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A Conversation with Dennis Lindley

Adrian Smith

Abstract. Dennis Victor Lindley was born on 25 July 1923 in south London, England, and his childhood was mainly spent in Surbiton, a southwestern suburb of London. He was educated nearby at Tiffin Boys' School and read Mathematics at the University of Cambridge, where he later became Director of the Statistical Laboratory. In 1947 he married Joan Armitage, cousin of the statistician Peter Armitage, and they have three children: Janet, Rowan and Robert. He later occupied chairs in statistics at the University College of Wales, Aberystwyth, and at University College London before taking early retirement in 1977. Since then he has travelled widely, holding visiting posts at Berkeley, Chicago, Madison, Tallahassee, Washington DC, Sao Paulo, Melbourne, Purdue, Duke, Davis, Iowa, Rome and Bloemfontein. He is a Fellow of the Institute of Mathematical Statistics and of the American Statistical Association. He has a Guy medal from the Royal Statistical Society, and in 1988 was the IMS Wald Lecturer.

The following conversation took place at Imperial College London on 14 April 1994.

HOME, SCHOOL AND CAMBRIDGE

Smith: Dennis, what was your family background?

Lindley: I was an only child and the family had little culture. Both my parents were proud of the fact that they had never read a book and they had a low opinion of classical music. It was essentially a family in which none of the ordinary cultural activities went on. When I went to Tiffin School, I realized that there were other things in this world.

Smith: I believe your schoolboy ambition was to be an architect?

Lindley: Yes. My father put roofs on buildings and nearly every evening he would come home with plans. I got fascinated by these and decided to make plans of my own—literally.

Smith: After secondary school you went up to Trinity College Cambridge in 1941 to read mathematics?

Lindley: Yes. My father wanted me to be an apprentice to an architect, but the war intervened and made this difficult, so I was allowed to stay on at school to sit for the higher-level examinations. I passed the art exams, but did much better in mathematics. Mr. Meshenberg, the maths teacher, managed to persuade my parents that I should try for Cambridge. Much to everybody's astonishment, except Mr. Meshenberg's, I got an exhibition (a form of scholarship) to Cambridge.

Incidentally, I owed this partly to Hitler. We were being bombed and often had to go down to the air-raid shelters at school. Mr. Meshenberg could not teach the class there, but he could sit next to me, so that I had hours of individual teaching that were enormously beneficial. I don't think I would have gotten to Cambridge if it hadn't been for that.

Smith: Those were the days when getting into Cambridge required evidence of a broad, cultural base?

Lindley: The examination itself consisted of mathematics and an essay, which fortunately was on "The English Parish Church," a subject on which I was knowledgeable because of my interest in architecture. Also I had to pass a separate, Latin examination, which I failed. The summer before going to Cambridge, I did Latin and nothing else for six weeks, took three different Latin exams at the end and passed them all. So I got to Cambridge.

Smith: Did that experience help you acquire rigorous work habits?

Lindley: No, I have always been lazy. Apart from languages, school was academically very easy.

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Smith: Yet you obtained a first-class degree after just two years in Cambridge?

Lindley: That was only because there was dispensation due to the war. Everybody could get their degree after two years.

Smith: What kind of mathematics did you study during those first two years?

Lindley: Half of it was quaintly called applied mathematics, but was really pure mathematics starting from things like Maxwell's and Newton's equations. Otherwise we did a splendid course on linear algebra and a dull one on analysis. There was a lot of geometry.

Smith: No probability or statistics?

Lindley: In the last term, all of us mathematicians were expecting to have to go into the armed forces. Then came this offer to go into S.R. 17 in the Ministry of Supply. I much preferred the Civil Service to the armed forces, but a condition was that we had to go to a statistics course. So Oscar Irwin put on one which I went to and did not understand, in order not to have to fight.

Smith: Statistics as "draft dodging"?

Lindley: Yes—absolutely correct.

WARTIME DUTIES AND STATISTICAL EDUCATION

Smith: What were your wartime duties as somewhat untrained statisticians at the Ministry of Supply? What did you actually do?

Lindley: We attended the National Gallery concerts at midday and we did the *Times* crossword. Seriously, it was like being a group of Ph.D. students without a supervisor. Much practical work was done, but generally speaking, in our section, under George Bernard, we were just learning statistics. We read all the "big papers" and slowly began to understand what the subject was about.

Smith: What were the "big papers"? What do you remember reading?

Lindley: Mainly the Neyman–Pearson material because that was the most mathematical. Then there were papers on probability, by Cramér and Doob, for example.

Smith: Had the Cambridge background prepared you for mathematical probability?

Lindley: Not really, because we had not done Lebesgue integration. It was thought too difficult, so we had Riemann instead. It was not until the return to Cambridge after the war that the Lebesgue integral appeared.

Smith: Were you aware of the debates that had gone on in statistics? For example, the Royal Statistical Society (RSS) discussions between Neyman and Fisher?

Lindley: I do not remember, though presumably we read them.

Smith: So statistics was learnt purely as a technical subject?

Lindley: Yes, Fisher was not a mathematician. I hope George Bernard will forgive me for saying this, but that was my attitude at the time, so far as I recall.

Smith: Who were your other fellow learners at the time in the Ministry?

Lindley: Robin Plackett, Peter Armitage, Frank Anscombe and Morris Walker. Others did not go on to become professional statisticians. Pat Rivett was there, later one of the leaders in operational research.

Smith: You say that you spent a lot of time at concerts and doing crosswords. Did anything emerge workwise that survived?

Lindley: Oh yes. For example, the Plackett–Burman designs. There is a good account of the activities of S.R. 17 in Barnard and Plackett (1985). I do not understand how we failed to develop sequential analysis. We knew the likelihood-ratio test and its optimum properties, yet, although Barnard had the idea of sequential methods, none of us thought of using the likelihood ratio. In retrospect, it seems incredible that we should have been so stupid.

Smith: At the Ministry of Supply there was military test data to be analysed, but here you are reading these very theoretical papers of Neyman and Pearson! What was the interaction between theory and practice?

Lindley: A rather remarkable man called Womersley had managed to persuade the Ministry to set up the unit and it had been done in a grand style, so that it was overstaffed. Consequently, several of us were going round the ordnance factories doing the practical work, whilst the rest of us were theorizing.

Smith: Did you draw lots for who went round the factories?

Lindley: No. We, in our section, were the mathematicians.

Smith: And you continued to do some mathematical research?

