Statistical Science
2018, Vol. 33, No. 1, 126–137
https://doi.org/10.1214/17-STS634

A Conversation with S. R. S. Varadhan

Ofer Zeitouni

Abstract. Sathamangalam Ranga Iyengar Srinivasa (Raghu) Varadhan was born in Chennai (then Madras). He received his Bachelor's and Master's degree from Presidency College, Madras, and his PhD from the Indian Statistical Institute in Kolkata, in 1963. That same year he came to the Courant Institute, New York University as a postdoc, and remained there as faculty member throughout his career. He has received numerous prizes and recognitions, including the Abel Prize in 2007, the US National Medal of Science in 2010 and honorary degrees from the Chennai Mathematical Institute, Duke University, the Indian Statistical Institute, Kolkata and the University of Paris.

The following conversations took place in his office at the Courant Institute on September 20 and September 28, 2017.

Key words and phrases: S. R. S. Varadhan, large deviations, hydrodynamic limits, homogenization, polaron.

1. CHILDHOOD AND ADOLESCENCE

Ofer: Probably we should start at the beginning.

Raghu: I grew up in Chennai, not in the city but in the suburbs. My father was a school teacher. My parents were married for 24 years before I was born—I was their late and only child. They married quite young—my father was about 40 when I was born.

Ofer: Did you express an interest in science when you were young?

Raghu: I wanted to be a medical doctor. There was a medical college in the city and once in a while they had a public show. So I went there with my cousin. They were showing dead bodies, where the heart, the spleen was, etc. So (laughs) it turned me off. I guess I was 8 or 9 years old, and my cousin who went with me came home and had a fever for 3 days, so big was the shock.

Ofer: And it made also an impression on you.

Raghu: Yes. Especially, because we were vegetarian. Even butcher shops in the city were hidden, it was not part of the market.

I was reasonably good at math and science. I did not really like social sciences. When I went to high school, I did fairly well. I graduated from high school fairly young—in India most kids graduated when they were 14 or 15. You started school at 4, and the duration was 11 years. Officially, I was 14.5 because that was the minimal age for graduating, not so much for graduating, rather for entering junior college. So I guess that my parents made sure that I qualified.

Ofer: And then you went to study math?

Raghu: No. Then it was junior college for two years. You could choose various subgroups—I chose math, physics and chemistry; some people chose chemistry and biology because it prepared them for med school. There was also a social sciences choice. After junior college, you would continue college for two more years, to get a bachelor's degree, and then two more years for a master's. But there was a shortcut—you could combine the master's and bachelor's degrees together in a 3-year program. So this is what I did. That program had a limited number of seats; it was not offered in all institutions. There were no entrance exams—admission was based on your final grades in the junior college. So I applied for chemistry, statistics and physics. I did not apply for math; I applied for statistics.

Ofer: So this is the ISI?

Raghu: No no. I had not heard of the ISI at that time. It was in the local university in Chennai. You had to wait to see where you were admitted.

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Ofer: Was there a reason why you wanted statistics? How did you hear about stats?

Raghu: I had a high school teacher who told me that statistics is an important subject. I had no idea what statistics was (laughs). Chemistry, they turned me down. Physics, they said they'd admit me if I promised them that I won't leave. What happens is that many students who are interested in engineering get a seat in a bachelor program and leave it if they get admitted to an engineering school somewhere else. By then, it is too late for the department to find other good candidates. So they asked for this promise. I said OK, let me take a day and decide. But the next morning, somebody told me that the statistics program had put up a bulletin board with the names of those who got admitted. So I took a train, saw that my name was up there and then I was happy. That's how I got admitted to the master's in statistics.

Ofer: Was it course work or a master's thesis?

Raghu: Course work. But it was interesting—it was combined with math. We had the same pure math courses as the math program.

Ofer: Did you know it would be that way? Or were you just surprised to discover it?

Raghu: I had no clue. We had the same teachers, same program, same exams as the pure math students. They had in addition applied math; we had stats. That was the only difference. In stats, we had lots of probability, distribution theory, estimation, hypothesis testing, etc. There was a lot of lab work—handling actual data; we had hand calculators, the data came in tables on paper. A considerable part of the final exam was lab work, so the actual handling of data was important.

When I finished, somebody told me about ISI, a 3-year program.

Ofer: How were the relations with professors at the college? Was there personal interest in students? Did you hear about ISI from one of your professors?

Raghu: Math was a big department; it gave service courses, etc., but stats did not have undergrads and was a small department. There were 3 professors in stats. I knew all of them, I still meet some of them when I go to Chennai. But hearing about ISI was not through them, rather it was through the grapevine.

