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A Conversation with George A. Barnard

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George A. Barnard was born on September 23, 1915, in Walthamstow, Essex, England. He received a B.A. in Mathematics from Cambridge University in 1936, did graduate work in mathematics at Princeton University from 1937 to 1939, and received a D.Sc. from the University of London in 1965 based on his publications. He was a Mathematical Consultant in the Plessey Co. from 1940 to 1942; a Scientific Officer in the Ministry of Supply Advisory Unit from 1942 to 1945; and a faculty member in the Mathematics Department, Imperial College, London, from 1945 to 1966, serving as Lecturer (1945–1947), Reader in Mathematical Statistics (1948–1954) and Professor of Mathematical Statistics (1954–1966). He was Professor of Mathematics at the University of Essex from 1966 to 1975, and Professor of Statistics at the University of Waterloo from 1975 until his retirement in 1981. He served as President of the Royal Statistical Society in 1971–1972, Chairman of the Institute of Statisticians in 1960–1962, President of the Operational Research Society in 1962–1964 and President of the Institute of Mathematics and Its Applications in 1970–1971. He has been awarded Gold Medals from the Royal Statistical Society and the Institute of Mathematics and Its Applications, and in 1987 was named an Honorary Fellow of the Institute of Statisticians. He has received honorary doctorates from the University of Waterloo and the Open University.

The following conversation took place during the Third Valencia International Meeting on Bayesian Statistics in Altea, Spain, in June 1987.

"IF YOU'VE DONE THAT, YOU'LL KNOW STATISTICS"

DeGroot: How did you originally get interested in statistics?

Barnard: Well, I was interested in statistics at school, partly from a philosophical point of view but also from a political point of view. In 1932 I won an exhibition, that is, a junior scholarship, in mathematics to St. John's College, Cambridge, and while waiting to go up to Cambridge in October 1933, I did a survey among the sixth-formers in my school, the senior people in school, to discover what their political opinions were and how they arrived at them.

DeGroot: You were interested in politics even at this point?

Barnard: I was interested in politics at school, yes. I think it's probably past history now, and quite forgotten, but at that time there was a famous resolution of the Oxford Union which said, "This house refuses to fight for king and country." That was a great stir at the time. The same resolution was carried all over England by student groups and school boys, and we did it in my school. The idea was that the 1914–1918 War had been fought and was a bloody slaughter, and it was done in the name of king and country. The view was that our generation might well be prepared to fight for other causes, but not for that one.

DeGroot: Was this already responding to Hitler coming into power?

Barnard: Not really. There was trouble brewing in Germany, it was visible, but Hitler really came to power in 1933, the following year. However, it was associated with that. The feeling grew up that one might have to fight against fascism but one wasn't going to be fighting just for king and country. Statistically what was interesting to me was whether the views of people were influenced by the newspapers and if so, whether they absorbed those views directly from the political end of the paper or from just the atmosphere. So I had a question in the survey as to what their opinions were and what the opinions of the newspaper were, and whether they first read the sports pages or whether they first read the political pages. And I was then struck because I wanted to establish whether the relationship was stronger if they first read the sports pages or weaker. I got in touch with Wilfred Stevens, who later was one of the coauthors of Fisher and Yates' *Tables*. He was working with Fisher at the time and he helped me do the partial correlations.

DeGroot: Where was he and where were you?

George Barnard, 1979.

Barnard: He had just joined Fisher, who had gone to University College London from Rothamsted. By this time, when I was analyzing the data, I had already gone to Cambridge. I had tried to find books in the university library about statistics with no success. The book by H. L. Rietz was the only one that I could find. I used to go to University College to see Wilfred Stevens and that was how I first met Fisher. It must have been around Christmas time, 1933. I said to Stevens that I had tried to read up statistics in the university library and couldn't find anything on it. And he said, "Oh, you'd better come and see the old man." And so he took me in to see Fisher. Fisher took down off the shelf a copy of his *Statistical Methods for Research Workers* and he said, "Do you see that book?" And I nodded. I was very junior. The curious thing was, Fisher had red hair at that time, but I don't recall any red hair. All I remember was that he was wearing boots because that was where I was looking all the time. [Laughs] And he said, "You're a mathematician." And I said, "Well, I hope to become one." And he said, "Well, if you read this book, you'll find there are a lot of statements in it that are made without proof. You're a mathematician, you should be able to prove them for yourself. And if you've done that, you'll know statistics." I think he was just about right.

DeGroot: [Laughs] Did you do that?

Barnard: Eventually, yes. Fisher became President of the Royal Statistical Society in 1952 and the custom is that the President announces four people who will serve as his Vice Presidents. Quite out of the blue, I heard my name announced as being a Vice President. I remember at that date, I said that I reckoned I had just about finished. I had actually just managed to decipher how he'd got the formulae about the components of χ^2. At the back of the book, there's this stuff about splitting χ^2 into components and I had seen a reasonably neat way of doing that. Of course, I didn't see any more of him after 1933 until toward the end of the war. In fact I didn't *see* him then, I only had correspondence with him.

DeGroot: What happened to your survey? Did you succeed in analyzing it?

Barnard: Yes, with a lot of help from Wilfred Stevens. Wilfred was a very careful fellow who, if he took something up, he did it extremely well. He produced lots of pie charts and so on to illustrate the interpretation of the data. I'm sorry to say that I forget what the interpretation was.

"AMERICAN MATHEMATICS WAS TOTALLY DIFFERENT FROM BRITISH MATHEMATICS"

DeGroot: But that led you to continue in statistics?

Barnard: Well, I sort of kept an eye on it, but I really was more interested in philosophy and logic and the foundations of mathematics as an undergraduate. In my first term at Cambridge I went to Wittgenstein's lectures and they impressed me very much. In fact, whenever I had an option, I always took the foundations of mathematics or basic real variables and such things. There was a fair bit going on at that time because Max Newman, who was my tutor at Cambridge, was also tutor to Alan Turing. Turing was at that time in fact working on the decision problem, demonstrating the impossibility of the solution of the decision problem, and I was following that very actively. Newman had really introduced me to that area, and that was what I was keen on.

DeGroot: Were you and Turing contemporaries there?

Barnard: No, he was a year ahead of me.

DeGroot: How did you revive your interest in statistics then?

Barnard: Well, I was still interested, and in my last year at Cambridge I did go to Wishart's course. Wishart gave a course in statistics and I had started going to it, but it was so bad that I gave it up. [Laughs] He never got beyond moments. I mean, he got in a total muddle defining moments about the origin and moments about the mean. And I decided this was not for me, so I gave that up. My special subjects in my final year were in fact logic and topology, and I then went to Princeton in 1937 to carry on with logic.

In fact, Turing had gone to Princeton to work with Alonzo Church and Church's lambda calculus. In fact, Church preceded Turing. I mean, the actual technical proof of the impossibility solution was first published by Church, independently, and more or less simultaneously done by Turing in terms of the theoretical computing machine. They were there, and I went there to work with them. But when I got there I found that American mathematics was totally different from British mathematics. In Britain the mathematical field was dominated by G. H. Hardy and Littlewood—number theory and very deep analysis, highly specialized work. Whereas of course in America it was very abstract, generalized, axiomatic treatments of everything. So I really had to learn it all again. I was to take a Ph.D. at Princeton and I always remember that I had to go over the syllabus of my preliminary exam with Lefschetz. I went over the algebra, and I had to learn vast quantities of algebra because we had virtually no abstract algebra at Cambridge. It just wasn't taught. And function theory was all done in terms of special functions and so on, and I had to know general function theory, and Hilbert space, and all that sort of thing. It was totally new to me. We got as far as projective geometry and I said, "Well now, projective geometry, that's really something that I think I do know." Because H. F. Baker had written a five-volume work on projective geometry. He was the senior professor, the Lowndean Professor, at Cambridge at the time. They said, "Well, what do you know?" So I said, "Well, I reckon I've covered more or less the first four volumes of Baker's book." They just said, "Good God, you don't call that projective geometry." [Laughs] Of course, what they meant by projective geometry was abstract projective geometry in terms of Galois fields instead of the ordinary number field. That sort of thing is totally different from what Baker talked about. So I really had to learn that sort of mathematics from scratch, and that took me the best part of the first year I was there. Then in the second year, I did go to some lectures by Alonzo Church, but he was a very difficult person to approach. I used to go and knock on his door and hear him talking to somebody, so I would go away. I did this for quite a period. It wasn't until about half way through the year that I discovered that the person he was talking to was himself.

