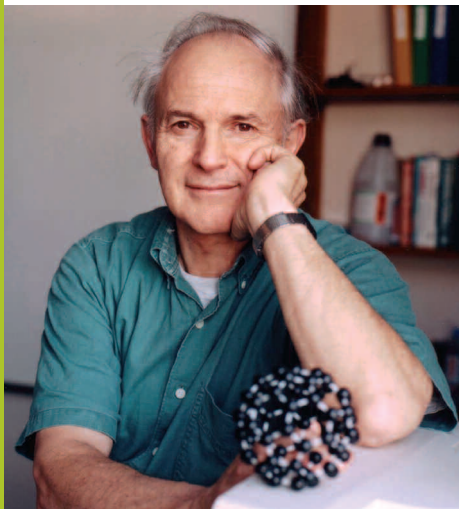


An Interview with Sir Harold Kroto

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Harold Kroto was born in 1939 in Wisbech, Cambridgeshire, and brought up in Bolton, Lancashire. In 1961, after getting a first class honours BSc degree in chemistry at the University of Sheffield, he took a PhD in molecular spectroscopy of free radicals produced by flash photolysis. He moved to Canada in 1964 where he spent two years at the National Research Council (Ottawa) doing post-doctoral research in electronic and microwave spectroscopy. During 1966 he studied liquid phase intermolecular interactions by laser Raman spectroscopy at the Bell Laboratories in the USA.

In 1967 he was offered a position at the University of Sussex where he became a Professor in 1985. He was elected a Fellow of the Royal Society in 1990, and a Royal Society Research Professor in 1991. In 1996 he was awarded a knighthood for his contributions to chemistry and, later on that year, his contribution to the discovery of C₆₀ Buckminsterfullerene, was recognized with the Nobel Prize in Chemistry. In 1995 he inaugurated the Vega Science Trust (www.vega.org.uk) to create science films of sufficiently high quality for television and Internet broadcast.

Professor Kroto is currently at Florida State University.

Professor Kroto's research interests are diverse and have covered several topics. In the '70s his work on the production and spectroscopic characterisation of unstable species and reaction intermediates with labile multiple bonds, led to the

creation of the first molecules with carbon-phosphorus double bonds. His strong interest in interstellar chemistry and his work on linear carbon chain species set the roots for the discovery of C₆₀. Presently, his research interest focuses mainly on fullerene chemistry and cluster science (Further information is available from <http://www.sussex.ac.uk/chemistry/profile1523.html>).

Besides his scientific accomplishments, Professor Harold Kroto has also received vast recognition for his work as a graphic designer.

A detailed autobiography may be found at <http://nobelprize.org/chemistry/laureates/1996/kroto-autobio.html>.

Introduction

In the winter of 2005 Professor Kroto visited Lisbon where he gave a public lecture "2010 a NanoSpace Odyssey" that closed a series of colloquia running in parallel with the exhibition "In Light of Einstein: 1905-2005" hosted by the Gulbenkian Foundation. This event, designed and developed under the leadership of Ana Maria Eiró and Carlos Matos Ferreira, involved the participation of a team of scientists from the University of Lisbon and from the Technical University of Lisbon. During the visit Prof Kroto was interviewed by Patrícia Faísca of the Centro de Física Teórica e Computacional da Universidade de Lisboa and by Sílvia Estácio of the Grupo de Física Matemática da Universidade de Lisboa. The interview that follows below was originally published in 'Gazeta de Física' which is the Bulletin of the Portuguese Physical Society.

When have you decided to become a scientist?

I don't think I ever decided. It just fell into place. I was interested in art and graphics, science, tennis and playing the guitar. I was not particularly good at playing the guitar, because I had learned too late, and I kept loosing in tennis. So, I was not going to be a Wimbledon champion. But I was good in art and graphics. Then I went to the University and I did science. This seemed to me a safer choice to start a career because it was important to ensure survival. Art became secondary although it was my

major interest. As I wanted to stay in the University - was having a good time - I ended up doing a PhD in chemistry. Then, since I wanted to live abroad, I went as a post-doc to Canada and I also lived in the USA for a while. I went back to England because I was offered a job there - I had to have a job - and I thought, "I will do it for five years and if it does not work I will go into art school and I will do graphics". It turned out that it went reasonably well and therefore I never got round into doing the thing I wanted to do, that is graphics. So, I never really made that decision "I want to be a scientist!" - I am not even sure that I want to be a scientist now. I want to be interested in what I am doing at a time. I did not even think that I wanted to be a professor. I wanted to earn a living and do everything I could as well as I could. My research was going reasonably well and I was happy with the idea of being a professor. It came a time when I actually thought of doing research in part-time to go into graphics, design and art. But that project "evaporated" when we discovered C₆₀. So, maybe I will start my career as a graphic artist in the next couple of years. I do not know. I have got so many other things on my plate. I never made any plans, things just happen, life just happens.

