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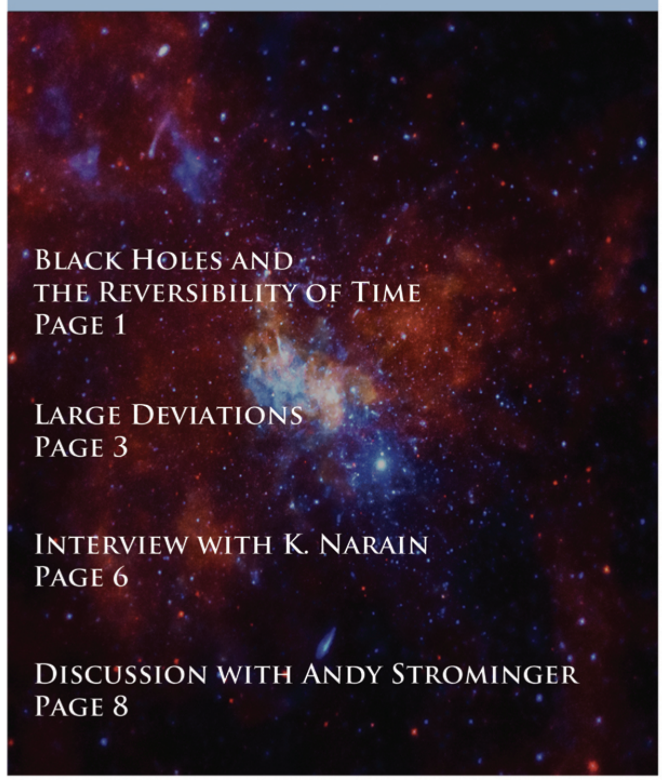

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This is the tenth year since ICTS came into existence on 2nd August 2007. The first program was held in September 2007. ICTS has come a long way since then, having grown to have 16+ faculty; having held over 150 programs and discussion meetings and moved to its new campus in Bangalore. To reflect on the journey thus far, and to go ahead with renewed energy onto the next decade, we are organizing a scientific meeting – 'ICTS at Ten' at the ICTS campus during 4-6 January, 2018.

There will be broad perspective talks across different themes in the theoretical sciences: 1) Astrophysics 2) Condensed-Matter and Statistical Physics 3) Mathematics 4) Quantitative Biology 5) Theoretical Computer Science and 6) String theory and Quantum Gravity. These reflect ICTS' present profile as well as the directions to grow into in the coming years.

The speakers include Nima-Arkani Hamed, Sanjeev Arora, Leon Balents, Manjul Bhargava, William Bialek, Sourav Chatterjee, Jennifer Chayes, Surya Ganguli, David Gross, Jonathan Howard, Shri Kulkarni, Mahan Mj, Ramesh Narayan, Joel Moore, Christos Papadimitriou, David Reitze, Nathan Seiberg, Ashoke Sen, Madhu Sudan, Mriganka Sur and Nisheeth Vishnoi.

On the 7th of January there will be a Public Lecture by Robert Dijkgraaf on the theme 'The Usefulness of Useless Knowledge' followed by a panel discussion. Among the panelists are Manjul Bhargava, Jennifer Chayes, Robert Dijkgraaf, David Gross, Narayana Murthy and K. VijayRaghavan.

Black Holes and the Reversibility of Time

Suvrat Raju

Almost immediately after general relativity was formulated by Hilbert and Einstein in 1915, the German mathematician, Karl Schwarzschild, discovered an exact solution to its equations of motion. This is now called the Schwarzschild black hole. *(See Illustration 1)* The defining feature of this solution is that it divides spacetime into two parts: the 'interior' and the 'exterior' of the black hole. While observers from the exterior can, in principle, enter the interior, nothing from the interior – not even light – can cross over to the exterior. The boundary between these two regions is called the 'black hole horizon.'

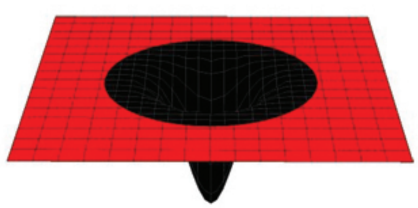

Illustration 1: A schematic description of a black hole spacetime. The sharp boundary between the black region and the red region is the black hole horizon. At the center of the black hole, we have a "singularity" indicated by a deep pit in spacetime. The singularity will not be relevant in this article.

For a spherical Schwarzschild black hole of mass M, the radius of the horizon is given by $r=\frac{2GM}{c^2}$, where G is Newton's constant and c is the speed of light. A black hole with the mass of the Earth, for example, would have a horizon with a radius of about 1 cm.

This feature of black holes sometimes leads to the colloquial observation that a black hole is a region where gravity is so 'strong' that not even light can escape. However, the true nature of the horizon is far more subtle and at the horizon of very large black holes, gravitational effects may not be strong at all. For example, last year, astronomers detected a supermassive black hole, about 150 million light years away, and estimated that it was 17 billion times as massive as the sun. A spaceship could freely coast through the horizon of this black hole without any immediate ill-effects. In fact, the tidal effects due to gravity would be so small that if the spaceship had an aluminum rod with a length of 1 m then gravitational tidal forces would distort the length of this rod by less than an attometer (10^{-18}m) as it crossed the horizon.

Nevertheless, try as it might, the spaceship would never be able to return to ordinary space. It would inexorably fall deeper into the black hole and in, at most, about three days it would crash into the singularity at the center. The fact that the horizon may be locally uneventful, while nevertheless being a point of no-return, is a striking feature of the Schwarzschild solution.

The Schwarzschild black hole solution is a 'static' solution and, by itself, it only describes a somewhat boring Universe in which a black hole has always existed. But black holes can also be formed from the collapse of matter. A simple solution that describes the collapse of a ball of dust into a black hole was discovered by Oppenheimer and Snyder in 1939, bringing to fruition a program initiated by B. Datt, working in Calcutta, in 1937. A few years later, in 1943, P. C. Vaidya (working with V. V. Narlikar at the Banaras Hindu University) presented an even simpler solution describing the formation of a black hole starting with a cloud of pure radiation. All of these solutions have various transient features as the black hole is forming, but after a while settle down to the simple time-independent Schwarzschild solution. [1]

In the seventies, physicists started conducting *thought* experiments with these black-hole solutions.

For example, imagine a process where one creates a black hole through collapse, allows it to settle down, and then throws in some more matter to induce a transition between two time-independent solutions.

[1] *More generally, the final solution may carry charge and angular momentum, in which case it is described by a generalization of the Schwarzschild solution.*

(See Illustration 2)

Remarkably, it turns out that such quasi-static processes obey a set of simple equations that are analogous to the laws of thermodynamics. More precisely, in any such process, the change in the mass of the black hole is related to the change in its area and the work done on the black hole by external forces through

$$dM = \kappa \frac{dA}{8\pi} + W$$

where κ is a constant, called the surface gravity. Moreover, while it is sometimes possible to decrease the mass of a black hole, by extracting some of its energy, the area of the black-hole horizon always increases

$$dA \geq 0$$

At first sight, the analogy between these equations and thermodynamics might simply appear to be another instance of the fact that different physical systems are sometimes described by the same equations. But, in 1975, Hawking argued that this analogy was not merely formal and that black holes genuinely had a physical temperature and entropy. Using simple and robust arguments, he showed that black holes would emit black-body radiation with a temperature proportional to their surface gravity. For a Schwarzschild black hole of mass M, the temperature is

$$T = \frac{hc^3}{16\pi^2 kGM}$$

where h is Planck's constant and k is the Boltzmann constant. The first law then tells us that we should also associate a physical entropy with the black hole,

given by $S = \dfrac{\pi kAc^3}{2hG}$ where A is the area of the

horizon of the black hole.

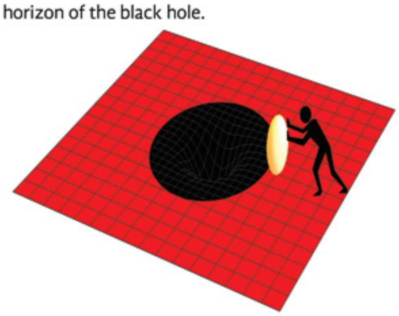

Illustration 2: A sufficiently advanced civilization could throw stars into black holes and observe how the horizon responds.

The temperature of astrophysical black holes is very small: a solar-mass black hole would have a temperature of only. 6×10^{-8} *K*. So, in most astrophysical contexts, Hawking radiation is not only unimportant but effectively unobservable.

However, from a philosophical point of view, the consequences of Hawking radiation are rather dramatic: the horizons of black holes are not really black but radiate weakly. This modifies our picture of the horizon to the one shown in *Illustration* 3, where we see that quantum effects cause the horizon to glow faintly with Hawking radiation.

The existence of Hawking radiation leads to the following question. Consider a black hole formed by the collapse of a cloud of dust. This initial cloud could have many distinctive features, such as a density profile, and these features can encapsulate information. However, soon after collapse, the black hole settles down to a simple Schwarzschild black hole. In an empty Universe this black hole would gradually lose energy through Hawking radiation with a temperature determined only by its mass. By the time the black hole has evaporated completely, one would be left with a gas of the emitted radiation that would know about the mass of the black hole but nothing about the details of the collapse-process that led to its formation.

