JOSEPH DALY and CHURCHILL EISENHART

This is an interview on the 10 July 1984 with Joseph F. Daly and Churchill Eisenhart at Mr. Daly's home in New Carrollton, Maryland. The interviewer is William Aspray.

Aspray: Why don't we begin, Mr. Daly, by asking you how you came to know Sam Wilks.

Daly: Well, it was a rather interesting time to be at Princeton. I went up there in the fall of '35, and at that time there were all kinds of different things going on at Princeton. There was differential geometry, because [Luther P.] Eisenhart, T.Y. Thomas, Einstein, [M.S.] Knebelman, and Walther Mayer were all working in that field, and everybody was excited about relativity theory. Then there was a lot of algebra going on. I had done my degree in algebra at Catholic University, and [Alfred] Clifford and some others were holding seminars in algebra. But the big drive at that time, the thing that challenged all the young graduate students, was topology. That was the beginning of the topological era there. I guess even Veblen and his people, still working on spinors and things like that, were getting over into topology. There was [James] Alexander. And the one that really awed the graduate students was [Solomon] Lefschetz. He had everybody frightened.

When I first came there, I had been working somewhat in differential geometry, so I took up with T.Y. Thomas for a couple of years. There was quite a bit of tension between Thomas's students and Lefschetz's. I don't know just why, because they both turned out to be perfectly good people to work with. I enjoyed both of them. After I had worked for a couple of years with T.Y. and Walther Mayer and people like that, T.Y. decided he was going to take off to the far West, where he could have a lot more fun, I guess. So I was left kind of stranded. I had been sitting in on Sam Wilks's classes, and I guess I knew something about statistics even before, because I had worked on it a good bit at the Catholic University.

Aspray: Which courses of Wilks's were these?

Daly: I took all the courses that he gave; he gave only one each year. We students helped write his notes. It was a real interesting time. As I say, the main things that were going on at Princeton at that time were geometry and topology, and it really took the kind of energy and push that Sam had to make any impact at all with a new field like statistics. He had really no support anywhere. He had no way to attract students or anything else except his own personality, his ability to somehow make you feel that what he was doing was important. This was at the time when not too much was said about statistics. There was the general notion that people had, that statistics was the gathering of information and so on, but there wasn't any real theory of statistics at that time. But Sam came back from England with quite a lot of knowledge of what was going on in the field of shortest confidence intervals and optimal allocation, with all kinds of ideas that were relatively new and had quite a mathematical flavor, rather than the governmental type of statistical flavor.

Aspray: In this article of Churchill's [Churchill Eisenhart, "Samuel S. Wilks and the Army Experiment Design Conference Series", *Proceedings of the Twentieth Conference on the Design of Experiments in Army Research, Development and Testing held at the U.S. Army Engineer Center, Fort Belvoir, Virginia, 23-25 October 1974* (ARO Report 75-2), Research Triangle Park, North Carolina: U.S. Army Research Office, June 1975, pp. 1-47], he gives the impression that research in statistics was going on at a number of places at the time. Maybe the two of you can tell me exactly where the centers of statistics-research were.

Daly: There wasn't too much awareness of that in this country. As I say, nobody really knew what statistics research was going on. There were people that were doing statistics, but you did't hear about them. At Princeton, of course, it was Sam. At Columbia things were beginning to move just a bit. Harold Hotelling was quite active in the field, but his was still a classical, old-fashioned type of statistics, although he was beginning to develop the more mathematical type. We weren't aware of the things that were going on at Iowa State College. I don't know when G.W. Snedecor came there. Those I guess were the three primary places.

Aspray: In the U.S. What about in Britain?

Daly: Of course in England you had Rothamsted, in other words you had R.A. Fisher and Jerzy Neyman.

Eisenhart: One thing that interested me very much when I was out at Iowa State was Herb Davis's talk at this semi-centennial of theirs. [See H.A. David, "The Iowa State Statistical Laboratory: antecedents and early years", *Statistics: An Appraisal* (Proceedings of a conference marking the 50th anniversary of the Statistical Laboratory of Iowa State University, Ames, Iowa, 13-15 June 1983), Ames, Iowa: Iowa State University Press, 1984, pp. 3-30.] Joe has just said that we didn't know what was going on at Iowa State. Now I knew a little about what had happened at Iowa City, since that is where Sam had come from and where Rietz was. I knew about Rietz because Dad had given me his little book, a tiny book, one of the Carus Monographs [Henry Lewis Rietz, *Mathematical Statistics* (Carus Mathematical Monograph No. 3), Chicago, Illinois: Open Court, 1927] But I didn't know anything about Iowa State, and the thing that was really surprising was that in one of the letters that Snedecor wrote to Fisher in 1936 or something like this, he said, "I hope that you are taking young Eisenhart under your wing, so that he learns statistics from the viewpoint of practicality and usefulness on the job, rather than just as an exercise in abstract mathematics." Now I didn't know that Snedecor even knew I existed.

