of Science and Letters. On May 21, 2007, prior to the Abel Prize celebration in Oslo, Varadhan was interviewed by Martin Raussen of Aalborg University and Christian Skau of the Norwegian University of Science and Technology. This interview originally appeared in the September 2007 issue of the *European Mathematical Society Newsletter.*

—Andy Magid

R & S: *Professor Varadhan, first of all we would like to congratulate you for having been awarded the Abel Prize this year.*

By extension, our congratulations go to the field of probability and statistics since you are the first recipient from this area. Incidentally, last year at the International Congress of Mathematicians in Madrid, Fields Medals were given to mathematicians with expertise in this area for the first time, as well.

How come it took so long time before probability and statistics were recognized so prestigiously, at the International Congress of Mathematicians last year and with the Abel Prize this year? Is it pure coincidence that this happens two years in a row? Could you add some comments on the development of the relations between probability and statistics on the one hand and the rest of mathematics on the other hand?

Varadhan: Probability became a branch of mathematics very recently in the 1930s after Kolmogorov wrote his book. Until then it was not really considered as a proper branch of mathematics. In that sense it has taken some time for the mathematical community to feel comfortable with probability the way they are comfortable with number theory and geometry. Perhaps that is one of the reasons why it took a lot of time.

In recent years probability has been used in many areas. Mathematical finance for example uses a lot of probability. These days, probability has a lot of exposure, and connections with other branches of mathematics have come up. The most recent example has to do with conformal invariance for which the Fields Medal was given last year. These connections have brought probability to the attention of the mathematics community, and the awards are perhaps a reflection of that.

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Career

R & S: *The next question is about your career. You were born in Chennai, the capital of Tamil Nadu, on the Southeast coast of India, in 1940. You went to school there and then to the Presidency College at Madras University. We would like to ask you about these formative years: What was the first reason for your interest in mathematical problems? Did that happen under the influence of your father, who was a teacher of mathematics? Were there other people, or were there specific problems that made you first interested in mathematics?*

Varadhan: My father was in fact a teacher of science, not so much mathematics. In my early school days I was good in mathematics, which just meant that I could add, subtract, and multiply without making mistakes. Anyway I had no difficulty with mathematics. At high school I had an excellent mathematics teacher who asked some of his better students to come to his house during weekends, Saturday or Sunday, and gave them extra problems to work on. We thought of these problems just as intellectual games that we played, it was not like an exam; it was more for enjoyment. That gave me the idea that mathematics is something that you can enjoy doing like playing chess or solving puzzles. That attitude made mathematics a much more friendly subject, not something to be afraid of, and that played a role in why I got interested in mathematics. After that I went to college for five years. I had excellent teachers throughout. By the time I graduated with a master's degree in statistics, I had three years of solid grounding in pure mathematics. My background was strong when I graduated from college.

R & S: Was there a specific reason that you graduated in statistics rather than in other branches of mathematics?

Varadhan: The option at that time was either to go into mathematics or into statistics. There was not that much difference between these two. If you went into mathematics, you studied pure and applied mathematics; if you went into statistics, you studied pure mathematics and statistics. You replaced applied mathematics with statistics; that was the only difference between the two programs. Looking back, part of the reason for going into statistics rather than mathematics was the perception that if you went into statistics your job opportunities were better; you could be employed in industry and so on. If you went into mathematics, you would end up as a school teacher. There was that perception; I do not know how real it was.

R & S: With your degree in statistics it seemed quite natural that you continued at the Indian Statistical Institute at Kolkata. There you found yourself quite soon in a group of bright students that, seemingly without too much influence from their teachers, started to study new areas of fundamental mathematics and then applied those to problems coming from probability theory—with a lot of success. You were able to extend certain limit theorems for stochastic processes to higher dimensional spaces; problems that other mathematicians from outside India had been working on for several years without so much success. Could you tell us a bit about this development and whom you collaborated with?

Varadhan: The Indian system at that time was very like much the British system: If you decided to study for a doctoral degree, there were no courses; you were supposed to do research and to produce a thesis. You could ask your advisor questions and he would answer you, but there was no formal guidance as is the case in the United States for example. When I went there I had the idea that I would be looking for a job within some industry. I was told that I should work on statistical quality control, so I spent close to 6 or 8 months studying statistical quality control; in the end, that left me totally unsatisfied.

Then I met Varadarajan, Parthasarathy, and Ranga Rao, who worked in probability from a totally mathematical point of view. They convinced me that I was not spending my time usefully, and that I better learn some mathematics if I wanted to do anything at all. I got interested, and I think in the second year I was there, we said to ourselves: Let us work on a problem. We picked a problem concerning probability distributions on groups. That got us started; we eventually solved the problem and in the process also learned the tools that were needed for it.

