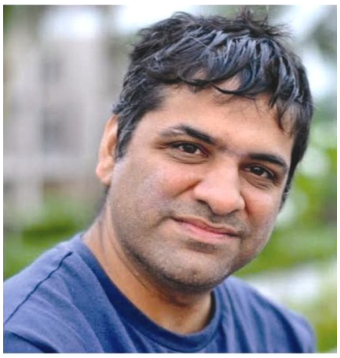
Shiraz Minwalla



Shiraz Minwalla

Shiraz Naval Minwalla is a theoretical physicist at the Tata Institute of Fundamental Research, Mumbai. He is noted for his pioneering work on string theory and quantum field theory. In November 2013, he was awarded the Infosys Prize by the Infosys Science Foundation and also the New Horizons in Physics Prize by the Fundamental Physics Prize Foundation, for his path-breaking work on establishing the connection between equations of fluid dynamics and gravity. The Infosys Prize Presentation ceremony was held on 8 February 2014 at Bangalore, where he shared his experiences in research in an interview. Following are excerpts from the interview.

Do you think that the fluid gravity correspondence is your most important scientific discovery?

Yes. I think the fluid gravity correspondence is the single most important discovery by me.

At a non-technical level, is it possible to describe this work?

The fluid gravity correspondence provides a well-defined connection between two most studied non-linear partial differential equations in history. The first is the relativistic generalization of the Navier–Stoke's equation of hydrodynamics. The equations of hydrodynamics are being studied by scientists and engineers for more than 200 years and they are relevant to various phenomena in the world. The second equation refers to Einstein's equations of gravity, which govern the geometry of space and time. They apply ubiquitously to gravitational forces and were formulated a hundred years ago. Mathematically, both these equations are partial differential equations, which a priori have nothing to do with each other. However, there were suggestions from string theory, that these were related. The suggestions came from the so-called AdS/CFT correspondence, which says that certain quantum field theories admit a classical description in terms of equations of gravity, at a certain limit (Figure 1). Also, the equations of hydrodynamics are believed to apply to all quantum field theories at appropriate mathematical regimes. We pointed out that these three things making a triangle imply that, in appropriate regimes, equations of gravity should describe hydrodynamics. If this is true, then it should also be seen mathematically. That's what we did; we showed that, at proper regimes, solutions of gravity equations have a one-to-one correspondence with those of hydrodynamics. It is not only interesting because it connects two otherwise unrelated but concrete mathematical equations, but also because it adds to our knowledge of hydrodynamics. Most of the modern study on hydrodynamics was done by Landau and Lifshitz. They wrote this beautiful book on *Fluid Mechanics*, where they have given a relativistic generaliation of the equations of hydrodynamics. They did it in 1930s, which has remained unchallenged since then. However, when my students and I were working with the equations of relativistic charged hydrodynamics, we found a mismatch with their general equation. It was a term that was missed by Landau and Lifshitz. This term had possible relevance to experiments and, as pointed out by Son and Surowka, also to something that must appear in quantum field theories with certain anomalous structures. So the fluid gravity correspondence taught us something new about a classical subject that most people thought was fully understood.

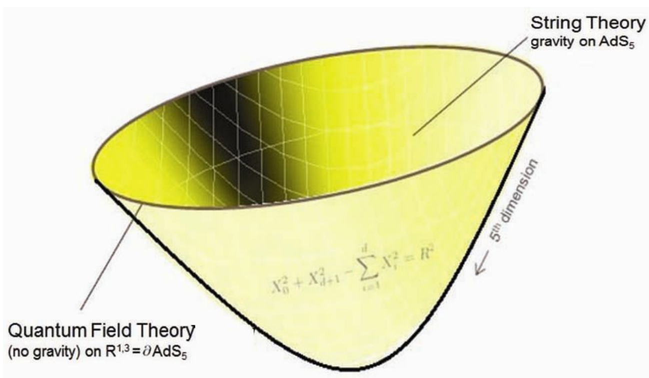
Do you ever get worried that this may be the most important scientific contribution of your life?

(Laughs) Yes. To be honest I do worry about that sometimes.

What is your current research interest?

Right now, I am working in two different areas.

One, I am studying the dynamics of Chern–Simons theories in $2 + 1$ dimensions. Ordinary gauge fields in the large N limit, have a large number of degrees of freedom, and are difficult to solve. However when they are self-coupled in a Chern–Simons theory, their effective degrees of freedom is 0; they show no dynamics and are of no physical interest. If the gauge fields in a Chern–Simons theory are coupled to a few matter fields, they become physical and are effectively solvable. It is interesting because it is tied to the rich class of theories we see in string theory. Also, there is a nice lesson one learns from this. There is a conjectured duality between Chern–Simon theories coupled to bosons and to fermions; under a certain map of parameters, they have the same solutions. This is of the nature of non-supersymmetric



The AdS/CFT correspondence suggests that equations governing hydrodynamics are related to equations describing gravity.

dualities, which we didn't know much about when they were discovered. When I was growing up in the world of physics, I saw supersymmetric dualities coming up one after another, and I was led to believe dualities are part of supersymmetric quantum field theories. However, now with the new non-supersymmetric dualities, we have a new class of theories which are as rich as the supersymmetric ones. This means there may be a huge number of dualities in quantum field theories, which we haven't explored yet. I think it is an important lesson to learn.

