

# An interview with Sir Anthony Leggett



In the summer of 2005 Professor Leggett visited the University of Lisbon where he gave a seminar and public lecture. The visit was organised by the Centro de Física Teórica e Computacional following the ‘tradition’ of inviting a renowned physicist to close the academic year’s activities. His predecessors were Professor Eric Cornell (2003) and Professor Pierre Gilles de Gennes (2004), recipients of the Nobel Prize for Physics in 2001 and 1991, respectively.

During the visit Professor Leggett was interviewed by Patrícia Faisca of the Centro de Física Teórica e Computacional da Universidade de Lisboa and by Pedro Patrício of the Centro de Física Teórica e Computacional da Universidade de Lisboa e Instituto Superior de Engenharia de Lisboa.

After completing your degree in Classics at Balliol College in Oxford, you decided to take a second undergraduate degree in sciences. Why? And why physics?

I think I was probably just very unimaginative and it really didn’t occur to me that I should try to find a career outside academia. I had no experience on anything else at that stage and academic life really seemed the most obvious option. During my Classics course I had concentrated mostly on philosophy, which was something that I was most interested in and that I, in some sense, most enjoyed. But as I started to contemplate going on to a full time academic career in philosophy I realised that I didn’t want to do it. And then I started asking myself why exactly I didn’t want to do it, and I was sufficiently unimaginative that it didn’t occur to me that maybe my problem was with academic life as such and so it had to be something wrong with philosophy as such. The more I thought about it, the more it seemed to me that basically what counted as good or bad work in philosophy depended so much on the precise term or phrase that you used, and the particular examples you chose, and it seemed to me that this was all somehow very subjective. And I really feel comfortable with a field of study where, in some sense, Nature will tell you whether what you are doing is right or wrong. I knew very little about physics in those days but I had had a slight encounter with it: I studied things on the edge of

physics, for example, I remember during my degree in Classics I looked at things like Zeno’s paradox; so I had some vague idea about what physics was about and it seemed to me that it was the kind of field where I would somehow feel much more comfortable and happier.

Was your original undergraduate training in Classics useful in your scientific academic career?

Yes, it was very useful. In some sense, all of it was useful and particularly useful was the philosophy part. I think that if you go through a course of analytical philosophy – I’m very conscious that philosophy means different things in different parts of the world – but in the Anglo-Saxon tradition, a degree in philosophy is very highly analytical, and if you go through it you do become much more conscious of the implicit assumptions that you’re making in your work. I do feel that one benefit I had from this is that – more than a lot of my colleagues in physics who hadn’t had this kind of experience – I’m conscious of the implicit assumptions I’m making.

Do you think your early training in Classics influenced your research in the conceptual foundations of quantum mechanics?

Yes, certainly, very much so as regards the philosophy component. To be quite honest, in the first few years when I was doing physics, I was not particularly interested in the foundations of quantum mechanics. Then I had a colleague at the University of Sussex, Brian Easlea, who had started life in physics but gradually wandered off to history and social studies of sciences, and he gave a little course of lectures, a mini-course, on the Quantum Measurement Problem. I guess that was what really persuaded me that it was something that was worth thinking about. Probably it was by the end of the 1960s that I really started thinking about it, but I was not able to do anything for another 10 years or so, my first paper on the foundations of quantum mechanics appeared in 1980. There’s a whole generation, I would say, roughly from let’s say late 1930s to perhaps the early 1970s, when the whole subject was almost taboo in the Anglo-Saxon countries but it is interesting that in the Mediterranean countries of Europe it never became taboo. People were always interested, even in those years. But I suspect I would never have got interested myself were it not for my education in philosophy.

When have you become interested in the work that was recognized in 2003 with the Nobel Prize, namely the theory of superfluid liquid  $^3\text{He}$ ?

