Constructing Science and Institutions

A Discussion with Spenta Wadia

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ICTS-TIFR - 3 November 2016

Session 1 [Venue: International Centre for Theoretical Sciences, Bangalore]

Avinash Dhar: What motivated your return to this country? What were the circumstances before that? At a time when that was not really the fashion.

Spenta Wadia: The issue of motivation did not arise. I went to the United States knowing that I would come back.

I think it is important to say a couple of things about graduate school first. I went to City College of City University of New York. My PhD advisor was Prof. Bunji Sakita. From him I learnt not just physics but how to function in a small institution, in the presence of very big institutions like Columbia, NYU and Princeton around you. It was a very important learning curve. I also learnt how to build a group in a small place.

I also learnt from him that you need to grow your own constituency. You need to train students and you need to create your own environment. While I was there I wasn't aware that I was learning something. But when I returned to India these were the things that were on my mind. I sort of knew what to do.

As a graduate student I did a couple of things that were very satisfying for me. I basically worked on the quantization of gauge theories. With my friend Tamiaki Yoneya, while working in the lounge of the International House in New York, we basically realized the fact that the problem of gauge fixing in the non-Abelian gauge theories was something people had not thought about. Fixing a gauge like the axial gauge or coulomb gauge was routine. But when you sit down and do a calculation you find that it's not always possible to put an arbitrary field configuration in any gauge you like. So we discovered the gauge degeneracy of standard gauges and we wrote up a small paper. The motivation to do this was some very important discussion around that time due to Callan, Dashen, Gross and Jackiw and Rebbi about the vacuum of semi-classical Yang-Mills theory. We wanted to understand this phenomenon in a gauge independent way. While working on this we arrived at the gauge degeneracy. Our paper was not noticed by a lot of people, except Roman Jackiw who came looking for us at a physics meeting. But this was the first work that later on came to be known as the Gribov ambiguity of non-Abelian gauge theories.

As a graduate student I had a lot of difficulty in writing a thesis because what I had found and worked on, got to be known as other people's work.

For my postdoc I went to the University of Chicago. I resolved that I would not work on continuum gauge theories again for a long time [Laughs], as I did not want to think about gauge fixing.

Shiraz Minwalla: So, was Gribov's paper published after yours?

SW: Yes, much later in fact. Those were the times when there was a lot of publicity of the work of Russian physicists like Gribov, Polyakov and others in the United States. Of course they were great physicists. But in this social climate our work somehow got lost. But these experiences were very important for the future.

Gribov's paper was basically this... since you cannot fix the non-Abelian Coulomb gauge there will be zero modes. These are the zero modes of the Faddeev-Popov operator. They are the reason he thought why you have the phenomenon of quark confinement. Remember that the phenomenon of quark confinement and demonstrating that quarks don't come out of hadrons was one of the most important theoretical physics problems around the time when I was a graduate student in the late 70s. This was after the discovery of asymptotic freedom and the proposal of electroweak gauge theory. Quark confinement was a very big issue. Gribov thought that he had a contribution to this problem. But what he said [about confinement] was not right. In the non - confining Higgs phase too, the coulomb gauge cannot be fixed!

After graduate school I spent three months at Brookhaven National Labs and gave a talk on my thesis work on quantization of Yang-Mills theory. T. D. Lee was in the seminar and he pinned me down… he was as usual very aggressive... He trivialized what I had understood, 'what are you talking about? This is just a change of coordinates. Your coordinate system is not good enough.' In retrospect this seemed obvious! Those were very interesting times – things that are so obvious today were being debated very passionately even by people like T.D. Lee.

AD: What was it like to move from City College to Chicago?

SW: When I went to Chicago, I was a converted lattice gauge theory person. Ken Wilson used to visit City College very often. He used to visit New York for personal reasons. There were – offices of professors and graduate students and a little sitting place outside the office area. So Ken used to sit there and write code for computing on the lattice. We used to have conversations and I must say that I got very deeply influenced by him. I consider him to be one of the greatest theoretical physicists of our times. I started thinking about lattice field theory. There was no gauge-fixing problem on the lattice. However I am not a numerics person. And what does one do when one does not want to use the computer and do something analytically on the lattice? [Laughs]. At that time there was a lot of interest in the large N expansion. So I started working in that direction.

At this time a couple of things were going on: there was t'Hooft's 1/N expansion, trying to make a connection of gauge theory with string theory, and an attempt to solve gauge theories analytically. Wilson had by then introduced the Wilson loop in gauge theories and had shown that there is an area law in the strong coupling expansion. What happens at weak coupling in the continuum limit? Nambu and Gervais and Neveu attempted to derive a string like equation for the Wilson loop using the Yang-Mills equations of motion. This could throw light on the quark confinement problem. They made approximations that cannot be justified at all and found the equation of the Nambu-Gotto string where the condensation of the operator $TrF\mu\nu^2$ gave rise to the string tension.

So we started (Eguchi was there at that time) – working on the lattice. I knew Wilson's theory, so we put quarks on a lattice and said that there wasn't one path [between the quarks] but a distribution of many paths, and wrote down a trial wave function for the meson. Then we said that the best configuration would minimize the energy. And again we made an unjustified approximation ignoring overlapping loops, and arrived at a string type equation. [There is a story here, which I would like to tell you. Eguchi sent the paper to Physical Review Letters. The referee was not accepting it. He was saying that our arguments are not justified. I remember that one day Nambu came to my office and said 'Ken called me up'. Ken Wilson was the referee. And I just didn't know what to say.] [Laughs]

Then I had the idea that maybe one should derive Schwinger-Dyson equations for the Wilson loop. I had learnt about the Schwinger-Dyson equations from Ken Wilson's paper on quark confinement. I don't know if you remember, his paper discussed the Schwinger-Dyson equations using the invariance of the measure of the path integral. I knew that and started working on the loop equations. It was a very complicated problem, at least at that time. Eguchi was also working on it and he then wrote a paper again neglecting overlapping loops and asked me if I wanted to be a co-author. I said no. I didn't agree with the approximation. There was also a letter from David Gross saying the same when Eguchi's paper appeared... I still remember it... if you have a loop that intersects itself in a non-Abelian gauge theory you cannot factorize it.

SM: Not trace times trace?

SW: Yes. If you factorize it you get the desired $TrF\mu\nu^2$ term. This is the result you want but you can't do this. In the midst of all this I lost a lot of time. There appeared a paper by Migdal and Makeenko. In a sense, I felt things went away...again. But finally I derived the exact [string] equations on the lattice and I wrote a letter to Migdal saying 'your equations are not right... what is your regularization?' I still have his reply. He wrote back, 'physicists should not serve regularization. It should be the other way around'. [Laughs] He did the regularization to obtain a certain version of the loop operator. Many years later Polyakov and Rychkov succeeded, with an appropriate [continuum] definition of the loop operator, in showing that the simple a Wilson loop satisfies a loop equation [without sources]. And if you see what is involved ... it is the point splitting of the loop derivatives. Anyway, these equations were derived [rigorously] on the lattice in my paper. When I gave a seminar on this in Chicago, Leo Kadanoff was present and he commented that these equations would be very hard to solve in 4-dim. Because I was looking at gauge theories, it also occurred to me that one should have a unified approach to the large N limit for all systems with a large symmetry group. The most natural thing to do for say spin systems and non-linear fermion models would be to introduce bi-local operators that are the most general set of operators, invariant under the symmetry group of the system. This is how bi-local operators were introduced in my paper.

Of course, Yukawa introduced bi-local operators in quantum field theory in the 1930s. One day Nambu walked into my office and said, 'Yukawa had introduced this...and what I did is simply put a lot of points in between [string].' [Laughs] An interesting way of thinking...[about strings].

SM: This is the open Wilson line?

SW: Yes... if you have a Wilson line with quarks at end points, fix a gauge along the line, you get bi-local operators.

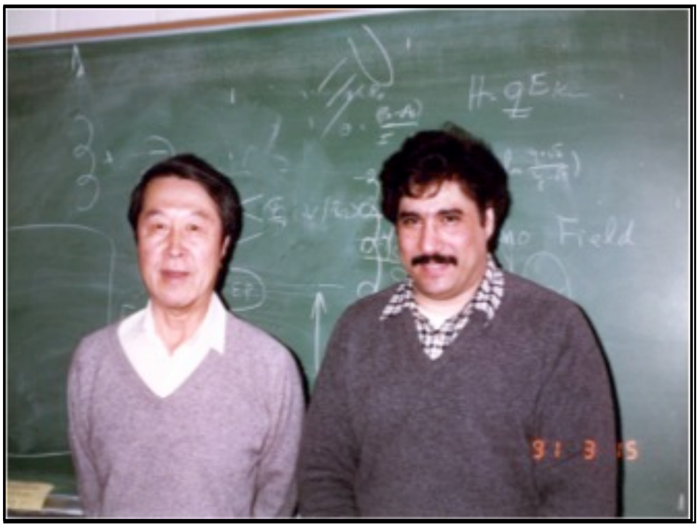
So, all those years in Chicago were very interesting.

AD: Your work on large N phase transitions was also done during this period?

SW: Yes.

Rajesh Gopakumar: You mention Nambu repeatedly. So Nambu was someone who had a lot of influence on you.

SW: Yes that's right. No doubt about that. As a young person in graduate school, and in Chicago, the three people who influenced me most deeply were Bunji Sakita, Yoichiro Nambu and Ken Wilson. There was also Leo Kadanoff who came to Chicago at the same time. These were the people who really guided my thoughts. Whenever I would want to do something [in the early years when I returned to India], I would always ask 'how would Nambu approach the problem'?



With Yoichiro Nambu at the University of Chicago

RG: What aspects of their thoughts did you find most impressive?

SW: Well, Ken Wilson probably influenced me the least. I didn't know him that well. He influenced me a lot intellectually, not so much as a person. What struck me most was that he was making sense of divergences in field theory. As a graduate student I was asked to study Bjorken and Drell to prove the renormalizability of QED. I don't know whether you have studied it. It's an extremely difficult problem. You have to use the Bogoliubov-Parasuik-Hepp-Zimmermann method, which is very complicated. I felt that there should be some deeper reason why all this was working. I think conversations with Wilson somehow gave me some insight into all that. I was also enamored by the fact that he didn't need gauge fixing [in a gauge theory]. That was for me a great liberation.

Bunji Sakita, of course. He was a very modest person. As a student I didn't know that he invented SU(6) [symmetry] of the quark model; introduced supersymmetry [in string theory with Jean-Loup Gervais]; introduced [with Gervais] the conformal dimensions (h, bar-h) [in conformal field theory]. He was also the person who [with Gervais] first introduced the path integral in string theory. He never mentioned what he had done, and that was for me an amazing thing – here was this person who had done all this and he doesn't even tell his students about it!

At the time [when I started working with him] he was working on collective coordinate quantization of solitons with Gervais and Jevicki, which fascinated me! I was assigned the problem of collective coordinates in gauge theories and in particular the quantization of the dyon solution in the SO(3) Georgi-Glashow model. This is where I [with Gervais and Sakita] introduced the notion of asymptotic gauge transformations in gauge theories to extract the global U(1) collective coordinate. This direction was inspired by the work of Regge and Teitelboim in General Relativity. I was of course in daily touch with Sakita. Every morning he would ask 'what is new?' Every morning! And it was great fun!

SM: So you spent a lot of time in discussion? In his office?

SW: Yes.

He told me once, 'you are my competitor'... he and Gervais. [Gervais used to be there at City College a lot of the time. Later on I found out that he too considered me as one of his students [I was once using his office at the Ecole Normale and by chance saw his CV]. The competition had to do with the gauge fixing issue. Gervais and Sakita had figured out how to eliminate the zero modes in soliton quantization using Faddeev-Popov method. And this too was before Gribov! They were very piqued by this whole Gribov business and [a bit I think] by the fact that I had an understanding of the gauge-fixing problem in a gauge theory in terms of locally fixed coordinate systems. When I was writing my thesis, they were writing their paper on this! So I was their competitor, which was a little difficult for me to take. I had explained to him about geometrical gauge fixing and how in the 'moving coordinate system' there is no zero mode!

