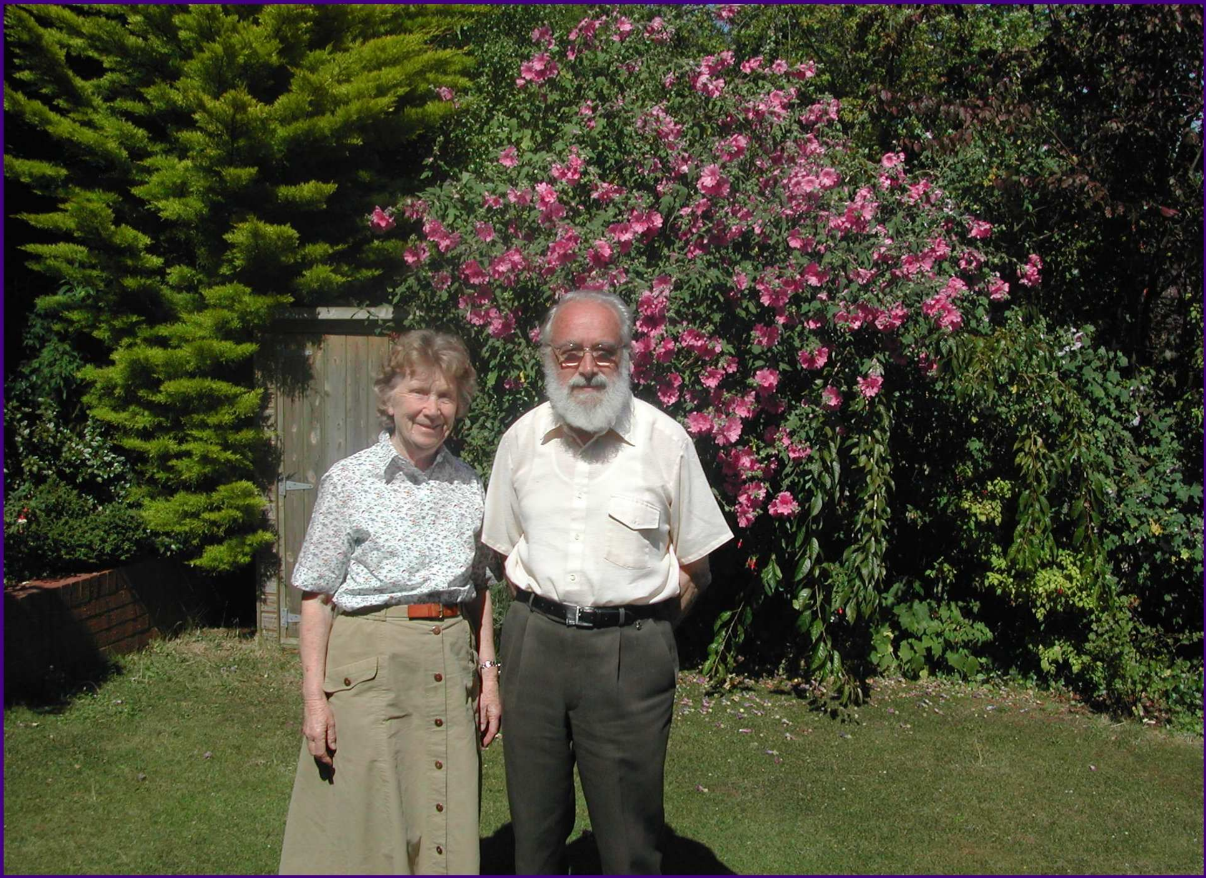
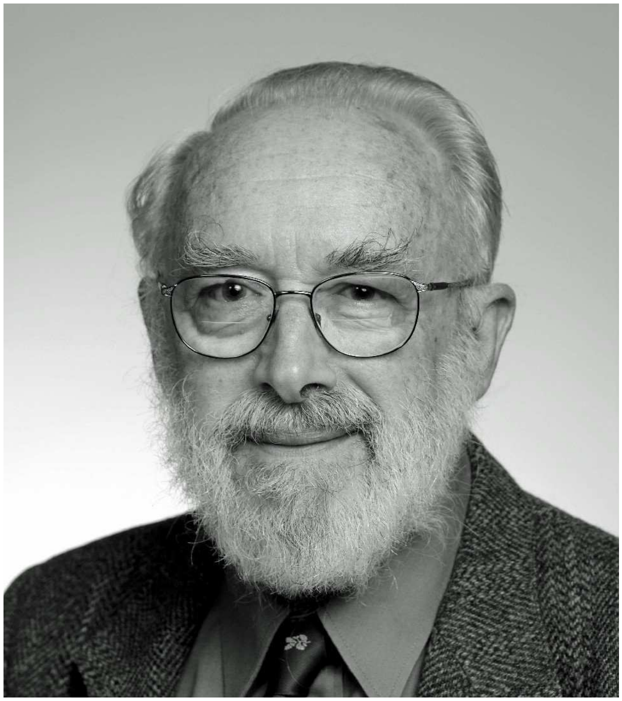
A Book for Dennis

To Dennis Lindley in celebration of his 90th birthday, 25th July, 2013



A Book for Dennis

To Dennis Lindley in celebration of his 90th birthday,
25th July, 2013



Dennis Lindley, c.2004

Preface

This book is presented to Dennis V Lindley on the 14th of August, 2013, at a lunch in his honour at the Castle Hotel, Taunton, to celebrate his 90th birthday.

More than fifty of Dennis's friends and collaborators over his long and productive career have written contributions to this book. They testify to the influence that Dennis has had personally on them, and are replete with happy memories.

Some contributors have provided photographs, and for several we have obtained nostalgic pictures of them around the time they would first have met Dennis.

This book has been constructed using the online publishing tool blurb.co.uk. Blurb is excellent for producing high quality one-off books with text and images. It is unfortunately not designed to handle mathematics, so I have adopted a variety of editorial compromises where contributors have used mathematics. I hope that this has not been too much to the detriment of those contributions.

Tony O'Hagan
June 2013

Elja Arjas

Dennis Lindley 90, an appreciation

My encounters over the years with Professor Dennis Lindley, either in person or by correspondence, are likely to have been less frequent than those of most other contributors to this collection. But they have all been memorable, and are much appreciated. Here is a brief account of them.

The first contact must have been forty years ago, either in 1972 or 1973 in Leuven, Belgium, where I spent a year as a post doc at the Center for Operations Research and Econometrics (CORE). Lindley came there and gave a guest lecture with the somewhat surprising message, as I recall it: "Do not read my (1965) book, where use of non-integrable priors is recommended". At that time CORE was one of the few places where Bayesian methods were taken seriously and successfully applied in econometrics. I, on the other hand, had no real knowledge of Bayesian statistics and could therefore not quite appreciate the significance of Lindley's warning. When at CORE, I was working in applied probability and on queuing problems, and his name had become mainly familiar to me from the concept of the 'Lindley queue'.

In chronological order, the second contact was almost thirty years later, in 2001. In my role, then, as Joint Editor of the International Statistical Review I had come up with the idea that some of the journal issues could be dedicated to introducing its readers to some particular topic of statistical methodology where interesting developments had taken place recently. Obviously, the best way to realize such a plan was by inviting some well-known experts in the selected area to contribute an article. As I recall, the idea of inviting a contribution from Lindley for an issue dealing with causal inference came from Nozer Singpurwalla. The result was a paper entitled "*Seeing and Doing: the Concept of Causation*" (2002), written as an extended review of Judea Pearl's then recent book "*Causality: Models, Reasoning, and Inference*" (2000). In addition to providing such a review, this paper, in only seven pages, offers a masterful summary of the central ideas relating to causal inference. My colleague Anders Ekholm in Helsinki once remarked to me that it was only after he had read this small note by Lindley that he could actually understand the core content of Pearl's book.

The third contact arose when I was asked by Juha Alho, Editor of the Scandinavian Journal of Statistics, to give a special invited 'SJS Lecture' at the 23rd Nordic Conference on Mathematical Statistics which was to be held in Voss, Norway, in 2010. When preparing for the talk I found that 33 years earlier, in 1977, Lindley had given a series of invited lectures at the 7th Nordic Conference on Mathematical Statistics, which had been held in Ystad, Sweden. I had then missed the conference and thereby also these lectures. Fortunately, however, SJS published a year later, in 1978, a comprehensive paper entitled 'The Bayesian approach', based on these lectures of Lindley and on a discussion that followed.

This exchange of views with the prominent Scandinavian statisticians at the time offered a good starting point for the preparation of my own talk. Into the manuscript I wrote: "Lindley made a bold attempt to bring 'the good news' of Bayesian inference to the Nordic Countries … But, as it turned out, the Vikings attending the Ystad conference were not to be convinced so easily …" In response to my having sent the manuscript to Nozer, he wrote to me in an email: "A copy of this paper needs to be sent to Lindley. Remember, he too is a Viking!" So, this is what I did.

Much to my delight, I received a letter from Lindley, dated 18 March, 2011. The letter contained several comments on my manuscript, all very much to the point. It seems that the only issue of disagreement between us concerned the prediction about statistical paradigms in 2020. That year being, at the time of the Voss conference, only ten years ahead I had felt confident enough to say: "The well known prediction (de Finetti, Lindley) according to which the statistical world will be Bayesian in 2020 is not going to be true, and it is unlikely to be true even in 2050!" Lindley responded to this in his letter by "…I think you are pessimistic in your first conclusion … 2020 is not impossible". Indeed, the main reason for my pessimism was that, as I had written, "frequentist methods are easier to use …" and "… the main reason behind the enormous popularity of *p*-values is that most people (= non-statisticians) who make use of them think that '*p*-value' = Pr(null hypothesis | data)." In other words, without realizing this, they are applying a Bayesian concept when interpreting a frequentist test result.

What then followed, a year later, becomes clear from the following letter:

Dear Prof. Lindley, Helsinki, March 18, 2012

I have had bad conscience about not having thanked long ago you for your kind letter dated exactly a year ago, on March 18, 2011, in response to my paper "*Future directions in statistical methodologies; some speculations*", which was later published in the Scandinavian Journal of Statistics (2011).

In your letter, apparently on noticing from the Acknowledgements of this paper that I had spent some time in Bristol in the spring of 2010, you wrote as follows: "It was a pity we did not meet during your stay in Bristol, which is only about 50 miles from here."

I will actually return briefly to Bristol in order to take part in a workshop called "Time for Causality", April 10 – 13. After having picked up some courage I decided to write this letter to you, to ask whether such a possibility would still exist. It would be a great pleasure, as well as honour, to come to Minehead for a brief meeting with you. Any time on Saturday, April 14, after the workshop has ended, would be convenient for me.

If this were possible, I would like to take with me Dr. Olli Saarela, my last PhD student who is now a post doc at McGill and whose presentation at the workshop, stressing the importance of exchangeability ideas in causal inference, was largely inspired by your paper with Novick (The Annals of Statistics, 1981).

Sincerely,

- Elja Arjas

And – no surprise there – again a letter arrived from Lindley, with a warm welcome to Minehead, and containing detailed instructions on how to get there from Bristol on public transportation. As Olli and I stepped down from the bus at the town centre, we didn't need to look at the Minehead street plan to find the way to Quay Lane because our host was already waiting for us, sitting in his electric four-wheeler. (I had actually some vague remembrance of Minehead from a time fifty years earlier: in the summer of 1962, after my first year at the university, I had worked, for two moths, in the local Billy Butlins holiday camp in the respectable position of unskilled kitchen porter. The reason for going there was that I needed to improve my English, to be able to read my statistics text books. Somewhat disappointingly, the kind of language skills I learned in Butlins kitchen was of little help in that respect.)

Olli and I were invited first to "Woodstock", and then to a superb lunch in the nearby restaurant, enjoying both good food and excellent company, as witnessed by the two pictures here.

There are many aspects in Dennis Lindley which I admire. He has had a pioneering position in the revival, and further development, of the Bayesian methodology in statistics. But more than anything else, I admire his uncompromising, absolute honesty, both in science and in life.

It is a pleasure, and an honour, to congratulate Dennis Lindley today.

Peter Armitage

Dennis at 90

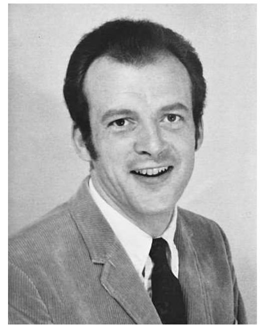
When Peter Freeman invited me to contribute to this compendium, which of course I do with great pleasure, he reminded me that I had written an introduction, *Dennis Lindley: the first 70 years*, to the volume *Aspects of Uncertainty* published in 1994. In that essay I had tried to sketch his career so far, but also to bring out the salient features of his approach to life – his deep conviction that Bayes was the way forward, his resolute rationality, the clear distinction he draws between ideological disagreement and personal antipathy, his deep devotion to his family, his contentment with life outside the metropolis, and so on.

So, has anything changed in the last 20 years? Well no, not really. It would have made a much better story line if one could recount Dennis's renunciation of the likelihood principle, his support for the Conservative Party and the Catholic Church, his fierce attacks on his erstwhile fellow-Bayesians, his wayward and dissolute lifestyle, and his move from Minehead to Islington. But, as far as I know, none of these events has taken place. As Dennis might say, no new paradigm shift here!

I am happy, therefore, to greet the Dennis I have known for some 70 years: the fellow-student at Trinity; the colleague in SR17, setting a war-damaged world to rights from an office in Baker Street; walking in the Lake District with (among others) my dear cousin Joan, shortly to become Mrs Lindley; seeing Dennis emerge as a world-leader in statistical theory and a dominant force in the UK statistical community; admiring the way in which he reinvented retirement and continues to wield the axe. Above all, I treasure the all-too-infrequent opportunities to meet – now mainly on family occasions or transient calls from holiday destinations. May they long continue,

Donie and I send our love and good wishes to Joan and Dennis, and congratulate Dennis on this latest and most impressive anniversary.

Anthony Atkinson



The choice of a model; for Dennis Lindley on his 90th birthday

For those of us fortunate enough to attend the meetings of the Research Section of the Royal Statistical Society in the nineteen seventies and eighties, the memories of your contributions remain sharp and enjoyable. The lecture theatre of the London School of Hygiene and Tropical Medicine had a steep rake. As a presenter and discussant your clear handwriting and cogent presentation of arguments was impressive, even from the back rows. In Isaiah Berlin's famous categorisation "the fox knows many things, but the hedgehog knows one big thing", you were certainly a hedgehog. Some of the foxes had less clear handwriting and, as is the way with foxes, a more diffuse viewpoint. Much was to be learnt by the then young statistician from this dialectic display.

My own contact with you in this setting was about a 1974 read paper, coauthored with David Cox. The subject of the paper was "Planning experiments for discriminating between models". With sequential experiments it is desirable to have posterior weights for the various models that avoid wasting effort on learning about poor models. On the other hand, it is necessary to keep learning about such models until the inadequacy is reasonably established. Particularly if the models are close, the standard Bayesian analysis does not behave like this.

Your discussion lucidly analysed the behaviour of the posterior probabilities of the models when the choice is between a constant and a straight line in one variable. You emphasized the importance of correctly formulating the problem, including the purpose for which the chosen model was to be used. When there is only a small difference between models, your Bayesian analysis favoured the simpler model, which you argued is correct if the chosen model is to be used for prediction. The connection between these Bayesian arguments and other methods of model choice, such as AIC and BIC, was presented in 1980 by Adrian Smith and David Spiegelhalter in another JRSSB paper.

The other personal scientific contact that I recall was when you were external examiner at Imperial College. My examination paper included a question on decision theory. I recall you thought that I had taken expectations at the wrong point in the argument. A clear description of how the problem should have been formulated and solved was finished by saying something to the effect that "Only a small change to the question is needed. I make the comments to help alleviate the boring business of examinations". This is a minor example of the feeling of intellectual energy and fun that I find reflected in your 1995 conversation with Adrian Smith in Statistical Science.

A lasting memory of that conversation is of the discussion of early retirement, an option that came and now seems to have gone, but which I still recall when the subject comes up. Your comment that "generally speaking, the older scientists are a drag on the place because they cannot keep up with modern work" appears to have been, if not ignored, at least not acted on. The current trend seems to be to stay on a few years longer than 65, particularly in the United States, where retirement has long been voluntary (and pension provision perhaps patchy).

I was surprised to receive Peter Freeman's invitation to contribute to this collection, as we were neither colleagues nor have I seen you since you began your long retirement. However, at the time of writing this in the spring of 2013, I have recently been clearing my final filing cabinet at the LSE (I retired at 65). I found several copies of the tables of Lindley and Miller, including one that I must have bought in my first term at Cambridge. I confess I have no recollection of what statistics I was taught in Part I of the Natural Science tripos, except for a lecture in which the word BLUE was repeatedly written on the board. David Cox reckons that he was giving an undergraduate course at that date, but not to the natural scientists. Even so, the name Lindley was well known to me, long before I started the transition from chemical engineering to statistics.

To finish with something trivial, which is the puzzling response you made to someone who had written to you as 'Denis'. I must have heard this second-hand, but you were reported as saying "I am a statistician, not a fire engine". However, like yours, the Dennis company's name contains two n's. If the website is to be believed, they continue to flourish as producers of buses and fire engines. Like Bayesian (and other kinds of) statistics, the main areas of expansion are stated to be the UK, Asia and North America.

So, finally, Happy Birthday! The Polish greeting is 'Sto Lat' (100 years). You are already almost there. I hope the ten years to go and those that follow give you great joy.

Richard Barlow

It is an honor to be asked to write a few words in honor of Dennis Lindley's 90th birthday. I do try to remember his birthday each year.

My introduction to Dennis was triggered by the time I spent the 1975-76 academic year at Florida State University in Tallahassee. My purpose was to complete a book on Statistical Reliability Theory with Frank Proschan. At the time, I was working on total time on test processes. At the same time, I started attending lectures by Dev Basu on statistical inference. It was Lehmann's hypothesis testing course and Lehmann's book was the text. However, I noticed something strange - Basu never opened the book. He was obviously not following it. Instead, he was giving a very elegant, measure theoretic treatment of the concepts of sufficiency, ancillarity, and invariance. He was interested in the concept of information - what it meant - how it fitted in with contemporary statistics. As he looked at the fundamental ideas, the logic behind their use seemed to evaporate. I was shocked. I didn't like priors. I didn't like Bayesian statistics. But after the smoke had cleared, that was all that was left.

Although Basu was a deep thinker, he gave no indication on how to use Bayesian statistics. Dennis on the other hand had written books on how to use Bayesian statistics. On returning to Berkeley, I decided I must have Dennis visit Berkeley so I could learn how to use Bayesian statistical thinking. Using an ONR research grant I invited him to come and lecture at Berkeley. He graciously agreed and gave some of the finest lectures I have ever heard. Students from the statistics department as well as the IEOR department in the college of engineering came and enjoyed the lectures. Subsequent Ph.D. theses in both departments incorporated Bayesian thinking in their dissertations.

After Dennis's first visit to Berkeley, I had many contacts with him at conferences and other venues. Since my research interests included primarily mathematical reliability, I am happy to say that Dennis also participated in some conferences on this subject that both of us attended. I especially remember when Dennis was the featured speaker at a conference on Accelerated Life Testing and Experts' Opinions in Reliability at the Villa Marigola near the seaside city of Lerici, Italy in 1986. He gave his usual excellent talk; however, one of things I remember most is his unhappiness with the Italian habit of eating dinner after 8 P.M.

I also remember attending a dinner in his honor at the University College London in September 1993. It was a very memorable event featuring not only Dennis but also some excellent wines. I am sure he appreciated them.

I will let others recap his considerable research work but I must comment on his important work in disseminating the Bayesian approach. His latest book, "Understanding Uncertainty", is an important contribution to that effort.

David Bartholomew

Dennis Lindley: Recollections and Reflections

I first met Dennis when he invited me to give a seminar in the Cambridge Statistical Laboratory in the academic year 1959/60. This was based on work I had published in Biometrika, and, as I later discovered, Dennis had been a referee. He introduced me to a PhD student, Roger Miles, who, Dennis thought, might be able to prove a conjecture I had made in that paper. (Roger subsequently proved the conjecture and also, later, came to Aberystwyth.) Not long after, I received a letter from Dennis saying that he had been appointed as Professor of Statistics and Head of a new Department of Statistics in Aberystwyth and enquiring whether I would be interested in a lectureship there. This was a surprise on several grounds but Dennis could not have known how timely the invitation was. I had been at Keele nearly three years and found it rewarding in many respects but it had become clear that I could not make a career there in main-stream statistics. Although I had attended seminars regularly in Bartlett's department in Manchester there were very few other Statistics Departments in the country and the prospects of finding a post in a main-stream Statistics department therefore seemed remote. I duly went for an interview in Aberystwyth and was offered the job, which I accepted.

Aberystwyth was remote geographically and academically and although the experiment of planting a new department there ultimately failed, I believe it has left a lasting and positive legacy for which Dennis is almost solely responsible. The intention was to begin with a postgraduate Diploma in Statistics modelled on the Cambridge pattern. Dennis and I, supported by Donald East from Cambridge, were to begin preparations in January 1961 for the first intake of students the following October. In October we were joined by Mervyn Stone and Ann Mitchell. Looking back it all seems to have happened with a smoothness and inevitability which, in retrospect, seems surprising. I do not remember there ever being any formal departmental meetings or minuted decision-making of the kind which would be considered essential nowadays. Dennis, of course, was the link with the College at large and he must have borne the brunt of whatever politicking there was.

The initial success of the enterprise would depend, most obviously, on being able to attract enough able students, adequately funded, to make the Diploma viable. Equally it needed to be possible to maintain academic links with the rest of the world by establishing two-way contact through lectures, seminars and so on. This was at a time, of course, when there was no electronic communication and when letters and papers had to be typed by a secretary. London was the centre of statistical activity and there was no way that the round trip could be done in a single day. Dennis needed to be in London fairly often on RSS business. This could be done by driving to Carmarthen in the evening to catch the night sleeper to Paddington, then breakfasting at Paddington station before the day's work began. The procedure was repeated in reverse the following night, leaving the day between free for work. Dennis could sleep on trains and his working life depended to some degree on possessing that rare skill.

Any decision to move to Aberystwyth involved the balancing of many factors, cultural and social as well as academic. For students the decision was short-term and the academic benefits were calculable and immediate. For staff, the long term was more important and the balance of advantage almost inevitably shifted with the passage of time towards moving on. It is a tribute to Dennis that he attracted the staff necessary to form a critical mass of people who stayed together long enough to establish the reputation of the new department. I was the first to leave, moving to the University of Kent at Canterbury in 1967 and Dennis himself was appointed later to the (then) most prestigious chair in Statistics at University College London. Mervyn Stone accompanied him and Ann Mitchell moved to Imperial College. In a short space of time therefore the original complement of staff had left. The department continued for a number of years after that but, eventually, was amalgamated with the other Mathematics departments. The original members of staff were re-united at a conference to mark the dissolution of the Department as a separate entity several years later. These events served to show how dependent the Department had been on its one star member.

One of the practices which Dennis introduced in the early days was a regular staff meeting at which we would take it in turns to read and present to one another, the substance of a recently published research paper. This was an admirable, if sometimes testing, educational exercise which, unfortunately, did not survive the growing demand for our services. I also re-call Dennis explaining to me how to write a useful referee's report on a paper – actually how he did it, but the implication was obvious!

I am sometimes asked what it was like working in such close contact with the arch-apostle of

Bayesian statistical inference. There were, of course, benefits and costs. In the early days, the two volumes of *Introduction to Probability and Statistics from a Bayesian viewpoint* existed only in duplicated form and any one strong enough to lift its considerable weight was invited to comment on it. Dennis believed in, and the first volume exemplified, the principle that mastery of the elements of the probability calculus was essential for students in statistics. This is something which I took on board and have propagated ever since, not least among social science students wishing to move in a statistical direction. This is even more necessary and, paradoxically perhaps, more feasible now that computer simulation of random processes is readily available. Bayes' theorem and its ramifications occupied the second volume of the work but I was never convinced that this was the royal road to knowledge. I think the explanation for this difference is as much a matter of one's general world view as of the relevance of the mathematics. In the best sense of the word, Dennis is a fundamentalist who was searching for the key which would put statistical inference into the same class as mathematics. That key was to be found in Bayes theorem which introduced an essential simplicity and rigour into what seemed to be the incoherent ramblings of contemporary statisticians (in those days, at least). I, on the other hand, am a pragmatist who notes that everything is conditional on the model, or the framework, in which one chooses to work. This introduces an inevitable arbitrariness into inference, but that is another story! In practice, the Aberystwyth department had no 'party line' and there was no pressure whatsoever to toe the Bayesian line either in research or teaching. There was, of course, lively and frequent informal discussion of inferential matters but this was conducted as the normal exchange of equals (even if we were not equal!). In my own case, I have some regrets that this led me into areas of research which, ultimately, proved to be relatively unprofitable.

I owe a particular debt to Dennis for initiating and encouraging me to visit Harvard University for an academic year. He had visited the Harvard Business School and had the personal contacts with Fred Mosteller and others which enabled me to fill the slot created by Bill Cochran's leave of absence. This was an enormously rewarding experience. At the time, things like this were too easily taken for granted. It is only on looking back, that I realise how much Dennis contributed to my development by arranging this and also by facilitating and supporting my application for a senior lectureship. This concern with individual development as well as immediate departmental interests was one of the great strengths of Dennis' work at Aberystwyth.

Aberystwyth was a small town with a large academic community, which meant that many of one's neighbours were university people. I had moved from a small campus-style university (and went on to Canterbury, which was another) where the dividing line between one's academic and private life was somewhat fluid. This could lead to tensions but, whether by design or default I do not know, Dennis respected what is nowadays called one's social and personal space and did not needlessly intrude.

My last academic contact with Dennis was after he had moved to UCL. After he arrived, the Department there needed an external examiner for its undergraduate examinations. The only staff remaining there had been rather junior in my time there but, as a former student, Dennis felt that there might be something reassuring about having someone they already knew.
It is now my lot to read drafts of many of what the RSS now calls 'pre-obituaries' of Fellows of the RSS. One quickly learns to recognise the euphemisms and circumlocutions with which writers seek to hint at the inevitable shortcomings of their subjects. There is no need for dissimulation in the case of Dennis. Unlike many distinguished academics, who are often seen to possess the qualities of prickliness or arrogance, almost as if that were something which 'goes with the job', no such concealment is necessary with Dennis. What you see is what you get – a distinguished statistician who is easy to get on with at close quarters whether you agree with him or not. As one who is not that far behind him in years, I am encouraged by his longevity and look forward to the celebrations when he reaches his century.

Jim Berger

Some reminiscences

On the occasion of Dennis's 90th birthday, it is delightful to reminisce about the many wonderful interactions I had with him, and the impact he had on my statistical development. Many of us in the US and around the world were major beneficiaries of Dennis's early retirement in the UK, as he was then able to embark on extensive travels, spreading the Bayesian message. I would not only frequently encounter him at meetings, but several times was able to schedule visits by him (and Joan) to Purdue University.

When I first encountered Dennis, I was a self-taught Bayesian, and not a very coherent one. (Yes Dennis, I am much more coherent today than then, even if I still have some ways to go.) I was also missing many of the key insights that a Bayesian needed. For instance I recall once being amazed in a discussion with Dennis to learn that multiple testing is not an issue with Bayesians; that it is automatically handled in the assignment of prior probabilities to the hypotheses. I lately returned to this subject, upon realizing that this was necessarily true only for true subjective Bayesian assignments of prior probabilities; indeed, common objective choices, such as the assignment of equal model probabilities in the problem of variable selection, do not control for multiplicity. (This was yet another example to me of Dennis's mantra that one needs to do Bayesian analysis properly to avoid getting into trouble.)

On his first visit to Purdue, we were not only delighted by Dennis's talks and discussions, but also by the journal he brought along which recorded details of his visit to Purdue in the 1950s. He was able to tell us much that we did not know about the situation with statistics at Purdue in that era. I promised myself I would take up his delightful habit of keeping a journal of visits but, alas, did not manage to keep to it.

I was also lucky enough to be taking a sabbatical at Duke University in 1988-89 at the same time that Dennis was visiting Duke for a semester. Being in the same place for an entire semester was quite a thrill for me. One amusing story (in retrospect): The statistics department at Duke was just beginning then and, although it was to have a Bayesian orientation, the acting head was a non-Bayesian econometrician. Not understanding that

Dennis was an enormously eminent statistician, the head assigned Dennis a large elementary statistics course to teach, rather than the expected graduate seminar. Well, being the good guy that he is, Dennis went ahead and taught the course, to wide acclaim by the students as I recall.

Dennis and I had a common interest in foundations. I was struck early on by Lindley's Paradox about testing – that, with a very large sample size, a Bayesian and a frequentist could be nearly certain about the truth of a hypothesis, but with the Bayesian being certain it is correct and the frequentist certain that it is wrong! Seeking to understand the effect on practice of this paradox led to several of my papers on robust Bayesian testing. The importance of the likelihood principle and conditioning in foundations was something else I learned from Dennis; exploring this led to my book with Robert Wolpert on the subject.

Although I still persist in occasional 'objective Bayesian heresies,' I have learned that Dennis was right in arguing that one should strive to be as purely Bayesian as possible. Things that look Bayesian but are not, such as fiducial inference, posterior predictive p-values or various cross-validation procedures using Bayesian measures, fail to measure up. (One of Dennis's early papers gave a wonderful demonstration of the incompatibility of fiducial inference with probabilistic reasoning.) I do enjoy showing that sound Bayesian procedures are also often optimal conditional frequentist procedures, but Dennis will reasonably question the wisdom of even that. In discussion of a paper of mine in which it was shown that the posterior probability of a hypothesis is the same as a certain conditional frequentist error probability for the hypothesis, Dennis amusingly observed that this only serves to confuse the issue; as Bayesians we have worked hard to convince people that $P(A \mid B)$ is not the same as $P(B \mid A)$, so why undo all that good work?

Of course, Dennis himself was not above a little heresy when needed. I recall two wonderful talks he gave at Purdue: the first on foundations of Bayesian statistics and why one must be a subjective Bayesian; the second on a genetics problem involving Hardy-Weinberg equilibrium, with Dennis utilizing a noninformative prior for the Bayesian analysis. When I asked about the contradictory message in the two talks, he simply observed that one must strive to be a subjective Bayesian but, if the circumstances don't permit (here the geneticist was unwilling to specify a subjective prior distribution), then one must stay as close as possible to the ideal.

Everyone who has interacted with Dennis knows he is a delight to be around. In addition to the many social interactions during his visits, I also had the pleasure of serving with him on many Valencia conference organizing committees. The organizing committee would typically assemble once at the next meeting site and once in London for the planning of the meeting, occasions involving fascinating discussions of statistics, but also great camaraderie, wine and food. Dennis would be at his most expansive glory on these occasions, leading to a wonderful evening for all.

Happy 90th birthday Dennis, and I’ll try to remember more for your book on your 100th birthday.

José Bernardo

A tribute to Dennis V. Lindley, my maestro

In 1970 I was an undergraduate of Mathematics at the *Universitat de València*, dealing with mathematical statistics for the first time in my life. The official textbook was Cramer's and I was not impressed. However, browsing in the school library, I found the 1965 textbook *Probability and Statistics,* which Dennis had written for Cambridge University Press; I went through it in a solid week and I was really impressed. I then decided to choose statistics, rather than functional analysis, for my two year graduate maths; I read Dennis SIAM monograph *Bayesian Statistics* and Morrie DeGroot's *Optimal Statistical Decisions*… and I became a Bayesian without ever bothering to get any detailed frequentist (mis)-education. For my MSc dissertation I tried to find a link between Bayesian Statistics and Information theory, and a key reference was an old, 1956 paper by Dennis in the *Annals*. I made a three page English summary of my dissertation and mailed it to Dennis with a request for guidance, since no one seemed to have ever heard of Bayesian Statistics in Spain at that time. A couple of weeks later I got a handwritten letter from Dennis; he pointed out that some of "my" results had already been published and a number of shortcomings but, when my morale was rather low, a last sentence said, "but the idea is good: come to London and I will teach you". A couple of months later I was at University College London, with a less than sufficient grant from Spain.