My PhD degree had two parts: one was connected with the interaction of phonons in liquid helium 4, so it hadn’t much to do with it directly. The second part was a study in the context of the Landau-Fermi liquid theory. The latter was published in 1957, I think, and in 1961, when I started, because of the delays in translation, the Landau-Fermi theory was still not widely known in the West. I knew it because one of the great pieces of advice that my adviser Dirk ter Haar gave me, at a very early stage, was ‘make sure you know enough Russian to read things in the original’. That was a very useful advice and I read all the Russian papers on Landau-Fermi liquid theory when they first came out and I was quite impressed by that, quite interested in it, and the second half of my DPhil thesis was on the helium-3 helium-4 phase diagram. You have to remember that, in those days, the phase diagram of helium-3 helium-4 mixtures had been explored only at quite high temperatures. People knew there was a phase separation but they didn’t know how it would go at lower temperature, in particular they didn’t know whether a small amount of helium-3 would be stable in helium-4 at zero temperature or vice-versa or both. I chose the helium-3 region of the diagram and looked at dilute solutions of helium-4 in helium-3. This was the wrong guess because the phase separation curve basically hits the axis on that side. The other side is much more interesting, and years later Bardeen, Bahm and Pines did some very interesting work on that.

What is the importance of superfluid  $^3\text{He}$ ?

From a purely immediate practical point of view superfluid helium-3 is probably the most useless system ever discovered. However, from a more indirect point of view, it is really quite significant because it is the most sophisticated physical system which we can currently claim a quantitative understanding of. We believe that we do understand a great deal of what goes on in superfluid helium-3. Some very interesting and almost unique phenomena do go on, some of which have analogues in other systems with more direct practical application, for example, high-Tc superconductors. I think it’s probably fair to say

that by trying to understand the properties of superfluid helium-3 we have a lot of spin-offs for these other systems. For example, we can apply some of the ideas developed there to the early universe and to particle physics.

**Can you tell us about one of your other research interests, namely the theories and experiments aimed at testing the limits of quantum mechanics? What is the state-of-the-art in the field? Do you think that the next generation of experiments will 'see' the 'failure' of (our understanding of) quantum mechanics?**

This is something I'm certainly been trying to push for the last twenty-five years. One of the explicit suggestions I made was to look for the effects of superposition of different flux states in an rf-SQUID set up. You have two SQUID rings, two states which are nearly degenerate. In one of them the current is going clockwise with an amplitude of, say, a few micro amps, and in the other it is going anticlockwise. Could you actually set up an experiment to look for the effects of interference between these two states? The reaction I got from quite a large part of the quantum measurement community in those days was: this is complete nonsense, everyone knows that by the time you get up to this kind of macroscopic level decoherence is going to kill you stone dead, you are not going to see any interference. So, we had to work quite hard fighting these objections, we published quite a few technical papers in the 1980s on this. Then, out of the blue, along came quantum computing (QC), something we certainly hadn't foreseen at all, and this meant that all these experiments which were done on a shoestring previously, now have no trouble attracting funding from the QC program. So, over the last five years a number of these experiments have been done and what's ironical is that the people who came into the field in the 1990s are complaining that the degree of coherence in the Q-factors of these SQUIDS is only 300! In 1980 most people didn't believe in it all, so I think it's quite amusing.

**Can you explain quantum decoherence and how it may compromise the future of quantum computing?**

Well, it may or not, we don't know. The sort of general trend of the argument which people have used about decoherence is right in the sense that the more complicated you make the system and the more interaction it has with its environment then, by and large, the higher the

degree of decoherence it's going to be subjected to the more difficult it's going to be to make it operate as a qubit. What, maybe, these arguments tend to forget, or at least did in the past, is that there is always some kind of specific reason for why you get decoherence: there is an interaction with a particular kind of degree of freedom of the environment and if you can just isolate that and, possibly eliminate it, then you're in business! And that's exactly how these recent experiments have worked; they basically thought about the possible mechanisms for decoherence and one by one have eliminated them. So, in the end you have up to a Q-factor of 300, even in the original arrangement that I proposed, which was completely unexpected as recently as 6 years ago. And it seems there is no reason in principle why it won't get a lot better. So, at least from the point of view of this simple, sort of common or garden, decoherence, there is no reason why a quantum computer will not work. In fact, even a quantum computer built out of SQUIDS may well work. But there are more subtle things which I think we will not know until we actually start building a working quantum computer. I believe there is a class of effects associated with virtual transitions via the environment which have not properly been looked at so far, and that by definition won't occur at the level of a single qubit or even 2 qubits or three. It's only when you go to very large numbers that these effects may play a role and then they could be extremely destructive. I actually have a student who will hopefully look at that question. We believe it's something which hasn't been dealt with properly in the past. If you asked me about the future of quantum computing, say in 10 or 15 years, I would guess that it will look like controlled thermal nuclear fusion does right now. In other words, the basic principle seems ok, there is absolutely no overarching theoretical reason why it won't work. But it's just such a pain to put all the bits together that people are going to ask: What is it worth to factorise a five hundred digit number? And the answer is not infinity! I mean it's very large but not infinity! And they may decide it's just not worth it.

