...to talk of Twistors, Tilings and many a thing

Sir Roger Penrose in Conversation with Oscar García-Prada

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Penrose at Festival della Scienza, Oct 29 2011

Roger Penrose was born in Colchester (England) on 8 August 1931. His father, Lionel Penrose, was an expert in human genetics, and had a great interest in mathematics, which he ably communicated to his son. His older brother, Oliver, who would go on to earn a PhD in physics got him interested in physics. His mother was also attracted to mathematics, but her training was in medicine, like that of her own father. The Penrose family was undoubtedly an illustrious family in the British intellectual life of the 20th century.

Penrose went to school in his hometown until the family, owing to World War II, moved to London (Ontario, Canada), before finally returning to London (England), where he completed his studies. The first university degree he obtained was at University College London, followed by a PhD at the University of Cambridge—initially under the supervision of Sir William Hodge, and finally, under John Todd. As a doctoral student, Penrose's time in Cambridge coincided with that of Sir Michael Atiyah who, exactly as opposed to him, was initially supervised by John Todd, but went on to finally earn his doctoral thesis under the supervision of William Hodge. Penrose's doctoral thesis, completed in 1958, was dedicated to the study of tensor methods in algebraic geometry. Even before finishing his thesis, Penrose was deeply interested in physics. This was greatly facilitated by his interactions with Dennis Sciama at Cambridge – a physicist friend of his brother Oliver, and who many years later would eventually be the PhD thesis supervisor of Stephen Hawking – as well as the courses he attended by Hermann Bondi (PhD thesis supervisor of Sciama), and Paul Dirac.

After completing his thesis, Penrose received a NATO research grant that allowed him to spend three years in the United States, first at Princeton, and later at Syracuse

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University. Upon his return to England in 1962–1963, he was an Associate Researcher at King's College (London), before returning to the United States as an Associate Visiting Professor during the academic year 1963–1964, this time at the University of Texas in Austin. In 1964, he was appointed Reader at Birkbeck College (London), where two years later he would be promoted to Professor of Applied Mathematics. In 1973, he was appointed to the Rouse Ball Chair at the University of Oxford, where he went on to become Emeritus Rouse Ball Professor in 1998. His move to the University of Oxford once again coincided with the return there of Michael Atiyah, with whom he would initiate a long scientific interaction. In relation to this, Michael Atiyah would say:*

"When I was at Princeton at that time, before going back to Oxford, I talked with Freeman Dyson and we discussed Roger Penrose, and he said: 'Oh! Roger Penrose did some very good things regarding black holes, which I have always admired, but he also did some very funny things with twistors. I didn't understand them, so maybe, when you go to Oxford, you'll understand what twistors are.' And he was right, exactly right. That was the connecting link."

During the period 1983–1987, Penrose juggled his position at Oxford with the Edgar Odell Lovett Professorship at Rice University, in Houston. Other positions occupied by Penrose at various times include being the Gresham Professor of Geometry at Gresham College, London and the Francis and Helen Pentz Distinguished Visiting Professor in Physics and Mathematics at Pennsylvania State University.

Roger Penrose has received several prestigious awards and honours, with the most recent one being the coveted 2020 Nobel Prize in Physics that he shared with physicists Reinhard Genzel and Andrea Ghez – a crowing moment in his long and distinguished scientific career.

Penrose is married to Vanessa Thomas, Director of Academic Development at Cokethorpe School (near Oxford), and formerly responsible for Mathematics at Abingdon School, and with whom he has a son. He has three other children from a previous marriage to Joan Isabel Wedge.

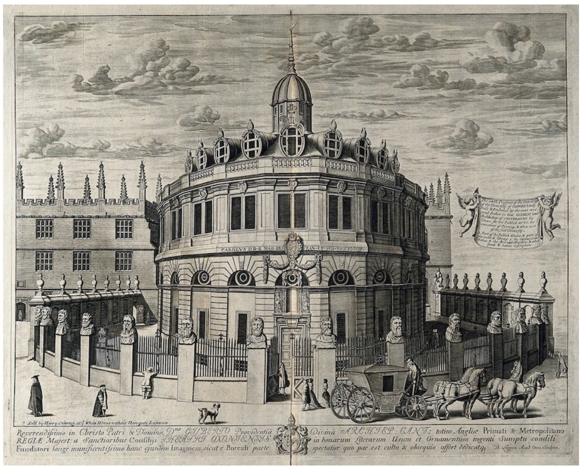
Personal recollections

My first encounter with Penrose took place in the academic year 1986–87 at Rice University (Houston), where I had begun my doctoral studies that very year under the supervision of Raymond Wells, working on a possible twistorial correspondence for the vortex equations of superconductivity—equations that are obtained via a dimensional reduction of instantons. As mentioned above, Penrose was connected to Rice University during the period 1983–1987, holding this position jointly with the one at the University of Oxford. Penrose had been invited by Wells, a renowned expert on Complex Geometry with a long interest in Twistor Theory, and who collaborated with Penrose and other experts. Before traveling to Rice, in my last year of university in Spain, I had been preparing myself in Twistor Theory and its relation with Yang-Mills theory.

Thus, the possibility of interacting with Penrose in the months he was at Rice, represented a real golden opportunity for me. Furthermore, I was extremely fortunate that Penrose invited me to visit the Mathematical Institute at the University of Oxford during the three summer months of 1987, with funding from a collaboration program between the Royal Society of London, and the Spanish National Research Council (CSIC)—the latter institution of which I have been a member, since 2002. I was not only able to interact with Penrose and his group that summer, but also with other mathematicians present there, such as Michael Atiyah, Nigel Hitchin and Simon Donaldson. This experience was not only very enriching but, in fact, even changed the course of my career, because the very next year, I moved to the University of Oxford to earn my PhD, under the joint supervision of Hitchin and Donaldson.

On my regular visits to Oxford over all these years, I have met Penrose on numerous occasions, not only at the Mathematical Institute, but also at the musical concerts held at the Sheldonian Theatre in Oxford (work of the great architect Sir Christopher Wren). Already during my very first stay in Oxford in the summer of 1987, I had bumped into him at several of such concerts organised as a part of the "Handel in Oxford" festival—those that took place for several years in Oxford from the mid to the late '80s to commemorate George Frederick Handel's first visit to Oxford in 1733; and also, at the premiere of Athalia, Handel's third oratorio, again at the Sheldonian Theatre. Penrose's mathematics, and Handel's music, have both had a strong and lasting impact on me, from thereon. The last time I met Sir Roger in person was at a concert during the closing ceremony of the conference titled "Science and Beauty", organised by Sir Michael Atiyah in 2015 at Edinburgh, and one in which I also had the honour of rendering a singing recital, accompanied by my musician friend, the British lutenist Din Ghani.

The following interview of him that I did about twenty years ago is, of course, a treasured memory for me.



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Sheldonian Theatre, Oxford: Line engraving by D. Loggan, after Sir C. Wren.

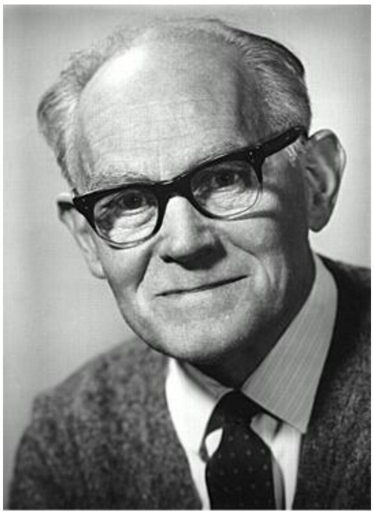
* Oscar García-Prada: *Interview with Sir Michael Atiyah*, Newsletter of the European Mathematical Society, 102 (2016), 22–30

When did you first get interested in mathematics?

RP: From quite an early age—I remember making various polyhedra when I was about 10, so I was certainly interested in mathematics then—probably even earlier, but it became more serious around the age of 10.

Are there other mathematicians in your family?

RP: Yes, my father Lionel Penrose was a scientist—he was a Professor of Human Genetics, but he had broad interests and was interested in mathematics—not on a professional level, but with abilities and genuine interests in mathematics, especially geometrical things. I also have an older brother Oliver who became a mathematician. He was very precocious—he was two years older than me, but four years ahead in school. He knew a lot about mathematics at a young age and took a great interest in both mathematics and physics; he earned a degree in physics later on. My mother also had an interest in geometry; she too was medically trained, just as my father was.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Lionel Penrose, Sir Roger's father

Did you have good teachers at school?

RP: I did have at least one teacher who was quite inspiring. I found his classes interesting, although maybe not terribly exciting.

Where did you go to school?

RP: I was at school in Canada between the ages of 8 and 13. I don't know that I got a great deal of my mathematics interests from there. I was back in England at the age of 14.