2. THE ISI YEARS

Raghu: ISI had an entrance exam. I took it and was selected. The same exam, if you scored high enough, they chose you as research scholar. Otherwise, you went to the stats training program. I took the exam maybe in May; in July I was told that I was selected and had to report at the first week of August.

So I went there and I was about 19. Varadarajan had just finished his thesis, was going to leave soon for a postdoc at Princeton. I met Ranga Rao and Parthasarathy, who were 2 years/1 year ahead of me, and also Sethuraman, who was a year ahead of me in Chennai but we didn't meet there. In college, the program was given to you—no choice, no electives, so no interaction. In Kolkata, there were no classes. They gave you a desk, a chair and they expected you to write a thesis in 2 or 3 years.

Ofer: Did you get guidance? An advisor?

Raghu: I think Varadarajan gave a course on point set topology. Bahadur was there at that time. He gave a course on measure theory. I learned probability theory in college, I knew distributions and so on, but I didn't know that there is such a thing as a singular distribution until I took the course from Bahadur. That was about it. There were these two courses, but that's it.

Ofer: Did you have an advisor at that stage?

Raghu: C. R. Rao was my advisor. There was no formal advisor when I came—you went to ask somebody to be your advisor and that's it. What I wanted to do really was applied statistics. For some reason, I was told to do statistical quality control (laughs). I started reading that stuff and it was rather dull—totally uninteresting. So I spent my time playing bridge and reading mystery novels. I would leave my desk, go to a friend in another floor, sit in his office and read mystery novels during the day, and then in the evening I would just play bridge. I spent a semester like that, and then Parthasarathy came to me and said, "You are wasting your time, do some math instead."

Ofer: How big was the incoming class?

Raghu: One or two research scholars. In my year, there were two. Sometimes if people did well in the training program, they would advance to the research program.

So I said OK, let's read something. Sethuraman, Parthasarathy, Ranga Rao and I, we decided to learn mathematics. Parthasarathy and Ranga Rao had interacted with Varadarajan and they had learned something. We started out by learning weak convergence (Prokhorov's theory). Learning meant that one of us took the responsibility to read it and lecture to the others.

Sethuraman did functional analysis. And Parthasarathy gave a course on information theory. Ranga Rao gave a course on group theory. I gave the course on weak convergence. We did all of that, and

FIG. 1. *Varadhan receives the Abel Prize from the King of Norway.*

then Sethuraman left and the three of us were there for a year. (Sethuraman graduated, he went to some place—maybe UNC, Chapel Hill—as a post doc.) So we decided that now that we had learned mathematics, we should work on problems.... We thought of some problems that we generated ourselves.

Ofer: Were you completely disconnected from the outside research community?

Raghu: We had no contacts at all. The project we took was the following. There is the Gnedenko–Kolmogorov book, limit theorems, infinitely divisible distributions and so on. You know that the theory works for the real line and more generally for Euclidean space. Can we do that for arbitrary Abelian topological groups? We solved the problem. Russians were also working on the same problem but they never got the full solution as we did. See there is a technical problem there. If you look at the Kolmogorov result, there is a term $e^{itx} - 1 - itx$. This is very important—the term cancels an infinity. What do you replace it with for an arbitrary group? What you want to replace it with is the log of a character. You want to choose for the character the log which makes it additive. There are some algebraic things that you need—it is not clear such things exist. It exists for connected groups, and doesn't exist for discrete groups. A general group can be a mixture of these, you need to know a bit of structure theory of groups in order to come up with a solution. And that's what we did. We learned the structure theory and found a solution.

Ofer: You solved it and published it?

Raghu: Yes, it was published in the Illinois Journal.[1] There is another publication before that, about whether you can factor a distribution as a convolution of two nontrivial distributions. If you call distributions where this is not possible prime, how do you construct primes? Some discrete distributions are primes. How do you construct continuous ones that are prime? So we said, "OK, why don't we prove that the prime distributions are second category?" It's soft functional analysis: if things are not prime, they are a convolution of two things. You can make sure that the two things are not close to being trivial. Then you have to show that what you get is a closed set. And then you have a countable union of closed sets, and that's it. So this was my first paper.[2]

And then I had to write a thesis. That was a solo paper: now we have an infinite dimensional space, a Hilbert space. I extended the Kolmogorov theory to random variables in an infinite dimensional space. Turns out it can be done, and I did that and it was my thesis. In fact, for part of my thesis, I put in the first two papers and there should have been something that you did alone, so I put that.[3] It was a bit hard because you can't use characteristic functions; you had to use something else.

That was my third year.

Ofer: So in terms of an advisor, there was no interaction?

