method is helpful. But even the staunchest critics of "axiomatizing" perhaps won't argue that it affected the way in which they present their own work.

The deep and powerful "odd order theorem" of W. Feit and J. Thompson and the whole classification project of finite simple groups (a collective effort of a group of first rate mathematicians).

Efim Zelmanov was awarded a Fields Medal at the Zurich ICM, 1994, for his solution of the Restricted Burnside Problem. He is currently Full Professor at Yale University.

## An Interview with David Chillingworth

*Your father was a mathematician. In fact the author of a very successful textbook on complex variable theory (*H. R. Chillingworth*, Complex Variables, Pergamon Press, 1973). Was that influential in your becoming a mathematician?*

Certainly. My father loved mathematics and, as he was unable to stay on at university after an M.Sc. degree, he went back to research in his later career as a lecturer in a college for teacher training and gained a Ph.D. at that stage. There was always mathematics around at home, on the backs of envelopes, margins of newspapers and so on. Nevertheless, I might easily have taken a different path when the time came to make key decisions about subject choices: perhaps it was ultimately through conservatism that I stayed with mathematics.

*You went up to Cambridge as an undergraduate. What was it like to be an undergraduate in Cambridge in the 60's? Who were the big names at that time? Did you happen to have some interaction with them?*

Undergraduates in mathematics may not be aware of who are the big names. I attended lectures on complex variables by Harold Davenport and on differential equations by Mary Cartwright, among others. I could have done but did not attend lectures by Paul Dirac, because (foolishly) I thought I wasn't interested in Quantum Theory which was 'applied' and therefore – following the unfortunate precept of G.H. Hardy – not as respectable as 'pure' mathematics. Of course now I wish I had heard Dirac in person. My Director of Studies was Frank Smithies, still at Cambridge and involved in mathematics.

It is amazing to realize now how few text books there were at that time. For one lecture course I attended on Analysis the recommended texts were by Goursat and de la Vallée Poussin, and there seemed to be only one book on Linear Algebra (Mirsky). It is rather different now!

Cambridge in the 60's was still fairly traditional. Academic gowns were (officially) to be worn when attending lectures, meeting tutors, and walking in the street after dark – as well as when dining. There were no mixed undergraduate colleges and few women. College gates were locked at night, so climbing in was fairly common: those whose rooms lay on popular routes were frequently disturbed.

David Chillingworth

*After graduating you stayed on to do postgraduate work. I think that W. B. R. Lickorish was your research supervisor. What was the subject of your thesis?*

I was offered the chance to move to the new University of Warwick as one of the first batch of research students, but lacked the pioneering courage and decided to stay in Cambridge. In my final undergraduate year I had been fascinated to read a short section from Hilton & Wylie: *Homology Theory* dealing with integration along paths and cohomology theory (de Rham cohomology); until then I had no idea that algebraic topology had any connection with calculus. Therefore I gave the proposed title for a thesis as Applications of Topology to the Solution of Differential Equations. In fact I worked on a different topic: curves on surfaces, and homeomorphisms of surfaces. At the time it seemed rather a backwater (that was before Thurston's ideas burst upon the scene!), but at least something I could understand, as it was low-dimensional and I did not have to grapple with exotic spheres, the Hauptvermutung, the s-cobordism theorem and other (to me) alarming monsters in the jungle of high-dimensional topology.

Part of my work was an extension of results by Raymond Lickorish, whom I found to be a very congenial and encouraging research supervisor. Life as a mathematics research student is a strange experience, as you have a very unstructured programme in which you devote almost all your energies to doing something that almost nobody is interested in ... but Ray's good humour and humanity were important in keeping me on track.

Another strand of my research arose by good fortune. One of the other research students Les Harris happened to come across a paper on *winding numbers* for curves on surfaces which he showed to me as he knew I liked that sort of thing. It was from trying to understand that paper that I was able to build a theory of winding numbers that gave a partial solution to a problem Raymond Lickorish had suggested to me, namely how to decide when an element of the fundamental group of a surface is represented by a simple closed curve. For that I owe a debt of gratitude to Les, and never forget how important it is to talk to others and to keep eyes and ears open for useful ideas.