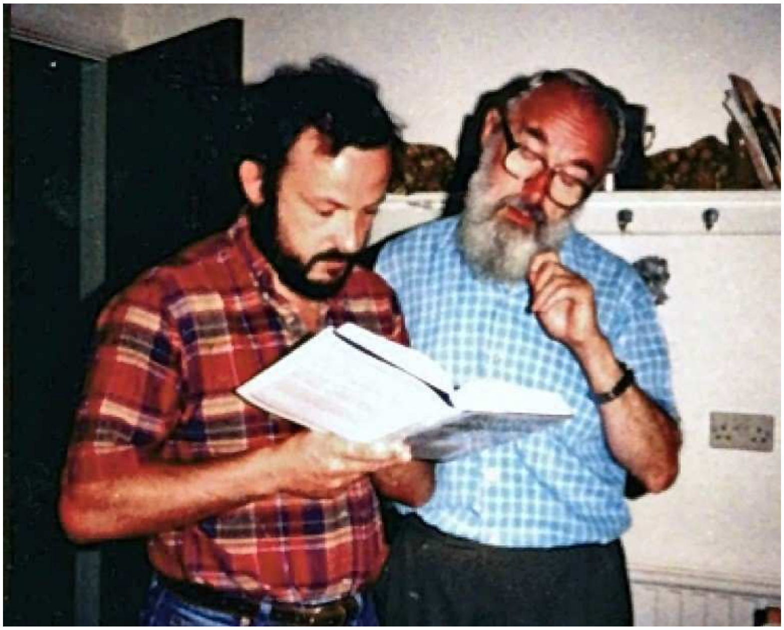
When I met Dennis I was certainly not aware that he was often considered to be the "Pope" of Bayesian Statistics. But, from the very beginning, our relationship was in many aspects that of father and child. He was my *maestro*. His help was absolutely crucial in many forms. He began by helping me in getting a British Council grant for my PhD period. Some months later I was back in Spain by Christmas to see my parents, and I was arrested by the fascist political police: they had found some old passports of mine in possession of a left wing political group who had manipulated them to allow their members to get out to the country, and they obviously did not believe my story that those passports had been stolen from my flat; I was only allowed to make a phone call from the police headquarters and I choose to phone Dennis; his action by the Spanish Embassy in London set me free in only 48 hours. Happily, Franco, the war criminal, had finally died by the time I got my PhD, and Spain reverted to a democracy. I must say that I was also sometimes a bad child: since Dennis was not using his parking space in the central court of UCL and I had my car with me, he allowed me to use his place… until one day, short of time to attend a performance at Covent Garden and blocked by a lorry, I drove over the centuries old lawn. Dennis never liked computing, but he appreciated my aptitudes in that area… until, tired of getting "exceeded CPU time" messages from the computer center mainframe, I hacked Dennis's password, used some weekend hours of CPU time, and was able to verify the (very slow) convergence of my algorithm to find the expected information from binomial data… and I have to say that Dennis was less than happy with my behaviour.

In the summer of 1976, when I was just fresh from my PhD, INSEAD, a French business school located in Fontainebleau, organized a set of Bayesian lectures featuring, among others, both Dennis and Bruno de Finetti. I asked them both out for lunch and, after they had recovered from my apparently too fast driving, I was the privileged witness of a conversation between them on Borel paradox and finite additivity where, over that very French red and white squares tablecloth, I was looking in awe, left and right, as if I were attending a tennis match.

Powered by Dennis's recommendation letter, I got a postdoctoral fellowship at Yale University. Soon after I returned from the States, I was appointed Professor of Biostatistics by the University of Valencia, and I got funds to invite him to give a set of talks in my home town. I got the use an amphitheater with simultaneous translation facilities, and (short of funds) I personally performed as a translator. Dennis appreciated that I was able to translate not only the maths, but also the jokes: the small minority who understood English produced some laughter after his (many) jokes… and this was shortly followed by a much louder laughter when the rest of the audience got the joke through my translation; he then paused to show me a thumb up sign. Later on that trip, I was able to have him try, for his first time in his life, fresh oranges directly taken from an orange tree: he was enthusiastic about it. I also took him to an excursion to Peñon de Ifach, a beautiful peninsula rising some 1000 feet on top of Calp Mediterranean beach; he made the 90 min steep footpath to the top clad in the full formal suit and shoes he always used! It was really nice to have him in Valencia!

There were no Bayesian books in Spanish at the time, and my students were not really good in English, so I translated Dennis's *Making Decisions* (*Principios de la Teoría de la Decisión*, Barcelona, 1977). I intended to also translate the 1965 Cambridge textbook which was so influential for me, but he did not like the idea: he had become by then a convinced subjectivist, and that 1965 book was too "objective" for his 1976 taste.

In a conversation with Morrie DeGroot at Carnegie-Mellon while I was visiting Yale, the idea of organizing for the first time a world conference on Bayesian Statistics was born. I mentioned this to Dennis, who originally had some reservations about the wisdom of the meeting because, "Bayesian statistics is a way of looking as the whole of statistics and not



just a branch of statistics, so that the ideas should not be confined within a group"; he was eventually convinced however that "the advantages from talking about ideas without the influence of frequentists outweighed any sectarian considerations"; when I got his full support for the project through a telephone call, I was really exultant: I then knew this would work. With the crucial help of my young and energetic friend and PhD colleague Adrian Smith, and that of my junior colleagues in Valencia, the first Valencia meeting (held in Las Fuentes, 100 km north from Valencia in May 1979) was organized. This proved to be a great success, and the Valencia meetings were born (some said for a long while that a true Bayesian had to make a pilgrimage to a Valencia meeting at least one in his or her life). Dennis was a member of the Programme Committee for the first three editions of the Valencia meetings, and then chose to step down from the organization; we then asked him to become the conferences President. Valencia 7, held in Tenerife in June of 2002, was a meeting dedicated to him. The Proceedings of the last meeting (Valencia 9, OUP 2011) still contain many references to his important contributions to our field.

I was rather surprised when, at 58, Dennis told me (using a decision tree for illustration) about his decision to leave his Chair at UCL to be free for traveling and research; I did not suspect at the time that I was to follow his path 30 years later. I believe the picture above was taken at UCL, when I visited him during his last months in London.

I have continuously been in personal contact with Dennis ever since I was his student. We have met in countless occasions throughout my professional life, he has often offered me his precious advice on my own publications, and I have had several times the privilege of both being invited to his place, and of hosting him in mine. If I were ever asked to single out the most influential person in my professional life, I would not hesitate: this is Professor Dennis V. Lindley, my dear *maestro*.

Phil Brown

Dennis had moved his Cambridge Statistics Unit to Aberystwyth in wild West Wales sometime before I was urged to study for a Master's in Statistics there. My mentor as an undergraduate in Mathematics at Leicester University, Dr Najib Rahman, was a committed advocate for Dennis. He wouldn't hear of going anywhere else and so two of my fellow Leicester Mathematics graduates and I found ourselves in October 1965 walking up the Penglais Hill in 'Aber'. We were not the first Leicester cohort to have made this choice, such was the power of persuasion of Dr Rahman – I discovered later that Tony Lawrance had also taken a Masters after Leicester graduation a couple of years earlier. Dennis' ability to inspire others was evident from the fervour of Najib's advocacy although at that stage I didn't detect much Bayesian slant to what we had been taught at Leicester. I didn't have much contact with Dennis that year apart from a pleasant evening with fellow students at his home on Penglais Hill. I also don't recall Bayes featuring strongly on the course. The one course I remember Dennis giving was straight out of J. Johnston's book on Econometric Methods delivered by Dennis with panache including occasional characteristic eye rolling. I also undertook a summer MSc project under Dennis' supervision, reviewing the asymptotic nonparametrics of Lehmann and Hodges: not much evidence of Bayes there either. What was undoubtedly impressive was the group of lecturers Dennis had persuaded to forsake the comfort of Cambridge: Mervyn Stone, David Bartholomew, Ann Mitchell, Roger Miles and others. Some of the students were from less mathematical backgrounds and one, Clive Payne, an earlier Masters student with a geography background, collaborated later in work on election night forecasting for the BBC from his employment at Nuffield College Oxford.

By the end of the year I was ready for a break from being a student. Dennis' influence was evident here in that O.L. Davies at ICI Pharmaceutics (now AstraZeneca) in Alderley Edge and Jeff Harrison at the new Head Office Management unit in Wilmslow both offered me jobs. I took the latter but my contact with O.L. Davies recurred a couple of years later when I was ripe to study for a PhD at Dennis' new home, University College London, with a project and data from ICI Pharmaceuticals on drug activity and related chemical structure; a project with subsequent external examiner OL Davies – but more of that later. Dennis' approach to supervision was both sensitive and inspirational. I don't recall feeling overawed or pressured but his approach of reducing a new and complex problem to its barest essentials was pedagogically very effective. We explored and modelled the relation of a binary (active /inactive) response to binary and unordered nominal covariates as determined by the substitutions of molecules in a chemical compound. The resultant *Key* models formed the backbone of the thesis which was written up with Dennis' encouragement and part of it gained the C. Oswald George prize in Applied Statistics (1971) as awarded by the Institute of Statisticians (now merged with the Royal Statistical Society).

I referred earlier to O.L. Davies being the external examiner for my PhD. By this time he had moved back to his Welsh roots occupying the chair vacated by Dennis at the University of Wales, Aberystwyth. Given transport difficulties and train timetables, we broke with tradition and conducted the oral half way at University of Wales, Swansea. Dennis, as internal examiner and supervisor, decided to drive us to Swansea. I remember him as a very particular and careful driver who wore gloves to drive but who was somewhat taken aback by a lorry driver haranguing us while passing through Reading en route. You can imagine that for me it wasn't the most relaxing drive, especially as he reminded me that I could be examined on anything in Statistics, not necessarily coming from my thesis. In the event the examining was an anti-climax that lasted at most half an hour – but with a successful result.

Dinner June 4th 1988 Valencia 3, Altea, Spain. Dennis standing in conversation with Peter Freeman with, from the right, Phil Brown, Trevor Sweeting, Tom Fearn's back

University College Statistics Department, then in the old Pearson building on the left of the UCL quadrangle next to the Bartlett school of Architecture and the Slade School of Art, provided a happy two years for me. It spanned the 1968 student upheavals and was a time of excitement and change. As important for my statistical development were the fellow students that Dennis had attracted to UCL. Contemporaries such as Adrian Smith and Phil Dawid guaranteed incisive discussion of current seminar topics and paved the way for lifelong friendship. In those halcyon days there were London University seminars occupying the whole of Friday afternoons and Royal Statistical Society Research meetings with many an edge to the lively discussions. Dennis was not a shrinking violet steering clear of controversy, in fact he seemed to relish it, and was a strong advocate of the Bayesian paradigm despite often evident antagonism from more traditional sampling approaches. Dennis' intellect also had the effect of attracting many eminent visitors; I recall Mel Novick and Bruno de Finetti. The former was a close collaborator of Dennis' who gave a series of lectures that I attended; the latter of course was an inspirational figure in his own right near the end of his active life but hardly a clear expositor.

I haven't had close contact with Dennis since those days except as a participant in all but one of the Valencia Bayesian meetings (see photo on the previous page). I have continued to value Dennis' inspirational influence and interventions in discussions, both oral and in the form of discussion and letters to the RSS. He has also been supportive when I've needed it, as with my move to the Chair at Liverpool University in the mid '80s.

Rex Brown

Dennis and me

For close to fifty years, Dennis Lindley has been a major presence in my personal and professional life, although we have been on different continents for most of that time. My first memories of him are in London, during the summer of 1964.

But first I must give some personal background. At that time, I was a junior management consultant, purportedly helping seasoned businessmen to make better decisions. To justify this presumptuousness, I sought to contribute some kind of logic to their decision-making process. All I could then find was "classical statistics", which I boned up on (having only a social science degree). I tried to adapt it to modeling judgment, but George Barnard and others derided my efforts as "not real statistics". Then someone observed that I was producing a mangled version of methods being developed at Harvard Business School. This led to an invitation from Raiffa and Schlaifer to spend a year with their decision analysis group as a Visiting Lecturer. They suggested I talk first to a certain Dennis Lindley, who had held that position the previous year.

This is what led me to taking long walks on Hampstead Heath with Dennis, and talking about "statistical decision theory". He also, incidentally, paved the way for what proved an important personal and professional relationship. He suggested I look up Andrew Kahr, a 20-year-old mathematician on the HBS faculty, who was shaking the Harvard academic establishment up with his scathing brilliance. I followed up his advice and Kahr and I have been friends and collaborators ever since. He was best man at my wedding and co-authored a decision analysis textbook, before going on to great wealth as a financial guru on Wall-Street.

Since then, Dennis and I have maintained a close relationship, which has been immensely rewarding to me. In particular, through a number of professional collaborations in the USA and UK, he has put his theoretical expertise at the service of my live problem-solving.

To continue my personal saga: I spent five years at Harvard and then four years at the University of Michigan, where I learned something about how people *do* make decisions from psychologists Ward Edwards and Cam Peterson. I then returned to consulting to try out what I was learning on the "real world". I spent the next quarter century working largely with government policy makers in Washington DC and taking "reverse sabbaticals" in varied university departments to develop complementary technical expertise.

One such interlude was at UCL, where Dennis arranged an SSRC fellowship for me. The nominal purpose of the fellowship was to foster the use of ADT (applied decision theory) in the UK. However, it was also an occasion to take advantage of the brain-power of Dennis and his colleagues in the UCL Statistics Department in the development of decision methodology.

A case in point is what I call "hybrid reasoning". I had observed that effective deciders usually approach any major dilemma two or more ways before coming to a conclusion. Most commonly these included unaided intuition, combined with, say, a probability-weighted utility model and an informal data-based study. (By contrast, academically focused analysts had usually relied entirely on a single approach to a problem – possibly to avoid the professional embarrassment of presenting conflicting results). I understood that decision theory can check if judgments are coherent; but it does not, at least not directly, specify how to resolve any incoherence.

Dennis worked with me on this and related problems; for example, how to allocate resources among multiple approaches. We explored implementations of Dennis's familiar maxims "Coherence is all" and "Inside every incoherent person there is a coherent person struggling to get out", as well as seemingly conflicting maxims, like "Coherence is not enough". (Peter Freeman proposed the useful concept of minimizing "cognitive strain" to fit a super-coherent set of quantified judgments to a decider's messy mind-contents). These interchanges with Dennis and his colleagues helped establish the theoretical basis for a score of hybrid reasoning research projects over the next ten years that attracted more than a million dollars in grants and a score of publications, several of them involving Dennis [1, 2, 3, 4, 5, 6].

Much of our collaboration has involved Dennis developing the theoretical foundations of decision methodology for problems arising from my decision consulting. Usually he would stay with my family and work in my offices in Washington. (When I originally applied for a visa for Dennis that would permit him to work in the USA, I had to convince the immigration people that he was qualified to do the research, in spite of not having a doctorate!)

In addition to contributing valuable research material, my association with Dennis has brought me significant career benefits. For example, he was instrumental in my receiving an applied statistics award for young researchers [7]. The linkage of his name as a world-class scholar to mine also eased the way to publication and research grants that, as an unknown researcher with an unimpressive resume, I would not otherwise have had access to.

One particular incident stands out. In the 1960s, the orthodox decision tree "roll-back" procedure treated a decider's actions subsequent to his primary choice as determined without uncertainty, given intervening modeled events. In practice, by design or inadvertence, the set of *modeled* events is invariably incomplete, so subsequent acts can only be assessed probabilistically. A paper I submitted to *Decision Sciences* to this effect [8] was rejected by the area editor, a prominent decision analyst, on the grounds that it was logically unsound. Dennis intervened with the journal's editor-in-chief, who published the paper as a lead article.

In recent years, since both of us have technically retired, Dennis has continued to support me unstintingly with, for example, detailed technical advice and a generous book review [9]
I am shortly publishing a text-book [10] that embodies what I think I have learned over 50 years in the decision aiding business. It will be dedicated to Dennis as my revered mentor, collaborator and life-long friend*.

References

1. Lindley, D.V., Tversky, A., and Brown, R.V. (1979). On the reconciliation of probability assessments. *Journal of the Royal Statistical Society, Series A*, *142*, 146-180.

2. Brown, R.V. with Lindley, D.V. *Rationality and the Resolution of Incoherence.* Department of Statistics, University College, London. November, 1976.

3. Lindley, D.V. (1985). Reconciliation of discrete probability distributions. In *Bayesian Statistics 2*, J.M. Bernardo, M.H. DeGroot, D.V. Lindley, A.F.M. Smith (Eds.) pp. 375-390. North-Holland.

4. Lindley, D.V. (1985). The reconciliation of decision analyses. *Operations Research.*

5. Brown, R.V. and Lindley, D.V. (1982). Improving judgment by reconciling incoherence. *Theory and Decision*, *14*, 113-132.

6. Brown, R.V. and Lindley, D.V. (1986). Plural analysis: multiple approaches to quantitative research, *Theory and Decision*, 20, 133-154.

7. Brown, R.V. (1968). Evaluation of total survey error. (Oswald George Prize Paper). *The Statistician*, *17*, 335-355.

8. Brown, R.V. (1978). Heresy in decision analysis: Modeling subsequent acts without rollback. *Decision Sciences*, *9*, 543-554.

9. Brown RV. *The Art and Science of Making up your Mind: Wisdom, Rationality and Decision Theory.* New York: Taylor and Francis. In preparation.

10. Brown RV. *Rational choice and judgment: Decision analysis for the decider.* New York: Wiley. 2005.

* It will also be dedicated to two others to whom I am comparably beholden: Howard Raiffa, who shepherded me through my early academic career, and Cam Peterson who, as my consulting boss, then taught me the practical state-of-the-art, at the time, of ADT.

David Cox

Dear Dennis, I am very happy to have this chance of congratulating you on a notable birthday, of admiring your striking contributions to our field and, at a more personal level, of thanking you for your support on all sorts of matters when we were colleagues at St Andrews Hill in Cambridge all those years ago. We first met in the summer of 1950 when after five years of industrial statistics I deserted that field for the academic world.

This was quite a lively period for our subject. I recall your elegant lectures on axiomatic probability theory. Agriculture and industry, rather less so medicine, were motivating sources for much statistical work and the disagreement between Fisher and Neyman and Pearson consumed a lot of time.

Because of the longstanding quarrel that Fisher had with Wishart, the Director of the Statistical Laboratory, formal communication between the Lab and Fisher was at a low level. It was known that Fisher was writing an account of statistical inference. Little was known about its content. In about 1953, when David Finney was leaving Oxford for Aberdeen, Fisher gave a seminar in Oxford on principles of inference and you very kindly drove two research students, Wally Smith and Ewan Page, and me the very long way from Cambridge to Oxford to hear. Quite recently a member of that audience told me that you had driven a group to Oxford with the object of disrupting the meeting; in fact of course we sat in the back mute throughout the rather rumbustious occasion. This was when John Hammersley asked whether fiducial probability obeyed Kolmogorov's axioms. Fisher countered by asking Hammersley to state the axioms; it soon became clear that neither had any real interest in axiomatic treatments. A few years later you effectively answered the question by torpedoing a direct interpretation of the idea of fiducial probability in its general form.

There were a number of interesting brief visitors to the Statistical Lab, including Feller, Norbert Wiener and for a longer period Leo Goodman, but, clearly though, the longer term one with the most impact was Jimmie Savage, who, if my memory is right, came for a period round about 1953-4 carrying the typescript of his book. It was read by you, by our colleague Frank Anscombe and by me. In the initial phases Savage took the line, I think, that the personalistic approach was a very interesting one worth exploring, but this changed a bit later into a much more assertive approach. Our reactions to the work were interestingly different!

Later when you were at Aber and later still at UCL our contacts continued, although less frequently. I recall also your highly effective work for Biometrika Trust, in particular your plans for extending the work of the Trust which unfortunately were considered too imaginative.

Finally, Joyce joins in sending you and Joan our very best wishes.

Philip Dawid

Now that Bayesian statistical inference has become a mainstream activity – even replacing frequentism as the traditional orthodoxy for young folks to rebel against – it is salutary to remember that this was not always so. Indeed, there was a time when Dennis Lindley could have been uniquely identified as "the British Bayesian". The massive change in fortunes for Bayesianism since then could not have taken place without Lindley's fundamentally important research contributions, his hugely influential books, his exemplary leadership in teaching and mentoring young proto-Bayesians, and his lively criticisms of frequentist ideas– though those who knew him only through his strongly argued contributions to discussions at the Royal Statistical Society would likely be unaware that here was a decent, gentle, thoughtful and entirely unopinionated soul, who had simply calculated that the best way to get a decent hearing for his then unconventional point of view was to take each argument to its logical and rhetorical extreme.

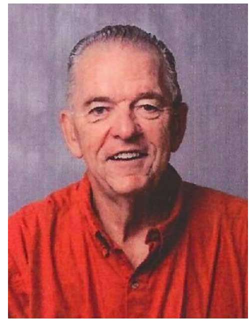
My first, indirect, contact with Dennis was through his two-volume textbook "Introduction to Probability and Statistics from a Bayesian Viewpoint", which I read while studying for the Cambridge Diploma in Mathematical Statistics. The clarity and force of this work rapidly converted me to the Bayesian cause, and eventually led to my becoming Lindley's research student at University College London: one of the happiest experiences of my academic life. Other budding Bayesians I overlapped with there included Phil Brown and Adrian Smith, who have gone on to spread Lindley's influence much more widely.

A turning point of my academic life came when a vacancy arose for a Lecturer at UCL, and Dennis arranged for me to take the job (in those days we could dispense with such boring formalities as advertisements, short lists and interviews – how the world has changed!). He had just set up a new "Advanced MSc" course, and I was to teach the main inference course – to an initial class of two, one of whom soon dropped out. But the course gathered momentum over the next few years, and has been responsible for training a generation of renowned statisticians, some of them even Bayesian. The only problem was that I got so engrossed in teaching that my PhD was not progressing. Here Lindley was no help: to my pleas that he might give me a push to complete it, he replied "Oh I wouldn't bother with that Phil, just concentrate on writing papers". Excellent advice, it turned out, in my case – but again speaking of a very different age from now.

Like the Pope more recently, Dennis shocked us all by the entirely unexpected announcement of his early retirement – characteristically, couched in a quotation from Housman's "A Shropshire Lad". He then disappeared from UCL to set up home in Minehead and travel the world. I am ashamed to admit that the next time I met him, at a conference in San Diego, I did not immediately recognise him, since a large and bushy beard now disguised the fresh face I had known so well. But the sparkling eyes burning through the undergrowth soon gave him away.

Those eyes, like his beautiful copperplate handwriting, are just the outward symbols of a brilliant, charming, warm and utterly delightful individual. Even had he not been such a strong influence on my own life, I would have been proud just to live in a world which contained Dennis Lindley.

A very happy birthday Dennis! But I hope you will now disown the remark you made at your 70th birthday celebration, quoting Oliver Wendell Holmes who, on reaching the age of 90, said "What wouldn't I give to be 70 again".



John Deely

Bayesian Interaction — a Tribute to Dennis Lindley

It is indeed a great honor and privilege, Dennis, for me to be asked to make a contribution to this celebration. You have been such a great inspiration to me and have contributed so much to my life that I could never thank you enough. It seems as if it was just yesterday when I attended your 70th birthday celebration in London. It was indeed a great time. I want to begin by reminding you of one of the happiest moments I remember in our relationship. It happened during my first study leave from Canterbury University in Christchurch during the year 1974-1975 while I was visiting the University of New Mexico in Albuquerque. For the Christmas holidays in 1974 my family and I were taking a trip to Ohio to visit family but we stopped at Purdue on the way. As I was walking through the Statistics Department I noticed an announcement "Dennis Lindley will give a talk at some time and is visiting Iowa State University this year". At that time I knew about you for two reasons. Firstly, of all things, your name came up when Herb Robbins was talking about the Secretary Problem when he was visiting Purdue in 1964 and I was a graduate student finishing off my PhD. It was only one line which went something like this “There is this fellow Lindley in England who claims to have solved the 'expected rank' problem”. The second place I had heard of you will be mentioned below. So after the Christmas holidays when I returned to Albuquerque I spoke with Bert Koopmans, the HOD, and suggested that we invite you to come to Albuquerque if you could. Bert agreed and asked me to be your host. I was more than happy to do so. You were contacted by the Colloquium Chairman and accepted the invitation. You gave a splendid talk as usual. As your host, I collected you from the airport on your first night and took you to your hotel and we went over the plans for the next couple of days. During those days, we went to some secluded spots because you said you like to be away from people. During these conversations I happen to mentioned to you, “Have you ever thought of making a trip to New Zealand?” And now for the happiest words I ever heard, you said “I’ve always wanted to go to New Zealand”. Well, that was it, because we know that you got out there at least twice and possibly a third time (thanks to the Erskine Grants) but we certainly had marvelous memories including the never to be forgotten "at the Gem Resort".   I hope you are laughing. It was during one of those visits in which you had the office next to mine and we were having times to have discussions that I was able to ask you about the second reason I had heard of you, namely your famous statement when you were a discussant of a paper I believe by Copas: "There is no-one more non Bayesian than an empirical Bayesian". As you may recall, I had done some work in empirical Bayes and was really puzzled by your statement. You explained it very clearly to me and through your explanation I found that I became a Bayesian and we produced a well read paper "Bayes Empirical Bayes".   We were able to co-author another paper on survey sampling using prior information in a coherent manner. You have given me inspiration for so much more.

So, what most recently has been going through my mind over the last two years is what I want to dedicate to you. I have always been puzzled and troubled with the definition of interaction in the ANOVA model. So, I have done considerable work applying Bayesian

At Gem resort with Dennis, looking out over Marlborough Sound

Dennis and Joan Lindley with John and Ann Deely at "Stumbles" restaurant, 1991

ideas to ANOVA. Over the last two years I have satisfied myself that I have a Bayesian explanation for interaction that for me is so much more satisfying than the frequentist approach. To illustrate it, I will just use a two factor model which consists of four varieties of corn and three varieties of fertilizers making up twelve cells of data. We are interested in maximum yield and that is the definition of "best". We can compute which corn is best (has the highest mean yield) using posterior probability and find which fertilizer is best using posterior probability. Suppose it turns out that corn 2 is best and fertilizer 1 is best, but when we look at the twelve cells, the cell corresponding to corn four and fertilizer three is better than all of the other cells and in particular better than the cell for corn two and fertilizer one. Whenever that happens we have what I want to call Bayesian interaction. By looking at the probabilities of best treatments, Bayesian analysis allows us to easily investigate and understand interaction effects. These ideas are made more precise in the development that follows this contribution. My appreciation for the computation goes to Glen DePalma, a PhD student here at Purdue, who took my Bayesian course and became an expert in BUGS. He has done a splendid job with the computations demonstrating the ideas discussed above.

I hope this idea appeals to you, Dennis, and that this idea it is not similar to another wonderful experience I had with you when I was visiting George Washington University with

Nozer and you were in the audience. I gave a talk on a subject called Further Assurance and after the talk was over, you quietly came up to me in the front. You just said quietly but with a firm voice "John, I think you are incoherent". So I hope you will find my definition of Bayesian interaction so much superior to that used in Fisherian statistics, and also totally coherent.

Bayesian interaction - John Deely and Glen DePalma

An ANOVA cells means model was used to analyze the data. This model is shown below:

$$Y_{ij} = \theta_i + \epsilon_{ij} \quad i = 1,2\ldots12 \quad j = 1,2,3,4 \quad \epsilon{\sim}iid\ Normal(0,\sigma^2)$$

where θ_i represents the mean of the i^{th} treatment combination and Y_{ij} the resulting yield for the j^{th} observation in the i^{th} treatment. We assume each theta comes from a normal distribution with equal variance. The layout of the data is shown in Table 1.

	Fertilizer 1	Fertilizer 2	Fertilizer 3
Corn 1	$\theta_1 \sim Normal\left(\frac{1}{4}\sum_{i=1}^{4} X_i, \frac{\sigma^2}{4}\right)$	$\theta_2 \sim Normal\left(\frac{1}{4}\sum_{i=1}^{4} X_i, \frac{\sigma^2}{4}\right)$	$\theta_3 \sim Normal\left(\frac{1}{4}\sum_{i=1}^{4} X_i, \frac{\sigma^2}{4}\right)$
Corn 2	$\theta_4 \sim Normal\left(\frac{1}{4}\sum_{i=1}^{4} X_i, \frac{\sigma^2}{4}\right)$	$\theta_5 \sim Normal\left(\frac{1}{4}\sum_{i=1}^{4} X_i, \frac{\sigma^2}{4}\right)$	$\theta_6 \sim Normal\left(\frac{1}{4}\sum_{i=1}^{4} X_i, \frac{\sigma^2}{4}\right)$
Corn 3	$\theta_7 \sim Normal\left(\frac{1}{4}\sum_{i=1}^{4} X_i, \frac{\sigma^2}{4}\right)$	$\theta_8 \sim Normal\left(\frac{1}{4}\sum_{i=1}^{4} X_i, \frac{\sigma^2}{4}\right)$	$\theta_9 \sim Normal\left(\frac{1}{4}\sum_{i=1}^{4} X_i, \frac{\sigma^2}{4}\right)$
Corn 4	$\theta_{10} \sim Normal\left(\frac{1}{4}\sum_{i=1}^{4} X_i, \frac{\sigma^2}{4}\right)$	$\theta_{11} \sim Normal\left(\frac{1}{4}\sum_{i=1}^{4} X_i, \frac{\sigma^2}{4}\right)$	$\theta_{12} \sim Normal\left(\frac{1}{4}\sum_{i=1}^{4} X_i, \frac{\sigma^2}{4}\right)$

Table 1: Data

Talking to farmers, the variance of each observation is known to be 20 which makes the variance of the thetas equal to 5. Theta is expected to be around 125 with a possible range of 115 - 140. Given this information, a hierarchical prior was used for parameters $\theta_1 \ldots \theta_{12}$.

$$\theta_i \sim Normal(\mu_i, 5)$$

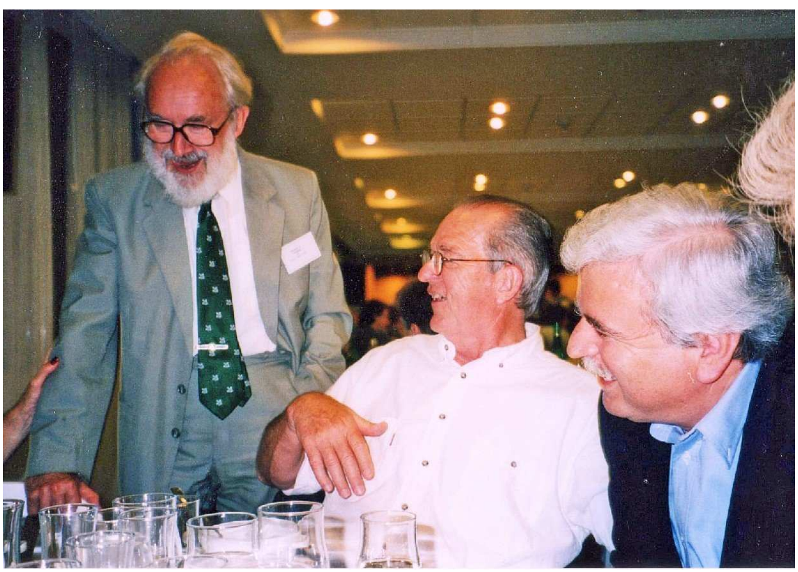
$$\mu_i {\sim}\ Normal(125, 225)$$

The distribution of each corn level is computed by averaging over the three fertilizers. For example, the distribution of corn 1 is: $Normal\left(\frac{1}{3}(\theta_1+\theta_2+\theta_3), \frac{\sigma^2}{4*3}\right)$. In a similar way, the distribution for fertilizer 1 is: $Normal\left(\frac{1}{4}(\theta_1+\theta_4+\theta_7+\theta_{10}), \frac{\sigma^2}{4*4}\right)$.

		Fertilizer			
		1	**2**	**3**	**Corn Effect**
		Mean - Prob	**Mean - Prob**	**Mean - Prob**	**Mean - Prob**
Corn	**1**	133.8 - .02	124.0 - 0	120.1 - 0	126.0 - 0
	2	135.0 - .04	134.4 - .02	130.7 - 0	<u>133.5 - .99</u>
	3	132.3 - 0	116.5 - 0	113.3 - 0	120.6 - 0
	4	131. 1 - 0	115.6 - 0	<u>140.2 - .92</u>	129.0 - .01
Fertilizer Effect		<u>133.2 - 1</u>	122.5 - 0	126.0 - 0	

Table 2: Theta Posterior Mean Estimates and Probability of Highest Yield

Table 2 presents the posterior mean estimates for theta, corn type, and field type. The probabilities of highest yield are also presented. Corn type 2 and fertilizer type 1 resulted in the highest yield with mean estimates of 133.5 and 133.2 respectively, and posterior probability of largest yield equal to .99 and approximately 1. Therefore one would expect the combination of corn type 2 and fertilizer type 1 to result in the highest yield; however this is not the case. The best treatment is actually corn type 4 and fertilizer type 3 having a mean estimate of 140.2 and posterior probability of largest yield equal to .92. This indicates there is a strong interaction effect between corn type and fertilizer type. By looking directly at the probabilities of best treatments, Bayesian analysis allows us to easily investigate and understand interaction effects.



Sharing a joke with Dennis and George Kokolakis at a Valencia meeting. (What a lot of empty glasses!)

Persi Diaconis

Thoughts for Dennis Lindley

Dennis Lindley is a fighter. It is impossible for modern Bayesians to understand how tough 1950s academic life was for a Bayesian of that day. Sneers, jeers, outright hostility were the norm. I think British academics have always been somewhat more direct and honest than others in such debates. Dennis held his own and triumphed. The current British Bayesian scene is world class and his predictions of a Bayesian 21st century are upon us.

I taught my first statistics course out of Dennis' classical set of two books, *An Introduction to Probability and Statistics from a Bayesian Viewpoint*, Volumes I and II. This was a Harvard summer school course with a wide variety of master's level students without previous statistical exposure. The books develop Bayesian theory, but bend backward to make contact with classical statistics through approximations. I still think they are first-class. Evidently, Dennis disagrees; in the 1980s he had a further "conversion" to his version of more radical (Ramsey−de Finetti−Savage) Bayesianism. I met him back then and remember asking, "What should we do with your books?" "Burn them," he said. I'm glad they are still around.