That summer he went to Aspen and came back and told me, 'I mentioned this to Murray Gell-Man and he also agrees with it.'

AD: This was Jean-Loup or Sakita?

SW: Sakita. Jean-Loup would not have told me this. [Laughs]

Nambu, of course, was a presence... his softness, not saying very much, not pushing any particular idea very much and yet being the creator of some of the most prescient ideas of high energy physics in the last century. These are facts that work on you in a sub-conscious way. Nambu and I used to talk a lot. I would be talking to him, and he would suddenly look out of the window and say, 'let me tell you what I am doing now.' Then he would start telling me all his weird ideas...about tensor gauge fields, string theory and how superconductivity of those types of gauge fields will give you string tension. What a fertile imagination! His way of thinking influenced me sub-consciously... that simple things are very important to look at.

With Bunji Sakita at the Institute for Advanced Study, Princeton

I wanted to tell you this about Leo. After I came back India, I visited Chicago again – after the time Ken Wilson got the Nobel Prize. Leo, whom many people thought would share this Nobel Prize, said to me 'Look, there was the equation, Ken had that equation; Ken had the notion of fixed points, and Ken had the notion of the multidimensional space of couplings.'

SM: That was very generous of him.

SW: Even though he was certainly one of the creators of this kind of idea.

SM: But it's not often that you find people who are able to see that their contribution is limited.

SW: Now Avinash asked me about the work on the large N phase transition. I too tried to solve simple models. The simplest model to look at beyond the Gaussian matrix models was the unitary matrix model [arising from 2-dim lattice gauge theory]. I got the integral equation for the density of eigenvalues...I was a bit scared as I did not know the mathematics to solve it... So I took an appointment with S. Chandrasekhar… Chandra. You had to take an appointment to see him. Then I found this Russian book in the library called the 'Singular Integral Equations' by Muskhelishvili. Oh boy, this book was fantastic. I ended up not meeting Chandra. So within a week I more or less figured out how to solve this equation by mapping it on to a Riemann-Hilbert problem. But there was one trick that was needed to solve the problem and which was not there. Using this trick I could solve the Riemann-Hilbert problem... in this way I got the solution.

There was a rumor that Gross and Witten were trying to solve a difficult integral equation, but in Chicago I did not know which problem it corresponded to. David [when he came to give a colloquium at Chicago] said he and Witten had found a solution to the same problem I was working on. Now my mistake was that I just kept quiet... my culture was very different... I had come from a small place… I was very different then from what I am today.

SM: You are saying it was too intimidating.

SW: Yes intimidating and one just kept quiet. There was also a hostile atmosphere. So I didn't say anything about what I was doing … the third order phase transition.

SM: But had you already solved the equation?

SW: Of course.

SM: You had the two solutions; you knew there was a phase transition?

SW: Absolutely. I solved it by a dumb person's method. Using the method [I mentioned] anybody can do it. If you look at the paper of Gross and Witten, you have to guess the analytic function just like in the Hermitian matrix model that was solved in the famous paper by Brezin, Itzykson, Parisi and Zuber. My method was… to start with the equation and solve it straightforwardly. You can solve any such problem, with any number of branch cuts by mapping to the Riemann-Hilbert problem. [I had used the Coulomb gas analogy to intuitively understand the appearance of a gap in the charge distribution, which would be described by 2 different functions. This I knew from the book `Statistical Theories of Spectra: Fluctuations' edited by C. E. Porter, which was emphasized to me by my friend Vivek Monteiro].

I was talking to Leo Kadanoff about this subject all the time. He felt that … the order parameters... looked liked the order parameters of Sherrington and Kirkpatrick for spin glass. So then I told Leo what had happened during the visit of David Gross. You know what he did? He picked up the phone and called David Gross in Aspen. I was there in his office. He said, 'I have a young man here who has done this, this, this...' There was a long conversation actually. He then put down the phone and said, 'now he cannot f*** you over.' Exactly those words! [Kadanoff knew that I had solved the problem.] But there was a lot of pressure in the high-energy group to mention Gross and Witten's work in the paper I wrote [denying me the credit]. Only a couple of years ago I just rewrote it and sent it out on the arXiv the way it had to be!

Immediately after this work on the 3rd order transition I invented a 0+1 dim gauge theory and solved it exactly. At large N it exhibits the same transition I had found in the unitary matrix integral, and published it. The large N transition occurs in this model when the Fermi level of the associated fermion theory reaches the maximum of the potential.

SM: Sumit was also there in Chicago then. You worked with him.

SW: Yes, Sumit Das was a student then.

We wrote two papers together. One was on electric-magnetic duality. The other one was the reduced Eguchi-Kawai type model. It was a good proposal but what they [Eguchi-Kawai] did was not justified. But at times, other people can develop good ideas further. Giorgio Parisi wrote a beautiful paper on how to understand the Eguchi-Kawai suggestion in terms of quenched random momentum phases. Sumit and I wrote a paper applying this idea to Yang-Mills theory and understood the randomization of the translation group, which is sitting as a sub-group in U-infinity. There was a competing paper from Gross and Kitazawa. David Gross and I have had lots of interactions...

AD: The work on confinement that you did with him [Sumit], you wrote it up 20 years later in India.

RG: There is one with Sumit where you had pointed out an error [in Polyakov's work]. That was published in 1982.

SW: Yes, yes that was the electric-magnetic duality paper. If you want me to summarize that briefly – Polyakov made a truly profound contribution in showing that in the confinement phase of a gauge theory in 2+1 dim. can be understood in terms of a plasma of monopoles [coulomb gas] that gives rise a massive particle in the spectrum and confines integer charged quarks. The would be long-ranged correlations become short ranged. That was Polyakov's great physical insight. That paper is part of a trilogy by Polyakov in 1976.

RG: Instantons, monopole plasma... ?

SW: The instanton paper [in 4-dim Yang-Mills theory], then the 2+1 dim. Abelian [lattice] gauge theory paper and there is one more on the renormalization of sigma models and geometry. These papers were special – physics before and after them is different.

Then what he did is to apply this idea of confinement to SU(2) gauge theories. That too became one of Polyakov's famous papers. I will tell you what his mistake was. He made a singular gauge transformation on t'Hooft-Polyakov monopole solution that has unit monopole charge in units of the basic electric charge. This is a gauge transformation to a unitary gauge. He then ignored half the flux that is there on the Dirac string in this singular gauge. That reduced the monopole flux by half. So now you've got a Dirac monopole. His plasma of monopoles had half integer charged monopoles and they obviously confine integer charged quarks because the half integer monopole plasma will screen the integer charged quarks. So this was what he had in the paper and it was wrong. First SU(2) gauge theory does not have integer charged quarks in the fundamental representation. We basically found that there was a mismatch between the magnetic gauge group and the electric gauge group that brought out the importance of the centre of the gauge group, a fact that was already emphasized by 't Hooft. We wrote a paper on this and I gave a seminar at the Enrico Fermi Institute. Leo [Kadanoff] was there at the seminar and he came up and told me, 'you must have got up fresh in the morning to find a mistake in Polyakov's paper.' Then I wrote a letter to Polyakov. I think I have that reply somewhere. He got really upset. He wrote, 'I cannot make a mistake like that. It's just a matter of normalization.' It cannot be normalization! He didn't admit it. So anyway, that's how it was.

I came to India thinking about the [mentioned above] reduced [Eguchi-Kawai] models. On the way I had stopped in Les Houches for the summer school and Kitazawa and I worked on the Hamiltonian reduced model. If you look at gauge theories in 2+1 dimensions, then those theories can be basically written in terms of a single unitary matrix...in a certain gauge. So I said, 'fantastic! Single matrix Hamiltonian and one should be able to solve this model.' No chance...because there are no symmetries due to the quenched random phases. So you cannot solve this problem. This was the problem with which I came back to India. I kept on working on this for one year.

RG: When you came back to India with this kind of background, what was your state of mind about physics? Did you feel there were lots of problems you wanted to tackle? Did you feel you would have a program?

SW: Yes I came with a program.

Before that let me tell you another related story. One day Nambu and I went out to dinner with Steve Adler and on the way back he [Nambu] was dropping me at my house. He stopped the car, put his hand on my knee and said, 'look, the problem you are trying to solve is too hard. You should be a little careful.' See, my problem was that I wasn't writing any papers because I wanted to solve the quark confinement problem. I had this in my mind that oh if I could just solve this it would be fantastic. Maybe Nambu is old so he cannot solve it. I tried and tried but I failed. So I came back with that program – to work on quark confinement and QCD. Then something very interesting happened. I was invited to give a lecture course in the Krakow school in Poland. My old friend Mike Peskin was also there. He is the guy who told me all about the paper by Witten on current algebra...the Wess-Zumino-Witten term. Michael Peskin is an amazing expositor.

I was thinking: all this math stuff? How would Nambu think about this? After all a meson is made of two quarks so there must be a fermion model underlying this. I still remember I was at the airport at Krakow and I wrote this [model] down...I knew that quantum chromo-dynamics has a mass gap. So if I integrate all the gluons mentally what would I be left with? I would be left with an expansion in terms of fermion operators. And the coefficients would be related to the mass of the glue-ball. What emerges is a non-linear fermion model. At that moment I didn't realize that this was a Nambu-Jona-Lasinio like model if one retains only the lowest dim operator. Very shortly I did! Heisenberg originally invented this model, and this is mentioned in the Nambu-Jona-Lasinio paper.

I think by the time I came back, Avinash and I were sort of talking a little bit.

AD: I remember how excited you were about this.

SW: Yes, I felt finally we were making some progress in QCD.

SM: The work of Witten you talk about is the non-Abelian bosonization?

SW: No. The Wess-Zumino-Witten model.

RG: The 4-dimensional current algebra. The 5-dimensional Wess-Zumino-Witten term actually which he later specialized to 3-dims.

SW: That was later.

SM: Oh the thing that gives you the anomaly in the Chiral-Lagrangian?

RG: Yes. Then Witten interpreted the baryons as solitons.

AD: The solitons were there. But how the solitons could be identified as baryons [for odd N] wasn't clear. This paper made it very clear.

SW: Fascinating. So we wanted to understand this paper from a microscopic point of view. Then Avinash and I started collaborating. We knew the standard tricks of how to do the large N limit for the non-linear fermion models. At large N we can use factorization and Lorentz invariance to essentially work with the lowest dimension term in the non-linear fermion model. You have the chiral field, and then you integrate out the fermions. And then there is this determinant that you have to calculate...that is related to the anomaly a la Fujikawa. We solved this in some way – it wasn't very satisfactory. But I think what we really found – and that was the most important point – was the differential equation in the space of the slowly varying chiral field for the determinant. And that differential equation we integrated in the space of chiral fields to find the 5-dimensional Wess-Zumino-Witten term. And we were very excited and delighted about that. I think that was the first important work that we did. At least the first important work I did in the Tata Institute. Shanks (Shankar) was a student then and we roped him in.

The problem was the determinant. There was a very important paper by Alvarez-Gaume and Witten on gravitational anomalies. If you read that paper very carefully – one of the most important technical things that they did was to give a definition of the phase of the determinant. There is a modulus and a phase. And we this took over and worked out everything rigorously and got the same answer as before.

AD: There was a rigorous perturbative calculation to see whether the five-boson vertex is there. That we could clearly see from the Nambu-Jona-Lasinio model. The only issue was how to write the non-perturbative determinant. Usually if there is a gauge field, you know how to do it. But there is no gauge field here as it is just a four-fermion model. So we had to do a lot of hand waving and inventing background vector bosons – work out the terms and then set the bosons to zero. You still have the meson term that is left. Something like that was done. And number of colors popped out naturally because this was a model for quarks.

