The Book Show

on ABC Radio National

Mathematician Roger Penrose and The Road To Reality (transcript available)

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In the second of our major feature interviews from the Edinburgh International Book Festival, we bring you one of the major thinkers in the world of science, Sir Roger Penrose.

Hide Transcript

Transcript

Ramona Koval: Welcome to this Edinburgh International Book Festival session. I'm Ramona Koval from the Australian Broadcasting Corporation. And today, a complete guide to the laws of the universe in 55 minutes, and then you'll be able to go forth and be time lords, I think. I'm pretty sure of that. Roger Penrose is an emeritus professor of mathematics at Oxford. He's much praised and awarded, including the 1988 Wolf Prize for Physics, shared with Stephen Hawking, for their joint contribution to our understanding of the universe. He's the author of many books and papers, including *Shadows of the Mind: A Search for the Missing Science of Consciousness*, and he has collaborated on a science fiction novel with Brian W Aldiss, called *White Mars, Or The Mind Set Free: a 21st-century Utopia* So you can see that his interests range far and wide.

The Road to Reality is the title of a very big book, this one here. A very big book with many mathematical formulae and illustrations penned by the author. They're illustrations about, for example, different possible universe evolutions, or 'quantum superpositions of alternative classical paths in configuration space'. Or the Newton/Carson spacetime. And that's only the easy ones. But for the interested general reader there are chapters and paragraphs of descriptive prose that give a sense of what the shining mathematical formulae point to. And since the book is one of the *Sunday Times* top ten bestsellers, there are many general readers who are prepared to step up to the plate and open a book which was described by *Wired* magazine as 'a mathematical Finnegan's Wake.' Did you not know that?

Roger Penrose: Didn't know that one, No.

Ramona Koval: Well today Roger Penrose is going to give us an introduction to the laws of the universe, then I'll talk to him, and then invite you to join us as well. And we're going to leave as time lords at the end of the session. So please welcome Roger Penrose. [Applause]

Roger Penrose: I think I'm not going to give an introduction to the laws of the universe, but maybe to the book. In fact I should explain, to those of you who can't see, it's about that fat—it's about 1,100 pages—and it does have quite a few equations in it.. In fact people ask me—including my wife, I have to say—why on earth I wrote that book. The reason I wrote it, I have to say, is that I had written another book called *The Emperor's New Mind*, which was presenting a point of view with regard to the mind; well, that's in the title...and why modern physics isn't efficient in certain important respects to accommodate the mind, and things like this. But there were a lot of things in that book that were contentious, and it did, however, have a lot of explanations of aspects of physics. And several people had come to me and said, well, you know, I could have used your book in my course 'Physics for Poets', or whatever it was, if it weren't for all that contentious stuff on the mind. So I thought, hmm, that doesn't sound too bad. They said well why don't you write another book without all that contentious stuff on the mind. So I said well that sounds quite easy. All I do is I take the book and cut out all the bits that have anything to do with the mind...mentally; I didn't actually do it with a pair of scissors...but I cut out those bits and that should be quite easy, and there's your book. So I thought, okay, and I got a contract and tried to do this.

But then of course when I started cutting out these chapters mentally, the whole book just fell to pieces. Mainly because there was no theme left. And so I realised that this was the guiding theme which more or less held the book together. So it needed another theme, and the other theme is the search for the laws which govern the world. So that seemed to me a reasonable theme. The only trouble is, that I then had to incorporate all sorts of things that weren't in my first book, so rather than getting much shorter—the original one was about 500 pages—so rather than only being 250 pages, I realised there were all sorts of other things that needed to be in the book, and so it grew to that size. I'm very bad at estimating how long things are going to be, and this is an example of how bad I am at that.

But the other thing was, of course, all the equations. Well it's the common story that each equation in a book like this reduces the readership by a factor of two. So I thought, let me see, if I hadn't had any equations in this book, how many readers would there have been. Well I didn't know, first of all, how many equations there were, so I started trying to count them, and I didn't quite know how you count equations. What is an equation—if there's just something with a lot of symbols in the middle of the text, is that an equation? So I made the rule that you count equals signs. So I tried to count the equals signs in the book. Well again, that was too much of a job, so I just took a sample of a certain number of pages, counted the equals signs and then multiplied it up to the scale of the book, to get an estimate. Well I won't tell you how many that was; I've forgotten now. But I will tell you what the readership would have been, had I not had any equations. This is assuming there was one person bought my book. Well the readership would have been—just think of the observable universe, and you think of how many protons there are in the universe, and suppose that each proton has bought—well how many copies of my book would it have to have bought. Well, you know what a googol is, that's one followed by 100 zeros—well it would have been a googol times a googol. So that'll give you some feeling for what the readership would have been.

