An Interview with Ian N. Stewart

You started out as a group theorist in the early 70's, then worked on successively in Catastrophe Theory, Bifurcation Theory, Dynamical Systems and Chaos, and now are turning towards Mathematical Biology. It's a rather remarkable trajectory. How did it come about?

It all seemed very natural at the time. As an undergraduate, I generally preferred 'pure' mathematics to 'applied', and I particularly liked the crisp logic of algebra. I was at Cambridge, and took courses from Philip Hall, one of the world's leading group-theorists. So I decided that my PhD ought to be in group theory. In fact, it was in a closely related area, Lie algebras, because my supervisor Brian Hartley was interested in a possible connection between Lie algebras and abstract groups.

Then, in 1970, Christopher Zeeman gave the first lecture course ever on Catastrophe Theory. I went to it, and really liked it. The underlying mathematics is very algebraic. But in addition there were applications, such as optical caustics. So I started to move into more applied areas. It was then natural to spread out into singularity theory and bifurcation theory. In 1983-4 I spent a year at Houston working with Marty Golubitsky, and that had a major effect on my choice of research area. Continuing the move into dynamical systems made a lot of sense... I just followed where the maths itself pointed. The recent interest in mathematical biology mostly came about because there are nice applications of dynamics with symmetry to biology.

One point which impresses me particularly in your career as a research mathematician is that you always seem to be in a field where the exciting action is going on — riding the crest of the wave — and you leave a permanent imprint. Would you like to comment?

I don't set out to follow fashion. There are lots of fashionable areas that I've never wanted to work in! I think it's just that the kinds of things that appeal to me are the kinds of things that appeal to lots of other people. The wave kind of forms itself, and I get swept along with it. In addition, I really like to work in NEW areas, where you don't need a lot of background to do useful research.

You were a group theorist in the days of the Appel-Haken proof of the classification of simple groups in 1976. Can you give us a first-hand account of the reactions of the mathematical community to the first computer-assisted proof in history?

The main reaction, oddly enough, was disappointment. "Oh, it's THAT sort of theorem, is it?" One that needs massive calculations that don't yield much insight. Most of us didn't worry much about the use of the computer — it was the philosophers who thought that changed the nature of proof. Our feeling was that it just changed the technique a bit.

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Turning to Catastrophe Theory, there was after the classification theorem of René Thom and the groundbreaking work of Christopher Zeeman the idea, in some circles, that there it was at last, a mathematical theory for everything. Of course no mathematics can live up to these expectations. Do you think Catastrophe Theory was a victim of its own success?

I think that the popularization of the subject got mixed up with the technical aspects, and people got confused. No one seriously claimed it as a theory of everything. People did point out that it had very broad applications (which was and still is true). Then priority issues muddied the waters even more. The quality of criticism was very poor — lots of noise but very little genuine content, and a lot of confusion. In those days, mentions of maths in the media were rare, and academics didn't really understand how such things went. The subject was inherently interesting, the media picked up on that — it wasn't "hype", deliberate attempts to exaggerate. You can't hype maths, people don't take any notice.

As a first-hand actor in the explosive development of Dynamical Systems in the past 30 years, how do you see its role either with respect to the rest of Mathematics as well as with respect to the other sciences?

It occupies a very central position in the link between abstract maths and applied science. The world is nonlinear, but until dynamical systems got going (thanks to Steve Smale and various others) virtually all applied maths was linear. People were tackling 20th century problems with 18th century methods. Now we have chaos, fractals, cellular automata... and powerful computers that make it possible to do the calculations for real systems. A few mathematicians still complain that chaos and fractals have never achieved anything useful... if they bothered to read NATURE and SCIENCE they'd realise how untenable that position really is.

Some mathematicians think (and say) that the future of mathematics is in Biology. What is your own view?

I think that one of the most exciting prospects for the next century is the interaction of maths with REAL, hard-core biology. The kinds of question that biology raises will need some very new ways of thinking, they will stimulate genuinely novel maths. Knowing an animal's DNA sequence is all very well, but that doesn't tell you very much about the animal unless you understand the processes that DNA controls. Well, more like a conductor controls an orchestra, which is to say, not very directly. DNA "orchestrates". I'm very encouraged by recent work of people like Enrico Coen and Hans Meinhardt, who are forging strong links between the general maths of pattern formation and the crucial role of genes.