Daly: We found out all these things later. While we were graduate students, we didn't really have any reason to know about these activities. But Sam seemed to keep track of everything that was going on. I don't know how he did it. I don't remember him mentioning any particular places, but lots of ideas were around, like the notion of best tests. I don't know where Abraham Wald was at that time, but Jack Wolfowitz was around. At the statistical society meetings we would encounter all these ideas. I guess we were right then at the beginning of modern statistics. The notion of risk functions and ideas of the type that eventually developed into the Neyman-Girshick-Blackwell approach to statistics were just beginning to dawn on us. Most of the time we were concerned with distribution theory. That's what Sam started out in, the distribution of correlation coefficients. He was a master of juggling determinants, and ratios of determinants, and all kinds of complicated functions. I guess I got into it partly because I knew something about that sort of thing from what I had done in algebra with a student of MacDuffee's, and from the work I had done in differential geometry. So the tensor notation and the ability to juggle matrices was there, and as a result, I guess, Sam decided he could take me as a student and maybe make me learn something.

Eisenhart: At that time the tools of statistics were evolving. In one of Sam's papers, by what the abstract says, he did something by integral equations, where today we would do it by using characteristic functions and Fourier transforms. So that some of the language he used in describing what he did is unfamiliar to us today. He had worked, you see, with Hotelling before he went to England.

There is a funny story, if I can put it in here, about Hotelling. Hotelling, you see, came to Princeton in 1922 or something like that as a graduate student. I understand that he came thinking that the Veblen in Princeton was Thorstein Veblen. He wanted to work with this great economist, you see. Then he found that it was the other Veblen, so he did his work in geometry with Veblen and my father. It was while he was there that he wrote his paper on the distribution of the correlation ratio [Harold Hotelling, "The distribution of correlation ratios calculated from random data", *Proceedings of the National Academy of Sciences*, vol. 11, no. 2 (October 1925), pp. 657-662], which is essentially the same thing as the distribution of F. If you take the numerator and denominator of F and put them both down in the denominator and leave the numerator up there, you get the correlation ratio. He did that when he was there in Princeton, which was the start of his statistical career, and then he went back to Stanford. But he kept pestering my father all the time about statistics; he sent him all his papers and so forth. Dad apparently had been pestered earlier by Sam Wilks' first teacher in Texas, E.L. Dodd, who wrote on theories of all kinds of means. When I got into the subject Dad had a wonderful collection of reprints of Dodd, and then apparently because [Thornton] Fry had been down there, Dad had a big collection of Shewhart's stuff. Then when Sam came ...

Daly: Sam had a knack of bringing out ideas that just weren't in the literature at that time. I'm sure not very many peóple realized, for example, that the analysis of variance, which was the big thing then, and the theory of linear regression are exactly the same thing. Sam made that perfectly obvious to me. It didn't become obvious to a lot of people until many years after that, I guess because you'd keep getting articles in these two fields as though they were entirely different. The notions of statistical tests, unbiasness, and optimum procedures were just beginning to develop, but we had a lot of fun with them. And Sam managed to attract some interesting students. We had George Brown. We had Alex Mood. He was working not only in distribution theory, but also in theory of runs, and in statistical tolerance limits, which has to do really with quality control, deciding what part of the distribution would lie between two percentile points. So that sort of thing developed, and it was a natural development commencing just before the war.

Aspray: Were these students that worked under him Ph.D. candidates, or were they just doing some research under his direction?

Daly: Well, it's awfully hard to give the flavor of Princeton at that time. Nothing was really quite that formal. You worked with the people, you saw what was going on, you sat in on seminars with Al Clifford on one thing and with Veblen and his people on something else, and eventually you settled down with one particular professor. Alex Mood and George Brown did get Ph.D.s I understand; I guess they were a little after my time. But it was a question of attracting students to really work in these fields instead of just working on something that was fashionable. And Sam had the knack of doing this. I don't know where he got the energy. He wasn't very social. He didn't go around shaking hands and patting people on the back, but he always had things going on. Somehow he organized the Institute of Mathematical Statistics, and he got involved in all kinds of governmental activities. I don't know where he ever found the time to do it.