Ramona Koval: I'm not sure a proton could carry it.

Roger Penrose: It couldn't. It's too fat. Yes. Anyway, this is really not telling you how many people would have read the book, but it's telling you not to believe that aphorism about how many people would have bought copies.

Well, one of the things that I'd always hoped, was that some young person would read my earlier book, you see, and be inspired by this. This is one of the reasons I wrote it, that maybe it would inspire young people to take up science. And I got lots of letters after the *New Mind* was published, but almost all of them were from retired people. And I thought this actually makes sense, because these are the people who maybe used to be scientists or something and they're the only people who have enough money to read my book. The young people evidently didn't. Well, there's one I won't mention who made a great success of his life as a singer. That was one. But there was another one.

I'll tell you, I was visiting a very good experimental scientist, Anton Zeilinger in Vienna, and he was showing me some wonderful things he'd just done. He'd done this superposition of mucky balls—I won't explain what that's all about, but it's a beautiful quantum mechanical experiment, very impressive. And so I visited his lab and his assistant came up to me. And he said, 'I'm so glad to meet you. It was your book that inspired me to do physics.' I thought, 'Ah! At last! It's happened.' And then two years later, I got an email from this chap saying, 'I've given up physics now.' And he showed me the pictures of these wonderful statues that he—he's a sculptor. And he's used physics to inspire his sculpture. Yes, nice things he's done, but somehow it wasn't quite what I had in mind.

There were a few other points I wanted to make. Let me remind myself what they were. Well, there's another reason for writing the book rather than simply explaining things. There are certain areas of physics which I feel somehow are given a bit too much...you know, you get some subjects you read about in the newspapers and so on, and you don't perhaps know what's going on, but they're big topics. And I didn't feel necessarily that these big topics were—they shouldn't really be as big as they seemed to be. And so I took the opportunity to attack a few things I didn't really believe—the big fish, I'd say. And one of these I should say is string theory, I'll confess to you. There's a lot in it. It's certainly an important subject, but I thought it was somehow given too much attention. Particularly I didn't believe all these extra dimensions you're supposed to believe in, you see. Inflationary cosmology is a slightly different story, but I have troubles with that too. And even the foundations of quantum mechanics. But I think there's a point there which has to do with the sociology of physics. Topics become important because—well, I don't know what it is; more people do it and then other more people do it and so on. Not for necessarily scientific reasons. And so the sociology of science is an element which I've tried to address in the book.

A final point I'd like to make is...well sometimes people would come to me and—and I saw in a review, I think, somebody said, well so-and-so (I won't mention who) manages to explain all these things without all these equations. Why does Penrose need to put all this mathematics in it? Well, I never think of the right response at the right time with these things, but it occurred to me later what the correct response to this comment would have been. And that is, well, you can appreciate the opera *The Magic Flute* by reading the plot and all that sort of thing. But unless you listen to the music, you essentially miss something. And you really have to appreciate something of mathematics itself if you really want to get something crucial which the book is trying to convey. And this is really one of the reasons I did it.

Ramona Koval: Well, when I read this book—and I didn't do all the maths...

Roger Penrose: You don't have to.

Ramona Koval: Yes, I know, you'd given me permission at the beginning, that's true. I found there were terms like—well 'music' is one you've just used—but 'beauty', 'intuition', 'miracle', 'simplicity'—these are the sorts of terms that you find in this book that you don't necessarily associate with complex mathematics and physics. And it is a fundamental yearning, isn't it, in mathematics, for the beauty of simplicity, the aesthetic attraction of mathematical coherence. And I guess this is what drives people towards the idea that they want to find a unified theory of everything, because it feels right, doesn't it?

Roger Penrose: Certainly aesthetic criteria play a fundamental role in doing mathematics, and I think people don't necessarily appreciate this, certainly if they don't do it themselves. And there are actually two aspects to this. One is it's the reason that people who do mathematics do it. It's because they enjoy it and they get excitement out of it and they appreciate the beauty of the subject; and this is why they do it. But there's another reason which is much more subtle, and difficult to understand really, which is that there's a very close relationship between the beauty and the truth. So you can appreciate—there might be two possible things which are true; this or that. And one of them would be extremely attractive if it were true, and the other one, well, okay, it'd be a bit of a mess, you see. So the likelihood is, and it's not an absolute rule, but the likelihood is that it's the one which is more beautiful which is more likely to be true also. So it's a guide for truth. And it seems to be independent—you do it because it's beautiful, but the beauty itself is a guide to what's true. And that's a much more subtle and difficult to comprehend concept.

Ramona Koval: But why should it be like that?

Roger Penrose: Well I don't know. It's probably one of these deep mysteries. I'm only intending to deal with some of them in that book. But I do try to address that question, yes.