[1] Parthasarathy, Ranga Rao and Varadhan (1963).
[2] Parthasarathy, Ranga Rao and Varadhan (1962).
[3] Varadhan (1962).

Raghu: I went to see Rao and explained to him what I did. That was it.

Ofer: And what did the others do? They also had to do their own problems.

Raghu: Yes. Parthasarathy worked on information theory. Ranga Rao did work on weak convergence with applications to the law of large numbers in Banach spaces.

Ofer: By this time, you were almost 22. And then you graduated.

Raghu: Well, Kolmogorov came then. Summer of '62. So C. R. Rao asked him if he would be the examiner of my thesis. The thesis required 3 examiners: one internal, the adviser, the other two external. In my case, one was Doob, the other was Kolmogorov. Doob was sent a copy and he wrote a report. The report was very interesting: this is a good thesis. You produce such good theses, there is no need for external examiners. You can do it yourself (laughs).

Anyway, Kolmogorov was there, and he said that he needed to take the thesis with him back to Moscow to write a report. C. R. Rao asked me to give a lecture, a lecture was arranged, and I gave it. The lecture was supposed to be for an hour, but I went on far too long, maybe one hour and 45 minutes. People in the audience began to grow restless. I finished my lecture, and Kolmogorov stood up with a chalk to say something. But people lost patience, and started to slip away quietly. He got angry, threw the chalk to the floor and stomped out of the room. So I immediately thought, there goes my thesis. . . .

Ofer: But he was not angry at you?

Raghu: We ran after him, 3 or 4 of us, and profusely apologized. He said, you do not have to apologize, in Moscow seminars run for 3 or 4 hours. But when Kolmogorov speaks, people should listen.

Kolmogorov took the thesis back to Moscow, the report didn't come for a long time—nine months. He left in May of '62, and I was to graduate in '63. Fortunately, in January of '63 Parthasarathy went to Moscow on a fellowship for a year, so he went to Kolmogorov and nudged him to write a report, which he did. The report was quite positive.

Ofer: So the students get the report?

Raghu: No!

Ofer: So it's your adviser who gave it to you?

Raghu: No!

Ofer: So how do you know that the report was positive?

Raghu: Because the report came to the office, and the clerk showed it to me first (laughs).

And then I graduated. Parthasarathy, Ranga Rao and I worked together on Lie groups. One of the things we did towards the end is study Dynkin's work on Markov processes. I was interested in stochastic processes and Brownian motion. Varadarajan, who came back that fall, suggested that if I was to work on diffusions then I should learn some PDEs. And he suggested that I go for postdoc to Courant.

Ofer: Because of PDEs, not probability?

Raghu: There was no probability at Courant then. Donsker was coming that year, and so when I was thinking of coming, he had just arrived. Varadarajan wrote to Peter Lax asking whether there were some application forms to fill. That was October or November of '62. Until April of '63, there was no response from Courant, and I was getting a little nervous. So I went to my adviser and said: "You know, I'd really like to go abroad this year, and I received no answer from Courant. Can you help me?" He said, OK, I'll write to Cornell. So he wrote to Wolfowitz. I immediately got a cable from Wolfowitz, offering me a faculty position, which I accepted. The next day, a letter arrived from Courant. It contained no forms but rather an actual offer. I went back to my advisor and asked him what do I do now; I really want to go to New York. And he said, don't worry, just write to Wolfowitz, with some explanation, it has only been a day. I did that, and Wolfowitz didn't talk to me again for a few years.

Ofer: This kind of thing somehow continues to happen. . . .

3. THE COURANT YEARS

Raghu: I arrived here (at Courant) in the fall of '63. It was a research position. I did manage to teach, but I got paid extra for that, usually in the summer.

I got engaged before I came, and in '64 I went back to India to get married, and then came back here.

Ofer: How was it to come to New York from India then? It was I imagine your first trip outside of India, quite a cultural shock.

Raghu: Yes. And during the first two nights, I stayed in Times Square because some student organization that arranged accommodations found a place there. Times Square was much worse than today. I arrived on a Sunday, and on Monday, I showed up at Courant, and when I told them where I was staying they were horrified. So they arranged for me to stay closer, at the Albert Hotel I think. People were nice those days—I found a studio, and it was $135 per month. I came on

FIG. 2. *Kolmogorov (front) with students at the ISI. Standing from left to right: K. R. Parthasarathy, B. P. Adhikari, S. R. S. Varadhan, J. Sethuraman, C. R. Rao, P. K. Pathak.*

September 15 and told the guy I won't have any money until October 1. He said that's OK, I'll hold the apartment for you, and as soon as you pay you can move in. So I stayed a few more days at the hotel—I think that I was paying $10 a day—and then I moved in.

Ofer: How was it mathematically?