In your Nobel autobiography you say that at some stage you became interested in Chemistry, Physics and Mathematics exactly in this order. What attracted you most in Physics?

In physics I was fascinated by quantum mechanics and quantum theory. I think quantum mechanics is the greatest intellectual breakthrough of the 20th century since it deals with the microscopic world on which everything is based. In fact I am really a chemical physicist. There is chemistry and physics and on that borderline there is spectroscopy which is what I am still interested in. Physics allows one to understand atomic and molecular spectroscopy from a quantum mechanical basis. Although I am not a particularly good mathematician, I am good enough.... But I am not a particularly good physicist. I think that to be a really good physicist one needs to have a

stronger, more fundamental mathematical understanding than the one I have. But for chemistry and chemical physics it is enough to understand quantum mechanics just up to a certain level.

Was your background in Physics an important determinant for the discovery of C60?

No, I do not think it was. From my point of view, the discovery of C60 happened because I was interested in chemistry in space. I did some nice work in that area during the 70s because I got interested in the fact that space is full of molecules and atoms. The discovery of C60 happened in the follow-up of that work. I met Rick Smalley, who had developed a fantastic apparatus for vaporizing metals, when I was visiting Bob Curl at Rice University. I thought that Rick's apparatus would be able to vaporize carbon and should show us something interesting about molecules in space. It was not a very important experiment on my agenda. It had been sitting in the back burner for several years. While I was in Rice I thought we could do this little experiment and then this very big surprise came out.

The story of the discovery of C60 is beautiful. It sounds almost like a fairy tale. Can you recall those days in 1985? Exactly what happened?

There were two experiments planned. A very simple experiment which was particularly interesting to me and a more complex one which was as interesting to me as it was also to Bob Curl and Rick Smalley. This second experiment was very difficult and so we decided to do the simple experiment as a starter. I got a phone call to do it and I went over immediately to Rice. There I worked mainly with the students, in particular with Jim Heath, but also with Sean O'Brien, and Yuan Liu. After a couple of days, the experiments were going very well, but the results were showing a surprising and very striking feature. We can make an analogy between vaporizing something with throwing a whole load of cards. What was happening looked as if the cards were coming down in the same particular order every time. A load of carbon atoms is thrown up in space and, instead of linear chains of all different numbers just like I was expecting—there was something which was very strong, stronger than anything else, just around number 60. That was very surprising. Then we completely diverted ourselves to looking at that, scratching our heads about what it might be. And then, of course, we came

to the conclusion that it might be the soccer ball structure. That was a big surprise! We were totally elated by this discovery because, although it was not yet confirmed, we were all convinced that we were right. I remember being so much elated that on my return back to England, when I was seated in the airplane, I thought it would not have needed engines to fly me over the Atlantic. I remember thinking that some people would ask me "Did you have a good time?" and that I was going to answer, "Yeah, do you know what we discovered?" and I would tell the whole story.

Then we had a few headaches because half a dozen papers said we were wrong. It was very difficult at that time. I decided that I would spend 5 years trying to prove it correctly because if it was not correct I wanted to be the one to show it. I do not agree with scientists who put out a theory and let other people prove it one way or the other. I think that is unethical science. There are people who do that and I criticize people for doing that, particularly Fred Hoyle, who was a very famous British scientist, a cosmologist or physicist or whatever. He put out a lot of theories that were total nonsense.

Then, together with Rick's group, we did some studies which all fitted very nicely with what we had suggested and we gradually built up circumstantial evidence to indicate we were right.

