Regards and Insights on the Universe Sir Roger Penrose's Mathematical-Physics works and Philosophy *

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Résumé

This paper is the transcript of an interview with Sir Roger Penrose [1], which took place on November 15, 2021. The main aim of the paper is to explore with him the various aspects of his work.

1 Introduction

Sir Roger Penrose is a British mathematician and mathematical physicist who brought an original and exceptional insight to the philosophy of science and particularly the philosophy of mathematics. He has received several prizes and awards, including the 2020 Nobel Prize for his work showing that black hole formation is a robust prediction of the general theory of relativity and the 1988 Wolf Prize which he shared with Stephen Hawking for "Penrose-Hawking singularity theorem". Penrose has made contributions to the mathematical physics of general relativity and cosmology.

He began his career as a mathematician in the 1950s with a thesis entitled, "Tensorial methods in algebraic geometry". He became interested in physics and general relativity and cosmology from the 1960s under the influence of the physicist Dennis W. Sciama who later became Stephen Hawking's supervisor.

Penrose introduced original mathematical methods from algebraic geometry and differential topology to solve the equations of the theory of general relativity and to better understand their predictions concerning the relativistic theory of stars and cosmology (Roger Penrose, 1987, 1988). In 1965, he demonstrated that what would later be called a black hole by John Wheeler is an inevitable consequence of general relativity applied to the gravitational collapse of a sufficiently massive star. This theorem discovered by Penrose also implies that the

*Sir Roger Penrose is one of the most accomplished scientists of our time. We would like to thank him for sharing with us his conception of the world and his regard on the foundations of mathematics and physics and the contexts of emergence of his fundamental ideas and breakthrough. We express to him our gratitude, our admiration and our friendship.

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1. The text of the interview was enriched by complements and figures to illustrate the answers and the thought of Roger Penrose. They are taken from the excellent book The road to reality : a complete guide to the laws of the universe, Jonathan Cape, London [4] and others references (see bibliographies). With courtesy of Sir Roger Penrose.

endpoint of the collapse of a black hole star is a singularity of space (Roger Penrose, 1965). Penrose also introduced other geometric methods, in particular, what came to be known as Penrose-Carter diagrams, which led to important discoveries in the field of black holes and cosmology (Roger Penrose, 2004). The calculations made by Hawking to discover his famous quantum radiation of black holes use such a diagram.

Later, Penrose also developed original geometric ideas for a quantum theory of physics in curved space-time. His theories of "twistors" and spin networks are used today to explore quantum theories of gravitation (Roger Penrose, 2004, 2018). In recent years, Penrose has also proposed and defended a new cosmology, known as Conformal Cyclic Cosmology, as an alternative to the theory of inflation, which he doubts. Penrose manifests the same doubt regarding the theory of superstrings (Roger Penrose, 2010, 2012).

We also owe Penrose, with the complicity of his father, and from the age of 16, some flat paradoxical representations of 3D objects that inspired the Dutch artist Maurits Cornelis Escher who evokes the now-famous "Tribar" or Penrose triangle [2] in some of his works. Penrose's interest in geometry and puzzles also led to the discovery of a new type of mathematical tiling of the plane that was thought to be impossible and which would have very concrete applications many years later with the discovery of quasicrystals (Paul Steinhardt, 1996).

In this interview, we would like to look at the origins and the conceptual and philosophical foundations of certain aspects of Sir Roger Penrose's work and his insights into the foundations of mathematics and physics. Roger Penrose can legitimately be considered one of the true successors of Newton.

2 Foundations

JJS & JK : Repeatedly you said that if you have a choice, your universe would have to be mathematical, and you expressed the hope that the complex numbers would underlie the actual universe. Do you think it is essential for understanding physics ?

Roger Penrose : Well, that's an interesting question. The power and the beauty of complex numbers was something I learnt in mathematics when I did my degree in mathematics, both pure and applied Mathematics, so it was specifically mathematics. I was more attracted to pure mathematics than physics. In particular, there was a course on complex analysis and I did find complex analysis particularly amazing in the sense that it would be able to take advantage of what I had regarded, ever since my days as a mathematics undergraduate, as the 'magic' of complex analysis and holomorphic (i.e. complex analytic) geometry.

I had learnt that the complex number system had not only a profoundly deep power and elegance, but that it had also found a basic realization in its underlying role in the formalism of quantum theory. When I had begun to study quantum mechanics in a serious way, and particularly following the superb course of lectures given by Paul Dirac when I was a graduate student (in algebraic geometry) at Cambridge, I became fascinated by the quantum description of spin, and how the complex numbers of quantum mechanics were directly related to the 3-dimensionality of physical space, via the 2-sphere of spatial directions being appropriately identified as a Riemann (or Bloch) sphere of the ratios of pairs of complex

2. Which characterize Sir Penrose is his capability and uniquely visual way of experiencing mathematics. Penrose can obviously hack the equations, but he also has to see them, and he is astonishingly resourceful at coming up with visualisations.

numbers (quantum amplitudes) where, in the case of a massive particle of spin ’ such as an electron (see Figure 1), we can think of these as being the complex components of a 2-spinor. Moreover, I had realized that in the relativistic context, there was another role for the Riemann sphere, this time as the celestial sphere that an astronaut in space would observe. The transformation of this celestial sphere to that of a second astronaut, moving at a relativistic speed while passing nearby the first would be one that preserves the complex structure of the Riemann sphere (i.e. conformal without reflection). The special (i.e. non-reflective) Lorentz group is thus seen to be identical with these holomorphic transformations of this Riemann sphere (Mobius transformations). Again, this was clear from the 2-spinor formalism, this time in the relativistic context.

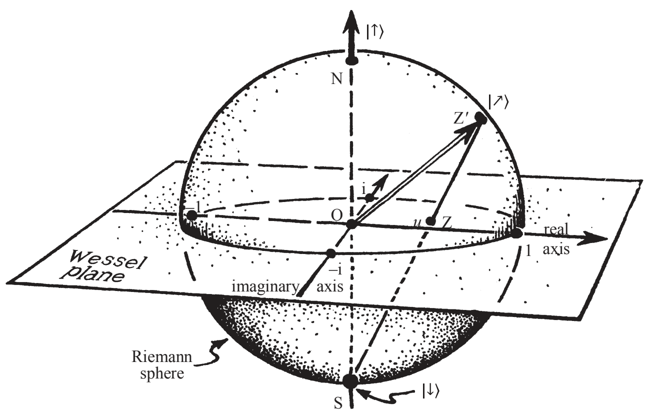

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FIGURE 1 – The Riemann sphere (here in its role as a Bloch sphere) projects stereographically from its south pole S to the complex (Wessel) plane, whose unit circle coincides with the equator of the sphere. A general spin state $|\nearrow\rangle = w|\uparrow\rangle + z|\nearrow\rangle$, of a spin-1/2 massive particle is represented by the pint Z on the Wessel plane denoting the complex number u=z/w, which is the stereographic image of Z’ on the sphere (so S, Z, and Z’ are collinear). The spin direction $\nearrow$ is then OZ, where O is the sphere’s center.

JJS & JK : So, the use of complex numbers starts with quantum mechanics and the use of spinors in quantum mechanics and 2-spinors in general relativity, then twistors.

RP : The twistors came later, that’s right yes.
I knew nothing about quantum mechanics then. Still, I had a view at that time that I should explain : wouldn’t it be wonderful if the physical world was dependent on complex numbers’

You first learn about real analysis and you learn about functions and how they can be smooth. You can differentiate them once, twice or three times and maybe not more. And you have different kinds of functions with different degrees of smoothness, it’s very complicated. And when I learnt about complex analysis, it was all the same. There’s only one degree of smoothness and then you can also expand things in power series. This was such an amazing thing to me.

I just knew about Newtonian mechanics and dynamics and things like that, and I had no feelings for quantum mechanics. And when I learned about quantum mechanics, I saw all the complex numbers are there! Particularly in relation to spin. I think what I found the most attractive was how the notion of spin makes use of complex analysis. It’s a thing that sort of fell into place. It was certainly a big feeling.

I didn't start to learn physics in a serious way. Well, we did have a part of mathematics, I learnt about things like Lagrange's equations. I knew something about the general framework of physics at the time. I was in London when I did my mathematics degree and I went to Cambridge to study algebraic geometry.

So, this is again just pure mathematics. But I got to know Denis Sciama and the cosmologists there and I went to their lectures. There were three lectures courses that were way much more influential than the courses I was supposed to be doing. One was a course on mathematical logic and I learned about Gödel's theorem and computation and that was important for me. I went to a course by Hermann Bondi, a wonderful course on general relativity which I got a genuine feeling for the subject. I think that course was an amazing course for me and important in later life. And the third course was a course, as I said before, by Dirac on quantum mechanics.

And I certainly found the subject extremely beautiful and puzzling at the same time. And I remember his first lecture, in which he described that if you had a proton, it could be in two places : it could be here, it could be here and it could be here and here at the same time. Then he talked about a piece of chalk, and could it be here and here at the same time.

I think he broke it into parts to illustrate this, and my mind wandered. I remember I was looking out of the window thinking about something completely different and when my attention came back, he had moved on to the next topic and I was left puzzled by how pieces of chalk were never found in two places at once. It's probably fortunate that I didn't hear the explanation because it was probably an explanation to stop your worrying about the problem.

I don't know what he said, but it was probably something about energy and how much energy it will cost or something like that. I'm glad I didn't hear it because it might have stopped me from worrying about that problem. But I worried about it ever since which is also very important in my later work. So, I gave Dirac credit also for that.

Well, the other thing which was very important later on was when I was a graduate student and I had a year after I got my Ph.D. in algebraic geometry, I played around with general tensor systems. It wasn't particularly what I was supposed to be doing but never mind. And then I went for one year, I taught a course and again it was pure mathematics at Bedford College in London in Regents Park. It's a very beautiful area in the middle of the park. But then, I got a fellowship at Saint John's College Cambridge on the basis of my fellowship thesis which was actually in pure mathematics, as a matter of fact. But then, when I got to Cambridge, I decided that I would pay more attention to physics. Largely because of the influence of Dennis Sciama, who was a great friend of mine. He was a friend of my brother initially and I had an interest in cosmology which I picked up from hearing Fred Hoyle's radio talks about the steady-state model. And I found it very puzzling. Dennis himself was very enthusiastic about the merits of steady-state model, and that is probably what got me involved.

And I remember talking to Dennis Sciama on a visit I made to Cambridge and he decided to try to convert me to physics. He didn't succeed in converting me to physics in the sense of changing my subject which is what he wanted me to do. But he did in the sense of teaching me, an awful lot of physics. And we used to go on trips. He would drive in his car to Stratford where we would watch Shakespeare plays. Dennis was very keen on Shakespeare and that sort of thing and I got a lot out of that. Sometimes, he used to describe to me his idea about Mach's principle, and when he would drive very fast in his car around steep and curves and when you get thrown to the side of the car, he would say that's the action of the fixed stars you see. Very good at getting himself free of any blame!

But anyway, this was very useful to me to get to these general ideas about physics and it was also useful because he knew people. He was a very good mixer. He talked to anybody on the physics side. He was also Dirac's only graduate student which was interesting, and I think it was important. I went to the second part of Dirac lecture which was on quantum field theory. The first course was on quantum mechanics. That was basic. And at one point, he deviated from his normal course to talk about two components spinors, which was very mysterious, because I was told later by somebody who was interested in Dirac's, I think it was *Graham Farmelo*, he said to me it's very unusual for Dirac to deviate from his normal course.