My students fought my Bayesian teaching. One day, I came into class and found some students had been there first. The blackboard was covered with a long rant in two columns. The left one was labeled "Lindley says." The right one, "Classical statistics says." I wish I had taken a picture. I found it wonderful to have a spirited discussion, didn't back down an inch, and kept teaching my Lindley.

Two more tales illustrate Dennis' feisty side. At an early Valencia meeting, I gave a talk on my work with Ylvisaker clarifying the definition of conjugate priors. Dennis was a discussant and seemed outraged by my heavy use of mathematics. He lit into me for a 20-minute harangue. I didn't take it personally but apparently some audience members did. The next day, Dennis got up in public and apologized! The reader can see a cleaned-up version of our exchange in Diaconis and Ylvisaker, "Quantifying prior opinion," *Bayesian Statistics* **2** (1985).

I must add that Dennis has made marvelous mathematics of his own. Before statistics he did queuing and applied probability. His introduction of "duality" and Lindley's equation is still cited and alive. I wrote a survey about Lindley's equation with David Freedman, "Iterated random functions," *SIAM Review* **41** (1999). For duality, see my paper with Jim Fill, "Strong stationary times via a new form of duality," *Ann. Probab*. **18** (1990).

My second tale emphasizes the enormous respect I feel for Dennis. The year was 1981. Brad Efron and I had been commissioned to write a *Scientific American* article on "what's new and exciting in statistics." Originally I was supposed to do graphical methods, projection pursuit, and so on. Brad had just invented the bootstrap and was doing that for his half. I realized how important the bootstrap was and thought that a focused, single-themed article would do more good. See Diaconis and Efron, "Computer intensive methods in statistics," *Scientific American* **248** (1983) for our effort.

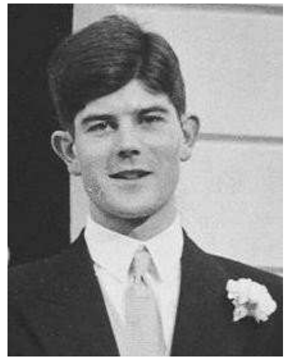
I was working on the galley proofs in a Berkeley coffee shop when who should walk in but Dennis Lindley and Richard Barlow. Richard had just had a Bayesian conversion and was a fanatic subjectivist (there is no one so righteous as ….). They spotted me, and sat down to ask what I was up to. I shamefacedly confessed to my non-Bayesian activities. Dennis was furious and the two of them tried to talk me out of co-authorship. "How could you be part of selling this nonsense?" "It's the worst kind of frequentism…!" I had worried about the issues but the bootstrap seemed like such a directly useful idea that I was sure it would have a Bayesian justification. Dennis wasn't buying it and the two left in a huff.

One other note about that article. I had originally written a section called "How not to bootstrap," pointing out that if the data wasn't a sample, or had dependence problems, the bootstrap was misleading. Brad cut this out, saying it would confuse general readers. A good Bayesian interpretation of the strengths and weaknesses of the bootstrap is still badly needed. Indeed, Brad's recent monograph (*Large-Scale Inference: Empirical Bayes Methods for Estimation, Testing, and Prediction*, Cambridge University Press, 2010) leans in this direction but I don't think he gets Bayes; as Dennis said, "There is no one less Bayesian than an empirical Bayesian."

I've had my goes at Dennis in return. In Diaconis and Holmes, "Are there still things to do in Bayesian statistics?", *Erkenntnis* **45** (1997), I used his frequent "turning the Bayesian crank" to call him "the great crank of statistics." After 40 years of lively interactions he still has my absolute respect. I'll use this occasion as an excuse to look at later papers and try to lead myself and students back to thinking about the foundations of our subject. (I just looked; there are 130 Lindley papers on MathSciNet — many of them are new to me.)

One gift from Dennis will be long remembered. He spent a term at Stanford in the late 1970s. Stanford then was quite anti-Bayesian, at least in the Statistics Department, and I was thrilled to have a fellow traveler around. Dennis introduced me to "a really interesting fellow I know you will like." This was the great psychologist Amos Tversky who was visiting the Stanford Center for Advanced Study in the Behavioral Sciences. Dennis was kept busy working with Amos, and their paper on appropriate practical remedies when you have detected an incoherence in prior beliefs is a classic: "On the reconciliation of probability assessments," *J. Roy. Statist. Soc. A* **142** (1979). Amos subsequently moved to Stanford and became a best friend.

Anthony Edwards



Bayes and binomials

1956-57 was Sir Ronald Fisher's retirement year as Arthur Balfour Professor of Genetics at Cambridge, and the Lecture list for Part II Genetics for the Michaelmas Term listed 'Mr Lindley. Statistical Methods. M. F. 6.' along with lectures by Fisher ('Theory of Recombination') and other members of the Department. I was the only student admitted by Fisher to do Part II that year, and although I had attended some statistics lectures by Henry Daniels the previous Easter Term, soon found myself at sea listening to Fisher. I asked him what I should do about it, to which he replied that he had written some books and I might try them.

I bought *Statistical Methods for Research Workers* (and his other books) and settled down to study it. Chapter III introduces the binomial distribution which I had heard about from Daniels, but Fisher goes further and fits it to some data. First he uses Weldon's dice data, finding a good fit provided the estimated (and significantly biased) parameter is used. Next he tackles Geissler's data on the distribution of male and female births in families of size eight and shows that the distribution has a significantly greater variance than the binomial, too great to be accounted for by identical-twin births.

At that point Fisher leaves the reader stranded and goes on to other topics. Thinking about the excess variance, it occurred to me that if the probability of a boy varied between families, that should do the trick. I was, of course, also attending Dennis's lectures, and at the critical moment he introduced the beta distribution. I immediately wondered what would happen if one gave the binomial parameter a beta distribution as an expression of its variability between families. Going back to my room in Trinity Hall I did the integration and was delighted to find it 'came out'. Of course I was later to learn that Gini and Skellam and no doubt countless others had done this before me.

Then came the problem of estimating the two parameters of the resultant distribution for Geissler's data. It seemed obvious that one needed two equations so I equated the observed and expected means and variances. Students had access to a desk calculator in the library, and whilst I was using this for my calculations one afternoon Fisher came in and asked me what I was doing. Research students had told me that I had invented Pearson's method of moments and that Fisher might not approve. But all he observed was that I should really be studying for my Tripos exams, though his manner suggested that he was actually more pleased to see me tackling something on my own initiative that interested me.

The upshot of this was my first big paper 'An analysis of Geissler's data on the human sex ratio' (1958) and undying admiration for *Statistical Methods*, which I was able to convey when asked to write the chapter on it in *Landmark Writings in Western Mathematics*, 1640-1940 (2005). But it was Dennis's timely lecture that started me off. And that was not all – the binomial distribution continued to fascinate me and eventually I wrote *Pascal's Arithmetical Triangle* (1987) to fill a gap in the historical literature. Dennis wrote to me about it:

> ... I was completely hooked and thought that you had provided an interesting explanation of many fascinating topics. The way in which the separate strands were woven together was admirable. A thoroughly enjoyable and instructive read.

He was kind enough to let me include this among the comments of reviewers that I circulated to publishers when canvassing for a reprint, and when Johns Hopkins University Press agreed to bring out a paperback (2002) I added the above story in a new Preface.

After leaving Cambridge I did not return until 1968, when I was fortunate enough to be elected to Fisher's old College, Gonville and Caius, on a two-year Fellowship which enabled me to write *Likelihood* (1972). My experience in the intervening years had persuaded me that likelihood was the neglected concept that scientists really needed rather than the endless *P*-values that dominated the literature. By then Dennis had written his Bayesian textbook (1965) but Ian Hacking had written *The Logic of Statistical Inference* (1965) and it was the latter that inspired me.

So Dennis and I became sparring partners, he for Bayes, me for just likelihood, and so we have continued over the years. My file of our correspondence starts in 1970 (and will end up in the Caius archives). Any difference between our viewpoints can be accounted for by the fact that his background is mathematical and mine scientific. Mathematicians naturally place great emphasis on coherence and think that induction, and statistical inference in particular, should fit a mathematical mould. Scientists are less certain, and geneticists especially are reluctant to make model assumptions for which we feel there is no warrant. It is simply the classical distinction between the idealist and empiricist philosophies. We will go no further than accept that Bayes rules when a *decision* is required.

But Bayes or likelihood, it does not usually make much difference, and indeed much of what is now called Bayesian methodology in science is really just clever ways of finding the likelihood. It is as if people think that 'Bayesian' means using Bayes' Theorem, whereas when Fisher coined the word in 1950 he meant accepting Bayes' Postulate. Dennis's advocacy has played a major part in changing attitudes, particularly in regard to tests of significance (though let us not forget Lindley and Miller's *Cambridge Elementary Statistical Tables*, 1953).

When, to the delight of the Fisher Memorial Trust, he accepted its invitation to deliver the XVI Fisher Memorial Lecture in 1992, it was music to my ears to hear him say that likelihood was 'one of Fisher's most brilliant ideas'. His typescript (which the Trust holds in its archive) does not actually contain the statement, but it does conclude:

> In summary then, I consider that Fisher was totally correct in distinguishing the statistics of the laboratory from that of the world: in separating inference from decision-making. Where he was wrong, in my humble opinion, was in thinking that a significance test provided a sensible inference, at least sensible in the sense that it could be used in applications to the practical world.

References

Edwards, A.W.F. (1958) An analysis of Geissler's data on the human sex ratio. *Annals of Human Genetics* **23**, 6-15.

Edwards, A.W.F. (1972) *Likelihood*. Cambridge University Press.

Edwards, A.W.F. (1987) *Pascal's Arithmetical Triangle*. London: Griffin, and New York: Oxford University Press. Baltimore: Johns Hopkins University Press (2002).

Edwards, A.W.F. (2005) R.A.Fisher, *Statistical Methods for Research Workers*, First Edition (1925). In *Landmark Writings in Western Mathematics, 1640–1940*, ed. I.Grattan-Guinness, 856–870. Amsterdam: Elsevier.

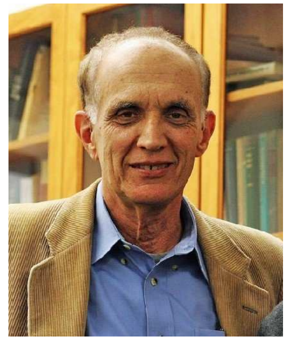
Fisher, R.A. (1954) *Statistical Methods for Research Workers*, 12th ed.. Edinburgh: Oliver & Boyd.

Hacking, I. (1975) *Logic of Statistical Inference*. Cambridge University Press.

Lindley, D.V. (1965) *Introduction to Probability and Statistics from a Bayesian viewpoint.* Cambridge University Press.

Lindley, D.V. & Miller, J.C.P. (1953) *Cambridge Elementary Statistical Tables.* Cambridge University Press.

Brad Efron



On the Occasion of Dennis Lindley's 90th Birthday

Dennis Lindley isn't as old as Bayes theorem but it's getting close. In my mind, Dennis is the Huxley of Bayesianism, its bulldog defender who brooks no compromise or backsliding. I've been bit a couple times myself. Here's Lindley commenting on my "curvature" paper in the 1975 Annals of Statistics:

> The defect arises from the fact that it [he means my whole theory] involves an integration over sample space and thereby violates the likelihood principle.

Besides curvature, that rules out a lot of useful stuff!

That kind of disagreement spilled over into the pages of the 1986 American Statistician, with my article, and Dennis' commentary on "Why isn't everyone a Bayesian?" At root, the argument is what to do when there really isn't any prior information to work with? Subjective priors? Objective priors ala Jeffreys? Empirical Bayes? [A loud NO from Dennis on that one, the slippery slope etc.] This question continues to be the dog barking in the basement of statistical practice, and nobody argues it more forcefully than Lindley.

Lindleyism, as opposed to Bayesianism, involves a deep paradox. Dennis, the unbending idealogue, is personally one of the nicest and most generous of statisticians. During my 1972-73 visit to England, a very pleasant time, my wife and I received exactly one dinner invitation, and that was from the Lindleys. There were further happy times hosted at his small conference in Aberystwyth, still the greenest place I've ever seen. And I want to take this occasion to thank him for not tearing into me in front of the Lord Mayor and Mayoress of Newcastle, though I could see the restraint necessary when the bootstrap was mentioned.
I wish I could be present for a slice of the 90th birthday cake, and a chance to tell Dennis how substantial a part he's been of my statistical education.

Ian Evett

Dennis Lindley and Forensic Science: An Appreciation

I read physics for my first degree at Birmingham. Although the study of statistics for dealing with what we called "measurement error" formed part of my course, overall the subject remained a mystery to me. When I first became a forensic scientist, in 1966, I worked as a handwriting specialist. This was, as indeed it largely is today, at the operational level, based entirely on qualitative observations of letter design, shape, relative size and so on. It seemed to me that there was scope for improving inference by carrying out measurements and I embarked, in my spare time, on collection of data from various samples of handwriting that I acquired. Then I got to the point that so many researchers reach – now I have all of this data, what am I going to do with it? So, gaining some kind of understanding of statistics became something of a priority and I was fortunate that my Director thought the same. The Civil Service was a different place in those days and, in 1972, I found myself a full-time student again, this time at University College, Cardiff, taking a postgraduate course in mathematical statistics and operational research. Although frequentist, it was a brilliant course, professionally delivered by passionate teachers. My external examiner? One Professor D.V. Lindley!

Each year, the statistics departments of the constituent colleges of the University of Wales met with that of Birmingham University for a week-end seminar at a lovely old house called Gregynog in mid-Wales. Dennis came up from London as an invited speaker; it was the first time I saw him – it was before he grew his beard. He opened by presenting a particular inferential challenge and continued along the lines: "…of course, I am going to address this from a Bayesian perspective, because I cannot imagine that any rational person would wish to do otherwise". Given that he knew that he was talking to a predominantly frequentist audience it seemed to me to be such a breathtakingly provocative thing to say that, I am somewhat ashamed to say, I laughed out loud. It was disgracefully rude, particularly as it was from a rookie in the presence of far greater intellects. But Dennis didn't take offence.

In 1975, back now in my forensic science post, I saw that Dennis had given a talk to the Edinburgh local RSS group on probabilities and the law. I wrote to him and he kindly sent me a copy of his talk, which was later published [1]. The Bayesian view of forensic evidence interpretation was not new but Dennis' analysis of the basic transfer problem was novel and enormously stimulating. At that time, I was attempting to improve the methods that forensic scientists were employing to compare refractive index measurements made on glass fragments recovered from the clothing of a suspect with those made on a control sample from a broken window. I introduced Dennis to the problem and the outcome was his beautiful Biometrika paper of 1977 [2]. Of course, I was still working with classical significance tests and (I am embarrassed to admit) I wrote to Dennis to explain that there must be something wrong. I gave the example of two glass samples that were adjudged different because the difference between their RI measurements failed a 99% significance test – yet Dennis' likelihood ratio exceeded one. With enormous patience, Dennis replied to say that this was precisely the point he was trying to make! I had not heard of Lindley's Paradox before then – it was the second major increment on my somewhat discontinuous learning curve.

My correspondence with Dennis has continued from those early days and he has done so much to deepen my understanding of the nature of inference. The remarkable features of my interchanges with him are his clarity of expression, his patience and a complete lack of academic arrogance. Right from the early days, I would wonder that a full Professor and a World leader in his field, was not only prepared to engage in discussion with me, a complete statistical novice, but also to treat my questions with respect and careful consideration.

It was in 1990 that I learned "Lindley's criterion". Colin Aitken organised the first international conference on forensic inference at Edinburgh. Afterwards, while waiting at the airport for the journey back down South, I asked Dennis what he thought of the conference. "I heard two good papers – therefore it was a good conference", was his reply. Since then, I have always judged conferences by Lindley's "good conference criterion".

There is another aspect of Dennis' character that is illustrated by the following anecdote. Arising from our discussions of the 1975 paper [1], I extended Dennis' approach to what we would now call an analysis of "activity level" issues. Before submitting it for publication, I sent it to Dennis. I explained that it drew heavily on his ideas and I was concerned that I had insufficiently acknowledged his contribution. His reply? "I don't worry about things like that. The most important thing is that these things are said. Go ahead and submit".

A friend and another forensic scientist who has learned first-hand from personal exchanges with Dennis is Dr John Buckleton, of New Zealand. He wrote to me about an occasion when Dennis visited him:

> Watipu is a wild and windswept beach on the West Coast of New Zealand. I took Dennis there, as I often do with visitors, because its raw beauty is a reminder that man

is very small. On the drive we discussed the Biometrika paper: I had been thinking for a while about glass evidence and was stuck on a particular aspect of Dennis' analysis. The discussion continued on the beach with Dennis writing out conditional probabilities by means of a piece of driftwood in the wet sand. I have revisited Watipu many times but I will always cherish the memory of two men and a stick discussing Bayesian inference with no company other than the wind, the waves and the seabirds.

Although forensic science in the UK is being downgraded to a low grade technical pursuit, the search for scientific understanding is being led by beacons of light overseas: New Zealand, The Netherlands and Switzerland, in particular. Today, the University of Lausanne has (probably!) the greatest concentration of thinkers and workers in the field of forensic inference and here is an appreciation from them:

> Professor Lindley has undoubtedly opened up new horizons in statistics, law and forensic science. He has influenced our thinking at the most fundamental level, on topics such as the nature of probability, the evaluation of continuous data and, more generally, Bayesian decision theory as a general framework to conceptualize the notions of evidence and proof in science and the law.

Professor Charles Berger from the Netherlands, writes:

> Within the Netherlands Forensic Institute Professor Lindley's work is receiving more attention now than ever before, due to an increased inflow of academics that are aware of the central role that probability has to play in forensic science. This attention concretely means that *Making Decisions* is on our desks, and that Lindley's ideas are applied and sometimes hotly debated. In our journey towards reasoning logically in the presence of uncertainty, his work continues to guide us and its continued relevance bears testimony to its quality. I also enjoy how he has managed to be provocative without being arrogant, with statements such as “Numeracy is not favoured by British justice” [3], a favorite of mine.

Bernard Robertson and Professor Tony Vignaux are authors of what I consider to be the best introduction to probability in legal reasoning [4]. They write:

> Shortly after we started our collaboration, we lured Dennis to speak at Victoria University in Wellington by promising him a visit to a local bird sanctuary. It was his talk that provided the stimulus for our book "Interpreting Evidence", comprehensively tackling Bayesian analysis in forensic science (rather than just writing esoteric papers!).

Today, we face the same frustrations as Dennis must have encountered, particularly during his early years as a Bayesian advocate. As I have said, there are some powerful beacons of light in the forensic science world but they shine in oceans of profound darkness. The particular frustration in the UK is that, whereas much progress has been made in advancing the foundations of forensic science at the practical level, we can claim no such progress in educating the legal profession, particularly the judiciary. The latter is evident from a series of legal judgments from the Court of Appeal that are obstinately reactionary and founded on a depressing failure to understand the nature of reasoning in the face of uncertainty. But we can only battle on – and we must take inspiration from the many battles that Dennis must have fought and won in his journey. Time and again, I find that problems arise because of a poor understanding of the nature of probability; so I recall my own journey in this regard and the help and support that I received from Dennis. My best advice to those who seek enlightenment is to beg, borrow or steal a copy of *Understanding Uncertainty*.

[1] Lindley, DV. Probabilities and the law. *In:* Utility, Probability and Decision Making. Wendt, D and Vlek, C *Eds*. Reidel, Dordrecht 1975. pp 223-232.

[2] Lindley, DV. A problem in forensic science. *Biometrika* 1977; 64(2): 207-213.

[3] Lindley, DV. Probability and the Law. *The Statistician* 1977; 26(3): 203-220.

[4] Robertson, B and Vignaux, GA. Interpreting Evidence: Evaluating Forensic Science in the Courtroom. Wiley, Chichester 1995.

Tom Fearn

I entered the world of Statistics via an MSc at Imperial College in the academic year 1971-72. Phil Brown, having recently acquired his PhD under Dennis, managed to turn a course in finite population sampling theory into a recruitment campaign for the Bayesian approach, and succeeded in enlisting me. When I expressed interest in undertaking a PhD the advice from Phil and from Ann Mitchell was unequivocal: there was only one place to go! In the event I was fortunate enough to be accepted as a PhD student at UCL in the autumn of 1972, with Dennis as my supervisor.

Although I have fond memories of Dennis as a supervisor, the ones that stand out concern his public performances. In the early 1970s the Bayes v frequentist debate was very much alive, and from time to time quite lively. There was an RSS research section meeting about once a month at the London School of Hygiene and Tropical Medicine. Dennis would regularly contribute to the discussion, demonstrating with an example why the methodology proposed was not coherent and was therefore flawed. These examples were always deceptively simple ones, of the sort that are so obvious when explained that you tended to overlook the fact that a lot of time and effort must have gone into crafting them.

The other discussions I recall are those in Departmental seminars at UCL. With an audience that included Dennis, Mervyn Stone, Philip Dawid and, when he returned from a brief affair with Oxford, Adrian Smith there was, more often than not, lively discussion. My memory may be inventing this, but I am fairly sure some speakers never got past their third or fourth slide. Under Dennis's leadership, the UCL Statistics Department was an exciting place to be a PhD student (as it is now, of course).

When the English translation of De Finetti's *Theory of Probability* appeared around 1974, Dennis advised all his PhD students to stop what they were doing and read it. I duly did so (then as now, most PhD students don't need much excuse to stop what they are doing and do something else), and was grateful for the advice. Around that time Dennis was the speaker for one of the joint University of London seminars. He was introduced, and had got as far as announcing his title when someone in the audience (I can't remember who) put up their hand and asked Dennis if he could talk about De Finetti's ideas on probability instead. He took a vote and then, with the motion in favour of De Finetti carried, calmly abandoned his intended seminar and talked eloquently for an hour on a different topic. Nobody coming in ten minutes late would have guessed that this was anything other than a carefully pre-planned talk.

Since obtaining my Bayesian PhD I have done a lot of applied statistics, both inside and outside academia. For an assortment of reasons, the usual one being the need to get a quick answer that wasn't too wrong, I have used a lot of methodology of which I doubt Dennis would approve. If I haven't been completely faithful to the cause, I have consistently applied one very important rule that I learned by studying with Dennis, and that is that if you want to understand a statistical problem you need to think about it in the Bayesian framework.

Stephen Fienberg

My First Interaction with Dennis Lindley: Being Bayesian in Iowa City

During my first year as a graduate student at Harvard University (1964-1965), I was a research assistant to Frederick Mosteller and Fred gave me two different problems to work on. At the time, I had yet to acquire statistical religion but these problems helped me along an inevitable path. The first involved assessing probability assessors, and while working on it I discovered a short piece by deFinetti on the topic! That work became part of a mimeographed technical report I authored with Fred and John Tukey, and it led some 17 years later to a series of papers I wrote with Morrie DeGroot on the topic, e.g., see [2], from the purely Bayesian perspective.

The second involved extensions to what at the time was an unpublished paper by Mort Brown [1], a Princeton graduate student, on what we would now call an empirical Bayesian approach to estimating the difference between normal means with a common variance. My job was to extend the approach to handle multiple normal means in one-way and two-way layouts with one observation per cell. Among the reading materials Fred shared with me was a 1962 JRSS discussion paper by Charles Stein on confidence intervals for multiple normal means, in which was a discussion by Dennis Linley setting forth a Bayesian approach to the problem [4]. I set out to emulate Lindley's approach to the generalization of Brown's problem and after many attempts succeeded. I worked on the problem for quite some time without understanding much of what I was doing and then returned to it a couple of years later and prepared a polished solution using the empirical Bayes methods that were about to come into vogue to estimate the common variance. I wrote up my results in the form of another mimeographed technical report and moved onto other research problems. Doing this work did, however, further my confusion between fixed and random effects in ANOVA models and likely hastened my Bayesian conversion.

Fast forward to the winter of 1971. I was now a committed Bayesian with occasional frequentist tendencies, and making a trip from the University of Chicago to Iowa City to participate in a workshop organized by Mel Novick, at the American College Testing Program. The occasion was memorable for two reasons. First, a horrible snowstorm stranded me at O'Hare Airport, forcing me in the end to go back to my apartment and start the trip over the next day when my flight to Iowa City finally took off despite the blustery weather. But second, another guest at the workshop was Dennis Lindley. By this time, early versions of the results of the thesis work of Adrian Smith were in circulation and Mel Novick was actually working with some related notions for binary outcomes and had them programmed so that one need not struggle with the computations to actually get numerical posterior results. Dennis wrote a lovely little piece describing Mel's research after his untimely death in the mid-1980s [7].

Dennis and I had a number of stimulating conversations at the workshop, and, during one of them, I described to him my graduate student research exercise based in part on his work. Dennis responded by telling me I should have published the results. Being a young assistant professor at the time, I was only beginning to understand that what was novel and worth publishing was in the eyes of the beholder. And if Dennis Lindley thought my early research exercise interesting then others might have also. But, alas I thought, it was now too late!

Not so. Shortly afterwards, I was invited to submit a contribution to the discussion paper of Lindley and Smith [8], and I took the opportunity to include some of the details of my earlier efforts at the ANOVA problem, and explaining that their results helped to simplify my calculations substantially. The response to the discussion included the following:

> Why did not Professor Fienberg's memorandum get published? Since writing this paper D. V. L. has obtained estimates closely similar to those he mentions, … The method is particularly attractive because it provides estimates of the cell means, which depend on estimates of the relative sizes of the main effects and interactions, thereby quite avoiding the usual significance tests. The estimates of variance he proposes do not seem sensible because they depend on the *data* , etc. rather than the *estimates* of the cell means.

Clearly this part of the discussion response was written by Dennis, likely recalling our earlier exchange. It also sounded like Dennis in public, when he made quite clear that he had little use for empirical Bayesian incursions into actual Bayesian work. Dennis subsequently published his work on this topic [5,6].

It was not many years later, when I had moved to the University of Minnesota and Dennis had retired and become an itinerant professor, that we had the occasion to interact once more. Dennis came to the School of Statistics for an extended visit and I was privileged to hear him give several lectures. Dennis was always inspiring and would typically take a complex problem, reduce it to an essential core, and then propose an elegant solution by "turning the Bayesian crank." And all of this with a twinkle in his eye, showing us that he was enjoying the result as much as we were.

I went on to have many other conversations with Dennis, some technical, some about Bayesian approaches and the law, several social, and yet others mixed, especially when we were discussing the quality of wine over dinner. Over the years we often disagreed, although rarely about wine and never acrimoniously.

[1] Brown, M. B. (1967). The two-means problem – a secondarily Bayes approach. Biometrika, 54, 85–91.

[2] DeGroot, M. H. and Fienberg, S. E. (1983). The comparison and evaluation of forecasters. *The Statistician*, 32, 12–22.

[3] Fienberg, S. E. (1967). Cell estimates for one and two-way analysis of variance tables. Memorandum NS-69, Department of Statistics, Harvard University.

[4] Lindley, D. V. (1962). Discussion of a paper by C. Stein. *Journal of the Royal Statistical Society, Series B*, 24, 265–296.

[5] Lindley, D. V. (1972). A Bayesian solution for two-way analysis of variance. Research and Development Division, American College Testing Program, 68 pages.

[6] Lindley, D. V. (1974). A Bayesian Solution for Two-Way Analysis of Variance. *Progress in Statistics* (European Meeting Statisticians, Budapest,1972), North-Holland, Amsterdam, pp. 475–496.

[7] Lindley, D. V. (1987). Melvin R. Novick: His Work in Bayesian Statistics. *Journal of Educational Statistics*, 12, 21–26.

[8] Lindley, D. V. and Smith, A. F. M. (1972). Bayes estimates for the linear model (with discussion). *Journal of the Royal Statistical Society, Series B*, 34, 1–41.

Peter Freeman

Dennis Lindley at 90

As a young academic my research interests in sequential decision making led me to one of Dennis's many marvellous papers, "On a measure of the information provided by an experiment". A timid request for a meeting to talk about this was received cordially and, on the great man's next visit to London from the remote Welsh hills, we spent one of the most formative afternoons I'd ever had.

Luckily, the teaching of Statistics was expanding so rapidly in UK universities at that time that there were many unfilled lecturing vacancies and I was able to join Dennis's department soon after he moved to University College London. There followed the happiest ten years of my professional life. A constant stream of visitors came from a huge range of backgrounds and there were all those brilliant research students who have since gone out and turned the whole world Bayesian. It was, in retrospect, a remarkably productive time, though I remember it more as demanding, challenging and very hard work. Dennis inspired us, for example, to start a new Masters course pitched at the level of what our USA colleagues see as PhD coursework but which had never before been attempted in the UK. But, above all else, Dennis ran his department superbly. He sat on all the committees and processed all the paper so that we were blissfully unaware of all the admin problems. He was always approachable, always fair, never cross (well, not visibly, anyway) and cleverly got the best out of each of us. And all of this in a building which shook as buses and ambulances raced each other past the windows, Galton's armchair presided over the departmental common room and Karl Pearson's collection of Phil. Trans. shared the basement with toilets that should have been condemned back in Victorian times.

Dennis seldom ate lunch but he always joined us for coffee in the College common room. Conversation would range from interesting snippets from papers in the new Annals that had arrived that morning (Mervyn Stone was particularly good at these), contributions from American colleagues towards the cost of restoring Rev. Thomas Bayes's tomb in Bunhill Fields, the correctness or otherwise of arguments just produced by some of those pesky research students, last night's Klemperer concert at the Festival Hall, the new Stoppard play at the National or the latest Haitink recording of a Mahler symphony. Then it was back to our offices for an afternoon's work that always stopped for tea and more discussion at exactly 4 o'clock.

One fine example of Dennis's wise governance sticks in my memory. He had discovered a dormant but quite large sum of money sitting in the proceeds of some of Karl Pearson's publications. From somewhere came the suggestion that we could use it to purchase a departmental country cottage. This was taken up with enthusiasm by us youngsters and ideas for use as a rest and relaxation centre for tired academics or for intensive weekend graduate courses were a constant topic in the staff bar. Dennis could, of course, foresee problems but he never squashed the whole idea. It appeared on the agenda of every staff meeting for over a year but somehow there was never time to get to it and we slowly got the message that it would never happen. I've often wondered, though, what did actually happen to the money. Stolen by some government, no doubt.

History will obviously remember Dennis as the world's leading exponent of that little theorem that was already 160 years old on the day he was born. But I know him as one of the most complete, profound and admirable human beings I have ever met.

When he announced his early retirement, he gave the following as his reason:

> Now, of my threescore years and ten,
> Fifty will not come again,
> And since to look at things in bloom
> Twenty springs are little room,
> About the woodlands I will go
> To see the cherry hung with snow.

Time, I think, for a little updating:

> Now, of your *four*score years and ten,
> None will ever come again,
> But each and every one can tell
> Of a life lived very well.

Rex Galbraith

Memories of Dennis Lindley at UCL

My wife Jane and I have many fond memories of Dennis as Head of the Department of Statistics at UCL in the 1970s. We were young lecturers starting our academic careers and learning our subject. We had many arguments with Dennis; we were intrigued by his Bayesian philosophy and the controversies surrounding it; and we were greatly influenced by him. Shortly after arriving at UCL, Dennis presented a seminar in which he posed the following problem:

> A stochastic process of binary variables $x_{i,j}$ (= 0 or 1) is defined sequentially on the two-dimensional lattice $i, j = 0, 1, 2, \ldots$ by a transition scheme in which the probability that $x_{i,j} = 1$ given its predecessors is either p_{00}, p_{01}, p_{10} or p_{11} according to whether the values of the two nearest predecessors $x_{i-1,j}$ and $x_{i,j-1}$ are (0,0), (0,1), (1,0) or (1,1). From given starting values $x_{i,0}$ and $x_{0,j}$, the (random) values of $x_{i,j}$ for $i, j = 1, 2, 3, \ldots$ may be thus generated sequentially. It stands to reason that as i and j increase, the probability that $x_{i,j} = 1$ will tend to a limiting "equilibrium" value θ that does not depend on the starting values. What is this value θ?

The corresponding process and problem in one dimension (i.e., the two state Markov chain) are easily solved by standard methods. By analogy with these, Dennis proceeded to write down four equations that must be satisfied by the equilibrium value of theta in terms of the four transition probabilities. But when he tried to solve them, it transpired that one of these equations was implied by the other three, so the solution was not determined. Where was the missing equation?

This became known as "the crystal problem" because it was motivated by a theory of crystal growth where each site on a regular 2D lattice was occupied by one of two types of atom, the type at a given site being determined by 'nearest neighbour' forces depending on the types of atom at the two previously occupied nearest sites. The crystal problem was remarkable for being so simple to state and understand but so elusive to analyse. Several of us tried to solve it, and more than once thought we had, but there always turned out to be a "missing equation". As far as I know, about 40 years later, a complete solution has still not been found.

