

# A Conversation with F. N. David

Nan M. Laird

Florence Nightingale David was born on August 23, 1909 in Ivington, near Leominster, England. She received her degree in Mathematics in 1931 from Bedford College for Women. In 1933 she became Research Assistant to Karl Pearson at University College, and in 1935 she was appointed Assistant Lecturer in the Statistics Department, University College, London. She received her doctorate in Statistics from University College in 1938. During World War II, she served as Experimental Officer in the Ordnance Board for the Ministry of Supply, Senior Statistician for the Research and Experiments Department for the Ministry of Home Security, member of the Land Mines Committee of the Scientific Advisory Council and Scientific Advisor on Mines to the Military Experimental Establishment.

She returned to the Statistics Department at University College in 1945 where she was appointed Lecturer, Reader and then Professor in 1962. Beginning in 1958, she made regular visits to the United States, principally as Visiting Professor and Research Statistician at the University of California at Berkeley with the Department of Statistics, and the Applied Climatology and Forestry Divisions. She was elected to the International Statistical Institute, Fellow of the American Statistical Association, Member of the University Senate at University College, Governor of Bedford College for Women, and served as Review Editor for *Biometrika*.

In 1968 she became Professor and then Chair of the Department of Biostatistics, at the University of California in Riverside. In 1970 when the Department of Statistics was created, she became Professor and Chair of Statistics. She retired from Riverside in 1977 and moved to Berkeley where she continues to be active as Professor Emeritus and Research Associate in Biostatistics.

F. N. David is the author of nine books (a tenth is in progress, on the measurement of natural populations), two monographs and over 100 papers in scientific journals. Many of these are actively referred to today. *Combinatorial Chance* (with D. E. Barton) contains fascinating combinatorial probability theory (much like Feller's Volume 1) impossible to find elsewhere. Her *Tables of Symmetric Functions* contains a 50-page introduction that is still the standard reference. Her *Probability Theory for Statistical Methods* contains the only available treatment of "Lexis Theory," a forerunner of contingency table analysis. Her book, *Games, Gods and Gambling*, is widely recommended as an entertaining and authoritative account of the history of probability. Citations for these and her other five books are at the end of this article. Some of her 100 published papers 1932-1976 involve joint work with co-authors, including notables K. Pearson, J. Neyman, N. L. Johnson, M. G. Kendall, D. E. Barton, C. L. Mallows and E. Fix. To become further acquainted with her work, we can warmly recommend the following semester-long seminar: go to the *Index to Statistics and Probability* (Ross and Tukey, eds.), look up F. N. David and read through a selection of her papers.

This interview took place in the Jerzy Neyman Conference Room at University of California in Berkeley in July 1988.

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FIG. 1. *F. N. David in her office at the Forestry Division at Berkeley, circa 1977.*

**NL:** Tell me about your early education. Was it unusual for a woman to go to college in your time?

**FND:** No, I was born in 1909 which would make me five when the first World War broke out, and we lived in the country. I went to do private lessons with a British parson who said, "Well, you'll of course have

to know arithmetic so you better start on algebra. And you can speak English so you'd better start on Greek and Latin." So I learned Greek, Latin, and algebra and I went to school when I was ten. I had no idea that I wouldn't go to a university, so I went.

**NL:** University College in London?

**FND:** No, I didn't go to University College. My mother was having fits by this time about my going to London . . . disgrace and iniquity and that sort of thing, so I went to Bedford College for Women. It was the first college for women and it went back to I think somewhere around 1828 or something like that. It was strictly for women. I didn't like it very much, but what I did like was I went to the theatre every night. If you were a student, you could go to the Old Vic for 6 pence; that would be about 3 new pennies. So every night I went to the theatre. I had a great time.

**NL:** What did you study?

**FND:** Mathematics. I just did nothing but mathematics for three years, and I didn't like that very much. I didn't like the people and I suppose I was a rebel in those days but I don't look back on it fondly, even though they made me a governor of the college much later on.

**NL:** So then what after you left there?

**FND:** Well, I don't remember really. I wanted to be an actuary for some obscure reason. But the actuaries would only take men. And I asked around and people said, "What you want to do is to take the actuarial exams and then they can't do anything about it." One day I was passing University College and I crashed my way in to see Karl Pearson. Somebody had told me about him, that he had done some actuarial work. I suppose it was just luck I happened to be there. Curious how fate takes one, you know. We hit it off rather well, and he was kind to me. Incidentally, he's the only person I've ever been afraid of all my life. He was terrifying, but he was very kind.

He asked me what I'd done and I told him. And he asked me if I had any scholarship and I said yes, I had. He said, "You'd better come here and I'll get your scholarship renewed," which he did.

**NL:** So he took you on as a student then?

**FND:** Yes, a research student. Graduate student. I don't know that the idea was actually to do a doctorate. He just sort of thought that I wanted to come and learn and I did. So that was it. That was about '31.