But you were born in England?

RP: Yes. We went over to the US just before the War. My father had a job in a hospital in London (Ontario, Canada), where he later became the Director of Psychiatric Research. He was interested in mental disease and its inheritance, the sort of thing that he became particularly expert at, later on. So, the question of inheritance versus environmental influence were of great interest to him.

I was born on 8th August, 1931 in Colchester in Essex—it's an old Roman town, possibly the oldest town in England. My father took on a project called the Colchester Survey, which had to do with trying to decide whether environmental, or inherited qualities were more important in mental disease. The conclusion he came to was that the problem was much more complicated than anybody had thought before, which is also probably the right answer.

This was before going to Canada?

RP: Yes. Then we went over first to the US when it started to become clear there was going to be a war. He had this opportunity to work overseas and he took it.

And when did you return to England?

RP: Just after the War, in 1945. I went to University College School in London, where I became more and more interested in mathematics, but I still hadn't thought of it as a career. I was always the one who was supposed to become a doctor, but I remember an occasion when we had to decide which subjects to do in the two final years. Each of us would go up to see the headmaster, one after the other, and he said "Well, what subjects do you want to do when you specialise next year?". I said "I'd like to do biology, chemistry and mathematics" and he said "No, that's impossible—you can't do biology and mathematics at the same time, we just don't have that option". Since I had no desire to lose my mathematics, I said "Mathematics, physics and chemistry". My parents were rather annoyed when I got home; my medical career had just disappeared in one stroke.


Pembroke College, Cambridge

Sir William Hodge (1905–1975) by Victor Coverley-Price

Where did you go to university?

RP: I went to University College London for my undergraduate degree. My father was a professor there, and so I could go there without paying any fees. My older brother had also been there as an undergraduate, and he then went on to Cambridge to earn a Ph.D. in physics. I went to Cambridge afterwards to do my Ph.D. in mathematics. I was mainly a pure mathematician in those days. I had specialised in geometry and went to Cambridge to do research in algebraic geometry, where I worked under William Hodge.

A contemporary who was also starting at the same time was Michael Atiyah, who later won the Fields Medal, became President of the Royal Society, Master of Trinity College at Cambridge, and was also the very first director of the Isaac Newton Institute. When you first become a research student you've no idea who the other people with you are. It took me a while to realise that there was something special about him. So, it was a bit intimidating, I remember, at the beginning.


COURTESY: MacTutor History of Mathematics Archive

John Todd

I worked with Hodge for only one year, because he decided that the kind of problems I was interested in were not in his line of interest. I then worked under John Todd for two years, but during that period I also became more and more interested in physics, largely because of my friendship with Dennis Sciama, who rather took me under his wing. He was a good friend of my brother's, and I think I made something of an impression on him when I visited Cambridge and asked him some questions about the steady-state universe, which I don't think he'd quite thought about. So, he probably thought it was worth cultivating my interest in physics.

So, was he one of the most influential people you came across?

RP: He was very influential on me. He taught me a great deal of physics, and the excitement of doing physics came through; he was that kind of a person, one who conveyed the excitement of what was currently going on in physics—it was partly Dennis Sciama, and partly lectures that I attended 'on the side' when I was in my first year.

I remember going to three courses, none of which had anything to do with the research I was supposed to be doing. One was a course by Hermann Bondi on general relativity which was fascinating; Bondi had a wonderful lecturing style which made the subject come alive. Another was a course by Paul Dirac on Quantum Mechanics, which was beautiful in a completely different way; it was just such a perfect collection of lectures, and I really found them extremely inspiring. And the third course, which later on became very influential although at the time I didn't know it was going to, was a course on mathematical logic given by Stourton W.P. Steen. I learnt about Turing machines and about Gödel's Theorem, and I think I formulated during that time the view I still hold—that there is something in mental phenomena, something in our understanding of mathematics in particular, which you cannot encapsulate by any kind of computation. That view has stuck with me since that period.

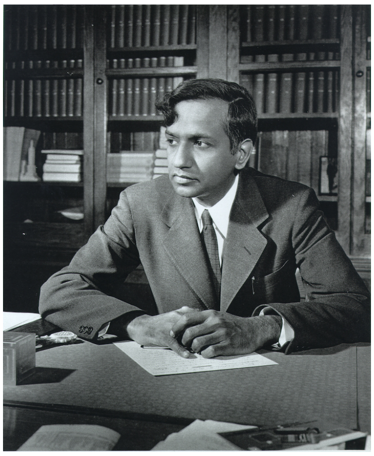
You've worked in many areas, but let me start with your 1960s work in cosmology. With Stephen Hawking you discovered the singularity theorems that won you both the prestigious Wolf prize. What are these theorems about, and what do they say about space-time?

RP: Well, singularities are regions of space-time where the laws of physics break down. The main singularity one hears about is the Big Bang, which represents the origin of the universe. Now cosmological models were introduced in accordance with the Einstein equations of general relativity, which describe curvature of space-time in terms of the matter content. The equations determine the time-evolution of the universe. You apply these equations to a very uniform universe, which is what people did originally, assuming that the universe is homogeneous and isotropic, in accordance with the standard models that are used to describe cosmology on a large scale. If you extrapolate Einstein's equations backwards, you find that at the very beginning was this moment where the density became infinite and all matter was concentrated in a single place. The Big Bang represents the explosion of matter away from this—in fact, the whole of space-time originated in this single event.


With Dennis Sciama at the 1962 Relativity Conference in Warsaw

Some people used to worry about this, just as I did, because it represents a limit to what we can understand in terms of known physical laws. The same situation arose later when people started to worry about what happens to a star which is too massive to hold itself apart, and singularities arise. Back in the 1930s, S. Chandrasekar showed that a white dwarf star, which is a really concentrated body, can have the mass of the sun, or a bit more. We know that such objects exist—the companion of Sirius is the most famous one—but if such a body has more than about one-and-a-half times the mass of the sun, then as Chandrasekar showed, it cannot hold itself apart as a white dwarf, and will continue to collapse; nothing can stop its collapse. A white dwarf is basically held apart by what's called 'electron degeneracy pressure'—this means that the electrons satisfy an exclusion principle which tells you that two electrons cannot be in the same state, and this implies that when they get concentrated, they hold the star apart. So, it's this exclusion principle in effect that stops a white dwarf star from collapsing.

However, what Chandra showed is that gravity will overcome this force if the star is too massive, and then its electron degeneracy pressure cannot hold it apart. This problem occurs again in what's called neutron degeneracy pressure, which is again the exclusion principle but now applied to neutrons. What happens is that the electrons get pushed into the protons and you have a star made of neutrons. Those neutrons hold themselves apart by not being able to be in the same state. But again, the Chandrasekar argument comes to bear on the neutron stars and you find that they also have a maximum mass which is believed to be not much more than that of a white dwarf. So, anything with, say, twice the mass of the sun would seem to have no resting place and would go on collapsing, unless it could throw off some of its material. But it seems unlikely that it would throw off enough material under all circumstances, especially if it started with a mass of, say, ten times the mass of the sun.



COURTESY: University of Chicago

S.Chandrasekhar as a young faculty member at the University of Chicago

So, what happens to it? Round about 1939, Robert Oppenheimer and various students of his—in particular, Hartland Snyder—produced a model of the collapse of a body. As an idealisation, they considered a body made of pressure-less material, which was assumed to be exactly spherically symmetrical—and they showed that it will collapse down, to produce what we now call a black hole.

A black hole is basically what happens when a body is concentrated to such a small size for such a large mass that the escape velocity is either the velocity of light, or even exceeds the velocity of light—the escape velocity being the minimum speed at which an object thrown from the surface of the body escapes to infinity, and doesn't ever fall back again. It's about 25,000 miles an hour for an object on the surface of the earth. But if you concentrate the density of the earth so much, or take a larger body with a mass of, say, twice the mass of the sun and concentrate it down so that it is now just a few miles across, the escape velocity will then reach the speed of light. And then it becomes a black hole, if the escape velocity exceeds the velocity of light, so that nothing can escape, not even light.

This is exactly what happened in the model that Oppenheimer and Snyder put forward in 1939. But it didn't catch on. Nobody paid any attention to it, least of all Einstein, as far as one knows. I think the view of many people was that if you remove the assumption of spherical symmetry then the exact model that Oppenheimer and Snyder had suggested would not be appropriate, and who knows what would happen? Maybe it would not concentrate into a tiny thing in the centre, but would just swirl around in some very complicated motion and come spewing out again—I think this was the kind of view some people had. And you begin to wonder whether assuming that there's no pressure was even a fundamentally correct assumption to start with, because matter does experience pressure when it gets concentrated.

