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A Conversation with Henry Daniels

Peter Whittle

Abstract. Henry Ellis Daniels was born in London on the second of October, 1912, but his family soon moved to Edinburgh. It was at the University of Edinburgh that he graduated; then he went on to continue studies at the University of Cambridge. He later took an external Ph.D. from the University of Edinburgh, but his periods of employment, 1935–42 and 1945–46, as a statistician at the Wool Industries Research Association, Leeds, provided him with what was probably the most formative experience of his career. During the intervening period, 1942–45, he was Scientific Officer at the Ministry of Aircraft Production, working particularly on position finding.

He returned to Cambridge in 1947 as a Lecturer in Mathematics; the Statistical Laboratory came into being during his time there. In 1957 Henry became the first Professor of Mathematical Statistics at the University of Birmingham, in which post he continued until his retirement in 1978. Since retirement he has settled in Cambridge, where he has an honoured place in the Statistical Laboratory.

Professor Daniels was President of the Royal Statistical Society for 1974–75 and was awarded the Guy Medal of the Society, in silver in 1947 and in gold in 1984. He was elected a Fellow of the Royal Society of London in 1980.

He probably values equally the honour of being created a Liveryman of the Royal Company of Clockmakers in 1984: a recognition of his contribution to watch design. His principal hobbies are indeed the repair of watches and the playing of chamber music on the English concertina.

The following conversation took place in his office at the Statistical Laboratory in Cambridge.

EARLY DAYS

Whittle: Something biographical to begin with, Henry: I believe you were born in London?

Daniels: In London, yes.

Whittle: But then moved quite early to Edinburgh?

Daniels: Yes, when I was perhaps two or three years old. To avoid the Zeppelins in the First World War, I understand.

Whittle: Really, that serious?

Daniels: Yes.

Whittle: You had your schooling in Edinburgh?

Daniels: Yes, first at Sciennes School, which is one of those excellent Scottish elementary schools. Then to George Heriot's School, Edinburgh, another very good school.

Whittle: I believe that you developed some interest in probability and such things already at this stage. Didn't you win a school prize?

Daniels: Well, yes, in a way, I suppose. I am basically an applied mathematician at heart. I became interested in probability because we did things like combinations and permutations. Not statistics as we know it, but related in a sense.

Whittle: So you enjoyed combinatorics?

Daniels: Well, I was never much good at them. You know that some people have a flair for them, and I don't think I had. But I rather liked the idea of probability itself, though I didn't quite understand what it was. I was also intrigued by the idea of experimental error. This arose from my attempts at doing experiments in practical classes, one of which I described in my R.S.S. Presidential Address. A classmate of mine called Harry Cowan, who was keen on biology, mentioned Fisher's book to me, and I tracked it down in the local library.

Whittle: He must have been knowledgeable for a schoolboy.

Daniels: Yes. He was actually a nephew of Hyman

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FIG. 1. *Henry Daniels, 1985.*

Levy of Imperial College, London, who was one of the earliest teachers of statistics to mathematicians. The Fisher text was an early edition; it certainly introduced me to the possibility of using such methods in science. Clearly, my main interest is in helping scientists, because I am in a sense a scientist myself. A secret physicist, if you like!

EDINBURGH AND CAMBRIDGE

Whittle: That's a point I would like to return to. And then you moved on: you did your undergraduate studies at Edinburgh?

Daniels: That's right; at Edinburgh University.

Whittle: Who was there then? Was Aitken on the staff?

Daniels: Yes, but we didn't have any lectures from Aitken, because he was teaching actuarial methods, which was something quite distinct. I got to know him very well later, but we really did not have much contact with him at that time. E. T. Whittaker was the professor. There is a book by Whittaker and Robinson which we had used at school, where we had done a certain amount of numerical analysis. (That was the school prize you mentioned.)

Whittle: You mean the *Calculus of Observations?*

Daniels: Yes, that's right, and a very good book it is. Well worth looking at even now, because it has a great deal about the history of least squares which became forgotten and has later been revived. It also has an account of the work of Whittaker and Aitken on penalised least squares in the twenties which predates the current interest in such things by about fifty years. However, Whittaker did not lecture on that material. He was a superb lecturer, giving courses on things like relativity and analysis. He was essentially an applied mathematician.

Whittle: You attended those lectures?

Daniels: Yes, I had to. Before Whittaker became professor at Edinburgh he had been Astronomer Royal for Ireland, so he had a very practical interest in numerical mathematics, too. That was essentially why he wrote the book with Robinson.

Whittle: Then you moved to Cambridge after graduating at Edinburgh; is that right?

Daniels: Well, Whittaker had this extraordinary idea that in your second year at Edinburgh you sat for the Cambridge Open Scholarship Examination. If you won a scholarship then, instead of going on directly to a Ph.D., you had to take the Mathematical Tripos course again from the beginning. Not quite from the beginning, because they allowed you a year off on the assumption that you had picked up a certain amount after four years at Edinburgh. But his idea was that you had not attained full manhood until you had done the Cambridge Tripos. But it was at Cambridge that I attended Wishart's lectures, and I found, you know, that although the lectures themselves were not very well given, the material was very interesting—especially the practical material. I found it fascinating that you could actually take observations and extract a lot of information from them using these statistical techniques. I was very interested.

Whittle: Was Wishart lecturing to the mathematicians?

Daniels: Well, he was lecturing mainly to the agricultural students. But he did a course for mathematicians, which I attended. He was himself in the Faculty of Agriculture.

Whittle: But you found the course interesting?

Daniels: I found the subject interesting—in a sense, in spite of his lectures. They were quite good, but it was the subject that interested me. I also went to Eddington's course on the combination of observations, which again was intriguing, and a little bizarre in some respects.