Lindley: Yes. It never got published, but I wrote a paper on the square root of a matrix. The encouragement again came from Mr. Meshenberg. He said that I should not waste my time, but do some serious mathematics. So I went, under instructions from him, to work with Paul Dienes at Birkbeck. Dienes was amazing, not only because he was a fine mathematician, but because he stuck it out in London, in Fleet Street, despite the bombing. I used to go on Saturdays and Sundays.

Smith: Meshenberg's name has cropped up several times. He was obviously very influential.

Lindley: Yes. Many of the masters at Tiffin were very good. Mr. Meshenberg was very forceful and would not hesitate to come home and talk to my parents. He was aggressive and I was captivated by him. To give an example, during the war we were all encouraged to grow vegetables. My father was furious with me—correctly—because instead of helping him with his vegetable patch, I would help Mr. Meshenberg with his. The reason was perfectly simple: the conversation would be better. "Mesh" was a marvellous teacher, and not just of mathematics.

BEGINNING RESEARCH AND BACK TO CAMBRIDGE

Smith: When the war ended and the group at the Ministry of Supply was dissolved, you went to work with Edgar Fieller at the National Physical Laboratory (NPL). In August 1946 you published your first paper, in *Nature*, called "Linear 'curves of best fit' and regression lines." What do you recall of getting started on research and the concept of doing research and publishing papers? Who encouraged that?

Lindley: Fieller. That particular paper, and the longer one that followed, were in response to a question. There was a metrology group at NPL who were not happy with regression because both the quantities they were measuring were subject to error. I was asked to look into this and found it fascinating. Incidentally, I learnt something then that nobody had ever taught me before. I had thought that if something was written in a book, it was correct. That is not so, even in mathematics. There are mistakes. I recall going to Fieller and saying that a paper on regression that I was reading must be wrong, to which he replied that it was quite likely. I was astonished and learnt a salutary lesson.

Smith: You then went back to Cambridge for a year's further study. What did you study during that year?

Lindley: The system then was that you could go to whatever maths lectures you liked. The examination consisted of six papers, each of which had many questions. Buried amongst these would be questions on each of the lecture courses. I went to Jeffreys, Wishart and Bartlett—the only statistics options available. Then to Dirac, just because he was Dirac, and to Besicovitch, who was my director of studies throughout my time; a wonderful, Russian analyst of the old school who worked on almost-periodic functions. He had been a student of Markov's. I also went to Littlewood's course. He had his notes printed and we were expected to read the material before a lecture, in which he would emphasize the main points. This is an excellent system. There were also lectures on Hilbert space which have made me suspicious of the topic ever since.

Smith: Did anyone advise you what a rounded mathematical education should consist of, or did you just select options to suit yourself?

Lindley: Besicovitch provided general advice, but he knew nothing of statistics or even probability. I never swotted up on Dirac for exam purposes. Harold Jeffreys' lectures were attended by about six of us who had come back from the war and fancied ourselves as statisticians. That was the first time that he had had to give the complete course of lectures. He was such a bad lecturer that previously all had given him up, but we stuck at them, and very rewarding they were.

Smith: Do you remember the group?

Lindley: Morris Walker, Chris Winsten and Norman Bailey. I do not recall the others.

Smith: Had you, at that stage, come across Fisher?

Lindley: Not really, Peter Armitage and I belonged to the Psychical Research Society in Cambridge. It was decided to do an experiment and he and I were deputed to go and see Fisher to obtain advice. Nothing came of it, but we did meet the great man, and very kind he was.

THE CAMBRIDGE STATISTICAL LABORATORY

Smith: You then went back to the NPL for a short period, but in 1948 you were appointed to an academic post in Cambridge. Do you remember at what point it dawned on you that you wanted an academic career?

Lindley: Wishart wrote to me from Cambridge saying that there was a post vacant and would I like to apply. I said to Fieller "Look at this extraordinary letter I've had," to which his reaction was "Goodbye Dennis." I suppose I was a respectful young man in those days, a habit I have grown out of, and did what I was told. I thought it was the most fantastic honour to be appointed to Cambridge. To go there as an undergraduate had been remarkable, but to go there to *teach*! Even Mr. Meshenberg was surprised.

Smith: Formally the Statistical Laboratory did not exist until 1953, but it was operating as such from about 1947. What was life like in the first few years you were there?

Lindley: One important feature was that we were outcasts. First, statistics was not regarded by the mathematicians as a respectable branch of mathematics. Second, none of us were Fellows of Colleges, so we were outside the collegiate system. Consequently, Wishart, Daniels, Anscombe and myself, and later David Cox, were very much contained within ourselves. The only real contact we had with the rest of the university—and it was a valuable one—was applied work. Wishart had insisted that all the postgraduate students had to do a lot of this, which meant we had to find applied work for them. We therefore developed a variety of contacts throughout the university. Life was very interesting statistically, but we were not fully part of the system. It did not bother me too much because I was so flattered to be there at all, but Wishart was furious, as he was entitled to be as the only Reader without a fellowship.

Smith: Wishart was the driving force. Did he single-handedly recruit all of you?

Lindley: I think so. I have respect for Wishart. He designed the diploma system, with its mix of theory and practice, and got it working. It is a system which, in its essentials, has survived until today.

Smith: What was deemed to be the core statistical education for your graduate students?

Lindley: You would get a flavour of it from the books by Maurice Kendall and Cramér. Anscombe lectured on design of experiments and Daniels on time series. What I did, with my wife, was to translate, from the German, Kolmogorov's 1933 book on the foundation of probability, and do a course based on that, which, for Britain, was novel. Essentially, it was measure theory in the context of probability.

Smith: Apart from being an academic and college outsider, how did you enjoy being in Cambridge?

Lindley: It was very enjoyable. The great thing was the quality of the students. Cambridge in those days had, and probably still has, extremely good maths students. Even the worst were good. As a result, it was a real pleasure to teach. Most of the Ph.D. students were first class: Wally Smith, Ewan Page, Martin Beale, Andrew Ehrenberg, Mervyn Stone, ...

THE FOUNDATIONS OF STATISTICS

Smith: Your list of publications has what would be seen today as a gap between 1947 and 1950. These days, a young person appointed to an academic post would be in terrible trouble if he or she went several years without publishing any papers. Presumably there wasn't that kind of immediate pressure in those days?

Lindley: There was no pressure. Also there was no pressure to get a Ph.D. I have no doctorate. But there is an explanation for the "gap." When I went back to Cambridge my aim was to make statistics a respectable branch of mathematics. How was this to be done? When I looked at all the other branches, whether it was topology, on the pure side, or electricity, on the applied, they all shared a common system. In the first lecture, the instructor would write down some axioms and the rest of the course would prove theorems. My ambition was to do the same for statistics. It took me several years to do this, resulting in the 1953 paper. That is why there is a gap.

