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A Conversation with Maurice Bartlett

Ingram Olkin

Maurice Stevenson Bartlett was born on June 18, 1910 in London. He received his secondary education at Latymer Upper School and subsequently at Queen's College, Cambridge. His first position in 1933 was as Assistant lecturer in the Department of Statistics at University College London. In 1934 he joined the Imperial Chemical Industries (ICI) Research Station of Jealott's Hill. Much of his early work was carried out during his stay at ICI, which he left in 1938 to become a lecturer in mathematics at Cambridge. During World War II he was involved in rocket research, and in 1947 became Chair of Mathematical Statistics at the University of Manchester. He returned to University College London as Professor of Statistics in 1960. From 1967 to 1975, when he retired, he was Professor of Biomathematics at Oxford. He was elected a Fellow at the Royal Society in 1961, was awarded the Guy Medal in Gold of the Royal Statistical Society in 1969, received the Weldon medal from the University of Oxford in 1971, an honorary D.Sc. from the University of Chicago in 1966 and from the University of Hull in 1986. He was President of the Manchester Statistical Society 1959–1960 and of the Royal Statistical Society 1966–1967. He became an honorary member of the International Statistical Institute in 1980.

The following conversation took place in late February 1987 in Santa Barbara, California.

THE MAKING OF STATISTICIANS

Olkin: Professor Bartlett, we're really very pleased to have this opportunity to review some parts of your professional life and career. I know that your connections in statistics have touched on Cambridge University, University College London, Imperial Chemical Industries, University of Manchester, Oxford, and that many of your biographical sketches and reminiscences are described elsewhere.

Before we start, I'd like to make sure that our readers have these references. There is the book edited by J. Gani entitled *The Making of Statisticians* published by Springer-Verlag in 1982 that contains an autobiographical chapter (pages 41–60) entitled "Chance and change."

Earlier, in 1956, you have an expository paper on a visit to Moscow for the Third Soviet Mathematical Congress in *Journal of the Royal Statistical Society, Series A* Volume 119, page 456. In 1965, you have a general article entitled "R. A. Fisher and the last 50 years of statistical methodology." This article comprised the first R. A. Fisher lecture in the United States.

There is another general article in 1980 "All our yesterdays," *Newsletter* Number 69 of CSIRO (Commonwealth Scientific and Industrial Research Organization).

Bartlett: You are very well informed, I must say.

Olkin: There is also an obituary on Egon Pearson, 1895–1980, in *Biometrika*, Volume 68, pages 1–11.

Bartlett: There is a longer obituary of Pearson in a Royal Society memoir. That was also followed by a biographical note of Pearson for the *Dictionary of National Biography* published in England.

Olkin: I note that there is a bibliography of some of your papers in *Statistica*, Volume 16, 1956, pages 97–100. Do you recall other general articles that we might let our readers know about?

Bartlett: I can't recollect any general articles which would refer much to my life, as such. I have a biographical note about J. O. Irwin in the *International Statistical Review* [**52** (1984) 109–114]. This is not particularly autobiographical except insofar as it relates to Irwin.

THE EARLY YEARS

Olkin: Many of these articles provide some background about yourself and your early days in statistics. I thought that we might begin with a discussion of some of your statistical papers. I note from your bibliography that there was a series of papers from 1933 to 1940. This was a very exciting time in statistics. There were papers by Hotelling on the multivariate t-statistic in 1931, on principal components in

1933 and canonical correlations in 1936. Let us focus on that period. What research were you engaged in at that time, and what are your recollections of that exciting period?

Bartlett: Perhaps I could go back even a year or so from the 1933 point that you mentioned. In 1932, I wrote my first paper with John Wishart, when I was still an undergraduate; and this was followed up in 1933 with a second paper on the derivation of the Wishart distribution by analytical means using characteristic functions. Previously Wishart's original derivation was obtained by a geometrical approach.

Olkin: What was the 1932 paper?

Bartlett: The 1932 paper was "Distribution of second-order moment statistics in a normal system" [*Proc. Cambridge Philos. Soc.* **28** (1932) 455–459], related to the distribution of the second-order moments or covariances by themselves, using these methods. The papers were of some interest, although not all that important; I think Oscar Irwin was one of the first to use the characteristic function for deriving the sampling distribution of a sample mean; perhaps the original point that I contributed was simply to bear in mind the definition of characteristic function as expectation (of the appropriate function).

Thus if you wanted to consider, for example, the characteristic function of $\sum X_i$ and $\sum X_i^2$ jointly, there was no problem. You were merely taking the expectation of a joint exponential function, and that led quite naturally to deriving the distributions of statistics like the mean and variance simultaneously.

Olkin: In reference to the characteristic function, I have a recollection that an analyst Ingham was involved in that work.

Bartlett: Yes. In the case of the Wishart distribution, we arrived at the characteristic function, but, of course, this had to be inverted, and A. E. Ingham was instrumental to seeing how it could be inverted, and that was quite interesting. He wrote a paper on that aspect. A further slight interesting point about this is that in his method of inverting it, he was, in a sense, factorizing the Wishart distribution, at least implicitly, and this led me to write my paper in the Edinburgh Proceedings, which I called "On the theory of statistical regression" [*Proc. Roy. Soc. Edinburgh* **53** (1933) 260–283].

This was in 1933, when I, in effect, factorized the Wishart distribution in this way, and I also generalized it to deal with so-called independent variables of arbitrary distribution, normal or fixed, whatever you like.

The factorization I think was useful; thus P. L. Hsu later gave another derivation of the Wishart distribution in the *Proceedings of the Cambridge Philosophical Society* [**35** (1939) 336–338], which was very nice and brief. I'm not sure whether he refers to my Edinburgh paper, but in effect, he uses the decomposition factorization to obtain the result by induction.

Olkin: It was interesting that the Wishart distribution generated so much interest. Wishart's paper appeared in 1928 and within a 10-year period there were quite a number of independent derivations, by yourself and Wishart in 1933, Mahalanobis, Bose and Roy in 1937, by Madow in 1938, by Hsu in 1939, by Rasch in 1948. In 1951 my thesis included yet another derivation, and a modified version of this derivation is contained in a joint paper with Roy in 1954. I am sure that other derivations have been obtained. In some texts your 1933 derivation is referred to as the "Bartlett Decomposition," whereas in other texts it is called "Rectangular Coordinates," a name that comes from the Mahalanobis, Bose and Roy paper in 1937.

Bartlett: Yes.