Aspray: How was he as a teacher?

Daly: Crystal clear as far as I was concerned. It was obvious what Sam was doing when he was talking about it. The notation must have been pretty formidable, and the procedures weren't too well understood at that time, but I could always tell pretty much what Sam had in mind.

Aspray: Other people have told me that there was a real variation in the quality of teaching at Princeton.

Daly: Well, it ranged all the way from Wedderburn, who was absolutely opaque ...

Eisenhart: Yes.

Daly: Then there was Luther Eisenhart, who had a real trick. He could get the guys so aggravated that they had to try to prove how stupid he was by reading the book faster than the course was going. Of course Eisenhart had written the book, but he could always tease his students into thinking that they knew more than he did. We learned more from him that way than we did from Bohnenblust, who gave the most beautiful lectures, just as clear as could be. So you understood all of it at the time. You went back to your dormitory, and the next morning it was as though you'd never heard it before. It was just too easy; you never ran into any problems in his lectures.

Eisenhart: Everything just worked perfectly.

Daly: Yes, the stuff never got home.

Aspray: That confirms stories I've heard from half a dozen other people.

Daly: Von Neumann I never could quite figure out. He was just too fast for me. That was a tremendous place. You had von Neumann, you had Einstein, you had Veblen, you had Knebelman and T.Y. Thomas and Al Tucker. Everything was going on at once, and the real problem for graduate students was to keep from getting so diverted into 16 different fields that you didn't get anything done. Then there were all these National Research Fellows around. C.B. Tompkins was talking about computers and all sorts of weird things that we never thought about. It started out being a very fearsome and frightening place, but because they had tea every afternoon you met all these people, people not only from the Math Department and the Institute for Advanced Study, but also from the Physics Department. There was Condon, Wigner, and all the guys who were fooling around with mass spectrographs and chasing positrons and I don't know what all. But it was a wonderful place to be, and you couldn't help absorbing some of it no matter how dense you were.

Aspray: Can you elaborate a little bit about your comment before about having to encouage people to go into statistics, partly because it was a new field and there were all these other, well-established fields with important research problems. You're certainly welcome to add anything also Churchill. What was the attitude on the part of the rest of the faculty toward statistics? Hostility? Indifference?

Daly: I don't think there was any particular attitude. There was no hostility. I think that probably the topologists felt it was a waste of time for anybody to study anything but topology, because that was the thing in those days, but I never encountered any real opposition. Lefschetz was as pleasant as could be. In comprehensive exams, there was some tension I think between differential geometry and topology, but not against statistics. In fact, nobody really cared much about it except Sam. But it was obvious to people like me and Alex Mood and George Brown that there was something here that was worth learning.

Aspray: Did you ever get any advice from, say, some of the better established mathematicians in the department that maybe you'd be better off going into a more traditional area of mathematics?

Daly: That just wasn't the way things were at Princeton. Nobody cared what you did, as long as you stayed busy.

Aspray: I see.

Eisenhart: Joe, from your description Princeton at the graduate level seems to have been quite different from what it is now. I am on the advisory council [for the math department], and the objection the graduate students have now is that each facet of mathematics—topology, analysis, algebra, geometry—is taught with too much intensity. One will say, "You know, I like algebra, but I am majoring in topology. I haven't even the time to audit the lectures in algebra, since I can't keep my head above water in topology. I have to work like a beaver, and I am getting to be a narrow specialist."

Daly: Well, we certainly didn't encourage that at Princeton in '35 to '40, because all the fields seemed tied to one another. I don't know what caused that. None of them were so far-developed that you had to get deep into something like fiber bundles to do research. So we went to all the seminars. It didn't matter whether it was Bohr on atomic structure or Condon on barred-nebulas or something like that.

Eisenhart: That's probably how I got that habit, if you want to call it that, or way of life. When I went to London I was regarded as very strange, because in the University of London there is a pure mathematics department and an applied, and I went to the lectures of both, which was just not done.

Daly: That's right. We found the guys like Condon were just as interesting. Condon and Wigner could come up with stuff that wasn't in your mathematical curriculum, and the things they could do with integrals and differential equations were just amazing.