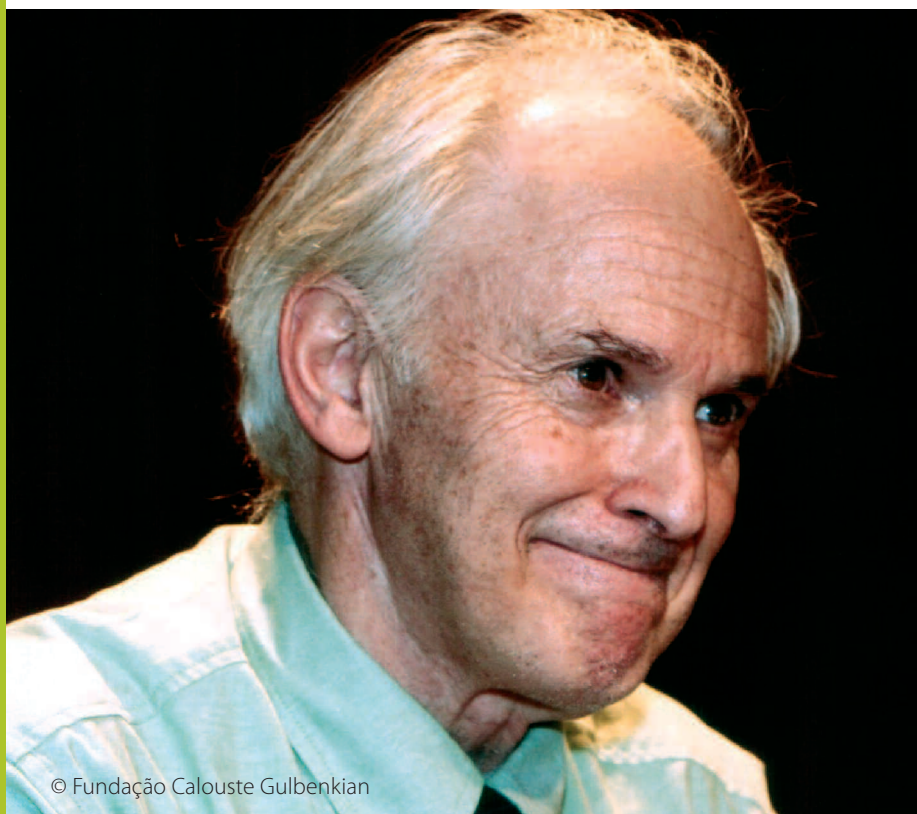
Would you agree that there is always a certain degree of serendipity in every major scientific discovery?

Yes, up to a certain point that is true. I would say that there are great discoveries that were not serendipity. There are people who try to build something and they know *a priori* that there is a strong possibility that their discovery might work. Let me give you an example. The discovery of the laser (or the maser itself) was not a serendipitous breakthrough because Charles Townes thought it could be possible by looking at Einstein's equations. I think that the serendipitous aspect in this case is not the discovery itself but the way in which it has revolutionized our world: CD players, communications, and eye surgery, just to mention a few. The way the laser revolutionized the technology and the world where we live was totally unforeseen. On the other hand, ours was a serendipitous discovery in the sense that we were not expecting to find the soccer ball. Scientific breakthroughs are not all of the same kind. I do not know enough about the discovery of the

transistor, that made computers possible, but Bardeen, Shockley and Brattain may have well thought of that ahead of time. I think one could say, however, that some major discoveries are not predicted. The latter are very important because they change the paradigm; they change our vision of the world. An example of a discovery of that kind is showing the existence of antiparticles by looking at Dirac's equations in Quantum Mechanics. I may not be right but this is the way I look at it. It is a bit like the square root of -1. If we use Pythagoras theorem we only take the positive root to this equation, despite there being two possible roots. Now, someone with the wonderful mind of Dirac would say "Hang on, let us not throw away the negative root. What does it mean?" And Dirac identifies it with a negative energy and concludes that there are antiparticles. I think Dirac's discovery can be used as an example of a serendipitous discovery. Serendipitous discoveries are probably the most important from a fundamental point of view in the sense that they change our understanding of things. The laser's discovery did not actually change our understanding of the world because it was predicted ...



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... by Einstein's equations. However, no one actually appreciated it until later, until Townes tried to build a maser. But Townes did not change the theory. The serendipitous breakthroughs change our fundamental understanding of things.

In terms of fundamental science how would you evaluate the impact of your discovery?