It was a lot of fun: the three of us constantly exchanged ideas starting at seven o'clock in the morning. We were all bachelors, living in the same dormitory. The work day lasted from 7 a.m. to 9 p.m.; it was a terrific time to learn. In fact, the second paper we wrote had Abel in its title, because it has something to do with locally compact abelian groups.

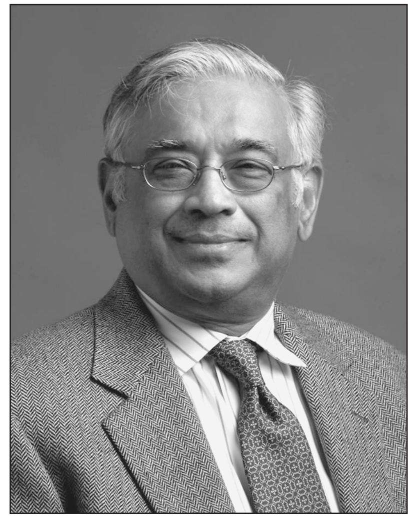

Photo: Anne Lise Flavik. Courtesy of the Norwegian Academy of Science and Letters.

Srinivasa S. R. Varadhan, winner of the 2007 Abel Prize.

R & S: From what you tell us, it seems that your work can serve as an example for the fact that the combination of motivations and insights from real world problems on the one hand and of fundamental abstract mathematical tools on the other hand has shown to be extremely fruitful. This brings us to a question about the distinction between pure and applied mathematics that some people focus on. Is it meaningful at all—in your own case and maybe in general?

Varadhan: I think that distinction, in my case at least, is not really present. I usually look at mathematics in the following way: There is a specific problem that needs to be solved. The problem is a mathematical problem, but the origin of the problem could be physics, statistics, or could be another application, an economic application perhaps. But the model is there, and it is clear what mathematical problem you have to solve. But of course, if the problem came from physics or some application, there is an intuition that helps you to reason what the possible answer could be. The challenge is how to translate this intuition into rigorous mathematics. That requires tools, and sometimes the tools may not be around and you may have to invent these tools, and that is where the challenge and the excitement of doing mathematics is, as far as I am concerned. That is the reason why I have been doing it.

India and the Third World

R & S: May we come back to your Indian background? You are the first Abel Prize recipient with an education from a Third World country. In 1963 you left Kolkata and went to the Courant Institute of Mathematical Sciences in New York, where you still are working. We wonder whether you still strongly feel your Indian background—in mathematics, in training, your life style, your religion, and philosophy?

Varadhan: For twenty-three years, I grew up in India, and I think that part of your life always stays with you. I am still very much an Indian in the way I live. I prefer Indian food to anything else, and I have some religious feelings about Hinduism and I am a practising Hindu. So my religious beliefs are based on my real life, and my lifestyle is very much Indian. But when you are living in the United States you learn to adjust a little bit, you perhaps have a combination of the two that you are comfortable with.

My training in India has been mainly in classical analysis. No matter what you do, even if you do the most abstract mathematics, you use it as a tool. At crucial points, I think you need to go back to your classical roots and do some tough estimates here and there; I think the classical training definitely helps there. The abstract mathematical tools then help you to put some results in perspective. You can see what the larger impact of what you have done is. To assess that, modern training gives you some help.

R & S: *The best known Indian mathematician of the past, at least here in the West, is certainly Srinivasa Ramanujan. He is known both for his very untraditional methods and results, and his notebooks are still studied by a lot of mathematicians around the world. He is certainly also known for his tragic fate and his untimely death. Has he played a specific role in your life as a role model? Is that still true for many Indian mathematicians?*

Varadhan: I think the name of Ramanujan has been familiar to most Indians today. Maybe, when I was growing up, it was more familiar to people from the South than from the North, because he came from the southern part of India, but we definitely knew of him as a great mathematician. At that age, I did not really know the details of his work. Even now, I have only a vague idea of what it is about. People still do not seem to know how exactly he arrived at those results. He seemed to have a mental process that led him to these things, which he has not fully explained in his work. In spite of the years Ramanujan spent with Hardy, the West was not able to penetrate the barrier and understand how his mind worked. I do not think we can do anything about it now.