Olkin: There were lots of other things going on in multivariate analysis during those years. You became involved with factor-analytic models. How did that come about? Were you associated with psychologists or geneticists?

THE FACTOR ANALYSIS CONTROVERSY

Bartlett: I think the connection with factor analysis arose from my acquaintance with Oscar Irwin

Maurice Bartlett; photo taken in March 1987 at University of California, Santa Barbara.

because he was in London, and although he was at the London School of Hygiene, he had some interests, I think, in factor analysis, which he communicated to me. I can't really recollect how the contact developed, but it wasn't long before I was in correspondence with Godfrey Thomson about estimating factors and what this meant. As an aside, Godfrey Thomson was at Moray House, University of Edinburgh, and one of the leading figures in Great Britain concerned with the possibilities of factor analysis in the assessment of pupils' mental abilities. The first edition of his book *The Factorial Analysis of Human Ability* was published in 1939.

I wrote a little letter in *Nature* about estimating general ability, which aroused some correspondence with Godfrey Thomson. In fact, quite recently I discovered a very long letter that I wrote to Thomson explaining the difference between my proposed method of estimating general and group factors, say, and his method, which was a kind of regression method for estimating the factors, and I explained the connection between the two.

One was, in effect, obtainable from the other by a straightforward transformation. But quite apart from the interest at the time, I think this raised an interesting estimation point which cropped up again later, for example, in connection with Charles Stein's work on estimating the mean of a multivariate distribution. The point is, if you have a population of individuals, that you can either (i) estimate all together or (ii) try to estimate the factors for an individual as such; you can imagine taking more tests on that individual, so that your increasing sample will acquire more information on that individual. This was, in effect, the method that I had in mind. Or you can do what Thomson was doing, which may be analogous to what Stein and others have considered, and that is to say, if you want the best accuracy for the whole population regarding the accuracy, say, in terms of the average mean square error, if you regress your estimates toward the mean, you'll find that your mean square error is less.

But it's a matter of what you need the estimates for that determines which estimates you regard as the better ones. Within this context, there is no particular criterion to say which was the better because they were, in any case, transformable one to the other. But the important thing is to distinguish between them, and this is what I did at the time to Godfrey Thomson.

Olkin: It's interesting that this idea of shrinking arose 50 years ago.

Bartlett: Well, yes. I think so. After having gotten in touch with Thomson, I became interested in factor analysis and in his approach to the problem, which was to emphasize that the concept of general and group factors could be construed as a sampling of many components which made up these factors. This was rather a contrast with the attitude of most psychologists that these factors were something real in themselves which we are trying to estimate.

I wrote a paper, for example, in the *British Journal of Psychology* called "The statistical conception of mental factors" [*British J. Psychol.* **28** (1937) 97–104], in which I explained how Godfrey Thomson's approach could be formalized in a fairly straightforward manner to lead to group and specific and general factors.

Olkin: Was there a controversy in England on factor analysis in the same way there was a controversy here?

Bartlett: There was. It was partly connected with how real the factors were. My own attitude was in sympathy with the Thomson outlook, that they were not real in any sense that you could, as it were, isolate them. They were merely statistical concepts of some kind which, nevertheless, were very useful.

I even wrote a short note later in which I indicated that if, for example, your ability was in any way genetically inherited and you had similar individuals marrying, as in the case of a husband and wife both with more than average intelligence, that such correlations, which you could pick up as having been measured, strengthened these factors which appeared in the Godfrey Thomson theory. That is, correlations would develop which, as it were, strengthened the concept of the factors that you had.

Olkin: That's an interesting point. I note one of your papers has the tantalizing title "Factor analysis in psychology as a statistician sees it [*Nordisk Psykologi*, Monograph Series 3, 1953]." I have not seen that paper. Is it related to what we're discussing?

Bartlett: More or less what we are saying. That paper was given at a conference many years later and was a survey paper. I was taking the opportunity to survey the point that factor analysis had reached and trying to clarify the way in which the statistician would regard it, as well as the statistical way that I had been previously telling you about.

I think one original point in the paper, as far as it went, was that I developed the idea of the factors being perhaps statistical artifacts by introducing a nonlinear factor and pointing out that this would lead to a sort of new factor which you might separate out from the others, but it would merely be a nonlinear part of the factor that you put in your model. That, so to speak, strengthened the fact that it was a purely statistical analysis.

HOMOGENEITY OF VARIANCES

Olkin: Perhaps we can move on to some of the other areas. Many elementary statistics textbooks

refer to the Bartlett test for the homogeneity of variances which was contained in your 1934 paper, "The problem in statistics of testing several variances" [*Proc. Cambridge Philos. Soc.* **30** (1934) 164–169]. Did that arise from a practical problem?

Bartlett: You may be referring to a note in the *Proceedings of the Cambridge Philosophical Society* where I talked about the problem of testing several variances. My actual recommended procedure arose later in 1937 when I found a better criterion [Properties of sufficiency and statistical tests, *Proc. Roy. Soc. London Ser. A* **160** 268–282].

I suppose the problem of testing variances was in the air at the time because there wasn't a very satisfactory way of doing it, at least not a very convenient way. Of course it also was linked with the work that Jerzy Neyman and Egon Pearson were doing on the theory of tests generally. In addition to their exact theory, for example, in relation to the power function of tests, they had introduced the more empirical criterion of the generalized likelihood ratio as a good criterion to use.

They would have considered the problem of testing variances and testing means in relation to this criterion, getting out rather complicated expressions for which they wouldn't know the distribution. The result of my first contribution, the one in the Cambridge Philosophical Society, was not, in my opinion, very satisfactory; but the result in the later contribution in the Royal Society was. Firstly, that having considered the property of sufficiency and conditioning on sufficient statistics for unwanted parameters, you could simplify the mathematical apparatus. For example in considering variances, you would eliminate the means and other unwanted constants and simply consider your likelihood criterion in relation to what you had left.

This made it very simple, and if you bore in mind that, especially due to earlier work by Sam Wilks, the log of the likelihood criterion was distributed as χ^2 quite generally (the particular case being the well-known Karl Pearson goodness-of-fitness test), then having considered the logarithm, I was able to obtain the distribution, or at least the moments of that and see that you could get a simple adjusting factor. This made the whole thing very manageable from a practical point of view, and I think this is how the test caught on a bit and was referred to by my name.