RG: So if I understand right the usual formulation you write in terms of the unitary matrix. But now this is effectively writing the unitary matrix as an exponential of the pi-meson field.

AD: Yes yes.

SW: I felt this was very nice, because we could do something about QCD at long wavelengths in the large N limit in this particular [chiral] sector. This project became Shankar's PhD thesis.

There was a competing paper on evaluating the determinant by Polyakov-Wiegmann. In those days there was no Internet. They also did the determinant calculation. The good thing was that it felt like playing basketball with Michael Jordan. We also played on the same court! Polyakov was there.

SM: It sounds from your description that you were motivated mainly by the aim of understanding quark confinement and other aspects. Looking back now, the problem in some sense is not yet been solved. Do you feel that it's an important problem and was the right one to tackle? What is your view on the problem?

SW: I think if one can make some headway one would have understood gauge theories differently. I still feel that we don't understand gauge theories of quark confinement except by analogy or numerically on the lattice. There was this talk by Nambu at a meeting, I still remember. He said, 'take a monopole and drop it into a superconductor' if you have a monopole and anti-monopole there will be a flux string in between that explains the linear potential. We understand that in terms of our understanding of superconductivity. After that Mandelstam and t'Hooft conjectured that the QCD vacuum behaves like a magnetic superconductor and that explains the confinement of `electrically charged' quarks. I think the achievement of Seiberg and Witten many decades later was to realize this idea in a weakly broken N=2 supersymmetric gauge theory. But without supersymmetry, it's like working on the real line rather than the complex plane. Then it is very difficult – we don't understand these things, but Seiberg-Witten solution may have a hidden message. Certainly it would be fantastic if somebody can solve this problem. [You will get rich as well... there is a million dollar prize for showing that a mass gap exits in a non-Abelian gauge theory in 3+1 dims.]

SM: But you know there are some problems you solve and get very famous for, but they don't lead to much progress. And there are some problems that are very rich and go very far. Which do you think this quark confinement problem is like?

SW: Because it is so complicated, the quark confinement problem led us to so many other things. It led us to exploring matrix models, supersymmetric theories, lattice gauge theories etc.

SM: So the attempt proved very fertile.

SW: Yes. It's like the Riemann hypothesis. As you know it's one of those great math problems, but in order to solve that problem, an amazing amount of new math has come out.

RG: I think in the quark confinement case also, it would depend a bit on how it is solved. If it is a purely mathematical demonstration, in epsilons and deltas, then it's not probably likely to… As you said the Seiberg-Witten solution is an example of a whole rich field.

SM: Another analogy might be the question you brought up earlier on about renormalizability. The BPHZ [Bogoliubov-Parasuik-Hepp-Zimmermann] stuff was great, but what did you learn from it? However the Polchinski-Wilson approach to renormalizability in field theory also did it, but it taught you so much.

SW: Yes, about the deep structure of quantum field theory.

RG: So you mentioned travelling... and also that the general atmosphere in TIFR was not to travel.

SW: Yes that is true, but after the initial Nambu-Jona-Lasinio model work Virendra Singh did give me leave and I went to KEK. During that time we [Avinash, Shankar and I] were corresponding a lot. In the old days we used to do calculations and send them by mail. KEK had some brilliant people Hirotaka Sugawara who was the head of the theory group then and Makato Kobayashi. I spent three months in that laboratory. I wondered what were the Japanese doing in experimental high-energy physics in comparison with what was happening in the West? Of course in hindsight, they were preparing for their big projects in high-energy physics!

There I was alone and I loved to hang out in libraries. I didn't like sitting in offices. So I was just going around and wondering what else new I could do. In earlier times at Chicago I had worked on matrix models, and the first *avatars* of random surfaces in connection with Polyakov's work on quantum geometry of strings. Friedan and Shenker were there in Chicago at that time. I was very familiar with the basics of string theory. So at KEK, I wondered that if in the work on the Nambu-Jona-Lasinio model, we go from four to two dimensions, we will have a string moving in this space of the chiral field, which is not a flat space. I thought of this idea – it was early 1984. As I mentioned after my mini-sabbatical at KEK, I went to Chicago. I mentioned this idea, of a new type of string theory, to Nambu and he liked it and found it interesting. I returned back to India after visiting SLAC and IAS Princeton.

In the summer of 1984 in Aspen, Mike Green and John Schwartz demonstrated anomaly cancelation in certain supersymmetric models of string theory. After that began the explosive interest in string theory because anomaly cancellation meant that there was a chance that string theory can describe chiral gauge theories of the real world. But I didn't know any of this as we were far away in India. We didn't get any mails, no seminars by visitors, nothing! The news came that this had happened and people were very excited. I remember that [during my visit to the US after KEK] I gave a seminar at the Institute of Advanced Studies on the Nambu-Jona-Lasinio model etc. I went to an office Witten was using but he was not there. His desk had only one paper and it was the review article by Schwartz. I noted that in my head, just noted it. I didn't know what it all meant.

RG: So this was before Green-Schwartz. Early 1984.

SW: Yes this was before the summer. Alvarez-Gaume and Witten had also written a paper on gravitational anomalies. So I suppose this [anomaly cancellation in string theory] was all building up in some sense?

I came from another direction, trying to do something new. I had no idea what had happened in the west. So I came back and started working on it with my two students – Sanjay Jain and R. Shankar (Shanks).

Now something happened which I think was perhaps a strategic error of my *physics* life. There was a nascent nuclear disarmament movement in Bombay and given my left political leanings I encouraged my friends to join the nuclear disarmament movement. The People's Science Movement was also going on, and so was the anti-nuclear movement. I felt that we should be doing something about that. Together with other activists Vivek Monteiro, Praful Bidwai, Achin Vinayak and Biswarup Banerjee (from TIFR), we had formed a Group in India for Nuclear Disarmament abbreviated as GROUND. We were all part of that. I was the scientist and convener. We spent an enormous amount of time in the anti-nuclear movement. This had started before I went to KEK Japan. When I was in Japan I actually went and met the Hibakusha. They are the survivors of the atomic bombing of Hiroshima and Nagasaki. I went to Hiroshima and met lots of people. I met Mr Iwakura and Ronny Alexander of the Hiroshima-Nagasaki Publishing Company. They were the people who created the '10-feet movement'. People contributed to buy 10 feet of film about the bombing, which was now available to the public from the US defense department. They made the movie 'Prophecy', which I brought to India. This film formed a very important part of our movement. People like Shanks, Sanjay, Krishnan, Diba Siddiqi and others made an exhibition on nuclear disarmament. There were lots of talks and meetings as well. So this took away a lot of time – from the summer of 1984 to the end of the year. And elsewhere in the world string theory was exploding.

We had already formalized many things like how to construct a string moving on group manifold, Weyl anomaly cancellation putting restrictions on the group manifold etc.

SM: So you had the Wess-Zuminio-Witten term?

SW: Yes because we had worked on the Nambu-Jona-Lasinio model. Just remove two dimensions and you get that.

SM: You had not read the non-Abelian bosonization paper?

SW: No. I was not aware of it *to begin with* in early 1984.

SM: You were looking for conformal theories?

SW: Yes conformal theories on group manifolds.

RG: Non-Abelian bosonization was 1983, I think.

SW: Where does he prove that the beta function is zero?

SM: With the Wess-Zumino-Witten term.

SW: Yes now I remember Witten's paper. It was published sometime in 1984. We became aware of this paper and its importance after our work had progressed. But he doesn't talk about string theory there.

RG: No no. It's purely two-dimensional field theory.

SW: In the string theory framework you need the anomaly cancellation. So you start restricting the group manifold. All this works out nicely. I still remember that the anomaly term of the current algebra was a little tricky to derive.

SM: Yes one can easily get it wrong.

SW: Then one day Shanks had slipped a paper into my office. It said, 'these are God's rules.' I still remember that – 'God's rules of how to regularize the current that gives the right answer.'

AD: Who was God?

SW: God knows who was God. [Laughs] You should ask him if he remembers.

I don't remember in which paper of Witten…

RG: Certainly he talks about the beta function.

SW: Continuing the narrative, I knew about the work of Friedan on sigma models where conformal invariance leads to the Einstein equations in the absence of matter. But there was no time to put all of this together in the context of strings propagating in an arbitrary curved space. One reason could be time-sharing with the nuclear disarmament movement.

After this we, as a group, came back to just doing physics ... you got to be strong in what you do and only then you can change a bit the world around you. As physicists we just needed to work and focus on physics. Spending too much time on disarmament was I think, an error. But, of course, during this excursion I learnt a lot about politics and how to manage people. It was a great learning experience. What was happening was that we physicists were amidst various factions of the Left. To begin with we did not know the political basis of why each faction presented a different way forward. But this was a great learning experience...but we lost a very big prize in physics...I think. However at a personal level I still continued my association with social and educational movements in India including the People's Science Movement.

The project on conformal invariance in string theory became the PhD thesis of Sanjay Jain. There were already contributions to this project by Gautam Mandal.

RG: There was this first strings kind of meeting in Santa Barbara.

SW: Yes I gave a talk there.

RG: So was that the first real meeting?

SW: Yes. It was organized by Mike Green and David Gross.

SM: Which year was this?

SW: 1985.

RG: You and Ashoke are the two Indians in the proceedings of that meeting.

SW: Yes.

So this is how we started doing string theory – in India. After that a couple of years were spent on trying to ask the question – What are the principles of string theory?

SM: I wanted to ask – how did you learn the subject?

SW: Oh string theory, I knew from Chicago.

SM: But there were all these new developments – would you be reading the papers? Were there review articles?

AD: You learn on the job.

SW: Yes.

RG: Did you have some sort of group discussions?

SW: No, not at that time. And this was a mistake even though we were very few.

RG: I guess Avinash had left.

AD: Only Spenta and students were doing string theory.

SW: Sunil Mukhi was also there by then. There was not too much of an overlap in the sense of directions of research. There could have been an overlap in writing down the classical string equations from sigma models. I was unable to put all our resources in people and physics together.

RG: He was also doing sigma models.

SW: Yes. Alvarez-Gaume, Friedman and Sunil had written an important paper on how to do background field perturbation theory and renormalization by expanding fields in sigma models using Riemann normal coordinates. All that couldn't be put together and that was perhaps just the lack of time, lack of communication, and other situations in the department at TIFR. So this was a big miss, I think. And the paper on sigma models and classical string equations came from Ashoke Sen and Callan, Friedan, Martinec and Perry who also included the dilaton.

RG: I wanted to ask you about your attitude towards supersymmetry.

SW: You know a lot about me. That is a very loaded question. [Laughs]

RG: Supersymmetry before string theory, after string theory.

SW: I had a little problem with supersymmetry… a taste problem because of the supergravity papers... this was not for me. It is too complicated and I felt that nature couldn't be like this. It's great that we have type IIA and IIB string theories– they contain the organizing principles. So supersymmetry was fine, but all the complicated aspects of supergravity did not agree with me. I had heard a talk by

Witten in Chicago at the American Mathematical Society meeting on supersymmetry and Morse theory. It was an incredibly beautiful talk and we used the ideas of supersymmetry many years later. And I will tell you about that. At that time we only knew about conformally invariant string theories. We knew about classical solutions of string theory. I did not like string field theory – I had heard talks on it by Kaku and Kikkawa who first constructed the bosonic string field theory. Sorry. That's not my taste.

Perhaps there is an alternative way of understanding the field theory of strings, when you know the classical solutions. We know from Wilson that there is a space of flows connecting critical points and in 2-dims Zamolodchikov had proved his beautiful c-theorem. We assumed that the space of RG flows and fixed points –is the configuration space of string field theory. So we tried to understand this problem from the viewpoint of stochastic differential equations. I'll tell you what happens here.