This was revived in the early 1960s when the first quasars were discovered. These extremely bright shining objects seemed to be so tiny, yet were so massive that one would have to worry about whether an object had actually reached the kind of extreme density limits that I've just been talking about, where you wouldn't see it if it was really inside what's called the event horizon, and where the escape velocity exceeded the velocity of light; but if you got close to it, then very violent processes could be seen taking place in its vicinity, which could also consequently produce extraordinarily bright objects. When the first quasar was observed, people began to worry again about whether what we now call black holes might not really be there out in the universe.

So, I began thinking about this problem and the whole question of whether the assumption of exact spherical symmetry could be circumvented, using techniques of a topological nature which I had started to develop for quite other reasons. What people had done till then was just solve complicated equations, but that's in itself not very good if you want to introduce irregularities and so on, because you simply can't solve the equations. So, I looked at this from a completely different point of view, which was to look at general topological issues: Could one obtain a contradiction from the assumption that the collapse takes place without any singularities? Basically, what I proved was a theorem which was published in 1965 in Physical Review Letters, where I showed that if a collapse takes place until a certain condition holds (a qualitative condition which I called the existence of a trapped surface), then you could expect to see some type of a singularity. What it really showed is that space-time could not be continued, and that it must come to an end somewhere—but it doesn't say what the nature of that end is, it just says that space-time cannot be continued indefinitely.

In a seminar in Durham, 1982

Can you test this theory in our universe?

RP: Well, the first question is: do black holes exist? They are almost a theoretical consequence of the kind of discussion I've just referred to. Then Stephen Hawking came in as a beginning graduate student working with Dennis Sciama, and he took off from where I'd started, introducing some other results mainly to do with cosmology, rather than black holes. Later, we put our results together* and

* Stephen Hawking and Roger Penrose, *The nature of space and time, With a foreword by Michael Atiyah*. The Isaac Newton Institute Series of Lectures. Princeton University Press, Princeton, NJ, 1996. x+141 pp. ISBN: 0-691-03791-4

showed that singularities arise in even more general situations than we had individually been able to handle before.

Now there is a big assumption here to which we still don't know the answer. It's called Cosmic Censorship, a term I introduced to emphasise the nature of this hidden presumption, and one that is often tacitly made. Cosmic censorship asserts that the so-called 'naked singularities' do not occur. We know from the singularity theorems that singularities of some kind do occur, at least under appropriate initial conditions that are not unreasonable—but we don't know if those singularities are necessarily hidden from external view. Are they clothed by what we call a horizon, so one can't actually see them? With a black hole you have this horizon which shields that singularity from being viewed from the outside. Now it's conceivable that you could have these naked singularities, but they're normally considered to be more outrageous than black holes. The general consensus seems to be that these don't happen, and this tends to be my view also. If you assume that they don't occur, then you must get black holes. So, it's a theoretical conclusion that if you have a collapse of a body which is beyond a certain size, then you get black holes.

> “ there is something in mental phenomena, something in our understanding of mathematics, which you cannot encapsulate by any kind of computation.

Now one type of system that astronomers have observed is where there is a double star system, only one member of which is visible. The invisible component is taken to be a black hole—Cygnus X-1 was the first convincing example. It's an X-ray source, and what is seen is a blue supergiant star which is in orbit about something; the 'something' is invisible through a telescope, but seems to be a source of X-rays. Now the X-rays would come about if material is dragged into a tiny region, and gets heated in the process of being dragged in; the material probably forms a disc, which is also the view people have. The material gets dragged off the companion star, the blue supergiant star, and it spirals into the hole, in the standard picture. It gets hotter and hotter until it reaches X-ray temperatures, and eventually becomes a source of X-rays; this is also what's observed.

Now this doesn't tell you that this object is actually a black hole, but the dynamics of the system are such that the invisible component has to be much too massive to be either a white dwarf or a neutron star, because of the Chandrasekar argument, and so on. So, the evidence is indirect: what one knows is that there is a tiny highly concentrated object which seems to be dragging material into it, and from the neighbourhood of which one sees X-rays emanating. Also, gamma ray sources seem to be black hole systems, and there may now be many other examples, other double star systems, or black holes in galactic centres. Indeed, there is convincing evidence for a very concentrated dark object at the centre of our own galaxy, of the order of something like a million solar masses.

It seems to be a standard phenomenon that galaxies may have these highly concentrated objects which we believe to be black holes in their centres. Some galaxies may have large ones, and quasars are now believed to be galaxies which have at their centres objects that are much brighter than the entire galaxy—so all you see is this central region which is extraordinarily bright. It's bright because it has dragged material into it, and it gets extraordinarily hot and spews things out in certain directions at nearly the speed of light. You see examples of things where jets emerge out of centres of galaxies, and things like that. But all this evidence is still indirect. It's not that one knows for sure that black holes are really out there, it's just that the theory tells us that there ought to be black holes there, and the theory fits in very well with the observations. But most observations do not directly say that these objects are indeed black holes, although there's impressive recent evidence of material being swallowed by one without a trace. There's also another potential possibility of the direct observation of a black hole: when I say **'direct'**, it's more because the theory of black holes is so well developed that one knows very closely what the relevant geometry should be. There's a geometry known as a Kerr geometry, which seems to be the unique endpoint of a collapsed object forming a black hole, and this geometry has very interesting specific properties. Some of these could be tested to see whether the concentrated objects that we know are out there, really do conform with the Kerr geometry. That would add much more direct evidence for black holes, but it's something for the future.

What would be the most striking physical implications of the singularities here?

RP: What the singularities tell us is that the laws of classical general relativity are limited. I've always regarded this as a strength in general relativity. It tells you where its own limitations are. Some people thought that it was a weakness of the theory because it has these blemishes, but the fact that it really tells you where you need to bring in additional physics is a powerful ingredient in the theory.

Now what we believe is that singularities are regions where quantum theory and general relativity come together, where things are both small and massive at the same time. 'Small' is where quantum effects become important, and 'massive' is where general relativity becomes important. So, when you get these two things happening together, which is what happens in singularities, then the effects of both general relativity and quantum mechanics must be considered together.

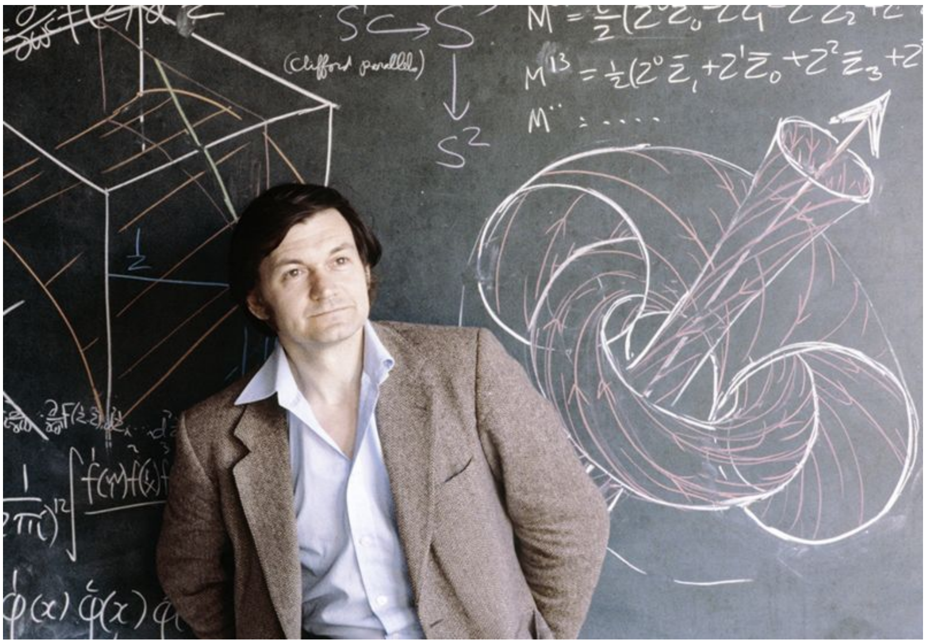
Now this applies both in the Big Bang, and in the singularities in black holes, and it would also apply if the whole universe were to ever collapse—although that would just be a conglomeration of all the black holes, into one bigger black hole. There's one thing I find particularly interesting, however, which is the stark contrast between the Big Bang and the singularities in black holes. It's a bit ironic, because in the earlier stages of the black hole singularity discussions, their reasonableness was that we already know there's a singularity in the Big Bang. It was argued that the singularities in black holes are just the same as the Big Bang, but time is going the other way—so if you have one, you should have the other. This was quite a plausible kind of argument, but when we look at these things in detail, we see that the structures are completely different: the structure that the Big Bang had was very smooth and uniform, whereas the structure we expect to find in singularities is very complicated and chaotic—at a completely different end of the spectrum.