I believe Egon Pearson was a little aggrieved at this, saying, "Well, this test is really the likelihood criterion which we proposed." But perhaps he didn't give sufficient weight to the fact that one had reduced it to a much simpler and more manageable sampling form which people would use quite readily. This was an important aspect. In fact, the adjusting factor which I suggested was taken up, and it's now called the Bartlett Adjustment Factor. I used it in connection with other regression analyses in my 1938 paper and got the adjustment factors there; and that also became quite important.

Even later after the war when someone actually worked out the complete distribution of the likelihood ratio criterion in that second problem, the tabulation of it which took place in *Biometrika* was assisted by bearing in mind the adjustment factor, to reduce the tabulation to as simple a form as possible.

Olkin: Could we go back a moment to canonical correlations? The paper I have in mind is Hotelling's 1936 *Biometrika* paper.

Bartlett: This was written after my 1934 paper in the *Proceedings of the Cambridge Philosophical Society* [The vector representation of a sample, **30** 327–340], in which I discussed multivariate analysis as an extension of one variable. To my knowledge, canonical correlations didn't exist then. Hotelling's generalization of the t test existed, and I referred to it, but canonical correlations didn't arise until 1936.

That was my first acquaintance with canonical correlations; and I used Hotelling's 1936 paper [Relations between two sets of variates, *Biometrika* **28** (1936) 321–377] for part of the 1938 paper to develop the theory of multiple, multivariate regression, including canonical reduction, as a wider approach than Hotelling's correlation approach, which I think is one important aspect of that 1938 paper.

Another contribution in that paper that I was pleased about was this adjustment factor. Let me go back a moment.

Hsu also developed the wider regression approach to canonical analysis in a paper during the war without referring to my 1938 paper [Further aspects of the theory of multiple regression, *Proc. Cambridge Philos. Soc.* **34** (1938) 33–40]. I don't think he could have been aware of it and I wasn't aware of Hsu's paper until I reviewed his collected works only a year or so ago.

FISHER AND GEOMETRY

Olkin: If I may, I would like to change direction for a moment from the detailed papers. You mentioned Neyman and Pearson and that your papers appeared just after their early work, so there must have been a lot of ferment in England during the early 1930s with the Neyman and Pearson results, and others such as yourself and Sam Wilks. Do you have recollections of that particular period? For example, was there excitement when Neyman-Pearson announced the results?

Bartlett: You mentioned Sam Wilks. Wilks came to Cambridge in 1933 when I was finishing my fourth

year at Cambridge with Jack Wishart. Wilks had just done some work on the generalized likelihood ratio criterion and its distribution, if I remember correctly. He was very keen on the analytical approach, and there was some discussion about how rigorous Fisher's geometrical methods were. Wilks wanted to derive these results and those for the analysis of variance analytically to put them on what he thought of as a better basis. Fisher was very annoyed at this and opposed its publication, and there was quite a row between Fisher and Wilks at the time.

To be honest, I sympathized with Fisher at this point because I was very impressed by Fisher's geometrical approach. This raises philosophical questions of mathematical rigor but I was impressed with Fisher's approach, which was very powerful and enabled one to see results very easily; although it was nice to have an analytical derivation as well, which usually came later.

The geometrical approach was very valuable in getting these results in the first instance. This may have prompted me to write that 1934 paper, in which I merely put out fairly simply the analysis of variance theory in, so to speak, geometrical or vector terms.

The one variable case was the one Wilks was concerned about at the time. Perhaps we can come back to the geometrical and analytical approaches when I get on to the canonical correlation distribution, because it arises there.

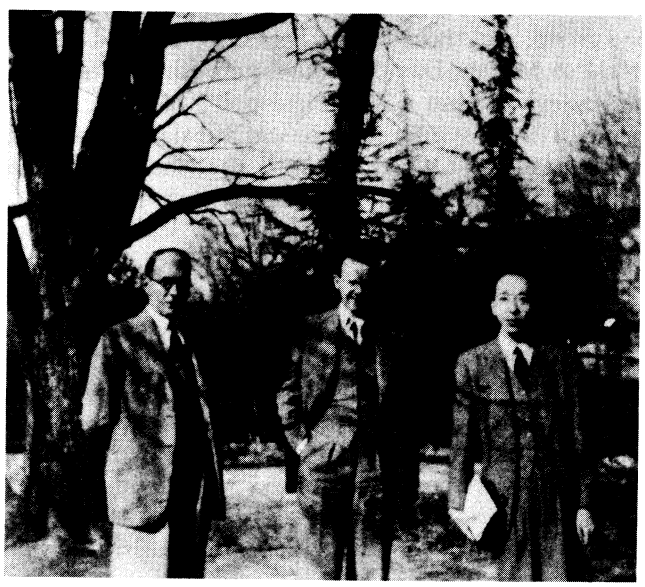
Olkin: This points out an important historical point, namely, that your 1934 paper was really a coordinate-free approach, and certainly was one of the earliest papers with this approach in the statistical literature. But please continue with the Neyman story.

Bartlett: The vector representation paper I suppose I may have started at Cambridge but finished at University College London where I went. And it was at University College where I had been appointed to an assistant lectureship by Egon Pearson that I met Neyman, who was there also and was lecturing on his theory, with Pearson, of testing hypotheses.

I was impressed with some of this work, but I was still strongly influenced by Fisher's approach; and some of my subsequent papers about that time are still very much in the Fisher tradition about studying properties of sufficiency, about his concept of statistical information, and so on.

I think my attitude generally was that Neyman and Pearson had brought forward some important ideas in the theory of testing hypotheses. For example, the power function which hadn't been considered by Fisher at all, and in a more empirical way, they suggested the generalized likelihood ratio criterion as a good test to use when you had several parameters in mind. But I was being rather practically minded. I was a little suspicious of making too much of a mathematical meal out of it, which I think some of us felt that perhaps Neyman was inclined to do at that time. I don't know if I need to say more than that.

When I had to teach statistics, obviously, I was very concerned to bring in the Neyman-Pearson theory, but I would not have stretched it out to an enormous length in the whole course because it might place an unnecessary emphasis on that aspect.



Hsu with M. S. Bartlett (left) and H. Cramér in Chapel Hill, December 1946. From Applied Probability Trust.

THE UNITED STATES CONNECTION

Olkin: While we're on the subject of some of the players in the statistical profession at that time, I'd like to mention a few of the people who were well known in the United States, and perhaps you could comment on some of them. Harold Hotelling was at Chapel Hill, William Cochran was at Raleigh, Abraham Wald and Jack Wolfowitz were at Columbia, Neyman was at Berkeley, Snedecor was at Iowa State and Wilks was at Princeton. What are your recollections of relations with them?

