An Interview with Sir Anthony Leggett

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Abstract

Professor Anthony James Leggett is regarded as one of the most eminent personalities in the domains of physics and intellectual inquiry of our time. Globally esteemed for his expertise in low temperature physics, he was awarded the Nobel Prize in Physics in 2003 for his seminal contributions in the theory of superfluidity. Pioneering novel directions of research, he has substantially advanced our understanding of the basic features of Quantum Mechanics governing large-scale dissipative systems and has devised ingenious means for utilizing the condensed matter systems like Josephson devices as tools for testing fundamental principles of Quantum Mechanics in the macroscopic domain of systems. Along with his then research student Anupam Garg, Professor Leggett had formulated in 1985 the famous 'Leggett-Garg inequality'whose experimental test has opened up an earlier unexplored avenue for critically testing the incompatibility between Quantum Mechanics and the notion of realism embedded in our everyday worldview; in other words, this landmark work has paved the way for the experimental tests of the celebrated Schrödinger's Cat Paradox in terms of what may be called 'Laboratory Cousins of Schrödinger's Cat'

The following piece has grown out of the engaging conversation **Anindya De** had with Professor Leggett during his last visit to India in connection with the Conference celebrating his 80th Birthday held at Raman Research Institute, Bengaluru.

Interviewer: Let me begin by recalling that you had graduated in classics, specifically in Latin and Greek languages and literature. How did you end up studying physics?

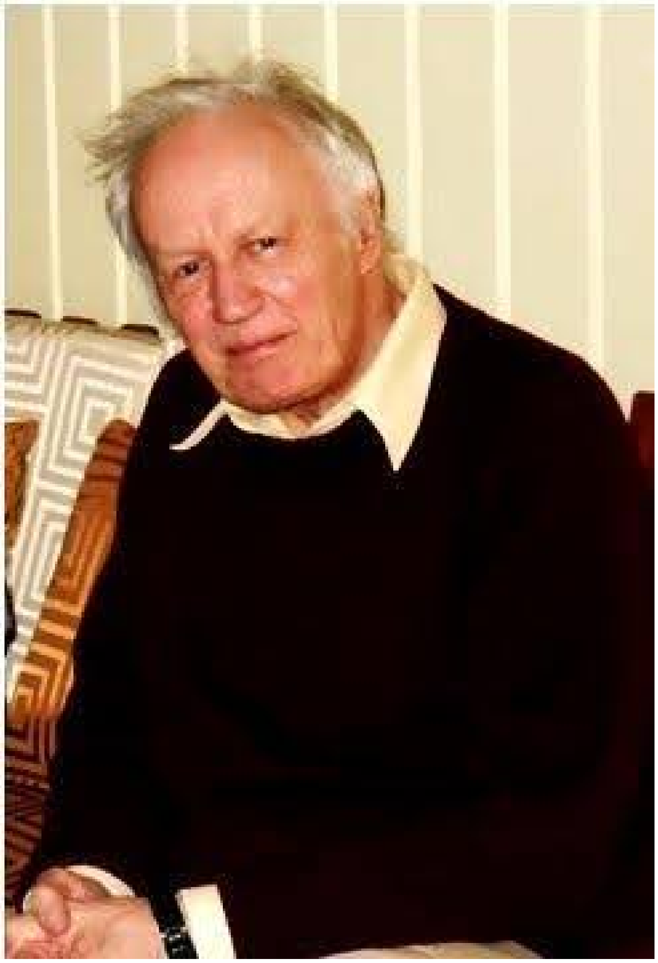

Figure 1: Professor Anthony James Leggett.

Leggett: Let me first clarify that I did have some exposure to modern mathematics in high school, and even considered becoming a pure mathematician. However, I distinctly remember telling myself that I did not want to pursue a career in mathematics. Why? Because in the field of mathematics, being wrong means you are considered stupid. I would like to be able to make mistakes and learn from them, without being labelled as unintelligent. That's why physics fascinated me - I could make non-trivial conjectures about the way the world works and, importantly, with the help of my colleagues, test them with experiments.

Interviewer: In this context, I am interested in learning more about how your background in philosophy has shaped your perspectives on physics.

Leggett: I do not think philosophy has helped me better understand specific ideas of physics, but it has made me a lot more sceptical about things that are usually taken for granted. I am probably a lot more conscious than many of my physicist colleagues of the provisional nature of our current scheme of things and perhaps much more inclined to believe that two to three hundred years from now, our whole picture of the physical world will completely change.

Interviewer: Many of us are curious to know what it is that makes some people come up with momentous discoveries and others, who work as hard as the discoverers, do not achieve that.
Leggett: Somehow, the people who do make the great discoveries are ones who manage to free themselves from conventional ways of thinking and to see the subject from a fresh perspective. But, I would not know how to quantify that.