In fact, this is all tied up in a deep way with the second law of thermodynamics. This law tells us that there is a time asymmetry in the way things actually behave. This is normally traced far back in time to some very ordered structure in the very early stages of the universe—and the further you trace it back, the more you find that this ordered structure is indeed the Big Bang.

So, what is the nature of that ordered structure in the Big Bang, and what is its cause? Well, in relation to what I was just saying, it is quantum gravity. We believe that this is where quantum theory and gravitational theory come together. And what this tells us—and I've been saying this for quite a few years but few people seem to pick up on this completely obvious point—is that the singularity structure, which is where we see general relativity and quantum mechanics coming together most blatantly, is time-asymmetrical. So, it tells me that the laws involved in quantum gravity, combining quantum theory with general relativity, must be time-asymmetrical, whereas the laws we normally see in physics are time-symmetrical.

It also tells us, it seems to me, that the laws of quantum gravity are not just concerned with applying quantum mechanics to general relativity—when I say 'just', it's a gross understatement because nobody knows how to do that, but I think it must be a union between these two theories, giving a new theory of a different character. It's not just quantum mechanics: quantum mechanics itself will have to change its structure and it will have to involve an asymmetry in time, but I have reason to believe that this is all tied in with the measurement problem—the collapse of the wave function, the curious features that quantum theory has which makes it in many respects a totally unsatisfying theory from the point of view of a physical picture, or as a philosophically satisfying view of the world. Quantum mechanics is very peculiar, because it involves incompatible procedures. My own view is that this is something that we will only understand when we've brought Einstein's general relativity in with quantum mechanics, and combined them both into a single theory.

So, my view on quantum gravity is quite different from that of most people. What most people seem to say is "Oh, you've got to try and quantise general relativity, and quantise gravitation theory, and quantise space-time": to 'quantise' means to take the rules of quantum mechanics as they are, and try to apply them to some



Anthony Howarth/Science Photo Library

Roger Penrose in 1980

classical theory, but I prefer not to use that word. I say that the theory we seek also involves a change in the very structure of quantum mechanics. It's not simply quantising something; it's bringing in a new theory that has standard quantum theory as one limit. It also has standard general relativity as another limit, but it would be a theory that is different in character from both those theories.

Let me come to another aspect of your work. One of your greatest inventions is twistor theory, which you introduced around 1967. What is 'Twistor Theory'?

RP: Well, the main object of twistor theory is to find the appropriate union between general relativity and quantum mechanics. I suppose I had this view for many years (actually, from 1963 on), before I talked about this singularity issue and the asymmetry, and so on. I'd already felt that one needs a radically different way of looking at things, and twistor theory was originally motivated by such considerations. Since we can't just 'quantise', we need other guiding principles.

Let me mention two of them. One was non-locality, because one knows about phenomena in which what happens at one end of a room seems to depend on what happens at the other end. These experiments were performed in the early '80s by Alain Aspect in Paris—all right, those experiments hadn't been performed when I introduced twistor theory, but the original ideas were there already—I mean the Einstein-Podolsky-Rosen phenomena, which tell you that quantum mechanics says that you have these 'entanglements'—things at one end of the world seem to be entangled with things at the other end. Now that's only a vague motivation: it's not really something that twistor theory even now has a great deal to say about, but it does say that somehow non-locality is important in our descriptions, and twistor theory (as it has developed) certainly has features of non-locality, over and above those I was aware of when I started thinking about these ideas.

Originally, rather than having points in space-time as the fundamental objects, I thought more in terms of entire light rays as being fundamental. The reason for thinking about light rays actually came from something quite different, which I regard as perhaps the most important motivation underlying twistor theory. In the union between quantum mechanics and general relativity, I feel strongly that complex numbers and complex analytic structures are fundamental to the way that the physical world behaves. I suppose that part of my reason for this goes way back to my own mathematical training. When I first learnt about complex analysis at university in London, I was totally 'gob-smacked'—it just seemed to me an incredible subject; some of the simplest ideas in complex analysis, such as, that if a function is smooth then it's analytic, are properties which I always thought were totally amazing.

What are twistors, and how are they more fundamental than a point in space-time?

RP: Well, you see, if I follow the complex analysis well enough, I can come back to this. First of all, complex analysis is just mathematics, and it's beautiful mathematics that's tremendously useful in many other areas of mathematics. But in quantum theory you see it being present at the root of the subject—for the first time one sees that it's really there in nature, and that nature operates (at least in the small scale) according to complex numbers.

Now the thing that struck me from quite early on—it's one of the earliest things I did in relativity—is that if you look out at the sky, you see a sphere; but if you consider two observers looking out at the same sky, one of whom is moving with a high speed relative to the other, then they see a slightly transformed sky relative to each other, and the transformation of that sky preserves circles, and takes angles to equal angles. Now those people who know about complex analysis know that this is the way you look at the

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The London University as drawn by Thomas Hosmer Shepherd and published in 1827-28. This building in now part of University College, London, which was founded in 1826.

complex numbers: you have infinity as well, and they make a sphere—the Riemann sphere—and the transformations that send that sphere to itself, the complex analytic transformations, are precisely those that send circles to circles, and preserve angles too. I was completely struck by this phenomenon, as it seems to me that what you're doing when you look at the sky is that you're seeing the Riemann sphere—they are these complex numbers just out there in the sky, and it seemed to me that that was a kind of an appealing mathematical connection. It seemed to me to be such a beautiful fact, and in a sense, the transformations of relativity are all contained in that fact. Surely that should mean something. We already know that complex numbers are fundamental to quantum theory, and here we see complex numbers being fundamental to relativity too, when we look at it this way.

> “ I'd say that the whole programme of representing Einstein's theory in terms of twistors is what I'm proudest of.

My view was to say "all right, don't think of the points you see when you look at the sky; what you are doing is seeing light rays. You and a star in the distance are both connected by a light ray, and the family of things that you see, as you look at the sky, is the family of light rays through your eye at this moment." So, the thing with the complex structure is light ray space, telling you that maybe you can see this link between space-time structure and complex numbers if you concentrate not on points, but on light rays instead.

So that was really the origin of twistor theory—well, that's cheating slightly, but I suppose one cheats when one gets used to a certain idea, because although these phenomena were known to me and I realised their importance, it was something else that really steered me in the direction of twistor theory. It's a bit technical, but had to do with complex numbers—all right, you see them in the sky, but you also see them in all sorts of other places—in solutions of Einstein's equations, and so on—they started to come up when people looked at specific solutions of Einstein's equations. It turned out very often that you could express things very nicely if you used complex numbers, and it suggested to me that somehow—I had this image of an iceberg, you see—what you see is a little bit at the top and there's the rest of it down underneath, which is invisible. It's really a huge area where these complex numbers at the tip poke up through the water, while the rest of it is underneath.

So, these solutions, where one sees the complex numbers, seemed just the tip of an iceberg, and they were really underneath governing the way that the first-hand structure works. It was a search to try and find what that complex structure was, and it wasn't until certain things that are not appropriate to describe here were clarified, that things became clear. These things are related to solutions of Maxwell's equations and Einstein's equations which show you that the space of light rays, although not quite a complex space because it's got the wrong number of dimensions, but looking at its structure, can be seen as being part of another structure, a slightly extended one with six dimensions; and this now effectively produces a complex objective space, which is also a complex projective 3-space.

Now with hindsight I can describe these things more satisfactorily. Let me put it like this. When you think of a light ray, that is a photon idealised in a specific way, and where you are just thinking of it as a path through space-time. But you have to bear in mind that massless particles (photons, in particular) also have spin (they spin about their direction of motion), and if you introduce the spin, they also have energy. The spin is a discrete parameter. It's either left-handed or right-handed, but when the particle has spin, introducing the energy (a continuous parameter) imparts one more degree of freedom. So instead of having just five dimensions of light rays, you find a six-dimensional space that is naturally the complex 3-space. So, you've got the whole thing, the right-handed ones, the light rays and the left-handed ones, and they all fit together to form a space that's called *projective twistor space*.

And it seemed to me that once you take this space as being more fundamental than space-time (the main reason being that it's complex), it ties in with other things that I've been interested in for years—the use of spinors and how you treat general relativity, things which I'd learnt in Bondi's and Dirac's lectures. This notion of spinors, as a way of treating general relativity, was something I found to be powerful, but it didn't quite do what I wanted, which was to get rid of the points. That was what twistor theory achieved, and it's still going on.