Raghu: It was very exciting because there were maybe 50 postdocs and visitors. There was money from the Sloan Foundation to build up probability and statistics. Senior people were also among the visitors. Some of the visitors were on a sabbatical, and there was no shortage of money. For example, when we came

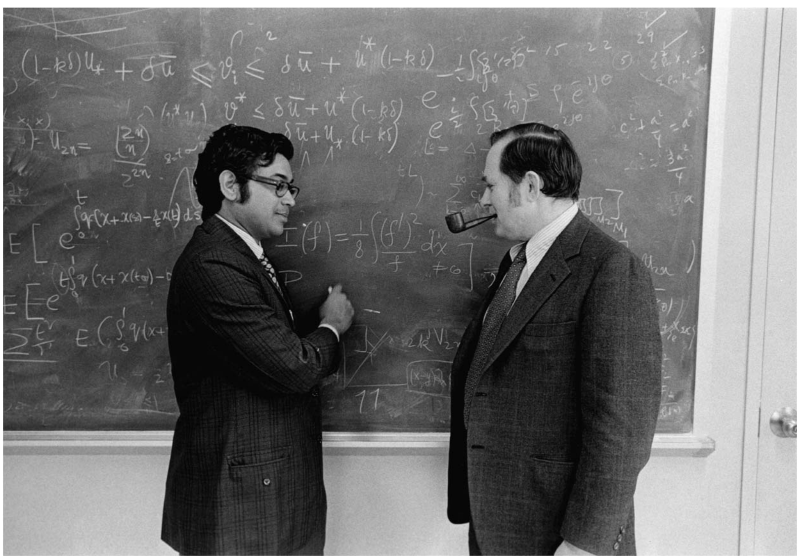

FIG. 3. *Varadhan (left) with Monroe Donsker, NYU.*

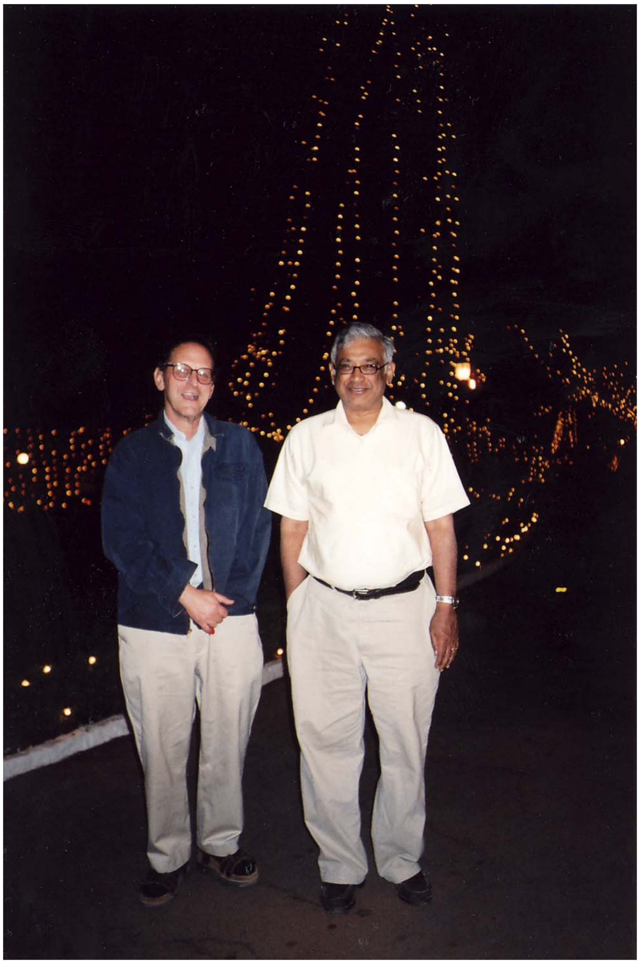

FIG. 4. *Varadhan (right) with Dan Stroock, New York.*

back after I married my wife, she, who had just finished high school, wanted to go to college. It was $1,600 a year and I couldn't afford that on my post doc salary. They told me not to worry, and paid for her scholarship. (Later, when I became faculty, this was covered by faculty benefits.)

There were lots of seminars a probability seminar at Courant and one at Rockefeller; Mark Kac used to be there, Feller used to visit there and Henry McKean used to visit from MIT. Donsker came to Courant in '62. Stroock was a graduate student at Rockefeller, and he joined Courant in '66, was postdoc for a year and then was on the faculty for 5 years. Other probabilists were Simeon Berman and Warren Hirsch.

Ofer: From the mathematical biography, those years were fantastic. You had your first paper on large deviations, your work with Stroock on the martingale problem... Schilder. Were you motivated by Schilder's result? Was that in parallel?