I worked on aspects of the crystal problem, off and on, for several years with David Walley, and also with a crystallographer called Richard Welberry, who had a method of generating pseudo X-ray diffraction patterns from simulations, which gave a different insight into the nature of the process. David and I made some progress. We found that not only was it hard to pin down theta but it was also hard to show that an equilibrium value actually existed. We had several different representations including: a non-linear auto-regression, random products of 2×2 transition matrices, products of non-square $(2n + 2 \times 2n)$ stochastic matrices of increasing size, and a sequential matrix equation method for obtaining closer and closer bounds on theta, assuming it exists. We found quite strong sufficient conditions for theta to exist, and solutions for it in some special cases, but existing mathematical theory was never powerful enough to provide a complete solution. I was always amazed by this and by the ramifications it had for understanding stochastic processes more generally.

Dennis is of course known best for his work and views on statistical inference and subjective probability (or, as he would say, probability). But before I met him, I knew of him as an author and pioneer in queuing theory. His work in that field used beautiful (and powerful) mathematics and inspired me to study that subject, and stochastic processes more generally.

The Department with Dennis as its Head was exciting and stimulating. We discussed statistics with Dennis himself and also with the many visitors he attracted. Once Oscar Kempthorne visited and Dennis invited Jane and me, as new young lecturers, to accompany them to lunch. I was interested to see how friendly they were, and respectful of each other, given their opposing views on statistics. I was aware of some antagonistic and quite personal comments Oscar had made both verbally and in print on previous occasions. Dennis's own writings never attacked the person, but they certainly attacked the person's ideas. As usual we talked abut many aspects of statistics and teaching, including the importance of mathematics. Dennis always emphasised how important it was for students to learn as much mathematics as possible at school and beyond. Oscar said that he did not think students needed to know any mathematics . . . (pause) . . . as long as they knew the Radon-Nikodym theorem. I prayed that he did not ask me what it was. I knew it was something to so with measure theory, but that was about all. We all agreed how important it was to teach measure theory.

Then Oscar turned to Jane and me and said

> "Let me ask you a question. Suppose that you have some data, measurements of some quantity, say, made under two conditions or by two different methods; and you want to know how the two sets of measurements differ, or more generally what may be inferred from them. You consult two famous statisticians, Egon Pearson and Dennis Lindley, and they recommend to you two different methods of answering your question. Which one would you follow?"

I think he gave us a rough idea of what these methods would be. At that time, Egon had retired but came in to the Department regularly and we knew him well. But Dennis was there with us, our boss, listening to our answer. I said, with not too much hesitation, that I would favour Dennis's method — not just because he was there, but because Dennis's arguments for the Bayesian approach were so clearly right. When we asked Oscar what he would do he said that on balance he would probably favour Egon's method. Why? "Well, Egon is a pretty sensible chap, so I don't think he would be far wrong." Of course this seemed to me like cheating, but now, after years of working with scientists, I have a different view of the wider role of Bayesian inference in science. I suspect that Dennis would not agree with me but would enjoy arguing about it. That day, though, Dennis had the last word, informing Oscar, to his great astonishment, that he was lunching with Student's granddaughter.

With respect to teaching, Dennis subscribed to the view than any lecturer in the department should be able to teach any course. We attended each other's lectures and we weren't allowed to give the same course for more than three years running. We all went to Dennis's lectures and learned much from them, including the fact that he gave beautiful lectures.

He had ideals that sometimes conflicted. He believed in being democratic, with everyone's views taken account of, and we were indeed consulted and listened to. He believed in academic autonomy, so a lecturer should teach what he or she regarded as important. He once said that it doesn't matter what you taught as long as you taught it properly. But he also wanted the right material to be taught. Ideally, this meant Bayesian inference from year one, and an emphasis on subjective conditional probability. He conceded that with the world as it was it was necessary to cover some frequentist methods in the final year, so that students on leaving would be able to communicate with others. He made a start on such a project one year, by teaching the whole of the first year BSc course himself — one-and-a half units of probability and statistics that had to serve as a grounding for the remaining two years of the degree. There was no suitable text book or even a coherent notation for this course, so he invented the notation and wrote detailed lecture notes by hand (he had beautiful handwriting). I still have a copy of these; they are remarkable both for their content and for the thought that went into them. I devised and ran the practical classes to go with this course, which was also an education for me.

Each year Dennis invited all the staff to his home in North London for a social evening and buffet meal. After one such evening he remarked how pleased he was that Joan had prepared just the right amount of food — plenty to go round but very little left over — whereas on previous occasions she had always over-catered. Had she perhaps applied his principles of rational decision making? But when he complimented her on this she had confessed to over-catering as usual, though this time she had been careful to keep most of the spare food out of his sight, to be produced only if needed.

Thank you Dennis for your inspiration. Jane and I wish you a very happy 90th birthday.

John Gittins

Dennis Lindley: A tribute

I first met Dennis Lindley in 1958 in a basement office in the chemistry laboratory in Lensfield Road in Cambridge. The purpose of the meeting was as part of an exploration of the various courses available in Part II of the Mathematical Tripos, and I had particularly enjoyed Dennis's lectures earlier that year on probability.

As it turned out, I chose the theoretical physics track at that stage, and it was not until 1964 that I finally followed up on the seed Dennis had sown, and enrolled, first for the Diploma, and then for a doctorate, in Statistics under Dennis's supervision at Aberystwyth. He was head of a stimulating, youthful, and happy department. This owed much to the hospitality of Dennis and Joan, and various social events, such as the annual stay at the University of Wales's country seat at Gregynog near Newtown, where we learned how to program a computer between walks in the countryside.

As a research supervisor Dennis was always perceptive, helpful and encouraging. I am particularly grateful for an introduction to his faculty colleague William Pennington, which led to my first paper, as well as introductions to Pontryagin's maximum principle, and to the two-armed bandit problem. Both of these became really fruitful lines of enquiry.

Dennis is, of course, and in my recollection always has been, a standard bearer for the Bayesian approach to inference. However I never felt any pressure to rally to the cause. Looking now at my doctoral thesis, I see that I was convinced that some aspects of pharmaceutical research require a Bayesian approach. I still do for that matter, and increasingly the world seems to agree. Thank you Dennis for your good example, in that respect and in many others, and many happy returns.

Michael Goldstein

Valencia Memories

Conferences are a great way to spend time with people. The absolute best conferences for me were the wonderful Bayesian meetings in Valencia, which Dennis did so much to help to create, particularly the early meetings where we were still working out what our subject was all about, to a great backdrop of sun, sea and wine. I had the good fortune to be an invited discussant on two of Dennis' papers at Valencia conferences and reading again the papers and the written discussion brings back many happy memories.

The first time I was a discussant for Dennis was at the very first Valencia meeting at the Hotel Las Fuentes. It was a very pragmatic paper, titled "Approximate Bayesian Methods", which worked by developing asymptotic expansions for the ratios of integrals which occur in Bayesian Analysis. The results were interesting and prefigured a lot of current work in this area. I greatly enjoyed discussing the paper, partly because it gave me the opportunity to think carefully about how to assess the value of such approximations and partly because it gave me the opportunity to link, by example, this type of expansion with one of my favourite things, namely Bayes linear assessments. There is basic work still to be done here. Reading the discussion now brings back fond memories of the way we were. In particular, here is bit of Dennis' reply to a particular technical query, which sparks recognition within me: "Packages and big computers terrify me. They are like some bureaucratic machine where workings and output are unintelligible;... So far as computers are concerned, Schumacher is right; small is beautiful."

The second time that I discussed Dennis' work was his presidential address at the fourth Valencia meeting. This session took place in the rather splendid Gothic Room of Peñíscola Castle, and Dennis' topic was "Is our view of Bayesian Statistics too narrow?", taking in a wide range of scientific, decision theoretic and humanistic considerations to make his argument. It was a great pleasure to have an opportunity to ponder and to discuss this issue which is very close to my heart (yes, in my opinion, our view of Bayes was, and even more so is now, far too narrow), and to give my view that Bayesian Statistics can only achieve its potential when we place our beliefs at the heart of the analysis, rather than as almost an annoyance which we have to pay lip-service to in order to gain the formal advantages of the Bayesian approach. Dennis happily confirmed, in his response, that this was largely his view, too. He also removed the question mark from his title - yes, he argued, we are too narrow in our view of the subject. In today's ultra-complex and computational world, we need to be reminded more than ever of this basic message: to look beyond formalism and computation to meaning.

Jeff Harrison

1958/9 was a *desperate* year for Dennis and the Cambridge Statistical Department. Dennis had the unenviable task of ensuring its survival. Essentially, the postgraduate Diploma in Statistics teaching rested on Dennis and new recruit Maurice Walker. There were eight students on the course including Bob Loynes.

The most notable feature for me was my project, supervised by Dennis. This concerned the consistency of examiners in marking the Cambridge Examinations Board Overseas GCE in History. The Chief Examiner set the marking scheme, which basically gave a mark a fact, so that precise examination marking consistency was expected. However the conclusion of the project was both astonishing and political dynamite. Based upon a single marking, in classifying each examinee as Credit, Pass or Fail, it was estimated that 22% would be misclassified! The Chief Examiner proved to be very consistent, but other markers were grossly inconsistent, both with respect to bias and variation. My hand written project was recalled and unsurprisingly lost!

Although with such sparse staff and relocation to the "cellar" in Lensfield Road the year was tough for Dennis, morale was very good as is suggested from the photograph, on the next page, of our picnic on the bank of the Cam. You will see Dennis, Joan and the children, Maurice Walker and his wife and you may be able to identify Ann Mitchell, Bob Loynes, Roger Miles, Andrew Noble, Jeff Harrison and George Mitchell. Ann will certainly remember the day for her punting performance involving the classic stunt of clinging to the punting pole as the punt floated from under her, before plunging into the water. I can still see a soaked Ann in need of a change of clothes.

At he end of the year I returned to Imperial Chemical Industries Limited and, after being transferred from Dyestuffs Division to Pharmaceuticals Division, resumed contact with Dennis on Cusums and later Bayesian Forecasting. After I accepted an invitation to found the Warwick University Statistics Department in 1972, Dennis was appointed as Honorary Professor. The Department specialised in Bayesian Statistics and visits by Dennis were eagerly anticipated by both staff and research students. Dennis also gave the occasional lecture to undergraduates, the most memorable being on "The Secretary Problem".

The Common Room photograph, on the following page, taken by Mohammad Akram on the occasion of one of Dennis' visits in the early 1980's, features (from L to R) Peter Walley, Jamal Ameen, Mike West, Dennis, Helio Migon, Jeff Harrison and Tony O'Hagan. Tony introduced a second year undergraduate course on Bayesian Statistics and we gave a number of third year Bayesian undergraduate and MSc courses. The success of the Bayesian approach can be judged by the fact that Jim Smith's 3rd year Bayesian course now attracts around 200 students. It has been a long process overcoming resistance to Bayesian statistics, but with Dennis in the vanguard success has been achieved.

After Dennis retired to Minehead we had some interesting correspondence on foundations, a loss function derivation of Cusums and another "secretary problem". The latter arose from a common room conversation with Tony during which I asked, "Tony what odds do you place on our secretary Jean being the next member of the Statistics staff to be married?" Knowing that she was to be married in ten days time, Tony replied, "About 100 to 1". I then asked, "What odds will you give if I place a bet with you that she will not be the next staff member to be wed, even if that marriage takes place as scheduled?" Suspecting he knew not what, Tony rapidly reduced the odds to "evens". I placed a bet and within a few days collected my "filthy" winnings – I knew that another staff member was to be secretly married the weekend before Jean's marriage. Discuss!

Thank you Dennis for your teaching, supervision, friendship, your contribution to a successful University of Warwick Statistics Department and for your lead role in promoting Bayesian Statistics.

Alan Hawkes

Memories of Dennis

Having just graduated down the road at Kings College London, I arrived at UCL in 1960 to follow a one-year postgraduate diploma in Statistics. I had been interviewed by Egon Pearson, but when I turned up in September he had been replaced by Maurice Bartlett, although he remained around UCL for a few more years looking after Biometrika.

At the end of the year I started on PhD studies, under Bartlett, applying queuing theory to problems in road traffic, and one more year after that I was appointed to a lectureship (well assistant lectureship in those days). Colleagues who departed to America soon after were John Saw, Norman Johnson and the redoubtable Professor Florence David while Dick Barton moved to a chair at Queen Mary and Toby Lewis to a chair at the Open University. Harvey Goldstein was an RA who came and went. By the time Bartlett moved to Oxford in 1967 these were replaced by Vic Siskind, Rex Galbraith and Dieter Girmes, with Jane Stubbes (later to become Galbraith) as RA/tutor. These were still in place when Dennis Lindley arrived in the autumn of 1967, as were Neil Please and Maxine Merrington, who both seemed to have been at UCL for ever, and Dave Walley who taught computing. (See photo overleaf.)

So there had already been quite a turnover of personnel before Dennis came. He soon brought in old Aberystwyth colleague Mervyn Stone. Peter Freeman joined from Reading in September 1968. Bayesian postgrads started to appear: Phil Dawid, Rodney Brooks and Margaret Brooks, Adrian Smith, Ian Wilson and one of our undergraduates, Tony O'Hagan, joined the band. Mervyn came from the post of Reader in Mathematical Statistics at Durham, in which position I was soon to succeed him in January 1969. So my time as a colleague of Dennis was quite short and, with one exception, our academic interests were not close. I have never been a Bayesian theorist, although occasionally happy to be a practicing one when the practical problem at hand seemed to be best dealt with in that way – and I suppose Dennis must take some credit for that, although he would say that it is best for every problem. In fact Statistics has been a relatively minor part of my career, which has largely concerned applied probability modelling. The exception mentioned above is the famous Lindley paper on queuing theory [1] and, as I said, my PhD thesis was about queuing theory. Nevertheless, Dennis was clearly worthy of great respect both as a scholar and a person – a real gentleman.

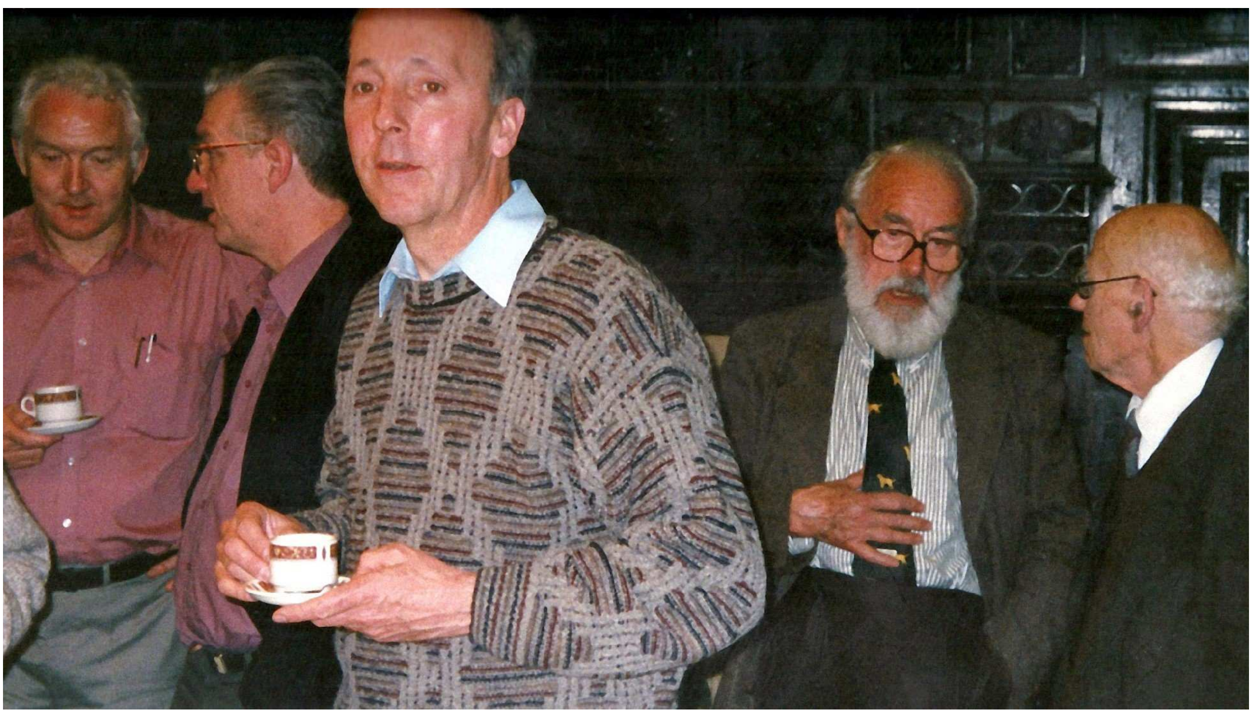
UCL Statistics Dept. 1968

In the middle of the front row are the Departmental staff: Vic Siskind, Jane Stubbes (now Galbraith), Maxine Merrington, Mervyn Stone, Dennis Lindley, Neil Please, Alan Hawkes, Dieter Girmes, Agnes Stuart (secretary), Rex Galbraith. The remainder are mostly undergrads and postgrads. They include Rodney Brooks, Aviva Petrie and Tony O'Hagan.

Our paths were, however, to cross again. In 1974 I moved to a chair in Swansea and soon began to enjoy the famous annual Gregynog Statistical Weekends. These were started in 1965 by Henry Daniels and Dennis as a joint venture between Birmingham and Aberystwyth, but Statisticians from other University of Wales Colleges, as they were then, also took part. Anyone who has been lucky enough to attend will remember the relaxed seminars and the chance to chat over meals, or in the bar, at this peaceful old house in the heart of rural mid-Wales. Sylvia would lead a Saturday afternoon walk and there were the sumptuous teas in the Blayney Room with gorgeous home-made cakes. After leaving UCL, Dennis became an Honorary Professor of the University of Wales and was therefore invited to attend later sessions of the series that he had started many years earlier. Of course, he was never short of some interesting comments to make.

There was also music, most notably Henry Daniels playing his squeezebox in a trio or quartet, often with Alan Sykes and John Copas. The saddest event was when the lovely Henry collapsed at breakfast: he was taken to hospital but died there aged 85.

[1] D.V. Lindley (1952). The Theory of Queues with a Single Server. *Proc. Camb. Phil. Soc.,*48, 277-289.



Tea at Greynog, 1977: Alan Jones, Barry Nix, Tony Lawrance, Dennis Lindley and Henry Daniels

David Hill

My main interactions with Dennis Lindley have been: through the Statistical Dinner Club; in occasional arguments about Bayesian versus frequentist statistical methods.

Statistical Dinner Club

The Statistical Dinner Club was founded in 1839. It is closely associated with the Royal Statistical Society, but not part of it. It has only one officer called the Honorary Treasurer but in fact the Honorary General Factotum. During the war years of 1940-1945 it became dormant, but was revived after the war by my father, Austin Bradford Hill, who was its Treasurer from 1946 to 1975. When he retired from the post it was Dennis who took over from him, and ran the Club from 1975 until 1979. Since he was sometimes abroad during those years he needed a deputy to act for him on occasions when he was not himself available, and recruited me as that deputy, so there was quite a lot of interaction between us during those years. In 1979 he decided that his circumstances did not allow him to continue in the post and Martin Beale took over from him. When Beale died in office, I became the next Treasurer and found that the arrangements that had been put in place earlier, to let me act as deputy, were very useful in allowing a smooth changeover.

Bayesian or frequentist

He that complies against his Will,
Is of his own Opinion still,
Which he may adhere to, yet disown,
For Reasons to himself best known.

Hudibras, Samuel Butler (1612 - 1680)

I am certainly of my "own Opinion still" as a confirmed frequentist and do not disown that opinion. I know that if I argue the case with Dennis he is very good at driving me into a corner with no obvious escape, but I always find myself creeping out again as soon as he is not looking.

This is not the place to try arguing it again, but I recognise how infuriating my attitude must be. Nevertheless I know that I should never feel comfortable to put forward any conclusions that depended, in a formal way, on prior probabilities.

If I were an ancient Greek, arguing with Zeno about his paradoxes, I might perhaps have said "I do not know where you are wrong, but I *know* that you are, because motion is clearly possible". In this instance there is no case for such certainty -- I cannot say anything like that, but only that my mental processes work like that, and that I shall continue to be stubborn until those can be changed.

On the other hand, I cannot agree either with the more severe of the frequentists. I regard the belief that you cannot have a probability of a hypothesis as absurd. Everyone (so far as I know) accepts that the probability that a random observation is less than the median is 50%. To my mind, to say that the probability that the median is greater than the observation is 50%, is merely a restatement of the same thing. Yet some people deny that you are even allowed to say it.

I have learned, in my studies of voting systems, that there are situations in life where sensible solutions of problems sometimes require us to accept that we must put up with paradoxes and do the best we can in spite of them, uncomfortable as that may be. Following that line, I am willing to accept a certain amount of paradox in other areas too, so long as it seems to me to be the best that we can do.

Wesley Johnson

"Always Leave a Little Room for Doubt," and by all means, "Extend the Conversation"

Dennis Lindley, circa 1985

I met Dennis informally as a graduate student at the University of Minnesota during the 1970s. Our department loved to invite visitors with diverse views. I recall a variety of contrasting ideas and personalities starting with Dennis, Arnold Zellner, Oscar Kempthorne, C.R. Rao, George Barnard, David Freedman, Bill Cochran, Ed Jaynes, Marvin Zelen, Manny Parzen, and many others. Dennis visited at least twice, and probably more than that.

Seminars were virtually always lively; Seymour Geisser, Don Berry, David Lane, Joe Eaton, Dennis Cook and others saw to that. But when Dennis and Oscar visited, the liveliness definitely emanated from them, on opposite sides of a certain track that Dennis will surely remember. Dennis' presentations were simple, elegant and deep. They captured our imaginations. I remember no others more than his.

Bayesian ideas had been presented to us in Minnesota alongside frequentist ones, so much so that some of us regarded them as equals. Dennis argued unambiguously on the Bayesian side with a passion that I don't think I have seen before or since. I was probably not mature enough then to completely believe in the advantage of one point of view over the other, but I was more convinced than ever that Bayes was at least equal to the frequentist approach, if not better. Seeds had been planted.

A wonderful part of my graduate education at Minnesota involved philosophical discussions with my cohort of graduate students about the advantages of various approaches; usually involving kegs of beer on cold Saturday nights in Minneapolis or St. Paul. I left graduate school more Bayesian than frequentist in the sense that I was completely open to publishing papers in each category; my thesis was Bayesian with frequentist asymptotics. I gave a talk at one of Arnold Zellner's Bayesian Econometrics meetings that Dennis attended; he was sitting in the front row just below the lectern. I mentioned the word p-value, and there was a loud and long gasp, “Oh my god!!!” from him. More seeds planted and growing.

Frank Samaniego and I invited Dennis to visit UC Davis as a Regent’s Lecturer during the 1980s, and he kindly visited with his lovely wife Joan; what a beautiful team they are. He taught a course on the foundations of Bayesian inference. Our faculty at the time were primarily non-Bayesian, with Frank and me and possibly others interested in both. A large crowd including many faculty attended his (brilliant to me) lectures. Mathematical arguments for superiority of Bayesian methods being necessary for coherence were stunning. I was leaning more Bayesian than ever. I taught our Bayesian courses, including an advanced one out of DeGroot’s book and I taught out of Jerry Cornfield’s notes, where coherence arguments were made as well. I met Dick Barlow and Nozer Singpurwalla, good friends of Dennis. I attended many Zellner meetings. I was becoming a real Bayesian.

During Dennis’s visit, he provided his definition of “Bayesian” at that time. As I recall it, it was:

(1) Model all uncertainty with probability
(2) Always obey the laws of probability

I still violate the first rule occasionally, by using improper priors. But I try not to. I wrote a book with friends (Christensen, Johnson, Branscum and Hanson, (2010) Bayesian Ideas and Data Analysis: An Introduction for Scientists and Statisticians) where we use this definition and attribute it to him. In our book, we focus intently on finding informative priors, at least for some of the parameters of every model. Even when we require diffuse/reference priors, we usually select proper ones.

At the time of Dennis’s lectures at Davis, I was still not a full Bayesian, but so many seeds were growing that I believe it was inevitable that I would become one. All that was needed was the ability to compute in a way that allowed for Bayesian analyses of complex models. This of course happened around 1990 with Gelfand and Smith’s presentation of Gibbs sampling to Bayesian statisticians in two JASA papers. The final push for me came from work on a paper with Joe Gastwirth that took us six years to solve as frequentists (very delicate and complicated asymptotics). The Bayesian solution, joint with Joe and with Tim Hanson, took about three weeks plus time to perform frequentist comparisons with our earlier frequentist approach to a forgone conclusion, to satisfy a probably frequentist referee. All the Bayesian seeds had resulted in a full bloom after that. I never at this point think of addressing a new statistical/ scientific problem using anything other than a Bayesian approach. I would add that it helps me sometimes to know frequentist things, and I certainly believe that students should be trained in both at least for the present, but I won’t go into that here.

Dennis once predicted publicly that the 21st century would be a Bayesian century. He also said, in his UC Davis class, that one should never be too sure of themselves, that they should "always leave a little room for doubt." I quote him in class on the latter constantly. On the 21st century prediction, I think we are off to a wonderful start; I plan to do my part to help make it happen. There are now dozens of Bayesian books that target both specialty topics and general audiences beyond only those studying Statistics per se. Scientific and Statistics journals are full of Bayesian works of art. There is no doubt in my mind that all of this progress was hastened to a great extent by Dennis's efforts to clarify the innate beauty and advantages of the Bayesian approach.

I miss seeing Dennis and Joan, very much. Comments from Dennis in rooms full of people were such delights. I was recently driving on the A38, which passes by Minehead from a distance, and thought about how nice it would be to see them. I do think of Dennis often, and I wish him and Joan (hi Joan) well, especially on his 90th birthday. Cheers.

David Johnstone

I first got to know Dennis after he examined my PhD thesis on foundations of significance tests in 1985. We met in Sydney and again in London. Dennis came to Australia to visit the CSIRO and some Australian Universities. We met up in London one afternoon around this time and I accompanied Dennis to a meeting of the Royal Statistical Society. I remember that this was very exciting, as I had been reading the papers and discussions that came out of these meetings and had developed great respect for the old fashioned scholarship and collegiality that they seemed to evoke. I also remember being introduced by Dennis to George Barnard, who shook my hand as Dennis's young guest with great warmth and openness.

I had sent the first chapter of my thesis, titled "Significance Tests in Theory and Practice", to *The Statistician*, and the editor chose to publish this work along with separate comments by Lindley and Barnard. The discussion was mainly about a rationalization that Barnard put forward for the use of tail areas, but I remember being delighted that two such great authorities and friends were prompted by my paper into deepening their long-standing debate over the logic of statistical tests.

Several things stick in my mind from talking to Dennis about hypothesis tests and statistical foundations. It was fascinating to hear personal insights and anecdotes concerning the great personalities and statistical theorists, including Fisher, Jeffreys, de Finetti, and Savage. One thing I have never forgotten was the story of how Dennis had visited Egon Pearson in a nursing home, and how Pearson had told him that the philosophical view of tests that is strongly associated with Neyman-Pearson theory – namely that tests are for choosing between actions, and not for assessing evidence for or against hypotheses – was Neyman's invention but not at all Pearson's.

Dennis was intrigued I think that I, who was teaching accounting and financial decision making in an Economics Faculty, had come to write a thesis about significance tests. He saw nothing intrinsically wrong with this, and wanted to help me, knowing that in my own field I might find it hard to get support. We became good friends immediately, for lots of reasons. For one, I was and remain extremely sceptical about the veracity of statistical testing in many parts of the social sciences, both because of the logical design faults of tail-area tests (P-values and alpha-values) but also because those tests yield nominally "significant results" so easily, thus satisfying publication hungry researchers, editors and funding bodies. I remember one sceptical colleague in my own Department, where no-one had any inkling of the depth of criticism of significance tests that theorists like Jeffreys, de Finetti and Lindley had mounted, asking me in a curious and almost scornful way whether significance tests were misused or illogical. When I replied "both", he looked at me in disbelief, and with some anger, but I remember feeling very secure in my position based on my reading and respect for Dennis and colleagues in statistics.

I felt from the start that I knew Dennis. My grandmother on my mother's side was a WWI war bride, who came to Sydney from the Cotswolds in the UK, and my mother grew up in Sydney very dear to her mother and very English. When I first met Dennis at his Sydney hotel, we drank several cups of tea before taking a taxi to the University. We arrived just in time for the regular morning tea session in the Faculty common room. When I asked Dennis whether he could drink another cup of tea, he announced that he "never said no to a cup of tea", something I had heard many times over the years growing up with my mother and her relatives. In my youth, Australia, and especially my family, was still very British, and I warmed instantly to Dennis's manner and personality. I think he liked my questioning and critical disposition, and the frankness with which I painted what I saw as the common abuse of statistics in my own experience.

It is well to remember that in many parts of the social sciences, you just write down a model that looks plausible, maybe a regression equation for example, and then you test for "significance" on one of its variables. If you don't get what you expect, you can blame the model and write down a different one – until significance appears and all looks right. The last model can be the one reported. In economics terms, this is an "agency problem", where the incentives of the agent (researcher) are largely to publish "significant" results, and the principal, whose incentives are more about truth or reliable knowledge, cannot tell whether the underlying work is sincere or an artifice. Arguments such as this were not too overstated for Dennis. Maybe I had some influence because Dennis has often written about significance tests giving "significance" too readily and mechanically, especially in large samples, and therefore playing into the hands of researchers who are motivated by publication pressures, and who like the look of "hard science" that P-values and "significant at ..." statements add to otherwise insignificant findings (e.g. economically insignificant differences). I can't however claim any credit for Dennis's humorous statement that by comparison with Bayesian methods, significance tests more resemble a tom-tom than an orchestra.

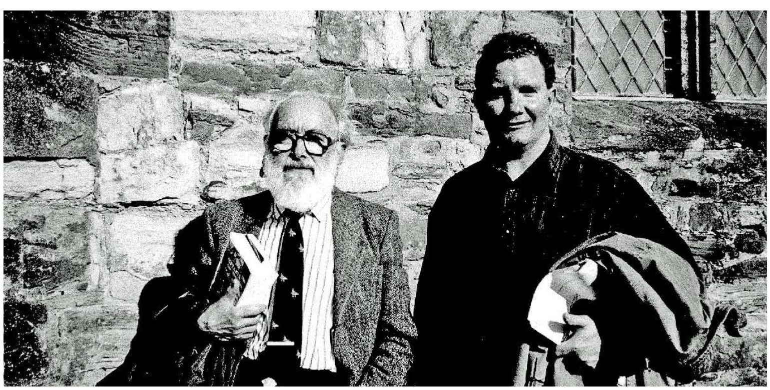
Until I started to teach an MBA class from Dennis's book *Making Decisions*, I had never heard of a probability scoring rule. At that time, around 1989, this made me like about 99% or more of all people who had ever studied a statistics course. I was always amazed when speaking to statisticians that they did not know even the words “scoring rule”, but this was because scoring rules, apart from their use in weather forecasting, were almost uniquely a Bayesian thing. Bayesians of course accommodated statements of personal probability, or of the probability of a hypothesis, and were interested in evaluating their accuracy. In 1990 I met Ward Edwards at a conference in California, and he invited me to present a class at Stanford on scoring rules, which Ward, who had worked with Savage, knew much about. I remember one student there, a mature business type, saying in front of the class “that’s a million bucks”. I have always wondered why in business and places such as legal practice, where probability judgements are critical and often made routinely and repeatedly, that there has not been a corporate consultancy that discovered the beauty of scoring rules, like I had in Dennis’s book, and made that million bucks.

I have written three papers with Dennis. The first one concerns the Bayesian interpretation, not of P-levels, but of alpha-levels. We consider the probability of a null hypothesis given only the information “significant at alpha”, with given fixed alpha. This is an extension of Dennis’s famous paper on the Lindley paradox. It came about because Dennis saw some results that I found in a simple case where the sample space was discrete, and suggested that he would show me how these could be generalized. I remember that the editor of the journal made us squash a twenty page paper into ten. I took up this task and was very happy with myself when Dennis wrote me a letter to say how satisfied he was with the reduced form of the paper. We might have erred by publishing this paper in *Theory and Decision* rather than in a more mainstream statistics journal, as it remains little cited despite its importance as I see it for its addition to Dennis’s 1957 paradox paper.

The second paper, also published in *Theory and Decision*, came from me getting interested in methods of financial portfolio selection based on mean-variance. In discussing these methods with Dennis, he became immediately critical and suspicious of their inherent reduction of arbitrary probability distributions of money payoffs to only their means, variances and covariances. As it turned out, similar suspicions had been investigated right from the time mean-variance portfolio theory was proposed by Markowitz in the 1950-60s, by a very clever Norwegian insurance theorist called Karl Borch. Dennis and I both read some papers in economics that we had never known about, and Dennis came up with some really new results. For one thing, he gave a general proof that mean-variance necessarily implies a quadratic utility function, when previously the proofs in the literature were all in the other direction (i.e. that quadratic utility implied mean-variance). He also produced a philosophical rebuttal of mean-variance generally, much in spirit with Borch’s earlier work. I remember that Dennis, who did this work in recent years, was delighted with one referee’s description of it as “masterful”. He said that it made his day. After much study, and a lot of thinking and re-thinking of both our respective understandings, Dennis and I produced a general review and critique of Borch’s attack on mean-variance. This paper, which is to be published in 2013 by *Statistical Science*, takes a more conciliatory line between mean-variance methods and utility theory than Dennis would likely prefer, but the hallmarks of his acute mathematical thought are still evident, and in the end I think he is very happy that it is to be published. It took a great amount of work, as it traversed a deep, wide and and now largely forgotten philosophical literature in financial economics and related disciplines. It is a symptom of how disciplines do not always give each other enough regard that this literature was new to Dennis, who found its parallels with decision analysis both troubling and fascinating. To top off his ambivalence, it has recently been claimed by none other than Markowitz, who won a Nobel prize for his portfolio theory invention, that the true inventor of mean-variance was none other than de Finetti, a man whom Dennis often mentions as the greatest of minds.