Ramona Koval: There's a sense that as human beings there are certain things that we find beautiful. Symmetry, for example, where faces that we find beautiful apparently are symmetrical faces. And as scientists we tend to also look for balance, I suppose. Can you talk a little bit about that—why should it be so?

Roger Penrose: Well I once gave a lecture which was on that topic, about—I can't remember the title of it—but it was to do with this interrelation between beauty and truth, I suppose. I'm just trying to think...there was a point here. Symmetry, yes. Sometimes it's symmetry and sometimes slight departures from symmetry and the reasons why there should be these slight departures—which can be a very subtle and compelling reason. I was about to say something else which has unfortunately got knocked out of my head.

Ramona Koval: I'll ask you something else and it'll come back to you.

Roger Penrose: I hope it will, yes.

Ramona Koval: But you don't believe that we're going to find a unified theory of everything.

Roger Penrose: Well, I certainly don't think that it's anything very close to what we know at the moment. Whether anybody will ever...I mean to find everything, I think that's saying a bit much. But you might, it's conceivable that the general underlying principles will at some stage be understood. I don't think we're at all close to that at the moment. Certainly a theory of everything is a big goal that people refer to. But I don't see any of the schemes that have been put forward so far as having anything like the kind of ring of truth that one would expect any such scheme would have to have.

Ramona Koval: but you say also that mathematics has the 'habit of finding its applications in very disparate fields.'

Roger Penrose: This is certainly true. And they're often unexpected. Some of these things go back to many centuries ago. A good example is—people have presumably heard of Fermat's Last Theorem and Andrew Wiles' great proof of this a few years ago. But perhaps not so familiar to people is the thing called Fermat's little theorem. It was proved by Fermat and it's well known, and the truth of it wasn't in dispute. But it's another thing he did, 400 years ago, which only relatively recently found applications in coding theory and so on. But there's things which sometimes the applications are immediately recognised; sometimes they can wait many centuries before the use of these things appears. And most of mathematics still doesn't have any application. You find in a book or journal on mathematics, you find 99% of what's in there is done for its own sake and does not have any direct relevance as far as we know to the physical world or things going on in the physical world. Nevertheless, it has its own value and... Mathematics has sort of suffered, in some way, for being useful. There are things in the arts that you could say, well the value is in the subject itself. And therefore you have to appreciate that. Whereas in mathematics, because it is useful, sometimes people think, oh well, that's why you do mathematics, and forget the reason, which is quite independent of its application, which is the same as the value in doing the arts, or maybe ancient history—all sorts of things which might be internal to the subject and not often appreciated by the world outside.

Ramona Koval: But there is a tendency for mathematicians to want to find important applications of their work to the physical world. And I wonder whether that was because we do have a need for the concrete. Or maybe we have a need to prove usefulness to the research finding bodies.

Roger Penrose: That's true. That is true. But I have sort of ambivalent feelings towards this because I certainly appreciate that, and I think that is a lot of the value of doing mathematics, and certainly the world would be much poorer and technology would suffer tremendously is the mathematical content was removed from it. Most things just wouldn't be there at all. How you worked out aeroplanes and how they're supposed to fly and so on. There's a lot of mathematical work goes into that. And sometimes quite sophisticated. But on the other hand, to think that's why you do it is detracting from the important values which are independent of that. So I'm never quite sure whether one should stress the applications or not. there is a danger in stressing them, because you find then that within mathematics there is a bias towards certain areas which are manifestly useful. And often you find, okay, they are useful, but then that means that the whole much bigger area of mathematics, which is not directly useful, is then maybe...some of the emphasis is taken away. And funding, as you say, is an important issue.

Ramona Koval: You're not convinced by string theory, as you said. Do we dare discuss string theory? But I think we might just say that—perhaps you could tell us what problem it's supposed to address and talk a little bit about...because I think you mentioned that idea of the insertion of different dimensions, or extra numbers of dimensions in order to keep your theory close to your heart. When things are going wrong, you're just sort of like, well let's have another fourteen dimensions and that will fix it. And then you say, well that's good. But you're saying, well what are these dimensions?

Roger Penrose: Again, it's a sort of ambivalence that I have here, as well. Because when I first heard about the ideas of string theory, which was—oh, I forget when it was, in the 80s, probably—I was very excited by them. I thought there's a new idea here and it's really something I'd like to think about and connect it with ideas that I'd been playing with and so on. Then it went off in these directions that were requiring—in those days it was 26 dimensions instead of four, so you needed an extra 21 dimensions which we weren't aware of. I had all sorts of reasons for thinking that was a bit fishy, you see. But okay, you can still do it. I'm not saying you shouldn't do the subject because you've got the wrong number of dimensions. The question is whether you believe it's really the physics of the world. Now it might be that there are these extra dimensions curled up into little loops, which is what they think in this theory. Some people like that because this theory of extra dimensions gives it a sort of aura and so on. The idea is that these things are, as I say, curled up into little tiny loops which you can't see, and so it still might be true.