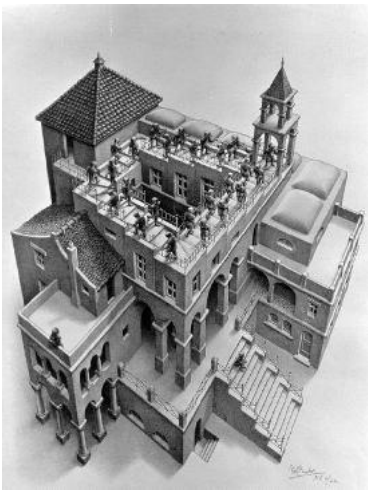
So how do twistors actually relate to these singularity theorems? Do they have anything to say about those theorems?

RP: The short answer to that question is no—or, not yet. The hope is that they will, but the subjects have been going off in two quite different directions. Twistor theory is motivated by trying to bring general relativity and quantum mechanics together. If it's successful in that direction, then it would have something to say about the singularity problem, but at the moment it has very little direct bearing on the singularity problem. I regard it as a very long roundabout route, but one needs first to understand how Einstein's general relativity really fits in with twistor theory. Although considerable advances have been made, some dating back to the '80s, it's still a question mark. We don't completely know how to represent Einstein's theory in relation to twistors; there are some very strong indications that there's a good connection between the two, but how one does it is still not clear.

So, my view is that the major problem in twistor theory is to see how to incorporate Einstein's theory into the twistor framework, and it's still not complete. What we seem to see is that in the process of incorporating Einstein's theory into twistors, we also have to incorporate ideas of quantum mechanics. So, my hope is that in bringing classical general relativity into the scope of twistor theory, one will also see how quantum theory must be made to combine with general relativity, and in that combination, one will see how to deal with singularities, because that is the place where the combination of the two theories comes in. Also, there must be a time asymmetry in the way it comes together, and that will explain the difference between the past and the future singularities. But all these things are hopes—they're not something I can do now.

Twistor theory has been tremendously successful in applications within mathematics, but has it been helpful in understanding the nature of the physical world?

RP: Not very much, I would say. It's rather curious, but I would say that this is not unique to twistor theory. One sees it in other areas—like string theory, for instance—where people start with great ambitions to solve the problems of physics, and instead come up with ideas that have had implications within mathematics; this is certainly the case with twistor theory, its applications and its interest.* If you rounded up all the people who claimed they worked on twistor theory, you'd find, I would think, that a vast majority of them were mathematicians with no particular interest in physics—they might be interested in differential geometry, or integrable systems, or representation theory. Very few of them would have physics as their prime interest, so it's kind of ironic that here's a theory that's supposed to be answering the problems of physics, and yet it's not caught on at all on the physics side.



mcescher.com/Wikimedia Commons

Ascending and Descending, by M.C. Escher. Lithograph, 1960. The image illustrates a remarkable dialogue between mathematics and art

You mentioned string theory. Are there connections between twistor theory and string theory?

RP: I think there probably are. It's not something that has been deeply explored, and the groups of people who work on these subjects are more or less disjoint. There have been some attempts† to bring the theories together, but I think that the right vehicle for doing so hasn't come about yet. I wouldn't be at all surprised to find that in the future some more significant links between these two areas are found, but I don't see it right now.

These new theories involving p-branes seem to be more suitable, somehow?

RP: Well, there is a connection, but I don't know how significant it is. I was talking to Ed Witten some time ago, and he was telling me about the 5-branes they're interested in. But that's curious, because in work that Michael Singer did some years ago with Andrew Hodges and me, the suggestion was made that what one should really be looking at is generalisations of strings. Whenever you see an ordinary string, you should really think of it as a surface, because it's a string in time. It's one-dimensional in time, so that gives you two dimensions. These things are studied very much in connection with complex one-dimensional spaces (Riemann surfaces), so they are in some natural way associated with these Riemann surfaces.

Now what we had in mind, which was much more in line with twistor theory, is to look at a complex three-dimensional version of this, which we called pretzel twistor spaces; they're complex three-dimensional spaces, so they are six real-dimensional, and if you can think of them as branes in some sense, then they are 5-branes. Now is there a connection between those 5-branes and the 5-branes of string theory? I just don't know, and I haven't explored it. I didn't mention it to Witten when I talked to him, but there might be something to explore here. That's just off the top of my head, I don't know, but yes, it might be that there's a connection there.

In the early 1970s you discovered two chickens that can tile the plane in a way that must be non-periodic. How did you find these non-periodic—or perhaps, I should say aperiodic—tilings?

RP: Yes, aperiodic tile sets, I suppose, but the tilings are non-periodic. Tiling problems have always been a doodling side interest of mine, just for fun; if I got bored with what I was doing I'd try and fit shapes together, for no particular scientific reason—although I supposed that there was some connection with my interest in cosmology, in that there seem to be large structures in the universe that are very complicated on a large scale,

* Here are two books on the topic jointly authored by Penrose: 1. Roger Penrose and Wolfgang Rindler, *Spinors and space-time. Vol. 1. Two-spinor calculus and relativistic fields*. Cambridge Monographs on Mathematical Physics. Cambridge University Press, Cambridge, 1987. x+458 pp. ISBN: 0-521-33707-0 83Cxx; and 2. Roger Penrose and Wolfgang Rindler, *Spinors and space-time. Vol.2. Spinor and twistor methods in space-time geometry*. Second edition. Cambridge Monographs on Mathematical Physics. Cambridge University Press, Cambridge, 1988. x+501 pp. ISBN: 0-521-34786-6 83Cxx

† Michael Atiyah, Maciej Dunajski and Lionel J. Mason, *Twistor theory at fifty: from contour integrals to twistor strings*, Proceedings of the Royal Society A. Vol 473, Issue 2206, October 2017.

A 4 meter high sculpture "Penrose-Triangle", built by *Treffpunkt Physik*, in Gotschuchen, Austria. The sculpture is an optical illusion. The two beams seen touching each other at the top do not do so in reality. They only seem to be touching each other, but even this apparent touching is observed only when viewed from a specific angle and position. The camera that took the photo is positioned exactly in such a position.

whereas one believes that they should be governed by simple laws at their roots. So, I tried to find a model where we have simple local structures that produce great complications when scaled to much larger areas; I had an interest in certain types of hierarchical design. So, I played around with such hierarchical tilings, where you form bigger shapes out of smaller ones; the bigger ones you produce have the same character but are on a larger scale than what you just did. I also had an interest in Maurits Escher, and his work and met him on one occasion: I had produced single tile shapes that would tile only in rather complicated ways, and Escher himself used one of these in his last picture.

What was the name of these tiles? The magic something?

RP: That's different: those are impossible objects. The staircase and the tribars that people now call the 'impossible triangle' were things my father and I played around with. Later, Escher incorporated them in some of his pictures: 'Ascending and Descending' used the staircase, and the 'Waterfall' used the triangle. And he actually used ours, because we sent him a copy of our paper.

I met Escher once, and left him a copy of a puzzle I'd made which consisted of wooden pieces which he had to try and assemble. Well, he managed to do this all right, and somewhat later when I explained the basis on which it was constructed, he produced a picture called Ghosts—as far as I'm aware it was his last picture, when he was quite ill—and it's based on this tile I'd shown him—twelve different orientations of this shape.

But that was just a sideline, an amusement really, and the way the tilings came about was in two stages. I'm sure I owe a debt to Johannes Kepler, although I didn't realise it at the time, because my father owned a book showing the picture that Kepler designed which had a number of different tilings that he played with. Some of these were of pentagons, and these tilings with pentagons are very close to the tiling shapes I produced later.

Now I was aware of these things because I'd seen them, but they were not what I thought of when I was producing my own. They just coloured my way of thinking, which must be rather similar to what happened to Dan Shechtman when he discovered quasicrystals. He hadn't thought about my tilings, but when I spoke to him later, he said he was aware of them. I suspect that it's the sort of thing that puts you in a 'kind of' frame of mind, so that when you see something, you're more receptive to it than you would have been otherwise. So yes, I'm sure it's true of me with Kepler that I was more receptive to his kind of design.

These three-dimensional forms of your tiles have appeared in recent years, as Quasicrystals. Did you ever anticipate such applications of your non-periodic tilings?

RP: Well, I did, but I was overcautious I suppose, because I certainly knew this was a theoretical possibility. But what worried me was that if you ever tried to assemble them, you'd find it very hard, and without a kind of foresight, it's difficult not to make mistakes. I sometimes gave lectures on these tilings, and people asked me 'does this mean that there's a whole new area of crystallography'—and my response would be 'yes, that's true—however, how would nature produce things like this, because they would require this non-local assembly?'. And it seemed to me that maybe you could synthesise such objects with great difficulty in the laboratory, but I didn't see how nature would produce them spontaneously.