Bartlett: You mentioned several people there. Bill Cochran I knew, of course, in England, and he was a friend of mine. He came to Cambridge a year after I did. I had left Cambridge by the time he went there, but we were in contact. As a matter of interest regarding my own life story, I mention in my autobiographical note that I was approached and offered a post at Iowa State University at Ames with Snedecor. When I turned it down, Bill Cochran was

the one to accept it. So he came to America and went to Ames.

Hotelling, of course, I knew by name in connection with canonical correlations before the war, but I didn't know him personally until after the war when he invited me to Chapel Hill.

Olkin: I think Hotelling was in England in the early 1930s.

Bartlett: Yes, that's right. I think he saw quite a bit of Fisher at that time, but I wouldn't have known him then. It was not until after the war that I was invited by Hotelling to visit Chapel Hill, partly with a view to seeing if I might wish to stay there, which I decided not to do. But I certainly was very pleased to have the opportunity of going there and joining up with other people of Chapel Hill at the time.

These included Herbert Robbins, P. L. Hsu and, of course, Bill Cochran, who was over at Raleigh. I can't remember meeting Hsu before then, although I may have because he was involved with important work with Fisher, I think just before the war. His work, published in the *Annals of Eugenics*, was on the distribution of canonical correlations in the null case, and involved a very difficult Jacobian.

Olkin: That was, I think 1939.

Bartlett: Yes. I think I was just aware of that, before the war broke out and then I disappeared into war work. Wilks, of course, I met at Cambridge, as I mentioned, and knew him quite well because he visited England occasionally and came to see us. I remember his coming to see me when I was still temporarily living at my parents' place in Netherwood Road, London. He was impressed by the Victorian character of the house and its interior and commented on this at the time. Then, when I went to Chapel Hill after the war, I went to Princeton to give one or two talks and met Wilks again then. We kept in good contact with each other.

Olkin: How about the Columbia group, the Wald and Wolfowitz connections?

Bartlett: I had met Wald, but I didn't know Wald and Wolfowitz particularly. What happened with Abraham Wald was that his important work on sequential analysis came out as a classified document during the war, and some of us who were in war work were thus able to see it before it was made public, in my country, at any rate.

In fact, I wrote a little paper at the time on the large sample theory of sequential tests because, before that note of mine, although the probability theory was general, the actual sampling theory as to how large a sample you needed, would have to be worked out afresh for each problem. What I did was merely to show that in a large sample sense, all these problems were the same. If you took the log likelihood and regarded that as a random walk, you had a random walk between two absorbing barriers, and this provided the large sample theory.

THE WAR PERIOD

Olkin: I'm interested in your comment about the classified work. There were two groups of statisticians doing war work, both called the Statistical Research Group, one at Columbia and one at Princeton; and most of the well-known statisticians and mathematicians, Girshick, Hotelling, Mosteller, Savage, Tukey, Wald, Wolfowitz, and many others were all involved in this. Was there a comparable group of statisticians in England during World War II?

Bartlett: There were one or two groups, but they weren't so well defined. There was a kind of quality control group to which George Barnard and Robin Plackett and others belonged. That was a fairly cohesive group, but I didn't have much to do with that. And then at the Ordnance Board, Egon Pearson took his department, I think more or less en bloc, to work on problems of effectiveness of shell weapons against aircraft, and so on, and also in connection with trials; and that would be a fairly cohesive group.

There were then variously isolated groups working in what you might call operational research areas. I was involved, again, in a different group that worked on rocket development, and I was responsible for studying the theoretical effectiveness of rocket weapons. I had a link with Egon Pearson. He was doing similar analyses for shells, and in due course, I got Frank Anscombe involved in this.

The rocket establishment as a whole included a group of mathematicians who were working in Wales under Professor Rosenhead, who was put in charge of the group; and these mathematicians included people like Pat Moran, David Kendall and others who were less relevant from a statistical point of view. David Kendall first got interested in statistical work through this contact, and it was not until after the war that he realized that this could be a useful career to follow.

My own position tended to migrate. The head of the group was Sir William Cook, Fellow of the Royal Society, and he was in charge, in effect, of Rosenhead and of me and other people. Some of us moved up to London to work in a sort of headquarters group there, but after a while, near the end of the war, we moved back to Wales. This was partly perhaps because they started thinking about what was going to happen after the war. It was then that I worked with David Kendall, who was there with Rosenhead. So that's how our connection started and what happened during the war period.

It may be a matter of interest that after the war was over I went back to Cambridge as quickly as I could. I was a lecturer there. The president of the Ordnance

Board wrote a letter to me saying that Egon Pearson was going back to University College, so would I take over his work. But I wasn't interested, and I didn't accept the offer.

THE INTERNATIONAL SCENE: CRAMÉR AND KOLMOGOROV

Olkin: The war period was important from a statistical point of view in that it was the forerunner of a very tremendous surge in the development of statistics and statisticians.

Bartlett: It was a great impetus. In fact, they started training mathematicians as statisticians for the war, and after the war, the universities realized this and created statistical posts. I suppose this is how they created the post I went to in Manchester. A chair of mathematical statistics was created; this may have been the first such chair in England. For example, Maurice Kendall was a professor of statistics at the London School of Economics, but it was not necessarily thought of in terms of mathematical statistics.

Olkin: On a more international level, do you have reminiscences about Harald Cramér in Sweden and A. N. Kolmogorov in the Soviet Union?

Bartlett: I don't have a very clear recollection of how I first became acquainted with Cramér. I think I must have first met him at that International Statistical Institute meeting in Switzerland just before the war. It doesn't seem quite right because I would have thought I may have corresponded with him before then when he published his Cambridge Tract *Random Variables and Probability Distributions.*

This was about the first tract, in England at any rate, which put probability on a firm mathematical basis. I was impressed by this book, although I found it rather difficult mathematically because I don't regard myself as a very strong pure mathematician. Nevertheless, I may have corresponded with him about one or two points then.

Olkin: That would have been about 1937.

Bartlett: Yes, that's right. It wouldn't have been until after the war that I went to Sweden and renewed my acquaintance personally with Cramér. However, he may have come to London because he knew Egon Pearson quite well, so I may have seen him again then.

You mentioned Kolmogorov. I knew of his work through my friendship with J. E. Moyal during the war. Moyal was familiar with continental work in stochastic processes and was able to tell me more about the literature, including the Russian work by Kolmogorov, which I wasn't familiar with at all.