Smith: You haven't mentioned the work you did then that, in some quarters, you are most famous for—the fundamental work on queues.

Lindley: That came about in a curious way. I was secretary of the Research Section Committee of the RSS, where there was a paper on queues by David Kendall that had been enthusiastically refereed. However, in those days people were reluctant to participate in discussion meetings. They had not realized it was an easy way of inflating their curriculum vitae. It needed some work on the part of the secretary to make sure there was a discussion, so I thought I would try a contribution myself. By good fortune, I saw that David's problem could be tackled differently. My resulting paper was just luck; I was not really interested in queues. When the problem was solved, the result was a Wiener–Hopf equation which was unfamiliar to me. Fortunately, Wally Smith was in the lab at the time, a much better mathematician than I, and he solved it.

Smith: So the work on queues was a pure digression, albeit a famous one. Your real goal was to put the foundations of statistics on a firm footing?

Lindley: What I thought I was going to do—and when I'd done it, what I thought I had done—was to provide a firm foundation for most of the Neyman–Pearson and Fisher techniques. That was the object of the exercise. Then, in 1954, I was in Chicago with Jimmie Savage. His approach to the axiomatization was far better than mine, but he had had the same idea. Neither of us would have known at the time what was meant by saying we were Bayesians. What we were doing was justifying the classical techniques. If you read the preface to the second edition of Savage's book, he says something about what a fool he was. We were both fools because we failed completely to recognize the consequences of what we were doing.

Smith: I seem to recall that Savage thought that minimax would be what came out of this as "the salvation." Would it be true to say that he was more targeting Wald and you were targeting Neyman–Pearson?

Lindley: Yes. The distinction was not important. We were both trying to underpin theory based on sample spaces.

Smith: Before you went to Chicago, did you know that Savage was also trying to do what you were doing?

Lindley: No. I had given a talk to the Operational Research Society about the work on queues. In the audience was Allen Wallis, who took me to tea at a posh hotel. He invited me to Chicago. When he asked what I was doing, he said that was interesting, they had a man in Chicago doing the same thing. That was the first I heard of Savage.

Smith: When you went to Chicago, did you immediately establish a rapport with him?

Lindley: Yes. Initially his manner put me off, but one quickly got used to it. He would question everything. He kept saying "Why?" It was embarrassing to have so many of your statements queried, but very healthy. He was a wonderful chap. I liked him. His book had just appeared and we both gave lectures, which were generally well attended. One astonishing thing is that neither he nor I then appreciated the likelihood principle, even though George Barnard had stated it back in about 1951 when reviewing Wald's book on sequential analysis. It was not until Birnbaum's paper (1962) that we appreciated it. Had we done so earlier, we would have seen that we were not justifying either Wald or Neyman–Pearson because they were consistently violating the likelihood principle, yet the principle is a trivial deduction from Bayes' theorem.

Smith: Was it perhaps because you had an implicitly decision-oriented perspective, focusing on procedures rather than isolating inference in some sense?

Lindley: Possibly. My 1953 paper "Statistical Inference" did not have decision in the title, but if you look at some of the inferential statements, you will find there squared-error loss functions and a genuine interest in procedures.

Smith: Savage having published his book and you not having known much about it until your visit to Chicago, did that encourage or discourage you? Did you think Savage had solved the problem?

Lindley: Yes, he had, but it was not discouraging, for now I had a powerful ally.

Smith: How did you get involved in information theory?

Lindley: I do not remember. It may have been Savage mentioning Shannon, but I do recall being excited when reading Shannon.

Smith: The famous 1956 paper on a measure of the information provided by an experiment was written when you were in Chicago. This has very much come to be seen in terms of an experimental-design criterion. Was this motivated at all by the many applied design problems you came across at Cambridge?

Lindley: No, I do not recall any connection. It was just here is this beautiful information stuff which attracted me, partly because no overt loss functions were involved. The suggestion that the logarithm of the density could be viewed as a utility function came much later.

Smith: In the 1940s and 1950s Chairs of Statistics were founded in various U.K. universities (for example, London, Manchester and Birmingham). Cambridge did not create a chair until 1962. Was this instrumental in your decision, in 1960, to leave Cambridge for the small University College of Wales at Aberystwyth?

Lindley: It was one of the factors. Maurice Kendall had done most of the work in getting the money for the chair at Cambridge. I was then Director of the Statistical Laboratory, so I had some contact with him. He said that he was terribly sorry but he didn't think I was going to get that chair. I did not fancy the idea of somebody coming in over my head and running the place, so I looked around else-

FIG. 1. *Cambridge University Statistical Laboratory outing, 1959. In addition to Dennis Lindley (far right), others present include Monica Creasy, Jeff Harrison, Bob Loynes, Ann Mitchell and Morris Walker.*

where, and Aberystwyth was the first of the new wave of statistics chairs that was created.

Smith: Was not getting the Cambridge chair a big disappointment?

Lindley: I don't think so, because I did not really feel of the stature of a professor at Cambridge. I was not a Jeffreys or a Fisher.

Smith: You moved to Aberystwyth with the task of setting up a completely new department. What are your memories of that?

Lindley: Of having lots of money to spend for posts and new buildings. It was an extraordinary time in the universities. There were not many statisticians, so one spent part of one's time trying to persuade people to come and lecture in Aberystwyth. The principal was a Welsh poet. He once said to me that he didn't understand at all what I was doing, but he had been told that I was rather good. He was most supportive and encouraging.

CONTROVERSIES

Smith: At the end of the 1950s, just before the move to Aberystwyth, I think you ran foul of Fisher?

Lindley: Yes, I fell foul of Fisher because I reviewed his third book and found what I thought was a very basic, serious error in it; namely, that fiducial probability doesn't obey the rules of probability. He said it did. He was wrong; it didn't, and I produced an example. He was furious with me. I was tactless, of course; I always have been. I was so pleased with myself to have discovered a howler by the great man. Homer was really nodding.

Smith: How did you come to know that this marvellous discovery of yours had not pleased him?