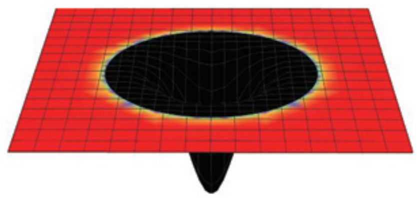

Illustration 3: When quantum effects are added, the horizon of a black hole emits weak black-body radiation

A little thought reveals that this is in contradiction with a venerable principle of physics: the reversibility of time-evolution. Both classical and quantum physics conform to the notion that if one knows the full state of the Universe at one time, then one can evolve this state either forward in time or backward in time. However, in the setup above, it is clearly impossible to reconstruct the initial cloud of matter given only the final radiation. Therefore, if the argument above is correct, then it would appear that the reversibility of time-evolution is violated by the formation and evaporation of black holes. This is called the *information paradox*

The information paradox, like Hawking radiation, is not relevant for astrophysical black holes. In the first place, astrophysical black holes absorb more energy from the cosmic microwave background radiation – which, at a temperature of 2.7 K, is much hotter than they are – than the energy they lose through Hawking radiation. Even disregarding this fact, in an empty Universe, a solar mass black hole would have a lifetime of about 10^{67} years, which is many orders of magnitude larger than the age of the Universe.

However, nothing prevents us from considering smaller black holes. For example, the lifetime of a black hole with a mass of about 400 tonnes is roughly 5 seconds. We currently lack the technology to produce such small black holes but we don't know of any principle that would forbid such technology. Therefore the information paradox does lead to an important question of principle that deserves a straight answer: can quantum mechanics, which is inherently reversible, be combined consistently with the theory of general relativity?

The 'anti-de Sitter space/conformal field theory' (AdS/CFT) correspondence, which was discovered in 1997 by Juan Maldacena, proved to be a significant step towards resolving the information paradox. This correspondence posits that a certain toy model of quantum gravity is exactly dual to another quantum field theory that does not contain gravity. More significantly, the formation of black holes in the theory of gravity is mapped to the *thermalization* of ordinary matter in the quantum field theory.

If this is correct, then the fact that black holes appear to be featureless regardless of the initial state that led to their formation is not surprising at all and maps onto a well-known phenomenon in quantum statistical mechanics. If one starts with an ordinary statistical system in a special state, and allows it to thermalize, then all the features of the initial state are gradually lost. This does not mean that information is lost; it simply means that information is transferred from degrees of freedom that are easy-to-access to other degrees of freedom that require more delicate measurements.

When applied to black holes, this results in the following resolution to the information paradox: information about the initial state is stored in very delicate correlations between the final emitted Hawking quanta. This resolution suggests that Hawking's initial calculation led to an incorrect conclusion because it was not precise enough to keep track of these delicate correlations.

A cartoon of this resolution is shown in *Illustration 4*, which shows an equal number of purple, red, blue and yellow dots representing Hawking photons placed randomly about a large black dot representing a black hole. At first sight, it might appear that this pattern contains no information. But, on closer examination, various delicate correlations emerge. For example, as the solid green lines show in the subfigure on the right, a blue dot is always diametrically opposite a red dot and a purple dot is always diametrically opposite a yellow dot.

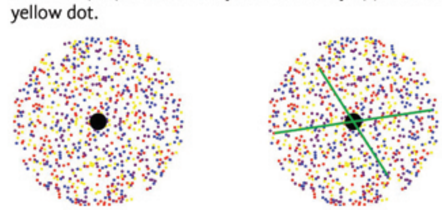

Illustration 4: (Left)A cartoon of photons emitted by a black hole. (Right) Despite initial appearances, the dots have delicate correlations.

These illustrations display an example of 'two-point' correlations. The AdS/CFT correspondence suggests that the correlations in Hawking radiation are far more delicate, and emerge only when we consider S-point correlators, where S is the entropy of the black hole.

For a while, these arguments appeared to have settled the information paradox. However, in 2009, Samir Mathur, at the University of Ohio, argued that this resolution was too quick. Mathur examined a large class of small corrections to Hawking radiation, and using arguments from quantum information theory, he argued that these corrections could not generate the correlations in Hawking radiation that were necessary to preserve the information present in the initial state. His argument was later elaborated by Almheiri, Marolf, Polchinski and Sully (AMPS) and has led to a significant revival of the discussions on the information paradox.

This has also been the focus of some of my recent work, with Kyriakos Papadodimas at CERN and several other collaborators, including Sudip Ghosh at ICTS, Jan-Willem Bryan at Groningen and Souvik Banerjee at Uppsala. We pointed out that Mathur and AMPS tacitly assumed that quantum gravity was an exactly local theory. We were able to show that tiny nonlocal effects which, in ordinary observations of the black hole would be suppressed by a factor of e^{-S} could, in fact, generate the correlations that were necessary to preserve information.

We do not know, for sure, whether such nonlocal effects exist in our world. But we were able to demonstrate their existence in some simple settings, within the AdS/CFT correspondence. A simple way to understand these nonlocal effects is shown in *Illustration 5*. This illustration shows a spacetime diagram of anti-de Sitter space. Here, time runs on the vertical axis. The spatial part of anti-de Sitter space can be thought of as a disk as shown in the illustration.

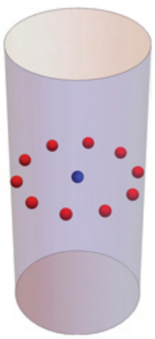

Illustration 5: A local measurement at the point marked blue can be reproduced by multiple local measurements at the red points

The nonlocal effects that we posited would imply that a 'local' measurement in a theory of gravity at the blue marked point could be reproduced by a sequence of measurements at the same time on the red-marked points.

If our argument is correct, this brings us back to the picture that information is preserved in black-hole evaporation through delicate correlations between the Hawking quanta. The mechanism that generates these correlations in quantum gravity is more exotic than the local interactions that operate in ordinary statistical systems. But, the net result is the same: when we carefully keep track of all degrees of freedom, the process of black-hole formation and evaporation is consistent with the reversibility of time-evolution.

From a philosophical perspective, this loss of locality is rather dramatic. It requires a significant departure from the ontology of classical general relativity where the degrees of freedom in one part of space are entirely separate from the degrees of freedom in another part.

One nice way of thinking of the ontology that results from taking these quantum effects seriously was suggested by Mathew Headrick (Brandeis). Perhaps one should think in terms of spacetime as a dense bundle of threads as shown in *Illustration 6*. Consider a set of observers who can carefully observer all the threads that emanate from a point in space. Through these observations, they can determine what is happening at that point instantaneously, without visiting that point directly.

But, for most purposes, it is not convenient to keep track of all the distinct threads, and the description of spacetime as a smooth fabric with exactly independent parts provides an excellent approximation.

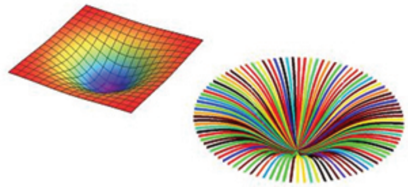

Illustration 6: General relativity suggests that we should view spacetime as a smooth fabric as on the left. But lessons from quantum gravity suggest that, for some purposes, one should think of spacetime as made up of a dense collection of threads as on the right.

So the recent discussions on the information paradox appear to have some bearing on our fundamental notions about spacetime. It is true that this paradox and the effects that are under discussion here – like many other questions in quantum gravity – are very delicate and currently unobservable. So it is important to ensure that the results of our thought experiments reflect reality, and not just our metaphysical prejudices!

But one of the attractive aspects of this field is that this discussion can be phrased entirely within a framework that is based on simple and well-established principles of gravity and quantum mechanics. This suggests that, by thinking about these problems, we might have a chance of using a process of inference to discover something truly interesting and profound about our world.

Suvrat Raju is a theoretical physicist and a faculty member at ICTS-TIFR.

Large Deviations

Satya N. Majumdar and Gregory Schehr

Extreme events such as earthquakes, tsunamis, extremely hot or cold days, financial crashes are rare events. They do not happen everyday. But if/when they happen, they can have devastating effects. Hence it is of absolute importance to build models to estimate when such catastrophic events may occur, and if they do, the amount of damage, i.e, the magnitude of such events. A first and basic step towards building such models is to study the existing statistics of such rare events and to construct a 'tool' that describes these extreme statistics well.

For example, suppose we look at the data of the height of water level of a river. One can easily construct a histogram of height (empirical probability distribution) from the available record. Typically they have a bell-shaped form, with a peak around the mean water level. The probability of small 'typical' fluctuations around the mean are often well described by a Gaussian form. This can be understood using standard tools from probability theory, such as the central limit theorem (CLT). However, we are interested in rare events (e.g., floods or droughts) where the typical height of the water level is much above (or below) the mean level, i.e, with very large fluctuations from the mean. These events are characterized by the tails of the histogram. The probability at these tails can be as small as 10^{-9} (one in a billion events!). How do we describe such tails? The CLT does not hold far away from the peak, and to describe these extremely small probability at the tails, one needs a new tool. The 'large deviation theory' provides precisely such a tool.