Aspray: Let me read you—I am afraid it is a fairly long quotation—from Churchill's piece where he is quoting Mood, so this is at least third-hand now.

> The thing that particularly annoyed Sam about pure mathematicians was their snobbishness about pure

mathematics, and worse, their success in generating the same sort of snobbishness in every mathematically talented student that came along. Sam was a very even-tempered man, but this was a subject that could summon loud indignation from him. He believed that for a reasonably even balance in the development of mathematics, a substantial proportion of the most talented students should go into mathematical statistics, mathematical physics, applied mathematics, econometrics, etc. As it was, he believed that pure mathematics preempted over nine out of ten of the most talented students. This completely deformed mathematical progress in the United States. In his later years he maintained that it was impossible for him to persuade enough sufficiently promising [American] college graduates to undertake work in statistics at Princeton, and therefore he had to go to Britain and Canada to find good students whose attitudes had not been corrupted by pure mathematicians in the United States.

Daly: I think that's a little exaggerated. You go back and look at some of Sam's students. You've got George Brown, you've got Alex Mood, you've got Will Dixon. Even Tukey got pulled into the subject somehow. I am sure that Sam had a lot to do with it. Tukey was about as pure a mathematician as you can imagine.

Eisenhart: When he first came.

Daly: All he was interested in was axioms and set theory and stuff like that. But eventually he found out there was life after ultrafilters and things, and he had fun in statistics. So I think that Alex is exaggerating just a little, as least as far as the situation was between 1930 and 1940. It may have changed some later, as all of these fields got more and more specialized, and you had to push really far out in order to get a Ph.D. thesis. But at my time the boundaries weren't that far away in any direction.

Eisenhart: I would guess he was writing about a period a little later, because that situation he describes has to some extent prevailed, which makes it hard to recruit today for the government because the major statistics departments and many of the students are foreign.

Daly: Yes, I think you could see it coming. I certainly had that same experience, because as part of my work for the Bureau of the Census, I used to go around to universities trying to encourage people to get into the field of applied statistics. I found that there was this feeling that it really wasn't mathematics, although I don't know how anything could get more mathematical than the things that Wald and Jack Wolfowitz and Girshick did.

Aspray: I've been given the impression, though we've never really talked about it, that later on when statistics started to break away from mathematics at Princeton there was a bit of ill will between the two groups.

Daly: Well, that could have been, but as I say that was after my time. I got away from there in '39 just before the war. I think Sam was simply tolerated. There wasn't any particular problem that I could see.

Eisenhart: I've often wondered how Lefschetz and Dad protected it.

Aspray: That's the question I was going to ask. When you said 'tolerated' I wondered about, for example, promotions. You almost had to have a benefactor, I would guess.

Daly: Yes, I would guess so. I just don't know much about the internal politics of it. I thought that was all done by the people like Miss Shields, Fine Hall librarian, and the secretary of the department, Agnes Fleming. Basically you need someone who knows how things happen. But they were the important people. You didn't know that you got promoted by promotion boards or anything.

Aspray: Let me ask you another question that comes out of your paper. Again, I'll quote from the paper. You write that Luther Eisenhart was able to effect Wilks's appointment to an instructorship in mathematics on a more or less arbitrary basis "over the opposition of almost every member of this department". Can you comment on it or explain more of the background than you do here in the paper?

Eisenhart: No, it's just that I got that, I guess, from Dad. Of course this was the Depression, and I am sure that the elder members of the department would like to have brought in mathematical mathematicians.

Daly: I think another thing that you have to mention is that Europe was in turmoil at the time. People were coming here, big shots, all kinds, Hermann Weyl, Einstein. They were a dime a dozen. Any university could get them. So it was really tough to bring in someone like Sam, who had no reputation at all. So I guess you wouldn't really call it opposition ...

Aspray: Just, why him rather than ...

Eisenhart: As I mentioned in that article, Sam was over in England, his fellowship had run out, he had a child, his principal benefactors in the United States, namely Rietz and Hotelling, were unable to pry a job for him at Iowa City or Columbia. Dad was convinced that he was the leading man on the horizon in mathematical statistics; Hotelling had persuaded him of this. So Dad just made a shot for it.

Daly: He could do it at that time too.

Eisenhart: I, of course, thought he had been brought to teach me [laughter].