You know what stochastic equations are, of course. The renormalization group (RG) differential equations take you from one fixed point to another. The stochastic differential equation, obtained by adding a noise term to the RG equations, feels around like a rat in the space of flows, in the theory space. Therefore, it's a good setting in order to understand the structure of two-dim field theories. We made a connection of Witten's paper [on Morse theory] with the 2-dim `theory space' and its critical points, using the c-function as a Morse function. Since you know the relevant operators at each [critical] point in theory space and suppose these relevant operators [instabilities] form a finite dimension space, then the Morse index at the critical point is the Morse index. I knew from the lectures of Raoul Bott about Morse inequalities and under very special circumstances they become equalities; then the knowledge of critical points enable us to figure out the topology of the space because of the equality of the Morse and Poincare polynomials. All the

Betti numbers are known. It's amazing that for c<1 models, you know all the Betti numbers of the space of RG flows. So Sumit, Gautam and I wrote this up ... The paper is not well known but I really like this work.

RG: Which year did Sumit come back to TIFR?

SW: I don't remember but I did make sure that he came back to TIFR. There was some difficulty in hiring him but it finally worked out and Sumit came around 1987.

SM: And when was Sunil hired as a faculty?

SW: He came as a postdoc and two years later…

AD: He was there before I left. So I think he must have joined around 1983.

RG: As a faculty?

AD: No as a post doc.

SW: So after two years he joined as faculty.

SM: Did you play a role in his hiring?

SW: No. I was a Fellow then. So I wasn't there in the departmental meeting. They just asked me for a write-up, discussing his papers, which I did. I also did the same for Sumit Das.

RG: Avinash, when did you come back?

AD: 1987.

RG: So already by 1987 there were four others.

AD: Yes there were four of us. Then students were there. Sanjay, Shanks and Gautam were also there already?

SW: Yes.

SM: Avinash, were you also working on string theory when you came back?

AD: Yes. I started working on string theory in SLAC. Along with this beautiful paper that came out on conformal invariance. We also had written one on how to construct vertex operators but in an old fashioned way. By writing down 2D gravity invariant operators – just explicitly constructing all of those. And we also worried about off-shell amplitudes. We had a whole section on this which Peskin said we shouldn't include. He felt there was nothing like off-shell amplitudes. We found that you needed to retain the Liouville mode in order to write those amplitudes, which is what Ashoke is also telling us. But Peskin was very bothered about that. He actually was so vehemently against us writing that section that we dropped it and put it only as a remark in a footnote. In fact not only that – we had also found that you needed a Liouville type of a counter term, $\partial^2 \Phi$, to remove divergences from the trace of stress tensor. At that point somehow we did not realize its significance.

SW: Now I will tell you a little bit about our work on Liouville theory and matrix models. The main question we continued to address is, `What is String theory?'. I remember Sumit and I were in Jaipur, attending one of the SERC schools. We were working on Liouville theory locked up in a room because of crickets. Studying the Distler-Kawai paper we realized – that the Liouville mode can be interpreted depending on the central charge as a space or time coordinate in string theory. We got very excited about that and wrote up a paper with Sachi Naik who was a postdoc at TIFR.

Why is Liouville theory important – it's because it enforces conformal invariance when you integrate over 2-dim gravity! So that must be a good reason why conformal invariance is an important part of the principles of string theory. However we wanted to make sure that this was a correct idea. So with Avinash, Narain, Jayaraman and some others we reproduced the Veneziano amplitude exactly! We started with 25 scalar fields coupled to 2D gravity using Polyakov's light cone gauge formalism and we got the Veneziano amplitude, including the measure. It was amazing. This was really the proof that at c=25 the Liouville mode emerges as a time coordinate in string theory.

AD: We wrote a paper with Sumit.

SW: Yes there was a follow up paper with Avinash and Sumit deriving general constraints on matter couplings dressed by 2-dim gravity. In this formulation there is no difference between critical and non-critical string theories. The couplings of the critical string theory were in one higher dimension that provided by the Liouville mode.

I think this whole conformal invariance business went on and on over many years. So that was the other obsession. After confinement, there was the question `what is string theory?'. What are the principles of string theory?

RG: You said the confinement problem was your main obsession earlier. And then it was string theory from the mid 80s. What was it about string theory that really gripped you?

SW: The fact that it is 'the theory of everything'... in physics. In some sense it gives you the framework to discuss particle physics and gravity. There was something unifying about it all. Conceptually unifying. Not maybe in terms of quantitative unification in which you can do a calculation. There is a notion of a conceptual unification which one should try to achieve. I think this was the attractive factor.

AD: Also at that time the atmosphere was such that people believed this was the theory that would solve everything. There was a lot of hype around what seemed like we had found the Holy Grail.

SM: And were particular people important for the hype?

SW: Witten was very convincing. But I think he was right since we all are still working in string theory!

AD: Yes. I think we were very influenced by Witten.

SW: So we started working on matrix models. We organized ourselves in groups.

SM: This is in which year?

AD: 1989.

SW: Yes. Now Anirvan Sengupta had also joined.

SM: You said you started working on string theory in 1984 and then till 1989 you were trying to understand the structure of conformal invariance.

SW: Exactly. The whole idea was how to get away from string field theory. And it was not only I. When I was at the Institute for Advanced Study beginning the fall of 1990, one of the nice things was that Witten would many times come after lunch to my office and we would go for a walk. I would ask him about lots and lots of things. When he started working on his famous paper on string field theory, he said that he wanted to see it go away.

SM: Open string field theory?

SW: Yes. But he didn't succeed and wrote a very nice paper on open string field theory.

During one of our walks I asked Witten why he wrote so many papers? He said that he had missed some things because he did not write them up. I have such a deep admiration for Witten; for his formidable abilities, his leadership and the poetic style of his papers. I asked him once whether he had read the Autobiographical Notes of Albert Einstein. He had not. So I told him that he should read them as they tell how Einstein relentlessly pursued fundamental questions of his times...over many years. In retrospect I feel very embarrassed that I said this to Witten. When he wrote his next few papers he would ask for my permission in jest!

SM: Sorry just one more point about this. Recently string field theory has found applications. Does that revise your view a little bit, or not?

SW: Of course it revises the view that it is useful. But whether I would work on it or be excited by it is another matter. For me, there are many other exciting problems in physics. Back to 1989 in TIFR: Avinash, Anirvan, Sumit and I wrote a paper, which was first to introduce double trace operators.

SM: Multi-trace operators in gauge theory?

RG: In matrix models.

SW: Yes in matrix models. Tr $(M^2)^2$ operator and how to work with it and what its implications are for new critical phenomena in random surfaces. Then Igor Klebanov and his students took it up did very insightful work, and there was also a famous paper by Witten in the AdS avatar. It felt good that we did something good and useful at that time.

Now something interesting happened. We were doing Liouville theory, right? And Liouville theory is related to matrix models, right?

Now comes the story of the c=1 matrix model. That occupied me for many years. This is the time when we had the meeting at the Tata Institute on Modern Quantum Field Theory.

AD: Matrix models. This was the first big meeting.

SW: Yes, David Gross, Frank Wilczek, Valodya Kazakov, Antal Jevicki and others participated… 1990 January I think. This was before I went to the Institute for Advanced Study.

AD: They actually announced their results on the double-scaled matrix models for the first time in this conference.

SW: It was this second [Matrix] revolution, so to say.

RG: Migdal...?

SW: Yes papers by Gross and Migdal, Douglas and Shenker, Brezin and Kazakov discussed the double scaling limit of matrix models to describe continuum world sheet of the string ... The 1990 meeting was good because we were also led to work on the c=1 models as they presented a possibility to discover the principles of non-perturbative string theory in 2-dims.

Subsequently there were several papers on the double scaling limit of matrix quantum mechanics. I was delighted to see that the double scaling limit in the matrix quantum mechanics model is taken exactly at the third order phase transition point, but there was no mention of my earlier work. This bothered me, but I did not say anything.

RG: Fits in well with your earlier interest.

SW: Yes. So I started working on the excitations of the double scaled c=1 model with Anirvan. The first thing we found was that the fermions were relativistic near the Fermi surface and the physical excitation was a massless boson. We derived the effective action including the effect of the wall, which leads to a cubic interaction. There were similar papers on the subject from the western world. One of them was from Polchinski and the other was the Das-Jevicki paper on the collective field theory formulation adapted for c=1 following the earlier work of Jevicki and Sakita. Around the time I was in Chicago, Sakita and Jevicki had, expressed the matrix quantum mechanics in terms of density variables by a change of variable. I did not think that it was the exact boson representation of the c=1 matrix model. Today I know it is not, and it is the hydrodynamic limit.

In between there was work on the two-dimensional black hole. Why did that happen? Let me tell you about that. When I went to the Institute, Witten told me that Zeldovich was there the previous year and they had almost written a paper on how to solve the unitarity problem of black holes. That stuck in my head, I realized it was a very important problem. We had done matrix models; we had understood two-dimensional string theory in terms of the Liouville mode, so there better be a black hole here. So that thought was there. There was a seminar at Rutgers by Polchinski on the c=1 type of models. And he said that in two-dimensions the flat metric was the only solution, and I raised my hand. I mentioned in his talk that this is not right if there is a zero of the dilaton field, because that's the signature of the black hole...we [Gautam, Anirvan and I] knew that at that point. And then I told Gautam and Anirvan, drop everything. Let's find this black hole solution. Anirvan used a gauge in which the dilaton was linear and found the black hole solution. Gautam and I worked out the solution in the conformal gauge. We did this as we were driving on Route 27 in between Rutgers and Princeton.

SM: Also known as Nassau Street within Princeton.

SW: At that time I had an injured leg and I was at home all the time. I remember Sunil Mukhi visited me at home and he told me that there was a lot of excitement in the Institute about Witten finding the black hole solution. I went and told Witten about what we had done. I used to sit in one of these ground floor offices opposite the mailboxes. Witten would inquire now and then about the progress of our paper. As you know when he wrote his famous paper, there was a big splash. We wrote our paper before his and he quoted us. I asked him point blank one day, 'do you think we couldn't have done it?' Our motivation was matrix model and its natural connection with the Liouville mode that contributes the spatial dimension of 2-dim string theory. If the black hole solution is understood in the matrix model we could solve the unitarity problem. He said, 'no no it's not like that.' I still remember Witten's seminar afterwards and there was no place to sit in the hall at the Institute. Anyway, this is about the two-dimensional black holes and the motivation behind it.

As I mentioned before we were trying to understand the microscopic basis of the c=1 matrix model in order to gain insight at some of the principles of string theory. Polyakov had found an infinite spectrum of discreet states in 2-dim string theory. It is a very beautiful piece of work. How can one understand this from the matrix models? One clue came from the W-infinity symmetry algebra and its conserved charges that we had discovered in the matrix model. Avan and Jevicki also arrived at similar results but in the classical limit. Gautam and I told Witten about this result and he was *hazzar* excited. Then we continued working on the problem with Avinash and Sumit who were in India. And even though the Internet was there, the communication was not so great. We were trying to understand the discreet states of Polyakov in terms of the W-charges. It took many months of work before we had a reasonable understanding of these states in the matrix model. In the meanwhile Witten wrote his paper on the ground ring of the c=1 matrix model and he did acknowledge us for discussions.

We worked on two-dimensional string theory and matrix models for a long time, trying to find signature of the black hole solution we had discovered in the matrix model. But this did not work out.

We also constructed the map from the matrix model to 2-dim string theory, to which Avinash made an enormous contribution. It is a highly non-linear map.

SM: This is with the leg pole factors?

SW: Yes.

SM: This is still not completely understood, right? Experimentally you know what the map is but the principle behind the map is still unknown.

SW: It is a recipe that you give.

SM: Do you think this is an important problem for the next generation?

SW: I think so. Because if you can solve this problem in AdS/CFT then you will (I think) understand the variables of space-time and also the internal degrees of the freedom of the black hole. We have a good understanding of finite temperature field theories and the plasma ball configuration is a holographic image of the black hole.