To illustrate this idea, let us start with a concrete example. Imagine that we have N unbiased coins and we toss them simultaneously. We record the outcome of each coin, which are either head 'H' or tail 'T'. In each trial, we count the number of heads N_H, which can be any number between 0 and N. Indeed, N_H fluctuates from one trial to another – thus it is a random variable. Let $P(M,N)=Pr(N_H=M)$ denote the probability distribution of N_H. Given that each outcome can be 'H' or 'T' with probability 1/2 each, it is clear that P(M,N) is given by the binomial distribution,

$$P(M,N) = \frac{1}{2^N}\binom{N}{M} \quad (1)$$

If we plot this distribution as a function of M for a given N (see Fig. 1), this histogram has a bell shaped form with a peak around the mean $\langle N_H \rangle = N/2$.
One can also compute trivially the variance of N_H, which is given by

$$\sigma^2 = \langle N_H^2 \rangle - \langle N_H \rangle^2 = \frac{N}{4} \quad (2)$$

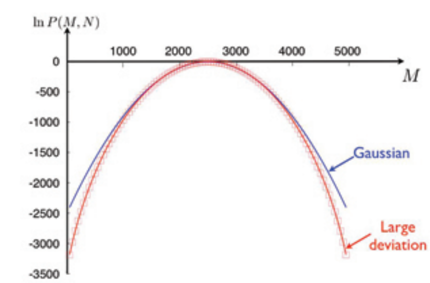

FIG. 1. Plot of ln P(M,N) given in Eq. (1) as a function of M (square symbols), for N=5000. The solid blue line corresponds to the typical Gaussian fluctuations in Eq. (3), which describes well the exact curve in the vicinity of M=N/2=2500. The solid red curve passing through the square symbols corresponds to the large deviation form in Eqs. (5), (6) and is almost indistinguishable from the exact formula given by square symbols.

This indicates that the typical fluctuations of N_H around its mean are of order $\sigma \approx \sqrt{N}$. Moreover, the shape of the histogram around the peak, on a scale of order $\sqrt{N}$ around the mean, can be very well approximated, for large N, by a Gaussian form (see Fig. 1)

$$P(M,N) \approx \sqrt{\frac{2}{\pi N}}\, e^{-\frac{2}{N}(M-N/2)^2} \qquad (3)$$

This Gaussian form is a direct consequence of the CLT. To see this, we can write $N_H=\sum_{i=1}^{N}\sigma_i$ where $\sigma_i=1$ if the i-th coin shows a head and $\sigma_i=0$ otherwise. Since the σ_i's are independent random variables, the CLT says that the sum of a large number of such independent random variables has a Gaussian shape. However, the CLT does not hold when the deviation from the mean is much larger than $\sqrt{N}$. For example, suppose we consider the extreme event where all the outcomes are head, i.e. $N_H=N$. Clearly the probability of such an event, putting $M=N$ in Eq. (1), is exactly

$$P(M=N,N) = Pr(N_H=N) = \frac{1}{2^N} = e^{-N\ln 2} \qquad (4)$$

On the other hand, putting $M=N$ in Eq. (3), one finds $P(N,N) \approx e^{-N/2}$ which is much bigger than the exact value $e^{-N\ln 2}$ for large N. This clearly demonstrates that the Gaussian form, while being a very good approximation near the peak, is rather poor at the extreme tails. This is exactly where the 'large deviation theory' comes to the rescue, as we show now.

Since we would like to describe events such that $M-N/2$ is of order N, we can set $M=cN$ (where the fraction of heads c is of order 1) in the exact expression in Eq. (1). For large N, we can use Stirling's approximation $N! \approx \sqrt{2\pi N}\, e^{N\ln N-N}$ to write

$$P(M=cN,N)=\frac{1}{2^N}\frac{N!}{(cN)!((1-c)N)!} \approx e^{-N\varphi(c)} \qquad (5)$$

where

$$\varphi(c) = c\ln c + (1-c)\ln(1-c) + \ln 2,\ 0 \le c \le 1. \qquad (6)$$

The Eq. (5) is usually referred to as a "large deviation principle", with speed N and a rate function $\varphi(c)$. This function $\varphi(c)$ is a convex function with a minimum at $c=1/2$. At the extreme end c=1, we get $\varphi(c=1)=\ln 2$. Thus for $c=1$, Eq. (5) correctly describes $P(N,N)$ in Eq. (4). Moreover, this large deviation form in Eq. (5) also describes correctly the Gaussian behavior near the peak at $c=1/2$. To demonstrate this, we note that $\varphi(c) \approx 2(c-1/2)^2$, as $c \to 1/2$. Using this quadratic behavior in Eq. (5) we recover the Gaussian form (3). Thus in this simple example, the large deviation form in Eq. (5) not only describes the extreme events but also the typical events around the mean (see Fig. 1).

While this large deviation theory is quite well developed in the mathematics literature [1–3], physicists are also quite familiar with this concept, though in a slightly different language (for a review see [4]). To connect to the language of physicists, let us again consider this simple example of the coin tossing experiment. Instead of asking for the probability of the number of heads, let us consider just the number of possible configurations

$$N(M) = \binom{N}{M} \qquad (7)$$

with a fixed number of heads $M=cN$. For large N, this can also be written, using Stirling's formula, as

$$N(M) \approx e^{NS\left(\frac{M}{N}\right)} \qquad (8)$$

where it follows from Eqs. (5) and (6) that

$$S(c) = \ln 2 - \varphi(c) = -c\ln c - (1-c)\ln(1-c) \qquad (9)$$

Hence we see that $N(M)$ also admits a large deviation principle with a rate function $S(c)$, which is thus simply related to the 'mathematician's' rate function $\varphi(c)$ via Eq. (9). We will now see that this $S(c)$ is nothing but the good old entropy density that physicists are familiar with. To demonstrate this, let us consider the same coin-tossing experiment in a slightly different language. Let us consider N non-interacting Ising spins $s_i=\pm 1$, subjected to a constant external magnetic field h. The energy associated to a particular configuration of the spins is $E=-h\sum_{i=1}^{N} s_i$. Writing $s_i=2\sigma_i-1$ (where $\sigma_i=1$ or 0) and setting $h=-1/2$, one gets, up to an additive constant,

$$E=\sum_{i=1}^{N}\sigma_i \qquad (10)$$

which is precisely the number of heads N_H in the coin-tossing experiment. If we now consider the statistical mechanics of this spin system in the *micro-canonical* ensemble (i.e.,energy E is fixed), we would like to compute the micro-canonical partition function $N(E)$ which simply denotes the number of spin configurations with a given energy E. But this is precisely the number of heads N_H in the coin-tossing experiment. Hence, setting $N_H=M=E$, and using Eq. (7), we get $N(E)=\binom{N}{E}$. Thus it follows from Eq. (8) that, for large N, it admits a large deviation form as in Eq. (8) with the associated rate function $S(c)$ given in Eq. (9). In statistical mechanics, $S(c)$ is the well-known entropy density at energy $E=cN$ (upon setting the Boltzmann constant $k_B=1$). The large deviation principle in this example just reflects that the entropy and the energy are extensive. Even though this interpretation of the rate function $S(c)$ as the entropy density at fixed energy $E=cN$ is demonstrated here in a simple example, this is actually more general. Indeed, for any short-ranged interacting system, thermodynamics tells us that both the energy and the entropy are extensive. Hence, for any such system, we would expect that there is a large deviation principle for the micro-canonical partition function.

One can also connect the rate function $S(c)$ (or equivalently the entropy density) to another quantity, very much familiar to physicists, namely the free energy per particle in the "canonical" ensemble (where the temperature T is kept fixed, but allowing the energy E to fluctuate). In this canonical ensemble, one first defines the so called "canonical partition function" $Z=\sum_C e^{-\beta E(C)}$, summing over all microscopic configurations C of the system with an associated Boltzmann weight $e^{-\beta E(C)}$, where $\beta=1/(k_BT)$ is the inverse temperature. One can convert this sum into an integral over energy

$$Z=\sum_C e^{-\beta E(C)} = \int e^{-\beta E} N(E)\, dE \qquad (11)$$

where $N(E)$ is the micro-canonical partition function. Assuming extensivity of the energy (which is true for any short-ranged system), we would expect a large deviation principle as in Eq. (8) $N(E) \approx e^{NS\left(\frac{E}{N}\right)}$, where $S(c)$ is the entropy density at energy $E=cN$. Using this result in Eq. (11) and making the change of variable $E=Nc$, one obtains

$$Z \approx N\int dc\; e^{-\beta N\left(c-\frac{S(c)}{\beta}\right)} \qquad (12)$$

For large N, the dominant contribution to the integral comes from the minimum of the argument of the exponential (the so called "saddle point approximation") leading to

$$Z \approx e^{-\beta N \min_c\left(c-\frac{S(c)}{\beta}\right)} \qquad (13)$$

In the thermodynamic (i.e., large N) limit, the free energy per particle is defined as $f(\beta)=-\lim_{N\to\infty}\frac{1}{\beta N}\ln Z$. This definition is equivalent to say that the canonical partition function Z in (13) admits a large deviation principle with speed N and rate function $\beta f(\beta)$ as in Eq. (13) with

$$f(\beta)=\min_c\left(c-\frac{S(c)}{\beta}\right) \qquad (14)$$

Hence the free energy per particle $f(\beta)$ in the canonical ensemble and the entropy density $S(c)$ of the microcanonical ensemble are related to each other via a so called "Legendre transform".