Dennis and I have one other piece of unpublished work. Like much of our work, I found some results (to do with scoring rules and their ability to rank forecasters in correct order) and Dennis added a general perspective that confirmed what I had found and made more sense of it. Interestingly, my findings were corroborated by Dennis, but were not at all what he expected. I am prompting myself to complete this work, as I owe it to Dennis for everything that he has done for me and all that I have learnt from him. Like many of his friends and co-workers, I have lunched with him at The Castle in Taunton several times over the years and these are among my most precious memories of academic life. On one occasion my wife Alison was travelling with me and joined us. Dennis was his charming self and since then he has always enquired of us as a family, and, better still, Alison knows first-hand what a wonderful character and friend he has been to me.

I will add one last story that is all about Dennis as a person. A few years back, I arrived at The Castle very early for an agreed lunch meeting. I did not want to be a second late, my mother would have been disgusted and I knew that Dennis was always very punctual. I



Outside The Castle, Taunton, 1998

stood outside the restaurant door waiting for Dennis to arrive, confident that I had beaten him there. After half an hour he had not arrived and I was very surprised. I looked inside the restaurant several times, but no Dennis. Then at some point, when I was worried that he might have forgotten or had some trouble in his car, I did a very thorough search of the building and there to my shock was Dennis, hidden in an anteroom, where it transpired he very often waited for visitors. He was happily discussing wine with the waiter, who had been looking after him to his great satisfaction. I was very embarrassed as it seemed that I was late. The problem was that Dennis had outdone me in arriving early, and I had not known his habits well enough to look for him in his usual spot. I should not have been so confident in my belief that Dennis had not gone through the door before me, and I should have allowed for him to be waiting in a cosy lounge chair, with waiter at hand.

Jay Kadane

I first met Dennis Lindley when I arrived in London a year out of graduate school, on a consulting assignment. I knew of Lindley, of course, and had a letter of introduction from Jimmie Savage (this was before the days of instant email communication). We talked for quite a while, and I wound up having dinner at his home that evening.

Ever since, Dennis has been a friend and mentor. Many of my papers were written with him in mind as my target audience. I admire the clarity of his thinking and writing, and his willingness to rethink his position in the light of new evidence and ideas. He certainly holds up his side of an argument, but his goals are always intellectual. There is no ulterior personal agenda, even when he's at his most critical.

We never wrote together, but his ideas and standards are always with me.

Happy Birthday, Dennis!

John Kingman

In 1951 David Kendall read a paper to the RSS on "Some Problems in the Theory of Queues", a theory which had been actively developed in the world of telephone engineering over the previous thirty years but was largely unknown to British statisticians. The vote of thanks was proposed by Dennis Lindley, and he pointed out a simple equation which was probably the most influential single advance since the original work of Erlang. In a queue in which customers are served in order of arrival by a single server, if one customer waits a time w before entering service, if his service time is s and the interarrival time before the next customer is t, then the waiting time of that next customer is the greater of $w + s - t$ and 0. All the models considered by Kendall, and most used by earlier authors, had the successive differences $s - t$ forming a sequence of independent random variables with a common distribution. This subsumed much of queueing theory within the well-developed theory of random walks, and Lindley himself pointed out that if the mean of s is less than that of t, then a stationary waiting time distribution exists, and is determined by an integral equation (subsequently solved by W.L. Smith).

I encountered Kendall's paper while working in my first summer as a Cambridge undergraduate at the Post Office Engineering Research Station. I had already attended Lindley's first year lectures introducing probability and statistics in a beautifully clear way (without a mention of prior or posterior probabilities). On returning to Cambridge I found that he was down to give a course on "Random Variables", which would better have been described as "Theory of Probability" had not that title been arrogated by Sir Harold Jeffreys, the Plumian Professor. The course lived up to all expectations, and was enriched for me and my contemporary John Bather by four specially arranged supervisions which Dennis agreed to give because neither our pure or our applied supervisor felt that probability came within his remit.

Lindley was able to convey the excitement of the subject without any loss of clarity or rigour. He presented probability theory as very much an applicable discipline, but one with firm mathematical foundations and clear logical structure. I owe him a great debt for showing me what a fertile and elegant branch of mathematics it is, and half a century later I still cherish the memory of his teaching.

Frank Lad

A tribute to Dennis

Dennis has loved New Zealand. He first spent two months here on an Erskine visit to the Mathematics and Statistics Department at the invitation of John Deely in the late 70's. Along with the natural and cultural beauties, he became quite a fan of Samuel Butler, an accomplished Englishman of the nineteenth century who engaged in the yeoman work of settling the high mountain ranges of South Canterbury for sheep farming. Although his taste in leisure reading runs largely in non-fiction, Dennis became enamored with Butler's zany novel *Erehwon*[1]. I remember well a delightful day in the early 90's during his second Erskine visit here at John's invitation when the three of us drove south through the plains in the shadows of the ranges, arriving at Erehwon station on the banks of the upper reaches of the Rangitata River. We lunched on the gravels of the braided river.

Happily, I had already become friends with Dennis well before this visit, and we had engaged in regular correspondence about statistical issues. How pleased I was when upon arriving he asked about the progress of my text manuscript[2], and further offered to read it as a base for discussions during his time with us! Any working author is welcome to drool. Some of my text was in well revised form, being used already for stage two mathematics lectures in probability. But much of it was in free-wheeling form appropriate for an initial draft of a spirited author with something to say. Dennis did not write extensive notes in the margins, but rather would pencil in a remark or two that signaled an area we should discuss. ... and did, in wonderful walks around the university gardens surrounding the staff club. One of my treasured intellectual records is the draft copy in which I raged about Kolmogorov's[3] "braying about independence", when I introduced the sections on exchangeability. Dennis has pencilled into the margin in his fine hand ... "Kolmogorov equals donkey?" The allusion did not pass my next edit.

I suppose this story signals our deepest scholarly differences. On the subject of probability and its subjective sources we have been in deep agreement. Our differences on subject matter wound mainly around matters of taste ... is de Finetti's betting-based construction preferable to the characterisation of belief preferences such as discussed in DeGroot? ... and tactics. My attitude towards our shared subject is based on the fact that the realm of every measurement we can actually make in any field whatsoever, the subject of applied statistics, is finite and discrete. Continuous distributions can be useful as approximations, but inferential issues that bear upon continuity and measurability are completely irrelevant to real problems. Perfectly additive measures on Polish spaces? Really?

Dennis Lindley ... thoughtful opinionated gentleman scholar. Friend. Bravo 90!

[1] Butler, S. (1872) Erehwon, or, Over the Range, London: Trubner and Co.

[2] Lad, F. (1996) Operational Subjective Statistical Methods: a mathematical, philosophical, and historical introduction, New York: John Wiley.

[3] Kolmogorov, A. (1933) Theory of Probability, N. Morrison (tr.), New York: Chelsea.

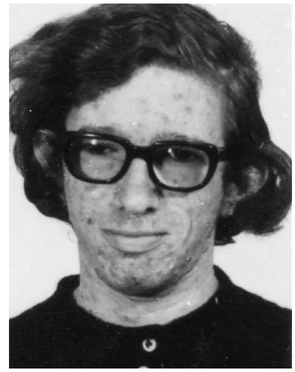
Peter Lee

I was never fortunate enough to be a colleague of Dennis's, although I have met him frequently throughout my career. After graduation from Liverpool in 1962, I went to the Cambridge Statistical Laboratory as a research student in probability theory, initially under David Kendall and subsequently under John Kingman. Although at that stage my work was not concerned with statistical inference, I felt that as that was a major interest of those around me, it would be good to learn something about it. When I started to do so, I felt confused, confused. Not because the mathematics was difficult—most of that was a lot easier than pure mathematics—but because I found it difficult to follow the logic by which inferences were arrived at from data. It sounded as if the statement that a null hypothesis was rejected at the 5% level meant that there was only a 5% chance of that hypothesis being true, and yet the books warned me that this was not a permissible interpretation. Similarly, the statement that a 95% confidence interval for an unknown parameter ran from −2 to +2 sounded as if the parameter lay in that interval with 95% probability and yet I was warned that all I could say was that if I carried out similar procedures time after time then the unknown parameters would lie in the confidence intervals I constructed 95% of the time. It appeared that the books I looked at were not answering the questions that would naturally occur to a beginner, and that instead they answered some rather recondite questions which no-one was likely to want to ask. I thought about these matters and still found it difficult to make sense of the standard theory. Burrowing around in the library of the Statistical Laboratory, I came across some lecture notes by Dennis in which he derived results numerically similar to those coming from the standard theory but with a much clearer interpretation from a Bayesian standpoint using conventional priors. Stimulated by this, I read Jeffreys' *Theory of Probability* and some of the rather limited amount of other Bayesian work then available. Not long after that I reviewed his book *Introduction to Probability and Statistics from a Bayesian Viewpoint* for *Eureka* immediately on its publication.

All of this was peripheral to my main interests in probability until I moved to York as a Lecturer in Statistics in 1975, whereupon I felt myself bound to devote more of my time to statistics. I began by teaching conventional statistical courses, but I became increasingly dissatisfied with them. I thought of trying to teach the Bayesian approach using conventional priors as Lindley had done, but that approach too has its limitations. I came to the conclusion that the only thing to do was to devise my own Bayesian course, but by the time I came to do that, Dennis was also there to help me through his *Bayesian Statistics: a Review* published by S.I.A.M. These lectures eventually developed into my book (now in its fourth edition). It is hard to believe that I could ever have begun on this work without the impetus given by Dennis's contributions.

I have always enjoyed his stimulating polemic, in which he usually succeeds in making telling points. Just occasionally I have known him fail—I recall one occasion on which he dismissively said, "At this point advocates of the Neyman-Pearson theory would tell us to do so-and-so" only to be met by a riposte from Egon Pearson, "I've never said any such thing in my life," which reduced Dennis to, "Most advocates of the Neyman-Pearson theory."

To my mind there is no doubt that he has been the leading light of Bayesian statistics in Britain throughout my career.



Barry Leventhal

How Dennis Lindley changed my life

It was December 1967 and I was looking for a university place, having passed my A-levels that summer. UCL was my first choice, and I was excited to be asked along for an interview in the Department of Statistics.

My inquisitors were Dennis Lindley and the late Dave Walley. After the usual introductory questions, they turned to my choice of degree subjects. "Why did you apply for the combined Economics and Statistics course?" asked Dave. "Well, I did not meet the entrance requirements for the pure Stats course", I pointed out helpfully. "Oh, don't worry about that!" responded Dennis. "You don't want to study Economics – come and do Statistics with us."

I was happy to accept and enjoyed an excellent three years with UCL Statistics, followed by two further years there as a research student.

The outcome was a career in Statistics (for the last 36 years and still counting) – so I can honestly say that my first meeting with Dennis Lindley probably changed my life!

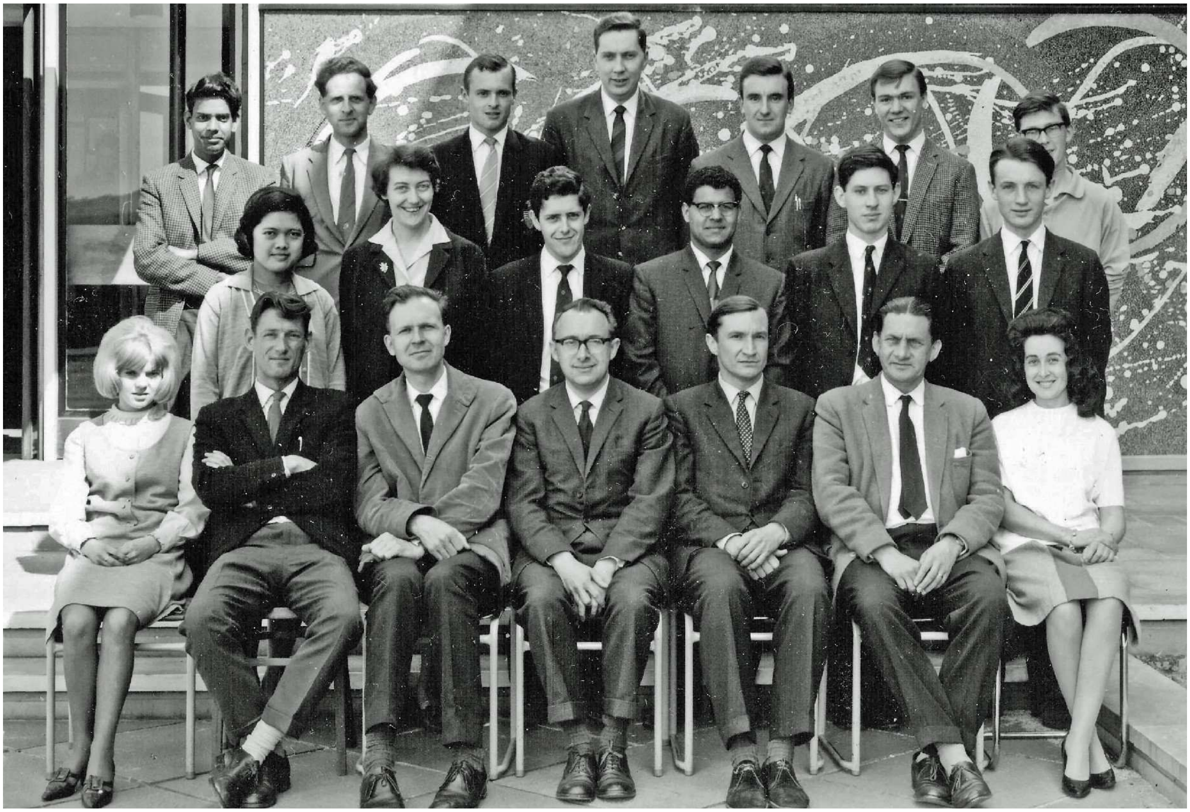
Sylvia Lutkins

I first met Dennis when I came to Aberystwyth in October,1961, to begin his brand new postgraduate diploma course in Statistics. The department was housed in 9, Laura Place, near the Old College and the Students' Union. There were 10 of us taking the course. David Bartholomew, Mervyn Stone and Dennis gave the lectures, and Donald East taught us how to do statistical calculations on a Brunsviga mechanical calculator.

Apart from the actual content of his lectures, I remember being so impressed by Dennis's beautiful copperplate handwriting on the blackboard!

The department moved up the hill the next year, to occupy the new Physical Sciences building, along with Pure and Applied Mathematics and Physics. One afternoon in September, Dennis and I took delivery of the college's first computer, an IBM 1620. The engineer showed us how to operate it, using paper tape input and typewriter output, before leaving us to play with this exciting new research tool. Dennis had worked out, in IBM machine code, how to play the game of Nim (3 piles of matchsticks), and we were able to use this to entertain all the important visitors to the College who were brought along to see the computer.

The IBM 1620 computer, with paper-tape input and output, in the Statistics Department, Room 102, Physical Sciences building, Penglais Campus, Aberystwyth, 1962.



Statistics staff and postgraduates outside the Physical Sciences building, in 1964 – 65. Dennis will remember some of his research students in this photo :- Sami (M.Samiuddin, back row, far left) and Gulal El-Sayyad (middle row, fourth from left).

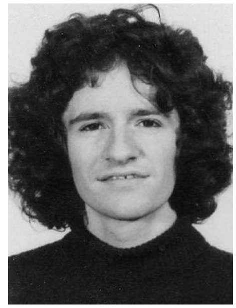
1963 was the year of the big freeze. Fortunately, the Physical Sciences building benefitted from having the same water supply as the new Bronglais Hospital being built further down the hill.

Those of us who lived out of town had frozen pipes for many weeks. When the thaw came, the house I was renting near Borth was flooded out. So I was thrilled when Dennis asked if I would kindly look after their new house in Dan y Coed while he went to the U.S.A., with the family, for a six-months sabbatical. My friend Pat Phillips, a postgraduate student that year, and I enjoyed playing the pianola, and reading the children's story books. The woodland behind the house is now a SSSI, called Parc Natur Penglais, looked after by local volunteers. [We have won two Green Flag awards recently.] Local schools and students also join in the working parties, which meet regularly to clear the brambles and keep the gorse at bay. Visitors [30,000 per year, according to the hidden counters], come from far and wide to see the bluebells, follow the circular trails and listen to bird talks by experts.

The Statistics Department continued to thrive under Dennis's leadership. Lecturers in Computer Science were appointed, and we were also responsible for giving statistical advice to scientists throughout the college. We needed more space. He helped to plan the new extension to the Llandinam building, which would accommodate the new Elliot 4130 computer, a large undergraduate laboratory with Casio electric calculators for Statistics practicals, lecture rooms for the growing number of Diploma/M.Sc. students, and study rooms for all our research students. We moved into this wing in January, 1966.

Sadly for us, Dennis left Aberystwyth the next year, to go to UCL. However, we were very pleased when he was appointed as an Honorary Professorial Fellow to Aberystwyth, in November, 1977, which has enabled us to benefit from meeting him regularly at the annual Statistics Conferences held in Gregynog since 1964/5.

Kevin McConway



I first encountered Dennis Lindley in October 1972, when I arrived at UCL to begin an MSc in Statistics. Dennis was Head of the Department of Statistics and Computer Science, as it then was. (Note the order of the disciplines in the title!) He addressed the new MSc and Postgraduate Diploma students on our first day. I don't recall many details, except that Dennis came over as approachable and human. (I don't think I even knew who the Head of Department was at the university where I had been an undergraduate.) I do recall, though, that Dennis told us that we should make a point of attending the Department's weekly Journal Club, and the University of London statistics seminars. He said that we wouldn't understand all that we heard, but that we should go anyway and see what we picked up. He added that he often didn't understand everything either.

It quickly became plain, however, that Dennis's lack of understanding of things in seminars meant something very different from my own lack of understanding. The first few talks I went to might just as well have been about Sanskrit rather than statistics, as far as I was concerned. But he was right that you can still learn a great deal from trying to make sense of something you don't really follow, and eventually I started to get a lot out of seminars and talks.

Lack of understanding was not, however, an issue for me in the MSc lecture course that Dennis gave to MSc students that year. I seem to recall that it was originally billed as being on Educational Testing, but by the time the lectures began, it was on Bayes methods for the linear model. This was, of course, not long after the Lindley and Smith (1972) paper on that topic [1]. Dennis's lecturing style was wonderfully clear and persuasive, and I was soon convinced that this was the only reasonable and logical way to deal with the linear model. It has to be said that, at the time, my understanding of non-Bayesian ways to analyse linear models was rather patchy, to say the least, so Dennis's carefully constructed and persuasive arguments did not meet much resistance from me. But, even though I have since been responsible for many data analyses of which Dennis would certainly not approve, his advice to avoid ad-hockeries, to think about why one is doing something as well as what one is doing, and to be coherent in every sense, has stayed with me ever since.

[1] DV Lindley and AFM Smith (1972), 'Bayes Estimates for the Linear Model', JRSSB, **34**, 1–41.

Ann Mitchell

For Dennis Lindley on his 90th Birthday: Distant Memories and Future Hopes

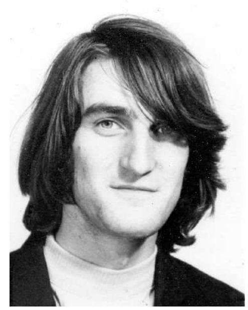
Arriving in Cambridge in 1957, from the very different traditions of Scottish life, academic and otherwise, was a daunting experience. The Fellows of Girton College were formidable women, as their outstanding achievements had demanded. Memories of inadequate heating and piercingly cold winds, blowing across the Fens, can still chill me to the bone. As my background in Statistics was very rudimentary, I am sure that my first meeting with Dennis Lindley must have been a daunting experience too. Nevertheless it cannot have been formidable as I have no memory of it whatsoever. However, at the age of 21, I was a little in awe of his greater age!

It was a time of burgeoning interest in Statistics worldwide, pointing to a wealth of opportunities for graduates in the future. The Cambridge Diploma in Mathematical Statistics was already well established in 1957-1958 and, perhaps surprisingly, in that year female students were in the majority. The lectures were given exclusively by Dennis Lindley and Wally Smith. Could there have been anything better? On the other hand, the practical classes were dominated by struggles with Brunsvigas. The memory of inverting matrices is still acute! Unfortunately, I lost touch with my fellow students and have no news of them.

At the very beginning of my Ph.D. studies, Dennis Lindley asked me to write down what I had done so far. He was insistent, even when I expressed doubt about having done anything. Under pressure I produced my paper. Dennis very kindly described it like a homework solution to a mathematical problem. He urged me to go away, try again and not to return immediately, as writing was difficult. I was to use more English, not exclusively mathematical symbols! This process continued through four more attempts, always encouragingly received with compliments on improvements, until, on the fifth attempt, he responded with something like 'Excellent, you have now learned how to write a scientific paper and you will never forget this training'. Not only was he correct, but also he was instrumental in my applying the same learning procedure to my own students, who became even more needy with the growing emphasis on projects in undergraduate courses. Dennis firmly believed that it is the writer of a paper, scientific or otherwise, who has the responsibility to make the subject matter clear to the reader, not the reader's responsibility to struggle to attempt to understand it. He has always been concerned with originality, elegance, clarity and economy in all aspects of his subject and was willing to devote time to developing these strengths in his students. Despite holding strong views, which he will defend robustly, he respects and enjoys the well-argued views of others and indeed, in my opinion, freed his students to pursue their own interests, provided they could give strong justifications for their decisions.

In 1960 the scene moved to the University College of Wales, Aberystwyth. Very quickly, with his reputation and leadership in research, Dennis attracted young vibrant staff of high academic quality and many students and visitors. Complementary to that he showed his business acumen and administrative skill in establishing a department with excellent facilities. The library was particularly noteworthy. Facits replaced Brunsvigas but, of course, it was the development of computing facilities which predominated. Many long lasting friendships were formed in Aberystwyth, not least with Dennis and Joan Lindley, who welcomed us to their home and were especially supportive and understanding in difficult times.

Although life in Aberystwyth had many advantages, the location was remote and many new opportunities elsewhere were bound to attract the relatively young and ambitious staff. When career decisions had to be made, Dennis always gave selflessly good advice, not necessarily what would be to his own or his department's immediate advantage. Although I moved on and experienced many departments, Dennis Lindley has never been surpassed in his ability to encourage and inspire, to criticise in a positive, non-personal way, to free the individual and to create a happy department, where members of staff feel fulfilled and appreciated. On this occasion of his 90th birthday I salute him as someone who has made a significant difference to my life and whose teachings have inspired me in many ways. May Dennis continue to be an inspiration to young and old alike and, with Joan, enjoy many more happy, healthy and fulfilling years .



Tony O'Hagan

To Dennis on his 90th birthday, with thanks

I owe so much to Dennis Lindley.

UCL

I first met Dennis at UCL in 1967. I had started the BSc Statistics course the previous year, when Bartlett was the Professor. The society for students in the Statistics Department was the Pearson Society, and I was elected Treasurer. Soon after Dennis arrived, the Society's officers went to petition him to give us a common room. Dennis's response, I have come to realise, was typical of his management style: he was very sorry, but the University did not allow departments to provide common rooms for undergraduates … so we would have to call it something else. We were given a room that was there and then named the Moot Room. We had a few items of furniture that were not needed elsewhere and Dave Walley (to whose kindness to students no words of mine could do justice) gave us an old carpet. The Moot Room allowed students in different years of the degree course to socialise. I remember in particular some intense groups of bridge players.

In 1968, during my final summer vacation, I was given a job cataloguing old books, lecture notes and other items hidden away in cupboards in the departmental library. I believe that Dennis knew my finances were somewhat strained and I suspect that the job was due as much to that as to any real need the department had for the surely rather amateurish cataloguing I did that summer. It did have the side effect that when the new term started I actually knew more about what was in the library, and indeed had also read more, than my contemporaries.

In my final year, I had a course taught by Dennis on Bayesian Decision Theory. I loved Dennis's wonderful clear presentation, supported with notes beautifully handwritten on the blackboard. He was easily the best lecturer we had that year. Nevertheless, I left UCL as a frequentist. Everything else in the degree programme was frequentist, so it was easy to think of the Bayesian approach as a clever novelty with little bearing on practical statistics. After all, it was obvious to me that more than 99% of statistical analysis in the real world was frequentist. (Also, Dennis's exam was the toughest I had faced in any of my three years!)

CEGB

After graduating, I went to work for the Central Electricity Generating Board in their statistics group. We were based in London, in a building beside Bankside power station (now the Tate Modern), but we provided services to the CEGB's three research laboratories around the country. I worked mostly with researchers in the Berkeley Nuclear Laboratories. It was while at the CEGB that I became a Bayesian. My conversion came about simply by observing how real scientists interpreted the frequentist statistical inferences that I was giving them when I analysed their data. They automatically thought of a confidence interval as if it were a Bayesian credible interval, and they automatically factored in their prior knowledge. For instance, if I gave an estimate that was higher than they expected, on the basis of experience with their own experiments or the results of others, they would presume that the parameter was more likely to be below my estimate, rather than above. It was only because I had benefited from Dennis's inspiring course at UCL that I was able to see that these scientists were all natural Bayesians.

So I became a Bayesian myself. I made some small Bayesian contributions at the CEGB, but decided that I had to go back to UCL to do a PhD under Dennis's supervision. I was delighted that he accepted me.

And beyond

My postgraduate years (1971-73) at UCL were wonderful. I didn't see Dennis very often because I only went to him when I had something to say or to ask about, but every conversation opened my eyes in one way or another. The whole atmosphere was enormously stimulating. I particularly enjoyed and was stimulated by discussions with several other postgraduate students – especially Tom Leonard but I also fondly remember Sylvester Young, Kevin McConway and Geoff Robinson.

From that time onward, throughout my career, Dennis's rigorous and deep thinking has been my rock, my unshakeable foundation. I have sometimes disagreed with Dennis, but always reluctantly and only after very careful thought.

Dennis read the manuscript of my first book, *Probability: Methods and Measurement*, and his comments were enormously valuable. In particular, he showed me how to write more sharply and succinctly – I have since tried to follow that advice in everything I write, but the results are surely not a match for Dennis's own immaculately clear style.

Here is one more example of how his incisive thinking has influenced me. In the first Valencia meeting, there was much interest in quadrature using importance sampling, a kind of Monte Carlo method, to compute posterior moments. Dennis criticised this during discussion, pointing out that Monte Carlo is a frequentist method. He said that estimates produced by any quadrature method were inferences, and inference should always be Bayesian. This led me to a series of papers in which I tried to follow through that idea, eventually resulting in my recent work on uncertainty in the outputs of simulation models. Today's Bayesian statisticians should take note, because Dennis's criticism applies equally well to the ubiquitous MCMC methods.

I could give more instances, but I think the above are enough to show how much I have gained over the years from Dennis – from his kindness and generosity as well as from his intellect. I am greatly privileged to count him as my friend as well as my mentor.

In March 2013, I interviewed Dennis for the RSS meeting to celebrate the 250th anniversary of the publication of Bayes (1763)

Clive Payne

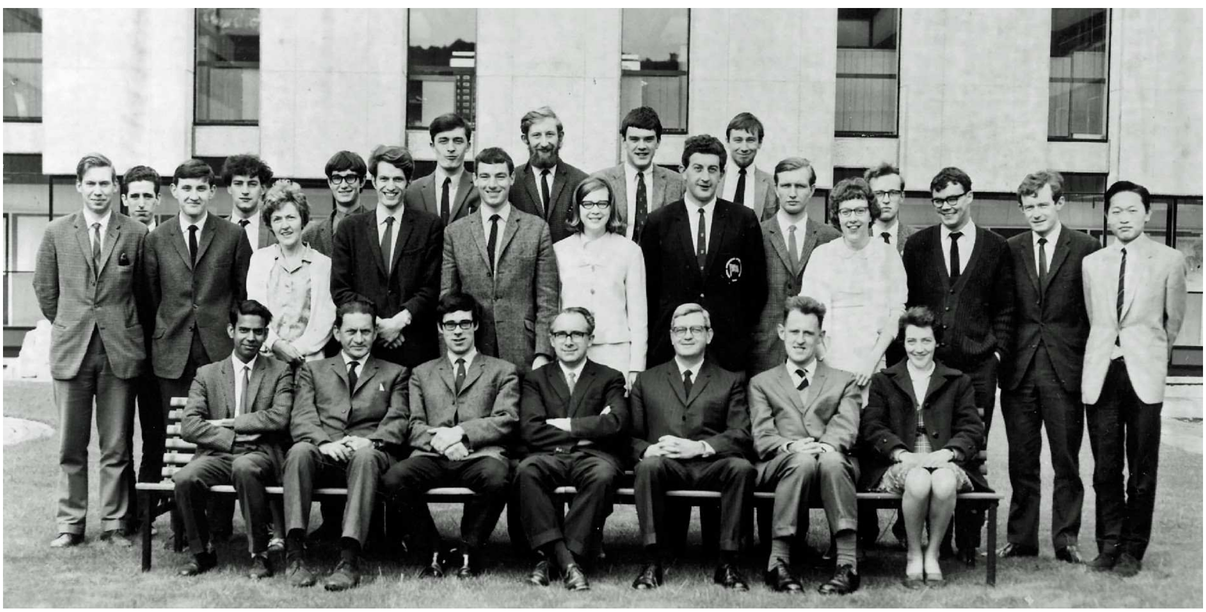
I will be forever grateful to Dennis for taking me on a postgraduate student in 1963 on the Diploma/MSc in Statistics in the Statistics Department of the University of Wales, Aberystwyth. I had just graduated in Geography so it was somewhat of a risk to select a non-mathematician to join all the other mathematics graduates on the course. The course and my subsequent appointment as an Assistant Lecturer in the Department a few years later led to a career in computing and statistical modelling in the social sciences spent largely in the Faculty of Social Studies and Nuffield College, University of Oxford.

I remember the department as being a very stimulating but friendly and happy place to be. Aberystwyth was a sociable place to work and Dennis's department was no exception. Dennis had recruited very good colleagues including stars such as David Bartholomew, Mervyn Stone, Ann Mitchell and Peter King. The department even had its own computer – an IBM 1620 – which was quite something in those days. It filled a whole room and ate paper tape. In fact the very first week of the postgraduate course was spent learning to program in FORTRAN at the delightful Gregynog mansion owned by the University of Wales. This course was to prove a very good foundation for me in the rest of my career. We still had to do practicals on Facit calculators though. They weighed as much as a sack of potatoes and were operated by turning a handle.

On the course I met another postgraduate student, Philip Brown, and this was the beginning of a thirty-year collaboration on election-night forecasting for the BBC, ecological inference and exit polling. I'm not sure I can claim to be a Bayesian – although Phil Brown tells me that the choice of the value four for the ridge constant in our election-night forecasting ridge regression model had a Bayesian justification. But I still remember Dennis's inspiring lectures on Bayesian statistics. And I still try to apply his decision-theoretic approach when deciding whether to take a raincoat when there is a prospect of rain.

Aberystwyth Statistics Department in 1966

Contributors to this book: Sylvia Lutkins(front row, far right), Ann Mitchell (front, third from right), David Bartholomew (front, fourth from left), Clive Payne (second row, far right), Philip Brown (back row, third from right),



Aberystwyth Statistics Department in 1967

Aviva Petrie

Memories of student life in the UCL Statistics Department in the 60s

As an undergraduate at UCL in the swinging 60s, it was with some trepidation that I entered the Statistics Department (then housed in the Pearson building overlooking the main quadrangle) for the first time in 1966. Was it how I might have expected – flower power, the gentle waft of hash permeating the air, students of both genders with long hair wearing flip-flops, Beatles music thumping in the background, and views of women burning their bras in the quad? Not a bit of it. I walked into the stats department to find it was staid and quiet, with its inhabitants conforming to the description of a typical statistician. In fact, it is said that there are two types of statistician – the introvert who looks at his shoes when he talks to you and the extrovert who looks at your shoes when he talks to you.