And he did, he deviated to talk about two components spinors and I think this was very strange because I had been talking to Dennis Sciama about those spinors. I found two components spinors very confusing. I tried to learn them from a book he'd recommended which was by a man called *Corson* which was almost unreadable. Dirac gave two or three courses on two spinors and it made it completely clear to me. He just put the key points and I just understood them. This was important for me.

Later on, I think I was in my second year as a research fellow, and Dennis again persuaded me to go to a lecture by David Finkelstein, about the coordinates, how you get through what we thought to be a singularity and you learn it's not a singularity it's just what we call a horizon now. And Finkelstein's lecture was very clear on this. I remember being very struck by the fact that what used to be thought to be a singularity at the Schwarzschild radius was not a singularity. Yet you still have the singularity in the middle and so you seem to have a problem. I wonder is it possible that there's a theorem that shows that you cannot get away from having a singularity somewhere. And I thought, well I wonder how you could prove such a theorem knowing nothing, almost nothing, about general relativity.

But then, I thought what do I know that perhaps most people working in general relativity don't know' Aaah, two components spinors! So, I decided to write general relativity in terms of two components spinors. And it worked beautifully particularly understanding the conformal curvature, which was the Weyl curvature, and it made it completely clear in a way that if I had not looked at two component spinors, I would not have got that understanding.

It was very important for me that I had these insights into two component spinors from Dirac. And the puzzle I had about them initially, about what could it be, it was taking the square root of a vector : how can you take a square root of a vector ?

It didn't make any sense to me, and then I understood how this worked and how when you use the two component spinors, it makes the Weyl curvature so simple. When you write it into tensors, it's very complicated and you can't see what's going on. But with two spinors it becomes very simple and very beautiful and this was my route. It took a long time before the singularity theorem. That was several years later because it was in 1958, I think, when the Finkelstein lecture happened, and it was in early 1964 that I had the insight into the theorem.

JJS & JK : So, in the beginning, the idea of 2-spinor attracted you the most ?

RP : I liked to think of a 2-spinor (often referred to by physicists as a 'Weyl spinor') in a very geometrical way, and I realized that, up to an overall sign, a non-zero 2-spinor can be represented as a future-pointing null vector [3], referred to as the "flagpole", together with a "flag plane" direction through that flagpole. The flag plane would be a null half-plane bounded

3. A vector pointing along the future null cone.

by the flagpole. This flag geometry can be thought of in the following way. Imagine the Riemann sphere $\mathcal{S}$ of null (i.e. light like) directions at some point O in space-time (see Figure 2.) We are thinking of the geometry in the tangent 4-space of the point O. The flagpole direction is represented by some point P on a sphere of cross-section of the future null cone of O, which we identify with $\mathcal{S}$, and we choose a point P' on $\mathcal{S}$ infinitesimally separated from P. The straight line extended out from P in the direction of P', when joined to O, defines the required flag half-plane. We note that as the point P' rotates about P, the flag plane rotates about the flagpole. The spinor itself is defined only up to sign by this geometry, but we must take note that if P' rotates continuously around P through 2π, the spinor becomes replaced by its *negative*. To reach the original 2-spinor by this procedure, the rotation of the flag plane would have to be through 4π.

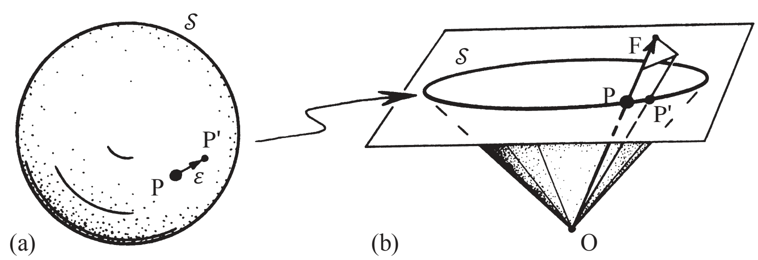

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FIGURE 2 – (a) The space of null directions at some space-time point O is represented as a Riemann 2-sphere S. The flagpole direction of a 2-spinor is represented, on S, as the point P. Infinitesimally near to P is P', where the direction $\vec{PP'}$ provides the 2-spinor's flag plane. (b) In space-time terms, the 2-spinor's flagpole is shown as the null 4-vector $\vec{OF}$, where we realize as a particular 3-plane intersection of the future null cone of O (all this taken in O's tangent 4-space), so that P lies on the line OF. The 2-spinor's flag plane is now seen as the null half-2-plane extending away from the line OF in the direction of P'.

I had found that 2-spinor methods were surprisingly valuable in giving us insights into the formalism of *general relativity* that were different from those that the standard Lorentzian tensor framework readily provides. Most immediately striking was the very simple-looking 2-spinor expression for Weyl's conformal curvature. Whereas the usual Weyl-tensor quantity C_{abcd}, has a somewhat complicated collection of symmetry and trace-free conditions, the corresponding 2-spinor is simply a totally symmetric complex 2-spinor quantity Ψ_{ABCD}.

Then, I had become interested in the issue of finding solutions of the general equation

$$\nabla^{AA'}\phi_{ABC\ldots E} = 0$$

in (conformally) flat space-time, $\phi_{ABC\ldots E}$ being symmetric in its n spinor indices, the equation being the (conformally invariant) free-field equation for a massless field of spin $n/2$. This equation (together with the wave equation in suitably conformally invariant form) had a particular importance for me, and I believed it to have a rather basic status in relativistic physics. For I had come to the view that nature might have a 'massless' structure at its roots, mass itself being a secondary phenomenon. In around 1961, I had found a formula for obtaining the solution of this field equation from general data freely specified on a null initial hypersurface. I had formed the view that this formula had a certain kinship with the Cauchy integral formula for obtaining the value of a holomorphic function at some point of the complex plane in terms of the function's values along a closed contour surrounding that point. I had felt that, in some sense, this massless field equation might be akin to the Cauchy-Riemann equations. There had to be an unusual 'complex' way of looking at Minkowski space, I had surmised, in which the massless field equations were simply a statement of holomorphicity'but in what sense could this be true ?

There was one remaining feature that I felt sure must be represented, as part of this mysterious 'complex' way of looking at space-tine. This arose from a discussion that I had with Engelbert Schücking when I shared an office with him in the spring of 1961 at Syracuse University in New York State. Engelbert had persuaded me of the key importance of the quantum field theory of the splitting of field amplitudes into positive and negative frequency parts. I was not happy with the standard procedure of first resolving these amplitudes into Fourier components and then selecting the positive ones, as not only did this strike me as too 'top-heavy', but also the Fourier analysis is not conformally invariant' and I had come to believe that this conformal invariance, being a feature of massless fields, was important (again, something that had been stressed to me by Engelbert).

I had become aware that for complex functions defined on a line (thought of as the timeline) we may understand their splitting into positive- and negative-frequency parts in the following way. We view this timeline as being the equator of real numbers in a Riemann sphere which, as before, is the complex plane compactified by the single point labelled by '∞', but where the sphere is now being oriented somewhat differently from that of figure 2, with the real numbers now featuring as the equator (increasing as we proceed in an anti-clockwise sense un the horizontal plane), rather than the unit circle. Functions defied on this equatorial circle that extend holomorphically into the southern hemisphere (with usual conventions) are the functions of positive frequency, and those which extend holomorphically into the northern hemisphere are those of negative frequency (see Figure 3).

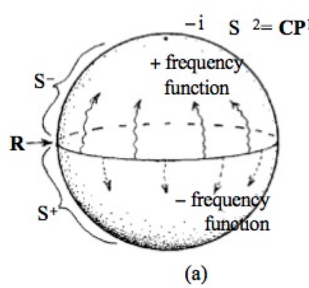

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Figure 3 – Circle of positive/negative frequency split

An arbitrary complex function defined on this circle can be split into a function extending globally into the southern hemisphere and one globally into the northern hemisphere'uniquely except for an ambiguity with regard to the constant part'and this provides us with the required positive/negative frequency split, without any resort to Fourier analysis. I wanted to extend this picture into something more global, with regard to space-time, and I had in mind that my sought-for 'complex' way of looking at Minkowski space should exhibit something strongly analogous to this division into two halves, where the boundary between the two could be interpreted in 'real' terms, in some direct way. This had then set the stage for the emergence of twistor theory !

JJS & JK : You mean that your reflection was motivated first by the idea of splitting'

RP : Yes, I remember the occasion very well because it was shortly after the assassination of Kennedy, and I was in Austin, Texas. It was a very traumatic occasion. I was there at the time with Engelbert Schücking, Rainer Sachs and Roy Kerr. So, we had a good number of people working on relativity theory there. Engelbert was in charge of the group, I think particularly. In Dallas was Ivor Robinson whom I knew very well and Wolfgang Rindler with whom I wrote my book on spinors later on. So, we were very close. And they were actually at the dinner that Kennedy was going to, and they kept wondering why he is not here. He has not come yet and this is because he had been assassinated. And I remember sitting in my office in Austin and I was there and the next-door office, I think it was Roy Kerr's office. And the phone rang and rang and rang and then had to get off. And in the next office, the phone rang and rang and rang right and then the next office it rang and rang and then somebody picked the phone up. And I saw Ray Sachs coming out and he was white as a sheet. And so, I phoned my wife and I knew the awful news.

It was several days later when as a way of kind of recovering ourselves. The Dallas people and the Austin people decided to have a holiday to try and recover. And so, they drove in several cars to San Antonio, and we went to the sea.

On the way back, the ladies all wanted to talk, and they had wanted to come in their cars and I was in the car with pista Oszvath [4] , who hardly spoke at all. He's Hungarian. He speaks English but not much, so I came back almost in silence in the car. And I was able to think about Ivor Robinson's construction of null solutions of the Maxwell equations.

Ivor Robinson, who had taken up a position at what later became the University of Texas at Dallas, had been working on finding global non-singular *null*[5] solutions of Maxwell's free-field equations in Minkowski space-time, where "null" in this context means that the invariants of the field tensor F_{ab} vanish, i.e. $F_{ab}F^{ab} = 0 = *F_{ab}F^{ab}$ where $*F_{ab}$ is the Hodge dual of F_{ab}. Equivalently, in 2-spinor terms, $\phi_{AB}\phi^{AB} = 0$, which tells us that

$$\phi^{AB} = \kappa^A \kappa^B,$$

for some κ^A. It is not hard to show that the Maxwell equations then imply that the flagpole direction of κ^A points along a 3-parameter family -a *congruence*- of null straight lines, which turn out to be what is called 'shear-free', which means that although the lines may diverge, converge, or rotate, locally, there is no shear (or distortion) as we follow along with the lines.