Interviewer: How would you react if something does not work as you expect?

Leggett: Well, some of the most stimulating experiments, to a theorist, are those which do not come out as you confidently expected them to.

Interviewer: What are the sources of inspiration in your scientific pursuit?

Leggett: I think that is a rather difficult question to answer. In scientific discovery luck plays an enormous role. But, I think, one can be fairly sure that if you have not been thinking about the problem continuously and perhaps even when you are lying awake at night, then it is unlikely that you will get the sudden flash of discovery that makes it work. I always find that the main stimulus to good theoretical work is some curious experimental result that seems prima facie outrageous and unnatural, and one tries to understand it.

Interviewer: So somehow usually experiment is ahead of theory.

Leggett: Often it is. But if one is lucky, one may be able to make predictions about some experiment which has not been done, one would like it to be done and come out the way you say.

Interviewer: But, if we consider superconductivity, we cannot predict the superconductors that would work in high temperatures, like room temperature. What do you think about this?
Leggett: Well, there are about a hundred elements known to us. If you consider a compound which, say, involves six of these elements, then, crudely speaking, there are a trillion of such compounds. Nature has never made most of these compounds. We will certainly not be able to make most of them in any reasonable time. I would take a large bet that somewhere out, there are substances that will be superconducting at room temperature. We just do not know where they are in this immense space. Once we have a generally accepted theory of cuprate superconductivity, I think we may be in a much better position to look for them.

Interviewer: Looking back, which physicist has had the most impact on you?

Leggett: From a purely physics standpoint, I feel a strong resonance with Lev Landau, even though I have never met him in person. Another individual who has had a significant impact on my work is Paul Dirac.

Interviewer: When faced with a complex problem, Dirac preferred to approach it as a whole, whereas Heisenberg tried to break it down into smaller parts and solve each part separately. We are curious to know which methodology you have preferred in your research work.

Leggett: With regard to the research on Helium-3, it was tough to break down the curious issues into smaller components. Therefore, I thought that quantum dissipation-based approach is perhaps the more useful holistic way around. However, we did make an effort to separate the study on Helium-3 into smaller parts and tackle them individually as much as possible. Overall, I believe I have used both the approaches in my career.

Interviewer: In your Nobel lecture you had mentioned a happy coincidence around 1972–73 that led to the finding of the explanation of the puzzling experimental results of Helium-3 which eventually resulted in your Nobel Prize. Can you please recall here that coincidence?

Leggett: In July 1972, while I was on a mountaineering excursion in Scotland, I heard that Bob Richardson, an experimental physicist at Cornell University in the US, was visiting the University of Sussex and wanted to speak with me. Although I was enjoying my holiday, when in the following morning it rained, I decided to head home early. As a result, I was able to spend several hours talking to Bob. He told me about the interesting experiments he was conducting on liquid helium. This conversation changed the course of my research career and, thirty years later, led to my presence at the Nobel Prize Ceremony in Stockholm. If I had not talked to Bob that day, I would have never found out about those experiments before they were published, and my career might have taken a different path.

Interviewer: What, in your estimation, have been the most significant developments or paradigm shifts within the field of physics over the past three to four decades?

Leggett: The general picture of the world given by physics has not evolved a great deal in the last 30 or 40 years. To be sure, there would need to be some important updates in some specific areas, but fundamentally both most of the "knowns" and most of the "unknowns" remain pretty much what they were 30 years ago.