So far, we learnt that the large deviation principle and the associated rate function $S(c)$ is a very useful tool to describe, within a single setting, both typical as well as atypically rare events. What else can we learn from this rate function $S(c)$? In this coin tossing example, we see that $S(c)$ in Eq. (9) is a smooth function of c with no singularity for $0<c<1$. It turns out however that in a system that exhibits a thermodynamic phase transition, the rate function $S(c)$ displays a singularity (non-analytic behavior) at some critical value c^*. As a simple example, let us consider the *2d* ferromagnetic Ising model. In the canonical ensemble, we know from Onsager's celebrated exact solution [5], that the free energy $f(\beta)$ has a singularity at a critical point $\beta=\beta_c$ (this corresponds to a second order phase transition from a high-temperature paramagnetic phase to a low-temperature ferromagnetic phase). From Eq. (14) connecting $f(\beta)$ and $S(c)$ one immediately sees that $S(c)$ will also exhibit a singularity at a critical value $c=c^*$. Indeed, it has been shown that $S(c)=(c-c^*)^2/\ln|c-c^*|$ for c close to c^*. Thus the second derivative of $S(c)$ diverges logarithmically at $c=c^*$ [6]. This fact that the thermodynamic phase transition manifests itself as a singularity in the rate function $S(c)$ turns out to be quite generic, both in short-ranged and in long-ranged systems [4, 7].

This idea of detecting a phase transition by studying possible singularities of the large deviation function associated to the probability distribution of some observable has recently been extensively used in various disordered systems, most notably in problems related to the random matrix theory (RMT). RMT has been a very successful tool in analyzing problems arising in statistics, number theory, combinatorics all the way to nuclear physics, mesoscopic systems, wireless communications, information theory, etc. The main goal in RMT is to study the statistics of the eigenvalues of a random NxN matrix with entries chosen from a specified ensemble. The simplest example is the Gaussian Ensemble of real symmetric matrices, for which all the eigenvalues are real.In this case the joint distribution of the N eigenvalues can be interpreted as the Boltzmann weight of a gas of N charges on a line, in presence of a harmonic trap, and with long-range pairwise (logarithmic) repulsion between them.

There has been a lot of recent activities on the statistics of the top eigenvalue, i.e., the position of the rightmost charge λ_{max} . For large N, the typical fluctuations of λ_{max} around its mean $\sqrt{2}$ (in scaled units) are known to be governed by the celebrated Tracy-Widom (TW) distribution, which is a bell shaped curve (see Fig. 2), albeit with non-Gaussian tails [8]. This TW distribution describing the typical fluctuations of λ_{max} in RMT is the analogue of the Gaussian distribution describing the typical fluctuations of the number of heads around the mean in the simple coin-tossing example discussed before in Eq. (3). However, the large atypical fluctuations of λ_{max} are not described by the TW law, similar to the coin-tossing example where the central Gaussian distribution fails to describe the extreme tails. The large deviation tails for λ_{max} have been computed and it turns out that the tails are rather different on the left and the right of the mean, at variance with the coin-tossing experiment where $P(M=cN,N)$ is symmetric around the mean c=1/2 (5). Moreover, while in the coin-tossing case the large deviation function $\varphi(c)$ is smooth around c=1/2, in the case of λ_{max}, the associated large deviation function is singular around the mean and its third derivative is discontinuous there. This is thus an example of a third order phase transition, according to Ehrenfest classification. One might wonder: this is a phase transition, but what are the two phases across this critical point? It turns out that the left large deviation of λ_{max} corresponds to a "pushed phase" where all the N charges are pushed to the left – this involves a collective reorganization of the N charges (see Fig. 2). In contrast, the right large deviation of λ_{max} corresponds to a "pulled phase" where only one single charge splits off the sea of N-1 charges (see Fig. 2). These large deviation functions have actually been measured in experiments in fiber lasers [9].

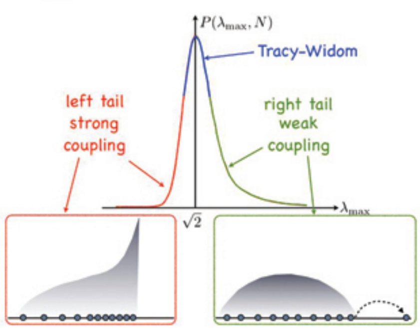

FIG. 2. Schematic picture of the probability distribution $P(\lambda_{max},N)$ of the largest eigenvalue λ_{max} of an NxN Gaussian random matrix. The central blue part indicates the Tracy-Widom distribution, while the red and the green tails correspond respectively to the left and right large deviations. In the inset we show the typical charge configurations in respectively the "pushed" (strong coupling) and "pulled" (weak coupling) phases.

This third order phase transition is different from the more familiar second order phase transition (as in the Ising model), which usually corresponds to a spontaneous symmetry breaking of an associated order parameter (like magnetization). However, the examples of such third phase transitions are quite abundant. For example the well-known Gross-Witten-Wadia transition in large N gauge theory is a similar third order phase transition from a 'strong' (analogue of the pushed phase) to a 'weak' coupling phase (i.e., pulled phase). In recent times, similar third order phase transitions have been found in a large number of examples [10]. For a less technical discussion of the TW distribution and the associated phase transition, we refer the reader to a popular article in Quanta Magazine by *N.* Wolchover [11].

So far, we have been discussing the applications of large deviation principles in equilibrium systems, both short and long-ranged. However, in recent years, large deviations have played a major role in open non-equilibrium driven systems. In many situations, the driven systems may reach a non-equilibrium steady-state, where the probability distribution of observables become time-independent. However, contrary to equilibrium steady states, there is a priori no notion of free energy or entropy density associated with such non-equilibrium steady states. It turns out that in such steady states, one can instead use large deviation functions of appropriate observables as substitutes of the free energy in equilibrium systems. Let us consider again a simple example. Imagine we have a sample of size L in one dimension which is connected to two different heat reservoirs at the two ends: a "hot" reservoir at temperature T_H and a "cold" reservoir at temperature T_C. The temperature gradient sets up a heat current through the system, flowing from the hot to the cold reservoir. Let $j(\tau)$ denote the instantaneous heat flux (or current) at time τ at any given point of the sample. Due to thermal fluctuations, $j(\tau)$ is a random variable and at late times its probability distribution becomes time independent, signaling that the system has reached a steady state. One useful observable in the steady state, which has been extensively studied, is the integrated current up to time t, $Q(t)=\int j(\tau)d\tau$. Its average value $\langle\langle Q(t)\rangle\rangle \propto t$ for large t, since $\langle\langle j(\tau)\rangle\rangle$ is a constant in the steady state. Hence it is natural to expect, and has been established in several models, that the probability distribution $P(Q,t)$ of $Q(t)$ satisfies a large deviation principle,

$$P(Q,t) \approx e^{-t\Phi\left(\frac{Q}{t}\right)} \qquad (14)$$

where $\Phi(z)$ is a rate function, morally similar to the free energy in equilibirum system. Note that the time t here plays the role of N in the coin-tossing example [see Eq. (5)]. Indeed,$\Phi(z)$ satisfies certain additivity properties, like the free energy in equilibrium systems. There has been a lot of recent analytical progress in this field, either by exact solution of $\Phi(z)$ in solvable models [12] or from exploiting a macroscopic hydrodynamic theory developed for driven diffusive systems [13]. In addition, large deviation theory has played a very crucial role in the development of so called "fluctuation theorems" in nonequilibrium systems [14] – a subject of great theoretical and experimental interest, but unfortunately beyond the scope of this short article.

To conclude, one sees that large deviation theory, though originally developed in probability theory, is increasingly becoming a very useful tool in several areas of statistical physics. These include the analysis of the extreme statistics of rare events in disordered systems and related problems in random matrix theory, in equilibrium systems with both short and long range interactions, as well as in systems out of equilibrium. Despite several analytical calculations of the large deviation functions in mostly one-dimensional models, these rate functions, in general, are hard to compute analytically. Hence, numerical methods play also an important role. Indeed, in recent years, very powerful numerical algorithms (using "important sampling" methods) have been developed that can probe probabilities as small as 10^{-100}[15, 16]. Similarly, on the experimental side also, large deviation functions have been measured (see for example [9, 17]). Thus the large deviation theory has seen an explosion of applications during the last two decades, bringing together researchers from mathematics, computer science, information theory and physicists, both theorists and experimentalists. There is no doubt that these rapidly evolving developments in this subject will continue to excite researchers across disciplines for the years to come.