I became highly intrigued by the geometry of general Robinson congruences, and I soon realized that one could describe them in the following way. Consider an arbitrary spacelike 3-plane E in Minkowski 4-space $\mathbb{M}$. E will have the geometry of ordinary Euclidean 3-space, and each ray of the congruence will meet E in a single point, at which we can determine the location of that ray within $\mathbb{M}$ by specifying a unit 3-vector $\mathbf{n}$ at that point, pointing in the spatial direction that is the orthogonal projection into E of the null direction of L there. Thus, we have a vector field of $\mathbf{n}$'s within E to represent the Robinson congruence. After some thought I realized what the nature of this vector field must be. The $\mathbf{n}$-vectors are tangents to the oriented circles (together with one oriented straight line) obtained by stereographic projection of a family of Clifford parallels on a 3-sphere. See Figure 4 for a picture of this configuration, and reference (Roger Penrose, 1986) for a detailed derivation. The large arrow at the top right indicates the direction in which the configuration appears to

4. His name is Istvan Ozsváth known to his friends as Pista.
5. In the Maxwell field you have two null directions which are the directions along the light cone that the Maxwell field determines and when they coincide this is what's called null.

move with the speed of light by continually reassembling itself in that direction, as E evolves into the future.

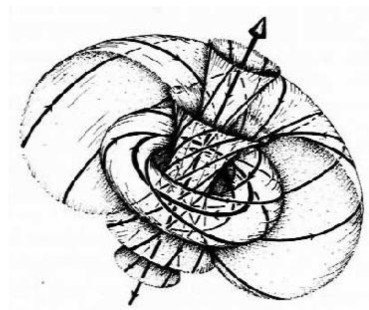

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FIGURE 4 – A picture representing a non-null twistor : stereographic projection of Clifford parallels on a 3-sphere to Euclidean 3-space E. The tangent directions to the circles point in the direction (projected into E) of the rays of a Robinson congruence. By continuingly reassembling itself the entire configuration travels with the speed of light, as E evolves in time, in the direction of the large arrow at the top right.

By examining this configuration and counting the number of degrees of freedom that such configurations have, I realized that the space of Robinson congruences must be 6-dimensional. Moreover, it was reasonably clear to me that by its very mode of construction, this space ought to have a *complex structure*, and so must be, in a natural way, a complex 3-manifold. Within this space would lie the space of special Robinson congruences, each of which would be determined by a single ray (namely L). The space of rays in $\mathbb{M}$ is 5-real-dimensional, and it divides the space of general Robinson congruences into two halves, namely those with a right-handed twist and those with a left-handed twist. The complex 3-space of Robinson congruences, which came to be known as 'projective twistor space' appeared to be just what I believed was needed, where the 'real' part of the space (representing light rays in $\mathbb{M}$, or their limits at infinity) would, like the 'real' equator of the Riemann sphere described before (Figure 3), divide the entire space into two halves. This indeed appeared to be exactly the kind of thing that I was looking for !

Let me say it differently, Ivor wanted to find solutions that were non-singular everywhere and he had discovered these solutions which twisted around in a complicated way. And I was trying to understand this. And I think I realized that these were Clifford parallels. This is the three sphere and they were the circles on three spheres linking each other. But, you see when you take a light ray and you somehow push it into the complex instead of having all the light rays meeting a light ray, so you have these twisting congruences. So, I thought, well what is the dimensionality of these Clifford parallel configurations and I counted the dimensionality. And I realized it was six. One less than the dimensionality of light rays. So, if the light rays represent the real points and the Robinson twisting congruences represent the complex points in a sense, you add one dimension. And they can twist right-handed or left-handed, so you have a space which is divided into two halves. And the real parts, you see directly is the light rays and these twistors as I call them were these twisted in the complex part. So, I realized this was what I was looking for. But it took me a long time to realize. I first had to see how Maxwell's equations do, how the massless equations do. And I had, first of all, a way of doing it which is complicated. But, then I realized you could use quantum integrals to see how to split them into positive and negative frequencies. And it was not until I talked to Michael Atiyah and understood that it's cohomology. And the first cohomology splits into two halves positive and negative frequency and it did exactly what I was looking for. That took about three or four or five years, I can't remember. I was still stuck for a long time because this is only special relativity and all the people I was working with, were working on general relativity. What's the point ?

Engelbert and Roy Kerr understood it, in a certain sense. It was very strange ; the idea of the twistors became clear to me the next day after I returned. I heard Roy Kerr explaining something to Ray Sachs, and they were getting very excited. I came into the next office which was Roy Kerr's office and said what on earth is this, what are you talking about' And Roy was saying I will explain what it is. Roy said, 'I have this way of generating all the sheer free twisting congruences.' These were the things that Robinson had found a special one. Roy said he can find the general ones. And I asked well what are they' He wrote down these formulae and said well you find an analytic function of these particular combinations of coordinates. I looked at them and I thought my god those are twistors.

So, his theorem was taken as an arbitrary holomorphic function in twisted space and this gives you a general twisting congruence, sheer free congruence. So that was the first application of twisted theory, the very next day. Just that is the coincidence.

JJS & JK : In the approach based on complex structures and more specifically holomorphy, we reason in terms of neighbourhood and not in terms of points. Thus, we can say that the holomorphic philosophy induces a new nonlocality in quantum mechanics, where the complex topology will be the fundamental structure. Do you agree with this ?

RP : Yes, that's right. Yes, that certainly was a strong motivation. Holomorphic was a very strong motivation. I mean there is a nonlinearity and nonlocality.

The twistor space that is referred to here, whose individual points represent light rays in space-time $\mathbb{M}$, is denoted by $\mathbb{PN}$. Thus, the point Z in $\mathbb{PN}$ corresponds to the locus Z (a light ray) inside $\mathbb{M}$ and the point R in $\mathbb{M}$ corresponds to the locus R (a Riemann sphere) inside $\mathbb{PN}$.

Now, an essential part of the philosophy of twistor theory is that ordinary physical notions, which normally are described in space-time terms, are to be translated into an equivalent (but non-locally related) description in twistor space. We see that the relationship between $\mathbb{M}$ and $\mathbb{PN}$ is indeed a non-local correspondence, rather than a point-to-point transformation. This locus R inside $\mathbb{PN}$, describes the 'celestial sphere' (total field of vision) of an observer at R, the celestial sphere of R being regarded as the family of light rays through R. As has been noted above, this sphere is naturally a Riemann sphere which is a complex 1-dimensional space. Thus, we think of space-time points as holomorphic objects in the twistor space $\mathbb{PN}$, in accordance with the complex-number philosophy underlying twistor theory.

The space $\mathbb{PN}$, of light rays, does not itself immediately fit in with the 'holomorphic philosophy', however, because it is not a complex space. $\mathbb{PN}$ cannot be a complex manifold because it has five real dimensions and five is an odd number, whereas any complex n-manifold must have an even number, $2n$, of real dimensions. But if we make our 'light rays' a little more like physical massless particles, by assigning them both spin (actually helicity) and energy, then we get a six-dimensional space PT, which actually can be interpreted as a complex space of three complex dimensions. The space PN sits inside PT, dividing it into two complex-manifold pieces $\mathbb{PT}^+$ and $\mathbb{PT}^-$, where $\mathbb{PT}^+$ may be thought of as representing massless particles of positive helicity and $\mathbb{PT}^-$, massless particles of negative helicity ; see Figure 6. However, it

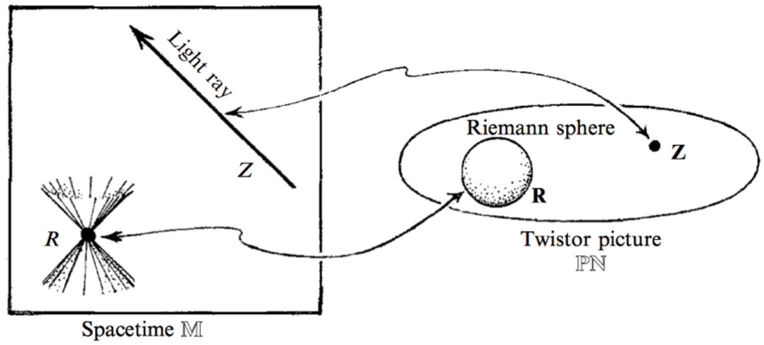

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FIGURE 5 – A light ray Z in Minkowski spacetime 𝕄 is represented as a single point Z in the twistor space ℙℕ (projective null twistor space) ; a single point R in M is represented by a Riemann sphere R in ℙℕ (this sphere representing the 'celestial sphere' of light rays at R).

would not be correct to think of twistors as massless particles. Instead, twistors provide the variables in terms of which massless particles are to be expressed. (This is comparable with the ordinary use of a position 3-vector x to label a point in space. Although a particle might occupy the point labelled by x, it would not be correct to identify the particle with the vector x).

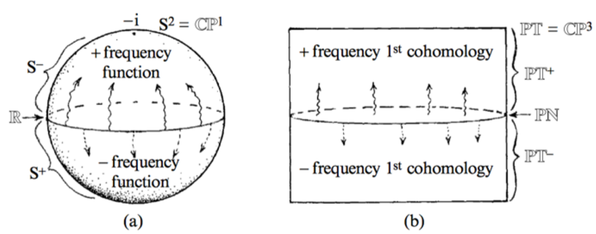

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FIGURE 6 – An analogy between the Riemann sphere $S^2(=CP^1)$ and projective twistor space $\mathbb{PT}(=CP3)$. (a) A complex function (i.e. a '0th cohomology element'), defined on the real axis R of S^2, splits into its positive frequency part, extending holomorphically into what is here depicted as the northern hemisphere S^-, and its negative frequency part, extending into the southern hemisphere S^+. (b) A 1st cohomology element, defined on ℙℕ (and representing a massless field) splits into its positive frequency part, extending holomorphically into the top half $\mathbb{PT}^+$ of projective twistor space, and its negative frequency part, extending into the bottom half $\mathbb{PT}^-$.

It is noteworthy that this feature of space-time geometry is specific to the particular dimension and signature actually possessed by the physical space-time we are aware of. Indeed, the fact that the Riemann sphere plays an important role as the celestial sphere in relativity theory requires space-time to be 4-dimensional and Lorentzian, in stark contrast with the underlying ideas of string theory and other Kaluza-Klein-type schemes. The full complex magic of twistor theory proper is very specific to the 4-dimensional space-time geometry of ordinary (special) relativity and does not have the same close relationship to the 'space-time geometry' of higher dimensions.

The twistor perspective leads us to a very different view of 'quantized space-time' from that which is often put forward. It is quite a common 'conventional' viewpoint that the procedures of quantum (field) theory are to be applied to the metric tensor gab, this being thought of as a tensor field on the space-time (manifold). The view is expressed that the quantized metric will exhibit aspects of 'fuzziness' owing to the Heisenberg uncertainty principle. One is presented with the image of some kind of four-dimensional space that possesses a 'fuzzy metric' so that, in particular, the null cones -and consequently the notion of causality-become subject to 'quantum uncertainties'. Accordingly, there is no classically well-defined notion of whether a space-time vector is spacelike, time like, or null. This issue had posed foundational difficulties for any too-conventional 'quantum theory of gravity', for it is a basic feature of QFT that causality requires field operators defined at space-wise separated events to commute. If the very notion of spacelike' is subject to quantum uncertainties (or has, itself, become a quantum notion), then the standard procedures of QFT, which involve the specification of commutation relations for field operators, cannot be directly applied.

Twistor theory suggests a very different picture. For now, the appropriate 'quantization' procedures, whatever they may be, must be applied within twistor space rather than within the space-time. By analogy with the way that, in the conventional approach, 'events' are left intact whereas 'null cones' become fuzzy, in a twistor-based approach it is now the light rays' that are left intact whereas 'events' become fuzzy.