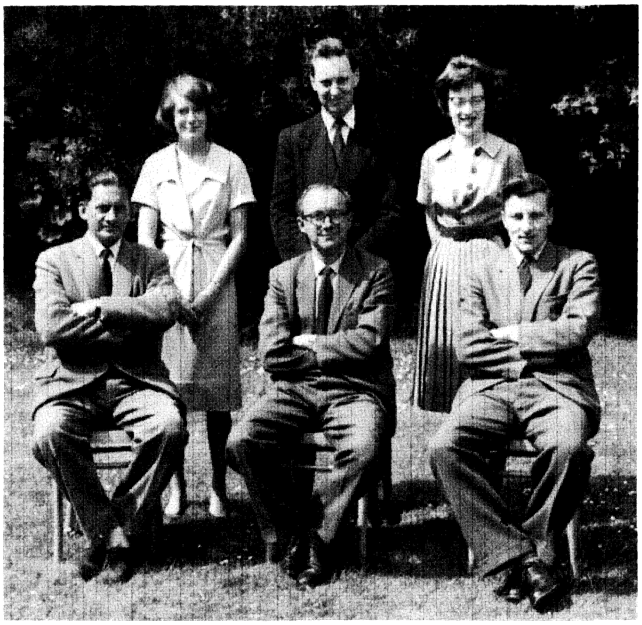

FIG. 2. *The first year of the Aberystwyth department, 1961. Front row: Donald East, Dennis Lindley, David Bartholomew; back row: Elizabeth Davies, Basil Barnett and Ann Mitchell.*

Lindley: I don't remember, but most likely through his colleague Owen. It was not until the book of Fisher's letters (Fisher, 1990) was published that I realized the full force of his fury. He was unreasonable; he should have admitted his mistake. Still, I was a bumptious, young man and he was entitled to be a bit angry.

Smith: Then you compounded the error by turning your discovery into a full-fledged paper?

Lindley: Yes, I was not going to waste a good example. Incidentally, it shows the liberalism of the editor, Oscar Irwin, who said that he would have to publish that paper because it was right—but did I realize what we were doing? We would have the wrath of Fisher on our heads, but it had to be done.

Smith: This was a paper entitled "Fiducial Distributions and Bayes' Theorem." That, I think, is your first publication that has the word "Bayes" in the title.

Lindley: That's interesting! I was written to recently asking who invented the word "Bayes" as a description. I do not know the answer, do you?

Smith: No, I always assume on these occasions that Jack Good would have been responsible, but this may not be.

Lindley: He does not think so. He admits to the word "Bayesian."

Smith: Five years on from the visit to Chicago and you are concerned with fiducial distributions and Bayes' theorem. Had you now come to the conclusion that there wasn't a way of underpinning Neyman–Pearson axiomatically?

Lindley: No. My two-volume text on probability and statistics from a Bayesian viewpoint (Lindley, 1995) was written at the suggestion of the Cambridge University Press, who had heard that I was giving a "different" course of lectures on statistics. The books were effectively my lecture notes. I go out of my way to support analysis of variance and other methods that were then standard. I can remember spending a long time with the posterior distribution of the correlation coefficient, trying to make it match with the fiducial distribution. All the time, I was saying "these clever people must be right."

Smith: But is there not a subtle shift? Were you then not, in effect, trying to show this clever person, Fisher, was right, rather than those clever people, Neyman and Pearson, were right?

Lindley: I think you are looking at it from a wrong historical perspective. Neyman–Pearson and Fisher were trying to do the same thing, and they were, in a sense, supporting one another. For example, Egon Pearson felt that Fisher was doing these wonderful things and his task was to put them on a firm, mathematical basis, as his way of achieving a full understanding. I do not feel that the situation was as antagonistic as those discussions at the RSS suggest.

Smith: Really?

Lindley: I don't think so. They were like Fisher and me over fiducial. We were at loggerheads over that issue, but if you had asked me whether I thought Fisher was a great man, I would have said "Yes," just as I would today. I still think that the analysis of variance and related ideas are a brilliant advance over least squares. Nearly everything that Fisher did was wonderful. I do not think that there was the degree of antagonism that people assume there was: they like to read drama into the situation.

The Fourth Berkeley Symposium paper on the use of prior distributions is interesting because it is the only serious, public row that I can recall having had in statistics. Neyman was furious with me in public. I was very worried, but Savage leapt to my defence and dealt with the situation, I think very well.

BECOMING A BAYESIAN

Smith: What was the gist of his attack?

Lindley: That it was ridiculous to do these Bayesian calculations because they were in parameter space, whereas they should have been in the sample space, looking at the properties of the estimators. By that time, conflict between the Bayes and non-Bayes approaches had begun to emerge and the difference appreciated. If we were to settle this issue, we had to understand the properties of the various systems. I said that all this paper was doing was trying to work out the properties of the Bayesian estimate: what does it look like? There was nothing to take sides on. Neyman was a pleasant person normally, but on that occasion he was not.

Smith: But your own defence at the time is not that of a committed Bayesian? It is rather, "I am just a technician, don't blame me"?

Lindley: Yes.

Smith: Throughout the 1960s, looking back at RSS discussions it would seem that you were almost alone in the U.K. in exploring and advocating the Bayesian position. You encountered a lot of criticism, which some might have taken personally. Did you find the role of lone voice in the wilderness stressful in any way?

Lindley: I do not recall it being so, though my memory may be deficient on this. The general attitude of statisticians to the Bayesian argument is to turn their heads the other way. You put the argument but most of them don't come back with any real response. In my view, they can't come back with a response, but they do not even try. The most distinguished of statisticians just say "Yes, yes," then turn their heads away and carry on as if nothing had happened. This makes them appear polite.

If I look back at what *I* said, it is often rude. I was ashamed when I looked back at one of the discussions of Jack Good's papers. Good is someone whom I, and perhaps Savage, misunderstood and underestimated. He did not get his ideas across to us. We should have paid much more respect to what he was saying because he was way in advance of us in many ways. That is one of my regrets, but I did have the opportunity of repairing this by writing an overview of his work in 1990 (Lindley, 1990). Looking back, I think many of us were unappreciative of Good. I don't know whether his explanations weren't clear or whether we did not take the effort, but the message did not get across, whereas we did understand Jeffreys.

Smith: That's interesting. I think Good's famous books are rather clearly written.

Lindley: Looking at them today, I agree with you, but I did not at the time. He has a clipped style and does not waste words. I think the fault lies with me, not him. With Jeffreys it was different because I had been to his lectures and had analysed his book in detail for examination purposes. Also I could talk to him later. If Good had been in that position, it would have been different.

Smith: But he, of course, had by then left the U.K. Apart from Good and Savage, I think you have on occasion also mentioned de Finetti and Schlaifer as particularly influential, Bayesian thinkers?