Mathematical Institutions

R & S: *You spent the last years of your life in India at the Indian Statistical Institute (ISI) at Kolkata. There is another well-known research institute in India, the Tata Institute. We know that there has been some competition between these two institutions although they specialize in different fields. Can you comment on this competition, the ongoing relations between the two institutes and their respective strengths?*

Varadhan: I do not know when the competition started. The Indian Statistical Institute was founded by Mahalanobis in 1931; the Tata Institute was founded by Bhabha in 1945. They were both great friends of Jawaharlal Nehru, the prime minister at the time, and he encouraged them both. Maybe there are some rivalries at that level, the institutional level. The mathematics division of the Indian Statistical Institute had Dr. C. R. Rao, who was my advisor, as its scientific director, and the mathematics division of the Tata Institute was headed by Dr. Chandrasekharan; he was the moving force behind the mathematics school of Tata Institute. Maybe there is some competition there.

I know many of the faculty of the Tata Institute; in fact many of them were from the same region in the South and they went to the same university, the same college, perhaps even to the same high school. So the relationships between the two faculties have always been friendly.

It is true, the emphasis is very different. At Tata, they have concentrated on number theory and algebraic geometry and certain parts of abstract mathematics. The Indian Statistical Institute on the other hand has concentrated more on probability and statistics. Although there has been some overlap, it is really not that much.

R & S: *We have heard that you still entertain close relations to India and to Chennai and its mathematical institute, in particular. And in general, you are interested in the academic development of Third World countries, in particular through the Third World Academy of Sciences. Please tell us about your connections and your activities there?*

Varadhan: I go to Chennai maybe once a year now. Earlier it used to be twice a year, when my parents were alive. I used to go and spend a month or two in Chennai, and I visited the two mathematical institutions in Chennai: there is the Chennai Mathematics Institute, and there is also the Institute of Mathematical Sciences in Chennai. I have visited both of them at different times. I have close contacts with their leadership and their faculty.

In earlier times, I visited the Bangalore Centre of the Tata Institute: The Tata Institute in Mumbai has a Centre for Applicable Mathematics in Bangalore. I spent some time visiting them, and we have had students from there coming to the Courant Institute to take their degrees and so on. To the extent possible, I try to go back and keep in touch. Nowadays, with email, they can ask me for advice, and I try to help out as much I can. The next couple of years, I have some plans to spend part of my sabbatical in Chennai lecturing at Chennai Mathematics Institute.

R & S: *You are already the second Abel Prize winner working at the Courant Institute of Mathematical Sciences in New York, after Peter Lax. At least in the world of applied mathematics, the Courant Institute seems to play a very special role. Could you explain how this worked out? What makes the Courant Institute such a special place?*

Varadhan: We are back to the 1930s, when the Courant Institute was started. There was no applied mathematics in the United States. Richard

Courant came to the U.S. and he started this mathematics institute with the emphasis on applied mathematics. His view of applied mathematics was broad enough so that it included pure mathematics. I mean, he did not see the distinction between pure and applied mathematics. He needed to apply mathematics, so he developed the tools; he needed to do it. And from that point of view, I think analysis played an important role.

The Courant Institute has always been very strong in applied mathematics and analysis. And in the 1960s, there was a grant from the Sloan Foundation to develop probability and statistics at the Courant Institute. They started it, and probability was successful, I think. Statistics did not quite work out, so we still do not have really much statistics at the Courant Institute. We have a lot of probability, analysis, and applied mathematics, and in recent years some differential geometry as well. In these areas we are very strong.

The Courant Institute has always been successful in hiring the best faculty. The emphasis has mostly been on analysis and applied mathematics. Perhaps that reflects why we have had two Abel Prize winners out of the first five.

Mathematical Research: Process and Results

R & S: *You have given deep and seminal contributions to the area of mathematics which is called probability theory. What attracted you to probability theory in the first place?*

Varadhan: When I joined my undergraduate program in statistics, probability was part of statistics; so you had to learn some probability. I realised that I had some intuition for probability in the sense that I could sense what one was trying to do, more than just calculating some number. I cannot explain it, I just had some feeling for it. That helped a lot; that motivated me to go deeper into it.

R & S: *Modern probability theory, as you mentioned earlier, started with Kolmogorov in the 1930s. You had an interesting encounter with Kolmogorov: He wrote from Moscow about your doctoral thesis at the Indian Statistical Institute, which you finished when you were twenty-two years old: "This is not the work of a student, but of a mature master." Did you ever meet Kolmogorov? Did you have any interaction with him mathematically later?*

Varadhan: Yes, I have met him; he came to India in 1962. I had just submitted my thesis, and he was one of the examiners of the thesis, but he was going to take the thesis back to Moscow and then to write a report; so the report was not coming at that time. He spent a month in India, and some of us graduate students spent most of our time travelling with him all over India. There was a period where we would meet him every day. There were some reports about it mentioned in the Indian press recently, which were not quite accurate.