I had contact with Cramér and just before the war there was the work on time series by Herman Wold. He must have sent me a copy, but I can't remember whether I studied it particularly before the war. I certainly referred to it in a paper I wrote for a Statistical Society Symposium just after the war.

I didn't meet Kolmogorov personally until I went to Moscow in 1956 to one of their mathematical congresses. They invited me to this congress, I suppose, because I had just published my book on stochastic processes, a book originally intended to be a joint work with Moyal. This cooperation fell through and I published my side of it, which I think was unfortunate, but there it was, and the Russians were very quick at translating this book into Russian. When I went to Russia, I was very pleased to meet Kolmogorov.

Olkin: Did you meet Linnik at that time?

Bartlett: Yes, I think so, although I remember Linnik much more a bit later. At any rate, I very much relished the opportunity of going to Russia to meet these people and, in particular, I was very impressed with Kolmogorov. I couldn't talk to him very well. I didn't know any Russian, and he didn't know any English. He knew German, but my German was fragmentary. So we struggled, and then had an interpreter so that we got on a bit better.

I was certainly impressed with his papers. He gave about three leading papers to different sessions of this mathematical congress: one in probability, one in functional analysis and one I think in topology. He was a very remarkable figure and still is. (Editor's note: A. N. Kolmogorov died in 1987 at the age of 84. For an obituary see *IMS Bull.* **16** (1987) 324–325.)

COMING BACK TO FISHER ...

Olkin: Speaking of remarkable figures, it's hard to continue this conversation without bringing up Fisher, who sort of dominated the field for many, many years. Would you like to discuss your association with him?

Bartlett: I'll try. Jack Wishart gave the first course in statistics at Cambridge. Maurice Kendall, incidentally, was before me at Cambridge but didn't attend any course in statistics at Cambridge, and it's rather remarkable the way he developed because of that.

Coming back to Fisher, Wishart expounded Fisher's ideas in the course because he came from Rothamsted and from being with Fisher. I was very impressed with Fisher's work; in particular, with his philosophy of using statistics rather than just treating it as some academic subject.

He was always very strong on this, so much so that when I went to University College and was asked to teach statistics, I felt embarrassed and left after a year to go to the Imperial Chemical Industries (ICI) Research Station of Jealott's Hill, where I was very much an apprentice. Much of all this time, I was certainly

studying Fisher and impressed with his work. He was, as you probably know, very warmhearted to those he felt were on his side. It was only if you, as it were, dared to disagree with him that you found it got rather more difficult!

I suppose my contact with Fisher was very much a fluctuating time series because at one stage, I might be, so to speak, in his good books; and another time, I'd be in his bad books because I was querying some point he put forward.

I was impressed, as I said, with his geometrical method which I used to good advantage later. And I was also sympathetic, I suppose, to his rather heuristic approach to mathematics because it sort of fitted in with my own abilities in mathematics and contrasted with what you might call the more rigorous continental outlook, which would include the work by Neyman and also people like Cramér, Kolmogorov and so on.

My own attitude here is that mathematics and the use of mathematics has a very wide range, and it takes all kinds to contribute to the progress of the subject.

Olkin: Now, I believe that Fisher rarely published a joint paper; at least that's my recollection. What was the modus operandi during periods of excitement when ideas were thrown out? Did Fisher not like to collaborate?

Bartlett: It's difficult to know. I was never with Fisher in the sense that Jack Wishart or Oscar Irwin or Frank Yates was with Fisher; so my contact with him was somewhat more indirect. I can't tell you in particular detail how his ideas went, but I suppose an idea would be mooted in his lab and developed, as for example, finding an exact test for the contingency table.

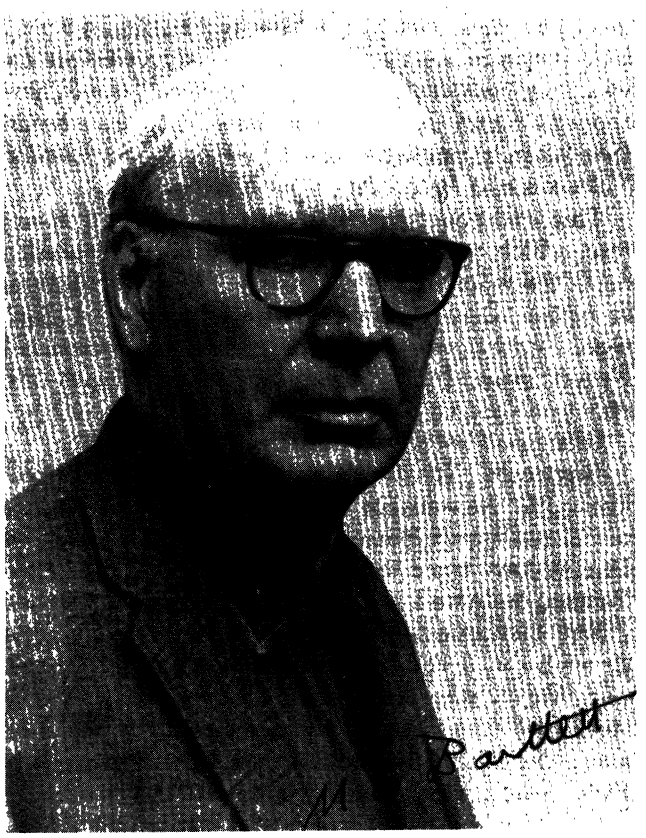
It's not quite clear who was responsible for that, whether it was Fisher himself or Yates or Irwin, but they were all together, and it was natural for them to discuss this problem. Then again, you take the distribution of the canonical correlation and the Hsu contribution that I mentioned. Hsu was at University College, apparently, at the time, and he would be in contact with Egon Pearson and with Fisher. Fisher must have raised the question of the distribution, and although they didn't publish a joint paper, they published two papers simultaneously in the Annals, so there must have been some sort of fusion of ideas there. I don't think I can really say much more on that aspect because I wasn't ever working with Fisher.

Olkin: Perhaps we could just comment on one or two other people, and then move back to the scientific part, the papers. There was Egon Pearson, who also was a leader for many years, and J. B. S. Haldane, who was an interesting individual.

Bartlett: Very interesting, yes. Well, first let me take it chronologically. I had met Fisher at Rothamsted by a kind of pilgrimage there at the instigation of Jack Wishart, and Udny Yule I met at Cambridge because although retired, he was still giving lectures which I attended. I was very impressed with him as a person and as a statistician.