**According to Arnold, the Russian mathematician, "the only computational experiments worth doing are those that yield a surprise". What do you think is the role of computer simulations in physics?**

I think it basically allows us to implement a lot of ideas that we've had in a qualitative

sense, and that we have been able to work qualitatively. And we want to know quantitatively how good they are. Within my field, one very nice example of that is liquid helium-4. People like Landau and Bogoliubov had a lot of very fruitful qualitative ideas but it has never been possible to implement them quantitatively by any known analytical technique. What was really quite revolutionary, as shown by people like my colleague David Ceperley, at the University of Illinois, is that you can actually use computational techniques to generate numbers, for example for the excitation spectra of helium-4 that may be compared with experiments, of normal fluid densities as a function of temperature, and thus tested. The degree

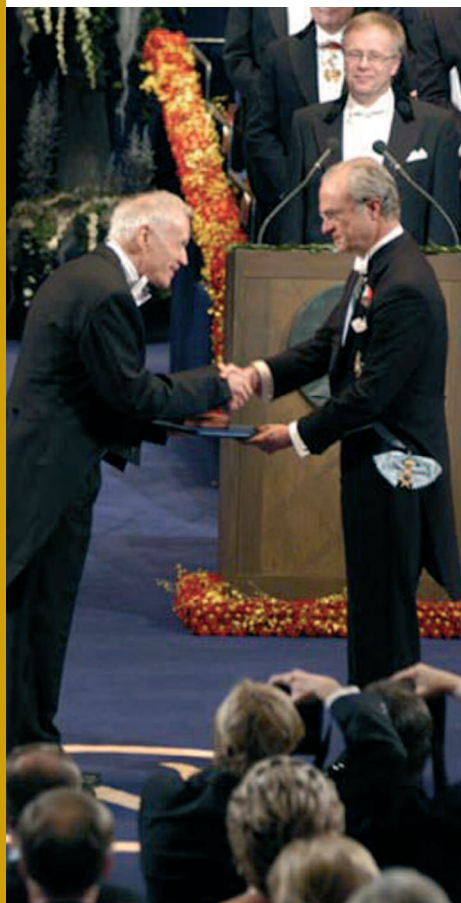
### Vita

Anthony J. Leggett, is the John D. and Catherine T. MacArthur Professor and Center for Advanced Study Professor of Physics, and has been a faculty member at the University of Illinois at Urbana Champaign since 1983. He is a leading scientist in the theory of low-temperature physics, and his pioneering work on superfluidity was recognized by the 2003 Nobel Prize in Physics. Professor Leggett has shaped the theoretical understanding of normal and superfluid helium liquids and other strongly coupled superfluids. He set directions for research in the quantum physics of macroscopic dissipative systems and in the use of condensed systems to test the foundations of quantum mechanics. His research interests lie mainly within the fields of theoretical condensed matter physics and the foundations of quantum mechanics. He has been particularly interested in the possibility of using systems, such as Josephson devices, to test the validity of the extrapolation of the quantum formalism to the macroscopic level; this interest has led to a considerable amount of technical work on the application of quantum mechanics to collective variables and in particular on ways of incorporating dissipation into the calculations. (Further information is available from [www.physics.uiuc.edu/People/Faculty/profiles/Leggett/](http://www.physics.uiuc.edu/People/Faculty/profiles/Leggett/)). In 2004 Professor Leggett was awarded a knighthood (KBE) for "services to physics" in the Queen's Birthday Honours list. A detailed autobiography may be found at <http://nobelprize.org/physics/laureates/2003/leggett-autobio.html>.

of agreement is really quite satisfying. And this is not something you could have done analytically.