Now I have two reasons for being very suspicious of this. One is a personal reason which has to do with my own scheme, which is the twisted theory described in the book which is based on the fact that there are three space and one time dimension, which is crucial to the whole theory, you see. But the other is just that these extra dimensions—it's very, very difficult to hide them in the way that the theorists promote. And they don't face up to this. Basically all the extra degrees of freedom which are hiding in these extra dimensions are completely uncontrollable. These things would be totally unstable. And I think there are difficulties which—perhaps I should tell you a story... I gave a talk in Cambridge in honour of Stephen Hawking's 60th birthday, and I attended the conference and my talk was the last one at the meeting. And I started by saying I might not get out of the room without being tarred and feathered, because I was criticising the extra dimensions and making the claim that they were unstable and so on. Well, they didn't actually tar me and feather me then, but the next morning I was attacked by various people. At one point two very important string theorists, one of them started arguing with me and said, well no, it's like this, and so on. And then another one came at me, and then the first one said to the second one, no, no, no, it's like this. And then they started arguing with each other and so I just went off. And then at lunchtime, somewhat later, another person who was one of the great originators of the subject came and sat down opposite me at lunch, and he said—now you have to figure out what this means—he said to me, 'You're completely right, of course, but totally misguided.' And I've been trying to think what he meant ever since. But I think there's maybe a feeling of—it's partly that they know these difficulties in the subject that you're not supposed to mention.

Ramona Koval: Well big careers are tied up with this, aren't they? Huge amounts of money and huge research organisations. Not to mention the cost of these experimental systems and...

Roger Penrose: There's not much in the way of experimental tests. And that's one of the troubles with the subject, it doesn't have any. Okay, there are a few experiments which you can do which test sort of off-beat variants of string theory. And so if they don't work, they don't mind, you see. It's not really testing the big subject. But there's nothing really which *really* tests string theory. And this is one of those complaints, that...

Ramona Koval: But is it science at all?

Roger Penrose: Well... My book is sort of mildly critical of string theory, and giving credit where it's due, I think. I hope. But there are two books published now; one by Peter Woit and one by Lee Smolin, which are absolutely attacking string theory. Peter Woit's is titled something like 'Not Even Wrong'. That's a quote from the great physicist Wolfgang Pauli, who was talking about some theory and he completely dismissed it—it's not good enough to be wrong. There's no test of it. And so this was applying this now to string theory, this was Peter Woit's book. But Lee Smolin's book is even more aggressive. He used to work partly in the subject in different areas. And he was commenting that the last 25 years in basic theoretical physics has been very remarkable in that absolutely no progress whatsoever has been made in that area. And he puts the blame completely on string theory. He says that all the best theoretical physicists are all working in string theory. And this is why no progress has been made.

Ramona Koval: You're very fair in your book, because you do...every time you want to criticise string theory or promote twistor theory you say, look, I just have to tell you, I've got this other theory and I rather like it and this bit that's coming up next is going to be because I really like it and you have to make up your own mind.

Roger Penrose: Yes, I try and warn people when I'm taking a view which isn't quite the—well, you're supposed to be able to read the book without having to accept my views.

Ramona Koval: You also say that there's an enormous quantity of observational data now that still needs to be made sense of, that hidden clues might be in there. But you say that all this data may be of interest to people other than the ones that have been generating it. Like, I guess, Einstein and the pre-Galileo observation. But you know, the thing is how to manage it all any more.

Roger Penrose: Oh, I think there are real problems. It's just due to the huge amounts of data. For example, if you take the satellites which are looking out at the sky and they're mapping the microwave background radiation, this is the radiation which is often described as being the flash or the big bang, cooled off by the extraction of the air—it's not quite a fair description because it happened I forget how many millions of years after the big bang—but in a certain sense it is the flash of the big bang. And there's a lot of information in this very, very detailed, slight variations in temperature over the sky. And people analyse this data in one particular way only. And they come up with these graphs and so on. And okay, huge amounts of data in these observations. And the analysis just picks out a few numbers from that. Now I have a colleague—I can't say he's a colleague, he's an Armenian—who's a very clever fellow, who's analysed this data in a completely different way, mainly by looking at the shapes of the little patches in the sky of any given temperature, seeing how they're distorted by the spatial geometry of the universe. And one of the stories that one often hears about the spatial nature of the universe is that it's pretty well flat. People say that Euclid was perfectly right that the geometry is flat. But according to this chap, it's not. It's slightly what's called 'negatively curved', like a saddle surface. Not much. It really is almost flat, it's true, but there are slight deviations which you pick up using his method and which the normal analysis doesn't really pick up.