SM: What I feel here is that often we tend to hide behind AdS/CFT, hide behind the fact that we can't solve the field theory. We don't understand AdS/CFT partly because it's strong coupling but that's bullshit, I think. Because in this one example where you can solve both sides completely, you don't understand the logic for the map? That makes it clear that you haven't understood something that is not a technical issue but a principle.

SW: Absolutely. I have some thoughts on this, but maybe this is not the time to discuss this further.

AD: Also in this case the map is written down because you know the other side.

SM: Exactly. But here you have this rich resource of having an example where both sides are solved, sounds like you should be able to mine that to discover the principle.

SW: Gautam, Anirvan and I had computed the boson S-matrix from the matrix model. You don't find the discrete states in the intermediate states! So, yes, we don't understand the map. I completely agree with you. These states are created by the charges of the W-algebra but we do not see them in the S-matrix! I agree with Shiraz, this is a very important problem to solve if you want to figure out the emergence of space-time and the internal degrees of black holes. We have discussed this many times.

Okay so this brings us to the time when I got fed up of string theory. Nothing was working [for us].

RG: Can I ask one question? By then TIFR had a large…

SW: Oh I see! You want to know how the string theorists got hired at TIFR?

SM: Yes, like Ashoke.

RG: And there was a growing international visibility... you mentioned the 1989 conference. Can you say a little bit about that?

SW: Yes. I knew I had to bring Sumit Das back because I knew him well and had a very high opinion of him.

I was not part of the decision-making as I was a fellow. The fellows were not part of the 'group committee'. Sumit was the first person I wanted to attract back. TIFR hired Avinash also... for all the work he had done in perturbative QCD. Then after some time we also hired Gautam because he was excellent and I knew that we needed a critical size to begin with. I had some difficulty with the induction of Gautam because he was my student. I knew from my Chicago days that hiring your own students is not something you do. But our circumstances were very different.

I visited Sumit in Fermilab. Both Sumit and Ashoke were post-docs there. I met Ashoke and spoke to him at length about coming to TIFR. By then he had already done some very beautiful work. I still remember - he was sitting in his office and his desk was almost empty. At that time there was the general impression that getting a job at the Tata institute was difficult. He said, 'Now I will have to work harder.' This was the first conversation I had with Ashoke Sen. I really urged him and I told Sumit as well to urge him to join TIFR. Sumit knew him very well – they were college friends. So it started like that. Then at the Institute [IAS], I overlapped with Sandip Trivedi who was a postdoc at that time. Atish Dabholkar was a postdoc at nearby Rutgers University. I urged both of them to come back [to India and TIFR]. I also met Shubha Tole with Sandip and encouraged her too to return to India after her post-doc.

This is how it was and I think we got really lucky. It was our good luck that people were working on string theory and related problems, and were interested in coming back. Then the next hire was Shiraz. He was in Harvard then. And I was in correspondence with Harvard. Shobo had asked me to do this. The correspondence

was basically with Andy Strominger. They didn't want to let him go. Then there was the issue of a joint appointment, and while Harvard was agreeable we had to get it approved at TIFR. We managed that. I knew more or less that once this guy is here he will not go back. I had that intuitive feeling somehow, just by talking to him. True, right?

SM: Yes true.

SW: Shiraz, you and our colleagues in Mumbai have a similar challenge sustaining the string theory activity. Shiraz has created all these incredible students – two of his students are our faculty [at ICTS]. This is an amazing contribution. Creating the ICTS has temporarily depleted the home institution. However this was never on my mind because the ICTS has separate and distinctly new missions.

Anyway, coming back to physics, after working hard on matrix models which did not yield the secrets of black holes I got fed up of string theory and started working on complex systems. Physics discovers simple laws but their manifestations are very complicated. We worked in this area for about two years.

SM: Where did this come from? Was it something you were always interested in? Or were there new developments?

SW: No. This had its seeds at the University of Chicago. The fact that Nambu, Leo and Chandra had made contributions to so many diverse areas impressed on me the fact that there is so much unity in what seem disparate areas of science. Leo even spent some time on urban planning. There was also Jack Cowan at Chicago, who worked on questions of biology. For me biology is an extension of physics except it is a much more complicated system that exhibits among other things reproduction. The living state is something we have to understand as physicists.

So I thought it was a great idea to work on something different. Biological systems are too complicated so we began work on the simplest system that exhibits complex behavior: cellular automata. We explored complex behavior at the edge of order and chaos that exhibits complex pattern formation and long-lived dynamical activity. We started working on these dynamical systems and most of the calculations were done numerically. My student Porus Lakdawala had a lot of insight in this area. We found the surprising result that the system under study can access ordered, chaotic or complex behaviour, provided one tunes the initial conditions. Few years later I got a letter from some mathematicians from Chicago that they had actually proved this rigorously. Our work was numerical and our finding could be rigorously established.

So this [diversion] was for about two years and Porus wrote his PhD thesis on some work that he did by himself, "On the computational complexity of symbolic dynamics at the onset of chaos". Porus is very deep thinking and philosophical ...but he didn't like string theory at all.

At the end of two years I was facing a problem– who am I in this subject? I also felt that I did not really have a solid background in computation theory or biology to make a mark in the subject. Fortunately by this time wonderful things were beginning to happen in string theory due to the works of Polchinski, Strominger and Vafa.

SM: So what fraction of the TIFR was doing the complex systems? Ashoke kept doing string theory, right?

SW: Yes, Ashoke did.

SM: What about Sumit?

SW: I think Sumit had left already. Or was he around? I can't remember that.

AD: Sumit was around. He had these collaborations that he was working on – with Jevicki.

SW: Yes that's right.

AD: He had veered off into a different direction.

SM: Inspired by matrix models.

SW: Yes. And I think Sunil Mukhi was working on topics like topological strings.

SW: Biology was something that I always wanted to do, even as an undergraduate. In St Xavier's College I had studied Watson and Crick's book and I used to visit the TIFR biology department and have lots of discussions with the biologists about how relevant physics is for what they were doing. There used to be big debates about this. I was always interested in understanding biology from the viewpoint of physics. And this time seemed like a good opportunity. Both my students Anirvan and Sanjay – work among other things on biology inspired questions. They arrived at doing so on their own.

SM: What does Porus do?

SW: He works in the Oracle Corporation in California.

RG: So with all these students you had, what was the interaction like?

SW: My students were all friends. Friends to the extent that one student of mine also punched me. [Laughter] Anirvan – he got all excited. We were at Parade Platz in Zurich and discussing the effective action of the c=1 matrix model. He got angry with me. So he just gave me a punch. Of course I returned it many months later. All my students were my friends and it was like a family.

RG: It must have been very satisfying to mentor these very good people?

SW: Oh yes. Most of the little we accomplished wouldn't have been possible if these people were not there. I worked closely with all my students. We didn't have very good post docs then, with the exception of David Tong, Allan Adams and Matt Headrick.

I just wanted to finish speaking about the physics first. So what were we talking about? Yes the work of Polchinski, Strominger and Vafa.

Strominger and Vafa made a decisive leap when they calculated the entropy of an extremal black hole by counting states and using Boltzmann's formula. This was a big development: the new degrees of freedom that Polchinski discussed, D-branes – account for black hole entropy! Even today if you ask me what is one of the most important achievements of string theory it would be the discovery of the new stuff (D-branes) and how the new stuff accounts for black hole entropy. It was unbelievable. It changed the direction of the subject and brought black holes back to centre stage.

We started further work on the modeling of black holes by D-branes. Gautam came to my office and said that now that there is a microscopic theory we could solve the problem of the asymmetry of time reversal and black holes. I said yes. So we tried to model the Hawking radiation using a variant of the Strominger-Vafa model to describe a non-extremal black hole. That's how the three of us (Gautam, Avinash and I) started working on trying to model Hawking radiation. As Strominger-Vafa in this calculation dealt with the ground state of an extremal black hole, the question was how to model the interaction between an external particle like the graviton and the brane bound state? So we had to study that. There was work done by Klebanov on the interactions of closed strings with branes. All that had to be understood.

The first calculation we did was in classical general relativity. When you scatter an s-wave off the black hole of Strominger and Vafa what is the cross section at zero frequency. We found that it was exactly equal to the area of the black hole horizon.

SM: You were the first to show that?

SW: Yes the first, in this model. Then we tried to reproduce this result in the D-brane model. We could reproduce the answer except for a coefficient that we couldn't fix from first principles. So we wrote our paper without fixing this constant.

When we sent our paper to the arXiv, I received a mail from a certain Juan Maldacena at Princeton University. I didn't know who he was. He wrote that they were doing similar things.

SW: Then I went on a sabbatical to CERN. My main focus for many years was how to obtain Hawking's formula entirely within the D-brane model. This was before Maldacena's groundbreaking work on AdS/CFT. With Fawad Hassan, who was a postdoc at CERN, I spent a lot of time on the D1-D5 brane system and the associated two-dimensional gauge theory with appropriate D-terms. If you set the D-terms to zero you get the moduli space of this theory that describes its low energy modes. Later on we learnt that this moduli space was related to the moduli space of a five-dimensional gauge theory. I persisted with this work because I was really keen on deriving the formula for Hawking radiation from first principles. There was a conference in ICTP where I gave a talk on this. At this meeting Maldacena also gave a talk.

SM: Which year was this?

SW: I don't remember now.

SM: Was this before his paper?

SW: Yes. I had a short discussion with him. I spent a lot of time trying to make solid the gauge theory-gravity correspondence so that we can compute the Hawking radiation coefficient. This issue we ultimately understood in a series of papers with Gautam and Justin using the AdS_3/CFT_2 correspondence. You have to fix the normalization of the 2-point function in the AdS/CFT correspondence in the sector that does not contain the black hole. This in turn determines the coefficient in the Hawking radiation formula. This important result is part of Justin David's PhD thesis.

As you say, we cannot prove the precise correspondence. Most people don't seem to care about this ... but this has always bothered me very much. This is why I spent many years trying to understand this coefficient.

After the D1-D5 system and the black hole project I spent some years working on the unitary matrix model that describes the N=4 gauge theory at finite temperature and reproduces the most important qualitative features of the tunneling between thermal AdS and the big black hole. We also *proposed* a correspondence of the small black hole – string transition with the large N transition in the gauge theory. This project formed the PhD thesis of Pallab Basu.

After some work on hydrodynamics in 2-dims with Justin David and his students, I started working on hydrodynamics with Shiraz, Sandip, Sayantani and Loganayagam. I am fascinated by hydrodynamics. For the c=1 matrix models collective field theory is the hydrodynamic approximation. We wrote a couple of papers. One was trying to make a dent in the most important problem of hydrodynamics – turbulence. That was our ambition and outlook at that time. That's why we did the stirred fluid-gravity correspondence.

Then came the Navier-Stokes equations where we showed that there is a universal scaling limit of any relativistic hydrodynamics that gives the non-relativistic Navier-Stokes equations. This correspondence also throws light on the symmetries of the Navier-Stokes equations. All that was very satisfactory. But the problem of turbulence is still at large.

In all these works I was working with super-smart students Sayantani and Loganayagam.

SM: Yes Loganayagam was the hidden author on that paper. He even submitted the paper on the arXiv but refused to sign his name on it. I couldn't understand why.

AD: Why?

SM: I think he felt he hadn't done enough work on it. But it's very hard to know with Loga.

SW: Yes, you can't figure out.

So that was the fluid dynamics work. Then came the Chern-Simons project with Shiraz, Sandip and their students and collaborators. I was working on it when the ICTS was being created. This project sustained me. I was working doubly hard, doing both things. Without this group at the Tata Institute and Shiraz, I don't know what the situation would have been like because continuing to work in physics was very important for me in order to go on working for the ICTS.

I think we did some nice work. Missed some important points. I was *hazzar* thrilled actually. It was immense fun working with Shiraz. The most gratifying part of my stay at the Tata Institute, the last part, was that. Thank you.

SM: Thank you.