**NL:** What exactly did you do when you worked with Karl Pearson?

**FND:** I went to lectures and various things. I produced tables of the correlation coefficient [*Tables of the Correlation Coefficient*, Biometrika Trust (1938), pages 44–59]. I estimated that I turned that hand Brunsviga roughly 2 million times. We used to use Brunsvigas and they would carry tens in one register, but they wouldn't carry tens in another. Before I learned how to manipulate long knitting needles, which was strictly illegal, I was always jamming the damn thing. When you jammed it, you were supposed to go tell the Professor and then he would tell you what he thought of you; it was really rather awful.

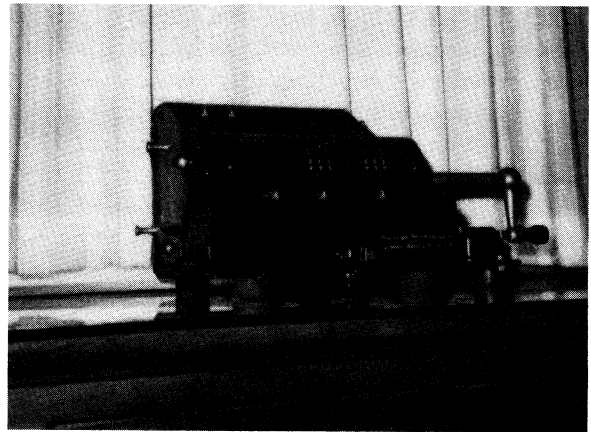


FIG. 2. This Brunsviga was presented to Karl Pearson on his retirement; F. N. David purchased it from his estate. It is now with Morris J. Farber at the University of California at Riverside, who received it from F. N. David at her retirement. Each turn of the crank would multiply a number one unit in a given register; multiplying a number by 29, therefore, required entering the number, turning the crank twice in the 10's register, moving the carriage to the 1's register and turning the crank nine times.

Many was the time I jammed the machine and had gone home without telling him.

**NL:** Did you work any with him when he retired?

**FND:** Yes, when K. P. retired, he went over to Zoology and I went with him. I had his sole attention for 2 years. I met almost everyone of importance in the statistical world because when they came to London they sought him out. So maybe that was a plus.

**NL:** One story goes that Pearson retired when his son [Egon S. Pearson] told him that he had gotten the degrees of freedom for a chi-square goodness-of-fit wrong. Is that true?

**FND:** No, no, I don't think so. I think he was just rather tired out, but he was full of beans when we got together again in Zoology. I don't think Egon ever argued with him about anything except perhaps the *Statistical Research Memoirs*.

**NL:** Now what was that?

**FND:** Well, they started a journal, Neyman and Egon, because K. P. didn't like their likelihood procedures and things. And so they started a journal and published what they thought. But they only published two volumes and then K. P. died and Egon took over *Biometrika* and *Statistical Research Memoirs* stopped. Neyman and E. S. P. (Egon) were quite a good pair, I think. I can remember Neyman coming into my office, laughing like hell one day, and he said "E. S. P. has got the stomach ache" and I said "Well, so what?" It was something I wasn't interested in. And he said "He is just going to tell his father about the foundation of the *Statistical Research Memoirs*."

**NL:** Did it anger Karl Pearson when they did that?

**FND:** I don't think so, I don't really remember, but I think it shows Egon just couldn't defy his father. He never really did anything very creative, but he had a very good critical brain.

Karl Pearson was an extraordinary person. He was in his 70's and we would have worked all day on something and go out of the college at 6:00. On one occasion he was going home and I was going home, and he said to me, "Oh you might have a look at the elliptic integral tonight, we shall want it tomorrow." And I hadn't the nerve to tell him that I was going off with a boyfriend to the Chelsea Arts Ball. So I went to the Arts Ball and came home at 4-5 in the morning, had a bath, went to University and then had it ready when he came in at 9. One's silly when one's young.

**NL:** When did the idea of getting your doctorate actually first emerge?

**FND:** Well, I don't think I wanted to take a doctorate, and in fact I didn't. You don't need it in England, or you didn't at that time anyway. But when Mr. Neyman came to London (that was later on), he said to me something about a doctorate and I said no. And he said, "Yes, you'd better have a doctorate." He was the internal examiner, and A. C. Aitken [developer of generalized least-squares] was the external. I took it by sending in 4 papers I had already published, which you were allowed to do. [Note: These papers served in place of today's Ph.D. dissertation.] I never thought of taking it until Neyman pushed me. He thought it was a good thing. But Karl Pearson didn't.

**NL:** Did your status change then when you got your doctorate?

**FND:** No, no. I was just out £20, the entrance fee. Pearson died in 1936. I worked with him until 1935, and then I went over to the Statistics Department. I forget what I was called. I suppose it's called an Assistant Lecturer. But I didn't do anything except conduct computer classes.