Raghu: Schilder was a student of Donsker. I was intrigued by Schilder's thesis. I liked the result, somehow it is a natural result, but I didn't like the proof. The proof involved approximations in finite dimensions. Well you have to do it in finite dimensions but you don't have to do it in that complicated way. My idea was that like in Prokhorov's theory, weak convergence should be a property of measures, and that should imply convergence of integrals (expectations) by some kind of continuity theorem. So there should be some kind of behavior that involves probability, and that should imply something about integrals. Schilder approximated both the functional and the probability measure in finite dimensions. So I thought you should separate the two. That is how I got into large deviations—analogues of Prokhorov theory.

Ofer: At that paper[4] you considered random walk approximations, diffusion processes—it was quite forward looking.

Raghu: Some reason for it was to study what is the class of functions that you can generate. If you only consider sums of random variables, all you can get are functions that are conjugates of moment generating functions. Those are not all convex functions. So if you want more convex functions, you have to consider modifications, and so on. I tried to do the best I can.

Ofer: This idea of continuous map, a contraction principle—this was just by analogy to Prokhorov's theory?

Raghu: Yes. There should be for large deviations an analogue of that theory.

Ofer: Now, at least from the point of view of large deviations, this laid dormant for some time, until the papers with Donsker came out later. What happened?

Raghu: I got interested in the martingale problem.

Ofer: So you started working with Stroock. Did you already pick up the PDE theory that you wanted to pick up at Courant?

Raghu: At least I knew something about Sobolev spaces... and why you need Hölder continuity for estimates.

We started working in '66 when Stroock came here. For a whole year, we did not make much progress—the problem was to know a priori that the solutions are in some L^p space—stochastic integrals which are defined in L^p and so on. Krylov knew how to do it but we didn't know what Krylov knew—what we ended up doing was to use perturbations. That is, we showed that any stochastic integrand is basically a constant, slightly

[4] Varadhan (1966).

perturbed: you have a σ that is integrated against Brownian motion, the σ is not a constant but is close to a constant, then basically by standard perturbation theory you can prove that the distribution of the integral is in some L^p because the constant is and you use Calderon–Zygmund to do the perturbation. That tells you immediately that at least for diffusion coefficients that are close enough to a constant in L^∞ you have existence, and uniqueness, at least for short time. And then by standard conditioning you can piece it together, and you get existence and uniqueness for continuous coefficients.[5] So you avoid having to go through partitions of unity and things like that to get estimates.

Krylov was more interested in the semigroup approach. He wanted to prove that there is a unique semigroup in $W^{2,p}$. We were more interested in individual x, because for limit theorems and so on this is what one needs. But Krylov had enough estimates that if he wanted to, he could have done it.

Ofer: So you moved from large deviations to studying diffusions, and then you came back to large deviations in your work with Donsker. Your project with Stroock was very productive, and later you wrote a book together.[6]

Raghu: Stroock moved to Colorado in '72, and our collaborative project ended at that time. Later, when writing our book, we spent some time together in Paris. And then in '76–'77 I was in Stanford and I had plenty of time, and I worked on the book then.

Donsker had this question—the large deviation formulas give variational formulas for integrals of some type. The Feynman–Kac formula looks like it is an exponential (of an additive functional) and you can think of it as some kind of functional space integral that you are evaluating asymptotically, and the exponential growth is some constant, which has a variational formula. Is there a large deviations interpretation?

Ofer: So you were not motivated by the application to statistical mechanics?

Raghu: At that time, I had no idea what statistical mechanics was. The key point to understanding was to write the variational formula not as optimization over L^2 but rather over L^1 by replacing f by f^2. That makes the connection obvious. I was struggling with that for a long time and then I was at Duke, to give a talk, I was sitting in the Duke library and it occurred to me what the solution was.

[5] Stroock and Varadhan (1969).

[6] Stroock and Varadhan (1979).

Ofer: That gave a formula for the occupation measure LDP.[7]

Raghu: And once you realize what the problem is, then the next problem that Mark Kac proposed was the Wiener sausage.[8]

Ofer: Did that increase your interest in problems motivated by statistical physics?

Raghu: Well I knew by then the connection with problems in statistical physics. It's interesting: Mark proposed the problem with Wiener sausage. And he suggested "If you can't prove it for BM, at least do it for random walk." But the random walk problem is harder! (laughs)

Anyway, I enjoyed doing that because it is related to ε-entropy, and I learned about ε-entropy when learning information theory. Although I didn't use any specific result from that background, it was good to make the connection.

Ofer: In fact, you didn't start from finite state space Markov. Was it clear in your mind that there is a link with types and entropy?