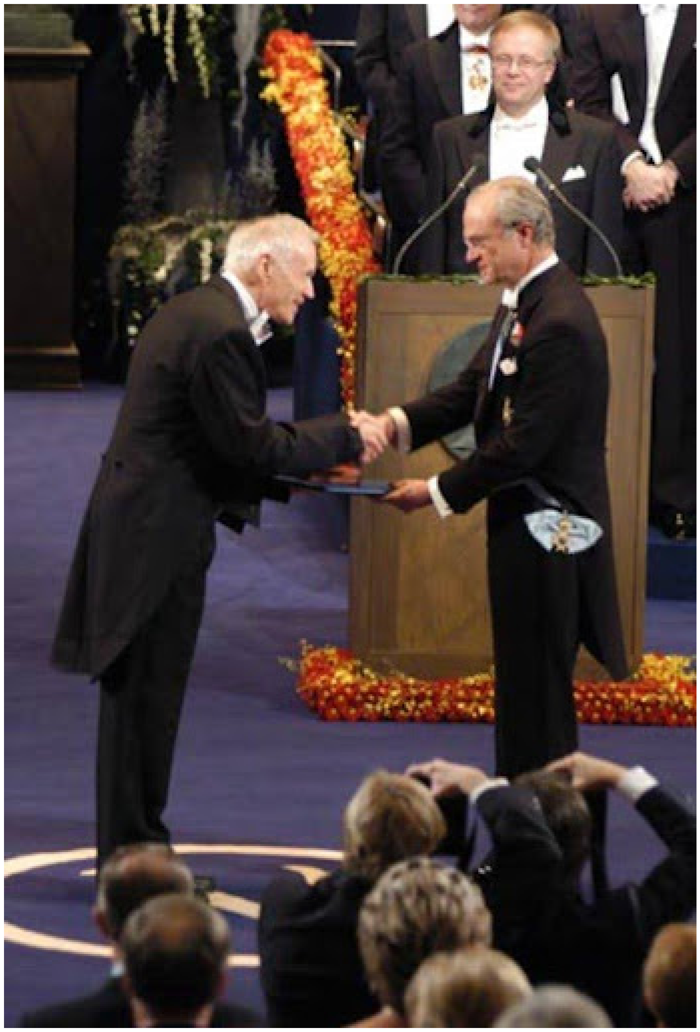

Figure 2: Prof. Leggett receiving his Nobel

> I always make a parable that if while climbing a particular rock face, you think you are the first person ever to have done it — it doesn't really matter if you find out later that you weren't. [1]

In the area of cosmology, there has been at least one major unanticipated development and one somewhat anticipated. The surprise has been the discovery, in the late 90's, that the expansion of the Universe appears to be actually accelerating, contrary to the predictions of the simplest version of the FRW model in which, irrespective of the value of the mass density, the rate of expansion should always decrease with time. The most widely accepted solution to this unexpected state of affairs is to postulate that apart from the "dark matter" which was already believed to make up a large fraction of the furniture of the Universe there is also a substantial component of “dark energy”, though its nature is unknown till today. The more widely forecast development - a real triumph for large-scale experimental physics - has been the observation by the LIGO collaboration in early 2016 of gravitational waves, a result which puts the final seal, should one be needed, on Einstein’s general relativity. Moreover, the shape of the received pulse agrees excellently with theoretical predictions for radiation by two colliding black holes. In the area of particle physics, the most noteworthy development of the last 30 years has probably been the long-anticipated detection, in 2012, of an excellent candidate for the Higgs boson, with a mass of about 125 GeV. A somewhat less anticipated discovery is that at least some neutrinos have a very small but definitely nonzero mass of nearly 1.8×10^{-36} kg, which corresponds to ~ 1 eV[1], while both these results fit into the “standard model”. But, it is no clearer than 30 years back whether this model is the ultimate truth, or whether the numerous constants occurring in it have a deeper explanation. A major upset in condensed matter physics (which actually occurred in late 1986) is the discovery that superconductivity can occur at temperatures much higher than the previously accepted limit of about 25K. The phenomenon of “high-temperature superconductivity” was originally thought to be limited to a class of strongly layered materials, the cuprates, and to be due to a mechanism quite different from those which apply to most previously known materials; but more recently it has been found elsewhere, and ironically, in the record-holder system as of late 2016 (a hydrogen-sulfur compound with a transition temperature, un-

[1]Interviewer’s Note: The latest upper limit for neutrino mass, set by the KATRIN experiment, is less than 8×10^{-37} kg (~ 45 eV), a significant reduction from previous bounds and about a millionth the mass of an electron, with results published in Science in April 2025. While the exact mass is still unknown, this experiment provides the most stringent limit, showing neutrinos are incredibly light, with ongoing measurements aiming to push this boundary even further.

der extreme pressure, above two-thirds of room temperature) the superconductivity appears to be of the "classic" type [2]. One new and exciting area of physics which did not exist 30 or 40 years ago is "quantum information" - a group of technologies which exploit the bizarre features of the quantum-mechanical description of the world to permit secure communication ("quantum cryptography", already realized), massive parallel computation ("quantum computing", still largely on the drawing board) and more.

Interviewer: We are very fortunate that you have written an updated preface for the Bengali translation of your book 'The Problems of Physics' [2], which will appear soon. In this piece you have commented on the postulated idea of Dark Energy. You wrote that ".... this idea may turn out to be no more than a band-aid". Could you please elaborate on why you think this way?

Leggett: I am not an expert in cosmology, just an outsider. However, from what I have gathered, there seems to be a growing desperation to explain the accelerating expansion of the universe within our current framework of ideas. So my bet would be, for what it is worth, that in the next thirty years or so there is going to be a major revolution in cosmology. While we cannot predict the nature of this revolution, I am inclined to believe that it will happen.