**NL:** Is that the department Egon Pearson was head of?

**FND:** Yes, he was head, and Neyman came in when Karl Pearson retired. I'm inclined to think that I was brought in to keep Mr. Neyman quiet. But it was a tumultuous time because Fisher was upstairs raising hell and there was Neyman on one side and K. P. on the other. And Gosset [who published the *t*-test under the pseudonym "Student"] coming in every other week because they [Guinness Brewery] were busy then transferring from Dublin to Port Royal. So it was quite amusing really. I think Fisher was without exception the worst lecturer I have ever heard. Karl Pearson lectured very well indeed; he lectured so well you would sit there and just let it all soak in. If you thought he'd make a mistake, you would say, "If you please" and he would say in a deep voice "Wait a Minute! Wait a Minute!" Then I went

to Fisher's lectures and his were awful. I couldn't understand anything. I wanted to ask him a question, but if I asked him a question, he wouldn't answer it because I was a female. So I would sit next to Churchill Eisenhart (later Statistician at the National Bureau of Standards) or Sam Wilks (Professor of Statistics at Princeton), who were visiting, and I'd say to them "Ask him!, Ask him!" Maybe it's a good thing to have a bad lecturer. After Fisher's lecture, I would go spend about 3 hours in the library to try to understand what he was up to. Perhaps I got more out of it than I did of Karl Pearson's because I was busy trying to understand where he was going. There were a lot of Americans there.

**NL:** What were Churchill Eisenhart and Sam Wilks doing there?

**FND:** Eisenhart came as a graduate student, and then he got his doctorate. I think Neyman was his internal examiner. Sam Wilks was just visiting. Helen Walker [Professor of Statistics at Teacher's College, Columbia University and the first female president of the ASA] was just visiting, I think.

**NL:** You once stated that you were interested in writing a history book about this period. What sorts of things had you wanted to record?

**FND:** Well, I think I am far enough away from it all now. I think the period between the 1920's and 1940 was really seminal in statistics and I saw all the protagonists from a worm's eye point of view. It's now been 50 years. I am far enough away to be able to see the pattern without having to take sides. I remember Gosset coming into my office. He was awfully kind to me. I was daredevil when I was young and I used to ride a motorcycle in cross country runs. I had a hell of a crash one day into a 16 foot wall which had glass on the top and I pitched over and hurt my knee. I was in my office one day and I was miserable and Gosset came in and he said, "Well, you better take up flyfishing," because he was an ardent flyfisher. He invited me to his house. There was him and Mrs. Gosset and various children in the house in Hendon. He taught me to throw a fly and he was very kind. There was him and three or four young men from Port Royal and we would go fishing sometimes.

Gosset was an extraordinary man. I think he was really the big influence in statistics. He would come in my office and I would be busy working on some problem or other that I had taken ages over, and he would look at the thing and say, "What are you doing?" and I would tell him and he would look at it and say, "Oh there's nothing there." Just like that. In a week I knew he was right [laughter]. Most extraordinary person. But he asked the questions, there's no question about that. He asked the questions and Pearson or Fisher put them into statistical language and then Neyman came to work with the mathematics.

But I think most of it stems from Gosset. I had enormous respect for him, he was a great man.

NL: Was he ever at University College?

FND: One year he took off, in 1906 when he did the standard error of a mean. He came as a research student of Karl Pearson's for a year when he did that. But otherwise he was all the time in the brewery.

Mostly he would visit the college during the moving period. Guinness moved its main brewery from Dublin to Port Royal in North London and so Gosset came from Dublin to Port Royal and back again and so on, while they were moving and he would come into University College and talk to friends. Or he would come and see Karl Pearson.

NL: He got along both with Pearson and Fisher?

FND: Yes. You couldn't quarrel with him [Gosset]. It wasn't because he was kind. If he put forward an idea and people started in on him, he would just shut up. He didn't care.

NL: What was it like being a woman there at that time? You mentioned Fisher's attitude toward women?

FND: University College was founded by Jeremy Bentham, among others, and the mummy of Jeremy Bentham all dressed up still sits in his sedan chair in the cloisters. It was founded for "Turks, Infidels, and such as do not profess the 39 Articles," because in those days to go to a university you had to say that you believed in the 39 Articles of the Church of England. So University College was founded, and a year after that King's College in London was founded as a Christian antidote. Women were admitted to lectures very early on at University College. So they theoretically had no impediment bar the usual male one. Fisher was anti-women, at the beginning at any rate. His last child was I think a daughter, and the story went round that one son remarked to the other son, "Father's had another failure."

NL: What was it like working with Neyman?

FND: I got on all right with him. He was kind, helpful and although theoretically I think nowhere in his life had he any prejudices, he actually used women rather than men to do the statistical-computational clean-up work. That was all right, but I think he also expected them to do the things that he didn't want to do. He always called me the Duchess because whenever he would ask me to do something, I would say, "No, I don't think I'll do that." He was good and helpful, but he had had a very different upbringing from the English academics of that time. He came into a closed English society. I think he fitted rather well into it.

NL: Before the War, at University College, everyone there was very involved in applications, weren't they?

FND: Well, Fisher, Neyman and Karl Pearson were. Neyman was busy sampling human populations. Egon Pearson was busy editing *Biometrika* after his father died. I don't remember what I was doing, probably anything that came along. I was friends at that time with J. B. S. Haldane and so I probably was busy with something genetical. Haldane was a funny fellow; he didn't have much mathematics, but he had an enormous intuition as to what some experimental data ought to show and he would tell you about it. But he was also vain so when you came back and told him it was rubbish, he wouldn't speak to you for 6 months. I liked him except he was much too left wing. We both belonged to the Fabian Society, the socialist group that G. B. Shaw and others started. But they were so hot airish that I got fed up with them after two or three years.