But there is one incident that I remember very well. I was giving a lecture on my thesis work with Kolmogorov in the audience. The lecture was supposed to last for an hour, but in my enthusiasm it lasted an hour and a half. He was not protesting, but some members in the audience were getting restless. When the lecture ended, he got up to make some comments and people started leaving the lecture hall before he could say something, and he got very angry. He threw the chalk down with great force and stormed out of the room. My immediate reaction was: There goes my Ph.D.! A group of students ran after him to where he was staying, and I apologized for taking too much time. He said: No no; in Russia, our seminars last three hours. I am not angry at you, but those people in the audience, when Kolmogorov stands up to speak, they should wait and listen.

R & S: *That is a nice story!*

Among your many research contributions, the one which is associated with so-called large deviations must rank as one of the most important. Can you tell us first what large deviations are and why the study of these is so important; and what are the applications?

Varadhan: The subject of large deviations goes back to the early 1930s. It in fact started in Scandinavia, with actuaries working for the insurance industry. The pioneer who started that subject was named Esscher[1]. He was interested in a situation where too many claims could be made against the insurance company, he was worried about the total claim amount exceeding the reserve fund set aside for paying these claims, and he wanted to calculate the probability of this. Those days the standard model was that each individual claim is a random variable, you assume some distribution for it, and the total claim is then the sum of a large number of independent random variables. And what you are really interested in is the probability that the sum of a large number of independent random variables exceeds a certain amount. You are interested in estimating the tail probabilities of sums of independent random variables.

People knew the central limit theorem at the time, which tells you that the distribution of sums of independent random variables has a Gaussian approximation. If you do the Gaussian approximation, the answer you get is not correct. It is not correct in the sense that the Gaussian approximation is still valid, but the error is measured in terms of difference. Both these numbers are very small, therefore the difference between them is small, so the central limit theorem is valid. But you are interested in how small it is; you are interested in

[1] *F. Esscher, On the probability function in the collective theory of risk,* Skandinavisk Actuarietidskrift **15** *(1932), 175–195.*

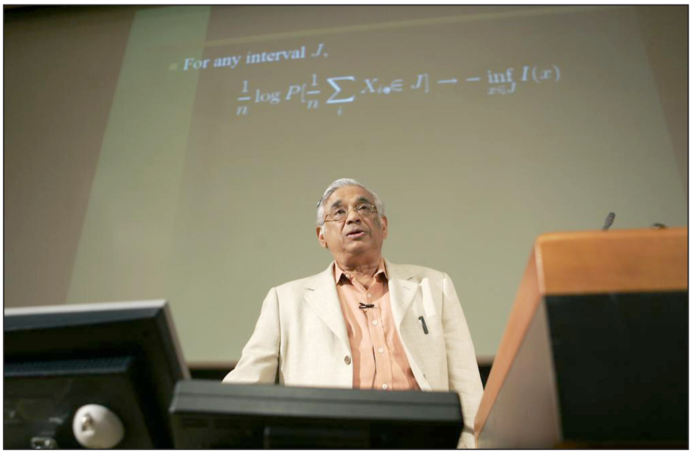

Photo: Terje Bendiksby/Scanpix. Courtesy of the Norwegian Academy of Science and Letters.

Varadhan at the University of Oslo where he gave his Abel lecture.

the ratio of these two things, not just the difference of these small numbers.

The idea is: how do you shift your focus so that you can look at the ratio rather than just at the difference? Esscher came up with this idea that is called Esscher's tilt; it is a little technical. It is a way of changing the measure that you use in a very special manner. And from this point of view, what was originally a tail event, now becomes a central event. So you can estimate it much more accurately and then go from this estimate to what you want, usually by a factor which is much more manageable. This way of estimation is very successful in calculating the exact asymptotics of these tail probabilities. That is the origin of large deviations. What you are really interested in is estimating the probabilities of certain events. It does not matter how they occur; they arise in some way. These are events with very small probability, but you would like to have some idea of how small it is. You would like to measure it in logarithmic scale, "*e* to the minus how big". That is the sense in which it is used and formulated these days.