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BETWEEN THE SCIENCE

MANAS KULKARNI was selected as an associate member of the Indian Academy of Sciences.

BALA R IYER, Simons visiting professor at ICTS, took charge as the chief editor of the journal *Living Reviews in Relativity.*

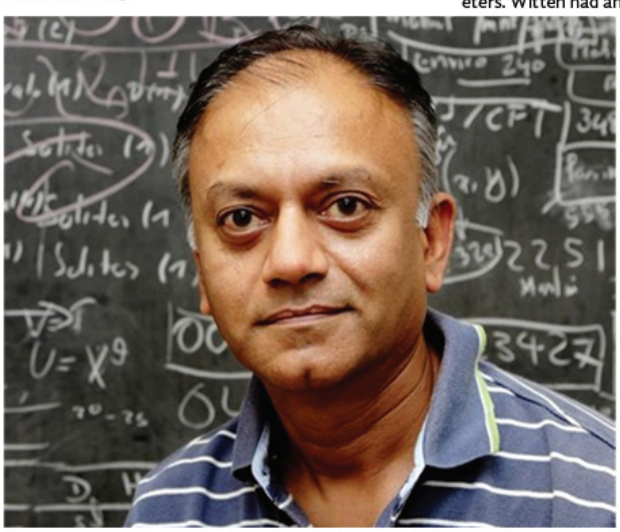
SUBHRO BHATTACHARJEE and **ANUPAM KUNDU** were awarded the SERB Early Career Research award by the Department of Science and Technology, Govt. of India.

RIDDHIPRATIM BASU received the Ramanujan Fellowship from SERB, DST.

'I JOINED ICTP FOR WHAT IT STANDS FOR'

INTERVIEW WITH K. NARAIN

K. Narain is a string theorist at the International Centre for Theoretical Physics in Trieste, Italy. His many contributions to String Theory include the discovery of the widely used so called 'Narain Compactifications' of the Heterotic String.



What inspired you to get into physics? Did you have a role model?

I grew up in Kanpur. Not in IIT Kanpur but in the city. And Kanpur is not a city where a lot happens. Apart from the IIT, there's not much that goes on. The IIT is a totally different world. So basically it was my father who got me interested in physics. When we were kids, we would often go for walks at night. He would look at the stars and start telling me and my sister about them and about Einstein's theory. He had studies chemistry but his real interest was in physics. He used to make us read popular science books and that's how I got interested.

What about the undergraduate years?

Undergraduate years were also in Kanpur University. And Masters was in IIT Kanpur. After that I went to Syracuse University and did my PhD with Kameshwar Wali. Then my first post doc was in ICTP.

Which year was this in?

In 1982-84.

Then your famous work on toroidal compactifications was in 1985? How did you start working on Narain compactifications'?

Yes in 1985. When I was working in Rutherford.

While I was in ICTP, I was working on totally different areas. Not string theory at all but on instantons etc. It was towards the end of 1984, Greene and Schwartz's work became known. I was visiting Princeton at that time. I had finished my time at ICTP and was waiting to join Rutherford. And I happened to talk to Witten. I had a question on something I was working on. Nothing related to string theory. So when I went to his office, he said, 'Ah I see you are working on this? It will be very useful if you apply your knowledge to string theory.' He told me that David Gross was about to write a paper and I should read that and see if I could apply this technology.

Then David Gross's paper was published and I started reading it very carefully. I always remembered what Witten told me. So Witten was always a great inspiration. He made a big difference to my work.

After this you wrote a paper with Witten.

Yes. So I had some models, certain number of parameters. Witten had an interpretation, an understanding of what these parameters meant. Then we tried to prove that vigorously and wrote that paper together.

Did your paper with Witten, make your previous work more well-known to the world?

Well in some sense the paper that Witten and I wrote together demystified the previous paper. It gave an explanation.

After this you had an appointment at CERN.

Yes but that was rather brief. Although it was a six-year position. I joined in January, 1988 and then came to lecture at an ICTP Spring School. And Salam called me to his office and asked me if I was interested in joining. I had spent time in ICTP and I really liked this place. Not just as a scientific institute but what it stands for.

Is that why you joined?

Yes exactly. That was the main reason.

How do you think ICTP has helped in promoting physics in the developing world?

Well, of course, it is a very slow process. Many times you don't really see the actual effect. Or you often cannot associate something with ICTP. But when I was a postdoc here, I could already see a huge number of scientists from developing countries, particularly during summer. Sometimes we would share offices. As I would talk to them, I started realizing that for many of them ICTP was the only reprieve. Back home was very different – they would have to teach all the time. Then some countries had adverse political pressures. And this was the only place where they could come during the summer, forget about all the problems and get some breathing space. This really impressed me a lot. For the Indians, even though there were no political problems back home, it was similar. This was because during the academic year they would be just teaching. I am not talking about institutes, but universities. So they would come here and get their batteries charged. It was an important thing.

I wanted to ask you about the ICTP's diploma program.

In the beginning I was dividing my time between CERN and ICTP. After about two years, in 1991, I decided to move here permanently. And it was exactly at that time that there was a discussion going on about the diploma program. Salam and Seif Randjbar-Daemi, who was the head of the group at that time, were all part of the discussion. It was clear that something needed to be done. The reason for this was – we had the summer schools, the spring schools and the people who could benefit from these were already from reasonably advanced levels. They were already doing research in frontline topics. But in many countries there was no one at that level. So I think it had become clear to Salam that we needed another level where we could train students to reach the advanced level. Perhaps not immediately but afterwards. Then, of course, there was a lot of debate about precisely what kind of program it should be.

Finally it was decided we should have another specialized program. The debate had been whether we should have a more general kind of thing, where the students would learn little bit of condensed matter, little bit of high energy and then after one year decide in what direction he or she wants to go. But finally it was decided that it should be more specialized – so we assume that the student has done an undergraduate level and has a general idea. It would be like a pre-PhD program.

Can Indians also apply to this diploma program?

In principle it is for students from countries where they don't have any infrastructure, any facilities like a good masters program.

So mainly African countries?

Yes Africa, Latin America and other Asian countries. The country might have one masters program but that cannot be enough, right?

In India they have plenty of excellent masters programs. There is no need for Indians to look elsewhere.

So you would give preference to such countries.
Yes that's right. So India, China, Argentina, Brazil, Mexico, Chile – these are the countries from where we don't normally take students into the diploma program. Though we have made exceptions. For example in China, there is a region where mostly Muslims live. I forget the name. Near Afghanistan – I think it's Xinjiang. Historically it is a much neglected region. So we made an exception when we accepted one female student from there. She came, she was successful, she went on to do a PhD in Germany with a great mathematical physicist. And then she went back and started developing physics in her region.
Then after a couple of years we got another student from the same region. If you just train one person, he or she goes back and might get lost. The idea is to develop a group – two or three people who can talk to each other, encourage each other. The second time it was again a female student. In fact they told us that they were the first female students who had gone up to the university level. So this woman also went on to do PhD in England and returned to her country. Then they started sending us good students once in a while. And two years back they managed to build an institute there. I feel this is a very good example of the impact that the diploma program has been able to make.

Was Freddy Cazacho a student of the diploma program?
Yes that's right. Of course he did not need the diploma program. When he came here he was already so advanced. But there is definitely a role played by ICTP – I think if he had not come here he would not have been able to go to Harvard. Because we realized how exceptional he was and contacted Cumrun and told him. On that basis Cumrun took him. Of course once Cumrun saw him he realized his potential.

So you would say that the diploma program has had a big impact? Has been a success?
Yes. The thing is the resources are very small. We can get 10 students in each group and that's very little if you think about the world. So what we have tried to do is to focus on some regions. Like I said, if you train one person that person goes back and will most definitely be lost. There are so many difficulties one has to face. And not just at the scientific level but on many other levels. This is something we have done with Egypt. Not consciously, but it just happened that we kept getting good Egyptian students. They have also returned and built institutes in Egypt. These are a few examples and I am sure in other groups like condensed matter must have many more examples. We need to focus because the resources are so little.
We try to have geographical distribution, like 3 from Africa, 3 from Latin America, 4 from Asia. By Asia I mean Bangladesh, Indonesia, Thailand, Pakistan. We used to get students from Iran as well but I think now they have a similar situation like in India. They have good institutions. Palestine is another place because they have a very difficult situation.

Do you get good students from Palestine?
Not too many. I think so far we have had 3-4 students. In fact this year we have one very good student from Palestine.
Anyway, then we found out that the students coming from Africa, with a few exceptions, had done their undergraduate levels on paper. The problem is that the different universities don't have the same level. undergraduate levels on paper. The problem is that the different universities don't have the same level. Say for example an undergraduate from Khartoum in Sudan is from a good level, you can trust. Or an Algerian University. If someone gets 50 or 60 per cent then you are sure that he or she has good knowledge. But then there are other universities where they get very high grades but then we realize that they have no idea. Of course the problem lies with the teaching in those places. So then we thought that we should have another special program which will fill in those gaps. A one-year program which is more like the last year of undergraduate – like a preparation for the masters. We tried these for a few years and were getting students only from sub-Saharan Africa. And this course was not specialized to high energy, it involved many different subjects, like mathematical methods etc. I thought it was a successful program but it turned out to be so much work. And we have altogether 30 members, right? It just became too much. I was teaching for about 90 hours one semester. And similarly some of my colleagues were having to teach too much. So then we decided to drop it.