SW: No really. I wanted to say it at some point. And it happened because our tastes match. In a sense our tastes are very similar, except that he likes supersymmetry a bit more. [Laughter]. It's great that it worked out that way. It was a lucky thing. All this very beautiful work on scattering theory – that was amazing, right?

SM: Yes amazing.

SW: You think you know about some problem but actually you know little [referring to the Aharonov-Bohm effect.]

SM: It's still largely unknown in the world. But it will come.

SW: I was thinking about this article I wrote on the talk I gave at the Salam memorial meeting. I haven't put it up on the arXiv. I was wondering whether we should write a review based on that. What do you think?

SM: Yes, we should.

SW: We should do this. The template is there and you are so fast that we will have the review within… [Laughter]

I will tell you one incident about Shiraz. This is concerning our paper on Chern-Simons matter theory on $S^2 \times S^1$, right? Here there is a new type of matrix model in which the density of eigenvalues is bounded reflecting the discreteness of the Chern-Simons fluxes. There was a difficult integral equation to be solved, along similar lines as the integral equation of the unitary matrix model, which I had done a while ago in Chicago. But I could not have done this new problem as fast. Within a day the problem was solved. The whole solution was there. Shiraz I think is just incredible. We are very happy that we have you.

This guy [Rajesh] just wouldn't come to us.

SM: Why not Rajesh?

RG: I will tell you when I am 65. [Laughter]

SW: I may not be around. This is not fair, you must say it now.

SM: Rajesh used to say all this about being in the heart of India, the Ganga. But now he has moved to Bangalore.

SW: Cauvery. Now he is near the Cauvery.

RG: So from the time when you came and till the time you are talking about, the string theory group had a certain visibility in the world. So how do you see this evolution? Is that a model for how other areas, groups could build?

SW: Yes certainly.

In retrospect I can say that while building institutions you have to be mindful about the times and about the yardstick for induction. If your bar is too high then there is a problem. The problem is that you cannot be sure, beyond a point, about the potential of people. Many in our group at the Tata Institute, did their best work while at the Tata Institute. So you need to have a good sense of what the person may do in the future. Even at ICTS, which we will talk about later, it has been a nightmare building the faculty.

The issue of travel is very important – going out and talking to people about the science you are doing in India and urging potential faculty to return. I think the other groups did not do enough of that. I will give you an instance that made an impression on me. In 1982, I was sharing a long train ride from Saclay to Paris with Ludwig Faddeev. We were talking and he said, 'every summer I come out to tell the world what we are doing in Leningrad.' That made an impression on me. That he went around telling the world what they were doing in Leningrad made them very visible. As a student I still remember his Loeb lectures at Harvard. My advisor would never accept a student without a test. So he asked me to read the Faddeev-Takhtajan paper on the Sine-Gordon equation. I don't know if you have studied that paper, but it is an extremely difficult paper for a beginning graduate student...the inverse scattering method, the Gelfand-Levitan-Marchenko integral equation etc. I just didn't know what to do. I didn't know how I was going to study all this and report to him. My friend Rashid Shaikh was a student at MIT, so I went there. I attended the Harvard lectures they were very inspiring. I learnt a lot from them. There I witnessed the confidence with which Faddeev was handling Sidney Coleman who was asking many questions. Faddeev's example influenced me a lot– that you go out every summer telling people in the best places what you are doing back home. This is how [I think] the postdoc position for Avinash at SLAC came about. I went there to give a talk and told Peskin about Avinash. I also told him that he wasn't my student. But then he said, 'it doesn't matter that he is not your student. You have shown him the right way.'

So the first great success of our work at the Tata Institute was Avinash going to Stanford as a postdoc. Both the work we did and that I travelled telling people what we did at TIFR helped in this. Before Avinash most theory group students went to Europe for their postdoc.

SM: In any subject?

SW: Yes as far as I remember. Please correct me if I am wrong.

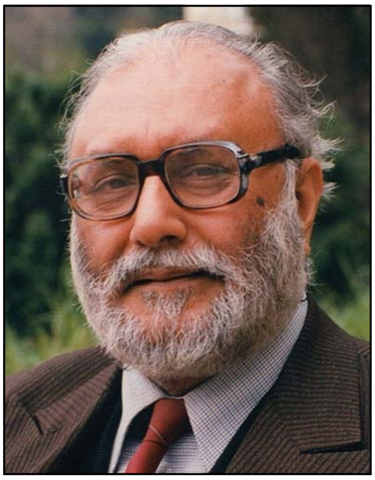
AD: I don't know this.

RG: No he is talking about post docs.

SW: Virendra Singh, S.S. Jha and others had gone as students. Bhabha had sent them to the best US universities for their PhD. I had noted this fact.

AD: Okay then I don't know of any case.

SW: Right. So we were very thrilled and excited. Some recognition was coming slowly. So travelling and talking about our work was very important. Salam played a key role in the early years. When we wrote our string theory paper, he invited me to give talks at the ICTP summer school in 1985. The other lecturers included David Gross, Curt Callan, and Lars Brink. After 1985, I went to ICTP almost every summer. I was at the back end of the organization of the ICTP schools. At that time Narain and Seif were not there. I always preferred to be at the back end, you don't want to get into the limelight being organizer and all that. What was great about



Abdus Salam

being associated with ICTP was that Salam was very encouraging and would also facilitate my travel from ICTP to other places. So when I was invited to give a talk at the Berkeley meeting on High Energy Physics and I didn't have any source of money for travel ICTP gave me the travel money. I still remember that when I went to the conference they asked me for my registration fee but fortunately for me Salam was coming that way. I told him about the registration fees. He said to them, 'this is daylight robbery'. He had a presence, with his coat and his hat. I thought that was amazing!

SM: They waived the fees?

SW: Yes of course. Every person in the TIFR string theory group was an associate of the ICTP at some point or the other. I also was a staff associate spending more time there. ICTP played a fundamental role in sustaining us in the early years. There was no support in the theory group for what we were doing. The only person who appreciated the importance of string theory was Virendra Singh. This is the reason why the others fell in line. You cannot hire somebody if Virendra Singh did not agree, right? There was no way. And the most difficult hiring was of Atish Dabholkar.

[Ashoke Sen enters]

AS: Is the recording going on?

SW: Yes you are being recorded. Sit down. We were talking about the hirings of the string group at the Tata Institute, and the one person who was convinced of string theory was Virendra Singh. He just put all the objections away. I still remember the core committee meeting for Atish Dabholkar. The Director (Virendra Singh) was there and all the files of people who were being considered for induction at that time were there. He brought Atish's file out and put it right on top and said, 'this is the best.' I just burst out into laughter. I didn't know what else to do.

AD: Yes we had made a lot of preparation.

SW: Yes amazing preparation.

AD: Spenta went for the meeting and came back and said he didn't need to do anything.

SW: I think he [Virendra Singh] played a very important role. The buck stopped with him. It is important to mention that he played a crucial role.

SM: Was it just intellectual appreciation? Or was it that string theory has its roots in S-matrix theory, did that play a role?

SW: I think both. He was the smartest guy and he knew what was going on. I remember in one of the Puri meetings we had invited John Schwarz, and Virendra Singh as the Director of Tata Institute gave the opening remarks. After the talk, John said, 'it's amazing. I don't know of any other director who can give this type of a talk.' So he was very good. I think it is important to record that if he hadn't agreed and appreciated we could not have built the string theory group… he also had enormous confidence in me.

Then Ashoke left TIFR. And I was shocked. I didn't even know what was going on. Then Sumit left for personal reasons, Atish left for personal reasons. By then Shiraz had come.

Now let me talk a bit about Strings 2001. String theory in India before and after that event is different. It gave enormous visibility to the string theory effort in India. That was an amazing meeting. There were so many threads to it. Earlier I had done the groundwork for it and David Gross, Curt Callan, Michael Green and Ed Witten showed enormous enthusiasm for holding this meeting in India. It would be the first time the annual strings meeting would be held outside of North America and Western Europe.

Mainly Atish, Sunil and myself organized it, and it was a truly backbreaking experience. David Gross strongly suggested, 'get Stephen Hawking'. So we wrote to Hawking and he agreed to come. How does one bear his expenses? Let me talk about one incident. I went to meet Air India's managing director Mr Jussawala to request a ticket for Hawking. It did not bear fruit. Then I called up Mr Rusi Lala at the Dorabji Tata Trust. He told me that they were committed to host a visit by Henry Kissinger. I asked him if I could speak to the chairman of the Dorabji Tata Trust. He said, how could I speak to the Chairman, and banged the phone on me? At that time my colleague Shyamasundar had invited Shibulal, an old student of

TIFR in computer science and one of the Infosys founders. He gave a grant for inviting distinguished visitors – the Sarojini Damodaran professorships. Prof Jha was the Director then, and he introduced me to Mr Shibulal. I mentioned to him the problem of funding Hawking's visit. He immediately made a phone call and in two minutes the matter was settled. Everything we needed for Hawking's visit was done. This type of quick assistance for a good cause, I had never seen before. That's how we fixed Hawking's visit. It was accidental.

Hawking's coming to Strings made headlines. Laxman had a cartoon of Hawking in the Times of India. Vithal Nadkarni and I sat in my office and wrote the front-page announcement for the Times of India. Mahindra & Mahindra provided a special van for Hawking. The meeting was great and lots of eminent people came. There is a famous photograph of Witten, Gross and Hawking in the almond grove. Lots of good things happened. There was plenty of publicity and string theory became known to the public in India. Before the meeting there were articles on string theory in leading newspapers. Three of us also wrote a popular piece, 'The hidden harmony of string theory'. I invited Gopalraj, a science journalist of the Hindu from Trivandrum to be the journalist in residence. He also wrote many articles. Strings 2001 was covered at that level in the Indian media. String theory in India had arrived in 2001. This was it.

What do you think Ashoke?

AS: Yes you are right.

SW: That it is possible to hold a meeting of that level was the main eye opener. People thought these guys must have done something. For the subject also there was some appreciation, some respectability. I used to hear in the beginning, words like, 'this is not science. These guys are not doing science.' I won't say who said this. So it was really encouraging to have this meeting.

SM: Did Gross, Witten meet people outside the string theory community, the scientific establishment? CNR Rao for example?

SW: No. At that time we didn't know CNR Rao.

SM: They didn't meet any politicians?

RG: Witten came to Kanpur, right?

SW: No he went to Bangalore and Infosys.

AS: They gave lot of newspaper interviews.

SW: Yes, Gross and Witten gave interviews. We also had public lectures by Gross, Witten and Hawking. That was an amazing event at the Tata Institute. Hawking also gave a public lecture at Sanmukhananda Hall. Four thousand people attended that lecture. It was like something had happened in Bombay. It was very encouraging that we put in so much effort and something good came out of it. The three of us worked so hard, you have no idea. We were all going mad. There was little secretarial help – we had to do the poster, write letters, invitations everything.

And of course I forgot to tell you another important thing: '*Paisa kahan se aayega?*' I already had my first grant from Miguel Virasoro [ICTP Director]. When I told him that we were planning to organize Strings 2001 in India, he said, 'Of course, Spenta. India deserves this.' He gave us 35,000 dollars. [Virasoro was a close associate of Bunji Sakita. He was his postdoc when he wrote his famous paper. I knew Virasoro very well.] He gave this money and we used it as security. Because without that how could we even begin to organize? We had this solid support from ICTP. Then came the support from the Clay Foundation.

SM: This was for travel.

SW: Yes. There are always people who help you out. Now I don't remember – it could have been Andy, Witten, Gross or Vafa.

AD: I vaguely remember you had said it was Witten.

SW: Perhaps. Then three of us had a long discussion with Arthur Jaffe. I think Witten knew Jaffe very well. So we did this interview after which Clay gave us 100,000 dollars. This plus the ICTP contribution plus Mr Shibulal's generosity formed the financial backbone of Strings 2001. TIFR contributed the usual small amount with which one could not have organized this kind of meeting. Initially there was no effort at TIFR to recognize the very special nature of this meeting. But we managed.