JJS & JK : Your attachment to complex and sophisticated structures also implies a marked geometric aspect which shows that the geometric representations of mathematical structures are an essential part of your intellectual discipline. This highlights another key mathematical concept, that of conformal transformations.

The demonstration of the particularly sophisticated character of complex structures gives an a priori argument on the privileged mathematical character of these structures which you call holomorphic philosophy, can you elaborate on this philosophical conception'

RP : I use the geometric approach all the time and holomorphic geometry in particular. That's right. Well, you see my algebraic geometry upbringing was quite useful to me because I knew the Klein correspondence for instance. That was very basic to things like I do, and I had some feeling for algebraic geometry although I didn't have the feeling that the people who were the mainstream there at the time like Michael Atiyah who was a contemporary of mine. I knew Michael. We were both students under Hodge at the same time. But he was rather intimidating because his knowledge of algebraic geometry completely surpassed mine. He knew what was going on completely and I was very bewildered by what was going on.

Twistor theory, as we have just seen, initially exploits a manifestation of complex number magic different from those to be found in quantum theory, namely the classical feature of space-time geometry that the celestial sphere can be regarded as a Riemann sphere, which is a 1-dimensional complex manifold. The idea is that this provides us with hints as to Nature's actual scheme of things, which must ultimately unify space-time structure with the procedures of quantum mechanics.

For the space-time representation of the wave function of a free massless particle of general spin, the Schrödinger equation translates to a certain equation known as the massless free-field equation, we have seen at the beginning of our discussion :

$$\nabla^{AA'}\phi_{ABC\ldots E} = 0$$

As I said before, $\phi_{ABC\ldots E}$ being symmetric in its n spinor indices, the equation being the (conformally invariant) free-field equation for a massless field of spin $n/2$. It turns out, remarkably, that there is an explicit contour-integral expression that automatically gives the general positive-frequency solution of the above massless free-field equations simply starting from the twistor function $f(Z^a)$. In fact, the expression also works perfectly well without this positive-frequency requirement, though the requirement is easily ensured in twistor formalism.

The very existence of such an expression strikes me as being somewhat magical. The massless field equations seem to evaporate away in the twistor formalism, being converted, in effect, to *pure holomorphicity*, and when we examine this expression more carefully, we find that there is an important subtlety about how a twistor function is to be interpreted, and this relates in a striking way to the positive/negative frequency splitting of massless fields. This subtlety is also crucial to how twistor functions manifest themselves in active ways and provide us with curved twistor spaces. What is this subtlety ? It is that twistor functions are not really to be viewed as 'functions' in the ordinary sense, but as what are called elements of *holomorphic sheaf cohomology*.

What is sheaf cohomology ? The ideas are fairly sophisticated mathematically, but actually very natural. Now think of a manifold built upon which the transition functions f_{ij} differ from the identity by only an infinitesimal amount. In fact, the class of function f_{ij} that one may be concerned within cohomology theory can be extremely general. In twistor theory, one normally deals with holomorphic functions. This gives us the notion of 'holomorphic sheaf cohomology'. An important feature of cohomology is that it is essentially non-local. This non-locality, for twistor functions, tells us that there is no significance to be attached to the value attained by f_{ij} at some particular point. We can, indeed, restrict down to a small enough open region surrounding that point and find that the cohomology element disappears completely. This non-locality, as exhibited by twistor functions (regarded as first cohomology elements) is tantalizingly reminiscent of the nonlocal features of EPR effects and quantum entanglement. In my opinion, there is something important going on behind the scenes here that may someday make sense of the mysterious non-local nature of EPR phenomena, but it has yet to be fully revealed, if so.

We are to think of this 'cohomology element' as being a 'thing' defined on space Q, which is a bit like a function defined on Q, but which is fundamentally non-local. One example of this kind of 'thing' is actually an entire (complex) vector bundle over Q. We recall that, in the definition of a bundle, that part of the bundle lying above a small enough region of the base space (here Q) is 'trivial', in the sense of being just a topological product. This is an example of the fact that if we restrict our first cohomology element down to a small enough region, it becomes 'trivial' also ; i.e. it vanishes. Thus, the 'information' expressed in a cohomology element is something of a fundamentally non-local character. For example, consider the case of the drawing of an impossible object' sometimes referred to as a 'Tribar' (Figure7).
It is clear that the '3-dimensional object' that the drawing apparently depicts cannot exist in ordinary Euclidean space. Yet locally there is nothing impossible about the drawing. The impossibility is non-local and disappears if one considers a small enough region in the drawing. In fact, this notion of 'impossibility' in such a drawing can be expressed as a specific cohomology element. It is a relatively simple type of cohomology, however, where the functions f_{ij} are taken to be constants.

There are many applications of these ideas in mathematics, not all of which are concerned with holomorphicity. The 'sheaves' that one is mainly concerned within twistor theory are those expressed in terms of holomorphic functions, and there is a special magic in cohomology theory in this particular context. (Roughly speaking, the term 'sheaf' refers to the type of

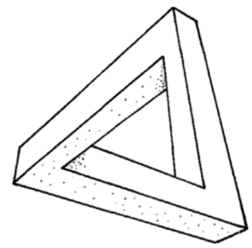

Figure 7- Tribar

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Figure 7 – Tribar

function that one is concerned with, but the sheaf notion actually applies considerably more generally than just to ordinary functions) There are many other types of use of cohomology, including some that have importance in the study of the Calabi-Yau spaces that occur in string theory, for example. Also, there are several other quite different ways of defining sheaf-cohomology elements, all of which can be shown to be mathematically equivalent, despite their very different appearances. In my opinion (sheaf) cohomology is an excellent example of a Platonic notion, where, like the system of complex numbers $\mathbb{C}$ itself, it seems to have a life of its own' going far beyond any particular way in which one may choose to represent it.

JJS & JK : How do you describe the role of Cohomology and fiber bundle in physics'

RP : Well you can see how it works, partly. It's a puzzling story and certainly, twistor theory has not properly come to terms with general relativity.

You see, to me, the non-linear graviton was a very remarkable thing. In twistor theory, you can describe massless particles of any spin. And it works very well for electromagnetism because you've got spin plus one and spin minus one. Let's call it helicity so you've got the right-handed and the left-handed spin. But in twistor theory, one of these has a homogeneity which is zero and the other has a homogeneity which is minus four. The homogeneity is $-2S-2$, S is the helicity.

And so $2S$ is either plus $+2$ or -2, and it gives you either 0 or -4. So, the right-handed ones were -4 and the left-handed ones were zero. It looks very lopsided. Twistor theory was very lopsided. I began to think well maybe it is very lopsided, maybe physics is very lopsided. But then when you look at the graviton you've got the left-handed graviton which is a $+2$-homogeneity function and the right-handed graviton which was a -6.

Particle	**Helicity**	**Homogeneity**
Graviton	+2	-6
Photon	+1	-4

Very lopsided, but for the $+2$ one, you could see how to take cohomology. We will think of the cohomology in a very simplistic way. Twistor space and is basically two parts that are glued together. So, you have, one half of the top and half the twisted space and the other part and there's a glue (see Figure 6). You glue the two parts together and that gluing is done by means of a +2-degree homogeneous twistor function. And the gluing gives you a non-linear version of cohomology. If you just look at infinitesimal shift of one to the other, then that's given by a +2-homogeneity function. I can't explain this without going into detail.

It's cohomology as long as it's just an infinitesimal deformation. But if it's a real finite deformation then that gives you a general deformation of the twistor space. And you can look for the lines in this deformed twistor space. I should say that I give a lot of credit to Ted Newman, because Ted Newman was a very close colleague of mine. He worked in general relativity in Pittsburgh. I knew him well first, I guess, when I was in Syracuse because he was there for six months period or more. This was in 1961 when there were a lot of very good relativists, among them was Ted Newman. With Ted, we worked on spin coefficients. Ted had the idea of what he called H-space. It's a very ingenious idea.

If I had to explain it, I would have to talk about infinity first. I have found some insight initially when I went to a lecture by Andrzej Trautman when I was in Syracuse. Andrzej Trautman was one of the very distinguished polish physicists. And he worked with Ivor too.

Andrzej Trautman was also important in understanding what was going on in particle physics, which is you're really looking for connections and bundles. And I think he was really the first to understand that. Although I'm not sure how much credit he gets from that now. He was the first to understand what you're really doing in strong interactions. I think the weak interactions are a little bit obscure. Electromagnetism was much more well-known. When you're looking at strong interactions, you're looking in fact at connections and bundles also. But, this is not what Andrzej Trautman was talking about in Syracuse.

He was talking about gravitational radiation and he was looking at the leading term in the radiation. He saw that the field was null when you looked at it with spinors. This is what I was doing after the Finkelstein talk, when I started to worry about how two spinors could be applied in general relativity.

I thought about the Weyl curvature and I realized that it was completely symmetrical with two spinors. It had to factorize into four directions, so you have these four directions along the light cone, and the special cases are when they converge in different ways. So, you can have all separate or two converging or three converging or two and two. So, these are the different ways, this was a nice way of characterizing. This was done completely differently by Petrov classification.

I didn't know about these directions. But I was realizing that this was important by thinking about general relativity. This was actually when I came back from Syracuse. Andrzej was talking about the null case. This is where they all coincide. So, when you look at the leading term, when you go right out to infinity, and what is the dominant form of gravitational radiations where they all coincide, you have these all null directions along the light cone all pointing together outwards with the wave. So, I learned that from Andrzej's talk. On the other hand, I'd heard about Ray Sachs theorem about how the leading term is like, that's $1/r, 1/r2, 1/r^3, 1/r^4$. We used to call this the peeling-theorem as you come back they peel off one after the other like this.

So, when I got back to London, I was thinking about these things and I realized that this peeling-theorem came out immediately if you realize that infinity was conformally smooth. You see the history here was that, after Andrzej's talk, I began to think : can you do this another way' I don't like all these complicated calculations. I'm very impressed by what he could do. But I'm not good at doing complicated calculations. I'd much rather think about it geometrically, what's going on ? And so, I remember talking to Engelbert Schücking and I said well what we can understand about' Maybe we could do a conformal transformation, and I thought about conformally taking an inversion from a Schwarzschild solution. And I realized then that when you do invert it, infinity is singular. So, the point at infinity is singular, so it doesn't work! But Engelbert told me about Maxwell's equations being conformally invariant.

And I thought that's very beautiful. You have this conformal structure. You can change the scale factor in different places and this is another reason for being interested in the conformal structure. This was important for me. But, when I got back to England and began to think again about Ray Sachs peeling property, I realized that I was looking in the wrong place. If you look at space like infinity, it's singular yes. But how about a null-infinity where you go out along the light cones. And I thought at first, it must be singular. And then I looked at it and no, it's not! That's only $1/r$. It's not singular. That was an amazing revelation to me it's not singular and this gives you the peeling, all the leading term $1/r, 1/r^2, 1/r^3$. So that's the peeling property of Ray Sachs, simply a feature of the gravitational field, is finite. You have to think of it as a spin 2 field. And that spin 2 field is finite at infinity and that's facts is Sachs peeling. So, that was a big step for me and the right way to look at gravitational radiation. You do the conformal map and infinity is this cylinder.