There was also Jeffreys' course. As everybody knows, Jeffreys was a little hard to understand when he lectured, partly because of his north of England accent and partly because he had a slight stammer. You really gave up after about three lectures. Of course, he writes beautifully.

Whittle: But later you came on closer terms with him?

Daniels: Yes, that was after I left Cambridge. Because I was interested in asymptotics.

Whittle: Bartlett was contemporary with you to some extent, wasn't he?

Daniels: Well, he was about a year ahead of me, but we did not actually make any contact when I was an undergraduate. But I did know Cochran at that time.

All Scotsmen and pseudo-Scotsmen like myself belonged to a club which really existed to put on Burns Night suppers and St. Andrew's Night dinners and the like. Cochran used to make speeches which were extremely witty, and I got to know him well. He was proving Cochran's Theorem at the time, although he did not call it that, using his background of algebra. He had acquired this in Glasgow, because we didn't do much algebra in Cambridge.

Whittle: Why?

Daniels: It was not considered quite respectable as mathematics, compared with analysis. But of course he was a product of the Glasgow school. I have forgotten who was teaching there, but it was a well-known centre for things like linear algebra.

WIRA AND WAR

Whittle: You didn't think of staying in Cambridge, Henry, after graduating there?

Daniels: Well, I did. In fact, I had registered for a Ph.D. with John Wishart. Then I woke up one morning and thought that after six years, four in Edinburgh and two in Cambridge, I really couldn't stand any more of this. So, I decided to look for a job, and was extremely lucky. I managed to get a job at the Wool Industries Research Association (WIRA). It was very interesting, because WIRA was directed by B. H. Wilsdon, who was a schoolfriend of Egon Pearson. He was all fired with enthusiasm for the new statistical methods just then beginning to be applied in industry, and he was very keen that someone should come to WIRA to apply them. So he took me on, and it was for the analysis of the variability of industrial processes that I developed these methods.

The interesting thing is that, whereas at Cambridge, and particularly in agriculture, one was concerned to make the best of the inevitably small samples available, in an industrial situation like the wool industry you essentially had an infinite number of observations. You could take as many as you liked. So, things like significance tests were not entirely relevant, or even particularly relevant. What you really wanted to do was to take the data and analyse the variation in it. In fact, nowadays we have the same situation in a sense, because again we now have so much data from these satellites that it has become more a matter of marshalling the data and analysing it.

Whittle: What they now call "data processing"?

Daniels: If you like. But the whole idea behind what we were doing was to try and pinpoint the sources of variation in a particular industrial process – in this case, the "card," a machine that figured in one of the processes for producing woollen yarn. You would assign variances to the various source of variation and then put them in order of magnitude. And, because they are variances, you get far more benefit by reducing the biggest than by playing about with any of the others. Having eliminated the biggest, you could then proceed to look at the next biggest and so on; that was the benefit of this method.

Whittle: It was at this stage that you wrote on components of variance, wasn't it?

Daniels: That's right.

Whittle: I have the impression that your interest at WIRA was not purely statistical. You mentioned the carding machine; you have a general interest in mechanics and gadgetry, don't you?

Daniels: It just so happened that WIRA suited my interest, because I am rather interested in applied physics and the like. At one stage I was running a fibre measurement lab, which gave me opportunity to indulge my interest by inventing apparatus for it. So I was doing various things of an experimental nature. In fact, around that time, the standard method of measuring the length of wool fibres was one I had developed.

WIRA really was very good for me. It was a small organisation; we all interfered in each others' business in a sense. There was no kind of exclusion between different parts, and so there was a very lively scientific atmosphere. At that time, in fact, Martin and Synge were developing partition chromatography, which won them a Nobel Prize. That raised some interesting diffusion problems, which I worked on with them. If you look at Martin and Synge's paper, you will find that they acknowledge my help there. And this is just because it was the kind of place where you all worked together, you see. The director was very good in this respect, although he was slightly crazy in many others. If you listened to his crazy ideas – if you could put up with them – he would put up with yours. And that is exactly what you want in a research establishment. Unfortunately, they become too big and too organised, and you lose a lot when that happens.

Whittle: Yes, it obviously suited you. You went there in 1935, is that right?

Daniels: In 1935, yes.

Whittle: And continued until 1942, when the exigencies of war took you . . .

Daniels: To the Air Ministry. Or rather, to the Ministry of Aircraft Production, which was almost synonymous with regard to what I was doing. I was in a unit called "Air Warfare Analysis." This was run by L. B. C. Cunningham, who invented an extraordinary theory of combat, for which he has never really been given much credit. But what he did was quite amazing. I am not sure whether it was ever written up for open publication. The unit was one of those little groups which were interested in the mathematical problems arising out of warfare and things of that sort. My particular interest was in the devising of methods for assessing the accuracy of radar in some of the new systems which were coming in at that time. Like the so-called "G," which was a hyperbolic system. Then there were systems like H2S for high-precision bombing radar. This raised many nonstandard statistical problems. Later, I wrote a paper on the theory of position-finding in the *Journal of the Royal Statistical Society.* That sums up the kind of thing I was doing, or mostly doing. The problems also raised quite a number of interesting philosophical points, as to whether one should be a Bayesian or not. That is discussed quite a bit in the paper.

Whittle: That is the nearest you came to being a Bayesian, right?

Daniels: Only just. Yes, I shied away afterwards.

Whittle: But it is true that, when there are several solutions for your possible position, you have to take some kind of a stand as to which is the most likely.