Raghu: It dawned on me when I started computing things and they began to look like some form of entropy.

Ofer: You also had some work also concerning normalization of local time, the work of Symanzik?

Raghu: That was much earlier. I thought I would work on quantum field theory, so I talked with Symanzik for some time. Most of the time I had no idea what he was talking about. But then there was a concrete problem that I could compute, it was not a difficult computation to me and he was quite happy with the result. I think that he put it as an appendix to one of his papers.[9]

Ofer: And the polaron? You heard about it also from Mark Kac?[10]

Raghu: No, it was from Elliot Lieb, who came one day and lectured to us about it.

Ofer: This is a problem you revisited recently....

Raghu: Yes, Chiranjib (Mukherjee) is interested in it.[11]

Ofer: Then you started to get into homogenization.

Raghu: I was interested in homogenization all along, and worked with Papanicolaou in the '60s and early '70s.[12]

[7] Donsker and Varadhan (1975–1983).

[8] Donsker and Varadhan (1975).

[9] Symanzik (1977).

[10] Donsker and Varadhan (1983).

[11] Mukherjee and Varadhan (2016).

[12] Papanicolaou and Varadhan (1973).

FIG. 5. *Varadhan (left) with George Papanicolau, NYU.*

Ofer: That would play an important role for you, also in the early '80s. There is a famous nonexisting paper of yours with P. L. Lions and Papanicolaou....

Raghu: Yes. I think Lions was visiting here, and he was interested in homogenization of periodic Hamilton–Jacobi equations. I was thinking of a way of doing it, and it worked out, so he said he would write it up. He went back, and what he wrote was a huge generalization of the idea, with a lot of technicalities, and I did not have the courage to go through it (laughs).

Ofer: My understanding is that it circulated, people had copies of this.

Raghu: Yes, I think that it got published eventually, maybe included in something.

Ofer: The idea of the environment viewed from the point of view of the particle, where did it come from?

Raghu: In the '80s, Claude Kipnis came to Courant, and he was interested in the behavior of a tagged particle in an exclusion process. So I thought about it, and realized that in the reversible case, the CLT was easy to prove.[13]

I had done some earlier work with Papanicolaou and Stroock[14] on homogenization and proving CLT's using martingales. Basically, the idea is that Ito's formula writes a function as a sum of an additive functional plus a martingale. If you are interested in a CLT for the additive functional, this is very simple. This is the basic idea, and the question is how far can you push this idea. And it turns out that you can do it for a tagged particle in an exclusion process.

Ofer: Did your work on hydrodynamic limits stem from this?

Raghu: I was in Luminy, in '84–'85, and George (Papanicolaou) told me about the problem of interacting diffusions with binary repulsive interaction, and he wanted to prove a limit theorem. I thought it was an interesting problem, but I made no progress whatsoever. George had a student working on it, Guo, and finally they did fluctuation theory, which is in Guo's thesis. I was thinking about the model on and off, when Joseph Friz gave a talk in the probability seminar. He was talking about lattice field models, Ginzburg–Landau models, for which you could prove convergence to a nonlinear diffusion equation, under certain conditions. As soon as I heard the talk, I knew that the places where I was stuck with interacting diffusions, I would get unstuck here. The difficulties will not arise in this problem. You see, one block estimate is easy to prove in any context—what is hard is the two block estimate. Two block estimates are hard for hard particles because you can't move a little piece at a time, whereas fields you can move infinitesimally, so certain computations are easier. I could easily prove the required limit theorem in this setup, and that resulted in the paper with Papanicolaou and Guo.[15] This is how I got interested in hydrodynamical limits.

Ofer: What did you do in the work with Papanicolaou and Guo?

Raghu: You have a field (variable attached to each lattice site), with nonlinear interaction, you scale it and

[13] Kipnis and Varadhan (1986).

[14] Papanicolaou, Stroock and Varadhan (1977).

[15] Guo, Papanicolaou and Varadhan (1988).

you look at bulk activation. The total sum is conserved, so something is shifted from one site to the next. There is a drift, and Brownian noise is added. There are multiple equilibria. Before the system reaches global equilibrium, local equilibria are established described by parameters that depend on space and time. You want an equation that describes their evolution. So it is not hydrodynamics but it is the same idea.

Ofer: And in the '90s you mostly worked on hydrodynamic limits?

Raghu: Yes. I had some graduate students—Jeremy Quastel, Fraydoun Rezakhanlou, Claudio Landim was Claude Kipnis's student but was a visitor as postdoc for a while, Stefano Olla was a Courant instructor.

Ofer: So this was a period with a lot of activity around Courant?[16]

Raghu: Yes, H. T. Yau and Sznitman also came here.