R & S: *Large deviations have lots of applications, not the least in finance; is that correct?*

Varadhan: I think in finance or other areas, what the theory actually tells you is not just what the probability is, but it also tells you if an event with such a small probability occurred, how it occurred. You can trace back the history of it and explain how it occurred and what else would have occurred. So you are concerned with analysing entire circumstances. In Esscher's method, there is the tilt that produced it; then that tilt could have produced other things, too; they would all happen if this event happened. It gives you more information than just how small the probability is. This has become useful in mathematical finance because you write an option which means: if something happens at a certain time, then you promise to pay somebody something. But what you pay may depend on not just what happened at that time, it may depend on the history. So you would like to know if something happened at this time, what was the history that produced it? Large deviation theory is able to predict this.

R & S: *Together with Donsker you reduced the general large deviation principle to a powerful variational principle. Specifically, you introduced the so-called Donsker-Varadhan rate function and studied its behaviour. Could you elaborate a little how you proceeded, and what type of rate functions you could handle and analyse?*

Varadhan: If you go back to the Esscher theory of large deviations for sums of random variables, that requires the calculation of the moment-generating function. Since they are independent random variables, the moment-generating functions are products of the individual ones; if they are all the same, you get just the *n*-th power of one moment-generating function. What really controls the large deviation is the logarithm of the moment-generating function. The logarithm of the *n*-th power is just a multiple of the logarithm of the original moment-generating function, which now controls your large deviation. On the other hand, if your random variables are not independent, but dependent like in a Markov chain or something like that, then there is no longer just one moment-generating function. It is important to know how the moment-generating function of the sum grows; it does not grow like a product but it grows in some way. This is related by the Feynman-Kac formula to the principal eigenvalue of the generator of the Markov process involved. There is a connection between the rate function and the so-called principal eigenvalue. This is what our theory used considerably. The rate function is constructed as the Legendre transform or the convex conjugate of the logarithm of the principal eigenvalue.

R & S: *Before we leave the subject of the large deviation principle, could you please comment on the so-called Varadhan integral lemma which is used in many areas. Why is that?*

Varadhan: I do not think Varadhan's lemma is used that much, probably large deviation theory is used more. The reason why I called it a lemma is that I did not want to call it a theorem. It is really a very simple thing that tells you that if probabilities behave in a certain way, then certain integrals behave in a certain way. The proof just requires approximating the integral by a sum and doing the most elementary estimate. What is important there is just a point of view and not so much the actual estimates in the work involved; this is quite minimal.

R & S: *But it pops up apparently in many different areas. Is that correct?*

Varadhan: The basic idea in this is very simple: if you take two positive numbers *a* and *b* and raise them to a very high power and you look at the sum, the sum is just like the power of the larger one;

the smaller one is insignificant, you can replace the logarithm of the sum by just a maximum. The logarithm of the sum of the exponential behaves just like the maximum. That is the idea when you have just a finite number of exponentials, then in some sense integrating is not different from summation if you have the right estimates. That was how I looked at it, and I think this arises in many different contexts. One can use the idea in many different places, but the idea itself is not very complicated.

***R & S:** That is often the case with important results in mathematics. They go back to a simple idea, but to come up with that idea, that is essential!*

You realized that Mark Kac's old formula for the first eigenvalue of the Schrödinger operator can be interpreted in terms of large deviations of a certain Brownian motion. Could you tell us how you came to this realization?

Varadhan: It was in 1973, I just came back from India after a sabbatical, and I was in Donsker's office. We always spent a lot of time talking about various problems. He wanted to look at the largest eigenvalue which controls the asymptotic behaviour of a Kac integral: I think people knew at that time that if you take the logarithm of the expectation of a Kac type exponential function, its asymptotic growth rate is the first eigenvalue. The first eigenvalue is given by a variational formula; that is classical. We knew that if we do large deviations and calculate asymptotically the integrals, you get a variational formula, too. So, he wanted to know if the two variational formulas have anything to do with each other: Is there a large deviation interpretation for this variational formula?

I was visiting Duke University, I remember, some time later that fall, and I was waiting in the library at Duke University for my talk which was to start in half an hour or so. Then it suddenly occurred to me what the solution to this problem was. It is very simple. In the Rayleigh-Ritz variational formula, there are two objects that compete. One is the integral of the potential multiplied by the square of a function; the other one is the Dirichlet form of the function. If you replace the square of the function and call it a new function, then the Dirichlet form becomes the Dirichlet form of the square root of that function. It is as simple as that. And then the large deviation rate function is nothing but the Dirichlet form of the square root of the density. Once you interpret it that way, it is clear what the formula is; and once you know what the formula is, it is not that difficult to prove it.