Daniels: Well, you see, I have this view that if you have a small sample problem, a really small sample problem, then you just have to be a Bayesian, because you don't have enough information otherwise. But most problems are not entirely small sample problems; they are mostly medium size problems. And then, you know, you are not so heavily dependent on being a Bayesian. I tend to lean to the attitude that one should use likelihood or something like that to summarise the information from the experiment. And then let somebody else add in the prior information. It's not my responsibility. That's my present view, anyway.

Whittle: I think it was about this time that you became interested in spectral analysis.

Daniels: Oh, that's all part of what I had to do during the war. I learned quite a lot about time series then.

Whittle: Then, after the war, back to the Wool Industries Research Association?

Daniels: Yes, I went back there.

Whittle: Much as before?

Daniels: No. Sort of, but not quite, because I had an assistant who unfortunately was killed in an accident, so I needed a new one. We advertised, and were extremely lucky in that David Cox applied, because he had been interested in a paper of mine on bundles of threads. This happened to be exactly what he had been concerned with in his job at the Royal Aircraft Establishment. So, that clicked. And there was obviously no competition with other applicants. He was much better than I was in certain respects. And so we took him on, and that was a very happy time. But I was only there for another year or so, and then I went to Cambridge.

Whittle: It is interesting that the period at WIRA left a considerable mark both on your own work and on David Cox's. You both refer back to problems from that time.

Daniels: Well, you'll find this with anyone who has been in textiles – that you never forget it; it becomes part of your system. It's a kind of one-dimensional physics. And I think that, as I remarked to David Cox, almost every interesting statistical problem has a textile version. There is no doubt of that. Particularly point processes, which are beautifully exemplified in the whole of textile production. And David Cox really admits this: that almost everything encountered in point processes will have been encountered in textiles.

CAMBRIDGE AND THE STATISTICAL LABORATORY

Whittle: Then in 1947 you went to Cambridge. Was there any particular reason for the move, or did you just think it was time to move?

Daniels: Well, I had a feeling that it was time I returned to academic life. You know, I rather liked the idea. And John Wishart was developing statistics in Cambridge at that time and was looking for someone to fill a lectureship he had managed to acquire – not to take Maurice Bartlett's place, because Bartlett was still there. Wishart and I knew each other during the war; we had kept in contact. He was in the Admiralty, I think. So he essentially asked me if I would like to come. I think it was late 1946, maybe 1947, when I moved to Cambridge. To begin with I was giving service courses to agricultural students, which I quite enjoyed. There is no doubt that if you have a good class of scientists who are not mathematicians they tend to ask you embarrassing questions, applying to their own particular science, which may not occur to mathematicians. It is very good to give service courses, just for this reason.

Whittle: Who was in Cambridge at that time whom we know as statisticians now?

Daniels: Well, the history is that Wishart was fighting very hard to start up the programme in mathematical statistics. Bartlett had been lecturing, but left to take the chair at Manchester about a year after I came. Wishart had, about this time, succeeded in starting the Diploma in Mathematical Statistics. There was a certain degree of friction between Wishart and the Faculty of Mathematics, because Wishart was in the Faculty of Agriculture, which Mathematics looked a bit askance at. But Wishart worked very hard and managed to get the Diploma going. About 1949, I think, before we actually had a hut to live in, we started up with people like Jim Durbin, Violet Cane, Mervyn Stone and so on – all people who came back from the war, essentially, and completed their degree or did graduate work. So we had a wonderful list of people who all went on to do brilliant things. Not perhaps through any real effort on our part; they just returned to Cambridge. The first lecturer after me was Frank

Anscombe. He had attended Bartlett's lectures and taken very good notes; he must be one of the few people who managed to take good notes on that course. I read through Anscombe's notes, and realised that Bartlett's approach to the subject was one that I sympathised with very much. Maurice Bartlett has been a tremendous influence on me ever since.

Whittle: I remember that, when I passed through in 1952, Lindley and Cox were also there.

Daniels: That is right. Lindley followed, and then we acquired yet another lectureship, and so Anscombe went up to Leeds to have a chat with Cox to persuade him to come down. And he did, and we were very lucky to have him. That was the heyday of the Lab. And then, David Cox had to leave Cambridge for some ridiculous administrative reason which did not allow him to continue his lectureship, so he went off to the States. I have forgotten when that was. I can't remember exactly when Wishart died; I think it was around 1956. He was at a conference in Acapulco, Mexico, where he drowned. For a year or two I was Director of the Lab, actually running it. Then a chair became available in Birmingham, because the mathematicians there, R. E. Peierls and C. A. Rogers, wanted to start up proper statistical teaching.

Whittle: It was well before all this that you took your sabbatical, wasn't it?

Daniels: Oh, yes. In 1952 I managed to get a year off and spent this year at Chicago. Jimmy Savage was very keen to have Dennis Lindley there at the time, because he had gone over to the Bayesian camp. However, Dennis could not manage that year, and somehow I took his place. I was very happy in Chicago. It was a very good department, very practical. That is where I actually wrote up the saddle-point material.

Whittle: What was it that started you on the saddle-point theme?

Daniels: Oh, that goes back a long way. When I was at WIRA I took an external Ph.D. from Edinburgh. Of course, one got no supervision, and I did far more than I need have done for a Ph.D. But I was working on a problem that had really intrigued me, arising out of textiles. That was the problem of the strength of bundles of threads.

I published part of this work as a Royal Society paper in 1945, but the rest is embedded in my Ph.D. thesis, which revolved around what I think is an ingenious application of saddle-point approximation to another aspect of the problem. At that time I communicated with Harold Jeffreys, because he was my guru in this area, as it were. The power of the method then became plain to me.

Incidentally, talking about Ph.D.s, David Cox did a Ph.D. at Leeds with me. I managed to persuade Wilsdon, the director, against what he thought was his better judgement, that David Cox should do a Ph.D. at the University of Leeds, with which we were affiliated.