The second project I am working on is a bit speculative. It has to do with Wald entropy. We know that Bekenstein and Hawking proposed a beautiful formula for black hole entropy in Einstein gravity. But, in string theory most dynamics is governed by higher derivative corrections to Einstein gravity. Now Wald entropy, proposed by Robert Wald, is an analogue of the Bekenstein–Hawking formula, in higher derivative gravity. You can think of these as the first law of thermodynamics in a black hole. However, the deeper elements of thermodynamics come from the second law, that says entropy always has to increase. Hawking showed from the dynamics of Einstein gravity, that the black hole entropy always increases. It is not yet known, if this is true for Wald entropy. It seemed possible to me, that this can be answered from fluid gravity correspondence. Hawking showed that for an entropic current in a perfect fluid, there is an entropic current in Einstein gravity. I have been discussing with my students and collaborators whether it is possible to address the problem of the second law of Wald entropy systematically, from the lessons we learnt in fluid gravity. It is far from clear, if it is really possible.

Is your work of the sort, where a lot of people are thinking of the same problem?

If you are in a field where a lot of people are already working, it can be difficult to do something really impressive. It may not be the same as in a field where no one is working. I think I have worked in both kinds of fields. Before our work on fluid gravity, a lot of people were working on linearized hydrodynamics, linearized response functions, etc. Although we worked from a different angle, I would say it was already a very active field. However some of my other studies, which I find satisfying, are in areas where no one was working before. For instance, ten years ago, I wrote a paper with Marsano, Kyriakos, Papadodimas and Raamsdonk on how to compute thermal partition functions in Yang Mills theories in $S^3 \times S^1$. No one worked on that before, but it became a part of an active field. So that was quite satisfying. So, I think the answer to your question is sometimes yes and sometimes no.

Do you think quality theoretical physics research is done in our country?

Yes, definitely. Let me restrict my answer to string theory. A lot of quality research has come from just one scientist, Ashoke Sen, who is one of the greatest physicists of our times. Even if you take him away, good quality research is done in India. Fifteen years ago, it would be fair to say that there were only two groups in the country dominating in string theory research, those in TIFR and HRI (Harish Chandra Research Institute). Both these places continue to have very strong groups. But interestingly, string theory research in India has broadened. Now there are many small groups in the country where very good work is done. These places include IISc, which I can't say is small anymore, and ICTS, both in Bangalore. In Chennai, we have good young groups at IIMSc, CMI and IIT. My student Sayantani Bhattacharya, who is an exceptional physicist, has recently joined IIT Kanpur. We also have good string theorists at IISER, Pune, IOP (Bhubaneswar) and IIT Bombay; ... I may have missed a few places, which I don't mean to be not good. What is important is the diversity around the country. People at different places develop their own ways of thinking instead of getting influenced by one another, and this is very healthy for research. Good postdoctoral positions are available inside the country. Unlike most people twenty years ago, today we have many young physicists who obtained their PhDs inside India. Our research ecosystem is not dependent on people outside the country anymore and I believe this autonomy is something very positive.

In the past most major scientific discoveries were made by physicists thinking independently of what others were thinking around the world. But in the modern setting, we can easily access others' work through the internet. Does it affect our research?

It definitely does. It is both good and bad. It is good because very fast scientific progress can be made. Once a discovery is made, there is an explosion of knowledge in that area. It is bad because it hurts the diversity of the ecosystem. However, communication through internet is not same as communication in person. If you are in Harvard or Princeton, your thinking may get influenced by the trends, and the people around you whom you admire. This is not the case if you are in India; being far from that influence, you can make your own judgement. Even though it is important to participate in scientific discussions, historically isolation has helped India creatively and is more of a strength, than a weakness.

In history, we have seen many scientific breakthroughs were made by people who were young. Do you think age is a barrier for making major scientific discoveries?