The experience of Strings 2001 gave me the idea that `survival' can be ensured if you raise funds from private donors and foundations. There are people who will give you money if they are convinced. So that's how Strings got organized. It was amazing.

Children from the TIFR Housing Complex spoke to Stephen Hawking, even my kids! Many a times I observed that Stephen Hawking is a very sensitive person. He knows that it is very important to encourage people and support our activity.

I think we asked Ashoke to give the final talk. Didn't we, Ashoke? But he refused. Then who did it? John or David?

SM: John.

SW: Yes John Schwarz. He [Ashoke] refused even this time. I think there will be another chance. [Laughs]

AS: I will be 85.

SW: So what?

So this was Strings 2001- a very important event in the history of string theory in India and in the history of TIFR.

After Strings in Mumbai, we organized a talk by Ed Witten in the Indian Academy of Sciences in Bangalore. I accompanied Chiara and Ed to Bangalore. They were staying in the Academy guesthouse. Any special attention given to them was met with a response 'don't worry about all that...we are very simple people'. I asked him if he wanted to visit the temples in Belur and Halebid. Ed said no, he wanted to visit the `temples of modern India'. He would like to visit the Infosys campus. Shibulal very kindly arranged this visit. We went to the Infosys campus and met Nandan Nilekani. This visit to the Infosys campus was a stunning experience for me. Have you ever been to the Infosys campus in Bangalore? This was of course in 2001.

SM: When it was more surprising.

SW: Yes. This visit made the point that it was possible to imagine that we can to create an international level campus in India? It was just amazing. Many of the lecture rooms were named after famous mathematicians and computer scientists and the architecture and ambience was special.

Okay so then what did I do? Between January and May I sent a proposal to Mr Narayana Murthy for setting up a new type of institute in India. I did not hear from him. In 2004 I mentioned to David Gross, about not getting a reply from Narayana Murthy. He told me, 'You don't get this kind of money from private companies. This institute has to be funded by the government.' I still have his comments and edits on the proposal to Murthy. Then we had two full days of discussions in Santa Barbara where he told me how to go about it. His understanding of these matters is just amazing: `the first thing you do is to have all the well-known Indian scientists support your proposal. [Laughs] Then carry all the people in the institute with you'. I knew that would be the biggest block. So slowly somehow I managed this. I remember this meeting with Shobo Bhattacharya [then the Director of TIFR] and Nitin Nitsure and another person from the Dean's office. He agreed to a theoretical sciences program at TIFR and some secretarial help will be given. There could be four faculty members. All within TIFR at Colaba – the idea of going out was not there at all.

In the last Council meeting of Shobo Bhattacharya as Director in October 2006, Prof CNR Rao asked me to make a presentation for a National Centre for Theoretical Sciences in the Tata Institute. Even before I started my presentation… CNR said, 'India needs a centre like this'. He was a visiting professor at the Centre for Material sciences at UCSB Santa Barbara on many occasions and he was familiar with the KITP in Santa Barbara (where David Gross was Director), which had a stream of international visitors. Ratan Tata the Council Chair and DAE Chairman Anil Kakodkar received the proposal very well. Both K. Vijayaraghavan (NCBS Director) and Mustansir Barma (who succeeded Shobo as TIFR Director) were present at this meeting. After my presentation the council gave an in-principle approval to create the centre and asked for a realistic budget estimate.

I felt that in order to realize the mission outlined in the proposal we needed to build new infrastructure in the Tata Institute. There was space to build a new building along the south wall that could have a very good guesthouse also. Another possibility was to build a novel architectural structure in the huge parking lot, which is not high and consistent with the architecture of the older buildings. However the proposal to create new infrastructure to accommodate the centre for theoretical sciences at TIFR Mumbai was turned down by Mustansir Barma (TIFR Director) and S. Ramakrishnan (Dean of Natural Sciences Faculty).

In the August 2007 meeting of the TIFR Council I presented the financial and other requirements for the Centre. During the presentation Prof Rao said, that the Centre must pay for business class air travel to distinguished scientists, and hence the travel budget needs to be enhanced. He also suggested that we call the Centre an International Centre. The Council approved the creation of the `International Centre for Theoretical Sciences of TIFR' at a suitable location in India. It would be a Centre like the NCBS with a Centre Director and the management structure would include a Management Board, Advisory Council and Program Committee.

After this approval we faced the question of where to set up the Centre. Now you are interacting with your `Motherland' for a piece of land! By now Avinash was already on board. We set out looking for places in India. First we went to NCRA, to

Rajaram Nityananda. They had a 2-acre plot to locate the Centre. This did not work out. Then we went elsewhere in Pune, waited outside the Collector's office with no success. I then went to Lonavala with some other people, looking for land and then to Mysore, with the then vice-chancellor of Mysore University. We met the physics department and they gave us 3-acres of land there.

SM: This is Mysore University?

AD: Yes. They had a very progressive vice-chancellor at that time. I forget his name.

SW: TV Ramakrishnan had introduced us to him. They showed us the land but the nicer part near the lake was given to the Indian Institute of Science. We also found out that the Indian Neutrino Observatory had also been given some land in the university. Then the vice-chancellor changed and everything went out of the window. Then we came here, to Bangalore to look for land.

AD: We went to Bangalore University to meet the VC there.

SW: Ah yes! K. VijayRaghavan sent us there. The vice-chancellor told us that the university campus is 2000 acres but they are inclined not give land because 'people were already saying why aren't you [the university] doing as well as the people you are giving land to?' I think he was referring to the Indian Statistical Institute that is located next to the university.

AD: He was really upset about it.

SW: In retrospect I feel it's very good that this didn't work out. Next I went to the State Government. Through the good offices of Renuka Raja Rao a friend of my wife Leena, we met Mr Sudhakar Rao, Principal Secretary to Chief Minister Kumaraswamy at the Vidhan Soudha. This was my first introduction to the state government.

One day Sudhakar Rao called me up and said, 'I have 50 acres of land for you in Bidadi (Ramnagara), on way to Mysore. The cost would be 50 lakhs per acre.

AD: This was the place where Sholay was shot!

SW: I went to Dr Anil Kakodkar and asked him how to respond. He told me not to say anything. Why should the central government pay state government? Land for ICTS should be almost free. That was the end of that. And good in retrospect as Bidadi would not be able to support ICTS.

After that we met the Mr. Vidyashankar, Principal Secretary, Science and Technology. He was a very friendly and helpful person. And in the tenure of the same chief minister, he gave us 30 acres of land in Bangalore.

AD: It was not far from here. He showed us a beautiful piece of land.

SW: What we didn't know then was that land is given basically by the Revenue Department of the State Government. That had not happened and the moment a new chief minister assumed office, the proposed site was not available to us.

SM: Given you meaning he wrote you a letter?

SW: No letter. There was nothing in writing.

AD: He showed us the land. And we had said this was good.

SW: Now I am going to say something that is important.

So all this is going on and I am at the Tata Institute. By this time Uma Mahadevan IAS Karnataka cadre, who had joined TIFR as OSD, was actively involved in working with us and played a pivotal role in our dealings with the DAE and the State Govt of Karnataka.

RG: This is roughly 2007?

SW: This was 2007-08. The Hyderabad campus was a possibility. The first team that went to investigate the possibility of TIFR Hyderabad included Ramky, BJ Rao, Chari and I. We met the additional secretary in government and then the vice-chancellor of Hyderabad University. After that visit I took a decision that Hyderabad was not the place where ICTS could be because Hyderabad did not have the eco-system to sustain a centre like ICTS. This point nobody understood or appreciated and the TIFR Director and Anil Kakodkar were putting enormous pressure on me. But I said no. So we were again trying in Bangalore. Then I met somebody who helped me a lot. And this was Dr Kasturirangan. He told me exactly what to do, whom to meet etc. He put me in touch with Mr Baligar Principal Secretary to Chief Minister who was an IIT Powai graduate, and he understood our cause and took us step-by-step forward. I was still a little unsure that Hyderabad was not the right place. It was a big decision for me.

RG: Especially when other options were not working out.

SW: Yes, and TIFR was putting enormous pressure. After a meeting with the Govt of Karnataka when it was clear to me that we will be allocated land for ICTS in

Bangalore, I called up Prof CNR Rao (who had also spoken with the Chief Minister about land for ICTS) from outside the Vidhan Soudha and explained to him my predicament, 'Prof. Rao, this is the issue. What do you think?' He said it should be Bangalore because of all the surrounding institutions. And that was the point that I had also internalized. For a centre like this to survive it requires the right environment. For a visitor-driven place, you need the right people and the right ecosystem. Also the sister institute NCBS was here in Bangalore. He also said, 'your problems are in your building in Mumbai. They are not anywhere else. When it rains put an umbrella over your head.' Even today I don't know what he meant by that. Since his view was similar to mine and for the same reasons, my doubts disappeared and I was convinced that ICTS should be in Bangalore. So we went for Bangalore and began preparing the Detailed Project Report. We were not experienced enough but we prepared the DPR that was approved by the TIFR Council.

Then we went to the Atomic Energy Commission of the Govt of India. The Commission consisted of Dr Anil Kakodkar, Chair of the Commission, Prithviraj Chavan, Minister of State for Science and Technology in the Prime Minister's office (who later became the Chief Minister of Maharashtra), Professor CNR Rao, Dr M. R. Srinivasan, Prof. Rama Rao, and Dr S. Banerjee (Director BARC), Principal Secretary to the Prime Minister Mr T.K. Nair, National Security Advisor – Mr M.K. Narayanan, Foreign Secretary Ms Nirupama Rao, Member of Finance Mr S.V. Ranganath (who later became Chief Secretary of Govt of Karnataka). There were also other members including Mr Prasenjit Mukherjee Joint Secretary DAE (vice-CAG today), Finance Secretary of Govt of India and the AEC secretary.

SM: That's really high power.

SW: I made a presentation in the AEC. There was a long and involved discussion. Prof CNR Rao was not present that day. The Commission was in favor of approving a Centre like ICTS in India but we needed to answer the question: why Bangalore? They told me to come back with a justification as to why ICTS should be located in Bangalore. I must record here the strong support of Dr Rama Rao who drew the parallel between ICTS and ICTP that was established by Abdus Salam in Trieste, Italy.

So we went back to Mumbai. It was a Sunday and Dr Kakodkar called me. He said that the Commission had previously never before turned down an agenda item. He once again tried to convince me that ICTS should be in Hyderabad. However I did not agree and we prepared a substantial document for the AEC as to why ICTS should be in Bangalore. For my next presentation to the AEC, I made sure that Prof CNR Rao was there. So he came and it was amazing. He said to the Commission, 'how can you look a gift horse in the face? Chief Minister [of Karnataka] has told me that he has given land to ICTS as a present.' So this is how the ICTS was approved by the AEC.

SM: CNR Rao is no longer part of AEC, is he?

SW: No.

SM: Is there anyone from science?

SW: Dr Kasturirangan and Prof. Rama Rao.

Then the next steps were the involved processes to select the architectural firm and the construction company. We needed to build the ICTS faculty and the find a temporary campus in Bangalore while the campus was being built.

Okay so now it was in Bangalore. They gave us approximately 19 acres instead of the promised 30 acres? The secretary said that later on we could build our housing and the government would give additional land for that. They had more land. I asked him to consult Prof Rao. I also requested Prof Rao to call the principal secretary, but that did not happen. We missed that opportunity. I feel we could have got a little more land if the circumstances were right.

With David Gross and CNR Rao during the Foundation Stone Laying ceremony of the International Centre for Theoretical Sciences in Bangalore

So once the land was there we needed to go for the architects. That took more than a year to get the approvals, to get the right architect. There was a competition. I made a presentation on the Centre and expectations from the architectural design. And I was so unrealistic. I gave them an architectural plan that came partly from the Roman villa I had seen in Granada and partly from some great institutions from around the world. We read many books on architecture. In the end, David Gross told me to be wary of architects. They tend to put emphasis on design rather than functionality. Ultimately there was a very good jury that made the selection. The architect was finally done. Then for construction I went to consult Cyrus Mistry of Shapoorji Pallonji, as they had built the Tata Institute.