Interviewer: In the famous 2005 Berkeley Debate[3], you expressed support for physicists trying to change quantum theory. Borrowing your own theme, I would like to ask 'Is Quantum Mechanics the whole truth' [4]?

Leggett: The answer is probably no. I believe that as we try to

[2]Interviewer's Note: The maximum critical temperature (T_c) for high-T_c superconductors varies, but record-holders include cuprates reaching 135 K (around -138 °C) at ambient pressure, while certain compounds under extreme pressure have shown superconductivity near 250 K (around -23 °C), though these require immense pressure. The goal is room-temperature superconductivity (around 290 - 300 K or 16.85 °C - 26.85 °C), which has been observed in some compounds under very high pressure but not yet stably at ambient conditions

really understand coherently the quantum measurement paradox or Schrödinger's cat paradox (viz. the puzzle as to how a definite outcome, say a dead or alive cat, occurs in any measurement process, while quantum description of the process predicts a superposition of states, say a live and a dead cat) by applying quantum mechanics all the way as we move up from electrons to our conscious experience of the outcome, there will come a point where our current understanding will break down. We do not know when or how this will happen, but I am quite certain that it will[5].

Interviewer: We have observed the phenomenon of quantum entanglement to be true, but we do not yet fully understand its physical nature. You had mentioned your intuition that if a solution is ever discovered, it will require a significant revision of our understanding of the arrow of time. Will you please expand a bit on this?

Leggett: One of the assumptions necessary to derive the Bell-CHSH inequality based on the ideas of locality (no action-at-a-distance) and realism (pre-existing definite properties of any object, independent of measurement) is that the properties of ensembles are determined entirely by the initial conditions. Thus, one way of reconciling the apparent contradiction between the idea of local realism and experimental refutation of the Bell-CHSH inequality would be to reject this assumption, i.e. to postulate that our usual assumption that the past can affect the present and the present can affect the future but not vice versa, is incorrect. I have never been able to understand Hawking's arguments[3] about why this point of view is unviable. There is a good

[3] In Chapter 9 of his famous book 'A Brief History of Time: From the Big Bang to Black Holes' (Bantam Books, 1988) Stephen Hawking identified three distinct 'arrows of time': a psychological arrow (our memories of the past and imagination of the future), a thermodynamic arrow (the direction in which entropy increases), and a cosmological arrow (the direction in which the size of the universe increases).

discussion about this point in Huw Price's book 'Time's Arrow and Archimedes' Point' (Oxford University Press, 1996).

Interviewer: It has been said, for example, that a phenomenon like superfluidity exhibits at the macroscopic level the evidence for the validity of quantum mechanics. What is your comment on this?

Leggett: I think one has to be a little careful about such a statement, because the comment that "one can see the evidence of quantum mechanics on a macroscopic scale" is quite ambiguous in its meaning. It is certainly true that in a superfluid, or in a superconductor, you have a very large number of atoms or electrons which are all behaving coherently. So, the effects which normally occur at atomic level are enormously amplified. However, there is a much more dramatic prediction, namely that, under certain circumstances if you apply quantum mechanics consistently, you reach a description of the world in which there is a quantum superposition of two macroscopically different states. The famous example, of course, is a cat inside a closed box. If you apply quantum mechanics consistently to this situation, you appear to arrive at a description in which the cat is neither alive nor dead, but in a quantum superposition of the two states, until some further observation is made. However, it is difficult to express the above feature in classical terms. If you take the interpretation of quantum mechanics seriously and if you apply the same interpretation at the level of the cat, as you do at the level of the atom, then you do seem to reach the conclusion that it is not definitely in one state or the other until observed. That is the famous quantum measurement paradox or Schrödinger's paradox. But it is a very different situation from what one normally gets in the sort of standard applications of superconductivity and superfluidity.

Interviewer: Is it not bizarre to have such a superposition somehow at a macroscopic level?

LeggettThe existence of liquid helium at low temperatures that does not solidify at ambient pressure is essentially a quantum phenomenon. Such an object should not exist according to classical laws. I think that it is one of a number of cases one could quote in which one sees, in one sense or another, the macroscopic effects of quantum mechanics. But then, here one regards the difference between a liquid and a solid as a macroscopic difference. I do think there is a big difference between this kind of case and the genuine Schrödinger's cat kind of situation, which is one which we have not yet been really able to probe directly in experiments, although we are working towards it.

Interviewer: In order to investigate the applicability of quantum formalism at the macroscopic level, you had suggested utilizing condensed matter systems, such as Josephson devices for showing what has been called Macroscopic Quantum Coherence. What progress has been made toward success in this area?