Aspray: Looking over the faculty at the time, it seems to me there were three people who might possibly have been sympathetic to the appointment. I don't know these people well, so maybe you can comment. Luther Eisenhart, H.P. Robertson—possibly because of the strong interest in probability theory at the time—and Wedderburn. The reason I suggest Wedderburn is because of his appointment by Luther Eisenhart earlier on in this College Board program.

Daly: I wouldn't know about that. I don't think, though, H.P. had much to do with it. And Wedderburn, I think, would have been completely ineffectual.

Eisenhart: I would have said that the person that was important to Sam's appointment was Carl Brigham, on the College Board. There was some committee set up in 1924 or something like that—I didn't know this either until I read it in the *Mathematical Monthly* when I was going through one of the back issues. Apparently there had been some complaints about the way that people were graded, so a committee was set up, with Dad as chairman. The other members of the committee were Rietz and E.V. Huntington, and Wedderburn from Princeton. Also Craithorn I think, and I have forgotten who else. They apparently reviewed the situation and made some recommendations. I remember Hotelling was not on it, though he was around at this time. It became clear that what they needed wasn't available, namely analysis of variance. So when Sam became available, Dad must have talked with Carl Brigham, because Sam got a job at Princeton and the College Board went almost nuts. So that helped.

Daly: That makes sense. I never detected any particular closeness between Wedderburn and Sam, or between H.P. Robertson and Sam.

Aspray: Did Wilks have any close ties among the faculty members that you were aware of?

Daly: No, I don't think so. He was pretty much a loner, wasn't he?

Eisenhart: Well, I suppose he was close to Atch Duncan. But he was pretty much a loner I guess.

Aspray: Did he have other mathematical interests outside of statistics that he continued to pursue?

Eisenhart: I'm not aware of any.

Daly: As far as Sam was concerned, mathematics was a tool to develop the theory of statistics. Nothing stopped him. He could use any kind of mathematics that was necessary.

Aspray: Let me go on to another question that arose from reading your article. Churchill tells the story there about Achison Duncan being sent away to learn about statistics so he could come back and teach it, and points out that this is one of the reasons that delayed Wilks's teaching statistics at Princeton for a period of time. One of the things that you don't address in the article at all is how it was that Duncan was chosen out of this group of people in the economics department. Can you tell me something about that?

Eisenhart: All I know is from Duncan's own testimony that it delayed his doctorate for several years because he was working on his dissertation on South African gold and international monetary policy, or some such subject, and was suddenly hauled in by Professor Frank Graham I think ...

Daly: I wouldn't be surprised.

Eisenhart: ... in economics and told that he was to go back to do this. From what I gather from Achison Duncan, that's just the way it happened.

Daly: Well, Frank Graham was pretty powerful; there's no question about that. I guess that at that point there was a real struggle in the economics department. There were people who felt that mathematics would be the death of economics, and there were other people who felt that ...

Eisenhart: Yes, Charles Roos had been there and had whetted the appetite of some of the people in the kind of mathematical economics that Griffith Evans was doing. If one may be derogatory, there were in those days two kinds of economics in the economics department: mathematical economics and verbose economics.

Daly: There was certainly no love lost between the mathematicians and the economists. I can remember my roommate across the hall, Howard Richards, was in economics; he didn't have much use for us mathematics types.

Eisenhart: As a matter of fact, that existed in other places too. When I got out to Wisconsin it was still pretty much that way, although there were some mathematical economists. Of course Milton Friedman was there for a while. There still was the long-winded kind of economics, and that was one thing Atch Duncan did something about. Economists have, you know, supply and demand curves. Apparently there was a big paradox involving these curves. When there was a certain change, some people figured that you moved over from the before curve to the after curve and other people figured that you moved down from the before curve to the after curve. This was the paradox. Atch Duncan showed that there was no paradox, that it was a matter of the speed at which the change took place. If the change took place instantly—in other words, if you devaluated the currency fast—you went one way. If, on the other hand, the currency devaluated slowly, you wound up down at the bottom, and if the change was less than infinitely fast, you went in between. This was a very simple piece of mathematics, but apparently it was beyond the ordinary economist. That was the flavor of the thing. Roos came in and really got the people interested.

Daly: This wasn't entirely an academic exercise either. At that point Hitler had completely cut off all foreign trade, and still his economy was thriving, with absolutely no foreign exchange at all. I remember Frank Graham at the Graduate College sitting there suffering, trying to figure out how in the heck can you run a country with absolutely nothing in the treasury. So as I say, these weren't just academic exercises.