Turing was there at the same time, and we sort of saw each other socially, Church, Turing and I. We were the only three people working in logic at that time. Though von Neumann was slightly interested, he didn't lecture on that aspect. He was interested in the logic of quantum mechanics, and he was working with Garrett Birkhoff on that. In fact, I spent a lot of time talking to von Neumann and other people, and not so much really to Church, so I learned a lot. Now coming back to statistics, Sam Wilks was there. I met him very early on because he always made a point of inviting any Britisher who arrived at Princeton to his home the very first week he arrived. I was then really supposed to be working in logic. Wilks hadn't written his book, and he wasn't at that time giving a course, but I went to some seminars with him. For example, I remember Hotelling coming and talking about multivariate analysis at one of the seminars. So I was just sort of not forgetting about statistics, but not really learning any.

I went back to England in the summer of 1939 expecting to come back to Princeton to finish my Ph.D., but the war started so I stayed there. At the beginning of the war the navy obviously realized there was a shortage of mathematicians for the fleet—somebody decided this—and an advertisement appeared saying that mathematics graduates were required to serve as instructor lieutenants, the functions of the instructor lieutenant being to teach the midshipmen basic mathematics and also to advise the captain on tactics in battle. [Laughs] Practically the whole of my contemporaries and I showed up as applicants for this job, including, for example, Fred Hoyle. We were introduced first of all to Instructor Commander Somebody-or-other who asked us about our mathematics—what did we specialize in. When I said, "Topology," he said, "That's some kind of n-dimensional geometry, isn't it?" And I said, "Yes." And he said, "Well, if you now go and be interviewed by Instructor Commander Gascoyne, he'll tell you a bit more about what you are expected to do." Instructor Commander Gascoyne said, "Well now, I gather that you've done some mathematics but my job is to make it clear to you that your prime function aboard ship is to answer questions about bets. You're in the Officers' Mess and people will ask you about poker. How are you at it?" So I said that I didn't know that I was very good at it. Anyway, we all passed through this process in which the real function was to see how you would get on, and we went through color blindness tests and so on. I subsequently met most of the rest of the group from my year during the war, and learned that *none* of us had been appointed as instructor lieutenants. [Laughs]

"THAT WAS THE MOST FANTASTIC LUCK I HAD"

Barnard: So I then got a job with the Plessey Company, which at that time was a very general engineering company. In fact, the man that ran it said that he was prepared to make anything from a hat pin to an elephant, so long as the order was large enough. It is now a very big organization and a major competitor in the radar business, telephone manufacturing and electronics generally. But at that time it was not so large, and they took me on just as a mathematician. The head of the design office decided that they could use just a mathematician around the place. And that was the most fantastic luck I had, to be appointed to a job like that, because I could just wander around. I remember the first thing that I was asked to do was to see whether it was possible, with a given set of gears on a milling machine, to get the machine to cut a metric thread although it was in fact made to nonmetric units, to feet and inches. They said that everybody had tried to get a set of gears that would do this but they had never been able to manage it. So I just took the ratio—we know 2.54 centimeters make 1 inch, which means that the ratio has to be $^{254}/_{100}$. If you reduce that to its lowest terms, $^{127}/_{50}$, then if you haven't got the prime 127 in your gears, you can't do it. I also had the job of looking after the manufacture of loudspeakers, just as sort of something to do when I wasn't doing anything else. I learned about acoustics that way, as well as manufacturing technology and the theory of designing magnets.

DeGroot: Did you get involved in statistics with that company?

Barnard: No, actually I finished up at the Plessey Company being chairman of their shop stewards; I was still involved in politics. By that time, I had read Shewhart and had been persuaded that the proper thing for me to do in the war effort was statistical quality control. But the kind of thing they were doing at Plessey's, and that I was doing at Plessey's was not in that line. So I wanted to leave, but the Plessey people, both the unions and the management, wanted me to stay there as chairman of shop stewards, not as a statistician. But a chap named John R. Womersley had got the Ministry of Supply to set up a unit for statistical quality control, and I was recruited to that in 1942. That was where statistics really got going for me. Incidentally, you know Fisher's correspondence is going to be published, and I discovered just the other day that there is a letter from Fisher to Shewhart, dated 1940, saying he's read Shewhart's book and has been very impressed with it. I was reflecting about what would have happened if Fisher had gotten involved. You know, Fisher was kept out of anything to do with the war; he was under suspicion.

DeGroot: Why?

Barnard: Nobody knows. My guess is that the Provost of University College was responsible. He was a very narrow-minded man, and Fisher had a real row with him. The man was a fool. I've never actually checked on exactly what happened. Fisher was prepared to have dealings, I think, with German geneticists in the 1930s when other people refused to have anything to do with them on the grounds that they were terribly racist. I think that was the sum total of Fisher's sympathy, if you call that sympathy. The other thing of course was that Fisher did in fact support the Eugenics Society, which again, viewed in proper context, I think has got nothing whatever to do with fascism either. The main plank in their program was family allowances to middle class people so as to balance out the birth rate drop in the middle classes and large birth rate in the lower classes. Anyway, Fisher was in no sense Nazi, and was in fact intensely patriotic, really ultrapatriotic in many ways. But he was never involved in the war, and it occurred to me to wonder what would have happened had he been, because it might or might not have been a good thing. [Laughs]

DeGroot: I remember learning as a graduate student that in your work during the war you had discovered and developed sequential analysis, independently of, and more-or-less simultaneously with, Wald's development in the United States. Was that done in connection with statistical quality control?

Barnard: That was in this group, yes. I was going to say that we had Dennis Lindley; Peter Armitage; Robin Plackett; Peter Burman; Patrick Rivett, who subsequently went into operational research and was the first professor of operational research in the United Kingdom; Dennis Newman, of the Newman-Keuls test; and Frank Anscombe. They were all in this group. Actually, Dennis Lindley and Peter Armitage had been mathematicians who were given the option of taking an introductory course in statistics by Oscar Irwin or of taking a radar course. They had to do one or the other and they took Irwin's course. So they then were recruited to this group. According to Robin Plackett, what they learned about statistics from Irwin was not too sure either.

DeGroot: Was this option because of the war?

Barnard: Oh yes, it was required. It was either that or the army.

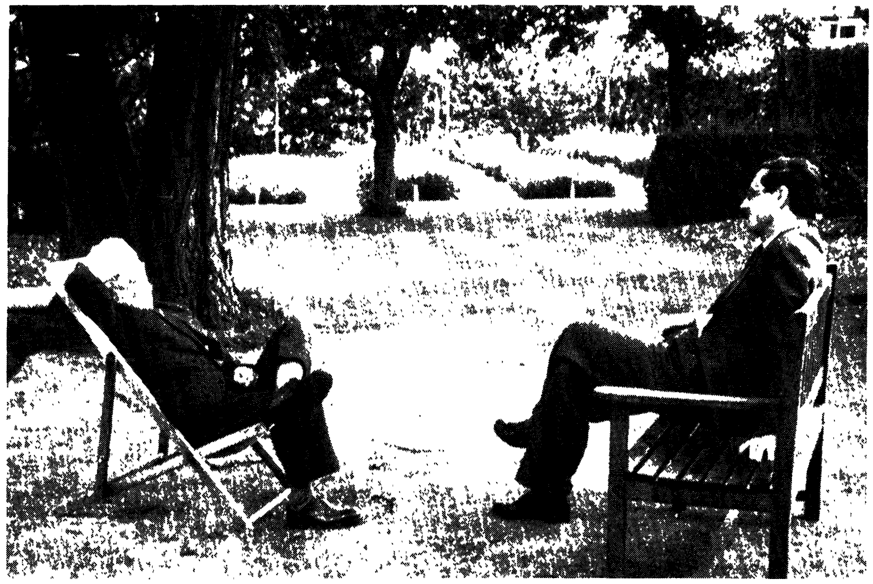
"I HAD NEVER EVEN HEARD OF THE NEYMAN-PEARSON LEMMA"