I think the chemistry of C60 is in its very early days but it has already changed our view of sheet materials. There are 1-dimensional, 2-dimensional and 3-dimensional crystals and graphite is effectively a 2-dimensional crystal. The discovery of C60 showed that on a small scale 2-dimensional sheets actually curve into cages, which is an important finding. I think the chemistry of C60 has only just started partly because science is so well developed that even big discoveries will take a long time to break into applications where things are already fully in place. Consider the example of silicon-based technology. Gallium arsenide (GaAs) could be an alternative to silicon (and possibly better than silicon) but it would never break through because there is so much research going into silicon which, for that very reason, is getting so much better, that for other materials to break in they would have to be better than silicon but without that much

research. The same reasoning applies to C60. C60 will break through in areas where the known technologies are not so well developed and so mature. I would guess that if C60 is to be a major contributor to useful devices it will be in molecular electronics, which has not really started, and where it has got very interesting properties. But billions have been spent on silicon research and unless molecular electronics can clearly show up as a better alternative it won't move forward. But it may do one day...

Do you feel that the discovery of C60 – possibly the most beautifully designed molecule in the Universe – was a reward for your passion for design?

I never thought of it that way. It's possible but I don't know... It's a nice thought!

Can you tell us what your views on interdisciplinary research are?

I know what multidisciplinary research is. I am a chemical physicist so I cover both chemistry and physics. I think the future is going to bring biologists, physicists and chemists together and that we will obtain great benefit by bringing the ideas from one area to another. When you get stuck in a rut, a person from another field may probably help you if you work together. If you know only slightly (less?)

about other fields than yours you can make breakthroughs because you need to be naïve in science. The older you get the more stupid you are because you accept more received wisdom and you shouldn't do that in science.

How do you feel about computational work?

Computational work has to be hand-in-hand with experiment. It shouldn't be open-ended. You should be racing with an experimental horse. If the computation is too far ahead of experimental confirmation, then my guess is that it is not so worthwhile. I worry about computations that are too far ahead of the experimental technology that surrounds them. That's my own personal opinion. Experiments are ultra-important. And I see many, many examples, in very simple cases, of how wrong you can be. The discovery of C60 is just one of them. There are a lot of theoreticians doing work on carbon and none of them predicted what C60 would be.

What advice would you give to a young person taking their first steps in scientific research?

I would say do it only if you're curious and passionate about it. Otherwise it's very hard although I know that other areas are difficult too. But if you are in the University you have to teach, you have to do research, you have to look after students, you have to do some administration, and you have to get research grants... It's not easy and I don't think it is any easier now than it was for me. I think I had a very tough start and I survived it. But I didn't know how tough it was because when you're young you're resilient. But don't go into research to win prizes. When kids ask me how to win a Nobel Prize I say I don't know. I certainly didn't go into research to win the Nobel Prize; it never had an important effect on me. It was only after the discovery of C60 that I thought there was a possible chance of getting the Nobel Prize. But I was very satisfied with the work I had done before that important discovery. I was satisfied with my chemistry-in-space work and with my laboratory work in various areas of phosphorous and sulfur chemistry. That was fine and if I had not done anything else I would have felt that those were worthwhile satisfying contributions. And after that I would have done anything else. Now, C60 is over for me and I'm doing something else. Winning an award is simply a bonus!

In 1995 you inaugurated the Vega Science Trust to create science films of sufficiently high quality for network television broadcast. Why?

It started just like everything else. I was giving a lecture in London, which I wanted to record, and I got a disastrous recording of it. So, I decided to contact a BBC producer who was interested in recording lectures like the one I'll give today. He told me that since this sort of productions was not a major planning area for the BBC it would cost us a certain amount of money to do a decent job. I got some funding and we did record it again. Then we did a second recording of a lecture by Bill Klemperer, a Professor from Harvard, who is a very good lecturer and someone I have a great admiration for. And then a third one turned up. It was then that we decided to set up a foundation, the VEGA SCIENCE TRUST, to do projects of that kind. In the meantime, my colleague, who had a position in the BBC, more-or-less disappeared due to a conflict of interests, and, to some extent, I was left holding the baby myself. He was very important at the beginning, for the first two or three programmes, but now we have more than 100 programmes. Moreover, the idea went on a bit further because the internet became a very important technology and now, many of our programmes that were originally showing on the BBC - and in fact over 50% of them still are - have gone on to our Web site and can be watched free worldwide. And now that is my major commitment. Television is finished as an educational or cultural thing. More than 90% of the material on TV is nonsense, terrible and not worth watching. For me the Internet is the future broadcaster because any individual can broadcast. You can make a program yourself and put it on in your website and any individual will be able to see it in any part of the world. That is a fundamental difference and it is fantastic!

Did you feel at that time, i.e. in 1995, that young people were beginning to lose interest in the hard sciences? In your opinion what may be causing such loss of interest?