Leggett: I would say that the level of success we have achieved is surprisingly high. Back in 1980, when I first proposed this idea, there was a lot of scepticism, especially from the quantum measurement community. Even as recently as 1999, a very senior theorist solemnly told his experimental colleague, who was beginning to build such a device, that he was wasting taxpayer's money, saying it would never work. However, we have now reached a point where Josephson devices are routinely referred to as 'artificial atoms' in scientific papers. In fact, many of the atomic level phenomena that we observe can also be seen in these devices. Recently, we were even able to conduct an experiment that not only tests quantum mechanics at a reasonably macroscopic level involving superposition of oppositely circulating currents in a superconducting ring, but also provides rigorous experimental constraints on the notion of macrorealism.

Interviewer: Accepting weirdness of quantum mechanics has been a daunting issue even for the colossal scientist Albert Einstein. He could never reconcile himself to quantum mechanics, because it was too bizarre for him. What is your perspective on this issue?

Leggett: I personally think it is entirely possible that in the year 3000 we will still believe that quantum mechanics is the whole truth about the world. But then, I guess that our attitude towards the physical world at the everyday level will be radically different from what it is today. We will then really have had to face up to this weirdness, which by that time, I am confident, will have been amplified to the everyday level. I think it is at least equally probable and perhaps more so, that as we go from the level of the atom to the level of the cat, we will find that somewhere along the line quantum mechanics breaks down and some new theory, of which we can have no conception at present, will take over. I am personally hopeful that it is the second thing that happens.

Interviewer: Suppose the quantum superposition of macroscopically distinct states of a macro-object is found to be empirically well verified up to the level of macro-measuring devices, what would be in your view the Quantum Mechanical measurement problem - how then to reconcile the apparent inconsistency between the formalism of Quantum Mechanics and the actualization of definite measurement outcomes subsequent to measurement interaction?

Leggett: In a sense we have already empirically verified 'quantum superposition of macroscopically distinct states of a macro-object', see e.g. Section 6 of my latest paper[6]. I think a more important milestone would be to verify its occurrence at the level of macroscopically distinct states of the human brain. Were this to be achieved, while the subject still registered the consciousness of a definite outcome, the only way I would know of interpreting the situation would be to regard the whole of quantum mechanical formalism as merely an abstract prescription or calculus which corresponds to nothing at all in the real world, but is simply a formal procedure for calculating the probabilities of directly observed outcomes. A trenchant defence of this interpretation has been given by L. E. Ballentine[3] and I would merely defer the 'realization' process to a point much closer to our everyday experience than he needed to in 1970. But I would not feel comfortable with it!

Interviewer: What about quantum computers?

Leggett: Well, if quantum mechanics does describe the whole universe at all levels, then it seems that there is no reason, in principle, why one should not build a functioning quantum computer. I think, however, one may well find that the practical difficulties of doing that are just so enormous that in the end people will conclude that it is just not worth it.

Interviewer: So, how would you summarise the present state of studies on quantum computers?

Leggett: I would think it is probably fair to say that, at least right now, the challenge of quantum computing is not throwing up any very deep new conceptual questions. It is a matter of, in some sense, engineering, and so it is a matter of taste, whether you regard that as interesting or not.

> Always try to follow your own curiosity, and don't worry too much if other people say, "Well, you know, everyone knows that. It's well understood." If you don't understand it, then beaver away at it, and don't stop until you think you've got something which really satisfies you." A. J. Leggett in Oral History Interview, American Institute of Physics [7].

Interviewer: It is suggested that reality and information are, in a deep sense, closely linked. Professor John Wheeler described it as: "it from bit"[7]. What is your view?

Leggett: From the exposition of the "it from bit" thesis quoted in Wikipedia from one of Wheeler's last writings on the subject, I infer that what he was really proposing is that the history of the universe is determined at least in part by the set of experiments we (or Nature) choose to conduct and the answers which are obtained. This seems to me a not necessarily unviable point of view (a valuable discussion on this topic also can be found in Huw Price's book), but it does not seem to me well summarized by statements like "physics derived entirely from information" unless we postulate that the experimental outcomes (or other macroscopic realizations) have to be consciously registered by a human observer. But then the subject of information is about the outcomes, which are observer-independent. (If we believe that a human observer is essential, we can ask, following John Bell, whether he/she has to have a Ph.D. in physics. I do not think Wheeler ever answered that question.)

Interviewer: What are your views on the role of science concerning social issues in the contemporary world, and the significance of the interplay between science and liberal arts?

Leggett: I believe it is a fallacy to consider science, specifically physics, as a model for social science. Instead, we should view them as separate entities and analyze them independently. As regards the interplay between science and liberal arts, one can try to envisage possible useful interactions between the practitioners of these two fields. However, I doubt that merely bringing a scientist and a liberal arts person together will result in significant progress. We have to remember that some interdisciplinary efforts can seem forced and contrived.