Barnard: And so they were recruited for that group. I was put in charge of those people, of Dennis Lindley, Robin Plackett, Peter Armitage and others. Frank Anscombe was on his own because he was older. But I had them and a few other people as what we called the research group that studied problems for which there was no standard solution. The other people were basically engaged in putting in Shewhart charts. Actually although they are called Shewhart charts, at that time one of the best references on that subject had been written by Egon Pearson. It was called BS600, British Standard 600. He had written it in 1935 because he had been interested in industrial applications through the thirties under Gosset's influence. So strictly speaking, they were putting in Pearson's British Standard charts because there was a difference. Shewhart used 3σ and the British Standard used 3.09σ because that exactly corresponds to the one in a thousand probability. Actually there were other slight modifications of the Shewhart charts. Shewhart, I think, only used one limit. The British typically used two: a warning and an action limit. That kind of thing. The difference between 3σ and 3.09σ is of no importance but the relationship to probability was clearer in the Pearson approach. Because of that, when we were dealing with nonstandard problems, we would use the probabilistic approach.

Instead of actually seeing whether your material is within specification you narrow the specification and see what proportion fail. It is really like using an upper percentile and a lower percentile to estimate the location and the scale. That is essentially what you're doing, and that can be considerably more efficient. The Shewhart ideas in principle require the exact measurement of the item, and that can be expensive. It's much cheaper to determine whether it's above or below a given limit. We worked on that theory, but also mainly I was concerned with the development of proximity fuses. The idea is that they send out a radar beam and when they get near enough to the target, the reflected beam triggers the explosion and the thing hopefully blows the target up. They were being developed during the war; they had not existed before. We used to have to try and make up models of these things and test them out. That's how Plackett-Burman designs got developed. We were very short of equipment and components, and their designs were worked out in order to see what tolerances could be allowed on the components to still retain the effectiveness of the fuse. And that was how the sequential idea arose, because we were continually having to do trials on these things and they wouldn't work; there were too many failures very often. I mean the kind of thing that used to happen was that on the way to the firing range the shells were stored in the back of a lorry and they were liable to go off. [Laughs] That wasn't very popular with the surrounding population.

I had the idea that we could set up a model in which we would have some old-style fuses made according to the pattern that we now had, and then we would modify the pattern in hope of getting it to improve. The question then arose of how to design that experiment. How many of the old type and how many of the new type do you make? They all had to be made anyway, and you want to have 5% significance of evidence of improvement. You hope by your improvement that you're going to get no failure, so you have a table with a failures and $m - a$ nonfailures for the old type, and no failures and n nonfailures for the new type. What you want to do is minimize $m + n$ to get 5% significance. The answer is that a has got to be 3. It's easy, it turns out. And then the idea was, well, if a has got to be 3 the obvious thing to do is to fire until you've got 3 failures as soon as you can. And that means turning your problem around the other way and allowing your sample size to fluctuate. And then the notion arose, well, why don't we let the sample size fluctuate in inspection problems, and all kinds of things like that. Wald invented the likelihood ratio sequential test. I actually had never even heard of the Neyman-Pearson Lemma at this time, or I might perhaps have thought of it. [Laughs] Instead of that I went and read up Daniel Bernoulli on the problem of points—essentially the early random walk theory, because that's what we were doing and we worked in those terms.

John Womersley had made it clear that I ought to publish this work. He was a very good organizer. He had a rather sad life later because he went on to become Head of the Mathematics Division of the National Physical Laboratory where Turing was also working. Turing had absolutely no time for anybody who pretended to be a mathematician and wasn't. Womersley didn't really pretend to be a mathematician; I mean he had a degree in mathematics, but his real strong point was that he could organize. He never got on with Turing. Womersley finally had to retire and died quite young. But he was extremely good with us in that group. I mean the thing is, he let us get on with it, and argued for us with the top brass.



R. A. Fisher and George Barnard at Whittingehame Lodge, Fisher's official residence as the Balfour Professor of Genetics at Cambridge, about 1957.

"THE REFERENCE SET SHOULD BE THE REFERENCE SET OF CONSTANT LIKELIHOOD RATIO"

DeGroot: So by the end of the war you no longer had any interest in returning to Princeton to complete a Ph.D. in mathematics.

Barnard: No. I actually left the Ministry of Supply before the war ended because the government had an arrangement with Imperial College in London that a skeletal staff should be left to continue training people for radar, one of whom was Bill Penney, subsequently Lord Penney, the creator of the British atomic bomb, you might say. As the Los Alamos project got going, he was required to go across to help with it. The government told the head of the department that he could nominate anybody he chose to replace Penney, and he selected me. This was in 1944, and as far as we were concerned the war was over. Nothing we were going to invent was ever going to be used in that war but they were still hanging on to us, of course. So I started teaching at Imperial before the war ended, and stayed there. In fact, until the war ended I was teaching at Imperial three days a week and continuing to spend two days a week at the Ministry of Supply.

DeGroot: What were you teaching at Imperial then?

Barnard: Elementary mathematics essentially and statistics. I was by that time starting to teach statistics, and I was starting to write things up. Womersley made me publish two papers, two letters in *Nature*. One of them contains an account of what we did in sequential testing up to the time when Wald's information arrived from the United States. All of this stuff was classified, of course, at that time, but we read the classified material and Womersley said that I should put on record what we had done. So I did, and there is a fairly long letter in *Nature* which is usually not listed in the bibliographies. ["Economy in sampling," *Nature* **156** (1945), 208].

The other letter was on 2×2 tables, ["A new test for 2×2 tables," *Nature* **156** (1945), 177 and 783], on my proposal for what was later called the CSM test. The thing about 2×2 tables was that one had the feeling that one had to use much larger samples than really ought to have been necessary to establish significance. So I got on to the idea that the actual probability of rejection of the null hypothesis when true was much smaller than Fisher's test would suggest it was, and I wrote up the proposal which I claimed to be more powerful than Fisher's test. That brought a reply from Fisher attacking my test. In particular, he said that one of the reasons I got a lower p-value than he gave was because I included the case where all the animals died. I hadn't said anything about animals in my letter, and all the animals dying came quite out of the blue. I learned much later from Harold Ruben why this mention of all the animals dying arose. He told me that Fisher was doing experiments on animal feeding with a chap named Blaxter in Scotland, trying to work out the most efficient way of providing animal feedstuffs to cattle to produce the maximum ratio of meat to input of foodstuff. The regression line has an intercept on the y axis of the output of meat so, of course, theoretically the ratio of the output of meat to input is maximum when none of the animals get any food. Fisher had, in fact, cut the food down and in fact all his animals did die. [Laughs] Apparently Blaxter, who was the agriculturalist involved in this, was very annoyed because he knew damn well that would happen.

DeGroot: That's a funny story.

Barnard: Anyway, I replied to Fisher's reply and we carried on a friendly correspondence in which he pointed out to me the example I've used many times since: What happens if I plant 10 plants and I'm interested in their color, whether they are white or purple. Then somebody treads on one of the plants and it dies. What is the sample size? Is it 10 or 9? I actually had a paper in *Biometrika* discussing that question and, in fact, supposing there would be probability p of somebody doing that, so that the sample size would have a binomial distribution from 0 to 10, and trying to work something out from that basis. ["The meaning of a significance level," *Biometrika* **34** (1947) 179–182] Then I came to realize that this is a load of nonsense; that's not the proper thing to do. You ought to regard the sample size as a conditioning variable. While I was doing that, I got Wald's book on sequential testing for review, and in my review I discussed this question that Jimmie Savage picked up. What I said there was that it seemed to me that the reference set was not a reference set of fixed sample size but the reference set should be the reference set of constant likelihood ratio. That was essentially what Wald was doing, and I couldn't see why that shouldn't be applied just as well to allegedly fixed sample sizes as to anything else. I fortunately did hedge it around and exactly what I said there is, I think, still correct. But I gather it was what triggered Jimmie Savage to say that he was amazed that anybody could be so stupid as to say that sort of thing.