That was a good way of thinking about radiation and you could understand Bondi's theorem about mass loss. It was a very nice way of looking at it. Looking at energy and momentum. And energy-momentum fits together as an energy momentum four-vector and you get momentum conservation as well as energy in terms of the Bondi formula and the generalization for momentum. But what about angular momentum?

Ted Newman started to think about what you needed basically was a surface of cross-section of this cylinderized infinity, which didn't shear. The shear at infinity looks like a saddle. The surface has intrinsic curvature to it so it's like a saddle circle. So, your curvatures in two directions are different. And it can be negative in one. And that's what gradation gravitational radiation does. You started off with something it looks spherical and it makes it saddle surface and you need it not to be saddle surface to do the angular momentum properly. So, what did Ted do, he says okay you move up your cylinder in complex direction, you don't just take a real time, you take a time that is complex, and you make get rid of the saddle surface. Crazy, completely crazy idea. He called this space H-space, the Heaven space. He had a nice Jewish joke. The Heaven space is where the good cones go. So, it's a good cone, you see, it's meant to be a cone. But when you take it with the sort of New York accent, it sounds like goy, where the good cones go. This was his Jewish joke, a New York tourist joke.

JJS & JK : Another subsidiary question concerns the development of spectral sequence and cohomological sequence in twistor theory.

RP : Well, I think it does. But it's hard to understand and I'm sure it does it in ways that need to be understood as the next level. I think that's probably what it is. It's a pity Michael Atiyah isn't around because I think he would have revealed in this. You see, I think the reason we've never understood it properly is that we haven't gone to the next level. You see, in the Dirac equation, he went from two spinors to four spinors. But that's not quite what you do with twistors. You go from the two spinors to the four spinors of the conformal group and those are the twistors. Now what you have to do is to go from the four spinors of the conformal group to the eight spinors of the octonions, the spin octonions and this is a new realization. I think it's a new realization. This is the quantized version. I never noticed this before. I'd known about the quantized twistors for years, but I never looked at the group which they satisfy. And this is G_2^*. So, it's that simple. It's the split form of this simple group G_2. And it's just exceptionally simple; in the Coxeter-Dynking diagram you have a triple connecting line.

JJS & JK : The group G_2 appears even in string theory and many other frameworks.

RP : I wouldn't be surprised if it doesn't have a role. I've never looked at these things. It is a shame. Well, I was too slow. I've been too slow in thinking about things. I should have thought of this idea three years earlier.

I'm always slow. From when I was at school, I was much too slow. I was too slow with my non-periodic tiling, just too slow because my father and Escher died just before I thought of the non-periodic tiling. Both of them would have enjoyed them so much. I'm afraid that I missed the boat twice, three times.

JJS & JK : People recently tried to use twistors in string theory.

RP : String theory is a difficult one because it's certainly been very motivating by physical ideas. In a certain sense, it's still incomplete. The string theorists could say well yes that's still incomplete too.

The trouble is that they don't pay much attention to dimensionality. And the problem is if you have a space in which your functions exist, a function of many variables then you have many functions, or a certain amount of functional freedom which in physics is very restricted. This functional freedom is really function of three variables. And it's because it depends on the space, you find that the temporal behaviour is governed by the spatial behaviour. If you want to deviate from that you have to see where all the extra functional freedom disappears. In string theory, people don't address that question. They try to say, well the energy is enormously large and to excite these extra degrees of freedom you need to have too much energy. But I think that argument is just wrong because the energy is not all that large. The energy is certainly much less than the energy of the earth in its orbit around the sun. The trouble is this energy is for the entire universe and the earth going around the sun has far more energy than you would need to activate these degrees of freedom. It's a point that has never been addressed by the string theorists. I mean, I tried to make this point, particularly in my book 'The road to reality'. And I had never seen anybody in the string theory world address those arguments.

JJS & JK : Did you discuss this issue with Edward Witten ? He is interested to make the bridge between string and twistors theories.

RP : You see twistor theory is not like those theories, because you've got to stick with space-time, which has the dimensionality we see. Now, you could say that twistors space is six dimensional or eight dimensions. Well, the projective space is three complexes dimensional, so it's already six dimensional. But that's not the point. The points are these Riemann spheres if you like, and they have to be restricted to the space $\mathbb{P}N$ which is the five-dimensional subspace and then you have a four-dimensional family of these things. So, it's the right number of dimensions. So, with twistor theory, the dimensionality comes out right, but people don't pay much attention to that.

But there is a weakness in twistor theory right from the start. I mean it's been a combination of puzzling issues and things working in a way which I had much better than I had anticipated. In a way, I hadn't anticipated.

I always regretted having my conventions the wrong way around. Now it wouldn't have mattered except that when Edward Witten got interested in twistor theory. He went to my original paper where all the conventions are wrong, and he took the wrong conventions.

Most particularly what concern the twistor points along the light cone or the null twistor. Now you see, I thought the points were in terms of a vector but it's not a vector it's a co-vector. I realized much later when you understand the quantum mechanics and everything. It's a momentum, it's not a vector it's a co-vector. But I didn't know that at the time, so I made it a pointing vector. It's really a dual vector. I corrected it afterwards in my next paper, but E. Witten didn't refer to my next paper maybe. I've always regretted that the upstairs (indices) was should be downstairs and downstairs should be upstairs.

JJS & JK : Did you have the occasion to tell Edward about this ?

RP : Oh, yes ! but that didn't get anywhere. But you see, I am fussy about signatures. And working in Lorentzian geometry, I believe you have to have one time and three spaces.

Very clear. Well Riemannian geometry has four spaces. Now many people who work in differential geometry in Oxford work with four $(++++)$. In fact, I worked with self-dual and anti-self-dual. That was fine for them because you can have real ones which are $(+++)$ signature, which is self-dual. Whereas Lorentzian, they've got to be complex. Now in twistor theory, Atiyah and his group also picked up on twistor. They did important things with Richard Ward who was a student of mine and who developed these ideas. His ideas were picked up on by pure mathematicians. They did a lot of very interesting work on Yang-Mills theory. But this means in the twistor theory, they're not using a 3-1 signature i.e. you're not using a space-time signature. But what Witten did, was a space-time which has two times and two spaces and then you make your twistor theory real. With the Lorentzian space-time signatures, the twistor space is 2-2 signature. With the positive definite space-time structure, your twistor space is quaternion. It's quaternionic twisted theory. With the Witten type, he has two times in two spaces and the twistors theory is real. It's a real space-time.

And I find this is going in a completely different direction. I can see why he did it. He did it because he wanted to be able to talk about delta functions and step functions and polyhedral. And this kind of notion which if you're stuck with holomorphic, you can't do. And you've to look at hyper functions instead of those functions. And hyper functions are functions where you look at the analytic approach which I think is the right one. But it's more complicated and if you want to look at things where you can talk about general functions which can have steps and delta function, then you face not discontinuous and not smooth and so on. Okay, you can make twisted theory real but that means a space time that has two spaces and two times dimensions. To me, that's not physics theory. It's too removed from the physics.

You see, I think the idea is that you do the physics in the wrong signature and then you do analytic continuations and you continue back to the right signature. I mean this goes back to these ideas for stationary space-time. It's quite useful to make the time imaginary, but it's very limiting if you want to think about physics generally. Because I mean your time is not such, time is just real time. It's all right if it's stationary but if it's a general space-time, this doesn't work.

So somehow, you're going into, in my view, the wrong direction. You may find that doing path integral or something like that is easier, because you don't have so many horrible divergences, if you're trying to minimize things and paths. But, in the Lorentzian space, it's a mess because they're not bounded, you can make them zero and the inequalities don't work somehow. This approach is limited because you're not doing what the physics is telling you to do.

You're doing something where the mathematics is attracting you in a certain direction which is limited. And if you're trying to do things like path integrals in a positive definite space where it may make some sense and then you try eventually to do a Wick rotation or something which means you change, your make things analytic and then you rotate back. This is not really, what physics is telling you to do. You're being motivated by things that you find are convenient mathematically but, in a way, not telling you where the physics is going. This is if I can express myself in a way which is perhaps slightly rude.

JJS & JK : You mentioned that all the time you were appealed by finding discrete numbers and complex analytic approaches. Do you think this new paradigm will play a role in the overall picture of physics in the future ?

RP : It's a good question. You see, they were more or less equal and when I was trying to think about physics in a deep way. I was equally divided between combinatorial ideas and complex analytic ideas. And I produced this spin network scheme which is purely combinatorial. And I played along with that. I'm not sure how far it went. But I remember, I was very curious, because when I talked about this lecture I heard given by David Finkelstein, Dennis Sciama had driven me to London King's college and this converted me to work in general relativity. But David Finkelstein was working in relativity and I explained to him spin networks. And he then went on to work on combinatorial physics. So, we swapped subjects. I was working in combinatorial physics and he was working on general relativity.

My own particular goal had been to try to describe physics in terms of discrete combinatorial quantities, since I had, at that time, been rather strongly of the view that physics and space-time structure should be based, at root, on discreteness, rather than continuity. A companion motivation was a form of Mach's principle, whereby the notion of space itself would be a derived one, and not initially present in the scheme. Everything was to be expressed in terms of the relation between objects, and not between an object and some background space.

I had concluded that the best prospect for satisfying these requirements was to consider the quantum-mechanical quantity of total spin of a system. 'Total spin' is defined by the scalar quantity $j(= 1/2n)$ that measures the amount of spin as a whole, rather than a particular component of spin in some direction, measured by a quantity m. (The letters 'j' and 'm' are those commonly used in the discussion of quantum-mechanical angular momentum, taken in units of $\hbar$, where m ranges, in integer steps, between the integers or half integers $-j$ and j) Moreover, though direction-independent, n is nevertheless intimately related to directional aspects of space. It had seemed to me that total spin, as measured by the natural number n, was an ideal quantity to fix attention upon if one were interested in building up, from scratch, some discrete combinatorial structure that leads to a notion of actual physical space. As a further ingredient, if one sets things up in the right way, one could exhibit the quantum-mechanical probabilities as being pure probabilities, not dependent in detail on the way in which different parts of a physical apparatus might be oriented with respect to other parts.

This was the sort of idea I had for getting pure probabilities, and I had formed the opinion that any such probability had to come out as a rational number (since it would amount to Nature making a random choice of some kind between a finite number of discrete possibilities). All the units in a particular spin network are imagined as being initially produced from initial 0-units (although this would not be normally expressed explicitly in the diagram), so there is no bias with regard to any particular spatial direction. Subsequently, various pairs of units may then be brought together to form single units, and the spin values for the resulting units are noted. Individual units are also allowed to split into pairs of units.

However, for the original spin-network theory, there was to be no actual background space-time presupposed. The idea was to build up all the required spatial notions simply from the network of spins and from the probabilities that arise (and these can be computed using quantum mechanical rules) when two units are brought together to make a third. A particular feature of these spin networks is that, at each such vertex, exactly three lines come together. This leads to uniqueness in the probability calculations. The topological (graph) structure of the spin network, together with the specification of all the spin numbers on the lines, is all that is required.