NL: What about Karl Pearson, was he heavily involved in applications?

FND: He was always the same, everything he did was to solve a practical problem. I think that's true. He was, and is, a great man. He wove together all the work done by Quetelet and by Poisson. He had great admiration for Laplace. And he wove it all together to try to make practical sense. He didn't agree with hypothesis-testing, but I am not alone in thinking this is greatly overdone nowadays. I mean, hypothesis-testing should only really serve you as a guide for practical action rather than be the final answer.

NL: Tell me about your work during the war.

FND: I was called up as an experimental officer to the Ordnance Board in the end of June 1939; I worked for one year I think, maybe one and one-half years, with them.

NL: What was the Ordnance Board?

FND: Well, they were busy at that time analyzing what happened when you fired guns. I remember doing an investigation which was something to do with anti-aircraft against German bombers going up the Thames. But I got rather bored with this.

NL: Why?

FND: Well, I didn't seem to be doing much good. Anyway, I transferred to Ministry of Home Security with Bradford Hill. I started off there with counting corpses, and various problems like what happens when you have a bomb fall on the road, where's the safest place to be when the bomb falls, all this sort of thing. In fact after about a year, I knew more about what a bomb would do than a great many other people.

NL: So you analyzed data to try to answer these questions?

FND: Yes, we looked at the data but it was pretty silly. You couldn't really get any decent data. A bomb falls in the road in London. What happens? Well, everything runs down the middle of the street. So, we



had sewers cut, the water cut, the electricity cut, the gas cut. Then you have the business of having to have something done about the water pipes because they're probably contaminated by the sewer. And at the end of the day you say to the gas people, "How long did you take over so and so and how much piping did it take to do something or other? And they'd say, "Oh, well, we don't know. We sent out two men with a cart, and told them to just go on until they finished." So you never got any reasonable data. I was then seconded to the Land Assault Wing, and so I worked with them for a bit. I was busy investigating the effects of land mines and that sort of thing. I worked also with the American scientists on that because land mines were a big problem for them, as well as for us.

And then the V-bombers started, and I got called back. There was a map in London on which was plotted where the bombs had fallen and the problem was to figure out where they were coming from. Lots of scientists were working on it. I assumed a normal bivariate surface and found the direction of the major axes.

**NL:** Did it work?

**FND:** Well, we found they were changing the silos that they fired them from. Probably they changed about once a week. You see you only had four minutes I think from the time you got the warning to when it arrived, or something like that. And you would have to send out your patrol, and as soon as they started firing someone would drop something on them, so that if they knew where it was coming from they would stand a better chance.

And then the war stopped on V. E. Day. I went back to University College. I was very lucky. Because I'd gone in so early and V. E. Day was the beginning of May, I said, "Very well, I'm going at the end of the month, I've had enough of this stuff, I've wasted six years," and it was agreed. On the 3rd of June a request came from the Pentagon that I should go there, because they wanted a task-force to report on the atomic bomb. I got chased up, but I said, "No, no, you said I could go at the end of May, good-bye." I missed all that, which was absolutely marvelous. I went back to supervising the library, coming back to the Department of Statistics from where it had been during the war. Everything had been evacuated. The libraries were hidden, a lot of them in caves in Wales. Some libraries never recovered their complete collections.

After the war, I was still working in an advisory capacity with the Land Mines Committee of the Scientific Advisory Council. Mines had been put down over the beaches, in case the Germans came, and most of them had been removed. But some of them they had difficulty with. You see, when you were fighting with tanks in the desert, and you holed up for the

night, you put mines around you and then took them out the next morning. The Germans were very methodical, at least it looked like it from the plan that I saw. Given that you found one mine, it was easy to find all the rest because they were laid in a hexagon; they had to be more than 3 feet apart, but they were laid in a hexagon pattern. The British were doing the same, and then some genius hit on the idea of using random patterns. He had ropes A, B, C, D, E, five different ropes, and used random numbers to locate the mines. The ropes were the same lengths, but the knots were tied at random intervals. So you take 3 feet, add a random number on and then you gave a rope out to the chaps who were laying the mines. You laid Row A, and then Row B, and so on. And nobody could possibly crack your minefield because they couldn't figure out the pattern. You put the rope down and you put the mine in at the knots.

The same thing was done on the British beaches. But on the beaches in Norfolk Sands, which is one of the places it was thought the Germans might be coming, they forgot to record the pattern. It was quite a job. A friend of mine got blown up but not before he had a bright idea. He suggested they take a high power fire hose and wash the beach. Wash the soil away and expose the mines.

**NL:** So after you went back, after the War, what was your position then at University College? By then Neyman had left.

**FND:** Oh, Neyman went in 1938. In 1945, I was then up for what I think you would call an Associate Professor, and that went well. There were about 14 people for three positions, so I was fortunate.