DeGroot: Well, he said that then, but he also said that he later came to realize that that was obviously the correct way to think about it.

"FISHER SAID THAT BARNARD IS THE ONLY STATISTICIAN WHO HAS EVER ADMITTED THAT HE WAS WRONG"

Barnard: I should tuck back a little. At the end of the war the Statistical Society started meeting again and I was asked to give a paper on the work of the SR17, the Ministry of Supplies unit. I discussed what we had done about sampling inspection. I laid it down quite clearly there that the proper way to approach sampling inspection problems is to use Bayes' theorem and to take what we call the process curve as the prior. The probability that a given batch would have a given quality would appear in the analysis. As Fisher later agreed, this is an objectively existing prior distribution and the function of your sampling inspection is to give you a conditional inference about what the distribution is and tell you what to do with the batches when you get them. And that was reasonably clearly stated in 1946 in the first paper of the revived industrial applications section of the Statistical Society ["Sequential tests in industrial statistics," *J. Roy. Statist. Soc. Suppl.* **8** 1–26].

That was one paper. Then thinking along from Wald's review, I sort of worked the ideas out more philosophically in the paper in 1949 called "Statistical Inference" [*J. Roy. Statist. Soc. Ser. B* **11** 115–149], in which I put forth the notion that there are two aspects of uncertainty: forward inferences and backward inferences. The forward inferences are in terms of probability and the backward inferences are in terms of likelihood. Well, the thesis isn't all that clearly stated in the paper, and I think I really ought to rewrite that paper at some time or other.

DeGroot: There's certainly still a lot of discussion about that topic.

Barnard: It needs to be discussed. I sent that work to Fisher. I had previously told him that I was interested in the fiducial argument but couldn't understand it. And in one of his letters, which unfortunately I've lost, I do recall he said that he thought that the way to really look at it was in terms of pivotals. But I did not pay too much attention to it at the time; I was preoccupied with likelihood. So I sent the 1949 paper to Fisher, and in that paper I say that I come to the conclusion that Fisher was right about the 2×2 table after all. Which evidently pleased the old boy, because Harold Ruben, who was working with Fisher at that time, told me that he said that Barnard is the only statistician who has ever admitted that he was wrong. [Laughs] I had a very nice letter from him saying he had read this paper and he liked it very much. He said, "You seem to have developed a general theory of likelihood, and that's a very good thing to do." So we were very friendly from then on, until 1958.

DeGroot: What happened then?

Barnard: In 1958 he published *Statistical Methods and Scientific Inference*. In there, there is a piece about Bayes—Bayes' billiard table—and an argument in which he says that the fiducial argument is essentially the same as the argument used by Bayes in the billiard table case. As a matter of fact, there's a paper of mine bearing on this that will be coming out in the *International Statistical Review* in August ["R. A. Fisher—a true Bayesian?" *Internat. Statist. Rev.* **55** (1987) 183–189]. What happened was that when Fisher's book was published, Dennis Lindley reviewed it very critically [*Heredity* **11** (1957) 280–283]. Fisher was right up the wall. I think actually what annoyed Fisher was when he could see that people were bright and that they disagreed with him. That really shook him. If somebody was just a fool, he could brush that off. But he was very upset by Dennis' review. Dennis went on to show that if you took Fisher's argument, the argument he was using about Bayes, then two different fiducial distributions would arise. You would get a contradiction if you took two samples. Fisher and I must have talked about this, as well as writing letters. I've lost the letters he wrote to me up until about 1950, but from then on Fisher kept copies of his letters so those still exist and Henry Bennett's got the copies. So I've actually just been reading the letters he was writing to me in 1958. And he had told me about this argument. But then we met in Brussels; there was an ISI special meeting in Brussels to celebrate, I think, the centenary of the founding of the ISI or something of that kind. At a cocktail party there, Fisher and George Box and Joan Box and I, the four of us, were having drinks together and this question of the Bayes argument came up again. I said, "Well that didn't seem to me to be the argument that Bayes was using." Whereupon he stormed at me, "You talk like that and you'll ruin the whole development of the subject. You should keep yourself quiet. You don't know what you're talking about," and so on. I just stood there and said, "Well, you know, I'm afraid I'll just have to go and see what it was that Bayes said, and I'll do that tomorrow morning." I remember it well because I went to the Bibliothèque National in Brussels. And Brussels, Belgium, didn't begin until 1832 so their run of the Royal Society Proceedings starts in 1832. I had to wait until I got back to London. Another letter in this correspondence is from me when I got back saying "I have now reread Bayes' paper and I now see that you are right."

DeGroot: Fisher was right?

Barnard: I think so. Whether he's right in saying "This is what Bayes meant" can't be certain because you can't be absolutely certain of what it was Bayes did mean. But I think his interpretation is entirely consistent with what Bayes actually is on record as saying in his writing. That's the argument of the paper in the *International Statistical Review*. But in any case, he had made that clear, and made it more clear in subsequent editions, and he had really already made it clear to me in this correspondence. That's why he lost his temper, because he had actually told me. I now see that reading through the correspondence. He had said that and I just hadn't listened or read it carefully. He was entitled to be annoyed. One of the points, you see, with Dennis' example, was that it's a case of two continuous variates, two continuous distributions, and the section in Fisher's book is headed "Observations of two distinct kinds." It's a case where the initial throw of the ball has a continuous distribution and the subsequent observations are discrete. It's only the first observation, being continuous, that can generate a fiducial distribution. All that the other ones can produce is a likelihood function. He does in fact make that perfectly clear, that the argument is restricted to that situation where you have one source of a fiducial distribution and the other one is a likelihood. And Dennis' example fails because each of his samples can generate a fiducial distribution. In fact, that's how the contradiction arises because it depends on which one you take first. So Dennis' example does not in fact contradict what Fisher said.

"THEY ALL SAID, 'IT WASN'T ME; IT WAS GEORGE BARNARD'"

DeGroot: What is your position about fiducial distributions?

Barnard: I try to explain that in the *International Statistical Review* article. Essentially what I think is that Fisher is entitled to say that a statement of fiducial probability is a probability statement about a parameter but, as I say in the paper, the parameter does not thereby become a random variable in the sense of Kolmogorov. The reason why it fails to be a random variable is that in Kolmogorov's definition a random variable is a function on a probability space, from which it immediately follows that a function of a random variable is again a random variable, and that is not true of fiducial distributions. It is not in general true that if you have a fiducial distribution for θ you can derive from it a fiducial distribution for any measurable function of θ. Specifically, you typically cannot marginalize. If you've got a two-dimensional parameter, you can't project onto the subspace. Actually, Dave Sprott and I had laid down conditions which you have to satisfy in order that you can operate like that on a function of a parameter that has a fiducial distribution ["The generalized problem of the Nile: robust confidence sets for parametric functions," *Ann. Statist.* **11** (1983) 104–113].

Of course, the notion of random variable in that Kolmogorov sense didn't exist in the 18th century, and I think it is perfectly possible to interpret Bayes as following on Fisher's line of argument. And it is a plausible interpretation in the sense that there is a section in Bayes' paper where he seems to take an awful lot of trouble to do something when he could have done it very quickly. And he wasn't that bad a mathematician; in fact, he was a good one, contrary to what Steve Stigler argues [Stephen M. Stigler (1986). *The History of Statistics*, Harvard University Press, Cambridge, Massachusetts]. You know, Steve says Bayes was just a minor figure and it was really Laplace who did it all. In fact, I think Bayes had a subtle sense of logic. But that was my row with Fisher in Brussels. The reason I put this thing into the

Stephen Stigler and George Barnard, 1979.