Interviewer: In 2014, you co-authored an article with Pickett and Chu titled 'Science Diplomacy in Iran'[8]. What is your opinion on the role of Science Diplomacy in today's world?

Leggett: I think the exchange of knowledge and ideas between the West and the former Soviet Union during the Cold War was very important. Many individuals had the opportunity to establish both scientific and personal relationships with leading Soviet scientists, which helped dispel the notion that the Soviet Union was an entirely evil entity. During the Cold War era, roughly between 1965 and 1990, I crossed the Iron Curtain several times and entered Poland a number of times as well. Though it was not exactly a matter of science diplomacy, I never felt that the inhabitants of these states were somehow different from us. I observed the usual human virtues and faults in them. I have a number of friends and contacts on the other side of the Iron Curtain. I believe that a similar situation has occurred with Iran. Many people in the United States have held a stereotypical view of Iran, but by visiting there and speaking with leading scientists and politicians, we were able to develop a more nuanced perspective. This new perspective could then be shared with our colleagues who have not had the opportunity to visit Iran themselves. Overall, I believe that such visits are very useful for gaining a better understanding of a country.

Interviewer: You have been visiting China often and during the visits you must have interacted with Chinese people at various levels. In this context, it would be very useful if you could please comment on the scope of science diplomacy and interactions with China in the present world.

Leggett: I have indeed visited China much more than I have visited Iran. In fact, I had a summer appointment at Shanghai Jiao Tong University for the period 2014-2021 (though because of the pandemic I had to do the last two years remotely). So I got to know a number of my Chinese physics colleagues pretty well. On the other hand, in the case of Iran, my visit was deliberately organized as partially an exercise in science diplomacy. But, in the case of China, with the exception of a meeting in 2014, I did not get to meet any of the Chinese political leadership, so any opportunities for science diplomacy were limited and indirect (as I think they were for the Western scientists who visited the Soviet Union during the Cold War). I still think that just in that case they were very worthwhile.

Interviewer: The relationship between religion and science is a topic of ongoing debate. What is your opinion on this issue?

Leggett: I am inclined to think that one's religious beliefs do not necessarily impact their abilities as a physicist. From my experience, I have encountered many exceptional physicists who are atheists, as well as many others who are devoutly religious. In my opinion, one's faith does not make much difference in one's professional abilities.

Interviewer: What would be your suggestion for fostering scientific attitudes among present-day school-going children?

Leggett: I believe for this purpose it is particularly important for teachers to be willing to admit when they do not know something. In India, this may be difficult because teachers are often viewed by their students as having all the answers. I have been asked by Indian school teachers what to do when a student asks a question they do not know the answer to. My recommendation is to simply say, "I am sorry, I don't know, but I will try to find out. In case I am able to find out the correct answer, I shall certainly tell you".

Interviewer: As a science teacher, I would like to know your views on the desirable method of teaching science at the school level.

Leggett: I am not entirely certain that there is an ideal method of teaching science. Similarly, I do not believe in the existence of ideal courses, since what may work perfectly for one set of students may not be successful for another. However, there are some simple rules that one can follow, such as explaining concepts in the simplest possible way and using easy-to-understand experiments. Besides that, I do not have any special recipe to share.

Interviewer: Would you encourage your grandchildren to go into science?

Leggett: I would encourage my grandchildren to pursue a career in science, but I would discourage them from choosing physics. I believe that there are more exciting and promising areas in science, such as neuro-psychology and others, that would provide more challenging opportunities for intellectual growth.

Interviewer: In school, you participated in long-distance running, excelled in chess, indulged in long hikes and bicycle rides into the countryside, and fell in love with mountaineering. How do you like to relax now?

Leggett: Nowadays, my schedule is extremely hectic and I have numerous deadlines to meet. A lot of the time, the best answer would be to simply get some rest. Despite this, I still enjoy hiking and engaging in some light mountaineering. However, my wife prefers that I avoid highly demanding pursuits, so I have not gone rock climbing in many years. I also enjoy watching films and reading the odd thrillers, though nothing too intense.

Interviewer: You had written an unusual article titled 'Notes on the Writing of Scientific English for Japanese Physicists'[9]. In that article, there are a number of lucidly explained valuable suggestions concerning the structuring of scientific papers, construction of sentences, use of appropriate words, the optimal length of a sentence, and so on. Why do you think this article made such an impact on Japanese physicists?

Leggett: For what it is worth, my own feeling is that, the most important and possibly most novel feature of my essay on scientific English and the reason it seems to have struck a resonance in Japan lies not in my comments on the kind of relatively 'microscopic' points which you mention, but rather in my attempt to identify the ways in which the deep differences between Japanese and English 'ways of thinking' are reflected in the writing of scientific papers. I would be very interested to know whether you think that there are similar 'deep' differences between English and other Indians, such as Bengali, ways of thinking which affect the writing of scientific papers (I tend to suspect not; I think this kind of difference may be peculiar to Japanese. But, of course I do not know a word of Bengali or any other Indian language).