I developed an entirely combinatorial ('counting') procedure for calculating the required probabilities (which, in fact, are all rational). The rules originally come from the standard quantum mechanics of spin, but we can then 'forget' where they come from and simply consider the spin-network system as providing a kind of 'combinatorial universe'. It is then possible to extract the notions of geometry (ordinary Euclidean 3-geometry in this case) by considering spin networks that are 'large' in an appropriate sense. The picture is that a unit of large spin might be considered to define a 'direction in space' (to be thought of as like the axis of spin of a tennis ball, for example). We can envisage measuring the 'angle between the rotation axes' of two such large units by, say, detaching a 1-unit from one and attaching it to the other. The joint probability that one spin goes up while the other goes down, in this operation, gives a measure of the angle between the spin axes.

This almost works as it stands, but not quite, and a further ingredient is required. What is additionally needed is a means to distinguish the 'quantum probability', coming from the angle of spin axes between the large units, from the 'probability through ignorance' that can come about simply because of insufficient connections between the two large units.

It will be seen that the underlying motivation behind the spin networks that I originally had is very different from that underlying the loop variable approach to space-time quantization, there being no actual place for gravity in the spin networks, as originally put forward. Of course, there is something very much in common between the two programmes, because, in each case, one is trying to break down the notion of space into something more discrete and quantum-mechanical. There is, however, the important difference that in the loop-variable context, the quantity n is really an area measure, rather than the spin measure of the original spin networks. I regard as a necessary feature of the correct quantum gravity union that it must depart from standard quantum mechanics in some essential way, so that (state-vector reduction) R becomes a realistic physical process, but my original use of spin networks did not address such metric issues nor, in fact, any aspect of gravity at all, the spin numbers n referring to angular momentum. However, my original ideas demanded that each of these numbers must be, in effect, the result of an individual measurement of total spin value (action of R at each edge), where probabilities arise in the bringing together of two units to make a third. If R is an objective gravitational phenomenon, then the involvement with gravitational processes would have to enter at this stage. In that case, it is not possible to separate gravity from the probability issues of spin-network theory. It may be that the full combination of loop-variable and spin network ideas will need to incorporate state reduction into the formalism.

3 Cosmology

JJS & JK : You mentioned that after attending Finkelstein's lecture, you started to get interested in the problem of singularity.

RP : Yes, I was starting to think not in a way which was well. I learned enough about the

Weyl curvature that was important to know about. Because it was so simple for spinors. I think people were put off because you write the conditions down in tensors and it's messy. And so, they say, oh look I won't bother with that unless they're very good at computations and then they're happy.

But with spinors, it's just simple. For, it's all the same symmetrical, the equation, the Bianchi identities. It's just two wave equations and the relationships with other spins are just beautifully clear. I'm trying to write something even now on *CCC* with Christopher Meisner which uses a theorem which I'm just realizing how easy it is. Looking at week field gravity in terms of two spinors is so much easier than if you're using ordinary methods. And you can talk about the conservation laws of energy-momentum much more easily than you can if you try to write it all in conventional notations.

There is a theorem. Let me just tell you this much, I don't think Christopher Meisner wants me to talk about it too much because we're still writing a paper. You see in Maxwell's theory, you have a nice Gauss law. Let's think you're in space-time. So, you've got four-dimensional space time and you've got a charge current going through in space time surrounding these by two spheres. Now you do integral over the 2-spheres and you can work out the electric charge and the magnetic charge if there was a magnetic charge. So, the electric charge is an integral over the 2-sphere and the magnetic charge is zero. Now that's just to spin one. In two spinors, that's two indices, how about four indices ? Okay then look at spin two. And you have a quantity that changes the spin which moves you up and down into spin and that's a twistor. Twistor moves up by spin half so if you want to move up from spin one to spin two, you need a two-index twistor. That two index twistor converts the Gauss integral into a four integral and that four integral tells you the energy momentum inside the 2-sphere. Well, we can do that. I mean Ray Sachs wrote a paper where you do that sort of thing. But you can see immediately what this is because the twistor is the spin raising or spin lowering. Or I should say helicity lowering operator and the dual twistor is a basically raising operator.

JJS & JK : This gives us a connection with *CCC*. Anyway, we will ask you later on about the *CCC* and twistors theories.

RP : It's a beautiful connection, that's right.

JJS & JK : Another question concerning the singularity, because it seemed that you didn't share the same conception about the singularity with Stephen Hawking, right'

RP : That is true in a certain sense because the terminology was different. It is true that it appeared in the title of the paper. I think it was called 'space-time singular.' I can't remember the title of my paper, the one that got the Nobel Prize. Certainly, I didn't mention black holes because that terminology was not used then. "Gravitational collapse and space-time singularities", I think I did use the word "singularities".

But mainly the argument in the actual paper, I just said well look, there's a problem if the energy flux across that, light rays is never negative, maybe this that you have no Cauchy-surface, maybe you have non-completeness or something. I don't think I'd call it singularity. But of course, the idea is why does it stop' Why does your space-time stop' Well probably because curvature becomes infinite, but it doesn't say that. Now you see, when I gave my talk explaining my theorem at King's college in 1964, I remember J.L. Synge was there. He was a wonderful Irish relativist and he had written two books using a very geometrical point of view. So, I was very pleased he was there. He was not normally in London, but he went to my talk and I was very proud to have J.L. Synge present in my lecture not Stephen Hawking.

Stephen Hawking was not there, despite what the movie seems to imply. However, I gave a talk and Dennis Sciama heard about it. He was in Cambridge at the time. And so, he invited me to give a repeat talk in Cambridge. It was sometime in 1965. I think maybe January or February. And I gave a repeat talk, the same thing. And following the talk, I had a private session with Stephen Hawking and George Ellis possibly Brandon Carter but certainly George Ellis was there. And I remember describing the techniques I was using. And Stephen Hawking picked up very quickly on these ideas.

A few months later, he wrote a paper on using my actual theorem. Not changing the theorem but applying it in a cosmological context. So, you could derive a slightly weak theorem, but it was sure it was a theorem that showed that in cosmology that singularities were inevitable. But you see in cosmology, you don't have this problem. If you're looking at a local collapse, you're comparing an infinite initial surface with the finite region. You look at the boundary of the future of the trapped surface and it's, unless you have a singularity of some sort, a bounded finite region. And then you have a contradiction when you try to project that back into the infinite unbounded plane. You have a contradiction between the compact region in the black hole and the non-compact surface.

So, when Stephen Hawking tried to apply this thing in the cosmological context. He had to assume that the universe was non-compact. And so, he wanted not to have to assume that. So, he developed these ideas following this in many ways and introduced a lot of new ideas, new concepts. A lot of this was done with the help of Brandon Carter. I have to say Brandon Carter is an unsung hero in this story. Largely because Stephen had very good ideas. He was often a bit careless. So, there were sometimes mistakes in the arguments. But I would call these mistakes of the first kind, not mistakes of the second kind. When I say mistakes of the first kind, these are if you can get around them and correct them. And then they're all right. Mistakes of the second kind are when you can't correct them. That is just a disaster. These were all mistakes of the first kind, and Brandon Carter noticed the mistakes, pointed them out to Stephen. And Stephen would then correct them. This happened almost right up to the Ph.D. thesis when there was some mistake still in the argument.

But the movie was wrong there, in that Kip Thorne was not present. Dennis Sciama and I were present. But Stephen had already found all mistakes and corrected them. So that was fine. But these were not serious mistakes, I would say. The ideas were certainly very good, and he had ideas for developing the techniques I use.

Mainly looking at things like what he called Cauchy horizons. You could take a region and look at what is the boundary or what points are determined by the data on that surface. So, you look at the past light cone and you could see that all the time-like pairs which go through that point and do they meet this initial surface. And ideas like that which were very important and enable you to move these theorems forward in a way which I had not done. And that we got together later at the end and wrote a paper together. But Stephen had developed a lot of these ideas originally in three papers in the Royal Society. So, he certainly carried these ideas further and not exactly single-handedly but a lot of the work one of the important it was done by him.

JJS & JK : Did he use your space-time diagram ?

RP : Yes, he certainly knew about the diagrams.

JJS & JK : Because you see, maybe you don't know, but we were in the same lab with Brandon Carter, at Luth Lab at Paris Observatory and our offices were close.

RP : I think there's something that's honouring him. I think Brandon was a very important figure in the development of these ideas and to some extent, an unsung hero. I would say the he formalized the diagrams. There was a big argument about should you call them Carter diagrams. My ideas were not formalized to the degree that he had achieved. I mean he had certainly looked at many solutions and showed how these diagrams could be used in a very much more formalized way than I had. I was thinking of more as helpful ideas and not necessarily formalizing them. I would call them either conformal diagrams or strict conformal diagrams. And these strict conformal diagrams are really Carter diagrams, you could say that. And the general ideas if you like those were the ones I was playing with initially. But I just prefer to call them "disconformal" diagrams or strict conformal diagrams. But I'm not sure that even Brandon quite had this full strictness that I had eventually, with certain rules about when you have points which are filled in or unfilled in points and whether they represent circles. They're only spherically symmetrical solutions, strictly only apply in the case of spherical symmetry. And it's a question of whether the points at infinity are actually spheres or only points. So, the sphere went with the open points and the points with the closed points. So, I had a very strict definition of what these diagrams meant.

JJS & JK : Question about at the black hole singularity, when Weyl curvature goes to infinity, does the quantum gravity still have a sense ?

RP : Well you see this is a huge story and I think people are often very confused about that story. And the problem has to do with the theorems, particularly the theorems that Stephen and I developed and Bob Geroch. I should mention Bob Geroch was an important figure in this too. So, Brandon Carter, Bob Geroch and Stephen and George Ellis worked with Stephen on the book.

What I was concerned with initially was the gravitational collapse and the singularities in what we now call black holes and I sort of took the view that singularities are generic. That's the main lesson. Stephen said okay ; let's look at the big bang. Is that generic ?

Well you see, if it's a generic case you still get singularity, so you don't avoid it by having a bounce through a complicated non-generic type of singularity. So, that was the important argument he made. But the problem with all this is that the arguments are symmetrical in time. Everything we did future or past, we just change the sign. Go this way or that way, everything you do in one way it works the other way.

However, and I remember, this was an important very brief conversation I had with James Peebles. This must have been after the singularity theorems. I was probably still in London and visiting Princeton. And people were going from Princeton to Stevens Institute which was in Hoboken New Jersey on the river separating New York from New Jersey. Stevens Institute was a good spot for conferences on general relativity because the people in Syracuse could drive there from upstate New York and from Princeton. It was a good location. And they had frequent conferences there. I was about to join a car driving to this conference, and I noticed sitting in one of the other cars in the back was Jim Peebles. And I asked him a question, because I had been very worried about the fact that there were all these different cosmological solutions that had different kinds of singularities and were very complicated.

I asked him : 'well look there are all these complicated solutions in cosmology and all possible singularities you could have. Why do you cosmologists, only look at this simple case ? Why don't you look at all these other possible singularities for the structure of the big bang" And he looked at me and he said, 'The code of the universe is not like that' I thought my God "it isn't, is it ?".

I presume he was thinking about the microwave background and you see this very uniform structure. And it's not complicated like all these other solutions. So, this was to me, a huge point. It was not just, 'it's not like that', but "why is it not like that ?"

So, I began to worry about why the big bang is so special. And thus, it must be tied up with the second law of thermodynamics. And then I remember giving a talk somewhat later in Caltech. Here I have to reverse time slightly. Maybe I have to tell you about a conversation that somebody had with Richard Feynman.