International Statistical Review was so that Henry Bennett could refer to it when he publishes Fisher's correspondence. As it is, there's a gap in the sequence of letters which doesn't make sense unless you know what was happening.

DeGroot: Let's talk a little about your political activities and political interests. You have mentioned that even in your school days you were interested in political issues.

Barnard: Actually, my main interest above everything was politics from about 1933 until 1956. Well, that's not true—until the end of the war, it would be fair to say. At the end of the war, when Hitler was defeated, I had the feeling that there was no longer any need to go in for politics. I had been very active, and I discontinued. In fact, when I was recruited to the Ministry of Supply it was very strange because the Ministry had been given a report by MI5, the military intelligence, that I was labeled a subversive character and should not be employed by them.

DeGroot: Why did they feel that way about you?

Barnard: Oh, because I'd been active as a communist since 1933. I was active at Cambridge University and also in the United States. You wouldn't remember Joe Lash, Joseph P. Lash? I was in contact with him and, in fact, used to go around making lots of speeches at that time.

DeGroot: While you were at Princeton?

Barnard: Yes. We organized the first piece of political activity on the part of the Princeton undergraduates when I was there. Mayor Hague of Jersey City, is his name still remembered? We set up a Liberal Club amongst the undergraduates—"liberal" in the American sense of the term—and decided that this chap Hague was not the sort of character who should be doing what he was doing.

DeGroot: He was sort of a prototype of the big city boss.

Barnard: Oh yes, he really ran the city. It was his habit that anybody who objected to what he was doing got ran out of Jersey City. And then he was faced with a bunch of Princeton undergraduates whose fathers were quite prominent. I recall Wendell Willkie's son was one of them and the son of the president of Standard Oil. Hague didn't know quite what to do with them because they wouldn't have accepted being run out of town. He was in fact finished. That trip from Princeton was the beginning of the end for Hague.

DeGroot: You joined the Communist Party in England?

Barnard: Yes, I was a member of the Communist Party all that time and was active in it, and made no secret of the fact. But then I think what happened in the States was that quite a number of people that I had known in America were put on the stand by McCarthy and they, I think perfectly rightly, said it wasn't them, it was me. You know the various things, who did this and who did that; and they all said, "It wasn't me, it was George Barnard." [Laughs] I have been told, I've never checked on it, that there are five volumes of subversive activity which are on record in

George Barnard with his eleventh grandchild, Clare, December 1987.

the McCarthy files which all belong to me. In fact, I think the extent of the subversive activity that I actually engaged in was this kind of thing against Major Hague. And a trip—I always remember going down to Washington to lobby the local congressman on the subject of arms for Spain. The Spanish War was still continuing, and a bunch of us got in a car to drive down there. We were stopped just after we got into Maryland by a policeman who wanted to tell us that the speed limit in Maryland was 50 miles an hour. He just wanted to tell us so we wouldn't go over it, because we had been. But he wanted to see our license. There were five of us in the car and the nearest thing to a license that anybody possessed was a license to drive in the city of Chattanooga, Tennessee, for 1932. We were therefore in a difficulty, and so was the cop because he couldn't reasonably allow us to continue to drive anywhere. So he got in the car. There were two of them, they were a motorcycle combination, and the other chap drove his motorcycle to the nearest judge. The judge asked us how much money we had, and between us we had something like $15. So he fined us $10. There we were about half way between Princeton and Washington with no way of driving. The best he could do was to give us a certificate saying, "These people have already been fined $10. If any other judge has to, will you bear that in mind." By that time, we were nearer to Washington than we were to Princeton, so we decided that the best thing to do was to go on to Washington. I always remember that. We asked the congressman what he thought about the Spanish War, and he said, "Well that was over a long time ago, wasn't it?" We made it clear that we didn't approve of the government's policy of not allowing the Spanish government to have arms.

"THEY ALWAYS HAVE TO GO BACK AND LOOK UP MY FILE"

DeGroot: Why do you say that you think it was perfectly proper for these other people to mention your name in congressional hearings as having been the one who did all these things?

Barnard: Well, I was in England. There was nothing they could do to me. Except, of course, the result was that when I was invited to go to the States and I had to have a visa, the answer was no.

DeGroot: When was that? What year was your first try?

Barnard: I think my first refusal would have been in the mid 50s. McCarthy was still alive and had not been totally gotten rid of, though he wasn't doing at all well at the time. I had an invitation to visit somewhere and I applied for a visa, and was asked to state my past political associations. I had been advised that the thing to do is to say nothing at all, because if I had said anything I would then have been put on the stand under oath and I might have been required to recall other people I knew who were at risk. I mean, the worst they could do to me was to send me back home. So I was advised the thing to do was to say nothing at all. So my visa application said that I did not believe in the violent overthrow of the government of the United States. In fact, my general views on political matters at that time were that principles of operational research could be applied with value to problems of government. General liberal principles, but so far as my past political activities were concerned, I did not consider that they were any concern of the United States or any other government. I was prepared to give any assurance they wanted as to what I should do in the United States, but not prepared to tell them my past history. And so they turned me down. I had several interviews with a counsel in which we discussed the pros and cons of various ways of handling the situation. The communists were getting in and out of the United States as easy as anything at the time, no problem. But they turned me down. It wasn't until 1961 when Jerry Cornfield organized for me to go to the National Institutes of Health that I was admitted.

They decided in effect to modify the procedures in such a way that I was given a special waiver. Actually ever since then I've always had trouble. They always have to go and look up my file, and then they see this long file with yeses and noes and maybes. [Laughs] I was visiting Waterloo at one time and Marvin Zelen, who was then at Buffalo, said "Why don't you come to Buffalo?" I said, "Fine, I'd be glad to come to Buffalo, but I always have this damn nuisance." And he said, "Oh, I'll fix it." I said that I was at Waterloo from September until December and it would be convenient if I could get, say, six entries in that period. And sure enough they gave me an entry in October to leave in December, but only one entry. So I had one visit to Buffalo and that was it. And every time it's like that. It's nothing serious, nothing other than sheer bureaucratic muddle. One time Seymour Geisser arranged for me to go to Minneapolis. By that time I had a special thing stamped in my passport indicating that I could go there anytime. Well, anticipating trouble, I started the process nearly a year ahead of time. I thought, "Look, I'll get this fixed. Just so everything goes smoothly, I'll apply for the visa." They looked at my passport and said, "You don't need a visa, you've got one." I thought, "That's fine, that's remarkable." My wife, Mary, was coming, and she would need a visa. So she applied about three weeks before we were going. For her to get a visa she would have to explain how she would not become a charge on the public, and that I was going to be paid for lecturing and so on. So they had to see my visa. By that time the policy had changed. They cancelled my visa, and didn't give Mary hers. So I said, "All right, then I'm not giving the lectures, and I'll let Seymour Geisser know. I'm fed up with this." And they said, "Oh, no don't do that. Don't do that." It was literally the Friday before the Monday we flew that I got a visa.

DeGroot: What year was this?

Barnard: That would have been about 1968. I subsequently had several other visits to the States. In any case, what with my legs giving out, I'm not traveling far anyway. I've sort of taken the view that it's really too much. It's very annoying to never quite know and always have the feeling that you might even get there and then they'd turn you back. I really would be fed up at that. It's the immigration service and the bureaucrats there. But they have their jobs to do, and the laws on that subject are totally crazy.

"HE WAS SOMEBODY WHO REALLY WANTED TO BE CLOSE FRIENDS WITH PEOPLE"

DeGroot: Who do you feel have been the major influences on your career in statistics?

Barnard: Oh, Fisher of course primarily. But actually, again talking about politics and going back biographically, I first met Neyman in 1948 in a taxi.

DeGroot: Not by accident?