***R & S:** This brings us naturally to the next question: If you occasionally had a sudden flash of insight, where you in an instant saw the solution to a problem that you had struggled with, as the one you described right now: Do these flashes depend on hard and sustained preparatory thinking about the problem in question?*

Varadhan: Yes, they do: What happens is, once you have a problem you want to solve, you have some idea of how to approach it. You try to work it out, and if you can solve it the way you thought you could, it is done, and it is not interesting. You have done it, but it does not give you a thrill at all. On the other hand, if it is a problem in which everything falls into place, except for one thing you cannot do; if only you could do that one thing, then you would have the whole building, but this foundation is missing. So you struggle and struggle with it, sometimes for months, sometimes for years and sometimes for a lifetime! And eventually, suddenly one day you see how to fix that small piece. And then the whole structure is complete. That was the missing piece. Then that is a real revelation, and you enjoy a satisfaction which you cannot describe.

***R & S:** How long does the euphoria last when you have this experience?*

Varadhan: It lasts until you write it up and submit it for publication. Then you go on to the next problem!

***R & S:** Your cooperation with Daniel Stroock on the theory of diffusions led to several landmark papers. The semigroup approach by Kolmogorov and Feller had serious restrictions, we understand, and Paul Levy suggested that a diffusion process should be represented as a stochastic differential equation. Itô also had some very important contributions. Could you explain how you and Stroock managed to turn this into a martingale problem?*

Varadhan: I have to step back a little bit: Mark Kac used to be at Rockefeller University. Between New York University and Rockefeller University, we used to have a joint seminar; we would meet one week here and one week there and we would drive back and forth. I remember once going to Rockefeller University for a seminar and then coming back in a taxi to NYU. Somebody mentioned a result of Ciesielski, a Polish probabilist who was visiting Marc Kac at that time: You can look at the fundamental solution of a heat equation, for the whole space, and look at the fundamental solution with Dirichlet boundary data in a region. The fundamental solution for the Dirichlet boundary data is smaller, by the maximum principle, than the other one. If you look at the ratio of the two fundamental solutions, then it is always less than or equal to one. The question is: As t, the time variable in the fundamental solution, goes to zero, when does this ratio go to 1 for all points x and y in the region? The answer turns out to be: if and only if the region is convex! Of course, there are some technical aspects, about sets of capacity zero and so on. Intuitively, the reason it is happening is that the Brownian path, if it goes from x to y, in time t, as time t goes to zero, it would have to go in a straight line. Because its mean value remains the same as that of the Brownian bridge, which is always linear, and thus a line connecting the two points. The variance goes to zero, if you do not give it much time. That means it follows a straight line. That suggests that, if your space were not flat but curved, then it should probably go along the geodesics. One would expect therefore that the fundamental solution of the heat equation with variable coefficients should look like e to the minus the square of the geodesic distance divided by $2t$; just like the heat equation does with the Euclidean distance.

This occurred to me on the taxi ride back. That became the paper on the behaviour of the fundamental solution for small time. In fact, I think that was the paper that the PDE people at Courant liked, and that gave me a job. At that time, I was still a postdoc.

Anyway, at that point, the actual proof of it used only certain martingale properties of this process. It did not really use so much PDE, it just used certain martingales. Stroock was a graduate student at Rockefeller University at that time; we used to talk a lot. I remember, that spring, before he finished, we would discuss it. We thought: If it is true that we could prove this by just the martingale properties, then those martingale properties perhaps are enough to define it. Then we looked at it and asked ourselves: Can you define all diffusion processes by just martingale properties?

It looked like it unified different points of view: Kolmogorov and Feller through the PDE have one point of view, stochastic differential people have another point of view, semigroup theory has still another point of view. But the martingale point of view unifies them. It is clear that it is much more useful; and it turned out, after investigation, that the martingale formulation is sort of the weakest formulation one can have; that is why everything implies it. Being the weakest formulation, it became clear that the hardest thing would be to prove uniqueness.

Then we were able to show that whenever any of the other methods work, you could prove uniqueness for this. We wanted to extend it and prove uniqueness for a class which had not been done before, and that eluded us for nearly one and a half years until one day the idea came, and we saw how to do it and everything fell into place.

R & S: *That was another flash of inspiration?*

Varadhan: That was another flash; that meant that we could do a lot of things for the next four to five years that kept us busy.