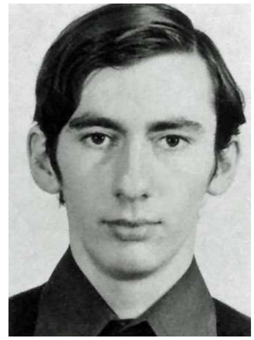
I intentionally use the word 'he' as there was a preponderance of males in the department. In my year, there were 16 undergraduates of whom only 3 were females. There were few staff members of the fairer sex, but notable amongst them was the indomitable Professor Florence Nightingale David, a distant relative, I believe, of her namesake. She gave the first lecture on my first day as an undergraduate, one that remains firmly etched in my consciousness, although I have to admit that much of my student experience has dissipated in the intervening 43 years. It was a warm and sunny October day. Prof David, dressed in her customary grey pinstripe skirt suit (trouser suits for women were little known at that time), white shirt, heavy brogues, hair cut really short and smoking a cigar, marched in, opened the window to the quad, and proceeded to start her lecture whilst leaning out of it. She informed us that teaching fledgling undergraduates was not what she aspired to, but it was part of her contract as an academic and therefore a necessity. She told us that if we concentrated and worked hard, we would learn much from her. How true that was: she was an incredible person, an inspiring teacher and one who set me on the path of a statistical career. Sadly, I was in the minority amongst my peers, both in my admiration for her didactic abilities and in becoming a statistician. Many of us had little prior knowledge of statistics before starting our degrees (it was not necessarily part of the maths A-level curriculum) and quite a few found that it was not to their taste. The financial benefits of the actuarial life attracted some, the burgeoning IT industry lured others and a few went in completely different directions. However, the tuition we received was rigorous and comprehensive and the emphasis on theoretical concepts appealed to me and clearly to many others of my generation, amongst them Professor Tony O'Hagan who was in my year, and Professors Phil Dawid and Sir Adrian Smith, both of whom were undertaking Master’s degrees when I was in my final year and with whom we had some joint lectures.

Lectures, of course, were only part of our education, using this term in its broadest sense. There was much to be learned in the Moot Room, the statistical common room which formed the hub of our existence. It was there that I understood that statistics and bridge go hand in hand (no pun intended). Statisticians ran the UC (it was simply UC in those days, not UCL) bridge club and, no doubt, they gathered much of their expertise by spending many months, weeks, days and hours refining their skills in the Moot Room. I had never been exposed to bridge and a kind second year student offered to teach me how to play. A quick overview of the rules, a cursory explanation about bidding, including a brief mention of the conventions, and I was invited to join a game. It was something from which I have never recovered – the cards were dealt, the bidding completed and we started playing. One quick hand and everyone put their cards on the table whilst I looked on in astonishment. Statisticians are pretty good on the whole at probability, and probability is an important component of bridge. Put the two together and there is no need to actually play the game! And since that time, I never have! What I did learn, however, was to play bar billiards. Our maths lectures, and we had many, took place in the maths department across the quad, just above the bar which may (Alzheimer's beckoning?) have been part of the student union. I don’t remember if we had to cross the bar to get to the lecture room or whether it was a convenient short cut, but I do know that bar billiards provided quite a bit of light relief from the not very inspiring maths lectures.

Life as a statistics undergraduate was not all fun and games, though. Our timetable was packed for 4 ½ days a week, the Wednesday afternoons being left free for sports, in common with other departments at UCL and universities. Being a relatively small group, our lectures were akin to lessons, and the style of teaching was not that different from that which I had received at school. We had no seminars and our ‘homework’, of which there was a fair amount, was hand written, as this was before the days of personal computers. Main frame computers were used and we had to learn how to program them. This involved one lecture a week to learn a particular programming technique, one homework a week to write the relevant program and one ‘run’ a week on the computer. To this end, we had to produce paper tape with the computer commands punched into it which we would hand in to the IT department, receiving the output about 5 days later. If we had made a mistake, for example, put a comma in the wrong place, we could correct it and have another run the following week, and so it went on. It was a nightmare – wrestling with the paper tape to ensure it remained intact in a small room packed with students was the first hurdle to overcome but this was nothing compared to the problem of trying to remember at the end of term what the original tasks were in the assignments in the early and middle weeks of the term. I may have complained bitterly at the time, and I know I had a strong aversion to computing for many years to come, but having a knowledge of programming was of real benefit to me in the early days of personal computing when DOS commands were the norm, and is now when using command driven rather than menu driven statistical software.

In those prehistoric days we did not have the luxury of the internet, laptops or even hand held calculators. But we had the Brunsviga, a distant cousin of the abacus! We were each assigned one of these mechanical calculators which had levers and knobs and handles and weighed about eight pounds. Some basic instruction and we were off, able, after adjusting the levers and much turning of the handle in one direction or another, to add, subtract, multiply, divide and even take square roots. The results from all our practical exercises were obtained using the Brunsviga: this meant that we had to understand the principles underlying the procedures, for example regression analysis and ANOVA, and know how to apply the formulae, something that has stood me in good stead in my years as a statistician. I doubt whether we ever ended up with the right answer as it was all too easy to turn the handle once too often or slip one lever to a six, say, instead of a five, but it was a lesson in fortitude, particularly in the final seven hour practical exam at the end of our final year, when the examination room reverberated with the not so gentle sound of 16 handles being frantically turned. In those days, although we had exams at the end of the first and second years, they did not count towards our degrees, so the end of the third year was a very pressurised time. The results, too, were not as they are today when any degree below a 2:1 is regarded as pretty worthless. In my time, the most common degree class was a 2:2, and this was reflected in our results, with only three firsts and two 2:1s being awarded and nearly everyone else receiving a 2:2.

I was fortunate during my undergraduate years to have been surrounded by some truly eminent and inspiring statisticians, amongst them Prof Egon Pearson, who had been Head of the Applied Statistics Department (as it was then called) in the thirties and who could be seen frequenting the corridors from time to time. Professor M.S. Bartlett, the stereotype of the quiet statistician, was Head of Statistics when I arrived in 1966 but he retired at the end of 1967 and was succeeded by the more charismatic Professor Dennis Lindley, about whom much is written in this fitting tribute to the man on his 90th birthday. As a mere undergraduate, I have to admit that my contact with Dennis was minimal, but I hope that these fond memories of an interesting time in a remarkable department provide some indication of what it was like to be a student there at the start of his days at UCL.



Lawrence Pettit

Thoughts of Dennis Lindley

I had the good fortune of studying for the MSc in Statistics at UCL in Dennis Lindley's final year there.

Dennis was an inspirational teacher. In his graduate level course on Bayes Methods for Linear Models he sat at an overhead projector and beguiled us with the elegance of that subject. I still have my notes from that course and still use some of them as the basis of some of my own graduate level course on Bayesian Statistics. There was a wonderful atmosphere in the Department. Much of this was due to the excellent group of staff that Dennis had attracted around him. Largely, but not exclusively, Bayesian it was a formidable group. Even as MSc students we were encouraged to attend seminars and the tea club. The talks could be really inspiring and after one tea club meeting led by Dennis I can remember thinking about it all the way home and half way through the night. I approached him the next day with my own rather naive thoughts about the problem and he treated me in his usual generous way, encouraging but pointing out the flaw in my thinking.

When it came to choosing a project title there was one on analysing data from the battle of the bulge supervised by Dennis. I was interested in military history and liked the idea of working with him so that I could employ some Bayesian methods. In fact Dennis and Alan Skene had already written a report using very classical multiple regression models for these data. I couldn't see that there was much scope to do much more with it. But in the introduction there was a mention of Lanchester's (1916) equations and so Dennis told me about those. They were sets of simultaneous differential equations which could be used to model attrition in battles. They could also be turned into stochastic versions. It was about then that the penny dropped that Dennis was synonymous with the Lindley of Lindley's (1952) equation which I had learnt about in Queueing theory. So I learnt about various stochastic models but was a bit miffed not to do anything Bayesian in my project! It must have rankled because many years later I returned to the problem and in Wiper, Pettit and Young (2000) and Pettit, Wiper and Young (2003) managed, with my co-authors, to do some proper inference about the parameters of the stochastic processes.

After we both left UCL Dennis kindly wrote references for me for some time. At conferences in Valencia and elsewhere we usually managed to meet at breakfast and catch up with what the other was doing.

I was delighted to be asked to contribute to this volume and I will always hold Dennis in the highest regard. Congratulations, Dennis, on achieving this milestone.

F W Lanchester (1916). *Aircraft in Warfare: The Dawn of the Fourth Arm*. Constable.
D V Lindley (1952). The theory of queues with a single server. *Proc. Camb. Phil. Soc.*, 48, 277{289.
L I Pettit, M P Wiper and K D S Young (2003). Bayesian inference for some Lanchester combat laws. *European Journal of Operational Research*, 148, 152{165.
M P Wiper, L I Pettit and K D S Young (2000). Bayesian inference for Lanchester type combat models. *Naval Research Logistics*, 47, 541{558.

Nick Polson

It is a pleasure to be able to write about my experiences with Dennis. I wish him well on his 90th birthday. I can't remember the first time I met Dennis – he no doubt does! – but it must have been back in 1984 when I started as a PhD student in Nottingham. I remember Adrian telling me to read Dennis' monograph on Bayesian Statistics from 1972. As a student one could not help but to be impressed by his clarity of thought and his exposition of the Bayesian viewpoint. I must have met Dennis at one of the Royal society meetings. It was a lively time in Bayesian statistics. Dennis was an important presence at many meetings asking his sharp and penetrating questions. The Bayesian viewpoint appeared so clear and correct when explained by Dennis.

Over the years I remember having many engaging discussions with Dennis. One of my favourite parts of these was Dennis' question of why subjective Bayesians don't directly assess predictive probabilities, circumventing the need for assessing priors, likelihoods and posteriors. Dennis always had the most thought-provoking ideas.

One of my fondest memories, however, is a long lunch I had with Dennis at the 1998 Valencia meeting. He was keenly interested in gambling and how the bookmakers' odds were set. Being the first week in June, the meeting coincided with the running of the Epsom Derby and Dennis was interested in the odds for that. If I remember correctly the filly Cape Verdi was 11/4 favourite and I pointed out that this was not good value. It seemed to me that the punters were placing far too much weight on her impressive 1000 Guineas win at a mile and not factoring in the fact that the Derby is a much sterner test at 1½ miles. Dennis wanted to know how I could have such a strong opinion. On another note, I remember Dennis thinking that making a market in the odds was the way to riches and was keen to explore that problem. It was a rare occurrence where I suggested a little caution. I warned Dennis that the punters usually had rather good information – in fact, sometimes better than the odds makers themselves. I remember Dennis being amused by the old Ramsey observation that assessing one's subjective probability by a betting rate is not as simple as it looks since ... the proposal of a bet may inevitably alter his state of opinion (Ramsey, 1927, p.34-35). It brought a wry smile to his face! As always, Dennis followed up with a letter which I have reproduced below.

8 June 1998

Dear Nick,

One of the highlights of the Valencia meeting for me was lunch with you discussing gambling. I promised to let you have a copy of what little I have done: here it is. You will see that two colleagues are involved. Cain is reader in gambling at Salford. Law, now retired, is an economist at Aberystwyth. I got involved because of a newspaper publishing odds for the football matches in the Premier League, and discovering that, converting the odds to prices, they always added to 1.10. So I wrote to Cain, who was at Aber', for advice on the literature. There seems to be very little and what there is is on the pari-mutuel system. He produced a draft of a paper that overlaps with my ideas, so I made it into a joint paper. The result that interests me is that an optimum strategy does not involve the prices adding up to a constant surplus above 1, but to a variable surplus depending on the variation in the population of bettors. The bookmaker's profit comes from the disagreement amongst punters.

I hope that you are sufficiently interested in this immature material to read it. Any comments that you have would be of great value to us. Are you familiar with a book: *The Art of Legging*, by Charles Sidney. Maxline International, London 1976 ?

Best wishes,

Dennis

Dennis Lindley

Nick Polson
Chicago

Once again it proves that Dennis is a great thinker with a wonderful eye for good problems. His extremely pragmatic approach to questions and the speed with which he understands the issues is an inspiration. He's more applied than people think. In finishing, I just retold this story to my wife, Anne, who attended the Valencia 1994 meeting and she quipped, "Oh yes, I remember Dennis, he was very charming!"

All the best from both of us on your 90th birthday!

Jim Press

I first met Dennis Lindley when I visited his department at University College London in 1970-1971. It was a well-run department with many faculty members who got along well and were interested in joint research and in studying fundamental problems in statistics. These included Philip Dawid, Peter Freeman, and Mervyn Stone.

I had received my Ph.D. in Statistics at Stanford University in 1964 and became a faculty member at the University of Chicago in the Business School. The University of Chicago at that time was a hotbed of Bayesian Statistics. Dennis Lindley had been there for a recent year, and Jimmie Savage had spent a decade there in the statistics department, and as its chair. Harry Roberts and Arnold Zellner were close by me in the Business School. Arnold asked me to work with him on his contract with the National Science Foundation, and I happily agreed. When I asked Arnold where I should go on my sabbatical from the University of Chicago in 1970-1971 he suggested Dennis Lindley's Department of Statistics in London. So I wrote to Dennis and he wrote back that he would be delighted to have me come to London to study and do joint research with him. So I became an Honorary Research Fellow in the Department of Statistics at University College London for that year. But Dennis turned out to have very little physical room for me. So Mervyn Stone, also in Dennis' department, agreed to share his large office with me. That's how I met Mervyn. During that year, Dennis warmly asked me to write an article for the Royal Statistical Society, which I did. I delivered that article at the next meeting of the Society.

Some years later, I met Jim Dickey, in London, I think. Jim, Dennis and I began to collaborate, so that in 1985, we published: "Bayesian estimation of the dispersion matrix of a multivariate normal distribution" in *Communications in Statistics, Theory and Methods* (14: 1019-1034).

Dennis and I remained in contact through the years, and he visited me at the University of California, Riverside, where I was then Department Chairman. We began to get interested in developing a Bayesian form of meta-analysis. Dennis and I wrote: "Coherent Bayesian Meta-Analysis", Technical Report No. 233, Department of Statistics, *University of California, Riverside*, May 1996.

After I wrote a book with Judy Tanur entitled "The Subjectivity of Scientists and the Bayesian Approach" (New York, John Wiley & Sons, Inc., 2001), I received a letter from Dennis. I quote it here, not only because I'm proud that Dennis liked the book, but to give some flavor of his lively response to an intellectual stimulus:

> "Hello, stranger! Wiley have sent me, perhaps. at your kind instigation. a copy of your book with Judith Tanur on The Subjectivity of Scientists and the Bayesian Approach. The speed with which I can absorb new material diminishes and my original intention was to read it superficially. In fact, I got 'hooked', have read it carefully and concluded that it is a splendid and important book. Congratulations, to you both.
>
> "The book attracts because of its entertaining style but it goes far beyond entertainment in the skilled way it exposes how eminent scientists have used, in our parlance, their prior views. Indeed, one can contemplate the thesis that what differentiates a famous scientist from the run of the mill, is the former's getting the prior 'right'. This does not always. apply; for example, the case of Einstein, who pursued logical reasoning and thought experiments to reach his theories. But Pasteur seems to have guessed wisely.
>
> "One issue on which I might part company with your thesis (and de Finetti certainly would have) is that concerning the objectivity of the likelihood (p.205). If one has a well-defined theory and small errors of observation, as with the observations of Mercury in connection with relativity, there is almost complete objectivity. But in the case of Darwin's theory, there would be much subjectivity in the connection between it and data. In our language, the probability of X given theta would be subjective. As statisticians we see this subjectivity in the likelihood when different models are considered for the same data. Objectivity often only means that nearly all subjects agree. We all agree that the tosses of a thumb tack are exchangeable, with the result that the binomial distribution appears objective, but the chance of falling point uppermost is subjective because we do not all agree initially on its value.
>
> "I was sorry to see Mendel described as extremely subjective. I recently read *A Monk and Two Peas*, by R.M.Henig and acquired a more sympathetic view. You may have seen my quote from it (ISBA Bulletin, December 2000, p.5). You mention Fisher's chi squared, but have you done a proper Bayesian analysis of Mendel's data? D. Sobel's book, "Galileo's Daughter" clearly presents Galileo's difficulties in dealing with the Church, that influenced his writing, if not his personal views. I do not think that Freud used the scientific method.

> "I trust all is well with you these days. I have angina and prefer to live quietly here, my statistical refreshment mostly coming from books and correspondence. I have recently been most impressed with Pearl's book on Causality and have written a little note about it, resulting in much interesting correspondence with him. Brad Efron drew my attention to some clinical trials with selenium, wherein the frequentists were bothered by the significant effect being one that the trial had not been designed to detect, the intended one not showing up. I did a fully Bayesian analysis that I can send you if you are interested and have time, for your comments would be of value.
>
> "It is a splendid book. I hope someone asks me to review it."

I couldn't arrange for Dennis to review the book, but I did invite him to my retirement party. Because it was difficult for him to travel at that time, instead of coming, he sent me a note saying:

> "Our collaboration over the years has been of much satisfaction to me and your work has been of real value to the profession, especially in putting forward the Bayesian approach. I cannot believe that you will really retire in July, but instead will continue to do significant work. I hope that you will find job retirement as beneficial as I did."

Dennis will have to revise his prior – I chose to spend time in my retirement writing fiction and non-fiction rather than continue to be as professionally active as Dennis has in his retirement. Surely our field is richer for his decision.

Maria Ramalhoto

A Tribute to Dennis

Dennis was the Head of the UCL Statistics Department during my time as a PhD student (1974-1977). I vividly recall Dennis´s remarkable lectures. The mathematics teaching gained a different touch from what I was used to. The emphasis on the *coherence concept* and the display of its relevance in the decision making process in sciences, law and governance in general has inspired my professional life decisions and research choices.

The department Journal Club sessions, where Dennis would find the time to be present and available even for final discussions, contributed greatly to my research and world citizenship education. It facilitated an informal discussion of the ongoing research work, over a cup of tea and biscuits (a British ritual due to Catarina de Bragança, wife of Charles the Second), among the departmental students (from UK, India, Japan, Iran, etc.) and academic staff including visiting professors (mainly from US and Europe's best universities). It also was a preparation for the formal Statistics University of London Joint Seminar. Both bridged out knowledge, culture and people, giving me the opportunity of multi-disciplinary debates inside and outside *UCL* (engineering, physics, social and medical sciences, philosophy, psychology, law), and networks for future research with academics from US, Europe and Asia.

In 1982 I got a one year scholarship to do research in US universities. Due to Dennis´s recommendation letter I was a visiting scholar at the Department of Industrial Engineering and Operations Research, University of California, Berkeley integrated in the research groups of Sheldon Ross, Ronald Wolff and Richard Barlow; collaborated in seminars and held discussions with several researchers, among them Gordon Newell. My talk at Stanford University on ill-posed problems in the context of inference in the infinite server queue, invited by Herbert Solomon, was another opportunity to discuss Stochastics (Probability, Stochastic Processes, Statistics, Data Analysis, Operations Research and the like) with well-known researchers including Bradley Efron.

1. The problems worked on

I obtained a doctorate in mathematical statistics in 1977 from the University College London under the supervision of Dieter Girmes (PhD thesis: Markov Renewal Approach to the Theory of Stochastic Counters and Queues; part of 1.1 in list below). My work at Berkeley in 1982 was mainly on inference on positive random translations of marked point processes (part of 3.1 in list below) and is published in Ramalhoto (1987).

My work covers research on methodology and on frameworks to help bridge and integrate different concepts and processes in the "Stochastics" arena.

Methodology: 1. *Unifying structures* (1.1 Markov renewal process, a unifying structure for some types of Markovian queues; 1.2 3rd Erlang formula unifying the 1st and 2nd Erlang formulas; 1.3 multi-server loss-delay queue unifying decomposition formulas through the 3rd Erlang formula; 1.4 Conservation laws). 2. C*ongestion reduction and control* (2.1 retrial queues; 2.2 managerial decision rules in queues). 3. *Parametric and non-parametric inference* (3.1 ill-posed problems and estimation in positive random translations of marked point processes; 3.2 robust graphical tests for bivariate normality). 4. *Control charts, reliability and maintenance integrated concepts*. For example Evandt, Coleman, Ramalhoto and Lottum (2004) deals with 3.2, Goeb, Ramalhoto and Pievatolo (2006) and Ramalhoto and Goeb (2006) deal with 4. Discussions with Gordon Newell led to adding 4. in my list of interests (before *UCL*, I worked at the Portuguese Post Office acquisitions and storage departments using control charts and acceptance sampling to assess the quality of materials sent by the suppliers).

Frameworks: 1. *Total Quality Queue Management, TQQM* (introduced in 1995, benefited from a three months visit to Massachusetts Institute of Technology Operations Research Center; further details in Ramalhoto (2000)). 2. *Stochastics for the Quality Movement, SQM* (introduced in 1999, benefited from the European Union Pro-ENBIS project; further details in Ramalhoto (2008)). The two frameworks are first attempts to create a body of knowledge (as complete as possible), under the umbrella of the Stochastics Science & Engineering, *SSE,* initiative, to reach the best mission availability in a repairable system of any nature (implementation perhaps to be inspired by institute of Medicine; physicians are organized worldwide through Clinical Practice Guideline under the supervision of the Institute of Medicine).

Ramalhoto (2011), dedicated to the memory of Soren Bisgaard, provides an overview of the most relevant research contributions, methodology and frameworks, with a brief discussion of further research. All the topics listed above are useful for *SSE* as discussed in that paper.

Over the years the research was mainly financed by European Union projects. The positive effects of *UCL* and Berkeley have been felt across almost all my research career. Indeed, direct and indirectly highly potentiated by Dennis.

2. The inspirations Dennis gave me

Dennis made me aware of:

(1) The importance of readings into the physical basics of stochastic systems to bring its deepest theoretical and practical consequences.
(2) The importance of bringing out other disciplines and promoting research and its eventual novel way of thinking.

Due to discussions with Dennis on how the famous *Lindley´s equation* has been discovered, I became interested in conservation laws, namely in the Little´s formula and its generalizations (which is still of interest in spite of a paper entitled *A Last Word on L = lambdaW* published in 1974). Indeed, my results in 1.2 and 1.3 were also based on careful readings into the physical basics of queuing systems.

The *3rd Erlang formula* is the stationary probability that an arbitrary customer on arrival finds *r* or more customers in an *M/M/r/r+d* queue (i.e., Poisson arrivals, exponential service time distribution, *r* servers and *d* waiting positions; Markovian loss-delay multi-server queue). That is the probability of *not immediate service* (the *proportion of customers* that are *delayed or lost* (blocked)). The *3rd Erlang formula* can be easily rewritten in terms of the *1st Erlang formula:*

$$C_d(r,r\rho)=\left| B^{-1}(r,r\rho)\left(\sum_{i=0}^{d}\rho^i\right)^{-1}+1-\left(\sum_{i=0}^{d}\rho^i\right)^{-1}\right| \quad ; \quad r\in N,\ \ d\in N_0,\ \ \rho\in R^+,$$

When $d = 0$ it coincides with the *1st Erlang formula B (r, r.rho)*; taking its limit as *d* goes to infinity it coincides with the *2nd Erlang formula. The M/M/r/r+d* queue's relevant characteristics can be rewritten as simple functions of the *3rd Erlang formula*. The number of customers waiting in the queue, the number of occupied servers, the number of customers in the system (waiting or being served), the waiting time in the queue and total sojourn time in the system are each distributed as the sum of two random variables weighted by the *3rd Erlang formula*. In steady state the number of customers in the system (the proof is based on rewriting the respective distribution in terms
of the 3rd Erlang formula; similarly to the other characteristics) is:

$$N_{r,d} \sim (1 - C_d(r, r\rho))(X / X < r) + C_d(r, r\rho)(R + N_{1,d-1}),$$

$N_{1,d-1}$ number of customers in the *M/M/1/1+(d–1)* queue in steady state with the same ρ (truncated geometric distribution with parameters *d* and *1–ρ*), $P(R = r) = 1$ and $(X / X < r)$ Poisson random variable of parameter $r\rho$.

Similar exact results for the *M/M/r/r+d* queue with constant retrial rate in steady state (by rewriting the respective distribution function in terms of the *probability of entering the orbit* ; which in the retrial case corresponds to the *3rd Erlang formula*), are in Ramalhoto and Gomez-Corral (1998); paper in honor of Marcel Neuts (whom I met through Lawrence Baxter).

3. Activities selection inspired by (2)

Back to Instituto Superior Técnico, *IST*, as Coordinator of the Mathematics Department Section Statistics and Applications, 1977-1990, inspired by the good practice learned, I established the following plan for the Section: (a) international exposition and high competence in research; (b) active participation in international applied mathematics research relevant to industry and commerce, including the creation of new concepts and frameworks; (c) leading activities in international cooperation between industry and academia, including quicker transfer of academic research into real life problems and advanced continuing (lifelong) education; (d) financial support provided mainly by available European Union programs; (e) PhD thesis taking no more than 4 years, having at least one leading researcher from abroad (in the respective research field) in its examination, and a couple of papers already accepted for publication in international periodicals, to ensure world recognition of our "Doutoramentos".

To promote national statistics awareness I co-founded, in 1979, jointly with Tiago de Oliveira and Bento Murteira, the *Statistics and Operations Research Information Bulletin* and co-edited it 1979-1983 (national and international conferences and courses, statistics education initiatives, ongoing research notes and problem corner, computer package available; Gani, Neuts, Barnett, Freeman, Anderson, Azorin Poch are among the well-known people who kindly collaborated with it) and in 1980 co-founded the *Portuguese Society of Statistics and Operations Research, SPEIO*.

To facilitate the implementation of the Statistics Section plan I became *IST* Research Council "Vogal" (one President and two "Vogal") 1980-1981 and *IST* Administrative Council "Vogal" (one President and three "Vogal") 1987-1988 as well as *IST* Erasmus Institutional

Coordinator 1987-1990. In fact these extra activities facilitated putting together with *UCL* Statistical Science Department pioneer successful ERASMUS projects on Probability and Statistics run 1987-1990. Six *IST* students, now university professors (the current President of the Mathematics Department is one of them), went to the *UCL* for a semester; and Dawid, Galbraith, Burridge and Girmes came to *IST* to lecture short courses and give talks. That was important for the Statistics development at *IST* Mathematics Department and must be acknowledged here. One student from *UCL* came to work with me for a semester and I went to *UCL* to lecture a course and give talks (was appointed *UCL* honorary external examiner for those ERASMUS students; all activities completely financed by the ERASMUS projects).

To create a structure of ERASMUS/COMMET type to improve teaching and research nationwide and speed up transfer of academic knowledge to industry and business led my to become the Portuguese Open University Vice-Rector 1990-1993. That dream did not quite come true. However, supported mainly by the COMMET program, participant fees and industry, I was the president of the Second European Forum for Continuing Engineering Education-International Cooperation Between Industry and Academia, Lisbon 1992 April 28-30 (first forum in Stuttgart 1988 November 30-December 2), inaugural lecture by Sir Robert Telford-Marconi UK, and edited the forum proceedings in 1993, 615 pages. The forum led my to another dream called *SSE* initiative.

To promote the *SSE* initiative I was guest-editor of the *International Journal of Continuing Engineering Education* special issue on *Applied Probability Modeling*, Vol. 4, 1994, in memory of W. Edwards Deming, seventeen authors among them David Cox, Adrian Smith, Sheldon Ross, Amadeo Odoni, Joe Gani); co-founded in Lisbon in 1994**,** with two colleagues from the *IST* Mechanical Engineering Department, the *Research Unit of Naval Engineering and Technology*, *UETN* (in 2008 became research center, *CENTEC*); co-founded in 2000, in Amsterdam, the *European Network for Business and Industrial Statistics*, *ENBIS* (Henry Wynn founder President and me a founder Vice-President); was co-founding associate editor of the international journal *Quality Technology and Quantitative Management* 2004-2012, and is its editor-in-chief for Europe 2013-2015.

I share the view that the University Institution, *UI* (all the universities and institutions of higher education worldwide) has a big role to play in the globalized world. I was co-guest editor, in 2006, jointly with Adhan Akay from Carnegie Mellon University, of the special issue *Globalization and Its Impact on Engineering Education and Research* of the *European Journal of Engineering Education* (of which I was associate editor 2004-2012). Ramalhoto (2006) introduces the hybrid university concept and put forward for discussion a possible way to make *UI* a bastion of globalization.

Understanding uncertainty, *integrated knowledge* (instead of fragmentation) and *solid ethic values*, are they three missing keys to enhance intelligent decision making in industry, businesses and governance? To my mind, yes, and Lindley (2006) is one of the relevant books to help to put it into action.

References

Evandt O., Coleman S. Y., Ramalhoto M. F. and van Lottum C. (2004). A little known robust estimator of the correlation coefficient and its use in a robust graphical test for bivariate normality with applications in the aluminium industry. *Quality and Reliability Engineering International* 20(5): 433-546.

Göb R., Ramalhoto M. F. and Pievatolo A. (2006). Variable sampling intervals in Shewhart charts based on stochastic failure time modeling. *Quality Technology & Quantitative Management* 3(3): 361-381.

Lindley D. V. (2006). Understanding Uncertainty, Wiley.

Ramalhoto M. F. (2011). Queuing systems and quality management as part of stochastics science & engineering, *Marine Technology and Engineering*. Guedes Soares et al (eds), 2: 1259-1281, *Taylor & Frances* (In commemoration of the 100 years of IST, 1911/2011).

Ramalhoto M. F. (2008). Stochastics for the quality movement: an integrated approach to reliability and safety. Chapter 12: Safety and Reliability Engineering, Part II", in *Statistical Practice in Business and Industry*. Coleman, S.Y., Greenfield, T., Stewardson, D.J., and Montgomery, D. (eds.): 317-335, *Wiley*.

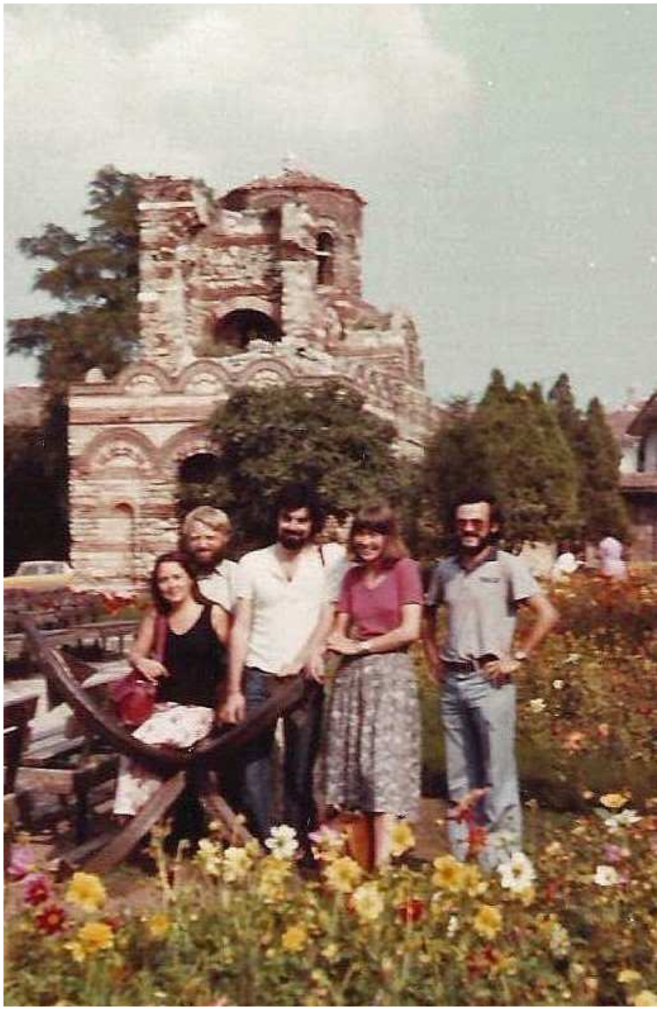
Ramalhoto M. F. (2006). Transforming academic globalization into globalization for all. Special issue on *Globalization and Its Impact on Engineering Education and Research. European Journal of Engineering Education* 31(3): 349-358.

Ramalhoto M. F. and Göb, R. (2006). An innovative strategy to put integrated maintenance, reliability and quality improvement concepts into action. *International Journal of Materials & Structural Reliability* 4(2): 207-223.

Ramalhoto M. F. (2000). Stochastic modelling for quality improvement in processes. *Statistical Process Monitoring and Optimization*. Park Sung H. and Vining G. Geoffrey (eds), (chapter 26) 1:435-456, *Marcel Dekker*.

Ramalhoto M. F. and Gomez-Corral A. (1998). Some decomposition formulae for M/M/r/r+d queues with constant retrial rate. *Communications in Statistics-Stochastic Models* 14 (1&2): 123-145.

Ramalhoto M. F. (1987). Some statistical problems in random translations of stochastic point processes. *Annals of Operations Research* 8: 229-242.



At the European Meeting of Statisticians in Bulgaria, 1979: (from the left) me, Roger Sugden, David Spiegelhalter with his wife (at the time) Eva and José Bernardo.

It is worth noting that my PhD award in 1977 was the first to be awarded to a female at the Department. To my mind that illustrates a gender issue that perhaps is not dramatically different from nowadays.

Anyway, I have to confess those years at the UCL remain my most enjoyable times ever.

Christian Robert

On the Lindley-Jeffreys Paradox

1. Introduction

Paraphrasing Proust's (1913) famous first sentence, for a long time, I went to bed... thinking the Lindley–Jeffreys's paradox was about the poor behaviour of vague priors when testing point null hypotheses. My own attempt at solving the paradox (Robert, 1993) was definitely written under this understanding. It is only very recently that I became aware that most people understand the paradox as the irreconcilable divergence between the Bayesian ($\mathfrak{b}$) and the frequentist ($\mathfrak{f}$) resolutions of the point null hypothesis testing problem.