That's just an example. But this is also true in the accelerator experiments with huge amounts. And it's natural enough, because people are looking for specific things. So they filter out exactly what they're looking for. Okay, it's fine, there's nothing wrong with doing that. It just does mean that there is also sitting there, all this data—if you thought of some other ways to analyse it you might see there's a great source of information out there which requires—well, it requires the right theory first of all to know what to look for.

Ramona Koval: But also you're saying that you don't think that the large groups of scientists working in a particular area in a particular way are necessarily going to be the ones that make the next great leap. And you're saying that the single intelligent mind working...coming at it in a sideways way is probably going to make that leap. Rather than all these other people. Why?

Roger Penrose: Again, there are two sort of approaches. One is the great groups of people, great packs who use computer analysis and all sorts of things and great programs with huge numbers of people. And that's the way most of science is done nowadays. And okay, you get a lot that way. But maybe the big breakthroughs which are going to make a real change in our picture of the world, I think they'll have to come. There are certain areas where, mainly in quantum mechanics I think, a big change must take place at some stage. That probably will be much more—some individual has a different slant, a different way of looking at things. It's in a way harder for people like that as time goes on. Particularly if they want a job, because you know you get a job because other people are working in the area that excites them, and those are part of the popular areas. So the popular areas propagate and the unpopular ones won't. But then there's another side to this, you see, because there are all sorts of crazy theories which people have which just simply don't make any sense at all. And there's an awful lot of that going on too. So it's very hard to make overall statements, to say things should move in a certain direction.

Ramona Koval: You said—your doubts about the inflationary theory, but you just said that after the big bangs happened...aren't you saying you can't believe in it necessarily, can you, in the big bang, if you don't believe that it's going somewhere, it's expanding out?

Roger Penrose: You have to be careful about the words here. There's a thing called inflation, which is different from the normal expansion of the universe. What's accepted now is not so clear, but there is what used to be called the Standard Model of cosmology which was an expanding universe from the big bang, yes. But then in addition to that, people have found that they want to introduce this inflationary phase, which is a much bigger expansion, This took place much earlier than almost all the observations that one had. And I initially was very sceptical of this, because it involved logical reasons for believing it, which didn't work. Now that's still my believe. But what I didn't anticipate is that there is a lot of—from this data, microwave background—there's a lot of evidence for something other than the old standard model, which people argue in favour of inflation. So inflation has a much better case than string theory, because there is observation. You're almost touching on something I shouldn't mention here, which is the topic of my next book. I won't talk about it here, but...you write a thing like this and you think you've finished, and then along comes an idea which changes something in a very, very fundamental way.

Ramona Koval: Oh, boy! We're really interested in that, though.

Roger Penrose: No, I won't talk about that here, because that's...

Ramona Koval: So you're on the way to this...

Roger Penrose: It's another scheme which would give an alternative. It doesn't involve inflation, but it's a different scheme which has some connections with it in some ways.

Ramona Koval: What about the idea of spacetime maybe having to be abandoned? We like spacetime and we're a bit sad about that.

Roger Penrose: Again, you see, always I'm a bit weaselly in my answers to your questions, because there are lots of different things one can say. I like spacetime too, and I've spent a lot of my life working on it. Twistor theory, you see, is a different angle on spacetime. It says that spacetime is not primary. It's a sort of secondary notion which comes out of something else. It is not saying you abandon spacetime. Spacetime is still there, but it's something which is not the basic theory; it comes out of something else. String theorists don't tend to do that. They say the spacetime is there, and they've got these little strings running around in it. but the spacetime is pretty well there to start with. The only thing is it's got the wrong number of dimensions. It's got far too many space dimensions.

Having said that, I have to qualify that also, you see, because they also say in some sense spacetime isn't very important because it might have four dimensions or it might have nine or ten or e11 or 26 or something. And it's not very important to them how many, and that always seems wrong to me.

Ramona Koval: Yes. It seems wrong to me, too.

Roger Penrose: It should have a very clear basis that...can't monkey around with it in that way. but they have their reasons.

Ramona Koval: So it feels like a theory in any area, really, that's gotten too big for itself, and that people want to hang on to because they just like it, and they're used to it.

Roger Penrose: Well let me say something positive about string theory. I should do that, you see, because...

Ramona Koval: To be fair, yes.