Lindley: Yes. In the mid-60s I spent six weeks in Rome with de Finetti. Language was a problem but I learnt a lot. De Finetti had this wonderful ability to make extraordinary remarks that you could not understand, so you had to probe what he meant. When you probed, you realized he was talking great sense. I am always quoting his question "Why did statisticians always think about Greek letters?" I could not make out what on earth the man was talking about. What he meant was: you should not think about Greek letters, but about the physical entity behind them. Those six weeks were amazing.

Smith: And Schlaifer? You visited him at the Harvard Business School?

Lindley: Yes, in 1963. Schlaifer has one of the most original minds that I have ever met, and with extraordinarily wide knowledge. It is astonishing that the statistical community has paid so little attention to him. I was bowled over by him. The book with Raiffa (Raiffa and Schlaifer, 1961) is wonderful. They had independently thought of the idea of conjugate families and worked out the details. The book also contains material on the design of experiments and the value of information. In another book (Schlaifer, 1971) he provides computer methods which were way in advance of the time.

Smith: Let's go back to de Finetti. Did you come to know him through Savage?

Lindley: Yes. Savage had discovered his work in the early 1950s and also that of Frank Ramsey. I did not know anything of Ramsey's work in decision theory, and neither did Jeffreys, which is surprising since they were only a few doors away from one another in Cambridge. Savage was a true scholar: that is, he would go through the literature carefully and attribute everything accurately. Also he was the first to understand what Ramsey was saying.

Smith: Are there other colleagues whom you have particularly admired?

Lindley: George Barnard! He was my immediate superior at the Ministry of Supply and—then and since—I have learned more from him than from anyone else. He is one of the most original thinkers around. We do not always agree, but he is always immensely stimulating.

I have also found Ed Jaynes interesting, with his approach to Bayesian ideas through physics and maximum entropy. The latter is not a panacea, and can produce nonsense, but is useful in areas like imaging.

And I must not forget Dev Basu! I have tremendous admiration for him. He takes apparently simple, trivial problems and then produces deep and subtle insights. His counterexamples to various non-Bayesian procedures are wonderful and I cannot understand how people can be so dismissive of them, as if they were just mathematical artefacts.

Alan Birnbaum was also influential, but when I came to have the most contact with him (in the last years of his life he had a Chair in London) he had moved away from his own writings. It was strange.

I found myself trying to convince him that he had been right in the first place.

Smith: At the time of the publication of your two volume text, that we mentioned, your perception was that what statisticians tended to do on a day-to-day basis was basically sound if you looked at it the right (Bayesian) way. Do you still think that 30 years on?

Lindley: No. Classical statistics contains many unsound procedures, like integrating over sample space after the data are to hand, but good statisticians have a lot of common sense, so that when a procedure produces nonsense, they modify it to provide a sensible answer. Thus there are often ancillary statistics to change the sample space. As a result, their procedures are almost always reasonably sensible though, because of this, the subject becomes a series of adhockeries. The real errors come with nonstatisticians who use the classical methods. I have in mind the plethora of significance tests that many editors demand. Most statisticians today are like mechanics who keep patching up an old car, instead of using the new, and relatively trouble-free, model.

Smith: Are there any major, remaining areas of everyday statistical practice where you think people do crazy things?

Lindley: Multivariate analysis is crazy because it is almost entirely based on the multivariate normal distribution, and that distribution is unsound as a description of reality. Knowing the pairwise behaviour of all the variables in the system cannot be enough to understand the whole—and this is what is claimed for the normal distribution. I have made serious efforts, without any success, to develop multivariate theory that escapes from this.

Smith: You are talking here of formal, multivariate analysis. Presumably you'd absolve the exploratory data analysts?

Lindley: Yes. That was my mathematical bias coming in.

Smith: In 1967 you moved from Aberystwyth to University College London, an environment with a substantial statistical history: Karl and Egon Pearson, Haldane, Fisher, Neyman and, most recently, Bartlett. When you moved to London, did you feel a paradigm shift in statistics was occurring?

Lindley: I would not have put it that way before Kuhn's (1962) book had been appreciated, but yes. I can recall being interviewed at University College and I think Massey, the physics professor, sounded me out on this saying they had learnt I was a different sort of statistician. People realized—and I realized—by that stage that something had happened. University College was tolerant in putting up with someone like me because the place was certainly not Bayesian. Not merely in the statistics department, but amongst statisticians scattered throughout the college. Two members of the statistics department promptly left.

Smith: Now we're 25 years on; de Finetti's prediction was that the paradigm shift would take until the year 2020 to come about. Are we on target?

Lindley: The change is happening much more slowly than I expected. The shift in geology to continental drift took place over about five years. We move slowly, but there is movement. The number of papers on Bayesian statistics appearing in the mainline journals in increasing. I think we are doing a great job, but it is a slow job. Also, the revolution has not been conducted by very good revolutionaries. For example, I assumed in a naive way that if I spent an hour talking to a mature statistician about the Bayesian argument, he would accept my reasoning and would change. That does not happen; people don't work that way. What we should perhaps do—and what we are beginning to do now—is to do practical statistics successfully. I think that the shift will take place through applied statistics rather than through the theoreticians. I was reminded recently that, about the time of the move to London, when asked how the Bayesian view could be encouraged, I replied "Attend funerals."

PRINCIPLES

Smith: Although you have been very much identified with foundational, theoretical issues, you have also had quite an impact on the worlds of medical decision-making, probability and the law, and educational testing. How did you get into these particular areas?

Lindley: In every case, through a personal contact. Ian Evett came along with a question in forensic science; Wilfrid Card with a medical query; Mel Novick with a problem in education.

Smith: But medical decision-making and the law have been long-term interests?

Lindley: Yes, but even there I have stayed away, generally speaking, from data analysis. It's the principles behind the topic that interest me. I am not good at data analysis. I was never trained in it. When I was taught statistics, there was no practical work, and I did little in the Ministry. I am not

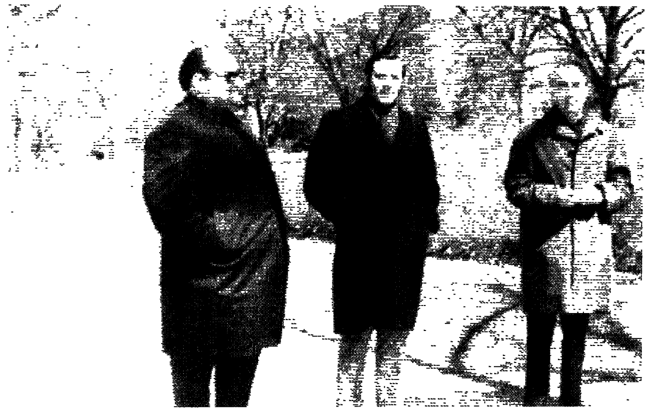

FIG. 3. *Iowa City, 1974; with Mel Novick and Steve Fienberg.*

saying it is not important—it surely is—but when I do applications, it is the principle that interests me.