Barnard: No, I was with somebody who was with Neyman, and we all jointly took the taxi. There was a congress on the history and philosophy of science at which he and I and de Finetti were all speaking. I recall that I got talking to Neyman about likelihood in the taxi. And he brought up what was in effect the Neyman-Scott problem of inconsistency of maximum likelihood for n samples each of size two. I then decided I could talk to Neyman about politics but not about statistics. Just as I could talk to Fisher about statistics but not about politics. I followed that rule fairly firmly for the rest of the time I knew them. I've always had the absolutely highest regard for Neyman's principles as a liberal academic. He was really the tops and a very courageous man. Do you know the incident that David Kendall put into Neyman's obituary for the Royal Society? Just after the war the Greek elections were held and the country actually wanted to get rid of the monarchy. But the American and British governments wanted to restore the monarchy. They set up an election system which was supposed to be free and liberal, and Neyman was one of the people who was appointed to go as a commission of statisticians to see to it that the election was fair. He went to Greece, and do you know what he did? He resigned from that commission. Went back to Paris and gave an interview to the French communist paper *Humanité* in which he said that the election was a complete fraud. I mean, his citizenship could have been revoked, and it was a very courageous thing to do. At that time things were very tense. When David Kendall asked me what I thought about Neyman, I said that I had a very high opinion of him, particularly for what he did at that time. I thought he was a very great man indeed and deserved the highest credit. Though, of course, what he did to statistics is another question. In a certain sense Neyman made mathematicians take statistics seriously. That was a very big service, but in doing that I fear he did a lot of damage as well.

DeGroot: Were there other major influences besides Fisher?

Barnard: Egon Pearson, of course. We worked very closely together and got on extremely well, but we never actually wrote any joint papers. He helped me a great deal with that first paper on 2×2 tables in which I put forward the CSM test. He published in parallel with it another paper from himself saying that he had similar ideas, which was true. We went around together consulting with the British Standards Institution on quality control. British Standards had a quality control scheme called the Kite Mark scheme. Egon and I used to go around the factories supervising the workings of that. We worked on committees together, and because he was at University College London and I was at Imperial College London, I was always on his examining board. He could never be on ours because the laws of our college didn't permit outside people to be on ours. But he could have people from outside on his board and he would always have me. I very much liked Egon. Of the three characters, I admired Neyman, as I said; I admired Fisher for his statistics; but as far as the person was concerned, I most liked Egon. He was a shy and quiet man, but very insightful. You know, he never really wanted to be a statistician; he was more keen on painting. His father sort of decided he should be a statistician. That was the amazing thing, that he had this sense of filial duty which led him to accept things which certainly nowadays children wouldn't accept. Egon got on extremely well with Gosset. I never knew Gosset, but he must have been a very attractive character.

DeGroot: He seems to have gotten on with everybody.

Barnard: He got along with everybody, yes. He fell out with Fisher at the end, and I think his opinion of Fisher's temper was quite strongly disapproving. But so was Fisher's own opinion of his temper. I always remember walking with him from a Statistical Society council meeting when he was President. I used to walk with him to the Liverpool Street Station where he'd catch the train back to Cambridge. I forget how it came up but he said that he wished he hadn't the labile temper that he actually had, and that it could cost one a lot. He was somebody who really wanted to be close friends with people, but he had this temper and every now and then he would fly off. And it was a matter of luck whether people could take it or not. He certainly paid for it—he was a poor lonely old man. Although he did quite well in Australia; he got on with them. There was nobody there he would upset. By the time he left England he had sort of upset most of the people he wanted to be friends with. But I think that the correspondence that will be coming out gives a different picture of Fisher's personality from the one that is broadly current. He was willing to be very patient and write helpful letters to people. He certainly did that with me. He was the biggest influence.

"ONE SHOULDN'T PUT IN AN ASSUMPTION THAT ONE DOESN'T NEED TO PUT IN"

DeGroot: Do you have particular favorites among your own publications, or ones that you think were the most influential?

Barnard: Well, not the most influential, but the one that contained the best theory was the one of 1949 about likelihood. But the trouble was that I didn't make it clear what I was getting at. I put too much into it; it's too long a paper.

DeGroot: Your name has always been associated with being a leading "likelihoodist," basing inference on the likelihood function. Is that still your view?

Barnard: Well, to some extent. Though more recently, as you know, I've come back to the idea of pivotals and have been playing with that. I realize yet again that it's a pity I didn't read carefully what Fisher wrote to me back in the late '40s because I think the pivotal idea in a sense allows you to be a Bayesian in so far as you need to be. My opinion now is that the proper process of statistical inference is conditioning on known values of variables whose distribution is known. And then the question is, of which variables do you know the distributions? In effect the Bayesian view is that you can assume all of your unknown quantities have known distributions. I don't think that's actually true. In any case, again coming back, I accept de Finetti's argument about personal probability. That's a valid argument. In fact, I'm saying that in the paper at this conference. I think it should be taught to undergraduates. But in the sense of probability as used in science, what I like to call experimental probability, I do not think that an experimental quantity like the velocity of light can be said to have a known probability distribution. In any case, for reasonable agreement among scientists you would have to have reasonable agreement about any prior distributions which you would use, and in the way statistics is used in science, it will not in general be possible to assign agreed distributions to all the unknown parameters. If you could, I'm perfectly happy to use them. But I think you would not always be able to do that, so you would either have to say that some problems are not solvable or adopt some other method. And sometimes you can; I think you can get away with using a pivotal argument without using assumptions of prior distributions. That's a long story actually.

I've been doing a lot of work on the Behrens-Fisher problem, the two-sample problem, just lately. In the *t* problem, the single-sample location-parameter problem, you can remove the nuisance parameter by simple marginalization of the pivotal involving the scale parameter, leaving you with a pivotal involving the location parameter. And that's all you need because that essentially contains the answer. The Behrens-Fisher problem has got this other parameter and it is typically done in terms of two independent scale parameters, which I think is wrong because you would not be comparing two samples in relation to their location unless you had some idea that their scale parameters were at least of the same order of magnitude, that they were reasonably comparable. And so what you should do there is to introduce a single scale parameter applying to both samples, and then a second parameter which expresses how much of the total variance arises from one sample, how it is shared between the two. It's that sharing parameter that causes the problems; you get rid of all the other parameters. That one has the property that the usual ignorance prior $d\sigma_1 d\sigma_2/(\sigma_1\sigma_2)$ corresponds to a singular prior for this sharing parameter with the prior element $d\beta/[\beta(1-\beta)]$ with a singularity at both ends. If you make it go between 0 and 1, then that in effect is saying that as soon as you get a finite value for the observed variance ratio, your data are contradicting your prior. So I think that there, to solve the problem, you do in fact have to assume a prior. You have to say that we are comparing these two samples, we are therefore implying that we do believe them comparable, we therefore believe that this β does not take the value 0 or 1 with virtually infinite probability and that it is bounded between 0 and 1. One prior would be a uniform prior or some kind of proper beta prior. The way it should be looked on and in fact is quite easy to do, is to have a program which will generate your p-value, your posterior probability for the difference if that's what you're interested in, as a function of the input beta prior. You can vary that as you wish and see what difference it makes. And that's the way to do it. But the singularity at both ends is something that is not tolerable. I've been writing papers recently—they are mostly not out yet—in which I spell ignorance with a double g, "iggnorance." The Behrens-Fisher solution and Jeffreys' solution to the Behrens-Fisher problem really assumed "iggnorance" which does not in fact represent your true state of mind.

DeGroot: What do you mean by "iggnorance"?