**NL:** When did you first come to the U. S.?

**FND:** I was flown over in one of the American bombers to Andrews Air Force Base. I came over to have a look at the first big digital computers that they built. This would be about 1944. I'm a bit vague about the date. This computer was screamingly funny by our present-day standards. You would laugh yourself sick if you'd seen it. It was a Nissenhut, about a 100 yards long, and down the center were a whole lot of duck boards, on bits of wood you could run on. Either side, about every few feet, there were two winking monsters and in the ceiling was nothing but fuses. Every 30 seconds or so the GI would run down the duckboard with his face up to the sky and push in a fuse. I think this was the very first one that was built and it was wanted for calculating trajectories of weapons. It was quite fun and when I got back I was telling somebody about it and so on and then they said, "Well, you better sit down and learn the language." And I said, "Not on your nelly. If I do that that's all I will ever do for the rest of my life and no I'm not going to, somebody else can!"

NL: Then your second trip was after the war when you came over on a ship.

FND: Yes, Mr. Neyman was wanting to get me over. The ship came in to New York. I stayed a few days with Helen Walker, and met Wald at Columbia. I taught at Berkeley for a summer session . . . that would be '48. But I was rather disheartened . . . this country was being very "We won the war, you know." I was greeted with things like "Welcome to the greatest country on earth, that has just won the greatest war," and so on. And I didn't like it. You see Churchill decreed that the scientists should work 7 days a week, 10 hours a day, 70 hours a week with NO VACATIONS . . . so I had just wasted 6 years of my life and they wasted only 3 years of theirs . . . so I thought "no, thank you." So I went back.

NL: So you went back to England. Who were some of your students then? Is that when D. E. Barton (later Professor of Computer Science at University College) was one of your students?

FND: Yes, that's right. I also had P. L. Hsu [later Professor of Mathematics at Peking University], and Colin Mallows, who is now at Bell Labs.

NL: And who were some of your colleagues at University College then?

FND: Pearson was the chief honcho; Norman L. Johnson [later Professor of Statistics at the University of North Carolina and co-editor of the *Encyclopedia of Statistical Sciences*] was a student and later on the faculty; H. O. Hartley was down in Texas [later, as Professor of Statistics at Texas A&M University]. There weren't very many of us. I think perhaps four.

NL: When did you and Barton write the book on combinatorics?

FND: All my life I have had this beastly business that I start something and then I get bored with it. I had the idea of combinatorics and I had worked on them for a long time, long before I knew Barton or taught Barton . . . but I took him on because it was time that the thing was finished. So we got to work and he did all the fancy work, proceeding to limits and things like that. He was a good chap. We wrote quite a lot of papers together, Barton and I, Barton and I and Mallows.

NL: You collaborated a lot with various people during that period.

FND: I must have been very tiresome when I was young. I would come into university and I would stop anybody and say, "Have you ever thought about so and so?" or some darned problem or other and if it was a student he might say, "Oh, no, isn't that interesting? Can I take over that, can I do it?" or some colleague would say, "Yes, you ass that is so and so and so and so . . ." But occasionally they would try do something.

NL: You worked on tables of symmetric functions with Maurice Kendall.

FND: Yes, he was at the London School of Economics. There are a whole lot of colleges in London and they are all University of London but there isn't much intercourse between them. He started something I think and published it and I thought it was rather hogwash so I looked him up and I got him interested. I think he had resources and I hadn't, so we both invented ways for calculations of these things and he would check them and then he would get his people to work them out. I have been interested in combinatorics I think because I am lazy. Well, I'd spend a long time thinking out a quick way for doing things.

But there were a number of interesting people about that time. We had military service in England up to '52, '53 or something and I think those chaps that I taught between the end of the war and '50 were probably the best lot I ever had. They knew they had wasted 6 years. There were these 6 years to catch up, and they worked like demons. And this was true also in the U. S. When I came here (Berkeley) in '48 I had a fairly big lecture theatre and I must have had 30 or 40 young men come up with a baby on their arms saying would I mind if they brought the child to class and I would say, "Oh it's quite all right but no screaming, if it screams, out with it," and every so often all through the class there would be a sudden scurry . . . father with baby and a yell from the corridor. But they would all be working like hell and they were awfully good chaps. Maybe it takes 5 years in the military or something to tone the present lot down.

NL: What about your book *Games, Gods and Gambling*. When did you write that?

FND: '62, I think. I had lessons in Greek when I was young and I got rather bored with people talking about dice, and I got interested in archaeology when I had a colleague in archaeology who was busy digging in one of the deserts, I think. Anyway he came to me and said, "I have walked about the desert and I have plotted where these shards [pieces of pottery] were. Tell me where to dig for the kitchen midden." Archaeologists don't care about gold and silver, all they care about are the pots and pans. So I took his map and I thought about it and I thought this is exactly like the problem of the V-bombers. Here you have London and here you have bombs landing and you want to know where they come from so you can assume a normal bivariate surface and predict the major axes. That's what I did with the shard map. It's curious there's a sort of unity among problems, don't you think? There's only about a half a dozen of them that are really different.