I must acknowledge being rather surprised at this focus as there is no reason both approaches should agree: (a) one ($\mathfrak{b}$) is operating on the parameter space Θ, while the other ($\mathfrak{f}$) is produced on the sample space $\mathcal{X}$, or, in other words, one ($\mathfrak{f}$) is dealing with credibility while the other dabbles in confidence; (b) one relies solely on the null hypothesis H_0 and the corresponding distribution, while the other ($\mathfrak{b}$) opposes H_0 to a marginal version of H_1 (integrated against a specific prior distribution); (c) following what may be the most famous quote from Harold (Jeffreys, 1939, Section 7.2), one ($\mathfrak{f}$) could rejects "a hypothesis that may be true (...) because it has not predicted observable results that have not occurred" ($\{X > x_{\text{obs}}\}$, say), while the other ($\mathfrak{b}$) conditions upon the observed value x_{obs}. A consequent literature (see, e.g. Berger and Sellke, 1987) has since then shown how divergent those two approaches could be (to the point of being asymptotically incompatible).

While the gap between frequentist and Bayesian degrees of evidence was certainly the reason for Lindley (1957) mentioning a statistical paradox, I thus remain convinced that the richest consequence of Jeffreys's (1939) and Lindley's (1957) exhibitions of this paradox is to highlight the difficulty in using improper or very vague priors in testing settings: as stressed by Lindley (1957), "the only assumption that will be questioned is the assignment of a prior distribution of any type" (p.188). This was also the argument made by both Shafer (1982) and DeGroot (1982) in their discussion of the paradox.

In connection with this special volume, I want to express here my deepest appreciation of the kind help provided by Dennis Lindley's on the meaning of Harold Jeffreys's *Theory of Probability*, when re-visiting this fundamental statistics book (Robert et al., 2009). He explained very clearly the mathematical limitations of Jeffreys's use of improper priors, which was doubled with a strong intuition that avoided most paradoxes. I may add in closer connection with this note that Dennis systematically refereed to Jeffreys for stating the paradox, both in his paper and his personal communications. Note that Jeffreys does not address the general problem of using improper priors in testing, using ad-hoc solutions when available and developing a second (and undervalued) type of Jeffreys's priors otherwise (see Robert et al., 2009, Section 6.4, for a discussion).

2. The paradox, paradoxes, or non-paradox

If one considers a normal mean testing problem,

$$\bar{x}_n \sim \mathcal{N}(\theta, \sigma^2/n) \,, \quad H_0 : \theta = \theta_0 \,,$$

using Jeffreys's (1939) choice of prior, $\theta \sim \mathcal{N}(\theta_0, \sigma^2)$ leads to the Bayes factor

$$\mathfrak{B}(t_n) = (1+n)^{1/2} \exp\left(-nt_n^2/2[1+n]\right) \,,$$

where $t_n = \sqrt{n}|\bar{x}_n - \theta_0|/\sigma$ is the classical t-test statistic.

The first level of the paradox is that, *when t_n is fixed and n to infinity, the Bayes factor goes to infinity while the p-value remains constant.* In Lindley's words, "we [can be] 95% confident that $\theta \neq \theta_0$ but have 95% belief that $\theta = \theta_0$" (p.187). As discussed previously in the literature, this is not a mathematical paradox as the quantities measure different objects and this is not a statistical paradox in that a constant[1] t_n is not of interest: when H_0 is true, t_n has a limiting $\mathcal{N}(0, 1)$ distribution, while, when H_0 does not hold, t_n converges almost surely to ∞, in which case the Bayes factor converges to 0. This behaviour is thus compatible with the overall consistency of the Bayes factor.[2]

At a second level, if we shift the interpretation of n from a sample size to a prior scale factor, namely that the prior variance is n times larger than the observation variance (or that the prior is n times less precise), the result derived from the above expression is that *when the scale goes to infinity, the Bayes factor goes to infinity no matter what the value of the observation.* (Note that both interpretations are mathematically equivalent.) Now, n becomes what Lindley (1957) calls "a measure of lack of conviction about the null hypothesis" (p.189), a sentence that I re-interpret as the prior (under H_1) getting more and more diffuse as n grows. I must however stress that nowhere in the paper is the difficulty with improper (or very large variance) priors discussed. I also think that the

[1]As pointed out by Lindley (1957): "5% in to-day's small sample does not mean the same as 5% in to-morrow's large one" (p.189).

[2]One could instead argue that the true paradox is that this consistency is overlooked in most commentaries on the Lindley–Jeffreys's paradox.

phenomenon is not a paradox *per se*: if the diffuseness of the (alternative) prior (under H_1) increases, the only relevant piece of information becomes that θ could be equal to θ_0, to the point that it overwhelms any evidence to the contrary contained in the data. As put by Lindley (1957), "the value θ_0 is fundamentally different from any value of $\theta \neq \theta_0$, however near θ_0 it might be" (p.189). There is therefore much coherence in the selection of the null hypothesis H_0 in this case: being indecisive about the alternative hypothesis means we simply should not select it.

3. On some resolutions

While the divergence between the frequentist and Bayesian procedures is nothing to complain about, the debate about constructing limiting Bayes factors or posterior probabilities that include improper prior modelling remains open and relevant. DeGroot's (1982) warning that "diffuse prior distributions (...) must be used with care" has been impressed upon generations of students and it is indeed a fair warning. There remains nonetheless a need to produce assessments of null hypotheses from a Bayesian perspective and under limited prior information, once again without any incentive to mimic, reproduce or even come close to frequentist solutions.

In Robert (1993), I suggested modifying the prior weights of both hypotheses $(\varrho_0, 1-\varrho_0)$ to compensate for the increased mass of the alternative hypothesis prior. While the solution therein produced numerical results that brought a proximity with the p-value, its construction is fundamentally flawed from a measure-theoretic point of view since it involves the value of the prior density π_1 at the point null value θ_0,

$$\varrho_0 = (1-\varrho_0)\pi_1(\theta_0)\,,$$

a difficulty also shared by the (related) Savage–Dickey paradox (Robert and Marin, 2009). I however remain of the opinion that this degree of freedom in the Bayesian formalism should not be neglected to overcome the difficulty in using improper priors. (Some will object at this choice on Bayesian grounds as it implies the prior does depend on the sample size n.)

Another direction worth pursuing is Berger et al.'s (1998) partial validation of the use of *identical* improper priors on the nuisance parameters, a notion already entertained by Jeffreys (1939, see the discussion in Robert et al., 2009, Section 6.3). While using the "same" constant in those improper priors for both models has no mathematical nor statistical validation, it eliminates quite conveniently the major thorn in the side of Bayesian testing of hypotheses. As demonstrated in Marin and Robert (2007) and Celeux et al. (2012), it allows for the use of a partly improper g-prior in linear and generalised linear models (Zellner, 1986). (Again, choosing $g = n$ should attract criticism from some Bayesian corners, even though it boils down to picking an imaginary sample (Smith and Spiegelhalter, 1982) size of 1.)

Yet another resolution is seemingly found in DeGroot's (1982) recommendation to keep "in mind that the assignment of a prior distribution to the parameter θ induces a predictive distribution for the observation" (p.337), as comparing predictives allows for an assessment of Bayesian models (meaning that either the sampling or the prior distribution may be inadequate). However, I think DeGroot means the prior predictive,

$$m(y) = \int_\Theta \pi(\theta) f(y|\theta)\,\mathrm{d}\theta\,.$$

in which case this approach is equivalent to the Bayes factor, hence does not solve the improperness issue and suffers from the same calibration difficulty. If, instead, one considers the posterior predictive, this is the solution advocated in, among others, Gelman et al. (2003), under the name of *posterior predictive checking*, but it implies using the data twice, and has been reinterpreted in (Aitkin, 1991, 2010), drawing strong criticism from many, including Dennis Lindley's now famous "One hardly advances the respect with which statisticians are held in society by making such declarations" (1991, p.131).

4. Reflections

The appeal of great so-called paradoxes is to exhibit foundational issues in a field, either to reinforce the arguments in favour of a given theory or, on the opposite, to cast serious doubts on its validity. The fact that the Lindley–Jeffreys's paradox is still discussed in papers (Spanos, 2013) and blogs, by statisticians and non-statisticians alike, is a testimony to its impact on the debate about the very nature of (statistical) testing. The irrevocable opposition between frequentist and Bayesian approaches to testing, but also the persistent relevance of the prior modelling in this case, are fundamental questions that have not yet met with definitive answers. And they presumably never will for, as put by Lad (2003), "the weight of Lindley's paradoxical result (...) burdens proponents of the Bayesian practice". However, this is a burden with highly positive features in that it paradoxically (!) drives the field to higher grounds.[3]

References

Aitkin, M. (1991). Posterior Bayes factors (with discussion). *J. Royal Statist. Society Series B*, **53** 111–142.

Aitkin, M. (2010). *Statistical Inference: A Bayesian/Likelihood approach.* CRC Press, Chapman & Hall, New York.

Berger, J., Pericchi, L. and Varshavsky, J. (1998). Bayes factors and marginal distributions in invariant situations. *Sankhya A*, **60** 307–321.

Berger, J. and Sellke, T. (1987). Testing a point-null hypothesis: the irreconcilability of significance levels and evidence (with discussion). *J. American Statist. Assoc.*, **82** 112–122.

[3]To conclude with a quote from Camus, "il faut imaginer Sisyphe heureux."

Celeux, G., Anbari, M. E., Marin, J. and Robert, C. (2012). Regularization in regression: Comparing Bayesian and frequentist methods in a poorly informative situation. *Bayesian Analysis*, **7** 477–502.

DeGroot, M. (1982). Discussion of Shafer's 'Lindley's paradox'. *J. American Statist. Assoc.*, **378** 337–339.

Gelman, A., Carlin, J., Stern, H. and Rubin, D. (2003). *Bayesian Data Analysis*. 2nd ed. Chapman and Hall, New York, New York.

Jeffreys, H. (1939). *Theory of Probability*. 1st ed. The Clarendon Press, Oxford.

Lad, F. (2003). Appendix: the Jeffreys–Lindley paradox and its relevance to statistical testing. Tech. rep., Conference on Science and Democracy, Palazzo Serra di Cassano, Napoli.

Lindley, D. (1957). A statistical paradox. *Biometrika*, **44** 187–192.

Lindley, D. (1991). Discussion of the paper by Aitkin. *J. Royal Statist. Society Series B*, **53** 130–131.

Marin, J. and Robert, C. (2007). *Bayesian Core*. Springer-Verlag, New York.

Proust, M. (1913). *Du côté de chez Swann*. Grasset, Paris.

Robert, C. (1993). A note on Jeffreys-Lindley paradox. *Statistica Sinica*, **3** 601–608.

Robert, C., Chopin, N. and Rousseau, J. (2009). Theory of Probability revisited (with discussion). *Statist. Science*, **24(2)** 141–172 and 191–194.

Robert, C. and Marin, J.-M. (2009). On resolving the Savage–Dickey paradox. *Elect. J. Statistics*.

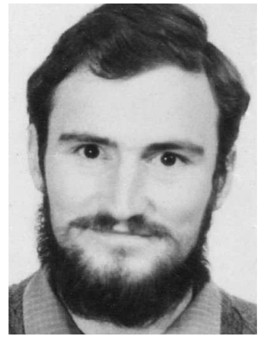
Shafer, G. (1982). On Lindley's paradox (with discussion). *J. American Statist. Assoc.*, **378** 325–351.

Smith, A. and Spiegelhalter, D. (1982). Bayes factors for linear and log-linear models with vague prior information. *J. Royal Statist. Society Series B*, **44** 377–387.

Spanos, A. (2013). Who should be afraid of the Jeffreys–Lindley paradox? *Philosophy of Science*, **80** 73–93.

Zellner, A. (1986). On assessing prior distributions and Bayesian regression analysis with g-prior distribution regression using Bayesian variable selection. In *Bayesian inference and decision techniques: Essays in Honor of Bruno de Finetti*. North-Holland / Elsevier, 233–243.

Geoff Robinson



Gentleman on a mission

The best statistics conference that I have ever attended was the Conference on Directions for Mathematical Statistics, University of Alberta, Edmonton, Canada, 12-16 August 1974. There were 10 invited speakers. Each had seventy-five minutes to speak and a seventy-five minute time slot allocated for questions.

Dennis Lindley was one of the invited speakers. I had been a PhD student in his Department at University College London for the previous two years and had read his books "Making Decisions" and "Bayesian Statistics: A Review", so the technical content of his talk was not new to me. However, it was interesting to see the argument in favour of Bayesian statistics presented in an evangelical manner.

At breakfast the next morning, a racially-Indian man approached me in a rather cautious manner. It was C.R. Rao, whom I had not previously met but for whom I had enormous respect. He wanted to get some more information about Dennis's arguments, but he felt concerned that if he spoke to Dennis directly then he might find Dennis's manner too pushy. He presumed from watching people the previous day that I would be familiar with the arguments that Dennis had presented and hoped that I could explain some parts of them in a non-confrontational manner.

My memories of Dennis from around that time can be summarized by saying that he seemed to be a gentleman with a mission. Primarily, he was a gentleman. And, by nature in my opinion, a quietly-spoken and cautious gentlemen. Secondarily, and a long way behind, he displayed missionary zeal for selling the argument that statistics should be based on subjective Bayesian principles. This zeal was based on years of cogitation. It did not fit very comfortably with his personality, but he had convinced himself that the statistical world needed to be changed and he was doing whatever he could in order to achieve this end.

Dennis wasn't my PhD supervisor. That was Mervyn Stone. However I felt a special relationship with Dennis because all of the other PhD students and most of the staff in the

Department were working within the Bayesian paradigm and were not particularly concerned to contribute to the arguments between paradigms. I was more like Dennis in that I wanted to contribute to the arguments between statistical paradigms. I didn't (and still don't) accept the Bayesian paradigm, but that difference between us was never very important.

My first published paper was "Some counterexamples to the theory of confidence intervals" Biometrika 62 (1975), pages 155-161. The first time that I presented that set of ideas publicly was after a seminar at Imperial College by Graham Wilkinson in about April 1973. The topic was something about fiducial probability. I presented a counterexample as a way of arguing that the fiducial approach was untenable, and explained that the counterexample was equally relevant to the theory of confidence intervals.

Graham wasn't convinced, but somehow he was invited back the next week to continue the discussion. This time the venue would be University College. Dennis had not been present on the first occasion, but would be chairman for the return bout, so he asked me what had happened.

Amongst other things, I admitted that the simple counterexample had taken about a year of my working life to construct... most of the effort being removing the non-essential details from more complicated counterexamples. He told me that he considered that it was a worthwhile way to have spent that time (or something equivalent to that).

After first drafting the paper, I sent it to Nature and was told that it "is not of sufficiently wide significance for inclusion in Nature". Later, I sent it to Biometrika. The last sentence of the rejection letter from the editor, D.R. Cox, was "If you feel that the referee has misunderstood the point of the paper, please let me know."

In my eyes, this was a clear invitation to argue with the referee. When I showed the letter to Dennis he said that he was currently arguing with Biometrika about another paper written by a staff member, and he thought that it was appropriate to argue. However, he didn't think that I would need his help. This was a very empowering conversation.

After a few rounds of correspondence, the paper was expanded from one counterexample to three and published. Dennis drew attention to the paper in a letter to the February 1976 issue of RSS News & Notes, indicating that he regarded it as a piece of solid material in support of his argument for dismissing confidence intervals.

Francisco Samaniego

An Ode to Dennis Lindley

In the dedication of my 2010 monograph *A Comparison of the Bayesian and Frequentist Approaches to Estimation*, I made special reference to Dennis Lindley as one of three statisticians who were primarily responsible for sparking my interest in Bayesian Statistics. I said that his visit to Davis as a Regents Professor in the 1980s "really rocked my boat". It's a pleasure to have the opportunity to elaborate on that statement and to add some comments on our other interactions and on his influence on me and my work. And, of course, it's also a pleasure to join in the celebration of Dennis's 90th birthday. Happy Birthday, my friend!

During his memorable visit to the University of California, Davis in 1988, Dennis gave a series of lectures on the foundations of Bayesian inference. While many of us were quite familiar with the mechanics of Bayesian inference and with the elements of the debate about the use of prior information in a statistical analysis, few of us had carefully considered the axiomatic foundations of the Bayesian principle that the only way for a rational person to deal with "uncertainty" was through the use of the probability calculus. Dennis gave several lectures on this particular aspect of the Bayesian approach to statistics. His argument was beautifully constructed and his exposition was engaging and convincing. While I had read the axiomatic development of Bayesian inference in DeGroot (1970), Dennis's treatment, which was no doubt isomorphic, was somehow more palatable to me, as it had a "constructive" flavor which led from an order relation to a metric to a probability measure to the conclusion that our judgments about uncertain events can be uniquely quantified by, and were in effect equivalent to, the assignment of probabilities to an appropriate sample space. For someone like me, who had majored in mathematics and minored in philosophy as an undergraduate, and was subsequently immersed in statistical theory, I took Dennis's lectures as an open and irresistible invitation to think harder and learn more about the Bayesian approach to statistical inference.

I can't recall when, exactly, I committed myself to the study of "comparative inference", but it was not long after Dennis's visit to Davis. This commitment of course reveals that Dennis didn't wholly convert me to the Bayesian paradigm, as I subsequently set out to try to determine when a Bayesian statistician would have an advantage over the frequentist. Thus, instead of addressing the question “Why should I (and, presumably, everyone else) be a Bayesian?”, I hoped to provide insights into the question “When should I be a Bayesian?” I published my first paper addressing this question in **JASA** in 1994 (with D. Reneau). While this paper was focused on a very particular problem (point estimation of a scalar parameter relative to squared error loss), it provided an explicit closed-form solution to the “threshold problem”, as we called it, the problem of determining the boundary, in the space of possible prior distributions, that separate good priors (for which the corresponding Bayes estimators outperform standard frequentist alternatives) from bad priors (whose performance is poorer than the standard frequentist estimator), where performance was judged by the Bayes risk of an estimator relative to the true (possibly degenerate) distribution of the target parameter. In the context studied, the threshold is a hyperbola in the space of prior parameters. To us, the surprising feature of our solution was that fact that Bayesian estimation could be seen to be remarkably robust. Priors which many would classify as poor or misleading turned out to be “good priors” under our definition.

In my book, I admit that I am not a fully committed Bayesian; our findings in the study mentioned here include the fact that a Bayesian analysis can do quite poorly. My answer to the question “When should I be a Bayesian?” turns out to be “When I’m on the right side of the threshold.” In a nutshell, a Bayesian will perform poorly if he/she is both misguided (with the prior mean far from the true value of the parameter) and stubborn (placing a good deal of weight near the prior mean). As it happens, having one but not both of these defects is generally not a fatal flaw, an interesting insight in itself, given the widespread impression (at least back then) that a good prior had to be “sharp” (see Diaconis and Freedman (1986) for a typical reference to this term), that is, both accurate (with the prior mean near the “true parameter value”) and precise (that is, highly concentrated around the prior guess).

When the **JASA** paper was published, I sent a reprint to Dennis Lindley. I must admit to a little trepidation as I waited for a response. After all, while the paper was positive about the Bayesian approach, it did not actually endorse the approach, but rather attempted to delineate the types of circumstances in which the Bayesian could expect to do well. In my 2010 book, I describe myself as a “Bayesian sympathizer” rather than as a Bayesian. My outlook is a pragmatic one, recognizing that the Bayesian approach will be preferable when the available prior information is at least moderately useful, but it stands to have performance inferior to that of a good frequentist procedure in the complementary situation. I wondered whether Dennis, who was, in my view (and in many other people’s as well), the king of the Bayesian Empire, would take a dim view of my investigation. In mid November, 1994, I received his response. I was much relieved by his opening paragraph. Dennis wrote: “I approached the paper with suspicion, since there have been several unhappy attempts to compare the two methods. But my prior was soon changed and the final conclusion is that a fair comparison has been made. It is an impressive paper with ideas that promise useful extensions.

Congratulations to you both."

Dennis, being Dennis, had much more to say, of course. In the six paragraphs that followed, he discussed a variety of other issues. All of his comments were constructive and useful, and, as expected, insightful. Among other things, Dennis proposed a refinement of our main result that he thought would be of special interest. He felt that our method of determining when a Bayes estimator outperforms a frequentist estimator could be recast, without our referring to a "true prior" – the true (possibly degenerate) distribution of the unknown parameter theta. Dennis's version of the result described it in terms of an impartial umpire who knows the true value theta0 of theta and determines "the winner" by comparing each estimator's expected squared distance from theta0. His version of our result appears as Corollary 5.2 in Samaniego (2010).

In his letter, Dennis encouraged me to expand my investigations, saying "There are lots of interesting possibilities for extending your results, for example, to several parameters where I would expect B to do even better." Inspired, encouraged and energized, I launched a number of studies aimed at exploring versions of "the threshold problem" in other settings. Among the important extensions I wished to explore, the behavior of Bayes estimators of vector valued parameters and their behavior in problems involving asymmetric loss criteria were given the highest priority. In Vestrup and Samaniego (2004), Bayesian and frequentist shrinkage estimators of a multivariate normal mean were compared. Under generalized squared error loss and specific assumptions about the sampling distribution (a multivariate normal distribution with covariance matrix of the form *v*I), conjugate prior distributions (with covariance matrix of the form *w*I) and taking the "true prior distribution" degenerate at a point, a threshold was identified which separates the class of Bayes estimators that outperform the James-Stein estimator from the class of Bayes estimators that don't.

This comparison between Bayesian and frequentist shrinkage estimators of a multivariate normal mean resulted in some notable findings. The conjecture about possibly striking Bayesian domination in higher dimensions was not supported by the study. Instead, in higher dimensions, Bayes estimators of a normal mean outperformed the James-Stein estimator only in rather restricted circumstances. What did prove true was that there was still an analytically identifiable threshold which separates good and bad prior distributions. The difference between the solutions for a scalar parameter and for a high dimensional parameter is that in the former problem, the subclass of prior distributions which give rise to performance superior to that of the best frequentist estimator constituted a fairly large fraction of the space of all priors considered, whereas when one estimates a k-dimensional parameter, the collection of superior priors becomes increasingly sparse as k grows. In high dimensional problems, the Bayesian has a strikingly narrower window for selecting an estimator that outperforms the James-Stein estimator. In general, Bayesian shrinkage will outperform

James-Stein shrinkage in such situations only when the prior variance *w* is neither too small nor too large, making the specification of *w* a rather delicate matter. We found that a Bayesian with a "sharp" prior distribution (with mean close to the true value of the parameter and with a relatively small variance *w*) will typically outperform the James-Stein estimator. But our overall findings underscore the fact that Bayesian estimation of a high dimensional parameter is a difficult enterprise, as the specification of a prior model which leads to inferences that are superior to notable frequentist alternatives is quite challenging. This suggests that Bayesian estimators of vector-valued parameters must be used with considerable care, as the importance of "good prior modeling" becomes substantially magnified as the dimension of the problem of interest grows.

Since writing the 1994 **JASA** paper, I've had the opportunity to explore the "threshold" idea in many different contexts. I feel that I owe Dennis Lindley a huge debt, both for the stimulation he provided during his visit to Davis that led me into the initial investigation as well as for his encouragement and his suggestions that led to much of the research that followed. It led me to examine estimation problems based on the multivariate normal model with an asymmetric (Linex) loss function, a scenario in which the threshold problem proved to be tractable and its solution provided useful insights into when a Bayesian has the advantage in such problems. Other scenarios in which a version of the threshold problem served to clarify issues in comparative inference include the Bayesian consensus problem, the estimation of nonidentifiable parameters (a problem type that Dennis himself investigated in Lindley and El-Sayyad (1968)) and the improvement of Bayes estimators (by a better Bayes estimator) in problems satisfying empirical Bayes modeling assumptions. The basic framework used, and the various settings in which the comparative performance of Bayes estimators was studied, is set out in detail in my 2010 monograph. Whether any of this would have happened without Dennis's influence is difficult to say, but I tend to believe that Dennis's influence played a pivotal role. In saying this, I feel that I must add that my opinions and conclusions are my own, and while Dennis deserves credit for sparking my curiosity about Bayesian ideas, he certainly should not be burdened with any responsibility for the conclusions I've come to, some of which he would no doubt take exception to. I nonetheless appreciate the many things we do share, perhaps the most important one being the strong respect that we have for the Bayesian approach to statistical inference.

References

DeGroot, Morris H. (1970), *Optimal Statistical Decisions*, New York: McGraw-Hill.

Diaconis, P. and Freedman, D. (1986) On the consistency of Bayes estimates, *Annals of Statistics*, **14**, 1 – 25.

Lindley, D. V. and El-Sayyad, G. M. (1968) The Bayesian estimation of a linear functional relationship, *Journal of the Royal Statistical Society*, Series **B**, **30**, 190 – 202.

Samaniego, F. J. and Reneau, D. M. (1994) Toward a reconciliation of the Bayesian and frequentist approaches to point estimation, *Journal of the American Statistical Association*, **89**, 947 – 957.

Samaniego, F. J. (2010) *A Comparison of the Bayesian and Frequentist Approaches to Estimation*, New York: Springer.

Vestrup, E. M. and Samaniego, F. J. (2004) Bayes versus frequentist shrinkage in multivariate normal problems, *Sankhya*, **66**, 109 – 139.

Nozer Singpurwalla

Dennis Lindley: Inspiration – Friend

In the dedication of his classic book on Dynamic Programming, Richard Bellman used the phrase above to express his debt to von Neumann. I have plagiarized this phrase, because it so aptly captures the spirit of the personal debt I owe to Dennis. However, in so doing, no pretense of any parallel with the likes of Bellman is implied.

Whereas many in this volume will speak to Dennis' path breaking contributions to statistics, his dedicated persistence in preserving, nurturing, and shepherding Bayesian ideas to the 21st century, allow me to comment some on the personal side of Dennis, as I experienced it, whilst working with him, drinking coffee with him (it had to be black), enjoying an evening glass of sherry (Tio Pepe was a consistent choice) followed by a bottle or two of wine (Bandol if red, Vouvray if white), and topped off with a meal (the cuisine of Mughal emperors was always sought). No comments on the cuisines Dennis avoided, but the point is that Dennis' sophistication and tastes go beyond his passion for probability. Yes, probability, because Dennis has often mentioned, and strikingly so for a statistician of his renown, that "all I know is probability and the calculus of probability". Indeed Dennis is the maestro of the essence, instincts, and underpinnings of probability. And talking about maestros, western classical music and opera is another passion of Dennis (and Joan), a passion that he so decidedly relishes. The maestro did not react much to the music of the Mughals, other than the polite English nod. The customary turning of one's head from side to side as a signal of approval (in classical Indian music) was decisively absent.

Working with Dennis has been a memorable experience; indeed, an affair to remember! He is quick, always willing to listen, and amazingly insightful in taking muddled random thoughts and converting them to a coherent idea. I long for working more with him and to this I say Insha Allah (God willing)! Dennis would of course object to anything having to do with any mention of an almighty. Hopefully, a case has been made for inspiration. But like probability and utility, inspiration/ admiration cannot be separated.

Dennis, you are a figure of historical importance and rank with the likes of Laplace, Ramsey, de Finetti, and dare one say Kolmogorov? After all, it is you who spawned in us a passion for probability. Happy Birthday!

At the Kennedy Centre (Washington): Dennis, Dick Barlow, myself, Barbara Barlow, my wife Norah and children Rachel and Darius.

Adrian Smith

I owe my own association with Dennis entirely to a conversation with a Cambridge probabilist called Bob Loynes.

When I studied mathematics in Cambridge in the mid 1960s, for the most part statistics did not appear on the curriculum. There was a reasonable amount of measure and probability – a final year course on the latter taught by Loynes – but a mere 8 hours of something resembling statistics in the whole of the 3 year course.

And I have to say that, at the time, those 8 hours were pretty incomprehensible. My memory is that the structure of the lectures was like a 2x2 table of approaches to the world – inference/decision, frequentist/personalist – very abstract for those of us who had never dealt with any practical problems involving data. However, I was somehow intrigued by this incomprehensible stuff. Optimal decision-making seemed a very attractive macho occupation!

I should also admit that I wasn't the most dedicated student of mathematics. It seemed to me at the time that economics in Cambridge was a much more glamorous discipline – more women students and higher profile, charismatic professors, like Joan Robinson, whom I remember giving lectures in a Mao suit. So I went to more economics lectures than was good for my performance in mathematics.

As the three years at Cambridge drew to a close, we were offered a kind of "career consultation" with a member of the mathematics faculty and I chose Loynes simply on the strength of having enjoyed his probability lectures. The conversation went roughly as follows. "What areas of mathematics do you think you have done best at?" "Measure and probability". What subject have you enjoyed most?" "Economics" – not perhaps the smartest answer from a mathematics undergraduate.

I recall an awkward short silence before Loynes asked me if I'd ever heard of a subject called decision theory. I muttered something incoherent about the 8 lecture course. Loynes then said that, given my strength in probability and my interest in economics, he thought statistical decision theory would be just the thing for me and that he knew just the man for me to talk to.

That, of course, was Dennis, who had just moved to take up the chair at University College. I went down to London to speak to him and he kindly offered me a package of three years support to first do a one year conversion MSc in Statistics, followed by a PhD. But I recall him saying "I think I ought to warn you that I practise an unorthodox form of inference".

I enjoyed immensely the year spent converting to a statistician. I read Dennis's two volume textbook cover to cover – many times – but the MSc was almost entirely classical, although Dennis gave a wonderful Bayesian Decision Theory course. I did well in the MSc and at the end of the year it was time to identify a topic for my PhD thesis.

But in early September, 1969, there was a wonderful distraction – the 37th Session of the ISI took place in London, with a number of events held at University College. For some reason, I ended up, with a handful of postgraduate colleagues, in charge of an extremely boozy party for the great and good of world statistics. It was as if the Wiley Series in Probability and Statistics and the Berkeley Symposium had come to town, among them Ted Anderson, Henry Scheffé, FN David and Jerzy Neyman. A wonderful evening culminated with an invitation from Neyman for the bar staff to join him at a fancy restaurant. My one and only brief period of negative thoughts about Dennis followed, when he made clear to us that we were to stay put and clear up the débris!

Somehow a Bayesian PhD topic – hierarchical linear models - miraculously emerged from some scattergun reading around shrinkage estimators, random effects models and ridge regression, with a joint paper read to the Royal Statistical Society in December, 1971. During those two PhD years, without my noticing, Dennis had taught me how to think, to research and to write.

Actually, by that December I had left University College to take up a lectureship, and College Fellowship, at Oxford – the first statistician, I think, appointed in the Mathematics Institute, where John Kingman had recently moved to be Professor of Stochastic Analysis. John had tremendous admiration for Dennis, who had taught him probability and made fundamental contributions to probability theory. I've often thought that this connection may have influenced John in what was, at the time, a somewhat risky appointment of someone still awaiting the viva for their PhD.

I was in my third year in Oxford, when, completely out of the blue, I received a telephone call from Dennis asking me if I could be persuaded to come back to University College as a lecturer. I did not take much persuading! Oxford had (too) many charms and many bright students that were a joy to teach, including Dennis's daughter. But – unlike in London - there simply wasn't a critical mass of like-minded statisticians and statistical activity.

In particular, Royal Statistical Society discussion meetings were enormously stimulating – and Dennis was often in the thick of controversy, his interventions often generating more discussion than the Paper itself. I learnt a lot from his unflinching commitment to calm, forensic support of his arguments, often in the face of rather unpleasant personal attacks. He almost single-handedly forged the intellectual environment which made it possible – even if then not quite respectable – to have a successful career as a Bayesian statistician.

My own professional career owes everything to Dennis. He took me on as a post-graduate student and taught me how to think and how to do research. He played a pivotal role in creating an intellectual environment in which the Bayesian argument was listened to and came to be influential. Given my own wonderful experience, I think it was a pity that Dennis didn't have more PhD students, but perhaps the rather large number I had in part served as a proxy. In the Bayesian genealogical tree, they certainly thought of themselves with pride as Dennis's descendants.

Walter Smith

To my old friend Dennis Lindley, on his 90th. birthday

Let me first address Dennis directly, as though I were before him. The rest of you can eavesdrop!

It is far too long since I was in your company, which I have always enjoyed, and for some years I have had to glean news of you, filtered to me from various sources, including the not very satisfactory channel of Christmas cards, which are never intended, or designed, to convey detailed accounts of health and family activity! But it has been good for me to know that we are still, after so many years, in some sort of touch. Whatever! Mary joins me in wishing you a GREAT BIRTHDAY, and several more to come!

As you may know, Mary and I now live in a so-called retirement community these days, and I rub shoulders often with people in their nineties who still find life worth living. I have even been to a few 100th birthday celebrations at which the centenarian being honoured was still finding pleasure in life. So 90 is a great, a signal, achievement. But you must not rest on your laurels! I do so wish I could be there with you.

And now I will turn to those who are with you on your birthday, though you may tune in, if you wish!