Now I think, although people now understand them better, the situation is much the same. I still don't think we know how they're produced spontaneously, and there are different theories about how they might come about—maybe there was something a little bit non-local, something basically quantum-mechanical, about those assemblies which I came to think is probably true, but it's not an area that people are agreed about—in fact, it's not totally agreed that quasicrystals are this kind of pattern, although I think it's getting pretty well accepted now.

> "There was no line – you couldn't say what was recreational and what was professional work.

I was first shown the physical objects, the diffraction patterns, by Paul Steinhardt at a conference in Jerusalem to do with cosmology. I was talking about general relativity and energy and he was talking about inflationary cosmology, and he came up to me and said, "Look, I want to talk to you about something that has nothing to do with this conference". He showed me these diffraction pictures that he'd produced, and it was quite startling but very gratifying—in fact, curiously enough, I wasn't completely surprised. I suppose I felt that it must be right and nature is doing it somehow. Nature seems to have a way of achieving things in ways which may seem miraculous; this was just another example of that.

Have any other examples of your work in recreational mathematics found reflections in natural phenomena, or in physics?

RP: One thing I got from my father is that you never draw a line between the two. He was like that—he did things for fun, which might be making children's toys out of wood—puzzles, or gadgets connected with his work. He'd make things like complicated slide rules that were supposed to test some statistical results. He made a bust of one of his patients, I seem to remember, and then he spent his later years producing wooden models that reproduce themselves. There was no line—you couldn't say what was recreational and what was professional work.

In 1989 you published a best-selling book, *The Emperor's New Mind*,* where you're concerned with computers and artificial intelligence, the mind, the laws of physics, and many other things. What is the central question you address in this book?

RP: I think I mentioned earlier that I had formulated a certain view while I was a graduate student. Before that, I'd been quite sympathetic to the idea that we were all computers, but it seemed to me that Gödel's theorem tells us that there are aspects of our understanding which you cannot encompass in a computational picture. Nevertheless, I still maintain a scientific viewpoint that something in the laws of physics allows us to behave the way we do, but that the laws of physics are much deeper. My view is that we know much less about them than many people would maintain.

So, I was quite prepared to believe that there was something outside computation. I've been interested in mathematical logic for a long time, and I'd known for a long time that there are things of a mathematical nature that are outside computation; that didn't frighten me—it just seemed to me 'all right, why not?' Then I saw a television programme where Marvin Minsky, Edward Fredkin and others were making outrageous statements about how computers were going to exceed everything we could do. What they were saying was logical if we were indeed computers—but since I didn't believe that, it seemed to me that this was something they'd completely missed—and not only had they missed it, I'd myself never seen it anywhere else. So, as I'd previously considered writing a popular or semi-popular book on physics—something I thought I'd do at some stage in life, but perhaps not until I had retired—this provided an opportunity: 'All right, let's explain things about physics and what the world is like, as far as we know, but with a different focus—let's explore the laws of physics to see whether there's scope for something of a non-computational character'. I'd never seen anybody put this forward as a serious viewpoint. Since it seemed to me that it needed to be put forward, that's what I did.

So, this was an attack on artificial intelligence; but you also proposed that since quantum mechanics was incomplete, the understanding of physical laws had to precede any understanding of the functioning of the mind.

RP: Yes, I'd felt that if there was something non-computational, it needed to be outside the laws that we presently understand in physics, because they seem to have this computational character. And it also seemed to me that the biggest gap in our understanding is when quantum theory relates to large-scale objects, where the rules of quantum mechanics give us nonsense—they tell us that cats can be alive and dead at the same time, and so on, which is nonsense; we don't see our world like that. Yet quantum theory was supposed to be so absolutely accurate, so why are we not aware of the manifestations of that theory on a large scale? It seemed to me that the theory can't be quite accurate and that there must be some changes that take place when it gets involved with large-scale objects.

I think I'd already thought that this had to do when gravitational phenomena started to become entangled with quantum effects—

* Roger Penrose, *The Emperor's New Mind: Concerning computers, minds, and the laws of physics. With a foreword by Martin Gardner*. The Clarendon Press, Oxford University Press, New York, 1989. xiv+466 pp. ISBN: 0-19-851973-7

that's when the changes start to appear, and there are good reasons for believing that. So that is what I believed in at the time, and still believe; but when writing *The Emperor's New Mind*, I didn't really have any clear idea on where in brain function, quantum effects could start to become important and have an influence on large-scale effects, and where these new physical processes that should be non-computable, could come in.

So, I started writing the book, expecting that by the time I'd finished I'd have some clearer ideas on these. This happened to some degree—I was very ignorant about lots of things that had to do with the brain when I started, and I had to study hard to write the chapters specifically on them, and in particular the idea of 'brain plasticity'—that the connections between neurons can change, and that these changes can take place quickly—it seemed to me that this was very important, and that the new physics comes in to regulate these changes.

So, has this given you any clue as to what changes are needed in quantum mechanics?

RP: A little, but the physical motivations are largely independent. I did, however, think about the needed changes in quantum mechanics a little more seriously than before, and I changed my views between writing the two books.

You're now referring to your second book *Shadows of the Mind*.* Did you pursue the same problems in this book?

RP: Yes, but *Shadows of the Mind* had slightly ambivalent purposes. Originally, I started to write it to address some of the points that arose out of people's criticisms and misunderstandings of *The Emperor's New Mind*, and in particular my treatment of Gödel's theorem. I just didn't expect the kind of vehement responses that I got. I was very naive, perhaps. I didn't realise that people would feel attacked in the manner they did, and would therefore respond by attacking me, even while misunderstanding a lot of what I was trying to say.

The main point that I was making about Gödel's theorem was that if you have a system which you believe in, and which you also believe might be usable as mathematical proof, then you can produce a statement which lies beyond the scope of the system, but which now you must also necessarily believe in. Now there is an assumption here that the system is consistent, something which I didn't bother to stress particularly, because it seemed to me quite obvious that there's no point in using the system if you don't think it's consistent: the proof is no proof if it's in a system you don't thoroughly believe in and therefore don't trust its consistency. That seemed to me to be obvious, but I didn't make those points strongly and so there are loopholes that people could point to, which of course they did. So, I felt it necessary to address these issues with a great deal more care in Shadows. It was not meant to be a particularly long or popular book; it was just addressing these points and was quite technical and complicated in places, but in the process of writing this book, two things happened.

With Jean-Pierre Bourguignon, Durham 1982

One of them was that I received a letter from Stuart Hameroff telling me about the cytoskeleton whose structures inside cells I was totally ignorant of. But most people who work in artificial intelligence didn't seem to know about them either; Marvin Minsky didn't, as he told me afterwards. But it seemed to me that here was a completely new area for which it was much more plausible that quantum effects could be important. They are much smaller structures than neurons and are much more tightly organised structures. The most relevant of these were microtubules, where one has a much more credible arena for coherent quantum phenomena to take place. It is still hard to see how this takes place, because it is difficult to maintain quantum coherence on the large scale that one needs in order for these ideas to work. One needs to go beyond what can be done in any physics lab today, and there is no physical experiment performed today that can achieve the kind of quantum coherence at the level that I would need in order for the kind of phenomena to take place, and that I suspect are taking place all the time in our brains. So, nature has been a lot cleverer than physics has been able to get so far, but why not? It seems to me quite plausible that this is the case. The cytoskeleton, and in particular microtubules, seem to me to be structures where quantum coherence is much more plausible. So, I changed the nature of this book. I wanted to put in microtubules and the cytoskeleton, and so I needed to learn a little bit more about it and to express why I think that's important in brain action.

The other thing which happened was that I somewhat shifted my viewpoint on quantum state-reduction, in relation to gravity. The viewpoint I'd held for quite a long time, and which is expressed in *The Emperor's New Mind*, is more or less that, if you have too big a discrepancy between two states, then they don't superpose and the state reduces. This discrepancy is to be measured in terms of space-time geometry; I called it the one graviton criterion. It is to do with how many gravitons come into this difference between the two states.

* Roger Penrose. *Shadows of the mind: A search for the missing science of consciousness*. Oxford University Press, Oxford, 1994. xvi+457 pp. ISBN: 0-19-853978-9

Now in work that I did subsequently, and also in work that had been done by others (particularly Lajos Diósi, a Hungarian, and Giancarlo Ghirardi in Italy) who had developed different ideas in connection with quantum state-reduction, it seemed to me that I needed to modify the view that I had before. I think it's quite a significant modification. Basically, when you have two states that are significantly different from each other, then their superposition becomes unstable, and there is a calculable time scale involved in how long it takes for the superposition to 'decay' to one state or the other. The details are probably a bit too technical for here, but there indeed is a finite time scale, rather than an instantaneous reduction, and this time scale produces figures that are much more plausible—also, it is easier to use and it may be much more relevant to brain action.