Interviewer: Thanks very much for raising this intriguing question. Surely, this will stimulate many of our readers to mull over this issue in the context of their respective languages.

The Interviewer thanks **Professor Dipankar Home** of Bose Institute, Kolkata for his valuable help in planning the content of this interview and guiding preparation of this write-up. Thanks are also due to the organisers of the conference celebrating Sir Anthony Leggett's 80th Birth Anniversary at Raman Research Institute, Bengaluru [10], for their support enabling this conversation.

Extended Insights:

Here we have added selected excerpts from Prof. Leggett's interviews with **Bin YI** posted in YouTube [11] and **Howard Burton's** book *The Problems of Physics, Reconsidered, Conversations About Physics, Volume 1, Ideas Roadshow Collection* [12] for supplemental material.

Excerpts from Prof. Leggett's interviews with Bin YI

Question 1: What is the most valuable thing that you've had?

Leggett: The most valuable thing that happened to me was when I started my permanent career in physics after the post-doctoral work at the University of Sussex in 1967. My job was teaching. My condition of work was that if I did a good job of teaching throughout the week, I could go home on Friday night and feel I'd earned my salary. And then, if I wanted to, I could spend at least part of the weekend doing research and the university would support me in this; I mean it would find me secretarial help and libraries, things like that. But research was not part of my job. And I think that was enormously valuable and had it not been for that I don't think I would have been able to do the work on superfluid Helium-3, which was eventually recognized by the Nobel Committee.

Question 2: On which problems are you thinking presently?

Leggett: Currently I'm trying to think about a number of problems. One of them concerns so-called Majorana Fermions and so-called topological superconductors. Topological superconductors are believed to occur in some cases. For example, in Strontium Ruthenate if these Majorana Fermions occur and have the properties that people think they do. Then they would be very useful in particular in topologically protected quantum computing. However, I believe that the theoretical arguments which have been used in favour of these quasi-particles, these Dirac Fermions are faulty and that these quasi-particles either don't exist at all or if they do exist, they don't do not have the properties which people usually ascribe to them. So I've been trying to find a way of settling that question one way or the other. I did publish a paper on this about a couple of years ago.

Question 3: Do you have any suggestions or advice for young students?

Leggett: Never suppress your curiosity, even if others dismiss your questions as silly or obvious. The greatest breakthroughs often come from questioning what “everyone knows”. For 300 years, since Galileo, it was accepted that all objects fall at the same rate in a vacuum. Everyone took this for granted, but Einstein asked a simple question: ‘Why’? He was likely met with pats on the shoulder and comments about it being a silly question with a known answer. By thinking deeply about this ‘why’, he eventually developed the theory of general relativity—the most beautiful part of modern physics.

Secondly, when you have a question, don’t let the fear that it’s already been answered in existing research, it will stop you from exploring it yourself. Try to approach the question with a fresh perspective, using your own reasoning and methods. Work through the problem your own way, without initially relying on established answers.

Thirdly, never consider an honestly conducted research project a waste of time, even if it doesn’t solve the immediate problem. Resist the urge to throw away research that didn’t ‘pan out’. Write it up properly, detailing your process and findings. Put this write-up away in a drawer. Years later, that archived work can resurface as the key to solving a completely different problem. The knowledge and techniques you developed become a hidden resource that matures with time. My own research on ‘two-band superconductors’ in Kyoto seemed like a dead end at the time. However, over a decade later, it unexpectedly provided the crucial clue for understanding the microscopic dynamics of Helium-3 - a breakthrough that was impossible at the time of the original work.

Fourthly, resist the urge to overcomplicate your work with unnecessarily powerful, abstract techniques. Strive for the simplest possible formulation that gets to the physical heart of the problem.

Finally, if your role involves both research and teaching, you should treat teaching with at least the same level of seriousness as your research, if not more. This commitment is a reciprocal benefit. Your students will receive a high-quality education from an engaged and dedicated teacher. On the other hand, the effort you put into teaching is directly repaid; the process of explaining fundamental concepts clearly often sparks new research ideas and provides profound insights. Some of my best ideas during my research career have come out of my teaching work. So it's certainly not a waste of time to spend time and effort on your teaching activity.

Excerpts from Prof. Leggett's interviews with Howard Burton

Taken from "The Problems of Physics, Reconsidered"A conversation with Tony Leggett (Ideas Roadshow) [13]

Question 1: There is a long history of people making grandiose statements about what it is to be foundational. What are your views about "fundamental" or "foundational"?