On June 9th 1949 I went to my first meeting of the Royal Statistical Society: a meeting of the Research Section: a Symposium on Stochastic Processes. I knew almost nothing about anything at that time. But I could sense that the three speakers were accepted as “Big Shots”. The audience was about sixty very wise-looking and scholarly men. I felt like a gauche schoolboy. Then came the discussion part of the meeting. It was then that this very young man, a quite good-looking young man, went for the “giants” with terrier-like intrepidity. He simply wanted a definition of a stochastic process. And he could not extract a satisfactory definition. That was the state of the art in 1949. But this is not to be a technical, historical, discussion. Just take my word. The young man was Dennis Lindley, and I was very impressed with his courage and self-confidence!

Then, in September 1950, after considerable personal tribulations, I arrived in the Statistics Lab in Cambridge as a research student, supported by an ex-serviceman's grant and hoping to attain a Ph.D. after three years or so.

I was surprised to see that Dennis Lindley was among the small faculty. He had the curious rank of "Demonstrator", and I went to at least two courses he taught. One was on the foundations of probability theory, and it was, for me, extraordinarily important in my final development. I cannot overstate this.

But things did not go at all well for me, obliged as I was by "he who was deputed to direct my research" to work on a problem to do with cockroaches which I found both uninspiring and needing far more neurological expertise than I could ever acquire in the time available. Nevertheless, in the spring of 1952 I was required to give a colloquium talk on what I had so far accomplished. It was, for me, a scary experience in various ways. But now I get to the point of all this personal stuff. After I had given that talk, and after the audience had filtered away, one person remained with me. Not my research advisor, but Dennis Lindley. He said some encouraging things which, in my very low state of morale, were beautiful music in my ears. And then he offered to take me out to afternoon tea in the Combination Room. Without being ridiculous, indeed with touching sincerity, he made me feel that there was a good hope that I would end up with a Ph.D. and not be the embarrassing failure which I felt was my destiny. I have no idea if Dennis Lindley appreciated the signal kindness of his actions that afternoon, and of how he rescued me from such real despair. I will never, ever, forget.

But then, unwittingly, he did much more. He wrote his paper on queueing theory. There is no space here to discuss why it was so important, which it was. But it led to a certain kind of integral equation which had fascinated me for some time. When frustrated with my cockroaches I would seek relief, with a feeling of guilt, by studying integral transforms, complex variable theory, even the foundations of probability theory. Dennis's integral equation got under my skin and, with much encouragement on the side-line from my lifelong friend Ewan Page (a fellow research student at that time) I wrote the manuscript of a research paper on queueing theory. When Dennis saw this, and read it, he urged me to submit it for publication. And, a month or two later, it appeared in the Cambridge Philosophical Society Proceedings. My life turned round rapidly. My appointed thesis advisor left for a sabbatical in the US and I was placed under the guidance of David Cox, who was another saviour for me. But by Christmas 1952 I had a number of publishable papers in the works, mainly joint with David Cox, and my morale was utterly different from what it had been only eight months earlier. I truly think without Dennis's encouragements "All the voyage of my life would have been bound in shallows and in miseries" (to adapt Shakespeare a little).

This piece is not meant to be about me at all, but about Dennis. But you must forgive my recounting my personal experience since you need to know what a critical role he has played in my life. If he had not cheered me up that afternoon of the colloquium, if he had not encouraged me in my first research in queueing theory, I feel sure my life would have been far, far, less than it has been.

A few years later I was myself a member of the staff of the StatLab and for most of one year Dennis and I had to "hold the fort" alone, so many of the original staff had left for one reason or another, including the tragic drowning of John Wishart. During this year Dennis and I became particularly close, and I have never experienced comradeship such as I experienced for that year. In the course of which year he somehow committed me to writing a discussion paper for the RSS research section. On top of the mountain of teaching work I was having to do at that time, and with frequent snowfalls interfering with one's life, encouraged by Dennis, I met the necessary deadline. That paper did me a huge amount of good. But it would not have been written but for Dennis.

We have been together in so many places other than Cambridge: Chicago, Chapel Hill, Aberystwyth, Washington DC, Corvallis in Oregon, and we once went together to St. Paul's Cathedral since we found that neither of us had ever visited that edifice, and we had a few hours to kill before a meeting of the RSS. There is so much I could recall, so many details, but I feel I have already taken up too much space.

Dennis, I do not know in what health you now find yourself. I must agree that my own is, looking around at the "inmates" of this retirement place, apparently "above average". But I feel too tired a lot of the time. Meeting deadlines throws me into a fit. And I often feel much less buoyant than was my wont. But I shall count always among my life's blessings my association with you. Really.

Happy Birthday!! And a million thanks!!

David Spiegelhalter

My memories of Dennis are inextricably tied up with being in the Department of Statistics at UCL between 1974 and 1977. It was a time of IRA bombs and hot summers, and for me at least, rather a lot of beer in the student bar.

But the atmosphere in the Department was exciting, and it really felt like a place of ideas and discussion.

Egon Pearson still had an office and used to appear occasionally – tall and impressive – but the Bayesian persuasion dominated. Visitors coming to give seminars did so at their own risk, and we graduate students enjoyed the spectacle: perhaps the closest image is what it would have been like for Roman Christians, who were invited to a tea party and then were surprised to find themselves thrust into a ring and confronted by heavily-armed and ruthless gladiators. While Dennis was typically penetrating and pithy, I think Mervyn Stone – the lion of the arena - must have been more disconcerting to the 'guests'.

I will forever be grateful to Dennis, Adrian Smith and the others for passing on some of their passion for the subject, which has been such an influence on my subsequent career.

And fast-forwarding 35 years, I would also like to thank Dennis for being so supportive in my new job as Winton Professor for the Public Understanding of Risk, trying to bring subjectivist Bayesianism to the masses without mentioning the 'B-word'.

Mervyn Stone

It takes a paragraph to set the scene for the amazing coincidence that came with my first meeting with the man we are here rightly celebrating. In 1954, I was lucky not to get a distinction in Part III of the Maths Tripos, and therefore lucky to be denied DSIR support to become a 'statistical mechanic(ist)'. At the last minute, a tolerant John Wishart let me scramble onto the Diploma course – and into the hut where that year's cohort of galley slaves were to spend nine months turning the handles of Brunsvigas and Facits. The cacophony in the thin-walled hut can hardly have aided the deeper thoughts of a constellation of lecturers – Wishart himself at the far end of the corridor but with Henry Daniels, Frank Anscombe and David Cox in cupboard offices down line. Doing better than Part III, I completed another DSIR form – for which I needed to name a research topic and a supervisor. The first came with surprising ease – it was the thinnest book in the StatLab library: *A Mathematical Theory of Communication* by Claude E. Shannon. Even to a beginner's eye, here was a new and perhaps accessible field whose small compass and elegant simplicity were more appealing than the volume and elegant complexity of k-statistics!

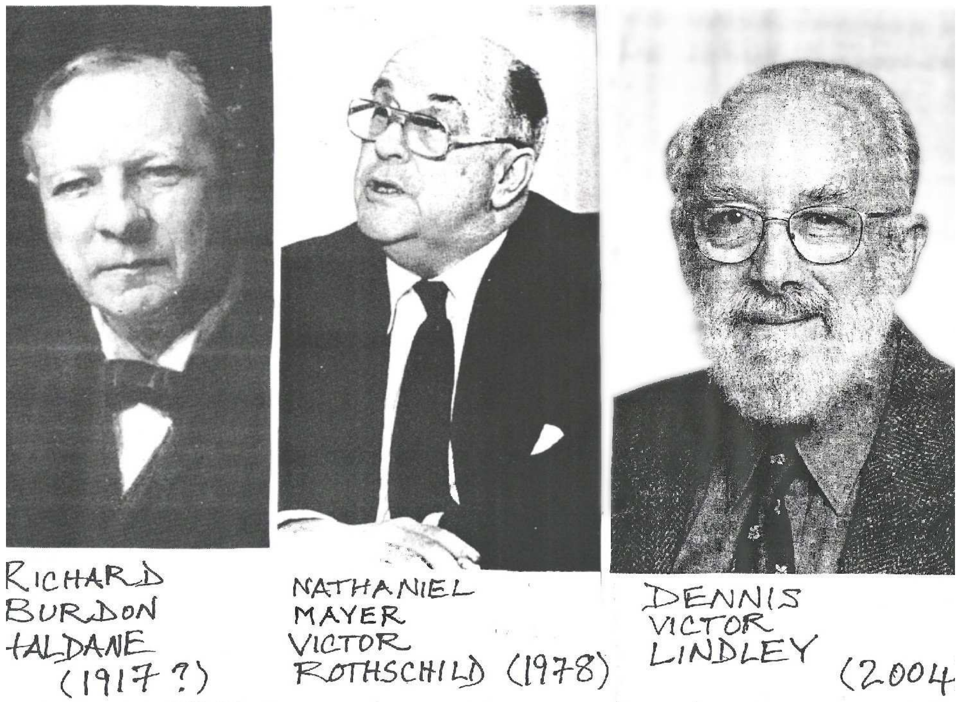
For a supervisor, I went to my tutor Daniels who tactfully said he was not looking for another research student and suggested one of their number, a young lecturer on sabbatical with L.J. Savage in Chicago – who fortunately agreed to take me on without searching enquiry. Imagine my surprise and delight when Dennis returned and gave me a copy of a paper that had nearly been rejected by the Annals because it was 'not mathematical enough'! That paper, entitled '*On a measure of the information provided by an experiment*', had the elegance that is in all that Dennis has written and that kept me going as a part-time Ph.D. student – to complete a deservedly fading mimeoed thesis, whose first sentence puts on record for ghostly posterity that '*in a paper published in 1956, D.V.Lindley placed the Shannon-Hartley measure of information in the Bayesian framework and thereby provided a satisfying basis for the application of the measure to statistical experimentation*.' If Dennis subscribes to Series C, he may have seen in Volume 61 a nice application of the 56-year-old Lindley-Shannon measure to an empirical Bayes problem.

It was after I had joined him in his new Aberystwyth department of statistics in 1961 that I saw in Dennis an uncommon quality – a willingness (for which he used to quote Cromwell) to admit error. Which was all the more remarkable because, for the single error I have in mind, he could easily have diverted attention to its originator, Sir Harold Jeffreys – who had not realised that data-point-wise limits of posteriors for a sequence of priors do not commute with the Bayesianity or otherwise of the limit posterior (despite the Bayesianity of everything in the sequence). Dennis's error, may I suggest?, was his initial and only briefly maintained unwillingness to believe that such a man (author with his Bertha of the monumental *Methods of Mathematical Physics*) could have been wrong in any fundamental respect – a reluctance that L. J. Savage may have shared for a time. I recall that, years earlier, I had (at the request of Dennis who had another engagement) chauffeured Savage to visit the great man in his home – a strange encounter in which Jeffreys had been largely silent, stroking his cat through what was far from a meeting of minds!

I have no particularly unique recollections of the years we shared in the high-ceilinged Pearson Building on Gower Street – that is to say, no memories that other contributors to this collection could not equally well evoke. To its newcomers, the building almost reeked of the traces of its well-documented disputes and it was not too difficult to keep up the tradition, with Dennis centre stage as bold advocate of a complete clearance of statistical concepts that violated the axioms of rational choice. However, civility was wisely maintained and many of us, including Dennis I am sure, will remember the vigorous Journal Club seminars in the carpeted tea-room (where RAF and KP may once have glowered at each over tea – milk first or not). Those were the days of paper & pencil and chalk-board mathematics, and there was a lot of enjoyable to-ing and fro-ing on the Bayesian front with Dennis sometimes leading the charge, sometimes conceding ground.

But things were to change! I have a vivid recollection of the tea-time when Dennis came in from a committee meeting and told us we had the option of taking a couple of computer scientists from the dissolving Institute of Computer Science. He presented the case for and against, as our democratic choice. We chose, rationally as we thought, to move out of the Brunsviga age into the new dawn of statistical computing and, within a year or two, to welcome the transition to a much larger department. Dennis retired – to become the world's recruiting officer for service in a Bayesian 21st century – before a new provost, James Lighthill, agreed to bring a flawed union to an end. My successor as head of department could expand on that.

What has distinguished Professor Lindley's lifelong contributions to our discipline has been his unwillingness to compromise principles of rationality. It has been my good fortune to have had his strong support for my efforts to get more rationality in an area of statistical activity not subject to that discipline – an area that still does not command enough RSS attention. Statistical reasoning rarely contributes to the ‘policy making’ of government departments that spills out as one weird formula after another for this and that. The arguments Dennis deployed in a preface to a booklet I wrote for Civitas were impressive enough to get it into one list as a book by ‘Stone and Lindley’ – on which happy note I want to end, rather than on a recent gentle disagreement about the Tuesday boy (or Florida girl) paradox. The booklet praises a Haldane but questions the Rothschild with whom Dennis was once co-author of a ‘bullish’ paper – three names that now hang over my PC in this collage.



Herman van Dijk

Two Issues in Bayesian Inference: The Lindley Paradox and Non-elliptical Credible Sets

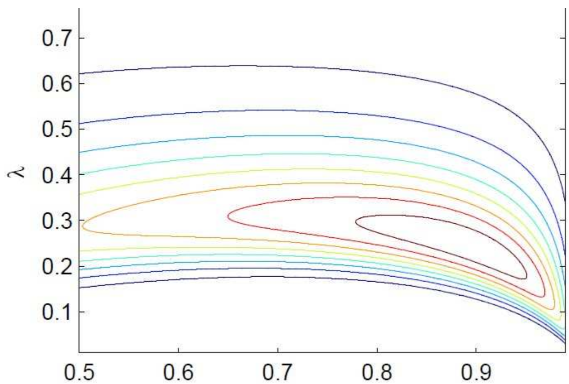
The purpose of this homage to professor Dennis Lindley is to stress that the so-called Lindley Paradox that appeared in the literature in the 1950s remains still very relevant – in the sense that all applied Bayesian researchers have to specify their priors very carefully when they compare alternative hypotheses on model structures with the intention to let the information from the data in the likelihood dominate that of the prior. Novel simulation methods can simulate from posteriors that display non-elliptical credible sets, but further methodological advances seem to be required for the evaluation of certain predictive likelihoods for complex models within a reasonable amount of computing time.

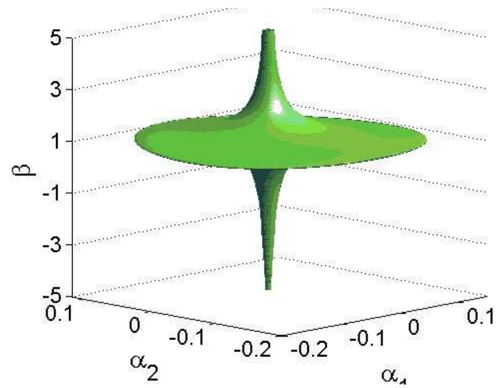
Personal recollection

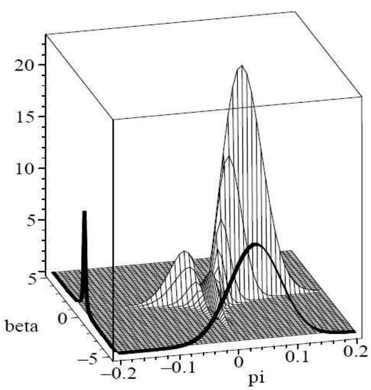
I met Dennis Lindley in June 1976 at a Conference in Fontainebleau. I presented my paper (with Teun Kloek) on Monte Carlo (MC) integration of posterior densities using Importance Sampling. MC deals with experiments using random numbers and usually frequentist logic is used to discuss the numerical accuracy of the estimated posterior probabilities. During the presentation in 1976 an expression for an Importance Sampling estimator was written on the blackboard with the additional statement that this estimator was "asymptotically unbiased". Dennis made then – in his eloquent way of making a strong point at a seminar or conference – the following remark: " *the authors are to be commended for introducing a new computational technique in Bayesian analysis but the term "unbiased" should be immediately removed from the blackboard. Consistency is an acceptable property for a Bayesian but unbiasedness is a frequentist property that does not belong in a Bayesian conference".* At later meetings and conferences Dennis repeatedly advocated a Bayesian version for Monte Carlo analysis of posterior and predictive analysis. Given the large numbers of draws in these experiments and the random nature of the generated draws, frequentist techniques are still dominant for analyzing accuracy of MC, but Dennis' thoughtful remark remains inspirational for me and for my collaborators. The discussion presented below is joint work with Lennart Hoogeheide.

Monte Carlo simulation methods for Bayesian analysis of models with non-elliptical credible sets

Monte Carlo simulation has freed the Bayesian approach from very restrictive model structures: it allows Bayesians to apply their inference to a wide range of complex models in many scientific disciplines. The three figures below show examples of complex (non-elliptical) posterior distributions in models for realistic problems in economics. The posterior distributions occur in finance (modeling daily stock returns), macroeconomics (modeling the joint behavior of variables with a long run equilibrium relationship), and microeconomics (modeling the effect of education on income), respectively.







Using novel simulation methods one can obtain reliable and accurate estimates of the properties of interest of such posterior distributions. For example, the *'bottom up'* method of Hoogerheide, Kaashoek and Van Dijk (2007) or Hoogerheide, Opschoor and Van Dijk (2012) starts with a simple approximation and continues until it has obtained a mixture of Student's t distributions that provides a usable approximation, to be used as a candidate distribution in Importance Sampling (introduced into statistics and econometrics by Kloek and Van Dijk (1978)) or the independence chain Metropolis-Hastings algorithm.

Lindley's paradox, predictive likelihoods and future research

Lindley's paradox – or Bartlett's or Jeffreys' paradox; see Lindley (1957) and Bartlett (1957) – implies that one has to choose very carefully the amount of prior information compared to the amount of sample information, when comparing alternative hypotheses on model structures with the intention to let the information from the data in the likelihood dominate that of the prior. Typically a naïve or malevolent researcher could 'force' the posterior probability of a certain model M – the 'restricted model' in case of two nested models – to tend to 1 by letting the priors in all alternative models tend to diffuse priors, thereby decreasing the marginal likelihoods of all alternative models, even if the particular model M does not make sense and poorly describes the data.

In an attempt to make the posterior model probabilities 'fair', one could use predictive likelihoods instead of marginal likelihoods; see Gelfand and Dey (1994), O'Hagan (1995), and Berger and Pericchi (1996). However, the use of predictive likelihoods brings several questions and issues. First, one must choose the *training sample* and the *hold-out sample*. Examples of important questions are: How many observations are included in these samples? Is one training sample used or does one average over multiple (or all possible) training samples? In the latter case, what does one average – e.g., marginal likelihoods, logarithms of marginal likelihoods, Savage-Dickey Density Ratios or posterior model probabilities? Second, if one chooses to average results over multiple (or all possible) training samples, then the computing time that is required for obtaining all Monte Carlo simulation results for all training samples may be huge. In future research we will develop simulation methods that are particularly suited for the evaluation of marginal likelihoods (or Savage-Dickey Density Ratios) for a large number of training samples. Note that in complex models the shapes of credible sets based on (possibly very small) training samples will often be highly non-elliptical. Therefore, a suitable method must deal with large numbers of different non-elliptical shapes in a feasible computing time. Third, for time series models computing the marginal likelihood for a random subsample implies that the estimation must be performed for an irregularly spaced time series, which is typically only feasible using an appropriate formulation and estimation of a state space model. Depending on the original model structure, this may be both difficult and time consuming. In future research we will investigate these issues, and develop computationally efficient and accurate simulation methods that can deal with these aspects.

Conclusion

Lindley's Paradox remains one of the key issues in Bayesian inference. The use of predictive likelihoods seems a good attempt to 'deal with it' and obtain 'fair' probabilities of alternative model structures, but this approach brings many questions that may not be easily answered,

especially when comparing models with completely different, complex structures, and it may require huge computational efforts which may need an enormous amount of computing time, even using state-of-the-art methods on modern computers. In future research we will investigate both issues. For the persistence in pursuing ideas we owe a great intellectual debt to Dennis Lindley.

Lennart Hoogeheide

References

Bartlett, M.S. (1957). "A Comment on D.V. Lindley's Statistical Paradox." *Biometrika* 44 (3/4), 533-534.

Berger, J.O., Pericchi, L.R. (1996). "The Intrinsic Bayes Factor for Model Selection and Prediction." *Journal of the American Statistical Association* 91 (433), 109-122.

Gelfand, A.E., Dey, D.K. (1994). "Bayesian Model Choice: Asymptotics and Exact Calculations." *Journal of the Royal Statistical Society Series B (Methodological)* 56 (3), 501-514.

Hoogerheide, L.F., Kaashoek, J.F., Van Dijk, H.K. (2007). "On the shape of posterior densities and credible sets in instrumental variable regression models with reduced rank: An application of flexible sampling methods using neural networks." *Journal of Econometrics* 139 (1), 154–180.

Hoogerheide, L.F., Opschoor, A., Van Dijk, H.K. (2012). "A class of adaptive importance sampling weighted EM algorithms for efficient and robust posterior and predictive simulation." *Journal of Econometrics* 171 (2), 101–120

Kloek, T., Van Dijk, H.K. (1978). "Bayesian Estimates of Equation System Parameters: An Application of Integration by Monte Carlo." *Econometrica* 46, 1-20.

Lindley, D.V. (1957). "A Statistical Paradox". *Biometrika* 44 (1/2), 187-192.

O'Hagan, A. (1995). "Fractional Bayes Factors for Model Comparison." *Journal of the Royal Statistical Society Series B (Methodological)* 57 (1), 99-138.

James Zidek

Reflections on Dennis Lindley and my visit at University College London

On September 9, 1970 I made the fateful decision to post a letter to Professor D.V.Lindley saying that I anticipated approval of a request for Sabbatical leave and requesting permission to visit his Department at University College London. I said that what I mainly required were "research facilities such as Office Space". Dennis said yes and I arrived in that department about one year later to commence what proved to be quite an extraordinary year and the beginning of a long and fruitful association with Dennis and his colleagues. More fundamentally, that visit was a landmark event in my budding career and completely changed its direction.

That was a great time to be in London. For one thing my wife and I could afford to be there then, even on my reduced sabbatical salary. Rent was 22 pounds per month and a cup of tea was 5 pence. But more to the point, London was a powerhouse of statistical thought, with many strong statistical groups and individuals. The joint Friday afternoon lecture series organized by the University of London colleges was making a great contribution to the education of budding research students and the formal seminar series brought numerous distinguished speakers to town. Then there were the RSS read papers and lively discussants that went with them in halls pervaded by Fisher's ghost and the high intellectual standards he brought to these occasions. At pubs after the meeting, I met a lot of stars and rising stars of English statistics such as the late Julian Besag prior to his work on dirty pictures.

Dennis proved a most congenial host. For example, when Jim Bondar turned up unannounced and told Dennis he was a friend of mine (from when Jim visited my UBC department), Dennis took him in as well! Dennis and his wife entertained me and mine in their home on a number of occasions. He also got me an invitation to the famous Statistical Dinner Club on the evening of November 24th, 1971, where a total of 18 of us plus 4 guests and the RSS speaker that day, the late George Barnard, enjoyed a banquet. Diners included many of the by then famous figures in the world of statistics such as the late Henry Daniels and Sir Morris Bartlett. Dennis also extended an invitation to the Department's annual dinner on Gower Street, which for those who do not remember, featured as its *pièce de résistance*, "Chicken a la King". "Devils on Horseback" was served just before coffee – a very festive entry into the holiday season.

Dennis's colleagues also made me feel welcome and I formed long lasting friendships with many of them. One was Mervyn Stone, with whom I had corresponded about topics in the application of mathematical group theory in statistics. I got to know him because of a journal club presentation he made. He began with the shocking revelation that the day's newspaper headlines were proclaiming the death of chi-squared. (In fact, we later learned it was the famous Chinese panda Chi-Chi that had bit the dust.) Also soon after my arrival, I met Phil Dawid who was then working on a PhD under Dennis's supervision. In the end he by-passed that degree in favor of a distinguished research career that included a DSc from Cambridge. Although retired, Egon Pearson came to his office on Gower Street regularly, giving me a chance to converse with him at the Department's afternoon teas. Quite an honour for a research statistician in his salad days, to meet such an important figure in the development of statistical theory, particularly one whose work had laid the foundation for my research area of Wald decision theory. All in all, thanks to Dennis I had a very pleasant academic home for my sabbatical year.

I did not know that hiding in the wings was another great figure in English statistics, the Reverend Thomas Bayes. His presence was revealed over lunch one day when I was asked about my research topic. I explained that it was about statistical procedures and their admissibility, a measure of their performance based on averages over the infinite number of samples that might have obtained in addition to the one that did. Silence. After many such conversations, I came to develop an appreciation for the foundations. Moreover the "Bayesian School (BS)" came to play quite a prominent role in my work at UCL. That work stemmed from Mervyn's lecture, which described an anomaly associated with some BS procedures, when so-called "improper" priors were used – these are the ones without finite integrals (and called that perhaps because they were imported from France). These anomalies could be explained with the help of group theory. Phil joined in this work and we gradually found ways of resolving this problem as well as problems where it was irresolvable. We wrote a paper and read it at an RSS meeting, a paper we called the "Marginalization Paradox". It featured two Bayesians B1 and B2 and an innocent lab technician. Using one of these improper priors and starting with data (y, z), B1 calculates a posterior distribution *p*(eta, zeta | y, z), which when "marginalized" for the parameter of real inferential interest, zeta, gets a marginal distribution *p*(zeta | z), that depends only on z. The technician was annoyed that he had been forced to collect the redundant data y. B2 enters the analysis late and finds that s/he cannot get the same answer using the Bayesian paradigm. More precisely B2 notices that B1 needs only data z with likelihood *f*(z | zeta) and learns that no prior *p*(zeta) yields B1's result when combined with the likelihood using the Bayes rule. B2 recognizes that the anomaly would lead to incoherent outcomes if B1's marginal posterior were used, things such as "Dutch book", making it unattractive to a Bayesian.

Our RSS read paper was quite a performance. Mervyn and Phil got the starring roles of B1 and B2 while I was given the supporting role of the technician. Lively discussion followed with Dennis playing a leading role. He said

> "Let me personally retract the ideas in my own book. A book that was written as a serious attempt to justify, within the Bayesian framework, much of conventional statistical wisdom."

That wisdom would include advocacy of common statistical procedures like the sample mean as an estimator of the normal mean, which are obtained by use of an improper prior. He thanked us for "clearing so much rubbish from the Bayesian scene" or "highway" in my terminology. Brad Efron, who followed Dennis, said he had just purchased Dennis's book and demanded a refund, although this remark was not published. Nor have we any idea if Brad got his refund. With the trash gone, the highway was open for us to travel into what Dennis later forecast to be the Bayesian 21st century.

And travel we did! The paper [Lindley and Smith, 1972] Dennis wrote with his former student Adrian Smith is a classic, a landmark on that highway. However, its genesis lay in the James-Stein estimator [James and Stein, 1961], which revealed a serious flaw in the RS paradigm and showed that unbiasedness, a hallmark of Fisher's theory, could be traded for variance reduction and an overall increase in estimation accuracy. But the reason why it worked was revealed in Dennis's work. Thus at a discussion session on the legacy of Wald, which I led in an Oberwolfach meeting of Wald decision theorists, the late George Casella, said simply "exchangeability" when I asked the audience about that legacy. Everyone in the room knew what he meant. The full force of Bayes was recognized and soon important applications were being made, for example in small area population estimation. The Valencia meetings organized by José Bernardo, another Dennis protégé, added impetuous and the strong advocacy positions taken by Dennis and his other students such as Tony O'Hagan plus Dennis's other co-investigators assured success. Thus here I am in the 21st century typing on a machine that has learned more about me than I have learned about it. It even predicts the new releases that I will most enjoy at my local cinema!

However, clouds were appearing on the horizon. The work of Tversky and Kahneman (see Kahneman [2011] for a readable account) led many normative Bayesians to retreat to descriptive or prescriptive subjectivist positions. And the improprietists re-emerged. One of these was José Bernardo, who with Jim Berger (see Berger and Bernardo [1992] for example) developed reference priors, which can be improper, to provide an objective basis for policy makers. But my return to my UCL days came from another source.

That source was the late E.T. Jaynes and an article in which he argued that the paradox arose "not from any defect of improper priors, but from a rather subtle failure … to take into account all the relevant information" [Jaynes, 1980]. A lengthy period of exchanges with Jaynes followed, leading ultimately to a chapter in his book [Jaynes, 2003], initially published on cyberspace along with our rebuttals on another webpage. A series of papers by others appeared, supporting or attacking the paradox, ending with one by Wallstrom [Wallstrom, 2007] who concludes:

> "Although we can [resolve some inconsistencies this way] probability limits do not exist for most of the problems previously leading to inconsistencies. Thus the MP remains a serious challenge to objective Bayes."

That led us to go "back on the road", as Mervyn put it, and a return to the track on which we embarked while I was on my sabbatical, towards a manuscript still under preparation at the time of this writing called "The Marginalization Paradox Revisited". Thus that visit to Dennis's department just "keeps on giving".

All and all, Dennis, that Department and the people in it, played a huge role in my developing academic research career. Although I did not have the opportunity to work with Dennis directly, he certainly spawned my interest in the foundations of the subject and led me to my work on what is now called multi-agent decision theory [Weerahandi and Zidek, 1983] and that on pooling opinions [Genest and Zidek, 1986].

Looking through my folder of correspondence, I see a letter of thanks dated August 23, 1972 that I sent him on my return to my position at the University of British Columbia's Department of Mathematics. It contains the following comment:

> "Since returning I've had occasion to reflect on my confusion over the foundations of statistics, and now, believing that confusion is the true path of enlightenment, I am attempting to confuse my incoherent colleagues. So far, however, I have only been successful in the case of 2 algebraic geometers."

I cannot find Dennis's response, although I am sure he appreciated my albeit limited success! I continue to read Dennis's work from time to time and much admire the great clarity of his vision and eloquence of his words that have had such a great impact on the development of statistical science. I warmly congratulate him on his remarkable achievements on his 90th birthday.

Finally I look back warmly at that year he made possible for me at UCL and the ensuing work that it spawned. Its continuation to this day keeps the memories of it alive in a very active way.

References

James O Berger and José M Bernardo. On the development of reference priors. *Bayesian Statistics*, 4(4):35-60, 1992.

Christian Genest and James V Zidek. Combining probability distributions: A critique and an annotated bibliography. *Statistical Science*, 1(1):114-135, 1986.

William James and Charles Stein. Estimation with quadratic loss. In *Proceedings of the Fourth Berkeley Symposium on Mathematical Statistics and Probability,* volume 1, pages 1-379, 1961.

Edwin T Jaynes. Marginalization and prior probabilities. In A. Zellner, editor, *Bayesian Analysis in Econometrics and Statistics*, page 43. North-Holland, Amsterdam, 1980.

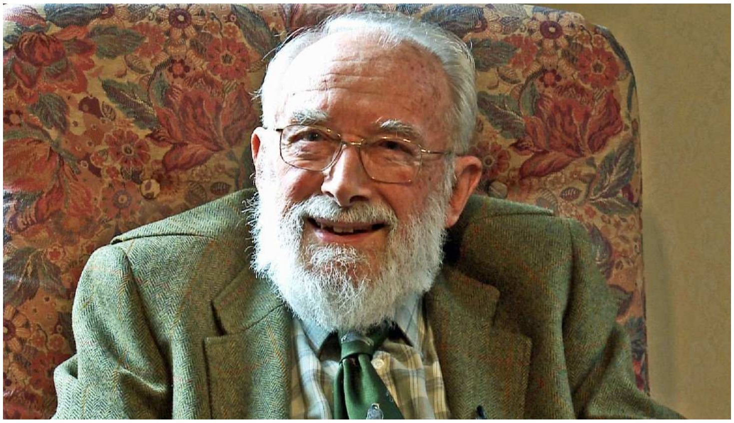
Edwin T Jaynes. *Probability Theory: The Logic of Science*. Cambridge university press, 2003.

Daniel Kahneman. *Thinking, Fast and Slow*. Farrar, Straus and Giroux, 2011.

Dennis V Lindley and Adrian FM Smith. Bayes estimates for the linear model. *Journal of the Royal Statistical Society. Series B (Methodological)*, pages 1-41, 1972.

Timothy C Wallstrom. The marginalization paradox and the formal bayes' law. *arXiv preprint arXiv:0708.1350*, 2007.

S Weerahandi and JV Zidek. Elements of multi-Bayesian decision theory. *The Annals of Statistics*, 11(4):1032-1046, 1983.



Dennis Lindley, 2013

Envoi

This book was to have been presented to Dennis Lindley at a lunch in his honour, at his favourite restaurant on the 14th of August, 2013. Unfortunately, Dennis was in hospital on that date. The lunch went ahead and the small group of friends present all signed the book. Dennis received it a few days later.

He was delighted with the book and very moved by the whole project. He enjoyed all the various contributions - touched by the more personal ones and intrigued, as ever, by the more technical ones.

Sadly, although he recovered from the illness in August, he died unexpectedly on the 14th of December, 2013, from a heart attack.

Goodbye, Dennis. A light has gone out that for decades had shone brightly on the field of Statistics, penetrating to the heart of the subject and illuminating it with brilliant and irresistible clarity.

This note was added to the original book in January, 2014.