Egon Pearson I met, I suppose, at Wishart's recommendation. I went to his department as assistant lecturer, and I got to know him a little bit then. He was rather a somewhat diffident bachelor at the time, and of course, as I mentioned, I was only there a year when I went to join the Imperial Chemical Industries. So it was later that I got to know Pearson better.

I met Haldane at University College. He was professor of genetics to start with and later the Weldon Professor. At any rate, he used to visit Fisher quite a lot in Fisher's laboratory, which was just upstairs from the statistical department. I met him simply because he was working on the theory of inbreeding. He had some matrix and characteristic-root problems to be solved, and he asked me to help with them. I found him very generous because we, in effect, wrote two papers together. He was the senior author by a big margin, but because my name came alphabetically first, he put my name in first, which I thought was kind of him.



Maurice Bartlett in 1975; photo taken for Festschrift "Perspectives in Probability and Statistics." From Applied Probability Trust.

We maintained this contact when I went to Jealott's Hill. He was supposed to be writing a book on mathematical genetics which he never finished. I was a bit concerned about his theory of inbreeding, that it was a kind of expectation theory. I tried to develop, somewhat fragmentarily, the stochastic aspects.

He was one of the editors of *The Journal of Genetics*, perhaps the only editor. He accepted a paper from me on what I called deviations from expected frequencies in the theory of inbreeding, which was, I thought, of some interest. It was not so much the results I achieved, but more the fact that I was raising the problem in the first place.

We kept in touch. I remember that long afterward when I had been appointed to the chair in biomathematics in Oxford, right at the end of my university career, that I visited India. Haldane had gone to India by then, and he and his wife took me in tow. I was visiting C. R. Rao at the Indian Statistical Institute, but I went to see the Haldanes at their house. We went and had a meal of curry in Calcutta, which was a bit nerve-wracking. I can't remember whether Haldane came, but his wife took me to the local zoo. I had a rather nice photograph of Mrs. Haldane holding one of the chimpanzees. I wish I still had it, but unfortunately, I presented it to her when I saw her again in London.

CONCERNS WITH TIME SERIES

Olkin: I do want to cover the second big area of research in your career, namely, stochastic processes, time series and statistical inference. Your work in time series actually started in the '30s. I recall that you published some papers in the early '30s [Some aspects of the time-correlation problem in regard to tests of significance, *J. Roy. Statist. Soc.* **98** (1935) 536–543]. What was the catalyst that generated this work?

Bartlett: Nothing much happened before the war. I had become aware of the problem of time series by meeting Udny Yule, although Yule had not really given me all the information about his own published work. I wish he had. I don't think he gave me the reference to his Royal Society paper on auto-regressive models, which I would like to have known about. At least I don't recollect that he did, but what he did do when I first met him is suggest a subject for the Royal Statistical Society competition for young statisticians. I was rather keen on competitions and I tried for it without success, but it may be of some interest. I wrote an essay called "Mortality and the Trade Cycle," and the interest in this from my point of view was, first, that I realized how difficult it was to do any sensible statistical work with time series. The second point of some interest perhaps is that I fitted orthogonal polynomials to the trends, to study the correlation of residuals, and I used Aitken's methods, which were very attractive.

They were sort of a rival to Fisher's, but never caught on because Fisher published his in his book, and you could use the figures in the Fisher and Yates *Tables*; but Aitken's derivation was much neater and simpler and very attractive.

However, I didn't get the prize, which didn't surprise me because I was struggling with time series in the essay, but it made me realize the problem. I published a paper before the war on some aspects of the time-correlation problem, but it wasn't very important. It merely indicates that I was struggling with them. It wasn't until after the war when Maurice Kendall became interested in time series and was putting out empirical series to show you how difficult they are to deal with, and so on, that I made progress. At the same time I was becoming aware of the continental work, the work of Herman Wold, the book I mentioned by him, and the work by Khintchine in Russia on stationary time series that gave a framework to build on for any practical methods one developed. This I tried to do in the paper to the Royal Statistical Society in 1946 [On the theoretical specification and sampling properties of autocorrelated time-series, *J. Roy. Statist. Soc. Suppl.* **8** 27–41], just after the war, on time series. If I could just interrupt this discussion of my work on stochastic processes, which started from that period, there was a paper on the general canonical correlation distribution, which I'd like to mention because it was quite an outstanding problem at the time.

One or two people, for example, Ted Anderson, got the distribution in the case of one nonzero canonical correlation. The general case was more difficult, and I wasn't able to solve it completely in closed form. I did see a way of developing a solution in the general case, which I published in *The Annals of Mathematical Statistics* [The general canonical correlation distribution, **18** (1947) 1–17]. Why I'm interested in this is because before the war, Oscar Irwin had said to me, "Can you follow Fisher's geometric derivation of the multiple correlation distribution?" because Irwin hadn't been able to follow it. I wasn't able to follow it at that time, and when I lectured at Cambridge in statistics, my first discourse on the multiple correlation coefficient distribution was done via characteristic functions. I think it was along the lines that Sam Wilks had covered it; but I wasn't very happy about this.

Incidentally, Maurice Kendall had written his general book on statistics by the time I published my paper on the general canonical distribution, after the

war. But he gets the argument about the multiple correlation distribution wrong, I'm afraid, because it's rather subtle.

After the war I went back to Fisher's paper and read what Fisher was doing. He had a very cunning method of getting the general distribution out of the null case by taking out a factor involving the sufficient statistics for the non-null parameters; and it was a very beautiful way of combining these two things. Once I saw what he was doing, I saw in principle that the method worked for the general canonical correlation distribution, and that was the method I tried to use.

Olkin: That's an interesting point, and new to me.

Bartlett: I think it's interesting because it still involves the geometrical approach and a point I hadn't resolved before the war, but which came up again after the war.

I was always surprised—coming back to Fisher—that his geometric intuition seemed to desert him, because he started making mistakes, which I tried to point out, and I realized they were mistakes by looking at the problem geometrically. But Fisher was making mistakes because he wasn't looking at the problem geometrically any longer. He was using some sort of argument by analogy or whatever. That is a curious point.

Coming back to time series, that was the first, I suppose, major work that I did in stochastic processes. One of the further developments of time series came with the problem of smoothing the periodogram.

Olkin: This was your paper published in 1950 [Periodogram analyses and continuous spectra, *Biometrika* **37** 1–16].