How do you think ICTP is evolving? Now that there is ICTP, Brazil as well? Could there be an ICTP, China as well?
I think you must talk to George Thomson. He would know much more about this.

Are you, in any way, involved with ICTP, Brazil?
Well we function quite independent of ICTP, Brazil. In 2004 we started a two-week school in Latin America. The first one was in Cuba, then Mexico, Brazil, Argentina. The idea is to have a basic crash course, starting from a very basic level like the construction of the Bosonic string. We get the students from different parts of Latin America to this particular place. We provide some financial support – the rest comes from the Latin American countries. This was a great success. You know in Latin America there are string theorists, but they are scattered. Like one person in Venezuela, two or three in Argentina. They don't have the manpower to have a regular course in string theory. Now this two-week course is also not enough. But hopefully it will give the interested students some encouragement. I think some of the very good string theorists have passed from this school. And this still goes on – every three or four years. Nathan Berkowitz, who is the director of ICTP Brazil, is one of the main organizers. So this is the way I have contact with the role played by ICTP in Latin America.
Though I know that in the beginning, for them to acquire funding it was useful to have some kind of connection with ICTP, Trieste. So I do remember those early conversations when Nathan would visit us. And now they have succeeded and it's a great place. I think they are doing a lot of work for the Latin Americans.

Is its philosophy similar to ICTP, Trieste?
Yes. But it's mainly centered around Latin Americans, which is great. Because ICTP cannot handle everything. Similarly I think there is some talk about China. And of course if the Chinese start it, it will be at a very big scale. [Laughs]
In India, of course ICTS already has similar ideas.

This is what I wanted to ask you next. How do you see ICTS impacting science in India, in the developing world or the world in general?
Well at a scientific level it is already very good. And it is just amazing how they have been able to get the top guys in such a short time. It is also a great venue for high-level international conferences.

What do you think about the future of string theory? Where do you see India's role in it?
Difficult question. India is, at the moment, one of the leading countries as far as string theory goes. And indeed some of the major things have come from India particularly the black hole issues.
Now, what is the future of string theory – is very difficult to predict. It's very difficult to relate it to the real world. After all, string theory is the theory of gravity but from that high energy scale you need to come down to the low energy scale. And a lot of things can happen in between. So there are many outstanding problems and I really don't know how much progress we can make. The main reason I am interested in string theory is because this is the only known consistent theory of quantum gravity. Perhaps one can make progress by understanding black hole entropy for less supersymmetric black holes where the problem continues to be well defined but computational techniques are much weaker.Who knows someday some clever person will come up with a new idea. At the moment this is the only one. But it is a very difficult subject. To connect it to the real world, is very difficult.

Finally, could you tell us a bit about your interactions with Abdus Salam?
When I was a post doc, I didn't have much interaction with Salam. Then Salam was mainly working on super gravity. And I was not interested in super gravity at all. In fact, I didn't like supersymmetry so much either. So if you were working on super gravity, you could work with Salam. But some other post docs and I were working on totally different things. But Salam had the habit of getting all the post docs, at least once a year, in his office and ask them to explain what he/she was working on. So, of course, we had such meetings. But that was it. Of course his personality was very impressive. You didn't need to work with him, you could just see it. His great ideas and the very fact that ICTP exists.
Later I started getting into string theory and Salam was very interested. I would visit ICTP during late 1988-89 to lecture at the spring schools. During that time Salam would call me and ask me what was going on in string theory.
Then when I joined in 1990, his health had started deteriorating. Everything was getting very difficult for him. He had great interest in string theory but he was no longer following it very closely. Of course he still had many other things to do, even with the very bad health situation. But once in a while I would see him in the corridor and he would ask me, 'What is Witten up to these days?' [Laughs] I would mention the papers Witten had recently written. He would then ask me to explain them to him. I remember Witten wrote a paper on two-dimensional gravity. In fact it was something related to what Spenta had been doing then. It was a very technical topic and I started explaining. When I turned back, I saw that he had fallen asleep. His health was really bad then. This happened often. I would stop and because of the

change, he would wake up. It was really nice to see that despite all that he had great interest. He was not working on string theory but he understood that this was a very promising direction and he wanted to know more.

[Interviewed by Ananya Dasgupta]

'String theory is richer, more beautiful and more profound than anybody imagined'

Discussion with Andy Strominger (Rajesh Gopakumar, Shiraz Minwalla and Sandip Trivedi)

Andrew Strominger, a string theorist at Harvard University, USA, is a widely recognized as one of the intellectual leaders of his field. His contributions include the co discovery of the so called Calabi Yau compactifications of string theory and the first reliable accounting for black hole entropy within string theory. He is a Dirac Medalist and a Breakthrough Prize laureate.

[Here are a few excerpts]

Rajesh Gopakumar: Perhaps we can start from your student days.

AS: I got into Harvard at a very young age and told my undergraduate advisor that I wanted to do theoretical physics. He told me I wasn't smart enough. People who want to do theoretical physics don't do things like go to China! So that was it and it kind of irritated me, all these people thought that they knew what I could do and what I couldn't, based on my past. They decided what kind of profile I fitted into. Even now when we get the applications for graduate school, and we rank them, we all rank them the same way. The files tell you how to rank them – their scores and everything. But there is very little correlation between their ranking and ultimate success. But when a student comes and talks to me, I still tell them that I got a 'C' in general relativity!

Sandip Trivedi: Are you kidding? That's great! Who was the teacher?

AS: Weinberg. [Everybody Laughs] I had since lost hope. But one of my principles was to never study for exams. You learn things because it's fun, not for grades! And the only person who knows how much he or she really knows is usually that person himself. When somebody comes to me saying they got a 'B' or 'C' in Quantum Field Theory and ask if I think they could be a string theorist, I tell them while it's not a plus to have gotten a C, they are the only ones who really know what they can do. You get people who might have got the top grades in every course but never have an idea in the rest of their career.
I did my masters in Berkeley. That was not the best time for particle theory at Berkeley. In the first year somebody gave a talk to the incoming theory students, saying he hadn't taken on a student for seven years because he couldn't do this to anybody. And one shouldn't become a theoretical physicist unless he/she thought he/she was Einstein. I had a kind of a checkered history and I was not like some

sort of hot student in Berkeley . But I had some feeling that I could do it.
Then something strange happened. In Berkeley I was living with June, the daughter of Tom Kinoshita, the author of Kinoshita's theorem. He is still alive. Around that time I had taken on myself to do the problems in Bjorken and Drell after first year of graduate school. Every time I went to the Kinoshita's I would interrogate Tom about the problems I was stuck on. He said to me 'You know I think you can do something. I have seen a lot of students and I really think that you can do something.' He is a very soft spoken and generous guy.

RG: Was your thesis advisor, Roman Jackiw, based in Cornell?

AS: Yes. I started working with him even though I was a student at Berkeley. I just walked into his office one day and said, 'I am out of Berkeley, they told me I shouldn't do theoretical physics, unless you think you can do it. So I want to get started doing some research.' And then he said, 'Great! I will give you a problem tomorrow.' He had never heard of me. It was kind of amazing.

RG: Did you officially transfer to Cornell?

AS: No I did not. There was nothing formal. I walked in, knocked on his door and said I wanted to start working now. He said 'Great!' He was a great advisor for me. He had a habit of saying things like, 'How can you possibly be so stupid?' 'Don't waste my time!' He never said anything positive. But he kept giving me problems and I would solve them. I later realized that was unusual. But he never gave away the fact that he was surprised.

Shiraz Minwalla: What were these problems on?

AS: He first asked me to find the wavefunction for Large N QCD in 2D. 't Hooft had already solved 2D QCD but Jackiw wanted to find the wavefunction. So I studied it. By this point I had figured out that the Holy Grail – at that point – was to compute the mass of a proton. Compute the mass, or prove confinement.

RG: In large N, or otherwise?

AS: Yes, large N, or otherwise. It's interesting that large N, at that time, was viewed completely differently from the way it is today. At that time large N was a technique to find a way to compute the mass of proton rather than to understand quantum field theory. There are some incredible quotes from that time, even from Ed [Witten] talking to mathematicians, saying something like 'The problem with mathematicians is that they are mostly interested in global issues. And physicists are really interested in solving local dynamical problems.'
Even at that time we already had a way to compute the proton mass – the lattice. But somehow, people weren't satisfied and wanted something more analytic. Large N was viewed as a tool to that end. Of course it would fabulous if somebody could come up with a nonperturbative method for computing the proton mass that didn't involve computers. But there is no reason that the nature should be so arranged that that is possible. And it hasn't happened yet.