Whittle: Was it at that time that David Cox developed his ideas about sequential tests?

Daniels: No, that was after I left. Before that, I remember remarking to him that he might like to try and sequentialise most of the tests we were using. But I can't claim any credit for this particular development. He did his Ph.D. on what was really a mathematical problem on the mechanism which underlies how a yarn is produced. I had been working myself on this, but thought he might like to try and develop it. He published a Royal Society paper on the work, and that was his Ph.D.

THE BIRMINGHAM YEARS

Whittle: To return, you went to Birmingham in 1957, is that right?

Daniels: Yes, 1957. Partly because I became fed up with Cambridge; I didn't like the social atmosphere. At that time no statistican in the Lab was a Fellow of a college. I found the whole atmosphere in Cambridge not to be very pleasant, as between Fellows and non-Fellows. So I decided to take this job at Birmingham, which I found to be a good move.

Whittle: You found Birmingham a contrast?

Daniels: I enjoyed it very much, yes. Well, you know, you belonged. In Cambridge, there were levels of belonging. Even when you are a Fellow, there are levels of belonging, as you know. But don't let me get on my hobby-horse about it. (Laughs)

Whittle: Well, I'd like to later, but we'll do the chronology first and get back on the hobby-horses. You were in Birmingham for some time?

Daniels: For twenty years, essentially, 1957 to 1978. We had some very good students there. We had David Hinkley, Peter McCullagh, Julian Besag and various others. I didn't go in for Ph.D. students myself very much. I used to pass them on to David Cox, who has many more ideas than I do, or Maurice Bartlett. So David Hinkley and Peter McCullagh went to Cox and Julian Besag to Bartlett. But my particular interest, really, was my own research. I didn't really like to bother much with Ph.D. students.

Whittle: And you managed to keep it going during that time? Well, obviously.

Daniels: Oh, very much so.

Whittle: Despite the administration, and so on.

Daniels: Well, there was always that. But I kept very much in touch with the other science departments. There is a paper, for instance, on the accelerating centrifuge, which arose directly out of some work we were doing. In fact, Vic Barnett was with me at the time, and we were actually analysing centrifuge data for one of the chemists. An ultracentrifuge essentially involves ordinary diffusion with a constant boundary. But, when you start it up, the boundary varies. There are various conventions about how you should allow for this, none of which is really consistent with the data. And so I solved the problem of the accelerating ultracentrifuge. As I discovered afterwards, nobody had done this before, and so it became quite a famous paper. But not among statisticians, of course.

Whittle: Hogben and Mather must have been in Birmingham at the time.

Daniels: Mather had been there for some time, of course, as professor of genetics. I kept in contact with him – we really didn't click very much. He tended to be a little authoritarian in his view of statistics. I don't believe in fighting with people; I just let them have their own views. That was why, when I was at Cambridge – perhaps I ought to go back to Cambridge and talk about my relationship with Fisher there?

Whittle: Please.

Daniels: Fisher came back to Cambridge, as you know, and became professor of genetics. We were on very good terms because we had a common interest in physics and so on. But I always avoided discussing statistical matters with him, because of his well-known propensity to lose his temper.

Whittle: You had an early correspondence with him, I believe.

Daniels: Well, yes. That was after I had left Cambridge as an undergraduate and gone to WIRA. I am afraid a lot of people will have heard this story, but it is interesting in its way. After I left Cambridge for WIRA, I was looking around for a research problem. And I remembered that Fisher's book, in its fifth edition at that time, contained a remark about the t-test in the case of two independent samples. The assertion was that, if the population variances were different, then the significance of the t-statistic would be somewhat enhanced, so emphasising the population difference even more. Well, I did a few elementary things, playing around with means and variances. The results seemed to indicate that, if the variances differed one way, then Fisher's assertion could be true or false, depending on how the actual sample numbers differed. I wrote to Fisher, asking if he could elucidate this. He replied, at some length, but not really meeting the point at all. Then I noticed that, when the sixth edition appeared, the word "somewhat" had been altered to "sometimes." Now, there was no other indication in the book that anything had been changed. The observation made a certain amount of sense, but not really the sense that he intended to begin with. George Barnard told me that he does this quite a lot – that he keeps making such changes all the way through the editions of *Statistical Methods*. And, well, that's Fisher: you just have to put up with it.

Whittle: He makes a concession, but tries to avoid the appearance of doing so.

Daniels: Well, yes. You have to make allowances, in a sense.

Whittle: Well, you managed to get along with him provided you kept off statistics?

Daniels: Yes. We found that the only person who could have a genuine discussion with Fisher was George Barnard. This was because George had admitted he was wrong on one occasion about 2×2 tables. From then on, if we wanted anything elucidated we would get George to ask him.

Launcelot Hogben was also contemporary with me in Birmingham, in the Department of Medicine. He was considered a kind of great man there, and they hung on every word he said about statistics. I worked with John Squire, who was head of the Department of Experimental Pathology, and he could not stand Hogben at all – partly because Hogben made what Squire thought were anti-Semitic remarks about him. So I had to choose one or the other, and I chose the other, as it were. We worked together very well, also with Tom Whitehead from Pathology, whom I introduced to cusum charts and the like. Hogben sent me a copy of his book, and I acknowledged it. But, somehow, we never really made contact.

Whittle: You had a second sabbatical, in Cambridge in 1975.

Daniels: Yes. Towards the end of my career in Birmingham I decided that, unless I took another sabbatical, I would never have one. And so, against a good deal of opposition from the Vice-Chancellor, I insisted. I went to King's College, Cambridge, where Robin Sibson fixed me up with a job, for which I have always been grateful. An unpaid job, but very good. I was running a sort of mathematical biology seminar, and enjoyed it very much. I got to work with people like Robert May and Dennis Mollison, and developed my interest in epidemics and so on. But the seminar was mostly concerned with problems of sociobiology, which people were crazy about in those days.