NL: How did you get interested in the history of probability?

**FND:** Well, I can read. The scientists were digging up curious bones and I would read their articles and those to which they referred. From their writings and allied literature, it seemed that the priest knew how to manipulate the bones. As a suppliant wanting to know the future, you went the first time and you threw the bones, or the priest did, you paid your cow, and he said go home and go to bed. But you didn't like this. So he said, "Have another go." And you paid your cow and it turned out good. I'm pretty sure that all this business about arithmetic and probability also came through the silk road down from China. I think it came down through Tibet where you have the first inklings of bone throwing. For some reason or other it crossed what must be Iran now but it went north in the Mediterranean as opposed to south ... It didn't go to Egypt, because that arrived with Ptolemy. I always wanted to go down the silk road from Peking and see what they have in the monasteries, because I think the monasteries held the learning. All over the world, the learning was in the church to start with.

**NL:** When did you first get interested in forestry and start your connections there? Is that after you came here [to Berkeley]?

**FND:** I was here in '58 doing summer school and while I was here the scientist running statistics in the forest service asked me to be a consultant. Then I got a lot of questions about fire spread in forests, although I only knew about it in cities. I don't remember that there was very reliable data. At that time, there was an historian at the fire who was supposed to write everything down that was going on, but when the fire got really bad the historian was out on the fire line, so they didn't really get much data at the crucial times.

One interesting thing was they hadn't learned the lesson that when a fire is going up a slope, go to the top, don't try to fight it. You see it is the good old chimney effect. Anytime that the slope is more than



FIG. 3. F. N. David with Jerzy Neyman and Evelyn Fix at Berkeley, 1960.



FIG. 4. F. N. David with Elizabeth Scott, David Blackwell and Evelyn Fix (all Professors of Statistics at Berkeley) at Berkeley, 1960.

15 degrees ... up comes the wind and the fire goes fast and the only chance you've got of stopping it is to sit at the top and try to get it as it comes up.

**NL:** Were you involved with actually doing experiments with forest fires?

**FND:** I didn't do much. I tried to get them to plot fairly accurately what was going on. I tried to get university people who lived all around the place to read the wind every morning before they came to work because if they had a forest fire in Riverside and you asked what the wind was they would give you the direction of the wind at March Air Force Base which was about 20 miles away. But I never really got anybody going about it, and I never had the time to give them a push.

**NL:** So you made quite a few visits on a regular basis to the U. S. after the war?

**FND:** Yes, starting in '58, once I got over my feeling that really I had wasted my life and I really didn't want to waste any more of it.

**NL:** But you remained at University College for a while after the war.

**FND:** I became a Professor at University College in '62. In universities, in those days anyways, you had one Professor in the department and then you had quite a lot of Lecturers [which are equivalent to Associate Professors now] and then you had the Assistant Professors who are Assistant Lecturers. You couldn't usually be a Professor unless there's a chair. I was made a Professor, without having the chair. It was very difficult to get. I was the second female. I was quite pleased about that. My friend Elizabeth Wilkinson, who was a German professor, I think was the first. But I noticed that the usual age that it happened with men was about 40, the usual age it happened with women was later.

**NL:** How did it happen with you? What made you successful?

**FND:** Well, I don't know. Egon Pearson had retired in about 1960. I had many more publications than anybody who was likely to be asked to take over,



but they brought in Maurice Bartlett, a good man. He thought I would up and go, but we talked and I stayed. I could have come to the States at any time. A lot of people knew me. In '67 Bartlett left. He went to Oxford. I was offered the chair but at the time I had committed myself more or less to going to Riverside and I thought, "Let's do something fresh."

**NL:** Were you angry that they didn't offer it to you in the first place?

**FND:** No, no. I felt peeved because they didn't perhaps but Bartlett was a decent person besides being an eminent statistician.

**NL:** So what made you decide to come to the U. S.? You made that decision after Bartlett came and after you became a Professor.

**FND:** '62, I think, to '67 I was at U. C. in London as a Professor, yes. '67 I came to Riverside.

**NL:** Tell me about Riverside.

**FND:** Well, I think it was a mistake my deciding to come to the United States at all.

**NL:** In what way?

**FND:** Well, I would have been better off quietly dying in England, I think.

**NL:** Better off for who?

**FND:** Myself [laughter]. I got in the middle of a battle in Riverside which really was fantastic. I had no idea things like this went on.

**NL:** What was the battle about?

**FND:** Well, I went there and there was nothing. [Note: A proposal for a department was apparently in the works at the time of F. N. David's recruitment. Approval was granted for a Biostatistics Department in 1968.] And I thought I was going to have a quiet life. Then by request I started teaching statistics. Riverside was at that time a very small campus; it had about 4,000 people, I think, and they were just starting up. So I was successful, I'm quite a good teacher . . . and after that I was teaching enough for them to start a department. They wanted to call it Biostatistics or Applied Statistics and I wouldn't have it. I said, "No, I won't call it mathematical and I won't call it statistical," so (in the end) we were Statistics.