R & S: *Before we leave your mathematical research, we would like to ask you about your contribution to the theory of hydrodynamic limits, that is, describing the macroscopic behaviour of very large systems of interacting particles. Your work in this area has been described as viewing the environment from the travelling particle. Could you describe what this means?*

Varadhan: I will try to explain it. The subject of hydrodynamic scaling as it is called, or hydrodynamic limits, is a subject that did not really start in probability. It started from classical mechanics, Hamiltonian equations, and it is the problem of deriving Euler equations of fluid flow directly as a consequence of Hamiltonian motion. After all, we can think of a fluid as a lot of individual particles and the particles interact, ignoring quantum effects, according to Newtonian rules. We should be able to describe how every particle should move. But this requires solving a 10^{68}-dimensional ODE, and only then you are in good shape. Instead we replace this huge system of ODEs by PDEs, a small system of nonlinear PDEs, and these nonlinear PDEs describe the motion of conserved quantities.

If there are no conserved quantities, then things reach equilibrium very fast, and nothing really moves. But if there are conserved quantities, then they change very slowly locally, and so you have to speed up time to a different scale. Then you can observe change of these things. Mass is conserved, that means density is one of the variables; momenta are conserved, so fluid velocity is one of the variables; the energy is conserved, so temperature becomes one of the variables. For these conserved quantities, you obtain PDEs. When you solve your partial differential equations, you get a solution that is supposed to describe the macroscopic properties of particles in that location. And given these parameters, there is a unique equilibrium for these fixed values of the parameters which are the average values.

In a Hamiltonian scheme, that would be a fixed surface with prescribed energy and momentum, etc. On that surface the motion is supposed to be ergodic, so that there is a single invariant measure. This invariant measure describes how locally the particles are behaving over time. That is only described in statistical terms; you cannot really pin down which particle is where; and even if you could, you do not really care.

This program, although it seems reasonable in a physical sense, has not been carried out in a mathematical sense. The closest thing that one has come to is the result by Oscar Lanford who has shown that, for a very small time scale, you can start from the Hamiltonian system and derive the Boltzmann equations. Then to go from Boltzmann to Euler requires certain scales to be large; it is not clear if the earlier results work in this regime. The mathematical level of these things is not where it should be.

On the other hand, if you put a little noise in your system, so that you look at not a deterministic Hamiltonian set of equations, but stochastic differential equations, with particles that move and jump randomly, then life becomes much easier. The problem is the ergodic theory. The ergodic theory of dynamical systems is very hard. But the ergodic theory of Markov processes is a lot easier. With a little bit of noise, it is much easier to keep these things in equilibrium. And then you can go through this program and actually prove mathematical results.

Now coming to the history: We were at a conference in Marseille at Luminy, which is the Oberwolfach of the French Mathematical Society. My colleague George Papanicolaou (who I think should be here in Oslo later today) and I were taking a walk down to the calanques. And on the way back, he was describing this problem. He was interested in interacting particles, Brownian motion interacting under some potential. He wanted to prove the hydrodynamic scaling limit. I thought the solution should be easy; it seemed natural somehow. When I came back and looked at it, I got stuck regardless how much I tried. There were two critical steps, I figured out, that needed to be done; one step I could do, the second step I could not do. For the time being, I just left it at that. Then, a year later, we had a visitor at the Courant Institute, Josef Fritz from Hungary. He gave a talk on hydrodynamic limits; he had a slightly different model. By using different techniques, he could prove the theorem for that model. Then I realized that the second step on which I got stuck in the original model, I could do it easily in this model. So we wrote a paper with George Papanicolaou and one of his students Guo; that was my first paper on hydrodynamic limits. This work was more for a field than for an actual particle system which was what got me interested in the subject.

When you look at particles, you can ask two different questions. You can ask what is happening to the whole system of particles, you do not identify them; you just think of it as a cloud of particles. Then you can develop how the density of particles changes over time. But it does not tell you which particle moves where. Imagine particles have two different colours. Now you have two different densities, one for each colour. You have the equation of motion for the sum of the two densities, but you do not have an equation of motion for each one separately. Because to do each one separately, you would have to tag the particles and to keep track of them! It becomes important to keep track of the motion of a single particle in the sea of particles.

A way to analyse it that I found useful was to make the particle that you want to tag the centre of the universe. You change your coordinate scheme along with that particle. Then this particle does not move at all; it stays where it is, and the entire universe revolves around it. So you have a Markov process in the space of universes. This is of course an infinite dimensional Markov process, but if you can analyse it and prove ergodic theorems for it, then you can translate back and see how the tagged particle would move; because in some sense how much the universe moves around it or it moves around the universe is sort of the same thing. I found this method to be very useful, and this is the system looked at from the point of view of the moving particle.

Photo: Heiko Junge/Scanpix. Courtesy of the Norwegian Academy of Science and Letters.