Whittle: Well, I know you have a very ambivalent attitude towards Cambridge. But, in fact, you did make that return in 1975, and then a more definite return in 1978.

Daniels: 1978, thanks to you. I could have stayed on in Birmingham, but it seemed to me a good thing to make a break. I had enjoyed my sabbatical year in 1975, and I was a Fellow of King's actually, for one year. It was very revealing to see the difference from the inside of life in Cambridge. That was a brief encounter with paradise, if you like. But I enjoyed my contact with the Lab here, and with you, Peter, and so we fixed up a contract for me to work on developing the saddle-point material – which I did, and which has given me a very fruitful ten years in Cambridge.

MECHANICAL AND MUSICAL

Whittle: Well, whatever you may say about Cambridge, Henry, we have appreciated your presence here very much, and continue to do so. We have gone through the chronology somewhat bone-by-bone. There are a few more general issues I'd like to discuss. You have an interest in gadgetry. You enjoyed the machines at WIRA, and have an almost professional interest in watchmaking, isn't that so?

Daniels: Well, watch-repairing, yes. Ever since I was a boy I have repaired watches, with varying degrees of competence. I regard myself as really rather good at it now. Because I am extremely short-sighted, I didn't have to wear a watchmaker's glass, or loupe, as they call it. Though I do now. Watch-repairing is now one of my principal hobbies. It was in a sense through this that I came to know George Daniels, a namesake but not a relation of mine. He is a very famous man, one of the few remaining men in the world who actually make watches. Every single part: the case, the jewels, everything. He not only makes them, but designs new escapements and things of that sort. I got in touch with him, I think, through a mutual friend of David Kendall's. George Daniels had designed this new escapement, and a particular form of it required a watch with two springs and two trains of wheels. It occurred to him that it would be a good idea to have a watch showing sidereal time alongside mean solar time. This required a gear train to do the conversion. Under very, very tight constraints, because you didn't have much room for wheels, and the wheels could not have more than about a hundred teeth. He had managed, with the aid of a friend, to arrange the gears so that he got a particular conversion which was accurate to seven seconds a year. He wasn't really happy with that, and so he got in touch with me through this friend of David Kendall's. I managed to produce three solutions which were accurate to better than half a second a year. This delighted him enormously.

He used to make these watches, taking a whole year; then some incredibly rich person would buy it at a phenomenal price. In this case, when he had almost finished the watch, the client wanted a moon-phase dial. We had to think up a very accurate conversion train for that, without much room for it, and managed to get one which was accurate to better than half a second a month. Not bad, when you come to consider it. George Daniels was Master of the Worshipful Company of Clockmakers, and he was so delighted with all this that I am now a Liveryman of the Company.

Whittle: You were involved here with the design of the mechanism. But I think you also enjoy the actual manual side, don't you?

Daniels: Undoubtedly; I think that is my main interest.

Whittle: That reflects itself in various ways. I think that even as far as your enjoyment of music is concerned there is also a strong appeal in the mechanics of the instruments themselves.

Daniels: That's right. I have always collected instruments of various kinds. I am basically a pianist, but I had always regretted that I had not taken up other instruments. Then in 1968 I picked up an English concertina in a junk shop. I found it a very good instrument for chamber music, for which it was indeed designed. Since then I have been trying, along with others, to promote interest in the English concertina as a chamber instrument, although it is now being revived as a folk instrument. But its real purpose, as intended by Wheatstone who actually invented it, was for playing chamber music. It was very popular in this role in the nineteenth century, and played by eminent people. Queen Victoria had one, as did the Archbishop of Canterbury. Arthur Balfour used to play Handel choruses on it with his friends until quite late in life. But I'm interested in the instrument because I also do repairs and tuning for other people.

Whittle: Well, I know that you are a doer rather than a watcher or listener. You enjoy the "minus one" records, don't you?

Daniels: Well, yes. But they never seem to play the thing in the style that I particularly want. What I usually do is just play on top of Yehudi Menuhin or somebody.

Whittle: Have you tried composition?

Daniels: I used to be interested. I almost wrote a quartet once: I wrote two movements.

Whittle: Daniels' unfinished quartet?

Daniels: That's right, and rightly unfinished. (Laughs)

PAPERS AND PEOPLE

Whittle: Returning to science, Henry, I think you started writing papers already as an undergraduate?

Daniels: That's right. In my final year at Edinburgh it occurred to me that a certain aspect of nonlinear first-order differential equations should be investigated. I produced a paper which I was encouraged to publish by the astronomer, Bill McCrea, later Sir William McCrea, who was a lecturer at Edinburgh at the time. Apparently this caused quite a stir among these people; they had never thought of my way of doing it.

Whittle: That was a theorem on the complete integral?

Daniels: Yes. Also I was always very interested in projective geometry, even at school. I have always been very sad that the subject has in a way dropped out of mathematics. I went to F. P. White's lectures in Cambridge on projective geometry. He tried to produce a proof of the existence of what is called the

FIG. 2. *Henry Daniels at Canadian Statistical Society meeting, Edmonton, 1992.*

Φ-conic. This can be done in two seconds by writing down some invariant or other, but it took him three lectures to do it by projective methods. I managed to produce a half-page proof. I gave this to him and, typically, he lost it. But Edge, who was a geometer at Edinburgh, arranged for it to be published in the *Edinburgh Mathematical Notes.*

Whittle: Your first statistical paper, I think, was the 1938 one on some statistical problems in wool research.