Roger Penrose: Something else which came up just as I was almost finishing this book. Okay, I don't like the higher dimensions and that's not changed by what I'm going to say. But there is a way in which string theory ideas—again this is usually this chap Edward Witten who's producing all sorts of ideas, and these ideas have had a great impact on mathematics, so if you want to find the people who are most favourable towards string theory, often they are mathematicians, in certain areas of mathematics. Because string theory, and often it's Witten himself, has produced ideas which have had an enormous impact on pure mathematics. It doesn't tell you that it's true of physics. It's just had a big impact on mathematics.

But one thing that happened quite recently—in fact I went to visit Witten in Princeton just before this book was finished, almost, and he told me something which...he was just describing this to me and I looked at this and thought, is this four dimensions? And he said yes. It was a four-dimensional type of string which was related to twistor theory, a new angle on things which I'd been playing around with 40 years ago or something. And so, okay, there are ideas there which could easily have an importance to physics in some way. But not in quite the way they say. So it's worth doing. And I'm glad there are people doing string theory. What I'm not glad is how almost the entire community of people who are doing basic fundamental physics are in string theory. Now that's wrong. But that some people are doing it, that's fine. I've nothing against that.

Ramona Koval: I said before that some of the words that I found in this book that I was surprised about, and one of the most surprising words was the word 'miracle'. And you say, 'these are more powerful than mere elegance in mathematical theories of the physical world.' Now I want to know—I want you to tell me about a miracle and tell me how you know you've got one, how it feels.

Roger Penrose: It was written...I did talk about, there's a chapter with miracles in the title, there, yes. But I'm not quite sure that I would...I was slightly...I don't know how to say this exactly—I was slightly saying not quite to believe that they're miracles. Sometimes they can be debunked if you look for the reason. It looks like a miracle and then behind that is some reason, which when you've appreciated that reason, it's not quite such a miracle as you had thought. And one of the examples I gave was something in what's called super gravity theory, which is a sort of precursor of some of the ideas of string theory, where you seem to require extra dimensions and it's one of the problems in these basic theories is that you run into infinity. You find your answers produce infinity and it can't be right if it gives infinity as the answer. So they found in what they call super gravity theory that if you had 11 dimensions then some of these infinities would absolutely miraculously disappear. These things just cancel. You do a very long calculation and you see these things just disappear, they've wiped each other out, these infinities, you see, and it looks amazing. And this spurred them to think, oh, this is the new theory of everything, you see. Later work showed that, okay, this doesn't happen at the next stage. but it was a miracle that spurred this theory on, you see.

So I was making the point in this book that you should be careful about these things. And I had experience myself in twistor theory; things which looked like miracles, you see, in a similar way. And are these things that you can trust? Does it mean you're really on the right track because these miracles are seen to come out. Something which you have no right to believe, and suddenly it works much better than you thought, you see. Can you trust that? So I try to say well, you can't really trust it because here are two subjects, both of which are supported by miracles and they're mutually inconsistent, you see. And one was twistor theory and the other was string theory. And there's this sort of irony which comes along later, which was this thing that Witten discovered, how to combine these two completely inconsistent theories into a way which seemed to make some sense. I don't really know what to make of all that, but I thought these were points worth making. Certainly some people say this idea must be right because what other reason could there be for this miracle—but I'm slightly debunking it. You say okay, that could be impressive but when you look deep underneath you see there may be some reason that you haven't yet appreciated for why it really is true. It's not such a miracle—or maybe it's a deeper miracle which really is telling you something about truth. So one shouldn't dismiss these things, but there's no clear answer.

Ramona Koval: Well, I'm going to put the lights up now, and you can ask your own questions and I will endeavour you...but I want to ask my very last question, which is, I desperately want there to be time travel, and I desperately want there to be teleportation. Am I going to be happy before I die?

Roger Penrose: I'm afraid that's most unlikely. I'm sorry to say this. Time travel is, I'm afraid you have to face up to certain things which...theories could always be wrong, and maybe there's something that Star Trek—hyperdrive or whatever they used to call it. No I don't think there's much chance of that. What was the other one?

Ramona Koval: Teleportation. Because I have to go back to Australia on Saturday.

Roger Penrose: So you want to do that by beaming yourself down to the other side...

Ramona Koval: Yes. I'd prefer that to going to Heathrow...

Roger Penrose: Better than Heathrow, yes. Well I don't know if it's a thing of the future. I've just been through Heathrow twice in the last few days and it's not too bad at the moment.

Ramona Koval: All right then, I won't wait for teleportation.

Audience question 1: I have a two-part question and your answer may be 'no' and 'no. If it is, I'll be satisfied. First part is, is it conceivable that somebody with no knowledge of mathematics could contribute anything of use to your search for understanding. And the second part is, is it possible that the theory for everything or part of it could be fundamentally non-mathematical?