Ofer: Maybe we should elaborate a bit. From the late 1980s to early 1990s, it seems your interests shifted towards hydrodynamic limits. In particular, a change that one sees when looking at Mathscinet is that you started working much more than before with students, postdocs, etc. How did this transformation occur?

Raghu: I was working with Donsker before, and then he died. I always enjoyed working with other people—not so much by myself alone—and at that time I had some very good students: Jeremy (Quastel) and Fraydoun (Rezakhanlou), both at the same time. There were others, too: Chang from China, and also Calderon (the son of Calderon); Yau was a postdoc, and Sznitman was a young faculty. So we were a group, and we would just discuss—work together as a group, although each had their own problem.

Ofer: How exactly did this happen: did you divide up problems in the Russian tradition? Or did the participants come up with their own problems?

Raghu: They came up with their own problems. First, Fraydoun came as a student, and then Jeremy joined, the other two joined, and then we were looking at problems. My idea was if you want to look at the motion of a tagged particle, you should look at two colors, and when the second color becomes very thin, in the limit you should have the motion of the tagged particle. So I felt that it was important to study multi-color systems, interacting. That was Jeremy's thesis problem. I had already worked out the evolution of an earlier model, which was a fields' theoretic model. It is usually a gradient model but you can make the diffusion coefficient not constant and then it is not gradient anymore. So I learned how to handle that. Although simple exclusion is a gradient model, the multicolor case is not. You must use the methods that worked for the field theory model, in the particle system case. Fraydoun had a different problem. In the 80s, I was interested in the following: if you put in some interaction, the invariant measure becomes a Gibbs' measure, Ising type. The theory required high temperature, so that you had a single invariant measure. What happens if you really have two phases; for example, free boundary determined through motion by mean curvature or something like that. That's what Fraydoun did.

And then Yau and Olla came as postdocs, we worked on interacting Brownian motions and Yau suggested the relative entropy method. It requires more assumptions but is in general easier to handle, at least in the gradient case. So we did that for classical Hamiltonian systems, with Yau and Olla.[17]

Later, coming back to Courant on sabbatical, Jeremy and Fraydoun did large deviations for the multicolor model. The idea was that if pushed the number of colors to infinity, each particle has a different color, then you should be able to do large deviations for the empirical measure of the paths. If you can do that large deviations principle, all other large deviations would be obtained by contraction. That was a joint paper with the two of them,[18] in the late '90s.

Ofer: This is the time when you got interested yourself in large deviations for random walks in random environments. How did that happen?

Raghu: I heard of the problem and thought I could do something. By doing a little bit more using carefully subadditivity, I could complete the quenched case—this is a technical improvement. But the annealed case required new ideas—you had to do something.[19] And then with Elena (Kosygina) we did some homogenization,[20] which is again reformulated as the same large deviations.

Ofer: So during that period, you worked less with senior people—you were now the senior person.

Raghu: (Laughs) I used to talk with Chuck (Newman) but our interests are quite different—he was more into percolation and he was interested much more in the physics. And then Gerard (Ben Arous) came, and

[16] A review by Varadhan of the activity at Courant during that period has appeared Varadhan (2013).

[17] Olla, Varadhan and Yau (1993).

[18] Quastel, Rezakhanlou and Varadhan (1999).

[19] Varadhan (2003).

[20] Kosygina, Rezakhanlou and Varadhan (2006).

FIG. 6. *Conference in honor of Varadhan's* 75 *birthday*, *Berlin*, 2016. *With students*, *collaborators and friends*. *Lower row*: *N. Zygouras*, *H. Spohn*, *F. Rezakhanlou*, *S. R. S. Varadhan*, *E. Kosygina*, *S. Olla*, *C. Landim*. *Second row*: *J. Ramirez*, *I. Armendariz*, *E. Bolthausen*, *P. Friz*, *T. Lyons*, *C. Mukherjee*, *G. BenArous*, *A. Ramirez*, *A. Yilmaz*, *S. Sethuramanan*, *R. Pinsky*.

the students started working with him on statistical mechanics problems, and things slowed down in the hydrodynamics direction, which is fine.

Ofer: Something I should have asked earlier: while you were doing large deviations, there was also the Russian school (Friedlin, Wentzell) working on related problems. When did you learn of their work?

Raghu: Much later. Oleinik came to visit in the early 1970s and she mentioned their work, and then I looked it up.

4. ON PUBLISHING

Ofer: I wanted to ask you about publications. When one looks at your publication record, a good deal of it is in CPAM (Communications in Pure and Applied Mathematics, the Courant journal), and another striking fact is that almost none (or maybe none at all) is in "big" journals. This does not seem to be an accident. Did you never want to submit to these journals?