Smith: Some people might argue that if you don't wallow in the data, you could be misguided in what you think should be the principles.

Lindley: Not if you are working with somebody in the field and are prepared to listen to them. It is most important to have principles. One should have some sort of overall view of the system, whatever it is. Otherwise every problem is a new problem. This is not true; problems are related. Nowadays, whenever I hear about a decision problem, I feel I have a bit of framework. I have to ask what I don't know, what are the probabilities, what do we really want, what are the utilities? I don't say I'm doing all the mathematics, but there is a framework. Whatever subject it is, I have always tried to find some sort of principle.

Smith: So how do you view, let's say, the world of John Tukey, which is almost the polar opposite: *here* are the ways we can mess around with these data; *which* turn out to be interesting?

Lindley: There is a good case for messing around; I wouldn't deny that. In a public debate that Tukey and I had, I summarized it by saying that he was concerned with identifying and giving names to different things, whereas I wanted to see what was common to all those things and pull them together. My vocabulary was enormously weak compared to his. The world needs people to do both these things, but I am built to do the pulling together.

EARLY "RETIREMENT"

Smith: In 1977 you stunned your colleagues at University College London by announcing your retirement—at the age of 54, which in those days was very, very early. What was the background to this decision?

Lindley: There were two backgrounds. There was a member of the department whom I felt was both incompetent and not pulling his weight. The obvious thing was to get rid of him. So I went to the appropriate college administrator and put the problem to him. He said it was very difficult to get rid of someone, that I would have to document all the evidence, go and sit on some of his lectures—and that I would never succeed. But he said there was another possibility: persuade him to take early retirement. New retirement rules had just been introduced. The administrator said he could not advise me very much, but suggested I read the new regulations.

I'm rather good at reading regulations, so I went away and read them and, as I read, realized that I could retire myself on a decent salary. So I began to think about this, which led to the second, background feature.

The system at University College then was that the head of department, within his budget, could virtually do what he liked. This is a splendid system except for the fact that the head has to do a lot of work, and furthermore he gets a lot of flak. If things go wrong, people know it is his fault. I was getting browned off with this. I did not mind doing the work—in a way it was quite enjoyable—but it meant that there was less time for research and students.

I thought about this and went back to the administrator. I said that I had made a wonderful discov-

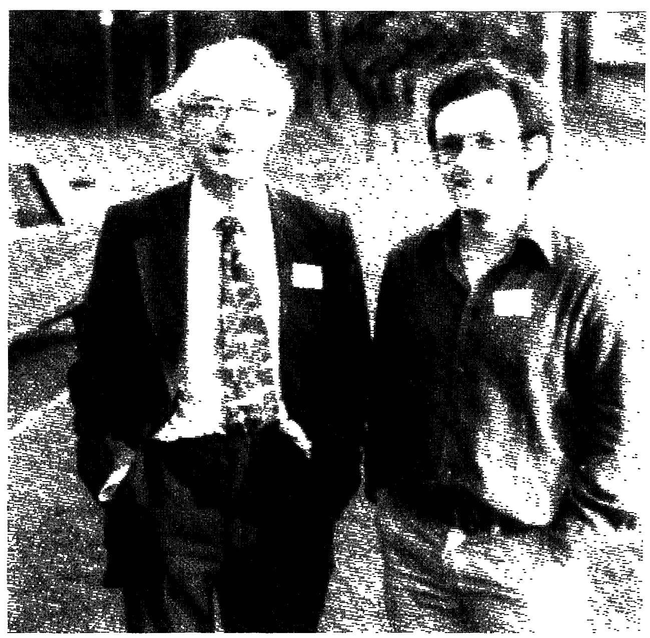

FIG. 4. *Fontainebleau, 1976; with José Bernardo.*

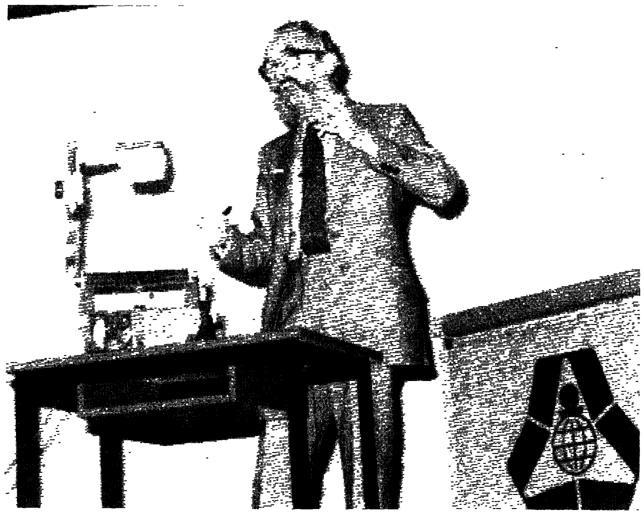

FIG. 5. *Lecturing at the first Valencia Meeting in Bayesian Statistics, Las Fuentes, 1979.*

ery and that I was going to retire. He looked at me in utter astonishment and said that I couldn't. The College did not believe me and wrote to the pension authority to ask their opinion. After a long time, they came back and said that I could retire, just as I had thought, so I did.

My opinion is that academics should retire early. Generally speaking, the older scientists are a drag on the place because they cannot keep up with modern work. There are exceptions, to be sure.

Smith: We have been talking about your "retirement" but, in fact, you've continued to do research and regularly visit colleagues overseas.

Lindley: Yes. When the question of retirement came up, I drew my decision tree. I had a high utility function for peace and quiet, and things like that. Two mistakes were made. First, I did not allow for the fact the people were going to invite me to places and pay me a lot of money, so my financial calculations were awry. Second, and in a sense more serious, was that I had not realized how much value I placed on *good* research students. Having research students around is tremendously important. You've got somebody to bounce your ideas on. Furthermore, they are, in a sense, brighter than you are. Their intellectual appreciation is quicker and better than your own. So you have this wonderful interaction. This loss has been a major regret—a feature that had not gone into my utility function. And—to be a bit conceited—I think I would have had more influence on the development of statistics in Britain if I had stayed and had research students.