Roger Penrose: I'll try not to give you as short an answer as you suggested. Certainly there are ideas which could easily have value and which needn't be mathematical. At least, not whose details are mathematical and which could have an importance. But it's pretty unlikely now. I think that as physics has progressed—and Einstein suffered from this because his general theory of relativity—before that he rather took the view, okay mathematics he liked, he had didn't have the same kinds of instinct with regard to mathematics as he did with regard to physics, and so he didn't regard mathematics as quite as important as we do perhaps now. But with general relativity he rather changed his view. And some people say that the reason that Einstein never made any absolutely great contributions—he made important contributions but not of the sort of calibre that he had before general relativity—and one of the reasons was that he was driven into an area where he didn't have the same instincts. And I think that may well be true, that he didn't have the instinct of relevance to generalising general relativity that would be needed for that. So I think perhaps he suffered from that.

The second question was something to do with the theory of everything...I can't quite remember what the question was, sorry...Oh, fundamentally non-mathematical. Well I just think that's unlikely, just from the way things are going. They seem to get more and more mathematical. But that doesn't necessarily mean harder to understand. Often the things which are hard to understand at first are the conceptual changes. And that applies to Einstein's general theory of relativity. It's not that it's mathematically complicated—which it is, it is mathematically complicated, actually to use it and to work out the consequences of the theory. But to appreciate the theory is a thing you can do by drawing pictures and so on and it doesn't really require profound mathematics to get the general idea of it. So in a certain sense it might be the case, you know, that the deeper we probe it'll be the conceptual things which we need to, in order to improve our understanding, rather than the mathematics in the sense of absolutely detailed calculations. And I think that's quite possible.

Audience question 2: There's been various stories in the media recently about reduced number of school people taking A-level physics, for example, and Britain needing more scientists in the next ten years or whatever and so forth. What do you think can, and/or should be done to make science kind of more accessible to people in general, and to make it a more attractive choice for people deciding on their career.

Roger Penrose: I think that's a very good question, and I don't think I know the answers, but let me just make some points—which may or may not be relevant. I keep wondering whether there is this tendency where people try to make science—let me say mathematics, perhaps what I'm trying to say is more relevant to that—trying to make mathematics more acceptable to more people by in a certain sense removing the mathematics. Because often one stresses, you say, well look, these ideas are useful in this area and somehow it's taking the mathematics out. Whereas what stimulates people to do mathematics is to appreciate the beauty in the subject itself. And that's often removed in this sort of dumbing down that takes place to make it more accessible to people. You actually take the mathematics out. And having done that, you remove this thing which attracts people. And there are more people, I feel...I thing the fact that a lot of people have bought this book, to some degree is evidence for that. That there are people who really want to know and are intrigued by mathematics. Sometimes it's not even the people who are best at it. My wife's a school teacher and she certainly experiences the fact that there are certain people who get very excited by the mathematics and not necessarily the people who do best in the exams. The ones who do well in the exams are the ones who can apply the rules quickly and it's no trouble to them to do that. But they don't get any thrill out of the subject. The ones who do get the thrill out of the subject are the ones who are more likely, ultimately, to go on with the subject and be successful with it. So I think you somehow have got to stimulate that aspect of it, which means the mathematics itself, rather than trying to avoid it by making the explanations somehow easier to understand by taking the mathematics out of it. So perhaps that's the best comment I can make at the moment, but it's a very good question and it needs further study, I think, to see what the reasons for this are.

Audience question 3:You said that you don't think at the moment we're near to a complete theory of everything. Do you think as humans we're capable, and is there like an unobjective kind of holistic theory that we can know and is true.

Roger Penrose: I think I tend to be an optimist here. I don't know, ultimately. Who knows, I can't make a comment on that. But I tend to feel that it is possible to understand things which at first may seem beyond comprehension. A good example, I think, of this, is quantum mechanics. Because quantum mechanics is often described as something...you just can't understand it. Okay, you can apply the rules, you look at the textbook and you know what you do in any problem like this. But do you understand what's really going on? No. And there's a famous comment from the great physicist Richard Heinemann about this, 'Does anybody really understand quantum mechanics?' Personally I think there's a good reason why people don't understand quantum mechanics, and that is that it's not finished yet, that the subject isn't quite right. And there are certain paradoxes in quantum mechanics which...I try to bring them out in the book here...which are a reflection of the fact that the subject is incomplete. As Einstein would have said. And that one needs to—there has to be a major revolution which will change quantum mechanics from its present state to something which is more logically consistent. So one of the reasons, I'm trying to say, that we don't understand quantum mechanics, is because it's not really logically consistent. And that's a problem. Whereas if you have a subject which is internally consistent, makes a whole which can be comprehended, that's something which could come through an advance in the subject. So I think that advancing a topic may make it more easily comprehended. At least in the area of quantum mechanics I think that's the case. And so I'm somewhat optimistic that maybe if you really get deeper, closer to the truth, the understanding will not necessarily be more difficult to come by.