What you are saying is more true in mathematics than in science. I recently read a book by (Godfrey) Hardy, which said that no major discoveries in mathematics were made by people beyond the age of 45. However, in science, I believe it is less true. As you said, people like Einstein and Heisenberg were young when they were most active; but there were also people like Bohr, who were not so young. In the field of string theory it is least true. Indeed as you grow older, you lose your quickness, and that certainly adds pressure. However, as you grow older you also get to know more; and knowledge always helps. In a complex theoretical field like string theory, where the body of knowledge is so vast, often a lot of progress comes from making inter-connections; and it is impossible to make connections between things you don't know about. We have people like Ashoke Sen, Strominger, Vafa and many others who were well over their forties when they were acknowledged as the leaders of our field. In our field knowledge certainly helps.

How does theoretical physics affect society?

Looking at it from a broad perspective, theoretical physics that was done hundred years ago has had an enormous impact on our society today. For example, without quantum mechanics we wouldn't have superconductors; and without them we wouldn't have computers and much of the electronic industry. Practical life would have been much different without theoretical physics that was done hundred years ago, although the concerned physicists were not aware of these applications. Even today research in theoretical condensed matter physics has a lot of potential applications. Theoretical physics of the kind that I do, seems unlikely to have any application in the next fifty years. It is very hard to imagine technical applications of our work, say, on understanding quantum chromodynamics (QCD) better; for which we need to bring nuclei of two atoms together to see the strong interactions, which sounds unlikely to be realizable in a fifty year timescale. However, the theoretical work of our sort is a part of an ecosystem. The mathematical formulations we do, lead to better understanding of other areas of physics, say condensed matter. The impact may be limited, but it is quite satisfying for us.

How can we motivate people with poor standards of living, towards theoretical physics? Is the Indian Government spending enough on this field?

In India, the Government schemes are quite impressive for a student in theoretical physics. Apart from getting a reasonable stipend in Ph D, one also gets government fellowships, like INSPIRE. However, the real problem in our country is the primary education, which fails to motivate people. Good education is the best way to motivate students, towards physics, or other areas in science. Roughly, three-fourth of the country's population gets so poor education that they fail to make any contribution to any intelligent activity of the country. Primary education in our country is dismal. Other countries across the world, including those poorer than us, give better primary education to their children. There is no excuse for the Indian society in this regard. Every school in our country must give reasonable education to the students, so that it can bring out their abilities. We, the middle class, don't take this seriously because our children go to reasonable schools. It is the middle class that makes the bureaucracy, and we must treat this with the seriousness it deserves. If we approach it with seriousness, I am sure it can be done.

How difficult is it to get a proper job for a graduate student in theoretical physics, specifically in high energy or string theory?

In India, there are plenty of jobs available for someone who shows a certain passion for the subject and has the ability to demonstrate. In TIFR, for instance, we can hire more people than we do if we find the right candidates. The same is true for the IISERs, IITs, and so on. Availability of positions does not hold us back, rather it is the other way round. Positions exist, but they can only be filled by suitable candidates; those who display both passion and ability. If you have these, apart from your accomplishments, it is not difficult to get a job. A career in physics or in science is not a routine job like, say, being a bank manager. It takes more than attaining certain number of skills. You need to be creative, have some spark and show your interest. Without them it can get difficult.

Should scientists ever retire?

This question has two elements to it. Should they keep holding formal posts at the institutes? When do they stop doing or loving science? Most find it hard to retire in the second sense. They can never stop loving science, or following it; because science is a passionate profession, where it really matters how much you are enjoying it. But part of the question is also about the jobs. Let's see how it is answered in different societies. In the US, the legal system won't allow a rule that says somebody must retire at a certain age; because that is age discrimination. They can continue their jobs as long as they keep doing their duties, like teaching and research. It is certainly true that as you grow old, you keep losing your quality and creativity in your research. However, it is seen as a compensation; after all, people who become professors, often have what it takes for jobs with five or ten times more money than what they get. They sacrifice that luxury, for serving science, and in return get a good old-age. In India, this is not the case, and it can be hard on people. I see that among my colleages at TIFR. When they reach 62 and all of a sudden they are out, they don't know what to do. On the other hand, we also need to make room for young people. ... I don't have a very clear opinion on this. May be I will, when I reach that age.

Say a few words about youngsters who wish to pursue a career in theoretical physics

Physics rewards different kinds of talents. Intelligence is obviously rewarded, but less than you might think. A certain level of intuition, instinct or feel for the truth is important. A certain level of confidence, that does not tend to stupidity, can be helpful. You may feel you are right about something and others may think you are being stupid. It is important to believe in yourself after weighing their words properly. A certain irreverence towards authority sometimes helps you to be creative. If you feel you are strong in some field, you can help by contributing to that field. We have contributions from physicists who are very mathematical in their work and also those who are not. Both are great. Don't get obsessed about things you can't. Follow your guts, follow your instincts and do what's fun.

Apratim Kaviraj, Centre of High Energy Physics, Indian Institute of Science, Bangalore 560 012, India.
e-mail: apratim@cts.iisc.ernet.in