Around the time of my Phd, there was also a good deal of discussion about the grand unified theory. But I wanted to think about what was likely to be the hardest problem of my lifetime. And that seemed to me to be quantum gravity. The black hole information puzzle had already been formulated, and the renormalizabilty problem was unsolved. So I decided that I would study quantum gravity.

SM: Did you go onto a post doc at the institute after your Phd?

AS: Yes, I worked on a number of problems as a student. Then Jackiw said 'Okay, you have done enough, it's time for you to do a postdoc.' Then he said, 'I am calling Steve Adler.' [Laughs] It doesn't work like that anymore. I had a rough first two years at the institute. I was working on problems on quantum gravity, renormalizability of higher derivative theories and ghost problem. I was trying to do large N expansion. I wasn't discouraged or depressed or anything. I wasn't simply going anywhere.

Then in '84. I had conversations with [Phil] Candelas about supersymmetric compactifications. He had done some work in Type IIB. We began to discuss whether supersymmetric IIB compactifications were possible. People then were mainly thinking about coset spaces. Phil and I were discussing related issues, probably in early '84. We were all in Aspen in the summer of '84. That summer Phil and I were working on supersymmetric compactifications of M-theory and type II supergravity when Green and Schwarz discovered anomaly cancellation in Aspen. The work of Green and Schwarz prompted us to start looking at Type I supergravity with SO (32). I think our Calabi Yau paper was actually finished before the heterotic string paper. Jeff Harvey had figured out that the anomalies would cancel for E8 X E8 before they found the heterotic string.

ST: So the Candelas-Horowitz-Strominger-Witten paper came before they found the heterotic string?

AS: Phil and I had worked out that before we knew about the heterotic string. I think that was the sequence – but I'm not completely sure - everything happened so fast. We had figured out the embedding of the spin connection over the summer. Gary (Horowitz) was in Santa Barbara then. I figured out the holonomy condition and I said, I need a space with SU(3). I was familiar with the review of Duff-Neilsen-Pope, the supergravity review, in which they extensively discussed spinors and holonomy. I think they already had squashed spheres at that point. I learned all that from Chris Pope and Mike Duff. I told Mike I need a space with SU(3) holonomy. He said 'I think Yau proved that they don't exist!' [Laughs]

SM: Sign error! [Everybody Laughs]

AS: It sort of rang a bell. I knew Yau – he had been a faculty member at the institute. Gary was his post doc. I read his paper and got very excited because somehow I got the impression that manifolds with SU(3) holonomy were almost unique; the 'Quintic' thing was the only one that was mentioned. So I called Yau, who was in San Diego, and asked him about manifolds with SU(3) holonomy. Yau is very good at understanding the questions. I had a couple of conversations with him and he came up with the number 10000.

SM: 10000 known Calabi-Yau manifolds?

AS: No, the 'Quintic' was the only known one. But Yau estimated that about 10000 must exist. Then I went back to the institute and spoke to Ed. I told him that I had found something that he might find interesting.

SM: Did the conversation with Ed take place at a party? I think I got this from Gary Horowitz.

AS: Yes, I think it was at party. And then it turned out that Ed had reached the same conclusion but from a very different point of view – by studying the worldsheet, the beta function. It was from Ed that I first heard the claim that the spacetime equation of motion is the vanishing of the beta function of worldsheet – even though all aspects of the statement had not been understood at that point – the dilaton had not been sorted out. It was so incredibly exciting.

ST: It must have been amazing!

AS: It was like throwing a basketball through the hoop from the other side of the court! 500 people just dropped what they were doing and started working on this stuff.

SM: Was the excitement entirely because of the physics. Or did the persuasiveness of the personalities play a role? And if so which personalities?

AS: I think the excitement was justified. The fact was that it was kind of a perfect storm of the physics and personalities like Ed and David [Gross]. Plus, it was an important problem. And for the first time in history there was a candidate for a complete unified theory. Also the other things that people were working on at that time were getting a little old. Yes, it was very exciting.

But there was an odd aspect to this. As I said around 500 people dropped what they working on and started working on string theory. Almost all these people, however, were particle theorists who were interested in reductionism. On the other hand, the people who were interested in quantum gravity, by and large, were uninterested in string theory. Science is so odd.

The emphasis on reductionism was illustrated by a famous statement in the heterotic string paper
'There is no obstacle to derive all known physics from the heterotic string, we have solved the reductionism program...' Of course thats not how things worked out - for two interesting and different reasons. First, because we don't have experiments. And second, even more interestingly because strings are not the final reductionist elements even in string theory. The web of dualities has taught us that string theory does not have a fundamental final constituent – we don't know the ultimate way to think about the string theory and it may not be in terms of fundamental constituents. Nobody will say now that there is a sequence, in which we discovered molecules, then atoms, then electrons, and then protons, then quarks, then strings and then we are done.

Nonetheless the focus then was all on reductionism. On completing this reductionism program, the grand programme of the 20th century. Most physicists were completely uninterested in the conceptual implications of having constructed a quantum theory of gravity. Even 10 years later Gross opined that black hole entropy was a problem whose time had not come.

ST: Wasn't there a hope that string theory would have a unique vacuum with a small cosmological constant?

AS: Yes. Though I was out of sync. When I went to that library in Santa Barbara and I thought that there was only one Calabi-Yau space, I had some momentary elation. But already with the estimate of 10000 Calabi Yaus my hope for uniqueness was almost dashed. And by the next year I even wrote a paragraph at the end of a paper asserting that superstrings were not going to make the connection with phenomenology. Nobody wanted to hear this. There was this weird statement that people kept hanging on to somehow – nonperturbatively we would find some global anomaly that would render everything, but the string compactifications that reproduced the standard model, unstable. Completely ludicrous wishful thinking with no basis. This went on and persisted beyond any reason.

Through all this, conceptual issues about black holes and geometry were not on anybody's radar. It was all string perturbation theory, worldsheet conformal theory. It was another 10 years before people even began to think of such issues. All this while the relativists somewhat correctly said, 'you are just souping up perturbation theory.'

ST: People viewed string theory as the grand unified theory plus gravity?

AS: Yes. And that was regarded as the goal of physics. Now we didn't achieve that goal, but the stuff that has happened in string theory – holography and black holes and structure of spacetime – it's such a rich story. The more we learn about it the more we marvel at its richness. People did not imagine anything so rich and beautiful at all in the 80s.

Look, if you had talked to me then I would be bubbling with enthusiasm. But when I look back at my career now, then there are plenty of periods in which I spent years doggedly working on something which added up to nothing. I spent a couple of years working on string field theory which didn't add up to anything in the end. My own vision then about quantum gravity, nonperturbative resummation – it was just a very narrow kind of limited tunnel vision. Everybody has a narrow limited tunnel vision but if we follow the facts, we are led to kind of amazing places. Nobody had any idea of how things would turn out – the structure of the theory is so much richer and more beautiful and more profound than anybody would have imagined.

ST: How did you combine your adventurous style of physics with mathematically rigorous standards? You sort of mix and match them, and I think you are unique in that way.

AS: Well, it's kind of interesting. I feel that the hardest thing about being a physicist is that it's an unstable trajectory. There are two kinds of instabilities. One of them is being too careful and being too concerned about what is actually right. And only doing things that you are confident about. And the other is becoming too speculative. The boundary between the two, the boundary between being too speculative and doing things that are uninteresting, is a knife edge. I don't want to sound pompous, but I somehow know where that edge is. And, earlier in my career, I wasn't sure that I knew it. There's also the boundary between, and this is the heart of this problem, giving up too easily and being too stubborn. It's not about being super-smart, and that's why you can't tell whether a graduate student with perfect grades is going to be a great researcher.

RG: Why did you start working on black branes?

AS: In order to understand Montonen Olive duality in string theory. In this context I had a funny conversation with Ed. I told him I wanted to generalise Montonen Olive duality to string theory. He asked why would anybody believe that duality? I said `Because of a paper by Witten et al'. And he said, 'What we showed in that paper was that all the consequences that were attributed to duality could be attributed to supersymmetry.' It wasn't until Ashoke's [Sen's] paper, counting the bound states that he underwent a phase transition. Anyway I studied branes to understand string duality. I tried to quantize the 5-brane.

SM: You spoke before about how exciting 1984 was. Was the atmosphere comparable in 1995? How was it like for you?

AS: Yes. Well, there were several different answers to that question. You know, at a personal level, I had a famous co-author on the 1984 paper. In '95 everything I personally had been working on for the last 10 years suddenly became important and started coming together. So it was a very gratifying thing for me to see all the bits and pieces of stuff I'd been doing become important. I had forays into wormholes and forays into dilaton gravity which is coming back now. So that was a very nice and, you know, personally gratifying to have my colleagues recognizing the importance of what I'd been doing over the previous ten years. Not that I had been grossly underappreciated earlier.

ST: Didn't you start trying to explain the one quarter area very soon after D-branes and duality?

AS: I computed the three-quarters from the D3-brane before Joe's paper on D branes was out.

SM: Did you consider publishing, given that you were off only by 3/4?