Daniels: Apart from a rather theoretical one in *Proceedings of the Cambridge Philosophical Society.* This was around the time when the so-called Supplement to the *Journal of the Royal Statistical Society* had been initiated, and had produced a very interesting series of papers on applications of statistical methods to industry. The paper you speak of was in a sense solicited for this series. I wrote up what I was doing at WIRA and how I was applying these methods—the estimation of variance components in particular. Then there were further details concerning variance components which led to a further paper. That was my first venture into genuine statistical publication.

Whittle: Then you got on to measures of correlation and rank correlation.

Daniels: That was because of a paper that Maurice Kendall had written on rank correlation. He of course was very interested in this area. I produced a sort of general theory which included his work. Also a method of proving asymptotic normality, a sort of combinatorial proof, which has now been superseded by Hoeffding's U-statistics. My method is really quite distinct from Hoeffding's and applies more generally. It is now being revived by Jack Cuzick for the some of the things he is interested in, to which U-statistics cannot be applied.

Maurice Kendall was a remarkable fellow. He had this insight into new ideas, but never carried them far enough, somehow or other. I used to follow him and clean up after him, in a sense.

Whittle: Then in 1945 came what is clearly a key paper in several ways: the paper on the strength of bundles of threads. It is important in that it presages the present interest in the strength of structures, especially structures made of composite or oriented materials. It is also important in that it started you on the technique of saddle-point approximation.

Daniels: Not quite—it was really more related to my interest in curved boundary problems. The way I originally solved the problem was rather special: later it turned out to be a case of a diffusion with a curved absorbing boundary. At the end of my publication list you may find something about the maximum of a random walk. That argument has enabled me to extend the solution to more general types of bundles, important in the theory of the strength of ropes and the like. I gave a talk on this at a symposium at Cornell, recently. It has been a kind of thread running through my life. It has led me into all this work on curved boundary problems, tangent approximation and so on.

Whittle: Well, I suppose you can say that a bundle is one of the first and simplest examples of a composite structure. Your paper must have been about the first example of a statistical model in this area.

Daniels: Well, that's true. It is called a Daniels system now, I am happy to say. It has been developed very considerably by other people in this area, and so it has been a very fruitful paper, I think.

Whittle: Richard Smith is one of those?

Daniels: Yes. He worked with Phoenix, in Cornell—a sort of mathematical engineer who is very well known in the field of strength of materials. That is where Richard did his Ph.D. work. I got to know Richard when he returned to this country, and introduced him to what I was doing on the curved boundary technique. He had not realised the connection with what I and Andrew Barbour had been doing in this direction. He realised then that his own previous work could have been carried through very much quicker by these methods.

Whittle: Was your 1952 paper on stiff chains also motivated by textiles?

Daniels: Yes, although that is something I would now do in a different way, as we all think ten or twenty years later. But that was another problem that intrigued me: random walks in which there is a certain amount of correlation between one step and another of a particular type. It is important in the theory of polymers, particularly the optical behaviour of polymers. Certain formulae that I worked out are now standard, and much quoted by polymer chemists.

Whittle: The saddle-point interest continues in 1954, and you have a major paper on the topic in 1987.

Daniels: That is when I first started really applying it to statistical theory. It happens in that particular application to be more or less identical with Cramér's result in large deviation theory. But in a way the saddle-point approach seems more natural to me. It has been revived recently, particularly by Barndorff-Nielsen and Cox, because it happens to tie in with their particular approach to inference, and has led in the last few years to a great deal of work. But there is also this interesting fact about saddle-point approximations: they always seem to be more accurate than one should expect. In fact, they seem to embody the truth of the matter far better than the exact formula, which is itself often difficult to comprehend. The saddle-point approximation somehow seems to capture the essence, which is why I pursue it. One does feel that there is some underlying reason why the approximation is so much better than expected. What I showed in the 1954 paper was that there is a whole class of cases in which it is uniformly relatively accurate over the whole range of the statistic. But it really is remarkably accurate in the middle of the range, not just at the tails. What we really need is a theory which explains why it is so good. Nobody really knows, and that is one of my abiding interests.

Whittle: You do seem to find applications of the saddle-point techniques everywhere you look—in the calculation of the velocity of an advancing wave, for instance.

Daniels: That intrigued me. I wrote a paper in Bartlett's festschrift, following up his work on advancing waves which you find mentioned in his little book on epidemics. It turns out that if you apply saddle-point methods you can demonstrate that his approach is inadequate, because it uses diffusion methods, which are not really appropriate for large deviations in this application. When you have a wave moving out you depend more and more on the large deviation behaviour; ordinary diffusion approximations are not good enough.

Whittle: You have mentioned this before: saddle-point methods are close to large deviation methods, and try to capture the same scale of phenomena.

Daniels: Essentially. Except that the large deviation people, especially the pure mathematicians, are always looking for rates. In this way they miss one of the essential aspects of the saddle-point approach, which is the extra factor, giving you the next level of approximation. Use of the rate term alone gives you something which is wrong by a term of relative order $n^{-1/2}$. It is this second saddle-point factor which gives you the astonishing accuracy.

Whittle: You have a 1960 paper: "Approximate solutions of Green's type for univariate stochastic processes." Is that related to WKB methods?

Daniels: Yes, it's an extremely heuristic paper, which I'm definitely not ashamed of, because I think there should be more heuristic papers. I think that this obsession with having to prove everything has inhibited mathematical development, in applied mathematics, anyway, and so there is a time when one ought to write very heuristic papers saying what one thinks is true. In this case I developed a method, very like a saddle-point approximation, but I didn't have a generating function, so I had to do it the other way, which is really looking at the Green-Liouville approach to differential equations. I applied this to stochastic processes in continuous time. It's not a complete paper, but it shows that if you actually apply the technique you obtain very good approximations. It's very much related to your own work; I remember you wrote a paper on normal approximations to stochastic processes. I happened to inadvertently repeat some of the techniques of that paper, not knowing of your work. All these things come together in a very remarkable way. We are all straining after the same thing.