Barnard: Just bloody ignorant; you know, total ignorance. You're saying that you don't know the scale parameter of this distribution, you don't know the scale parameter of that one, and furthermore if you knew the scale parameter of this one, you still would know absolutely nothing about the scale parameter of that one. And that's not sensible. The further point is that the t test is robust to departures from ignorance, in the sense that if you now say, "Well, let's suppose that we knew a little bit about σ, what difference is that going to make?" It's a mathematical property of the way the thing comes in that putting in a proper prior, so long as it doesn't concentrate unduly, is not going to make much difference to your t answer. But putting in a proper prior to this β variable does make a considerable difference in some cases. With large samples it doesn't make much difference, but with small samples it can make a very big difference. And one should be aware of that. What worries me with any general Bayesian approach is that people can get into a frame of mind whereby they happily go ahead and put in a noninformative prior and not worry, as they should worry, as to whether it's going to cause trouble. But the thing I like about the pivotal idea is that you don't really have to say in advance whether you're a Bayesian or not. Because you can put in your assumptions step by step and say, "If you make that assumption then this follows. If you additionally make this assumption then this follows. If you additionally make $\cdots$" Just like that, go on as long as you want to, in order to get the kind of answer that you find useful. For example, with a t, Student's answer to the confidence distribution for μ is a perfectly satisfactory answer. You can say it's a posterior distribution with a uniform prior, but so what? That's what it is and that's what you believe rationally, whether it's a random variable or not a random variable in the technical Kolmogorov sense.

DeGroot: But you do think that the important thing is to get an answer that you believe in as representing your uncertainty about the quantity in question.

Barnard: Yes. I mean, it's clear that it will not really represent your uncertainty about μ in the rigorous sense of Kolmogorov, which theoretically it should if you really are a strong Bayesian. Because that would mean that if you had 100 such μ's you would have a 100-dimensional t distribution, which has all those Stein problems attached to it, and you obviously don't believe that. But it doesn't matter, and that's the important thing. I think one shouldn't put in an assumption that one doesn't need to put in. But I am prepared to accept that assumption, unlike Fisher. Actually this correspondence will show that's not altogether true with Fisher. I think Fisher went rather overboard against Bayes in the design of experiments because he was attacking a view that had been held. I think if he had not had that view to attack he would have been less extreme. What I am sure of is that he and Jeffreys were far closer to each other than commonly is thought. Jeffreys, in that videotape he made with Dennis Lindley, said that when he and Fisher agreed, they both knew that they were right. And when they disagreed, they knew they didn't really know, as it were. There's no question about it: in their general approach, Jeffreys and Fisher were much closer to each other than to Neyman. They both applied it in their respective scientific fields, and they both applied it in essentially the same way. The correspondence shows that they were good friends, there's no question about that. I think Fisher attacks Jeffreys sort of by way of a joke. Fisher used to write reviews of Jeffreys' books in which he always had a dig at Jeffreys and the prior distributions, but I think it was a sort of joking dig. I don't think he really meant it as very serious. I'm not saying he agreed with Jeffreys; but you could read the thing as meaning that he regards Jeffreys' views as pernicious nonsense, and that's not actually what's meant.

"TO MOVE THEM TOWARD LIKELIHOODS WOULD BE A GOOD THING"

DeGroot: You've seen the field of statistics undergo a lot of development and change during your career. Are you optimistic about the future of statistics?

Barnard: I worry a bit. As far as broad applications in economics and such fields are concerned there is clearly a very big future. I worry a little about the position in natural science. And although statistics in medicine is very popular currently, I'm not altogether happy about the way things are there because I don't think the foundations are altogether firm. The medical people continue to use *p*-values and so on. They use them with some common sense, which of course enables them to avoid serious blunders. But really very often what they are doing should not be looked at in that way at all. Don Berry has written about some of this.

DeGroot: They shouldn't be testing hypotheses.

Barnard: They shouldn't be testing hypotheses, no; but that's what they think they are doing. And it worries me that there is a discrepancy between the model of what it is they're doing and what they actually are doing.

DeGroot: Why are you worried about applications in the natural sciences?

Barnard: On the same grounds. Their view of statistics is still being presented largely as a matter of *p*-values and so on. The proposal I was suggesting to move them off that toward likelihoods would be a good thing. Indeed, people like the high-energy physicists already do that. They do use likelihood, and geneticists do too.

DeGroot: They report likelihood functions?

Barnard: Yes, they report likelihood functions. Actually, some geneticists I know are fully Bayesian. Incidentally, you know that they've recently located which chromosome the cystic fibrosis gene is on. They did that last year. I was at a meeting of geneticists the year before last in which they had narrowed it down to three chromosomes and there was the question of which of these three to concentrate on to try and locate it further. And the decision as to how that was done did involve a Bayesian analysis of the data that was currently available. The Bayesian argument was that you assume that the number of genes on the chromosome is roughly proportional to the size of the chromosome. You had to make some such assumption to find the probability of the gene being on one chromosome or another. It was interesting because one of the people involved in it was a likelihood man, and he said that he wanted to use the likelihood. Cedric Smith and I were there and we agreed that it's not strictly likelihood he's using, he's really using the posterior probability. But you don't see Bayesian arguments used very often in natural science and that's a situation which I think is not too happy.

DeGroot: There was very little use of statistics at all in physics and chemistry for so long. Now they're just beginning.

Barnard: They started using statistics seriously in, for example, high-energy physics at least 25 years ago because I went to CERN about then at their invitation to tell them what to do. They were taking statistics quite seriously then. Basically, and I think I said this in my President's Address to the RSS, the physicists used to say what Blackett said to me: "If I need statistics to analyze my data, it means I haven't got enough data." And that used to be true. But of course high-energy physics is so damned expensive that you can't afford to get enough data and you've bloody well got to use statistics. [Laughs] But they also recognize, I think, that it's not the usual statistics that they have seen. They do tend to use things like likelihood arguments, that could be easily translated into posterior probability. What's the difference between a likelihood and a posterior relative to the uniform prior? But those things don't square as it were with what they've learned about statistics from the statisticians, and that's an unsatisfactory situation.

Even now the bulk of elementary statistics courses start off with the traditional *p*-values and so on. They might have a Bayesian chapter somewhere about three quarters of the way through the book, but they start off in that way and I don't think that is the way to start. Bayesian theory, things like de Finetti's arguments, should be put right at the beginning. There is no earthly reason why they shouldn't be.

DeGroot: In introductory courses?

Barnard: Quite. I did it with psychologists at Waterloo. Mind you, it shook them somewhat. Actually it was sort of a second introductory course. They had already had some statistics, but in principle this was the first course they were getting. So they thought they knew about statistics, but this was very different from what they had heard. So I think there's a big room for improvement. The other thing that has shaken me is the length of time it took to establish relationships with computing. I knew about Turing's work on the computer, having known Turing and known what he was doing. I remember saying, when they produced the first computer, "That means we can now actually plot likelihood functions." And I remember that you were the first person actually to have a likelihood function printed in the *Annals*, weren't you?

DeGroot: Was I? That's a surprise to me.

Barnard: Yes, you were. At least as far as I know you were because I watched for it. You had a paper in which you actually had diagrams.

DeGroot: That's right. But I didn't know that I held the record. [Laughs]

Barnard: That was some years ago, but not all that long ago and *long* after it should have happened. The first likelihood functions that I actually publicly produced as graphs were on nylon stockings. There's a journal called *Which* published by The Consumer Association in the U. K. in which they test things on the market and give you information about what's the best buy and so on. There is a similar magazine called *Consumer Reports*, I think, in the United States.

DeGroot: Published by Consumer Union. Yes, they do the same thing.

Barnard: Well, they did a study on nylon stockings, about how they wore. Their failure time is very much an exponential failure time. It depends on how quickly you catch your stocking on something and it starts a ladder. So the data were highly nonnormal, very skew, and the best way of conveying the information about the relative merits of the different brands of stocking was to give the likelihood functions that they would last for a given length of time. We drew pictures which had a maximum at the maximum likelihood estimate of the life, and then they tapered off, down to 0 here and off to infinity that way. We showed them alongside each other, and you could see that the mean life of some stockings was longer but the chance of failure at an early stage was higher. This was all conveyed in the picture.

DeGroot: When was that?

Barnard: That must have been in 1963.

DeGroot: Were you a consultant to that organization?