AS: First of all, I was worried that I'd made a mistake. I did write a note that was circulated. But the problem was, there have been so many people who'd claimed to have understood black hole entropy up to the 1/4 that you had to have the 1/4. I'd been thinking about BH entropy, for ten years at that, since my PhD. I'd tried all kinds of different things, some weirder than others. And I had been trying with the D-branes, moduli spaces and whatever, but I didn't know about the extra light states.

SM: The non-abeliansiation.

AS: Yes. And as soon as Joe explained the states to me, I said, okay we can do this calculation. And that's when I did the 3/4s. I tried to get Joe to work on it with me. I was trying to compute dimensions, asymptotic dimensions, of cohomologies, of moduli spaces. Ed had done this calculation of bound state degeneracies of D-branes, people were trying to get dimensions and I was trying to get some asymptotics. From that point on, whenever that was, September perhaps, when Joe explained it to me, I said ok I can do this calculation in string theory, and I didn't think about anything else until I solved it, which I guess took a year.

ST: In'95-'96, you came to Caltech, and you told me about the calculation that was off by 3/4. I said it still sounds interesting. You said so many people have gotten answer off by a factor, this won't work. And that you had a new idea which will get it on the nose. I remember that very very vividly. And how did your collaboration with Cumrun [Vafa] begin?

AS: I was visiting Harvard, because my family was there. So I was in Boston once or twice a year, and often they'd have an office for me. I talked to Cumrun and I asked some questions about dimensions of moduli spaces. He had been working on these symmetric products of K3s and – it just clicked.

SM: Were you very excited, or did you fear you might have made a factor of 2 mistake that someone would later spot?

AS: No. I knew I had it.

SM: What would you say, in your view, were the main things we learned from '94-'95?

AS: Well, of course the paper of [Nati] Seiberg and Witten. I mean just the idea that you could have that kind of control over the non-perturbative structure of a theory was fascinating. Before that paper – and this was my feeling back as a grad student – that non-perturbative QFT was a murky subject in which you would never be able to say anything very clear. There would always be some point you'd start waving hands, unless you know in 2 dimensions or something. All the instanton stuff that happened in the early 80s never quite worked– there were zero-mode issues with the instantons. The idea that you could control a theory non-perturbatively. It wasn't on anybody's radar screen. There were so many things that changed then. I mean, Ashoke's paper was really a paradigm shift. It's hard to understand now. The idea was that in physics you proved things, you found a new set of variables, some duality transformation, a path integral, and you'd prove quark confinement. That's what you were supposed to do. Ashoke came up with this thing that sounds sort of trivial now, but I think it was really rather revolutionary at the time. The most interesting thing about his paper was that he said, let's just assume it is true. It sounds so simple now and it's hard to understand how bizarre it was. Let's just assume it's true, what would the consequences be and let's check – that was revolutionary.

SM: Did you believe in AdS/CFT immediately? You're thanked in Juan's [Maldacena] paper.

AS: Yes, yes. I think it's fair to say that I was more confident in it than he was.

SM: And looking back now, it's been almost 20 years since AdS/CFT. Can you articulate its importance?

AS: Well, a complete definition of a quantum theory of gravity with no loose ends. And a new paradigm for emergent spacetime which we still, 20 years later, haven't been fully understood. It's a deep statement about the nature of spacetime, which is undoubtedly an important clue but. . . there are many reasons to think that there are pieces missing that are as interesting as the ones we already have.

SM: So maybe some final thoughts about today and the future. How do you see the field now, do you think it's in a healthy state?

AS: Very. Very. I'm very excited about what I'm working on now. There hasn't been some time in my life when I've been more excited. I don't like it when you go to an annual strings conference and everybody is talking about the same thing. I thought there was an annual string conference in China – there were five days and every day was a different topic, roughly. You know, there were a lot of interesting, different things going on. As I said, I'm excited about what I'm doing, but there's also other things that other people are doing that I'd be working on if my plate wasn't full. I think the whole SYK story, and the chaos bound. I think that is deep, beautiful stuff, and we're really learning something. I think we're in great shape.

[Transcribed by Yogesh Dandekar, Pranjal Nayak, Ronak Soni]

PROGRAMS

Large Deviation Theory in Statistical Physics: Recent Advances and Future Challenges

ORGANIZERS

Arvind Ayyer, Frank den Hollander, Abhishek Dhar, Juan P. Garrahan, Christopher Jarzynski, Manjunath Krishnapur, Tony Lelievre, Sanjib Sabhapandit and Hugo Touchette

DATES

14 August - 13 October 2017

VENUE

Madhava Lecture Hall, ICTS-TIFR, Bengaluru

Open Quantum Systems

ORGANIZERS

Abhishek Dhar, Manas Kulkarni, Jason Petta, Anatoli Polkovnikov, Sadiq Rangwala, Dibyendu Roy and Rajamani Vijayaraghavan

DATES

17 July -4 August 2017

VENUE

Ramanujan Lecture Hall, ICTS-TIFR

Summer School on Gravitational-Wave Astronomy

ORGANIZERS

Parameswaran Ajith, K. G. Arun and Bala R. Iyer

DATES

17-28 July 2017

VENUE

Madhava Lecture Hall, ICTS-TIF

Bangalore School on Statistical Physics – VIII

ORGANIZERS

Abhishek Dhar and Sanjib Sabhapandit

DATES

28 June -14 July 2017

VENUE

Ramanujan Lecture Hall, ICTS-TIFR

Summer School and Discussion Meeting on Buoyancy-driven Flows

ORGANIZERS

Jaywant Arakeri, Anirban Guha, Pankaj Mishra, Jai Sukhatme, Vishal Vasan and Mahendra Verma

DATES

12 -20 June 2017

VENUE

Ramanujan Lecture Hall, ICTS-TIFR

Correlation and Disorder in Classical and Quantum Systems

ORGANIZERS
Aveek Bid, Pinaki Chaudhuri, Tanmoy Das, Smarajit Karmakar, Prabal Maiti, Subroto Mukerjee and Srikanth Sastry
DATES
29 May - 2 June 2017
VENUE
Ramanujan Lecture Hall, ICTS-TIFR

Dynamics of Complex Systems – 2017

ORGANIZERS
Amit Apte, Soumitro Banerjee, Pranay Goel, Partha Guha, Neelima Gupte, Amitabha Nandi, Ram Ramaswamy, G. Rangarajan and Somdatta Sinha
DATES
10 May-8 July 2017
VENUE
Madhava Lecture Hall, ICTS-TIFR

Time Series Analysis for Synoptic Surveys and Gravitational Wave Astronomy

ORGANIZERS
Parameswaran Ajith, Jogesh Babu, Sujit Ghosh and Ashish Mahabal
DATES
20-23 March 2017
VENUE
Madhava Lecture Hall, ICTS-TIFR

Laser Plasma Accelerator

ORGANIZERS
Amita Das, Srinivas Krishnagopal, M. Krishnamurthy, G. Ravindra Kumar and Pattathil Rajeev
DATES
6-17 March 2017
VENUE
Ramanujan Lecture Hall, ICTS-TIFR

DISCUSSION MEETINGS

Stochastic Thermodynamics, Active Matter and Driven Systems

ORGANIZERS
Abhishek Dhar, Rajesh Ganapathy, Vijaykumar Krishnamurthy and Sriram Ramaswamy
DATES
7-11 August 2017
VENUE
Ramanujan Lecture Hall, ICTS-TIFR

Bangalore Area Strings Meeting – 2017

ORGANIZERS
R. Loganayagam and Suvrat Raju
DATES
31 July-2 August 2017
VENUE
Madhava Lecture Hall, ICTS-TIFR

Airbus Day

ORGANIZERS
CAM-TIFR and ICTS-TIFR
DATES
28 July 2017
VENUE
Emmy Noether seminar room, ICTS-TIFR

Candles of Darkness

ORGANIZERS
Gautam Bhattacharyya, Amol Dighe, Sreerup Raychaudhuri and Seema Sharma
DATES
5-9 June 2017
VENUE
Ramanujan Lecture Hall, ICTS-TIFR

Complex Geometry

ORGANIZERS
Indranil Biswas and A.J. Parameswaran
DATES
20-25 March 2017
VENUE
Ramanujan Lecture Hall, ICTS-TIFR

Remembering C. V. Vishveshwara

ORGANIZERS
P Ajith, Bala Iyer and Vijaykumar Krishnamurthy
DATES
23 February 2017
VENUE
Ramanujan Lecture Hall, ICTS-TIFR

Indian Statistical Physics Community Meeting 2017

ORGANIZERS
Ranjini Bandyopadhyay, Abhishek Dhar, Kavita Jain, Rahul Pandit, Sanjib Sabhapandit, Samriddhi Sankar Ray and Prerna Sharma
DATES
17-19 February 2017
VENUE
Ramanujan Lecture Hall, ICTS-TIFR

JETS @ LHC

ORGANIZERS
Gobinda Majumder, Sreerup Raychaudhuri, Vikram Rentala, Tuhin S. Roy, Rishi Sharma and Seema Sharma
DATES
21-28 January 2017
VENUE
Madhava Lecture Hall, ICTS-TIFR