Whittle: You mentioned that there is a place for papers which don't prove anything. One won't easily find journals which will publish that kind of paper. And, obviously, some kind of scrutiny is required on the part of the referees. They must know that, if rigour is lacking, there are at least compensating qualities.

Daniels: Well, I have no objection to rigour, why should I? It's absolutely essential. But the trouble is that, when people finally rigorise what they are doing, the main idea seems to vanish somehow in the detail. I think there is a place for people writing heuristic papers, just saying what their ideas are. I agree that there is this difficulty about publication. There was a period around the '50s and '60s when statisticians just became obsessed with proving everything, to show what good mathematicians they were. I just think that the more important thing is the ideal. Don't you agree?

Whittle: Up to a point, Henry. To continue, I remember being very fascinated by your 1962 paper on permutation expansions, which you spoke on at the time.

Daniels: That is another of Maurice Kendall's ideas. He wrote a paper in which he tried to show that, by taking ranks of the observations rather than the actual observations themselves, and then the ranks of the differences and so on, you could extract almost all the information in the sample. I followed this up with a modified approach which really was triggered by his idea. In the end it provided a generalisation of a binary expansion, in which the integers 0 and 1 were replaced by permutations. This generalises the interval (0, 1) to a simplex; quite intriguing. Another form of the idea led me to believe that one might even be able to find a generalisation of a continued fraction. But, unfortunately, as in all generalisations of continued fractions, there is a problem of uniqueness, which I am afraid I have not settled. But the statistical idea was indeed Maurice Kendall's

Whittle: Then you have a succession of papers on epidemics; how do you see them?

Daniels: I found it interesting that, if one considered the ultimate distribution after the epidemic had resolved itself and disappeared, one ended up with a distribution of non-infectives crowded around either the origin or the top. Either there were a lot of people who were uninfected or very few: a U-shaped distribution, in most cases. One could approximate the first part of the distribution; you did some work on this. But, at the other end, nobody knew what was going on. I realised that one could approach this by some of the methods I had used for the bundles of threads. I finally managed to prove that the other component of the distribution was a reflected Poisson distribution. And, of course, later I saw this again as a curved boundary problem.

Whittle: You have already mentioned your doubts of the Ph.D. system, Henry.

Daniels: Well, that may be partly my own individual makeup. But I don't really like the idea of the Ph.D. system because, in this country at least, you have to produce a thesis in the two or three years you are given. Very often the people who produce the thesis will never do any real research again. In many ways it somehow never really proves that the student is any good at research. I mean, you take somebody on, and you have this so-called training for research, which I think is a fallacy. If you are a chemist you learn how not to drop bottles and how to turn on the right bit of equipment; in our subject, training for research means perhaps using a computer or something. But this is not what it's all about; it's about having ideas. What happens with the run-of-the-mill Ph.D. student is that you lay a trail of clues for him, which he follows, you hope, and in the end produces his thesis. Well, it seems to me that this is self-defeating. Unfortunately, it so happens that in the States you can't get a job without a Ph.D. And so you have to maintain this charade. I have nothing against people working under some well-known researcher. But what should come out of this is a number of papers which are published. Or else this character should be told "You have no ideas. You can go away and make a lot of money somewhere else." I mean, it seems to me that to keep a graduate going because he needs the degree for a living in the end somehow is not what academic life is about. Or should be about.

Whittle: So, an extreme characterisation of your position would be that, if a graduate is suited for research, he will just do it, whatever the circumstances; if not, end of story.

Daniels: Well, I agree that that may be my own particular history. I did my Ph.D. externally without really any sort of supervision at all. Aitken was nominally my supervisor, but hadn't the faintest idea of what I was doing. I did far more than I need have done, and, in a way I was doing it because I enjoyed it.

IN RETROSPECT

Whittle: Let us have a last word on the Cambridge issue, Henry. You disparage Cambridge quite a lot, and yet you return. You obviously have an ambivalent feeling. To what is this feeling converging?

Daniels: Does it have to converge? Cambridge is a very pleasant place to be in. The Lab is a very good place to be, and I enjoy having you here, and various other people. David Williams has been a great stimulus, as has David Kendall. I have never been reconciled to the college system. It just seems to me a ridiculous system which is part of history and ought to be allowed to die. But there it is, it's just me again. I enjoy being here. I don't like all the bicycles; I have been knocked over twice. But, on the other hand, if I didn't like the place I would leave. So why do I stay here?

Whittle: Very well. Another thing that has struck me is that you are a person who has very definite, simple and explicit enthusiasms. A certain type of mathematics, music, mechanical things, instruments. Yet, at the same time you have a very sceptical, ironic, cynical attitude. How do you reconcile these two?

Daniels: Why should they be reconciled?

Whittle: I don't say they should be. I just wondered how you saw them coexisting.

Daniels: Well, my cynical attitude is not to these matters; it's to other matters. You just mentioned the Ph.D. system; I'm very cynical about that. That really hasn't much to do with these enthusiasms of mine. You know, being enthusiastic about music is something that is part of your being, as it were. You are never cynical about that. You could be cynical about people who pretend to be enthusiastic about music. Who go to operas and are not really enjoying them and so on. But I don't see any need to reconcile these things.

Whittle: It's just that it has always struck me as a contrast. One very positive, the other perhaps . . .

Daniels: Not negative, surely?

Whittle: Well, different, in any case.

Daniels: Constructively cynical.