#### What are your views on interdisciplinary research?

I think that interdisciplinarity is a much oversold concept. Let me make clear what I mean by that. I don't think interdisciplinarity is a sort of 'sauce' that you pour on a whole lot of ingredients which somehow improves it. In a problem which requires the expertise of a number of disciplines interdisciplinarity is going to happen quite automatically. Take for example the field of high temperature superconductivity. From the moment these materials appeared on the scene, it was obvious you had to have physicists, chemists, material scientists etc., collaborating on this. And they did. No one had to come along and say: we'll give you 500 million dollars extra if you can demonstrate that you have interdisciplinarity. It just happened in the natural course of events. Just look at the problem: does it require an interdisciplinary approach? If so, go out and find people to work with you. Don't be into interdisciplinarity just for the sake of being interdisciplinary. It does not make any sense.



**There is an increasing number of physicists working outside the traditional areas of physics (biology, economics, and social sciences). How do you feel about this?**

Certainly it is a very worthwhile thing to try but- and I have seen this happen quite a bit in the field of biological physics- there is a great temptation, for example if you come from the field of statistical mechanics, to simply go out, look at an area of biology and try to find yourself a problem which can be done with, or seems to be attackable with the kind of techniques you learnt in statistical mechanics. You solve the problem, you publish several papers on it and then you find that your colleagues in biology are just not interested. It does not seem a very important problem. If you are actually anyone who is going to do this kind of thing it is very important that you talk to the people who are and have already been in the field for years and have some kind of feeling for what the really significant problems are. Of course, what you are liable to find is that these are not the kind of problems that you could attack with your traditional methods, but nevertheless, you may have a somewhat generic kind of background which enables you to look at them.

**Many people say that biology is the science of the future. Would you agree?**

I would say our understanding of the workings of the brain today is almost certainly no better, in fact it is probably worse, than was our understanding of matter at the atomic level in the late 19<sup>th</sup> century. I think it is entirely conceivable that the 21<sup>st</sup> century will bring the same kind of major revolution as we saw in physics in the 20<sup>th</sup> century in that area and certainly the challenge is at least as great. I may actually add one thing which will probably shock you: I actually think that it should not be totally disreputable for physicists to study what are called paranormal phenomena. In paranormal phenomena there are a variety of things from card guessing experiments to speculations about an after life. It is obvious that in many of these areas there is a lot of charlatanism, and a lot of self-deception, but that does not mean that there is nothing there. You have to remember that, say in the 15<sup>th</sup> century, there were lots and lots of things which we now think of as alchemy. Most of them were rubbish, but underneath it, there was a core of what we know as

chemistry. I wouldn't consider as totally unconceivable that some of these phenomena, which today we regard as a sort of fiction, completely paranormal, to turn out to have some real scientific basis. In particular, I would say those phenomena which we exclude not because of the 1<sup>st</sup> law of thermodynamics but because of the 2<sup>nd</sup> law. I think those are the ones which we probably need to look at harder. Returning to your original question, I think that what makes the problem of the brain, specifically of neuropsychiatry, almost unique is that it combines a huge and immediate human relevance with very challenging scientific importance.

**Will you tell us about your favourite popular science book?**

I think there are a couple of rather good books. There has been dozens of books coming out over the last ten years on the conceptual problems of quantum mechanics that range from very good to absolutely awful. There are two which I think are especially good and I would certainly recommend to anyone who wants to find out about this field. One is by Nick Herbert. It is quite old now, it's from 1986, it is called "Quantum reality". That is a very nice book. It pulls no punches, it is written at a level which ought to be accessible to anyone with basic arithmetic and not much more. A second book in the same general vein is Gerard Milburn's recent book which is called the "Feynman processor", about quantum computing. I think it is a rather good book though it does actually put quite tough demands on the reader. It is rather amusing because there is this famous statement which Stephen Hawking attributes to one of his editors: "that every equation you put in the book halves its sales". This is a joke that Gerard Milburn took extremely seriously and he didn't put a single equation in his book, but it does require the reader to understand that  $(-1)^0$  is plus one. I think this book is very good, very nice.

**If you were back in the 1960s finishing your Classics degree in Oxford, would you still take a second undergraduate degree in physics?**

Possibly, but I think, knowing what I do now, I would more likely take it in something like neuropsychiatry. I really do think that that is going to be the field at least in the 21<sup>st</sup> century which will be looked back on as really exciting. Physics is still regarded as very substantial, but it won't have quite the same degree of excitement. Again, neuropsychiatry clearly is much more useful in a sense that most of physics is not. ■

◀ Anthony J. Leggett receiving his Nobel Prize from His Majesty the King at the Stockholm Concert Hall.