You are connected with the Clay Institute, which has instituted biggest-ever awards in Mathematics — the "Millenium problems", sometimes called "Million-dollar problems". Which one do you feel will be the first to crack? The last? And for how long will they stand?

The obvious answer is the Poincaré Conjecture, which may well have cracked already, thanks to Grisha Perelman. I am inclined to think that the P=NP? problem may be the last to go. Timescale: could be a hundred years. The existence problem for solutions of the Navier-Stokes equations could be just as hard (and I suspect the answer is "no").

Do you agree with those who say that the Riemann Hypothesis will stand for another century? And what is the current feeling about G. Perelman's recent work which, it is said, may have solved Thurston's geometrization programme, carrying Poincaré's conjecture along the way?

You could have said much the same about Fermat's Last Theorem, 20 years ago. I have a sneaking feeling that the Riemann Hypothesis won't last another 20 years. There is a growing belief that Perelman's ideas may indeed have proved the geometrization conjecture, which trivially proves the Poincaré Conjecture. Several prominent mathematicians have been saying as much in print, especially in the Notices of the AMS. Certainly everyone thinks that he has made major progress.

You have had lots of Portuguese Ph.D. students. In fact, over here we half-jokingly call it "Ian's Portugal connection", and some of us organized a conference in 2000 in your and Marty Golubitsky's honour. How did this "Portugal connection" develop?

Isabel Labouriau. She came to Warwick from Brazil and wanted to do a PhD in mathematical biology. I was almost the only person on the staff at that time, and I agreed to act as supervisor. She was working through a paper by Rinzel and Miller - numerics of Hopf bifurcation in the Hodgkin-Huxley nerve impulse equations. Then Marty G visited and talked about his recent work with Bill Langford on degenerate Hopf bifurcation, and the Rinzel-Miller results looked just like one of their pictures. So Isabel got the task of finding out why.

Then she moved to Porto, and after a few years started sending her students over to Warwick. It kind of grew from that. I am now supervising mathematical "great grand-daughters" with Isabel as the first daughter.

How many Portuguese students have you had? And how do you feel they compare scientifically with the universe of British postgraduate students?

It's 6, not counting Isabel. Plus a couple of Brazilians, which I tend to think of as honorary Portuguese. In total, about one third of my PhD students. They are entirely the equals of the British ones (and I've been fortunate to have some extremely good students). And it's very encouraging that Portugal produces so many top-quality women mathematicians.

What do you feel was the most exhilarating moment in your career as a research mathematician? Can you describe it to us? And the funniest one (not necessarily involving yourself)?

Most exhilarating — I think it was when a chance remark in a book review led to a long-term collaboration on animal locomotion. I was reviewing a book about connections between biology and engineering, and there was a paper on patterns in animal movement. They reminded me of symmetric Hopf bifurcation patterns, and I said something like "does anyone want to fund an electronic cat?". Next day, Jim Collins phoned from Oxford, and said: "I can't fund an electronic cat, but I know people who can." And that began a major collaboration and turned my attention more towards biology.

The other exhilarating experience was the year in Houston with Marty Golubitsky, 1983-4. We've been good friends and close collaborators ever since. It's fantastic to have someone who understands the maths in the same way, but can complement your own ideas.

Funniest? Around 1990 I went to a conference at Abisko in Lapland, next to a huge frozen lake, solid enough to run a car over, and my wife and I were persuaded to go cross-country skiing. We'd never done any skiing before, so when the Swedish organizers took us 10 km away from the Research Station and left us to ski home, we decided to ski on the lake. There was one Lapp fisherman, fishing for his dinner through a hole in the ice. We spent the morning practising skiing on the lake ice, falling over when we hit infinitesimal bumps. By lunch time there were about a dozen Lapp fishermen, all staring at us as if we were stark staring mad. In retrospect, this was hilarious.

Besides an outstanding mathematician, you are a brilliant and foremost popularizer of Mathematics. Good popularization is very difficult and requires a lot of hard work. Why do you think it important enough to dedicate a significant amount of your time and effort to it?

Well, nowadays I get paid pretty well for it, so that's a bonus. But I didn't to begin with, for years, and it never bothered me. I've always enjoyed writing, I like to write about things I understand, and it just seemed a natural thing to do.

Some hard-line mathematicians regard popularization, at best, as a meaningless waste of valuable time which could and should be put into serious things like research, implying in particular that popularization of science is not a serious activity. What is your comment?