> "Consciousness seems to be such a different phenomenon from the other things we see in the physical world...

One can see how to use it, and these ideas were developed in collaboration with Stuart Hameroff. We produced a number of suggestions about how this idea could be carried forward, but again we found a great deal of opposition. A lot of these ideas are clearly speculative, but I don't think it is that speculative that something like this is going on. It seems to me it has got to be. Consciousness seems to be such a different phenomenon from the other things we see in the physical world that it's got to be something very special, in physical organisation. I can't see that it's really just the same old physics put together in more complicated systems. It's got to be something of quite a different character from other things that are important in the way the world operates. This new physics would be only occasionally employed in a useful way, and you have to have a very careful organisation that takes advantage of whatever is going on in state-reduction, and channelise it in a direction which makes us operate. But it is very rarely actually taken advantage of in physical systems, where most things don't use this phenomenon in any useful way at all.

In these books you reveal yourself to be a philosopher. How have you come to terms with the Great Mystery?

RP: Well, there are lots of hugely unanswered questions—there's no doubt about that. I'd certainly want to emphasise that even if everything that Stuart Hameroff and I say turns out to be absolutely correct, it would still not answer these questions. I hope we are moving a little in the right direction towards answering those questions. However, I think there's very little progress towards answering the deep questions of what is going on in mentality, who we are, what is consciousness, why are we here, and why does the universe allow beings who can be self-aware, or if there is life after death? Any questions of that nature seem to me to require us to know more about what the world is like—we really know very little.

People say, and physicists often say: we nearly have the solution to the grand theory of the universe which is just around the corner, the theory of everything. I simply just don't believe that. I think there are major areas of which we have almost no understanding at all, and it's quite curious that one can have a view which seems to encompass (at least in principle) most of the things that you see around you, and of why they behave this way or that way.

One of the major things that isn't explained is simply ignored and just swept under the carpet by physicists generally, which is quantum state-reduction. They say: quantum theory is a beautiful theory; it works perfectly well and describes how tiny particles behave. But, to put it bluntly, it gives you the wrong answer. What the theory tells us is that, for example, if you had Schrödinger's famous cat, the cat could very easily be put into a state of being alive and dead simultaneously. That's simply wrong; it doesn't do that. So, what is it? I mean, there is something big missing from our view of the world, it's huge and it's not just a tiny phenomenon which we haven't quite got hold of, because we've got to get even that last decimal place right, and the coupling constant or something: it's a huge aspect of the way the world behaves which we simply do not understand, and understanding it is, in my view, one small step towards understanding what mentality is. I think it must be a part of it, but it is still not going to answer the question of mentality. We may well know what state-reduction is (and I expect that we will eventually know, if only we don't destroy ourselves first); then that theory will have as part of its nature some completely different way of looking at the world from the way we have now. We've already seen that happen in Einstein's general relativity, because before that we had Newton's theory which told us how bodies attracted each other with forces and moved around and so on. It's beautifully accurate: Newton's theory is incredibly precise. It tells you how the planets move around in their orbits to almost complete precision—not quite, but almost. You might think it only requires a little tiny modification to make it completely right, but that's not what happened. Einstein produced a theory that is so completely different. Its structure is utterly different from that of Newton's theory, but it gives almost exactly the same answers. The philosophical framework of that theory is quite different. The very nature of space is warped, and that changes our whole outlook. Now what I am saying is that when we see how quantum theory is to be changed to accommodate state-reduction as a phenomenon, with the measurement process as a phenomenon, we shall have to come to terms with a completely different way of looking at what matter is, and what the world is like.

Are you saying that this is a step that will help us to address questions that are typically addressed, by some people at least, by religion?

RP: Well, it will, I think so. But you see, that I'm more 'tolerant' is not quite the right word. I think I'm a little bit more supportive of religious viewpoints than many totally scientific people would be. It's not that I believe in the dogma that is attached to any of the established religions, because I don't. On the other hand, religions are trying to address questions which are not addressed by science, particularly moral issues. I regard morality as something with an absolute 'platonic' component which is outside us. Is there really a platonic absolute notion of 'the good'? I have to say, I'm inclined somewhat in that direction. I think morality is not just man made, but there are things outside us which we have nothing in the way of scientific understanding of at the moment, but which nevertheless are part of a big overall picture, and which maybe someday will all come together. So, I mean these questions are nowadays considered not to be part of science, but they're part of religion, you see. Well, as I say, I don't believe in the dogma of religion, but I do think that religion is groping for something which we don't know the answers to yet, and which is outside traditional science. So, I suppose what I think is that the whole scientific enterprise must broaden its scope, and eventually perhaps even change its character.

We have touched on only a fraction of your work but what is the result, theorem or theory that you are most happy with?

RP: Well, I'd have to say twistor theory. The non-periodic tilings are nice, and they're something to show to somebody. But this is not something which is as deep, and it's not something I've devoted so much of my life to. Even when I call it 'twistor', this is slightly inappropriate, because we don't know what the theory is yet, the real theory. But if I can answer your question in that way, I think I'd say that the whole programme of representing Einstein's theory in terms of twistors is what I'm proudest of. I don't know if you'd call that an answer to your question, because it's not a theorem, it's not one result, it's a body of ideas. I suppose in amongst those, I would think the 'non-linear graviton construction' is probably the one thing which I feel most pleased about so far, but it is yet part of a bigger programme, and I don't regard that as an endpoint; it's something on the way. I hope that when one really understands how the Einstein equations can be incorporated into twistor theory, we'll see something, a much broader picture of which that is just a part. But we don't quite have that fully yet.

How do you actually work? How do you select a problem? I've seen you many times in your lectures and seminars making pictures—do you visualise things that way?

RP: A lot is very visual, but not entirely visual. Certainly, some things I've done are not entirely visual; for example, algebraic things aren't. But I do find visual imagery absolutely essential in my work, and that's the way I usually do it. I often start to write something down, but it doesn't help very much. I may see a picture sometimes, but even drawing it on a piece of paper doesn't help, because it's not something you can still express very well that way. It's an image which is hard to put down, but I do find expressing things by drawing pictures reasonably accurately helps sometimes, but sometimes it's an accuracy that's also very misleading. You've got to know what it means. I mean, the pictures are not really accurate because I might draw a picture of something which has got the wrong number of dimensions, and it's really in a complex space rather than a real space, but you get a feeling for knowing what's reliable and what isn't in a picture. In some ways it's a relief when one can do a calculation. If you can reduce something to a calculation, then you can deal with it; but so few of the things I do actually find their way into calculations, because that's not the problem. The problem is a conceptual or a geometrical problem—often conceptual: you have to look at things in the right way. And that can be quite hard sometimes. But I suppose what I am probably best at, when it comes to mathematics, is the geometrical, where I can visualise things well. I don't find it very easy to work with complicated formulae or analytic notions. It's an uphill job sometimes. To a certain extent, things in analysis can be looked at geometrically.

> “ Nature seems to have a way of achieving things in ways which may seem miraculous.

What are you working on right now?

RP: Well, what I think about most of the time when I'm not doing something else, like trying to write up endless things one after the other, is this problem about the Einstein equations and twistors, which I think is almost there, or is very close to being resolved; but it's not completely clear. I regard that as the major problem. I also try to make progress with the quantum state-reduction problem—even with some possible experiments.

You've mentioned a number of mathematicians and physicists, but who is your most admired mathematician or physicist?

RP: That's a tough one—there are a lot of them. I'm not sure there's a single one as a mathematician. I always had a tremendous admiration for Bernhard Riemann because he was a miracle man. As a mathematician, I think it probably would be him, I guess, but it's not quite clear because on the physics side I've always thought of Galileo Galilei, Isaac Newton, J.C. Maxwell and Albert Einstein as being the four most major figures. Of those, I suppose one would have to say that Newton was a more impressive figure than Einstein, no matter how impressive Einstein is. I mean, he's had absolutely wonderful ideas. General relativity is a fantastic theory—of any physical theory that I've seen, I'd put general relativity at the top because it's amazing. But Newton was such a powerful mathematician, and Einstein was less so. Einstein had these tremendous physical insights, but Newton had this great power as a mathematician, as well as deep physical insights. And I've always had a soft spot for Maxwell, and also for Galileo—I think I've always admired him from a long time back. I don't know—I'm not trying to single out any one in particular. Riemann as a mathematician I suppose, possibly more than Carl Friedrich Gauss, I would say. Archimedes also, I suppose, was a very impressive character; as also Leonhard Euler.

What is your opinion about the direction in which research in mathematics and physics is going nowadays?