Feynman had noticed that I was giving a talk about the second law of thermodynamics and cosmology. And he had told this colleague saying "oh look at this talk, this guy is talking about second law of thermodynamics and cosmology. I'm going to go and heckle".
I didn't know this. So, I gave my talk about cosmology and probably, I don't remember exactly, I was talking about the Weyl curvature and things like this. Somebody behind Feynman probably a Nobel Prize winner, because Caltech is full of them, started to heckle me. Suddenly Feynman turned around and pointed to the man and said, 'You shut up and listen to what the man is saying'. Feynman came up and said, "Look this is real advance it's really important" and I said "well look I don't know why is the Weyl curvature', and he said, 'don't care about the second question you've made a big important step".

JJS & JK : You said the big bang is of a special kind.

RP : Yes, the big bang has to be of a special kind. What is special about it is probably the fact that Weyl curvature is zero. I think that the singularity in the big bang is a very peculiar kind of singularity. It's that case for which the Weyl curvature is zero or very small.

Take the example of a gas in a box, as the entropy goes up, it makes more uniform. But if there are stars going around each other, entropy goes up when they become less uniform, the Weyl curvature goes up and you get these singular states in black holes. And it's still singular state, but it's not anything like the big bang. So, the key point or the quick question was why the singularities in gravitational collapse are so utterly different from the big bang one. Everybody was saying, even me, "oh, its quantum gravity, we've got to learn about quantum gravity". Dennis Sciama, my great hero, who taught me so much physics, was a great proponent of the steady state model which was a very beautiful model. But when the big bang, and Penzias and Wilson saw this micro background, Dennis went around giving lectures to say : "I was promoting this steady-state model, I was wrong". You don't hear people or physicists say that very often. I had a lot of respect for Dennis. There is a double irony here because Dennis got a lot of graduate students studying the big bang by looking at quantum gravity. The irony is that he was still wrong. Exactly, it's still wrong because it can't be quantum gravity. In the beginning, I was on Dennis's side and think that we've got to learn quantum gravity. Quantum gravity is a very peculiar kind of quantum mechanics that is grossly time asymmetrical. It has to be a time asymmetrical theory and so I tried to say how you can have an asymmetrical gravitational theory for which the Weyl curvature has to be zero in initial singularities. So, I postulated the Weyl curvature hypothesis : The Weyl curvature has to be zero in the initial singularities.

It doesn't make any sense from quantum gravity. I did believe then and I still believe now, this is because combining gravity with quantum mechanics is not quantum gravity. It's partial ; it's *gravitizing* quantum mechanics as much as it's quantum gravity. The collapse of the wave function has to be a gravitational effect. And the quantum mechanics, as we now understand it, is an incomplete theory. And the gravity was what changes it, but we're still stuck with that.

JJS & JK : So, you think that quantum mechanics should be changed ?

RP : Yes absolutely. But I believe that from way back, but not quite in the same way as I do now. I think it's a stronger and quite slightly different view. I used to think that quantum gravity had to be an asymmetrical theory. But I think it's wrong to call it quantum gravity. So, when I say Dennis was wrong and I was wrong, I'm trying to say that it's not what we would call quantum gravity. It's something else.

So, this is where *CCC* comes in. That idea came much later. That was more like 15 years ago, I can't quite remember when I first thought about that, but it was the idea partly influenced by Paul Todd, who was a very good graduate student of mine originally. What I'm saying is that quantum gravity is not giving us an answer because it's a not very fruitful way of looking at it. The more fruitful way is to take Paul Todd's suggestion more seriously. So, his suggestion was : "look, let's study the type of singularity that we happen to find in the big bang, which is very special. Rather than just saying the Weyl curvature is zero, let's say it's extendable as a conformal manifold".

So, I'd vaguely thought about this myself, but I've never worked with him. Todd remarked that the Weyl curvature is not certainly zero. So, it has to be finite to be extendable. He worked on this and worked a lot of equations on it. And then I thought about CCC, and thus, it's got to be zero. That's because of some theorems, particularly Helmut Friedrich's theorem : if you have a positive cosmological constant and you have massless sources, then in the generic case, you're going to get zero Weyl curvature at infinity.

Well, you see it was awkward because I think Paul had some problems with it. I remember in his way of looking at it. And I said no you've got to do it this way and it means you have to look at it slightly differently. I can't remember what the problem was initially right now. But, if you're going to make the Weyl curvature equal zero, you run into problems that look like negative curvatures or negative energies or something like that. It's tied in with the production of initial dark matter. I think that's what it was. You need to have the creation of new material which is dark matter and that's in order to make *CCC* work. You have to have this initial material. And I think with Paul's way of looking at it you didn't have that. I'd have to go back and remember what he did there. But you had to generalize what he was looking at, and the generalization allows you to talk about an initial state which has dark matter in it.

JJS & JK : Quantum mechanics is working very well, with very beautiful results. The problem arises when you try to combine the two theories : general relativity and quantum mechanics.

RP : In a sense, when people think about quantum gravity, if that's the right term, they're looking in an unhelpful direction. They're trying to see how very high curvatures, Planck scale energies, all these kind of things may change physics. Sure, it may. Why I say its unhelpful is because it's not guided by experiment.

Whereas the other ends of the subject which is how gravity might affect the structure of quantum mechanics is very borderline. And when I say borderline, it means in a good sense that we're close to seeing these effects. I mean, my colleague Dirk Bouwmeester who is a Dutch physicist, who've been working on an experiment for decades trying to put a little tiny mirror into a superposition of two locations at once by beam-splitting a photon and hitting it many times with this photon and seeing whether you can see if that maintains itself or it comes one or the other.

It seems that he was getting very close to getting a result here. But he seems to have problems with getting anything cold enough and having very worries about temperature fluctuations and things like this, which is a difficult problem. I don't know exactly where it's gone this experiment at the moment. The last time, I heard him, and he changed it rather, I didn't quite understand the version that he was talking about. There are other types of experiment involving things like beads, little tiny spots which are put into superpositions. It's a bit like the mirror but in a different way. Or Bose-Einstein condensates and I think that's quite a fruitful way of looking at it. I have a colleague Yvette Fuentes, who is interested in putting Bose-Einstein condensates and making big enough ones, so you can actually see if you put your Bose-Einstein condensate into a double way and split it into two locations. Does that persist, or does it tend to leak one into the other spontaneously or does one of them disappear and become entirely in the other ?

There is a new experiment that would see this effect. Because you can estimate at what level it should happen. So yes, these are things which are not out of the blue ; they're not simply waving your hands and, who knows where. One can estimate from looking at conflicts between the principles of general relativity and the principles of quantum mechanics, and when I say that the principle of general relativity, which is fundamental, I mean the principle of equivalence. That is to say the equivalence between a gravitational field and an acceleration. For instance, Galileo's idea of dropping a big rock and a little rock, do they fall together as long as you can get rid of the air resistance ? He appreciated that perfectly well air resistance would spoil it, then would they fall together ? And he says yes. Is that correct' Well, as far as we know, yes. What effect does that have on quantum mechanics' A big effect because if you try and do your quantum mechanics in a way which is invariant under these replacing of the gravitational field by an acceleration, you run into problem with the gravitational vacuum or the vacuum. And this problem can be resolved by the reduction of the state. So, it gives you an idea of at what level should you expect to see that a superposition between a mass in two locations simultaneously one there and one here. How long can you preserve such a superposition and one can make an estimate of how long that should be ? The experiments are not yet at the level to see this. But they're not too far.

It's Planck scale energy in a certain sense, but it's not high energies like in particle physics. You're looking at big energies because they're spread you're looking at big things.

JJS & JK : What do you think about loop quantum gravity ?

RP : It's better than strings. For a very important reason, they're using the right number of space and time dimensions. It's nearer to reality but it doesn't sort of come to grips with these questions as far as I can see. I don't know. I haven't seen a way of making it come to grip with it. I mean if it really did relate to the measurement issue. But you find amongst these loop people they're not very sympathetic towards *CCC*. You know I've talked to Ashtekar and of course lots of times with Carlo Rovelli and you know they take my view on board in the sense that they don't think I'm crazy. But they don't incorporate these ideas into it, at least I haven't seen it maybe I'm too limited in what I've seen them do.

JJS & JK : They use, to some degree, a version of the spin network basically.

RP : They do to some degree but that's not my spin network. It's more aiming at the right kind of thing. But on the other hand, I don't see them bringing in what I regard as essential. And what I've seen when they talk about the big bang. Ashtekar talks about these things and he certainly hasn't taken *CCC* on board at all. So, I mean, I'd be happy to look at it, if they can see a way of making the loops fit in with *CCC* in some sense that would be very exciting. I would be very keen to see some serious theory loop quantum gravity or whatever if it somehow could relate to *CCC*. You see the time asymmetry seems to be very important. They tend to look at more like bounces. You had something non-singular on both sides. There are other schemes, well like Steinhardt and so on. Then they have schemes where they try to get rid of inflation, but I can't see how to make that work. But I have a lot of trouble with it.

Ashtekar has never taken it seriously. But this is so basic. It's the second law of thermodynamics ; none of these theories take it seriously. See as far as I'm aware, *CCC* is the only theory, the only one I've seen which makes a serious stab at explaining and deriving the second law of thermodynamics. And you can say where does it go, where does the entropy go' Well it goes into the black holes. The entropy in the current universe is almost entirely in black holes.

By a huge factor, I'm just taking Beckenstein-Hawking as the formula for the black hole Entropy. And it dominates enormously absolutely everything else. Now this will go up and up and up and as the black holes swallow galactic clusters, the stars get swallowed and the other black holes get swallowed and eventually you just have one black hole. Finally, that evaporates away by Hawking evaporation. Maybe a lot of entropy goes out in the evaporation. Maybe it goes disappears in the singularity.

Doesn't make much difference to me, because in the conformal diagram, all that gets squashed into one teeny window point which on the other side is less than the Planck scale. So that means much much much less and it's not just less. It's horrendously less. So, what happens to all the information. So, it's lost.

It comes out that you get a lot of information energy coming out. That you get mass energy coming out of the next aeon but there's no room for that much information.

JJS & JK : What is the major problem in cosmology ?

RP : This is my very biased personal opinion. The major problem in cosmology is they don't take CCC seriously.

And to take it seriously, you've got to think about various things. One is mass decay because the mass does have to disappear. That's connected with Higgs but there is more complicated story I think. So, the mass has to fade out in the very remote future. But it only fades away. It's never quite zero, I think. But that's one issue you have to have no inflation. Inflation spoils the picture. However, you don't need inflation. Well first of all it's supposed to smooth out the universe, but it doesn't. The universe is already smoothed out by the exponential expansion of the previous eon. The problem I guess is that inflation is often introduced in order to explain get getting rid of local divergences and things. I don't know, I'm no expert on inflation ; I've deliberately not looked at it.

I've had long arguments with Alan Guth, he gets very upset about these things because of course he wants inflation. And I don't want inflation. Inflation would mess up any signal. If there was an inflationary phase of any significant degree, it would mess up signals coming from the previous eon. That would include the Hawking points. It would include the ring. Now you see, there are so many arguments and complaints. When Vahe looks at his rings, he did the analysis in an inappropriate way originally and so people complain about that. The paper was originally sent to the Royal Society and it was rejected and then they reconsidered it and accepted it on the condition that Vahe would produce his analysis. And this means of course doing the analysis. He didn't want to do this. So, we sent it to another journal and had been accepted before the change of mind had come from the Royal Society. They changed their mind already when it had been accepted by this other journal. I was rather keen on getting it back into the Royal Society but Vahe said in no way, he was fed up with Royal Society. He was going to send it to the European Physical Journal. So, we sent it there.