Three Abel laureates at the Abel monument: (l. to r.) Lennart Carleson, Srinivasa Varadhan, and Peter Lax.

Work Style

R & S: *Very interesting! A different question: Can you describe your work style? Do you think in geometric pictures or rather in formulas? Or is there an analytic way of thinking?*

Varadhan: I like to think physically in some sense. I like to build my intuition as a physicist would do: What is really happening, understanding the mechanism which produces it, and then I try to translate it into analysis. I do not like to think formally, starting with an equation and manipulating and then see what happens. That is the way I like to work: I let my intuition guide me to the type of analysis that needs to be done.

R & S: *Your work in mathematics has been described by a fellow mathematician of yours as "Like a Bach fugue, precise yet beautiful." Can you describe the feeling of beauty and aesthetics that you experience when you do mathematics?*

Varadhan: I think the quotation you are referring to can be traced back to the review of my book with Stroock by David Williams. I think mathematics is a beautiful subject because it explains complicated behaviour by simple means. I think of mathematics as simplifying, giving a simple explanation for much complex behaviour. It helps you to understand why things behave in a certain manner. The underlying reasons why things happen are usually quite simple. Finding this simple explanation of complex behaviour, that appeals most to me in mathematics. I find beauty in the simplicity through mathematics.

Photograph courtesy of the authors.

May 2007 interview in Oslo. Left to right, Christian Skau, Martin Raussen, Srinivasa Varadhan.

Public Awareness

R & S: *May we now touch upon the public awareness of mathematics? There appears to be a paradox. Mathematics is everywhere in our life, as you have already witnessed from your perspective: in technical gadgets, in descriptions and calculations of what happens on the financial markets, and so on. But this is not very visible for the public. It seems to be quite difficult for the mathematical community to convince the man on the street and the politicians of its importance.*

Another aspect is that it is not easy nowadays to enroll new bright students in mathematics. As to graduate students, in the United States more than half of the Ph.D. students come from overseas. Do you have any suggestions what the mathematical community could do to enhance its image in the public, and how we might succeed to enroll more students into this interesting and beautiful subject?

Varadhan: Tough questions! People are still trying to find the answer. I do not think it can be done by one group alone. For a lot of reasons, probably because of the nature of their work, most mathematicians are very introverted by nature. In order to convince the public, you need a kind of personality that goes out and preaches. Most research mathematicians take it as an intrusion on their time to do research. It is very difficult to be successful, although there are a few examples. The question then becomes: How do you educate politicians and other powerful circles that can do something about it about the importance of education? I think that happened once before when the Russians sent the Sputnik in 1957. I do not know how long it will take to convince people today. But I think it is possible to make an effort and to convince people that mathematics is important to society. And I think that signs are there, because one of the powerful forces of the society today are the financial interests, and the financial interests are beginning to realize that mathematics is important for them. There will perhaps be pressure from their side to improve mathematics education and the general level of mathematics in the country; and that might in the long run prove beneficial; at least we hope so.

R & S: *In connection with the Abel Prize, there are also other competitions and prizes, like the Niels Henrik Abel competition and the Kapp Abel for pupils, the Holmboe Prize to a mathematics teacher, and furthermore the Ramanujan Prize for an outstanding Third World mathematician. How do you judge these activities?*

Varadhan: I think these are very useful. They raise the awareness of the public. Hopefully, all of this together will have very beneficial effect in the not too distant future. I think it is wonderful what Norway is doing.

Private Interests

R & S: *In our very last question, we would like to leave mathematics behind and ask you about your interests and other aspects of life that you are particularly fond of. What would that be?*

Varadhan: I like to travel. I like the pleasure and experience of visiting new places, seeing new things and having new experiences. In our profession, you get the opportunity to travel, and I always take advantage of it.

I like music, both classical Indian and a little bit of classical Western music. I like to go to concerts if I have time; I like the theatre, and New York is a wonderful place for theatre. I like to go to movies.

I like reading Tamil literature, which I enjoy. Not many people in the world are familiar with Tamil as a language. It is a language which is 2,000 years old, almost as old as Sanskrit. It is perhaps the only language which today is not very different from the way it was 2,000 years ago. So, I can take a book of poetry written 2,000 years ago, and I will still be able to read it. To the extent I can, I do that.

R & S: *At the end, we would like to thank you very much for this interesting interview. These thanks come also on behalf of the Norwegian, the Danish and the European Mathematical Societies. Thank you very much.*

Varadhan: Thank you very much. I have enjoyed this interview, too.