Barnard: Well, actually my colleague Chris Winsten is their official consultant. He got me into it and I suggested they should do that. But the first ones that were actually used, as far as I know, in a scientific issue were done some years before that by a chap who was working for the atomic energy authority at the time—a chap named Mercer who is now Professor of Operational Research at Lancaster. They were looking at assays of blood constituents and there was a theory about the way a chemical reaction went. They had done some experiments collecting data and they had a two-parameter likelihood surface to plot. It was one of these cases where the actual contours were banana shape. That meant that if you did a straightforward maximum likelihood calculation you got the wrong answer completely, because it was more or less known that the thing must either lie on one axis or the other. The maximum likelihood estimate was very near one of the axes and the ordinary calculation would suggest that it was almost certainly within that neighborhood and nowhere near the other one. The fact was that the banana went right across to the other one, and the other one was the right one.

"THERE WAS A REPORT KNOWN AS THE BARNARD REPORT"

DeGroot: Tell me what you like to do when you're not doing statistics.

Barnard: I used to play the viola. I also used to play second violin—the *second* is important—but I gave up playing the violin.

DeGroot: You played in a quartet?

Barnard: Yes, we had a fairly regular quartet at Imperial when I was there. We used to play concerts at the college. But then I managed to get a viola and learned to play it, and moved over there because it's easier to get into a quartet. There are not as many violists as there are violinists. You known, Henry Daniels plays the piano very well, but his real instrument is a concertina. The register of a concertina is the same as the register of a clarinet, so he can play a clarinet part or a violin or viola part with his concertina. The last time we played actually was with Bertha Jeffreys [Harold Jeffreys' wife]. We played the Mozart trio for clarinet, viola and piano. Henry played the clarinet part with his concertina, I played viola, and Bertha played the piano. That's what I like doing the best. I also have to go and look after the garden; we've got a sizeable garden where we live.

DeGroot: You retired in 1981. Did that change your lifestyle very much?

Barnard: Yes, in the sense that we had got this place at Brightlingsea—Mill House—in 1966 when we decided that it was a very nice place to live. But until that point, I had never lived there. [Laughs] But since I've retired, that is where we have lived and that's where we like living. It's overlooking the sea and it's quiet. I'm afraid I'm rather glad to be out of the universities in the United Kingdom at this time. Well, actually I don't know whether I should say that because when I gave up party politics I got involved in a lot of government administration. I was on money-allocating bodies like the Social Science Research Council and the University Grants Committee and the Computer Board.

DeGroot: Are these countrywide bodies? What do they do?

Barnard: Yes, these are national institutions. How much money each university gets is decided by the University Grants Committee. How much money the universities can spend on computers is decided by the Computer Board. Actually there was a report known as the Barnard Report. It's not much referred to nowadays but it was the report which said that universities should be provided with money to buy computers for uses other than scientific computing; that there should be funds for using computers in all fields of study, including the humanities and so on. This was one of the few reports that was ever acted upon by the government.

DeGroot: So you have to be careful what you say about the government now. When was that, George?

Barnard: That would have been in about 1970. Up to that point, physicists had been able to get money to buy computers as part of their experimental equipment, strictly for use on their experiments. People like statisticians or anybody else couldn't typically get access. That was about the time of transition from the major valve machines to the solid state machines. Soon after that it became very obvious that people should use these machines all over the place, and essentially we just said so. And as I say, what was unusual about it was that they actually did it; they did provide that money. I was on the grants committee for five years. In the U. K. there's a Social Science Research Council, and a Science Research Council which is natural science. I was on the mathematics subcommittee of the Science Research Council, and things like that. There was an inquiry into the supply of students in science and I was a member of that committee. I've been doing that more or less since the late fifties.

I was on a committee called the Building Operations and Economics Committee of the Building Research Station not long after the war, at the time when all the postwar planning was the rage. Everybody was saying that we've got to build highrise blocks of flats to preserve the green countryside; otherwise the place will be covered with little houses and nobody will be able to move anywhere. It was the architectural fashion to build high. There was a chap named Stone in the Building Research Station who seemed a dull sort of statistician that nobody paid much attention to, and he wrote a paper pointing out that if you built two-story houses with gardens in the standard dull, unimaginative British suburban way, that you would get more agricultural productivity, and you'd have more open space for everybody and more access to the land. I very well remember the committee meeting at which he produced this report, and it went around the table and *everybody* there said, "We can't see what's wrong with the argument, but it must be wrong." [Laughs] The fact is he was bloody well right. This fashion of highrise buildings is absolutely disastrous, and it didn't produce the results it was supposed to produce at all. It would have been far better if people had listened to him. He was at least 15 years ahead of his time. He published it as a paper in the RSS journal. I always think of that as a warning about simply adopting the current fashion. Everybody, including me, thought it must be wrong. So his paper wasn't acted on, but it should have been.

"I WOULD MOST LIKE TO GET CLEAR ON THE FOUNDATIONS"

DeGroot: What does the future hold for you?

Barnard: Well, I would most like to get clear on the foundations. I believe in Jack Good's Bayes-nonBayes compromise. He now calls it the Bayes-Fisher compromise, doesn't he? And I think that's right because Fisher believed in conditioning. The key thing is, do you believe in conditioning. That's the key as the inferential procedure. Just exactly what you mean by probability and how you interpret the meaning of that word is relatively minor. I think that it's important to get clear on the foundations partly to teach people, to teach students, and partly to lay a proper foundation for the use of statistics in science and, for that matter, ultimately in social decision making.

DeGroot: How come we've never seen a book by you expressing your view of statistics?

Barnard: Laziness. I could always find it nicer to talk than to write. But maybe you will. I've had this embarrassment, you know. I inherited more or less from Egon Pearson a large manuscript which he had written about Gosset, and the correspondence between Gosset and Egon. I've been trying to arrange to have this published, and most publishers think they don't really want what it was that Egon wrote about. And I can understand it. Gosset was responsible for Egon going to Winchester as a school, and Egon did very well at Winchester. It's a remarkable school. Gosset was the man who said that Karl should send Egon to Winchester. So there's a lot about Gosset as a man and about Winchester as a school which is not strictly statistical at all. So it must be edited. And then there's the quite important letters which really establish, I think, that the idea of confidence intervals originated in the Gosset-Pearson correspondence back in 1926, and various other things. I've got Robin Plackett to agree to join me in working on this, and we got together just a couple of weeks ago and decided that the way to tackle it is to have a series of chapters involving the Fisher-Gosset correspondence which is unpublished as well. I had suggested to Joan Box after she had written the life of her father [R. A. Fisher] that she should write something about Gosset, which she did. But she agrees that it's not completely satisfactory as it stands either. The Gossets were Huguenots; the name is a French name. They came over after the revocation of the Edict of Nantes in 1685. Gosset's great great great grandfather was silversmith to George III. There's all kinds of information of that type, which again is not relevant to statistics. But we should somehow or other try to merge all this material into a coherent account. It's left that I've got a list of chapters, the first of which is how the situation stood in 1920, what the position in statistics was then. Which I shall write. It is intended that the book will go through the various stages of development of statistics from roughly 1920 up to 1950, sort of telling the story in the chapters and then having as appendices the actual correspondence people were writing at the time.

DeGroot: That would be really fascinating.

Barnard: That's what I want to do before I write anything about the foundations of statistics. I must admit though, that is the other trouble about writing a book. You decide you're going to write one book and get yourself committed, and then you get much more interested in something else.

DeGroot: We've waited a long time for this book of yours on the foundations. I hope you do it.

Barnard: Well, Mary won't go into my study now. She refuses to have anything to do with it essentially. Piled with paper, large amounts of which are drafts of essays on experimental probability versus personal probability, and that kind of thing.

DeGroot: Well, you've certainly had a tremendous influence on the field and on individuals in the field. Everyone appreciates that.

Barnard: It's kind of you to say so.

DeGroot: I wanted to say thank you for your many contributions, and thank you for this conversation.

Barnard: Thank you.