We did not put in the paper the analysis that he had done to prove the significance. Instead, we looked at the argument of what happens when you deform the sky and make them look like ellipses. And then the significance drops dramatically, that is to say, the number of rings you see, drops dramatically. However, it's not the kind of analysis that other people use, so they didn't pay any attention to us. We also had this second paper in which we looked at Planck analysis (not the WMAP). And you can see by eye the enormous anisotropy in the sources of these signals. And it's just completely obvious that it's not isotropic. People just say they pay no attention to it. Now you see a polish group, completely independently of us did an analysis completely different way much more conventional in their way of looking at it, and they kind came up with a confidence level of the rings. This is the rings not the Hawking points. Confidence level of 99.4 percent. Which people ignore because that's not enough signal for people in particles physics. It's fairly strong evidence. I think it was 99.6 in their original WMAP data. The Planck data was 99.4. But then later on, we did the Hawking point analysis which is completely different phenomenon. People were confused with this, because the way of doing the analysis which was very similar for the one of looking for rings and they thought 'oh, you're just looking at different rings'. Completely wrong, that was not what we were looking for. We are looking for spots. They should be called Hawking spots because that's what they are. And these spots have a 99.98 confidence level.

Now there is an irony here, which I won't even talk about. There is an irony, because I've had a long conversation with Alan Guth. And there is something which wasn't quite right in our paper. I'm not going to tell you what it was. We're now having a new paper that was not aimed at this thing not being right it was aimed at something quite different, but we have to correct this thing. And it's extremely interesting because what is not quite right in that paper, is more interesting than if it was right.

But I don't think I'll go into that. If you want to explain that, you can listen to him. I have to give him credit because he pointed out something which we hadn't noticed. It doesn't help him because these points if they're there at this level, inflation has to be wrong. These points don't come from inflation. Inflation would wipe them out. I can't see however in inflationary theory, you would get these spots. You would say 'look elsewhere effect'. In fact, that's what I was trying to say. What they call a 'look elsewhere effect'. I don't see how it can possibly be a look elsewhere effect. He has more of a case than I thought originally. Because there is a certain mistake which is very curious how this mistake happened.

4 Philosophy

JJS & JK : In your opinion, which philosophy best describes your view of the world or your way of thinking ?

RP : I am very platonic in the sense that the platonic world exists independently of humanity if you like. It's a world on its own, which does not depend on. It's being perceived by anybody. So, if there were no beings in this aeon that mathematics would still exist, just as well as if they were beings in this aeon. Suppose we were wiped out tomorrow by a nuclear explosion. People are being distracted by global warming instead you see. Then what happens to the mathematical world, it doesn't get destroyed just because people don't think about it, it's still there. So, I'm a Platonist in that sense.

But is the question that the world itself operates according to mathematical laws ? With that, I think too it does raise a very tricky question. If the world was a deterministically operating world, then there wouldn't be much scope, except in the initial conditions perhaps. You could say well you put all your uncertainties or whatever you liked in the initial conditions or you say reduction of the state according to standard quantum mechanics comes about through random effects. I don't believe in standard quantum mechanics because I believe that the reduction of the state is where the randomness comes in the Schrödinger equation. There is nothing random in the Schrödinger equation. The Schrödinger equation describes how the state evolves. It's a deterministic equation, where's the randomness' The randomness comes because you change your mind about what describes the world. You say "whoops, I thought it was the Schrödinger equation ! No, no, it wasn't". It's suddenly become a density matrix. Where did that come from' Well, because I'm ignoring certain degrees of freedom in the world or something. It doesn't make any sense.

People have their way of fumbling away and getting probabilities out of it. In my view, they're completely wrong because despite the fact that we have this really great very distinguished mathematician Von Neumann, I don't know what he really thought. It's very strange ; I wonder what they were really thought. I did talk to him about this question. I think he didn't have quite such a clear view of what he thought. He certainly thought one view was that somehow consciousness was what collapsed the wave function. But then, it doesn't tell you what consciousness is. Where does it come from ? So, you see, my view is turning that around and saying that consciousness comes from collapsing of the wave function. But where does that come from' Does the free will give us non-deterministic view ? Does it make any sense ? Don't ask me, I can worry about it and I probably will. It's not clear that it's deterministic. It's not even clear what that means. It's not clear whether there is a consciousness. Certainly, most of the collapse of the wave functions are not what we would call conscious in the ordinary sense. I have persuaded my colleague Stuart Hameroff who wanted to call all collapses of the wave function, conscious. Our argument was to say "no, no I call it proto-conscious". That's not really conscious. It's the building blocks out of which consciousness is constructed. Okay well, that's the view we take. But what does it mean to have this building block. Is that building block something which is deterministic in some deep way that we don't know' I don't know certainly. We don't know. It could be anything. It could be deterministic. I'm saying it's non-computable. That's no deterministic in the normal sense of this word. It's something very subtle in physics.

JJS & JK : You believe in a deep unity between mathematics and physics, do you think they are, in a sense, the same ?

RP : Yes, well it's not the same ; I think it would be a mistake to think of them as the same and even the motivations are different. Still, there is some overlap between the motivations. In mathematics, you're not constrained by having anything to do with the physical world. And most of mathematics doesn't have any direct physical significance. I mean you know I have this picture which I often drew with the three worlds : the world of mind, the mathematics and the physical world and how they relate to each other. And the picture was only a very small part of the world of mathematics you actually find encompasses the action of the physical world and only a small part of the physical has to do with our conscious understanding. So, only a small part of conscious understanding has to do with mathematics. So, it was a sort of irony or a paradox almost but somehow how does this little small part encompass everything in the next world and then the small part encompasses everything in the next world and the small part encompasses everything in the next one and I rather like that picture because it seems to indicate a paradox.

JJS & JK : Do you think that the mathematical world is more fundamental and primary than that of physics ?

RP : In a certain sense, but I'm not sure. I think I did have that view. Perhaps yes, it's sort of. I put it at the top which meant it is in a certain sense more primary. But I was trying to make them more on an equal footing in a way. So, I wasn't trying to emphasize that point and certainly most of mathematics sure it has its own existence. But the existence of that mathematics is of a different kind from the existence of physical things, and only a small part of the action of the mathematical world is found to have this direct relevance to physics. And complex numbers are a very big important part of that small part. So, the complex numbers are a small part of the small parts if you like

But nevertheless, they have an enormous implication with regards to quantum mechanics. And then I was trying to extend that. And I certainly was thinking about spin and I spent a lot of time trying to encompass spin as a basic notion, but it really wasn't enough.

And you had to have to go from the spinors to the twistors. So, you need to extend them. I mean twistors are again a form of spinors, but they are exactly not the spinors for the Lorentz group. They're spinors for the conformal group. And then there's another step, I think in the last few months, and I found satisfaction in that, which is only, that you actually have another step which has the twistors, you pair up a twistor with a dual twistor and then you find, they naturally have a split octonion nature or a G_2. This particularly strange simple group is this G_2 but the Lorentzian version which is G_2. I only learned this a few months ago by thinking about it. I sort of thought about this puzzle for years but not really getting seriously into it. And I realized that this is the quantized version of twistor theory, but what I didn't realize was that the algebra of the quantized version of twistor theory has this G_2 symmetry. It's there ; it's hiding there all the time !

JJS & JK : You don't like the big and huge group. You prefer the simple one.

RP : Yes, that's absolutely right yes.

JJS & JK : In the Road to Reality, you use the idea of sophistication when you consider complex numbers as the primary fabric of the Universe, the founding element of twistor theory, and the argument is based on two fundamental pillars :
- i) the extraordinary precision of scientific theories formulated in the language of mathematics and
- ii) the dependence of the accuracy of physical theories on the sophistication of the mathematical formalism used.

RP : You're right exactly. It's not just that mathematics is beautiful or clever or even sophisticated. Let me mention again one big area which is not an area which I have pursued. Which is the area of string theory. The initial idea I always found it actually rather beautiful. I liked the idea when I first heard about string theory. But what I did not like is how you had to go up into these high dimensions in order to make the theory work. Now you see in certain sense, people are very attracted by the mathematics of string theory. But to me, this is going off in the wrong direction. And it's a bit hard to explain what I mean here. It's in some sense as if you're forcing physics into mathematics.

It's partly to do with this world and only a small part of it seeming to have to do with the physical world and there's a lot of very beautiful, very sophisticated, very elegant powerful mathematics which as far as we know has almost nothing to do with the physical world.

And I guess, this is true in string theory. You have ideas which can influence many different areas of mathematics in ways which had not been thought of before. And this is a very powerful idea. But nevertheless, I don't see its connection with the physical world.

It's kind of they got lost. There may have been a connection originally with certain some of the ideas, may be Riemann surfaces and so on, when you're really at the basic beginnings of the idea of strings. But it's kind of gets departed from what at least can be seen to be directly connected with physics. And so, I think that there are too many people persuaded by the beauty in mathematics, which is there I'm sure, and to believe that physics has to fit in with it because it's so beautiful. And this is a dangerous position to take. So that's where I kind of diverge from where many people.

JJS & JK : Alain Connes has the idea that mathematics is independent of Human Being. And even, he illustrated this with the example of aliens : if we have to meet alien people, what we could have for communication would be the numbers, because they have the same atomic spectrum.

RP : Yes, I believe that. We have this paper which Vahe and I wrote, which also appears in the European Journal of Theoretical Physics, where we play with the idea that there might be communication by very advanced civilizations in the previous aeon. And they could send their signals. Which is completely crazy, but not so crazy, it might even be true. In some way, we have no concept of at the moment. There could be signals.

JJS & JK : What is your advice for the new generations ?

RP : Very tricky one. You see, when people ask that sort of question I tend to say : 'do what interests you, do what excites you.' That's fine, I guess, for people who are going to do really good work in science. But it needs an awful lot of people doing pretty dreary work. I mean a lot of calculations, that are done or under putting things on computers okay, if you find that exciting, that's good sure, that's very good.

You see, I wouldn't find it exciting myself but that's just my fault. We need to have people who find that exciting and that's very good because that's a lot of things where things go these days. You have to see how to put a problem on a computer and make the computer do the hard work. You understand what you're doing, all the understanding is done by the human, but the calculations are done by the computer that's fine. And if that's what excites you, great, that's fantastic, that's really good because that's very important and what will happen in science. It probably wouldn't help me much because I would find that a bit dreary but that's just my trouble. But people who find that exciting are very important in the world perhaps more important to science development.

I'm not sure that's a tricky question because I think there is a danger of getting wrapped up in the schemes and getting blinded to where they have to be changed. I think this happens in quantum mechanics. You see the theory works so beautifully well and you can do so many things that you could never do before. That it makes you think, it has to be right. And the people who try to argue there's got to be something wrong with it, somehow have to be dismissed as cranks. Most of them are cranks, that's the trouble.

It doesn't mean that there isn't something deep in what they're trying to do. But it's just as well that most people don't do that. But you have to realize perhaps the limitations of the subject so to see that there are limitations in quantum mechanics, it's probably rather important too.

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