Audience question 4: In your efforts to research greater understanding of the universe, do they tend to promote or refute the concept of a creator god?

Roger Penrose: Ah! I try to steer clear of that one. I think it's quite legitimate to keep these things separate. I certainly have my views on this subject, which I don't put forward...

Ramona Koval: You do have a picture of someone who looks a bit like God in the book that you've drawn.

Roger Penrose: I'll tell you something about this. I'll tell you something about the one in my previous book, *The Emperor's New Mind*, where there's this character who puts a pin in the...

Ramona Koval: It's the same...

Roger Penrose: It's the same thing here. It's a different picture, but I have to refer to the other picture for what I'm going to say. I'd given a lecture in which I put a transparency up where I had used this picture to illustrate the unlikeliness of the original state of the universe according to the big bang, which was the thing I was illustrating with this picture. At the end of the talk somebody, I think it was a man, stood up and said, 'In your picture of God creating the universe, why did you depict her with a beard?' So I'd actually thought about this before, and I picked up my transparency—I had to rummage around a bit to find it—I picked it up and said, 'Well look, you can interpret this picture in more than one way. that could be a beard, or it could be the hair coming round and twirling around like this. It was deliberate, the way I've drawn it.'

So, that's not really relevant to your question, I'm afraid. But perhaps it does indicate I had a certain playfulness about this which maybe I wouldn't have had if I'd taken a different view about how to... Let me say it like this, I think that the more we understand about the universe—you see there are some people who might take a very strongly materialistic point of view, and I know in particular one scientist who does do this, and which somehow in the way of reducing what some people would call a reductionist approach to science, you know, reducing everything to little bits and pieces, and if you know the laws of the little bits and pieces then you know everything. And this kind of view somehow removes the point of the whole subject. And I certainly was on the side of those people who don't like that, you see. It does seem to remove the point of the whole subject. And I don't hold to that view myself. I think that somehow the deeper you get to understand what's going on in a certain sense the point would ultimately be revealed. I don't know quite what that means, you see, but there is a point to it. There is a point to the universe and that this point will be revealed by deeper appreciation of the physical universe, which means science. So in that respect you can interpret that comment as you like. You can say does this mean that it reveals something about a god or something, or the god is there or the god is not there. I don't mind which way you interpret it. Perhaps the way I've said it is more expressing what I think rather than a straight yes-no answer to the question.

Audience question 5: I'm always impressed when I read popular science books on theoretical physics about the power of the human imagination. But do any of these competing theories make a unique set of predictions which are capable of being tested by experiment. And if so, what are the key experiments and when might they be done?

Roger Penrose: Well I think it's pretty clear that string theory at the moment does not make these predictions. And even some parts of string theory, for example, it depends upon a notion of what's called super symmetry. And the experiments which are about to be performed when the...what's the...the one that's going on in CERN and so on, the new machine which is about to get working. It's supposed to tell us something about the hicks particle and so on and so forth. And the hopeful people might say that it will reveal evidence of the super symmetry. On the other hand the likelihood is that it won't reveal any evidence of it, and so it tells us more or less nothing about string theory. So I'm afraid I have to be a bit pessimistic about present-day experiments. Okay, they'll tell us something interesting and important about what's going on, but they won't resolve these issues, they won't tell us that string theory is right or wrong. And I'm afraid we're not going to see that for a long time. It doesn't mean it can't be experimentally tested. My own personal view is that it's much more likely that a good experiment will come about which will tell us the limitations of present-day quantum mechanics. that's the basic theory that applies to particles and small things and so on. So I think the limitations to that theory, which I believe there are, that these experiments will ultimately show quantum mechanics not to be fully accurate at all levels. That's a much more hopeful big change that could take place. But it's certainly no answer to a theory of everything either way. And I think we're a long way from that. But I wouldn't say that it's impossible. I just don't see it in any way in the near future.

Ramona Koval: Can I ask you to stay in your seats for about 30 seconds, because Roger Penrose is going to go next door to the signing tent to sign these mathematical Finnegan's Wakes and other books of his, and he'd be I'm sure delighted to discuss twistor theory with you en passant. But please for the moment, please thank Roger Penrose. [Applause]

Guests

Sir Roger Penrose
Emeritus Professor of Mathematics at Oxford University and winner of the 1988 Wolf Prize for Physics, which he shared with Stephen Hawking for their joint contribution to our understanding of the universe

Publications

Title: *The Road to Reality: A Complete Guide to the Laws of the Universe*
Author : Roger Penrose
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Weekdays 10am
Sun 7.10pm
Presented by
Ramona Koval

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