Leggett: Perhaps I can best define what I regard as "fundamental" by what it excludes. I think what it excludes is the kind of operation-which is very common, and all of us, including me, do it all the time- where one starts off from a set of assumptions which, at least for the purposes of the calculation, one is not going to challenge. One then tries to infer certain consequences of these basic assumptions. A typical example might be what a lot of electronic band structure work is about. You start off with, basically, Schrödinger's equation for a set of 1023 atoms or whatever, and you try to make approximations to them. Now, it's not obvious that some of this is not what I would call fundamental or foundational, because the process of making those approximations you may have to, at some stage, introduce new concepts. To the extent that you're introducing new concepts, which can't be totally explained away in terms of the existing ones, then I think you are, to an extent anyway, doing fundamental work. I think a very good example of what I would call fundamental or foundational work in condensed matter physics is the work of Lev Landau-in particular his beautiful work on superfluid helium, where he introduces the idea of elementary excitations, the idea of a normal component, a superfluid component and various other things. These were not things which you could define rigorously in terms of a simple picture of 1023 atoms or whatever.

Question 2: How much do you think being fundamental is linked to the spirit of reductionism? Should we be doing anything different about that?

Leggett: As a professional condensed matter theorist, yes, I think we should. I think that the instinctive view that you understand how things work by taking them apart and so forth, is an implicitly anthropocentric kind of view. You have a radio or a bicycle or whatever, and if you're curious you might unscrew the back and take the various bits out to try to figure out how it works. But that whole approach might be misleading, because that radio has been put together by human beings: it's not so obvious that this is generally going to work on the objects that nature creates. But even if you accept that, I frankly just do not see why the study of the microscopic components of a macroscopic object is more fundamental than the study of how these interact in subtle ways when they're all put together. This is the kind of point that Ilya Prigogine, Phil Anderson, Bob Laughlin and so forth have tried to make. Personally I'd go even further, and this is a very radical and minority point of view, I think. I would claim that there is a real possibility that there may actually be laws of physics that only come in at the level of subtle, complicated, macroscopic objects. In other words, I think my difference with people like Prigogine, Anderson, Laughlin and so forth, would go something like this: we all agree that even quite inanimate phenomena of various kinds can't be, in practice, reasonably explained-in any meaningful sense of the word "explained"-by simply writing down Newton's laws or Schrödinger's equation for 1023 particles. But people like Laughlin and company would say that at least the phenomenon is consistent with the speculation or conjecture that Schrödinger's equation does work. It may not be a very useful piece of knowledge, but at least it's consistent. I would go even further than that and say that I would not be totally surprised if there were actually new laws of physics that would come in at some level of macroscopicity or complexity which mean, crudely speaking, that quantum mechanics is not the whole truth about the world.

Question 3: You mentioned earlier that one of the things that you had been musing about was how issues related to the arrow of time might somehow be associated with some of the foundational issues of quantum theory. What is your speculation on that?

Leggett: Of course the issue of the arrow of time has been around for a long time and a lot of people have thought about it. One thing I'm relatively happy with- and it's part of the common wisdom on the subject is that if you're going to look for an ultimate explanation of the arrow of time, in the end it has to go back to cosmology.

The most general aspect of the arrow of time which we're conscious of in everyday life can be summed up in the Second Law of Thermodynamics: that disorder tends to increase as a function of time.

Given that general principle, I think it is not so difficult to extract particular applications; for example, that one can remember the past and affect the future and so forth. I'm not saying I can do it and I think that people have perhaps tended to go overboard in saying that they know how to do it but I can't see any obvious insuperable objections to doing it. It seems reasonable.

However, how are we going to justify the thermodynamic arrow of time? One possibility is to say: "Well, that's just the way it is. Entropy was low in the past and is increasing as we go into the future. That's just a fact of life."

If you don't like that then I think the obvious solution is to look at cosmology and acknowledge that entropy has to do with the fact that the universe is expanding from the hot Big Bang where the entropy was very low. Why? Well, that's a huge question and all the heavyweights of the field have tangled with that and fought over it. I'm not necessarily going to go into that (the reader who does want to get into it, however, is referred to the Ideas Roadshow conversation The Cyclic Universe with Roger Penrose, where the question is discussed at length).

But let's just suppose that problem is solved, I think you still have a problem with the following question: given that the overall arrow of time is in the direction that we all know and love, is it possible that there are fluctuations-and here I'm really talking off the top of my head - in small regions of space-time in which, in some sense, the normal arrow of time would be reversed-in particular, in which one could legitimately ascribe the cause of an event to what is going to happen in the future, instead of what has happened in the past?

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