Bartlett: This is interesting because it paralleled work which John Tukey did in the States—in a Bell Telephone publication, which I became aware of in due course because he pointed it out to me. But it was rather independent work.

I think my first reference to my proposals for periodograms was in a letter to *Nature* [Smoothing periodograms from time series with continuous spectra, **161** (1948) 686–687], published a bit earlier than the *Biometrika* paper. This letter, I think, has been published somewhere in a collection in America. I know I was asked permission if they could use it.

The *Biometrika* paper was of some interest because I thought this was a general problem which was of some value, so I asked Fisher whether he would communicate it to the Royal Society, but after some length of time, he wrote back and said that he had taken advice and felt that it wasn't of sufficient general interest and that a journal like *Biometrika* might be more appropriate. Well, whether or not it should have gone to *Biometrika* I felt his comment about it not being of general interest was not quite correct.

I was working with Moyal on stochastic processes, and, as I have said, we had an idea of writing a book, but this never materialized.

Olkin: Did the stochastic process part also come from practical problems, or was there just a more general evolution that led to this?

Bartlett: I think it was general evolution, and came slowly. I was always aware for the need of dealing with statistical situations which were changing in time. I was rather struggling with this before the war. I suppose it arose out of the time series work.

As I have said, Moyal's knowledge helped me in this. I was aware of Markov chains, and Wold was already working on time series. But I wasn't aware of Khintchine's work until toward the end of the war. Another interest that Moyal and I had was an acceptable probability theory for quantum mechanics, because that was a branch of science where probability was used to deal with changes in time, but without any apparent connections with any other branch.

And so Moyal tried to study stochastic processes in relation to quantum theory, and this, again, was part of the general outlook of trying to get a whole theory of stochastic processes as a theory of statistical change. This was rather a wide subject, and certainly I wasn't looking at it from a very abstract point of view. I wanted it more from the point of view of a technique for various fields, and that is what I concentrated on in my part of the book, which I published under my name.

My papers at that time really arose out of developing the work for the book; for example, the theory of recurrence relations. Some of these relations use the probability generating function approach for solving problems with branching processes. This all arose from studying stochastic processes and how you would attempt to deal with problems, not only in physics, which was Moyal's interest, but in biology, which was my interest and that of David Kendall, who was particularly interested, as I was, in epidemiological theory.

My 1967 *Biometrika* paper [Some remarks on the analysis of time series, **54** 25–38] was a review-type paper, arising out of a visit to Chicago where I gave some lectures on time series. I would think that my own contribution, apart from the periodogram work I mentioned, arises partly in problems of inference. Ulf Grenander had published his paper on statistical inference for stochastic processes, in which he developed his definition of the likelihood function in terms of a ratio, for example, if you were dealing with continuous time processes. This, so to speak, formulated the mathematical scene rather nicely, but particular aspects of the stochastic process inference problem were of course still hardly covered.

One specific problem, for example, was discussed in the paper I wrote in the *Proceedings of the Cambridge Philosophical Society* [The frequency goodness of fit test for probability chains, **47** (1950) 86–95] on inference for probability chains, which included a generalization of the notion of the likelihood function for simple Markov chains. This came out nicely, at least in its asymptotic theory.

Grenander and Rosenblatt in their book on time series had raised a question of having a confidence region or interval for a whole cumulative spectral function, if you were trying to estimate the density. They weren't, in my opinion, doing it in the most simple manner because they didn't condition on the total sum of squares.

So it was an open-ended random walk, which both complicated the problem and also made it more difficult to handle. If you divided by the total sum of squares and made it a kind of closed random walk, that is, a Brownian bridge, the theory was much simpler. You could use the Kolmogorov theory of cumulative distribution functions and the distribution was also more robust because by fixing the total sum of squares, you made it more robust against nonnormality. So although my modification was somewhat incidental, it seemed to be quite a useful piece of work in the time series area.

LECTURES ON STOCHASTIC PROCESSES

Olkin: You mentioned your joint work with David Kendall in physics and biology, "On the use of the characteristic functional in the analyses of some stochastic processes occurring in physics and biology" [*Proc. Cambridge Philos. Soc.* **47** (1951) 65–76]. How did this collaboration begin?

Bartlett: The paper was concerned with actual problems, but it was also a general problem. When I was at Chapel Hill, I gave lectures on stochastic processes, and unlike the first course of lectures I gave in Cambridge in which I divided the subject into time series and diffusion processes, at Chapel Hill I was interested by then in population processes, starting from Feller's work, and was able to solve the problem of the birth and death process not knowing that Palm had already solved it.

But also, I was interested in the more complicated population problem where you had an age distribution, and I didn't quite know how to handle that because I could only think of dividing the population up into discrete age groups, which of course is what would be done in practice if you were dealing with it.

But theoretically age is continuous and so I only did it approximately. David Kendall took this problem and introduced the characteristic functional which was a nice theoretical innovation in this context, although I think the first to have actually used the characteristic functional may have been Lucien Le Cam—I'm not sure about that. In this joint paper that you mentioned, we were developing the characteristic functional method actually to solve and get the characteristic functional in population and growth problems.

I think it was the first time one explicitly got out solutions for the characteristic functional, so it was of interest for that.

Olkin: In your autobiography, in the volume edited by Gani, you mentioned M'Kendrick in connection with his biological work in stochastic processes. In my own work I uncovered a bivariate Poisson distribution that M'Kendrick developed in the 1920s. He had developed a very nice Poisson distribution when he was in India and studying the wounds of soldiers from two different sources. (Editor's note: Col. A. G. M'Kendrick was a physician and statistician at the Laboratory of the Royal College of Physicians, Edinburgh. His work in the 1920s dealt with stochastic process models in medicine.)

Bartlett: I only wish I had known the work of M'Kendrick earlier because when I first wrote and lectured on stochastic processes, I wasn't aware of his work. The first edition or so of my book on stochastic processes doesn't mention M'Kendrick. I think it was

Maurice Bartlett in 1982; photo taken from volume "The Making of Statisticians." From Springer-Verlag.

David Kendall first of all who noted that M'Kendrick had published a paper on epidemic theory, which in effect was doing what I had done with my epidemic equation, but back in 1926.

I mean he actually got the U-shaped distribution for a closed population. I'm not sure whether he actually realized it, but anyway, he had the solution for it—the U-shaped distribution you get for the size of an epidemic (above the threshold) in a closed population—which was rather a remarkable result. Then we discovered that even earlier in 1914 he published a paper in the London Mathematical Society in which he derived things like the negative binomial distribution as a contagion process; this work was also in stochastic processes, and that was quite remarkable.