Then the mathematicians tried to swallow me. I fought them on the senate floor and all over the place. And various other departments thought it would be a good idea. You know I ended up, this always made people laugh, I ended up a Professor of Statistics, I was a Professor of Economics, in all I had 5 different titles. They were all trying to get a statistical monopoly.

**NL:** You are legendary as a teacher.

**FND:** Yes, you know it's the one thing I don't like doing.

**NL:** Well, you were so successful at it.

**FND:** To be honest, I loathe it. In my young days it was a necessary irritant to me to get on with my

research work. I would get into the university at 6 in the morning and I would think, "Oh, good, I just got three hours I can sit down before my beastly class at 9:00." Now I find I haven't got to teach, so I tend to be very lackadaisical. I think the thing to do really when you are teaching is to try to put yourself in the students' place and to discover with them how to do it. I used to walk around the class talking, talking, talking. I used to talk about actual things I had running through my mind like, given that you have a certain number of people convicted of crimes every year and this is increasing, how many prisons should you build given that it takes you three years to build the prison? This sort of thing. I was doing this 15 years ago; it seems very right today. But one talked about problems that had actually happened in relation to the particular thing you were teaching. I have done a lot of things in my time, I suppose.

**NL:** You were at Riverside about 10 years. But during that time you maintained connections with the department at Berkeley?

**FND:** Ah, well, I was a joke, I think. I had two cars, one here and one at Riverside, and I thought, "Well, 4 days a week at Riverside, about 12 hours a day is enough." So I used to come up to Berkeley in the evening, spend Friday either here or at forestry, and go back again to Riverside on Sunday night. I always had lunch with Mr. Neyman on Sundays, and anybody who was in the neighborhood or passing through was also at lunch, so I saw a lot of people; gave seminars occasionally, gave various conferences, but my time there was really very painful. I really didn't like fighting, it took such a lot of time and effort. There's an awful prejudice in America, overt prejudice.

**NL:** Yes, in what way?

**FND:** Well, academicians will always go for [attack] a woman if they can, rather than a man. It's also true in England, but it's not overt. I had to deal with that prejudice. Female assistant professors would come to me and weep because I was on the President's University Committee for Affirmative Action. I used to be very nasty to the complaining person. I used to say, "Well, you know what you have to do is work and work and show yourself and other people that you can do it." Perhaps she'd go away and try. And then I would ring up the chap who was doing it and say, "Do you want me to attack you on the senate floor?" That sort of thing, but I got so bored with it. Such a waste of time. And one met it all one's life. I remember the first time I met it was when I was quite young and I went home raging to my father about it.

And he said, "Oh, you'll meet that all your life. You better get used to it. Now get on with your work." I got turned down for some jobs because they said that they hadn't got facilities for women to use the toilets.

**NL:** What do you think about women today in academia? Are things any better?

**FND:** Difficult to say. I think yes, probably. I am rather turned off by the open aggressiveness. It's better to work hard and to just show that you can. Overall I think it's a lot better. I get rather tired of women who think that they ought to be given preference because of affirmative action. I always say, "Well, in my day you had to do 200% better than the nearest man they could possibly give the job to before you got it." Now I think it's come down to about 150%. But I still think there is prejudice. There always will be.

**NL:** What did you do for your junior faculty at Riverside, to help them get started in careers?

**FND:** Well, I don't know, let me see. I had the idea that it would be a good thing if you have three or four main core people who would teach statistics, but you also wanted people who were half statisticians, half entomologists and this sort of thing so that they would understand the problems and be able to attack them. So when I taught statistics courses to (say) entomologists and the brighter ones would come and talk afterwards, I would tuck their names away for future reference. I had two young Englishmen who were over here visiting and who wanted to stay, at different times, so I recruited them. I had difficulty with them, of course, because some sort of nonsense goes on where names have to be submitted to the Department of Labor in Los Angeles, and if they think there is an American more qualified for the job, he has to have it. What did I do to promote them? Well, I don't know, I guess just to make them work. Riverside is difficult because there was nowhere to go to eat or anything. So we used to bring bagged lunches and then people would talk over lunch and we would all be critical and suggest ways they could do things and this sort of thing. It was difficult there. At University College they had a society for scientists on the faculty but they wouldn't have women, so I founded another one which would have both men and women. We would get young scientists in and the old rednecks were rather upset. But it was difficult. You couldn't get that sort of thing going at Riverside. It was too scattered for there to be easy exchanges of the conversational type.

**NL:** You are perceived by the people at Riverside as being influential there.

**FND:** Oh, I think I was. But I don't think my job in life is to be influential.

**NL:** What is your job in life?

**FND:** To ask questions and try to find the answers, I think.

**NL:** So you left Riverside at about '77.

**FND:** I taught a course for a quarter in Berkeley on Fridays when I was still at Riverside, and I went on with it a bit when I came back, but I didn't like it



FIG. 5. F. N. David with Nan Laird and Elizabeth Scott at the Women's Faculty Club at Berkeley, July 1988.

because it became too popular and it got out of hand. There were about 450 in the class and you couldn't really keep it under control. So I gave up that one. I gave it up quite happily because I didn't like teaching. When you become a sort of favorite sport of the football fans, you know that you're doing something wrong.