CURRENT TRENDS

Smith: Do you agree that the last decade has seen a marked shift in activity in Bayesian statistics away from a focus on foundational arguments and principles, toward a greater concern with providing appropriate computational tools and getting involved in complex, messy applications?

Lindley: Yes, and a very healthy shift it is. Bayesian statistics can do things, not just think about things. I only wish I was young and flexible enough to participate: all that can be done is to understand and admire the work, but even that is a joy.

Smith: Do you think there is still too much unimportant, theoretical work going on?

Lindley: Yes, not only in Bayesian statistics, but in all statistics.

Smith: Why do you think this is?

Lindley: It's the pressure to publish papers. It is quick to take a small problem, solve it, then write the paper and get it published. When I was young, there was not this pressure on publication. You could sit on your problem for years, as we have mentioned. Today too much mediocre material is published. The trouble is that, at the time, it is often hard to tell the mediocre from the substantive, so my proposal of cutting down on publications always meets this perfectly legitimate objection that it would suppress important work.

Smith: Some people have argued strongly that there ought to be a journal of Bayesian statistics. I know you are vehemently opposed to that. Why?

FIG. 6. *George Washington University, 1986.*

Lindley: Because Bayesian statistics is not a *branch* of statistics: it is a way of looking at the *whole* of statistics. If a person is interested in a particular branch of statistics, Bayesian ideas have something to say to them. If you publish in a separate journal, it may not be seen. As someone said to me: we want to have good, Bayesian journals; they are called *Biometrika*, *JASA*,... .

Smith: Recently there has been a growth in the provision of computational tools for doing Bayesian analysis. For many years, people like yourself explained how they *ought* to be doing statistics in a different way, but once you'd got someone excited about your ideas, did you not see it as a problem that the computational tools were not available?

Lindley: I have never appreciated this argument. If I explain the Bayesian position to X and X agrees that I am right, then if X says he cannot do *this* problem, I would respond that there is this principle. He has been given all the rules and he ought to be able to prove the theorem or do the calculations. It is different from classical statistics with all its adhockeries.

Smith: But what if he has a large, messy, applied problem with thousands of data items and he simply does not know how to compute the posterior distribution?

Lindley: It is a research task and the next few years should be devoted to it.

Smith: But most people in routine, applied statistics—where many, many data sets pass across their desk every day—would not be able to do this. Isn't most applied statistics done in a context where, rightly or wrongly, you have to have routine, packaged ways of doing things?

Lindley: I have never worked in an environment like that. If it is a three-year problem, I'll take three years over it. I agree that it is difficult if you are working for some company and have to produce a six-month answer. However, I would still write a note to the head of the department saying that the problem needs doing properly. There is a connection here with the lack of investment in R&D that is prevalent in industry today, but if I am talking to my colleagues in universities, they *can* do it because of the principle inherent in the Bayesian position.

Smith: In retirement, have you missed being in an environment with access to modern work-stations and graphics and, if you like, the computational revolution in Bayes?

Lindley: What you have never had, you never miss.

Smith: And messy, numerical, computational mathematics is far removed from your congenial world of principles?

Lindley: It is not "far removed." It is performing the paradigm. Because I don't have anything to do with computers doesn't mean that I don't think they are wonderful; they are.

POLITICAL PERSPECTIVES

Smith: We've talked about being attracted to sorting out the principles of things. The Bayesian paradigm can be viewed in some ways as a self-contained, comprehensive view of the world, rather like Marxism. I believe you were attracted to Marxism in your youth; maybe you still are? Do you see a linkage, from a personal point of view, in terms of the intellectual attraction of working within "a system"?

Lindley: Yes. You've always got to be alert to the fact that the system might not be entirely satisfactory and be prepared to change your system, which is psychologically very difficult for people to do. I do not understand people who do not want to have a system. I do not quite understand what the Marxist system is, but usually Marxist writers talk more sense than other writers on specific issues.

Smith: I seem to remember that a "Marxist" remark in a statistical context got you into trouble with some American colleagues a few years ago. What was that about?

Lindley: It was in my discussion of Morris (1983). I was making the point that there were too many papers being written which were purely technical, and where the writer of the paper hadn't really thought about what he was doing. He had carried out a piece of mathematics without ever querying whether it was sensible in the context of the problem. (A point we have mentioned before.) I believe strongly that scientists, as part of society, are influenced by what society is doing, so I went on to say that this seemed to be true of the United States, which considers itself a democracy. At a certain, technical level, it is very democratic. Locally they elect more people than we do in the U.K.; judges, for example. It is very democratic at the local level, but they rarely ask themselves what democracy *is*. In my view, the United States government is nowhere near what I understand to be a democratic government.

I quoted Nyerere's statement, which I agree with, that the U.S.A. is a one-party state with the extravagance of having two of them. It is a dictatorship of the capitalists. As a result, any socialist, whether in

Nicaragua, Cuba or Chile, is not considered democratic by definition. Like statistics, sound at the bottom, rotten at the top. The editor published it and Morris was happy, but others were furious with me. They maintained that since I was frequently a guest of the U.S.A.—and, indeed, was paid by them—it was impolite of me to comment. My reply was that because I was a guest of the U.S.A., I had more experience of that country than other people.

What was also striking, just like Bayesian statistics, was that nobody came back and argued with me. Thus no one defended the action of the U.S.A. over Cuba: action which seems to me to be cruel and vicious, just because the Cuban government is socialist.

Smith: You presented a paper at the fourth Valencia conference which basically argued that if one returns to the decision-oriented roots of Bayesian statistics, then utilities, and hence values, are inevitable on the agenda once you look at any social or scientific question. Yet we tend to suppress this when we do technical statistics?

Lindley: My view is that probability is the only mechanism for studying uncertainty. Almost every situation in this world has an uncertain element in it, partly because it involves the future, and the future is uncertain. Therefore probability must have a contribution to make toward almost any question you like to think about, no matter whether it is politics, economics, sociology, ... Furthermore, when you go into the analysis, as you have mentioned, you have to include utility. People prefer one thing to another and therefore, in a sense, have a utility function. I think that probability, utility and the maximization of expected utility are tools that could be used almost everywhere. I think we Bayesians should be shouting from the rooftops about the wonder of our subject!

Smith: But you've spent most of your life shouting from the rooftops!

Lindley: I can't get to the highest rooftop. A colleague told me recently that he had just met the English Socialist, Tony Benn. "Isn't he a pathetic character?" he said. I reacted that here is this man, Tony Benn, who talks such sense, being called pathetic for shouting differently from the rooftops. How can you get unconventional ideas across? Look at Chomsky: he writes badly, nevertheless he has vital, political points to make and few take notice of him. You can see why: the media are largely controlled by the establishment which does not want these awkward people disagreeing with them. They don't want the Benns and Chomskys of this world. They will try to suppress them—and they do.