I was never greatly bothered whether anyone else approved of it or not. It was a kind of hobby. I thought it was worth doing. Nowadays, most scientists have realised that it's important to engage with the public. I've had a lot of support from colleagues, and very little criticism. It helps, though, that I still do a full research job. My popularization activities don't damage my research.

What would your answer be if you were told by another mathematician, as I have, that "popularization is worth nothing"?

Ignorant rubbish.

How many popularization books (if you can still track them) have you authored? In which languages are they translated? What is the best one, in your opinion, and does it coincide with the best-selling?

I've written about 70-80 different books, of which about 25-30 are popularizations. Between them they've been translated into at least 19 languages — Portuguese, Spanish, French, German, Italian, Dutch, Japanese, Chinese, Swedish, Norwegian, Danish, Indonesian, Russian, Romanian, Polish, Korean, Persian, Hungarian, Estonian, Greek, Croatian, Chekoslovakian...

Best one? I think they're all good! In some ways my favourite is Fearful Symmetry (written with Marty). Plus Flatterland, a modern sequel to Edwin Abbott's Flatland—but you either love that book or hate it. The best-selling ones are The Science of Discworld I and II, written with my friend Jack Cohen and Britain's best-selling fantasy writer Terry Pratchett. They both spent weeks in the Sunday Times top 10 bestseller list for nonfiction. Mostly thanks to Terry. The best-selling mathematical one is Does God Play Dice?

With which non-mathematical journals have you collaborated? And which has been the most pleasurable collaboration for you?

Oh, lord... which one's HAVEN'T I written for? I've written for Scientific American, New Scientist, Pour La Science, Times Literary Supplement, Analog science fiction magazine, The Guardian, The Scientist, Prometeo, The Economist, The Times, Daily Telegraph, The New York Review of Books, London Review of Books, Discover , Brand Strategy , The Lancet, Prospect , El Pais, Newton...

The most fun was probably with Pour La Science – Phillippe Boulanger, the editor, asked me to write a monthly mathematical games column, a successor to Martin Gardner's column in Scientific American. Eventually I ended up writing it for Scientific American too.

You even have time for other lives. I remember, when I was your student, that you were proud of having received a prize for a Science Fiction book (if I'm not mistaken, you had been nominated "Earth's ambassador to the other galaxies"). And you write for example about "The science of Discworld", of Terry Pratchett. Are there no limits to your imagination?

I'm just interested in lots of things. And I write fast. Jack Cohen and I wrote a science fiction novel Wheelers a few years ago. A sequel, Heaven, will be published in May. We've already planned a third book in that series.

How do you manage to do all this? I am assuming that your days have 24 hours, but please correct me if I'm wrong (as you have done in the past).

(a) I write fast. (b) My position at Warwick University is now half-time research and half-time Public Understanding of Science. So I save time by not having lots of courses to teach.

Thank you very much for your most valuable time!

Interview by Jorge Buescu - Department of Mathematics, IST, Lisbon

Ian Stewart was born in 1945, educated at Cambridge (BA and MA in Mathematics) and Warwick (PhD). He was awarded an honorary DSc by the University of Westminster in 1998, and an honorary DSc by the University of Louvain in 2000, and an honorary DSc by the University of Kingston in 2003. He is Professor of Mathematics at Warwick University and Director of the Mathematics Awareness Centre (MAC@W). He has held visiting positions in Germany, New Zealand, and the USA, and is a regular research visitor at the University of Houston, the Institute of Mathematics and Its Applications in Minneapolis, and the Santa Fe Institute. He is an adjunct professor at Houston.

Among the general public he is best known for his popular science writing on mathematical themes. In 1995 he was awarded the Royal Society's Michael Faraday Medal for furthering the public understanding of science. His book *Nature's Numbers* was shortlisted for the 1996 Rhone-Poulenc Prize for Science Books. He delivered the 1997 Royal Institution Christmas Lectures on BBC television and repeated them for NHK in Japan in 1998. He is winner of the 1999 Communications Award of the Joint Policy Board for Mathematics, and he was awarded the 2000 Gold Medal of the UK's Institute for Mathematics and Its Applications. His joint book *The Science of Discworld* was nominated for a Hugo award at the 2000 World science fiction convention. Jointly with M. Golubitsky he won the 2001 Balaguer Prize for a mathematical monograph based on the author's own research, awarded by the Institut d'Estudis Catalans, Barcelona. He was elected a Fellow of the Royal Society in 2001, and won the Public Understanding of Science and Technology Award of the American Association for the Advancement of Science in 2002.