I can't give you details, but I remember that when I attended a study group with bacteriologists there was some equation that was mentioned, and somebody mentioned that this had been given by M'Kendrick back in 1911. This was a deterministic equation but still of interest because nobody was aware that he had done this at that time. It's incredible what he did do.

Olkin: Are there other topics that you would like to talk about, Maurice, either papers, people, times?

Bartlett: It may be clear that I have a wide interest in statistics, and this, of course, has its advantages and disadvantages because one can be a bit fragmentary on particular topics, but at the same time, I think it's given me my interest in statistics. Some of my fascination with statistics is that it's such a wide subject and to some extent it trains one to try to see problems as a whole. For example, if you're dealing with a problem in operations research, it's no good just dealing with a little bit of it as maybe a mathematician or an economist might do. You have got to try to cope with it as a whole. This, I think is fascinating.

I have been interested in that as part of the general subject of probability, as such. We haven't mentioned, for example, that back in 1933 I came across the controversy between Fisher and Jeffreys. They were writing in the Royal Society and arguing about whether you should use Fisher's methods or the Bayesian method that Jeffreys was advocating.

So I wrote a paper back in '33 called "Probability and chance in the theory of statistics," [*Proc. Roy. Statist. Soc., Ser. A* **141** (1933) 518–534], in which I tried to distinguish subjective probability from the notion of chance or statistical probability. I was only following points that F. P. Ramsey had already made. I discovered a book in the Union Library by Ramsey published posthumously, because he died rather young, but it quite impressed me. It was a very clear exposition of probability, but at the same time admitting the fact that chance was, if you like, a separate concept and this I think impressed me because I was very taken with distinguishing between these two concepts. It interested me because it wasn't always accepted by others who were working in this field, so I thought it worth emphasizing.

SOME REFLECTIONS

Olkin: Where do you think the field of statistics is going, and do you have any predictions?

Bartlett: That's difficult. I think it's very much in a state of flux, and as I've been retired for many years now, I'm not sure I'm the best person to try to say this.

There have been so many developments which are of great importance. The use of the computer, of course, is very important and very much changed the nature of statistics. I think the theory of stochastic processes has very much changed the attitude to statistics because one does see the whole question of development in time as a problem in its own right; but of course it has raised as many problems as we have solved, so it's still a very wide field. I think we have innumerable problems to face.

I always see it as a growing subject. I don't really see it as a closed academic subject.

My own inclination is to work at, say, a research institute as I did when I was at Jealott's Hill; or in effect what I was working on in the war, which is on problems where one then uses any relevant methods and techniques rather than think of them as a university subject where I'm afraid there is a temptation to narrow one's outlook. For example, if statistics is in a mathematics department, you think all of it is a branch of mathematics; or if it's in the operations research department, then you may think of it as something else. The people working in that particular university department won't get much credit really if they try to see it in a wider context.

I think there has been rather better sympathy for people trying to be broader in the subject in England than perhaps here. I don't know. In a research institute, you don't get that kind of restriction.

Olkin: Do you think there will be more cognizance of statistics because of its widening applications in substantive fields?

Bartlett: Well, I hope so. I think statistics has a definite problem in being recognized for its importance. There is always the danger that its value gets overlooked. We mentioned before that there was the impetus of the need for statistics during the war, which got it going in England, and this created the recognition in the universities of the need for more statistics. That impetus has dropped a little, I think, at any rate in England. You may be feeling it here, too. In England the restriction on university grants has been general,

and has been complained about especially by the younger universities. This restriction applies in particular to statistics.

There may be other problems associated with the recognition of statistics as a subject because of its overlap. The fact that it overlaps with mathematics and overlaps with operations research and computer science means that it has to fight, so to speak, to keep its end up with these other disciplines which may be developed in their own right.

The difficulty, as I said, looking at it in a wider context, is that there is strictly no subject of statistics or operations research. It is all part of research, but you have to try to divide it up. Since statistics is of general application and sees problems as a whole, it's a little difficult to define it within the university department.

Olkin: I was quite intrigued in the following sentence from your autobiography: "Retrospectively, I would say that my time at Jealott's Hill was not only the happiest period but also the most creative."

Bartlett: I think that's probably true.

Olkin: It is interesting in that we would tend to think of someone at Cambridge, University College, Oxford, Manchester and so on—that those would have stood out as more attractive than being in industry. But somehow you were able to work on varied problems in a creative way.

Bartlett: Well, it was a very good atmosphere. It was a similar atmosphere to the one, I think, that Fisher had at Rothamsted, in the sense that you had scientists of different disciplines—chemists, biologists and so on. We'd meet together every week to discuss the problems that were involved, and there would be cooperation. At the same time you were free to develop your own ideas, and the director, a man called Page, was very farseeing in not restricting people very much. So I was able to correspond with Haldane, with Fisher, and so on, without any particular restrictions.

Unfortunately, someone at head office made a dictum, and half the scientists were sacked in ICI, including our director at Jealott's Hill, and I was transferred to work in London. So I had to leave Jealott's Hill, and was very sorry to do so.

Olkin: You keep saying you are retired, but I see that you are still publishing. What do you plan to do in the next decade?

Bartlett: Well, that's a difficult question. I think it might be fair to say that my trend is perhaps downward, but if I could sum up my reaction to research, it is that in a sense I'm a creature of my environment. I don't perhaps go out quite so much to find fundamental problems that one solves, so to speak, in a vacuum. I like to have problems grow on one. I don't really like to be in a particular hurry, necessarily, to solve them.

One doesn't want to have an output that has to be so much per year in order to get promotion; this is the way I see research. I suppose this is the way I am myself; I'm rather sensitive to my environment, so at Jealott's Hill, it was a happy environment and very fruitful correspondingly. When I was moved to London in ICI, I was concerned with just straight economic statistics of one sort or another, and there was no research atmosphere. This would have been very deadly in the long run, so I had to move out.

Olkin: Well Maurice, as I review your bibliography, it spans a period of 55 years from your first publication. It covers the fields of fundamental statistical inference, multivariate analysis, stochastic processes, time series, applications and a variety of other areas. It's hard to imagine that anyone could have had a greater impact on the scope of statistics. I'm very pleased that you let the readers share your ideas and reminiscences of some of these papers. Thank you for being with us.

Bartlett: It was very kind of you to say that, and thank you for inviting me.


M. E. Bartlett