Smith: Let's talk for a moment about the role of dissent in statistics. People presumably expect members of a profession—for example, lawyers or accountants—to be broadly in agreement about what their expertise is and how to do things. Can statistics be seen as a profession if we continue to place a high value on disagreement and debate? Some have argued that inferential debates are detrimental to the wider image and impact of statistics. Have you ever thought about that?

Lindley: Yes, it is detrimental, but truth is more important than image, in my view. We have got to get it right, and I think we will. We will all be Bayesians in 2020, and then we can be a united profession. Notice that the two examples you cite also have some internal dissension: accountants over the use of significance tests and lawyers over the soundness of the adversarial system.

THE FUTURE STATISTICAL AGENDA

Smith: The information-technology revolution means that we can collect and store enormous amounts of data. As a result, high-dimensional, exploratory, data analysis is a very active area. Do you see this in conflict with your view of statistics?

Lindley: I am not very familiar with this field and so my comments aren't worth much, but it does seem to me that statisticians have not developed the tools for going beyond about three dimensions. Collecting all these data when you don't know what to do with them is highly dubious.

Smith: I think a lot of statisticians might object to that. In imaging, for example, a data point can be a thousand by a thousand pixels of information.

Lindley: That is a different problem from the one I was commenting on. In imaging there are very high correlations and the problem is to utilize these. I was thinking of situations with much less structure to the quantities.

Smith: Doesn't this kind of task partly define the agenda for 21st century Bayesians?

Lindley: Yes. At the moment we do not have the material for handling high-dimensional problems. We know little about how to formulate a distribution in four dimensions. A natural way to do it is to think about quantity 1; then 2, given 1 and so on in sequence. That is not too difficult, but the result has got to be the same as if it had been done in a different order. A real contribution here is the directed graph with its strong assumptions of independence.

Smith: Even more difficult if, for example, the successive quantities are curves!

Lindley: Yes, even the description of your views about a single curve is hard.

Smith: But is it one that can be isolated as part of a purely statistical agenda? Doesn't this mean that we have to be sitting next to the electrical engineer, remote sensors and so on?

Lindley: Many advances in knowledge have come through the interaction of two, apparently disconnected, disciplines, so I wouldn't be surprised if what you say is true. Statisticians should always be talking to other people. We are in a peculiar situation because we are dealing with data that we have not produced. Statisticians do not always recognize their role as servants. When we talk about prior distributions, we should really mean the *scientist's* prior, not ours. Statisticians do not have priors. Our task is to help the scientist formulate their prior.

Smith: In the socioeconomic context, what do you think is going to be the interesting future agenda for Bayesian statisticians?

Lindley: An interesting question. Part of the agenda is to develop methods to help people formulate their probabilities: how to think about a situation in probability terms. I am doing an example at the moment about the Romanov dynasty and what happened to it. How do I formulate a probability structure for this problem? Some probabilities are easy, like those for DNA. Others are difficult: did that woman escape? did they bury the Tsar there? How do I think coherently about such things; for coherence is the essence of the argument. A related problem is the elicitation of utilities. We are like people with a concept of distance who have not developed tools for measuring distances. We have Euclid but not the theodolite.

Smith: A lot of basic, decision theory is predicated on an individual decision-maker. Where does Bayesian thinking stand when you have several participants, potentially in conflict, or playing games?

Lindley: Nowhere. The Bayesian paradigm is severely limited to a single decision-maker. One of the world's most difficult and important problems is to extend the Bayesian argument to decision-makers in conflict: to have an axiom system for conflict. The only results that we seem to have in that field, so far as I know, are that it can't be done —Arrow's impossibility theorem, for example. There has to be a way out of this impasse. It may be that the Bayesian approach can help in some modification of the problem, but we often get into the infinite regress, where you are thinking about what I am thinking about what you are thinking. My family say that I see Bayes everywhere. Almost true, but not with conflict.

WINE AND ROSES

Smith: What do you do when you are not thinking about statistics? Your family say that Thomas Bayes appears to you everywhere, so perhaps you are always thinking about statistics?

Lindley: I have always been an active gardener. It is good to work off my frustration by digging in the garden, and I enjoy eating the products.

Smith: Do you think the seeds of that were sown in helping your schoolmaster Meshenberg with his vegetables?

Lindley: Yes, maybe they were.

Smith: Are you into utilitarian gardening—things to eat—or aesthetic gardening—growing roses?

Lindley: I'm mostly interested in things to eat: fruit and vegetables. Mind you, fruit trees are beautiful things—"To see the cherry hung with snow."

Smith: Wasn't that a line of Housman you quoted when you notified colleagues of your retirement from University College?

Lindley: Yes. The first year after retirement I saw the cherry hung with snow three times: in Australia, California and Canada. I thought that was a real achievement, three years in one.

Smith: Besides gardening, I happen to know you are interested in wine. You haven't succeeded in keeping statistics out of that particular interest, have you?

Lindley: No. The problem vexing me at the moment is how reliable are the tastings, the results of which are reported in both the wine press and the ordinary press. The wine trade does not provide enough data for the statistician to work out the standard errors. I have tried, unsuccessfully, to get this information, but the trade appears reluctant to give it to me, perhaps because something unpleasant might be revealed.

I have data from one well-designed experiment in which 11 tasters tasted 2 sets, each of 10 wines, blind and scored them according to the recognized system from 0 to 20. It is a straightforward 10×11 analysis (done with Bayes factors, not significance tests). The standard error on the scale 0–20 is a little under 3. If this is general, then the consequences are serious. Typically wine tastings are

FIG. 7. *London, 1993, 70th birthday meeting; with Peter Lee, Jeff Harrison and Morris Walker.*

concerned only with quite good wines. You hardly ever get a score below 12, for example. At the other end, a score above 18 is unusual. So you are in the range 12–18 with a standard error of almost 3 —well!

Smith: Uniform experts!

Lindley: I may be wrong and they can do the job much better than one experiment suggests. My own experience with one case supports my doubts. One bottle was corked and two of them were definitely a bit funny, but the remainder were lovely. Part of this is due to the circumstances under which they were drunk: one was on a festive occasion, another to cheer me up. So it is hardly a controlled experiment. I have got quite interested in this.

Smith: Dennis, this has been fascinating, but if we are to do some practical wine-tasting, as opposed to theorizing about it, we must stop for lunch now. Thank you!

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