*In the second half of the 60's you moved to Warwick, then a very new university. You were then exposed to new ideas in other mathematical areas (Dynamical Systems, Catastrophe Theory . . . ) and also actively involved in them. For instance, you edited the proceedings of a year-long symposium on Dynamical Systems (volume 206,* Lecture Notes in Mathematics*). For a young mathematician it must have been an exciting place and an exciting time. Can you give us an idea of the Warwick scene at that time?*

My first post was as a 1-year Temporary Assistant Lecturer at Warwick, during which time I was finishing off my Ph.D. thesis. At that period Christopher Zeeman, who founded the Mathematics Department and Research Centre at Warwick, had moved away from 'pure' topology and become increasingly interested in dynamical systems – which was, after all, the motivation for the creation of much of the machinery of topology by Henri Poincaré in the first place. There was a big meeting at Berkeley in 1968 in which the pioneering work of Stephen Smale and his students and others in dynamical systems played a major role, and in 1968/69 Zeeman organized a year-long Symposium on Differential Equations and Dynamical Systems at Warwick in which many of the Berkeley people took part. In associaton with this there were a number of 3-year postdoctoral positions, one of which I was fortunate to hold: editing the Symposium Proceedings was an excellent apprenticeship in the subject.

My own interest in the area had been sparked by a visit by Zeeman to Cambridge when I was a research student. He had come to give a seminar talk in topology (I forget the topic), and at tea afterwards he was enthusiastically showing how the phase space for the spherical pendulum decomposes into pieces of different topological types at different energy levels, and so to understand this mechanical system it is necessary to understand some specific geometry and topology. To me this was a revelation: for the first time I realized that sophisticated tools from 'pure' mathematics were needed in even such an 'applied' problem as the motion of a pendulum. Suddenly the abstract world of topology in which I had been living and working for three years seemed to make contact with reality! So the wheel turned full circle and I ended up working in the applications of topology to differential equations after all.

It was certainly a very exciting time, with a wide range of famous visitors not only from the USA but from Japan and Eastern Europe: I had the pleasure to meet Urabe, Halanay, Kurzweil and others. (However, I do not recall any from the Soviet Union.)

The following year Zeeman and several of the Symposium participants moved to IHES, Paris, and I was lucky enough to be able to spend a year there too: it would be difficult to imagine a better way to learn the subject than this total immersion. Of course Catastrophe Theory was flourishing at that time: David Fowler was working on the English translation of René Thom's *Stabilité Structurelle et Morphogénèse*, and Zeeman was inventing his catastrophe machine. Also Floris Takens was there, developing ideas with David Ruelle on dynamical systems models for turbulence. Charles Pugh was busy constructing a chicken-wire and plaster series of models to illustrate Smale's famous theorem on everting a 2-sphere in 3-space, and Alexandre Grothendieck was renouncing mathematics and circulating tracts about the survival of the human race. That was a stimulating year.

*After Warwick you got a position at the University of Southampton where you have been since 1971. How has your mathematical career developed? How have your research interests varied over the years?*

Although I try to keep in touch with the main directions of research in dynamical systems, my own work has been mainly in the application of differential topology in general and singularity theory in particular to problems in bifurcation theory. Recently I have been increasingly interested in the role of *symmetry* in dynamics and bifurcation theory, and I have tried to relate some of the current thinking on detection of symmetry in chaotic dynamics to fundamental geometric ideas developed by Stewart Robertson and students here at Southampton. In a different direction, I have formulated a general description of phase-space geometry for a simple impact oscillator (say a forced linear oscillator with one degree of freedom and and obstacle at a fixed position); this, too, involves singularity theory.

*As a former Southampton research student I know that you are a very good lecturer and made intense use of some written notes of yours and of your book* Differential Topology with a View to Applications *(Pitman Research Notes in Mathematics, 9). Do you have plans for some more work in this area?*

Thank you for the kind remarks. The book you mention is now 20 years out of date: it was written before the Lorenz equations, the Mandelbrot set and other now famous dynamical examples had been discovered or come to public notice. I would like to write an updated version, but currently the pressure to publish research papers puts it at the back of the queue. Maybe when I retire ...

*We will finish on a lighter note. As it happens with many mathematicians music is one of your great interests outside mathematics. Do you still play the guitar?*

I have to say no, although I do take it out of its case every now and then. However, I still belong to the Southampton Classical Guitar Society after 25 years, and enjoy recitals of guitar or other plucked stringed instruments – including the Indian sitar. Now there's a project for retirement ....

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David Chillingworth was born near Manchester, England in 1943 and was brought up in London. He graduated from Cambridge University in 1964 and stayed on to do postgraduate work. His Ph.D. dissertation was in low-dimensional topology (homeomorphisms of surfaces).

He has held academic posts at the universities of Warwick and Southampton where he has been since 1971. He has given mathematical talks in numerous countries and spent several months at universities in Europe and USA, including visits to research institutes such as I.H.E.S.–Paris and IMA – Minneapolis.

These days he is attracted to applications of differential topology, particularly using singularity theory to study bifurcations of differential equations. He is the author of a very successful textbook, "Differential Topology with a View to Applications", published by Pitman.