**NL:** What are you working on now?

**FND:** I am stuck at the moment on a book, Griffin is going to crucify me, on the measurement of natural populations. I got fed up with this business of the endangered species act. They talk a lot of rot, these beastly birders and fishers and whatever you call these people who go after rare species. What set me off was I found that in London they were building a new road to Heathrow Airport and the ecologists got to work, saying you can't have it there because it goes through a meadow and there is a species something or other there and this, that and the other. So it cost about a million pounds to divert the road. Then I was being driven to London Airport and on the new road were these plants, these same flowers that those stupid people hadn't bothered to look for. This is what makes me mad. So I started in a warfare about endangered species. You may think it is easy to count birds, all right, have you ever tried?

**NL:** No.

**FND:** You just sit there and you can't count the beastly things as they just fly by, not with any ease. The usual way of counting is to walk through the forest and every so often stop and count the birds you see. Or you listen for a bird call, or go out at night with a bat locator and every 50 yards you will stop and listen to the bat locator, or you might drive down a road and every 50 yards you stop and get out onto the dirt road and look and see if there's a hawk. It never seems to occur to people that birds have curiosity. You can walk through a forest in England and you'll see a little blue tit bird with a white patch, he is unmistakable, and you walk 50 yards and then look

up, and there's the bird with the white patch. He's clearly following you to have a look. He wants to know what you're up to. And it never seems to occur to people that they might duplicate counts, never seems to occur to them the fact of human intrusion disrupts the birds; some of them are afraid and they go and hide. There's all sorts of errors to fall into, apart from duplication of observations. It makes me mad, because people come back with rotten data. They get hold of some mathematician who invents some lovely mathematical theory and then they draw all sorts of conclusions. It's all rubbish. The conclusions drawn are only as good as the data on which they are based and the data is not worth calling data.

I say you should select spots in the forest, say, if you're doing birds, far enough apart so that the bird cannot fly from one place to another, that you take a spot, sacred oak in the middle, and you go out to that sacred spot once every year at a fixed time after hatching because the activity of the bird depends on the length of time since hatching. And the same time of day because it probably depends on the sun. And you stand there at the sacred oak for an hour not moving so that the birds get used to you and then you spend one minute as you turn around through 360 degrees, counting what you see, and you do that every year, so you can tell after 5 years whether your population is going up or down by the numbers you count. I can't get them to do it. Same thing with fish jumping. Do you know what they do when they count fish? You put on scuba gear and go around the ponds. It never seems to occur to them they could count the same fish twice. I want them to make artificial weirs, six inches high, put it across the river.

**NL:** What do you think about Statistics today? What would you like to see happening?

**FND:** I'd like to throw the normal curve out the window, throw out hypothetical populations, like to have people sit down with data. Supposing you just say, "Well, all I am prepared to assume is randomness, and I am not entirely sure that that is good." I rather despair of a lot of the stuff that is going on . . . I know young men and women have to write because if you don't have publications behind you, you don't get promoted and you don't this, that and the other . . . I know all that, but I am sick of all this mathematical trivia that is taking up the pages of journals. I mean theorems all over the place and who cares? You don't really know if you want a normal curve, let alone what rate you're approaching it. I expect I was as bad as anybody when I was young but it is in a way rather awful, you know.

**NL:** Neyman himself, although you said he did the mathematics, was interested in applied work.

**FND:** Yes, but he hadn't got a practical sense. He would try to learn and I will say he recognized the

need for knowing the practical ins and outs, but he hadn't really got a practical sense. He was able mathematically, although by the time he had come to the States he had really done his best work.

**NL:** In terms of your own contributions, which are the ones that you are fondest of?

**FND:** None. Three weeks ago I had a bright idea—no, it was more than that as I was on a plane, going to London in April. I had this idea and when I got to my house I worked like the dickens for 3 weeks and then I suddenly thought, "You idiot, you published that 40 years ago." That's what happens when you get old. I don't like any of them . . . I don't remember many of them. I wish I had a bit more concentration nowadays. So many things I want to do. It is difficult. My mother died when she was 106 and she used to say to me, "Never live to be 100, dear." And now I know exactly what she meant.

**NL:** What about the training of young statisticians?

**FND:** When I was in London, I had a number of friends in business and industry and every summer I used to send the students off to work by arrangement. They were paid at ordinary rates and they had to behave and work and learn and talk to people. They were trained in the sort of world they would encounter when they went out. Actually they all did very well because when they finished they all went into those things as permanent jobs. But I think the important thing is we teach them a lot of theory and it's fun, it's rather simple mathematics, and then they go out and they haven't the slightest clue really of how to bend what they've learned to the job. They used to come back and at the beginning of the quarter they would tell one another or give talks about what they had been doing and I thought that was really a central part of their education.

**NL:** Do you have any advice for practicing statisticians today?

**FND:** I think the essential thing if you want to be a good statistician as opposed to being a mathematician is to talk to people and find out what they're doing and why they're doing it.

**NL:** Thank you, Professor David!

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