Yes there is a problem with the hard sciences although there has been a little bit of a shift back in the UK. I think there are lots of reasons that explain that and one of them is the following. In the West life is just too easy and there is no quick way into modern technology. For example, years ago when the old phone did not

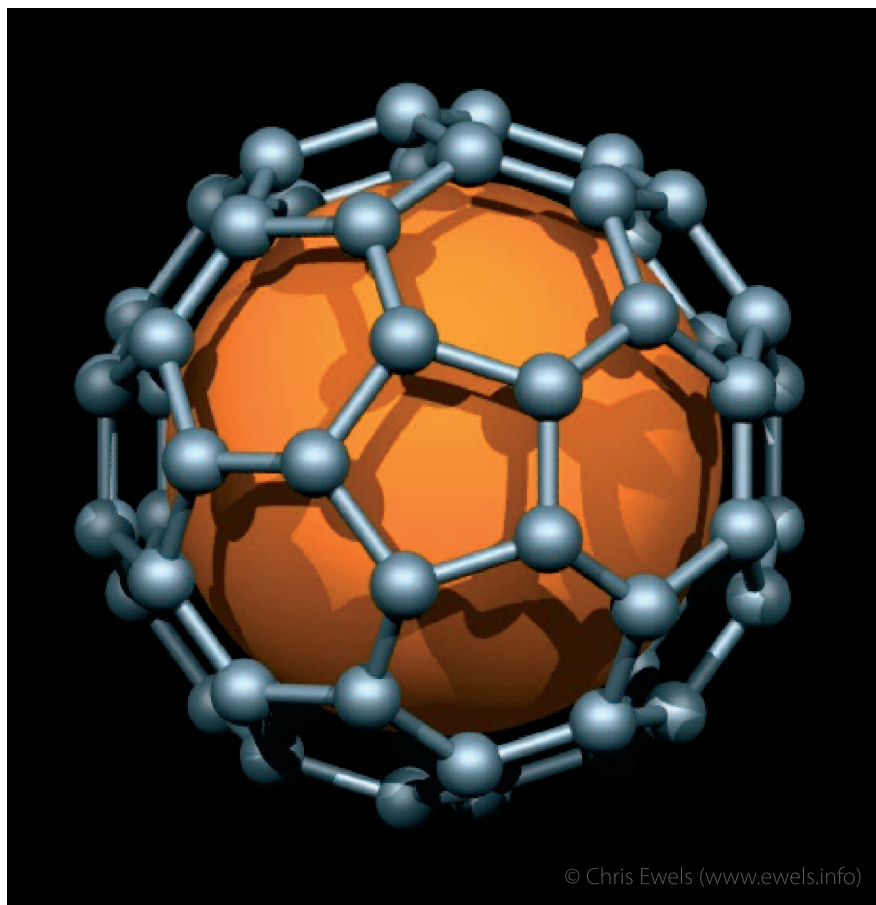
go you just fixed it and that is something you can't do nowadays with mobile phones. We have moved away from a repair-oriented technology to a throw away technology and that is, in my opinion, one of the most important barriers. I became a scientist by fixing things. In my father's factory I fixed the apparatus and because of that I learned how things worked. I made my own radio, and I will always remember switching it on and a voice coming out of this thing, it was incredible! It was just like magic! I think experiences of that kind just do not happen very often, not to enough kids. Somehow the modern child is not really made aware of just how fantastic modern technology is and what's more, if you have a mobile phone, you open it up and you don't know what the hell is going on in there. You don't develop, in general, a real respect for technology unless it breaks down and you manage to fix it yourself. That is something you can't do with modern technology. That's one reason. Of course there may be others, but I think that's a very important one.

Will you tell us about your favourite popular science book?

I think the most important popular science book, at least for me, is 'The Demon Haunted World' by Carl Sagan and I recommend everybody to read it, not just scientists. It's about the issues that concern me. The irrationality of the way things are going at the present time, mystical nonsense and things like that. I think the rational science community has to form an alliance with people that have grown up with mystical ideas. Otherwise they will be driven into the hands of the fundamentalists. Then, I suppose other books that I quite enjoy are some of Feynman's books,

If you were back in the 1960s would you still take an undergraduate degree in chemistry?

No, I think I would have probably gone into graphics and design. But it was a fantastic time! I may still have done it because I never thought it was the final decision. The problem is that I don't do anything unless I do it to the best of my ability. But nowadays there are many more options for young people and, what is more important, I would be less dependent on funding. Well, I don't know the answer! ■



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