Whittle: Different. Do you feel you have been influenced by anyone in particular?

Daniels: I think Maurice Bartlett was my main influence here in statistics. Harold Jeffreys in asymptotics. George Barnard's work during the war on sequential analysis I found fascinating; it was that which started me on things to do with curved boundary problems. I'm not sure I know of anyone else except perhaps David Kendall who really has influenced me. Maybe yourself, I don't know, in some of the things you have done.

Whittle: Well, you certainly have influenced more than one generation of workers in the subject. People still beat a path to your door. How do you see your work continuing? I know that you do see a continuation.

Daniels: I have a number of interests, all of which arise out of the papers we have been discussing. Also I keep myself going by offering a paper for a conference without being very clear what it will be about. I need a certain amount of panic to get the adrenalin going. I would like to get the approximations of Green's type on a proper basis. Despite what I say about heuristic papers, I would like to get that really well established.

Also, I would like to develop the bundle theory further on the lines I have been following. You see, the final paper on bundles used an idea that Durbin introduced, that in many cases a general stochastic process can be regarded locally as a random walk. This is an old idea, of course, in the diffusion context, but Durbin suggested that it was more generally applicable. If one can now discuss the maximum of a Gaussian process, whose mean path has a maximum, then one can generalise the classical bundle problem in many ways. For example, one can cope with the situation where the threads are not clamped properly, and there is a certain amount of slack. Also, one can cope with the situation where the threads yield a little before they snap, which makes the analysis much more practical in application.

Whittle: I know that you and Alastair Young have written on the bootstrap method.

Daniels: We had a seminar here from Anthony Davidson, in which he actually used saddle-point methods to replace resampling, in the case of a moderate-sized sample. Suppose you want the distribution of the mean. If you then consider sampling from the empirical population, you can replace the resampled means to get the distribution by a saddle-point approximation. While he was talking, it occurred to me that, if you are interested in tail probabilities and you get near the end of the range of your original sample, you must surely go wrong. Because, you know, there might be an underlying normal distribution, whereas that derived by resampling has finite support and can't possibly be right at the ends of the range. So I developed an analytic way of finding the expectation of the tail probabilities, from that sort of sampling, this to be compared with the genuine tail probabilities from simulations. Alastair Young performed the simulations, and he found some rather intriguing things. He did in fact find, as I had suspected, that the tail probabilities were

FIG. 3. *Henry Daniels showing how to calculate a sum of squares on a slide rule, Birmingham, England, 1978.*

underestimated. But the curious thing was that, when you got further out, they were overestimated. This requires clearing up. The other thing was, could one apply this method to the Studentised version of the mean? I developed a method for doing this, but you have to be careful. In estimating the standard error you square observations; this magnifies any peculiarity in the sample and can lead to really extraordinary results.

My general feeling about bootstrapping is that I don't like it very much. It's easy for me to say that, because nowadays I don't have to do practical problems for a living. It is a different matter for those who really have to provide an answer. The term "bootstrapping" has now of course been broadened to include what we used to call Monte Carlo estimation of a distribution – a perfectly valid procedure, in fact introduced by George Barnard. However, it really has taken off as a sort of Ph.D. factory now, and I really doubt whether it's worth the bother. Though I should apologise to all bootstrappers for saying this. (Laughs)

Whittle: Finally, Henry, how do you feel about the course of statistics, of the subject as you see it?

Daniels: Well, of course, it has changed a great deal since I started in the '30s. And, in a way, concerns are different now. In those days it was applied to agriculture or something similar. One was interested in small samples, and that led to the important work of Student and Fisher. A lot of things were left undone because they were to hard to do, in a sense. We didn't have enough computing facilities. Computers have changed one's attitude to the subject entirely. Not only bootstraps, which I have been rather rude about, but just generally, that one can do so much more. Multivariate analysis, for instance, is now a matter of routine, which it was not before.

What do I feel about the subject now? Well, there was a period in the '50s and '60s when it just became too mathematical. Not really too mathematical, but in a sense people became too self-conscious about their mathematics. I think our American friends tended to overplay this one. Perhaps because they couldn't get jobs unless they showed what good mathematicians they were. I don't blame them for that. But it seems to me that the subject is not mathematics. It is statistics, and data analysis has now taken over to a large extent in several directions. And, again, perhaps too much. It was a good revolution, if you like, to change the subject in that direction, but we have to strike a balance. Probability theory is now coming back into the subject, because of things like prediction, which are very practical and require a lot of sophisticated probability. And, you know, practical statisticians should not sneer at this. Certainly the engineers don't, and they are very practical people. Perhaps some of us should not be so rigid in our attitude to Bayes in this respect, because there are many situations where one cannot get started unless one is prepared to be a Bayesian. It is typical of these situations that, when you know nothing, you pretend to know something just to get started.

Well, I think that is generally my view of the subject. Some people say, "Is it a subject?" Well, I think it is. In fact, it really is the basis of all science, when you look at it, or at least experimental science. Of course, scientists don't usually admit that, and, at the preliminary theory-forming stage, statistics may not play a part. But they come and ask you what to do with their data when they can't see any way out. The basis of scientific thinking is surely the forming of inferences from observations. That's my view. Some people would disagree, of course. There are those in certain areas of science, which don't need a great deal of statistics, who say that if you can't interpret your data, do some more experiments. That is certainly not always possible, and, in any case, why waste the information you already have?

Whittle: Thank you, Henry; I think we have ranged as far as we can in one afternoon. I have tried to make you reveal yourself rather than feel comfortable, knowing well that whatever was revealed would be quality Daniels. As your eightieth birthday now approaches, Henry, there will be many, many others besides myself who would wish you to know the deep respect and affection in which you are held.