Raghu: As a probabilist, I wanted to publish in a place that probabilists would read. So I wanted my

FIG. 7. *Varadhan receives the National Medal of Science from President Obama.*

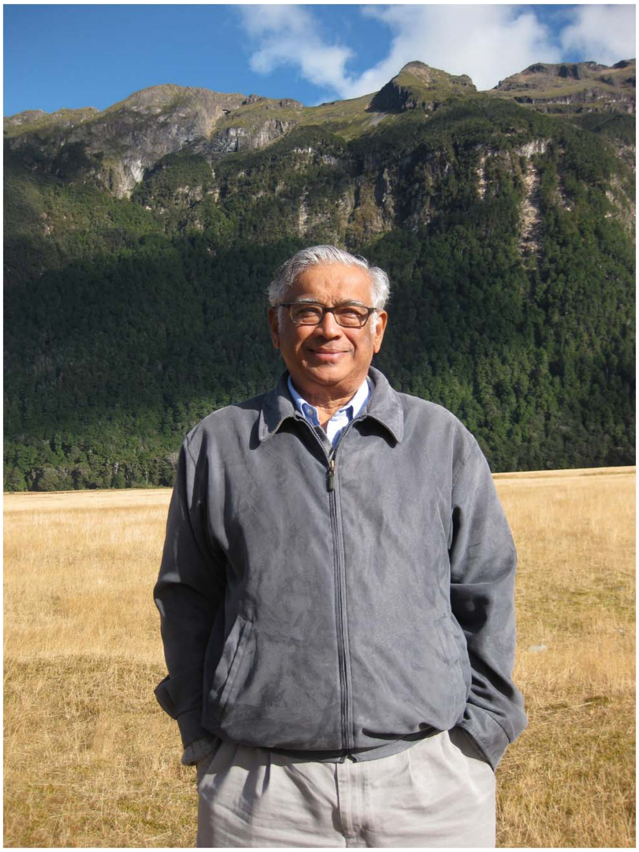

FIG. 8. *Varadhan.*

work to appear in probability journals—as long as it appeared in reasonable probability journals. I did not care where it appeared.

Ofer: It seems that there has been a big change—it used to be that probability papers rarely appeared in those journals, lately it is more common.

Raghu: I never thought of it. For me, CPAM was natural because it was our journal. Every once in a while I would submit elsewhere for a change—few to CMP (Communications in Mathematical Physics) or to the Annals of Probability and PTRF (Probability Theory and Related Fields). The earliest publications are in Indian journals, such as Sankhya.

To me, if you have a good paper and it comes out in a journal that people look at, I am happy—it doesn't really matter where you publish it.

Ofer: And that brings me to another question. Do you think people still look at journals, in the days of arXiv? Are journals important anymore?

Raghu: Indeed, there are no preprints anymore.... Journals are important for deans—for promotions, for the institutions. This is what the dean wants—what is the number of your publications, where have you published, what is the impact of the journal; the department needs to put these things together for a promotion to be approved.

Ofer: That's a somewhat cynical view of the publication world....

Raghu: That's the only reason we now have journals.

Ofer: So scientifically, their role is over?

Raghu: I think that what would make more sense scientifically is to put your paper on the arXiv, have reviewers read the paper and give their seal of approval. This could be anonymous—there could be a group that requests reviews for articles they deem important, the reviewers read and make their comments, find mistakes, etc., and eventually approve. There is no reason for journals—there is no reason for universities to spend millions of dollars to make publishers rich. Publishers have an important role in publishing books, not journals. They don't make that much money out of it, however.

Ofer: You do have a long experience as editor, both at CPAM and in the Annals of Probability. How does that align with what you just said about journals?

Raghu: 20 years at CPAM.... Well, so long as the system is there, you have to play according to the rules.

5. PROBABILITY TODAY

Ofer: You mentioned earlier that when you came, suddenly a lot of activity was occurring at Courant—two years earlier there was essentially nothing, and suddenly lots of visitors in probability.

Raghu: Courant always had a lot of visitors. Not so many in probability, until Donsker's arrival from Minneapolis. I recall that Gian-Carlo Rota was here, Stanley Sawyer, lots of visitors. It has been active ever since.

Ofer: How do you see probability now?

Raghu: I am not that familiar in the new areas. It is hard to keep track. I am more comfortable with the analysis side, not the combinatorics.

Ofer: Well there has been lots of analytic progress: SLE, rough paths, Hairer's work.

Raghu: Yes. However, I don't have the energy anymore to read everything. I did follow the SLE story, and I made several attempts to read Hairer's work.

Ofer: Nobody has the energy to follow everything.... Thank you for this conversation.

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