SM: I actually remember this. You came back and described the office.

SW: Wonderful office with several original Husain paintings. Mistry was very nice actually. He is a very smart guy. He was very helpful and put us in touch with people who were willing to advise us. But somehow I felt that those people were not artistic enough or good enough for ICTS. And anyway there had to be a competition for the selection of the construction company. They bid, but lost. This is how JMC got the construction contract.

Now we had to look for a temporary campus. At that point I was shuttling between Bombay and Bangalore. We tried very hard to get something in NCBS but Vijay was a bit reluctant. I had this feeling that he wanted me to find my own feet. The Advisory Council during its meeting in Bangalore in December 2009, strongly recommended the need for a transit campus in Bangalore while the main campus was being built. Then I realized that we had a TIFR Centre inside the IISc campus and the mathematicians were moving out to their new building in Yelahanka. We arrived there just in time to get one-corridor in the TIFR Centre. So in 2010 we began activities in Bangalore at the TIFR Centre in the Indian Institute of Science, much like Bhabha had started TIFR in the same institute in June 1945. Prof Balaram, the then IISc Director supported our temporary stay in the IISc campus.

The next few challenges were building the campus and its infrastructure, organizing programs in Bangalore, creating the ICTS faculty and admitting students and postdocs. I thought that beyond the buildings and architecture, the most important core of ICTS would be its faculty. Without the faculty there would be no ICTS. So most of my mental energies were spent there. How to attract people in diverse areas and convince them that ICTS has a great future, which in turn will depend on them. Slowly, slowly we built up the faculty.

While we were in the IISc, we were initially greatly helped by the NCBS and TIFR-CAM with regard to the management of administration and finance. I would like to put on record the enormous support provided by Prof. K. VijayRaghavan, Director, NCBS to the fledgling institution. The IISc Physics Department was a source of tremendous help and encouragement. Prof H. R. Krishnamurthy, then the

Chair of the Physics Department and Prof. Rahul Pandit, the divisional chairman for Physics and Mathematics at IISc were strong supporters of the ICTS.

Once we started the construction we realized that the original plan needed to be revised and expanded. This led to a 67% cost revision, which was approved by TIFR and the Govt of India. C.B.S. Venkataramana, Additional Secretary of DAE, successfully led this effort.

I was working on somebody [Rajesh] to come to ICTS and he finally came. [Laughter] I thought he would be the right person here. I also talked a lot to Rukmini in Hyderabad.

I constituted the International Advisory Board of ICTS in consultation with David Gross. That was one of the most important things I did, because I felt that when you do new things advice from outstanding people is very important. Clearly it paid off very well and David Gross was always there, advising me about everything. I always felt there was someone non-trivial as a *saathi* in a difficult and unknown terrain. I think India is a very difficult country... if you want to do anything different and new, there is always a reaction. It's not like the response will be *chalo*, let's do it because it is good for the country and good for the humanity as well. I must say my experience has been very difficult. Most people do not want to give you the space needed to do new things. This attitude took me by surprise.

SM: If you were doing things all over again for ICTS, what would you have done differently?

SW: I wouldn't do it again. [Laughs]

There is a fundamental problem with the Tata Institute system that has nationally distributed institutions. I think the way we are presently organized is working against us. TIFR needs to reinvent the way it is organized and administered. In his writings and speeches Homi Bhabha has already touched on matters of infrastructure and administration for an institution like TIFR.

SM: Spenta, coming back to something completely different, I wanted to ask what your views are about the state of research in high energy physics? What do you see as the interesting open problems?

SW: You mean the state of string theory or the state of string theorists in India?

SM: No not India. Internationally.

SW: I think string theory is a flourishing subject. But what you mean by string theory can be highly flexible. Today in string theory we are studying even random systems. We are looking at systems like spin glass. That's fantastic. Some amazingly talented people do string theory. I think it will always be wonderful – exploring the space of theoretical physics. But what it has to do with particle physics is a different issue and particle physics itself is in a difficult situation. The standard model seems to be doing well, but we know on theoretical grounds that the standard model has limitations. The hope was to discover supersymmetry but even those who had invested a lot in the search for supersymmetry (e.g. John Ellis) say that one has to wait and see. Perhaps the next few years of data collection and analysis may spring a surprise. So particle physics is certainly in a difficult situation vis-à-vis what to expect at the LHC. Also other very sensitive underground experiments looking for dark matter have not yielded results.

SM: Because you know it is there.

SW: Absolutely. We don't know if supersymmetry is there. It is one theoretical solution to the hierarchy problem. I am not very worried about all this. I feel good people will always do interesting science.

SM: The other question was about the Indian string theory community. How do you see it growing over the years?

SW: I think we had a very good slope at one point – most probably a historical accident. Whatever you say, even before one goes looking for people they have to be there first! This is a very difficult time for string theory in India. I am talking about outstanding people, and that's what you need. How many promising young people can you name? I don't know what the solution is except produce very good students and hope that they go and do something important and then come back. One good development is that string theory today in India is done at many more institutions than before and this is a positive sign for the subject. Higher numbers means you will have statistics on your side. So I am very hopeful.

Session 2 [Venue: Tata Institute of Fundamental Research, Mumbai]

Ananya Dasgupta: Can you tell me a bit about the new initiatives at ICTS?

Spenta Wadia: In ICTS we did a very important experiment. I hope people pick up the threads from that. We set up a big data pipe between the United States and India via the GLORIAD network. GLORIAD is a network for big data science. So about 7.5 terabytes of data was accessed during that one year during which the link was alive. It was a gift from Tata Communications. My wife Leena had a lot to do with this. She was part of the organizers of a meeting on computing in high energy physics while we were both in CERN and knew all the important people. Then she met the CEO of Tata Communications. He told her that they wanted to set up a big data link to facilitate research in India.

The link was supposed to be between TIFR Mumbai and the rest of the world. The GLORIAD people even sent a particular type of server. But that server was just lying downstairs here. It was supposed to connect to the National Knowledge network and the people here were reluctant to do it because it was from the US.

So I took the server to Bangalore and put it in TIFR-CAM because we were still functioning out of IISc.

The thought for doing this was inspired by a meeting we had in ICTS on big date science, where some of the most eminent people came. Big data in various sciences – astronomy, high energy physics, computer science, earth sciences. The meeting was held at the Jawaharlal Nehru Centre (JNCASR) because we didn't have a campus at that time. Amit Apte played a very crucial role not only in convincing people that this is the science for the future but also working a lot to make all this happen. For me as well it was getting to know of a new dimension of science.

In our type of field (string theory) we do analytical calculations by hand but large parts of the scientific and engineering enterprise all require large scale computing even though mathematical methods form the foundation.

Also how does one make sense of the immense amount of data that needs to be made sense of in various areas like meteorology, astronomy, high-energy physics, biology etc.? There is a lot of mathematics and computing involved to access the truth in big data. I think it is very important for ICTS to support some aspects of big data science.

ADG: Big data science is not studied so much in TIFR?

SW: No not really. The only two areas where big data is generated at TIFR are in astronomy at NCRA and in biology e.g. in genetics and protein dynamics.

Other institutions like the IISc also have a 'big data initiative'. We did it before them or around the same time.

In ICTS we have a very modern data centre with immense potential. The centre is not air conditioned as the machines are all internally cooled by passing cold water. It's like the radiator of a car. So it is a comparatively low cost data centre.

My dream is that the ICTS will become a big institution. It is doing very well and hence it should grow and grow and grow. It should augment all these facilities and do big things.

Why don't we create more big institutions in the country? If they [institutions] are capable of doing it, then why not? It's a tough job but is not impossible. I also feel the NCBS should grow into something much bigger. It should get out of its pure biology mode and get into systems science, big data science. Biology is a complex system, which needs ideas and tools from all across science and engineering to make sense of it. And I know that this is happening in some of the best institutions in the United States. As an example people who are working in neuroscience are thinking of similar mathematical problems as those who are trying to explore the `landscape' in cosmology and spin glass. People working in computer science and neuroscience ask about efficiency of computing and analysis of big data. One approach is using random matrix theory and this is something we also work on. You see the connection? Between statistical mechanics, computer science, biology, neuroscience? I (and VijayRaghavan) wanted ICTS to be next door to the NCBS, which is mainly a place for experimental biology. But this did not work out.

Also, NCRA should grow big. All these small 'start-ups' should become big institutions that would train a large number of students and postdocs. In this way Tata Institute would have given rise to many big institutions in India. And that's the way it should be.

It's a grand dream, but why not? You started something – it should grow, right? But the attitude of a lot of people in Colaba is that this [ICTS] is small and should remain small. But a child grows, doesn't it? It doesn't remain small all its life. If it's doing well then you have to let it grow.

ADG: You were mentioning the other day that it might have been better if the ICTS was a separate entity.

SW: Yes. It's very difficult to manage TIFR as a nationally distributed institution. There are inherent difficulties and limitations in the present model that just happened and was mainly born out of a lack of real estate in Colaba. How does one get out of this? One way is that centres that clear a bar of excellence should all enjoy a certain well-defined independence and manage their research, education and finance within a new framework. We can all remain friends but those whose growth will benefit the nation in terms of research and education we need to grow.

So we have a very big opportunity that the Tata Institute would give birth to many big institutions.

This is the way I dream about the future. This is the way it should be. The restrictions on number of faculty members and administrative staff are a given and determined by static rather than dynamic planning. But as I grow up don't I eat more food? This is exactly like that. The off-springs of the Tata Institute should be allowed to grow to their fullest potential. That way we can serve the institute and the nation better.

In the new model, which also reinvents the composition and role of the TIFR Council there could be a 'Director General' looking after the overall wellbeing of the institute. With renewed enthusiasm we can excel and we can even attract private funding and do a lot of good things.

ADG: I guess the private money is a very important part of this.

SW: Yes. First, there is no culture of such things in the country. Second, the people who give money have told me directly that they will not give a big endowment for us to manage. They are ready to give grants that they will continue to manage.

ADG: But why?

SW: This is because most institutions in India, according to them, do not know how to manage money. They don't know how to invest and they are not accountable. They just take the money and disappear. I have had many long conversations with Mr Narayana Murthy about these matters, about the ICTS and about how to create a type of ambience that makes an attractive institute. I have found him to be extremely sensitive, caring and wise. He has his views but I have found him always very encouraging. I have talked to many people so I can see the difference.

ADG: I think Infosys must be one of the very few business houses in India that supports science.

SW: Yes. But there are also others who support science but mainly research in chemistry and biology that is closer to immediate applications. But that is very different from us. We are doing science in which we value the serendipity of discovery, not necessarily a discovery made with an intention. Most important discoveries in science have been serendipitous, including the discovery of the electron that is the basis of the chemical bond and chemistry. I think very few people appreciate this and are always asking for deliverables. Including Mr Ratan Tata, who also feels this way. He thinks we have to do what the donor desires. I was in the Council meeting where he said this. So I agree and also disagree with this. There has to be accountability. There have to be some parts of science that deliver. But you also have to encourage people who engage in fundamental research, without any intention of application. History has shown us that these correlations are weak. Maxwell did not complete his equations with the intention of predicting light, thinking that some electricity company will benefit. It's amazing that what the human mind creates and understands about nature somehow becomes useful. Today we use General Relativity for the GPS! This is the amazing thing about science and the logical structures we create to understand the world around us.

Many people have thought about the unreasonable effectiveness of mathematics in the physical sciences. There is a lecture or an article written by Eugene Wigner - why are the structures we create like algebra, geometry, number theory relevant in the sciences? These are questions I relish thinking about.

[The discussion was coordinated by Ananya Dasgupta]

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