RP: Something I've found slightly discouraging about the way things go these days, which is not the fault of the mathematicians or the physicists, is the problem of technology. Although technology is a wonderful thing, it has the effect of magnifying fashions. It's so easy to communicate ideas from one end of the world to the other instantaneously, and this means that the fashions have global control over what goes on. It means that a lot of progress can be made in fashionable areas,* but there's something which is lost in there, I feel. It's a bit silly to hark back or think about things in this way, but when communications were much more difficult there were these little pockets of people working on different things. Maybe I have this starry-eyed view of what it was like; it probably wasn't like that. Fashions were still important in the old days too, but somehow there's something about the global nature of the fashions these days which I find a bit disturbing. But it's worse in physics than it is in mathematics, I think. I always used to think that mathematics was immune from this kind of thing. You would have these people working away and developing their wonderful ideas in relative isolation, and occasionally getting together, and some things would spring from the coming together of different viewpoints. But now it's much more as if you can go anywhere and they're all working on the same thing, which is rather discouraging. In physics, it is particularly like that. This applies to highly theoretical work where there is no stabilising input from experiment.

Communication is obviously beneficial, but the aspect you're remarking on is actually a negative one.

RP: It is a negative one, I think. And there's another thing that goes along with this. It's not relevant in mathematics, but in physics it's to do with the expense of big experiments, which means that you have to build bigger and bigger machines to look at higher energies, and so on. I can see why they do that, but since the experiments are so expensive, they require a lot of money and government backing and huge organisations, and therefore you must have committees to decide which to support, and so on. Committees have to decide where the money's going, so they consult people who are considered to be the experts in the relevant areas, and therefore the things tend to get locked in certain directions because the experts have already got there, as they were the ones who were important in the development of the current theories. It's hard to break away from this.

It becomes a political thing as well.

RP: It does become political as well, because money is involved. One doesn't have the free-ranging way of thinking about things that was there before. So, I'm only expressing the negative points, because maybe the positive ones are more obvious. Obviously, there's huge activity—all over the world you find people who work in areas and who have never before had a chance to think seriously about science. Now the internet allows them to get involved. That's all positive and I agree with that, but I'm just pointing out that there's a downside also, which I find disturbing.

University of Oxford

With Stephen Hawking

Are you questioning the way in which what's important and what's not are decided?

RP: It is hard to advise them, you see, because they're caught up in the system, and if young researchers want to get jobs after doing research, they must work in an area which is going to be recognised by the people who employ them. If I were talking from the point of view of science, I'd say that in quantum state-reduction there are some important problems: you see why quantum mechanics needs modification. But if they're working on that, it's regarded as marginal at best, and crackpot at worst. I could even say that with twistor theory: in mathematics it seems to have caught on, but as a physical theory—if you work in twistor theory you'll find it hard to get a job. There are very few people in physics departments who know about this subject and consider it important.

I remember reading in one of your papers that twistor theory was an esoteric subject from that point of view. Do you still maintain this view?

* Roger Penrose. *Fashion, faith, and fantasy in the new physics of the universe*. Princeton University Press. Princeton, NJ, 2016. xvi+501 pp. ISBN: 978-0-691-11979-3

Oscar García-Prada

Penrose tiling of the floor at the entrance of the Andrew Wiles building in Oxford

RP: Yes, it's not much studied as a physical theory. I'm not unhappy from my own personal point of view, because when it gets studied by lots of people it becomes too complicated to find out what they're doing. I'd have to learn and understand their notation, which would probably be different from mine—and that's hard work. But if I know that nobody's working on it, then that's fine—I'm not rushed, and I don't have to think I've got to get in there before someone else does!

> "Tiling problems have always been a doodling side interest of mine

That's right. But what recent development in science at large has made the greatest impact on you?

RP: Gosh, I'd say cosmology is one of the areas. Gravitational lensing—I worked on it for a little bit at one point; it's an amazing thing. I'd say that astrophysics and cosmology are exciting areas.

What aspects do you find most interesting?

RP: What I was talking about, gravitational lensing. I find it interesting because it's an effect of Einstein's general relativity, which I think people thought was very hard to measure. It was the first thing that convincingly suggested that Einstein's theory might be right. The Eddington experiment showed the deflection of light of the stars by the sun, but to see this effect on a cosmological scale, to see a galaxy focused by another galaxy, would have seemed absolutely ridiculous. Nowadays it's an observational tool, people use it all the time—it's a way by which you can tell the mass of an object just by saying how much focusing it exerts on the image behind it, and it's wonderful. It's not just that it's a powerful tool in cosmology, but also perhaps because it uses something which is close to my heart I suppose; that's why I like that one so much.

But that's just one area; there are lots of other things. I suppose the experiments on quantum entanglements (non-locality): you can get two ends of the room, 12 metres apart—well, nowadays it's longer than that*—by quantum entanglement effects, the two are now connected through quantum mechanics, and it's amazing. One knew it had to be there in quantum theory, but it's very impressive to see that it's real.

Quasicrystals are remarkable. High temperature superconductors are amazing, and so are developments in molecular biology. Some of the things that people are now learning about cells and about cytoskeletons and microtubules—I find them fantastic. Partly this is because I didn't know what was going on in biology, and having got slightly involved in this subject, I see some remarkable things.

I came across Erwin Schrödinger's book *What is life*, for which you wrote a preface.

RP: That's right, and it mentions ideas related to aperiodic crystals that he believed were at the heart of life.

Can you tell us something about this?

RP: My father was very taken with the idea, I recall, and we had these mechanical devices I mentioned earlier which reproduce themselves, and he then developed much more elaborate devices which he made of wood, little things with levers which copied themselves. He sometimes referred

* It is 1,200 kilometres, as of 2017

to these things in Schrödinger's terminology as 'aperiodic crystals'—a crystal that went on for a while and then stopped growing, because that was how it came to an end. Life was to be thought of in that way. So, it had some influence on him, and indirectly on me. But I don't know about the quasi-periodic tilings, or whether it has any such connection, historically.

> " I eventually realised that Bach is really my favourite by quite a long way

What books are you reading at the moment?

RP: Gosh, I never get a chance, though sometimes I have to review a book. I'm not reading one right now. It's one of my regrets that I find myself so busy that I have hardly any time to read anything for fun, which I like to do.

If the day had 36 hours, what would you like to do?

RP: If it had 36 hours, there should be a rule that the extra twelve were only allowed to be used for things that were not directly to do with one's work. I enjoy reading when I get the chance, and also going to the theatre, to films, and listening to concerts—all the things I'd love to do more of. But there are films that I never get the chance to see, including some wonderful ones that are going around now. I used to read quite a bit of science fiction, but I hardly have a chance to now. I sometimes read Michael Frayn; I find him tremendously funny.

What about music? Do you have a favourite composer?

RP: On the whole I prefer classical music, but I enjoy jazz too—my wife's influence! My parents each had a favourite composer: for my father it was Wolfgang Amadeus Mozart, who was his God, and for my mother it was Johann Sebastian Bach. I eventually realised that Bach is really my favourite by quite a long way; I've always regarded those two as a cut above the other composers. Bach—I think I see much more in his work, there's something you can always go back to, and there's yet more and more of it. But I think it's the perfection in Mozart that I find somewhat magical. I've always rated him above Ludwig van Beethoven, who never had quite the same magic for me. I can see he had the power and originality, but somehow there's not quite the magic there. Maybe even with Franz Schubert there's a bit of magic that I don't quite see in Beethoven. To go back further, I like Antonio Vivaldi, Henry Purcell and others. But I also quite like some modern composers, such as Igor Stravinsky, Sergei Prokofiev and Dmitri Shostakovitch.

What do you consider the most profound scientific development of the last century?

RP: Einstein's general theory of relativity. I might have also said quantum mechanics, but I think that that theory isn't finished yet, because of the measurement paradox. I discuss these things in my new book, *The Road to Reality*, which should be published soon.* It's almost completed, but still there's work to be done on it. It's about mathematics and physics and the profound relation between the two. In it I try to give my views of some of the popular developments in modern theoretical physics—I may get myself into more trouble!

> " It's the perfection in Mozart that I find somewhat magical

Finally, what will keep you busy in the future?

RP: I have many more books in mind, so I'll keep writing. Also, I'll keep working on twistor theory, as there's a great deal to do in that subject. I have new ideas to do with quantum state-reduction—even an experiment (FELIX) which I hope will be performed in space while I'm still alive! In addition to all this, I recently had a baby boy (Maxwell Sebastian—Max, for short) who was born on 26 May 2000, and which will keep me even busier! ■

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* Roger Penrose. *The road to reality: A complete guide to the laws of the universe.* Alfred A. Knopf, Inc., New York, 2005. xxviii+1099 pp. ISBN: 0-679-45443-8