Aspray: What was the relationship over time between Wilks and Duncan? Did they get along well? Did they work together?

Eisenhart: Duncan, I guess, always had to lean on Wilks for more advanced things. About 1936 or 1937—it says when in my article—the decision was was made that henceforth statistics as such, from the elementary to the advanced courses, would be taught in the Mathematics Department and that the Economics Department would be entitled to teach only statistics as it related to economics. Then Duncan and Jim Smith developed the two books of theirs. Now exactly when Duncan got interested in quality control, I don't now, but that subject is his major thing now. Later he was the head of the ASTM Committee E11, on statistics.

Aspray: I was going to ask you a question about that sorting out of the curriculum? Do you happen to know who was responsible for that? Did Duncan and Wilks get together, or was it done at the Graham-Eisenhart level?

Eisenhart: I'm sure it was at the Graham-Eisenhart level and Curriculum Committee level,. because the Curriculum Committee has to approve new courses, which is less likely when the budget is tight. So I am sure that it had to wait until more or less the beginning of Roosevelt's second term when economic conditions got to the point where the university could expand.

Aspray: I see.

Daly: It is leaning on a couple of reeds, trying to get back to the politics of the time when I was at Princeton. We were too busy trying to get our degrees.

Aspray: There's something we've gone over thatI want to come back to for a minute. This again refers to your paper. You talk in here about the period when Wilks was still in England on a fellowship and was looking for a teaching position and was having trouble finding one. There are two things in this section that I have questions about. You mentioned that he sent resumes of his professional career to universities in the United States known to have programs in probability and mathematical statistics. By that, do you mean the same places we talked about as centers of research or do you have a wider collection of places in mind?

Eisenhart: A wider collection. He sent resumes to to Columbia and so on, but he also sent them to Michigan, Texas, and I don't know where else. You see, Dodd was still down at Texas, and Michigan was strong in statistics at that point.

Aspray: A little bit later you point out that he had no prospects of a job. Had he been a mathematician with the same kinds of qualifications, would the situation at that time likely have been similar? Was it just the effect of the Depression?

Eisenhart: I expect so.

Daly: Nobody had prospects of a job.

Aspray: It wasn't because statistics was a new field?

Eisenhart: No, I think if he had been Coxeter or something, he would have had the same problem.

Daly: Yes, I think R.A. Fisher couldn't have gotten a job then. Not at Princeton anyway.

Aspray: I do have another question about curriculum. It's something in your paper that I am not sure I would have agreed with on the basis of several of my other interviews. Let me read you this section.

> Until Sam was appointed to an assistant professorship in 1936 he was only an instructor, and in a department having the stature nationally and internationally of Princeton's mathematics department, it was definitely not customary for an undergraduate, much less a graduate, course to be initiated by and be the sole responsibility of an individual with the rank of instructor.

Now, I think that's generally true, but there were some decided exceptions during this period. I think first of all of Alonzo Church and his responsibility for developing a logic program at Princeton at the time. The other one I had in mind was Bohnenblust, who was essentially given responsibility for teaching a graduate course in complex variables.

Eisenhart: That's right. He was just an instructor at the time?

Aspray: Yes, I think Bohnenblust was an instructor until 1935-36, something like that.

Daly: He was pretty young anyway.

Aspray: I'm not sure what title Church had at that point.

Eisenhart: If Bohnenblust did it, I would think that's a good exception. If Church did it, I would regret that as a negative exception, because I often heard Dad remark about the fact that Church had shined and then floundered. So that it might have been a case of not wanting to repeat that experience.

Aspray: Can you elaborate on that?

Eisenhart: That's all I can say. I don't know anything about mathematical logic. You'd have to ask Rosser.

Aspray: I've talked to Kleene and Rosser and Church.

Daly: It's pretty hard to talk to Church, but it's not hard to talk to Barkley Rosser.

Aspray: That's right.

Daly: If you can get him to stop singing canal songs and stuff like that.

Aspray: How popular were Wilks' courses? How many students would take, say, the graduate courses in the early years?

Daly: Twenty-five or so. Quite a good group, but it was the same people who took courses in relativity theory and anything else that looked like applications. A good many people sat in on Sam's course.

Aspray: Why don't we turn then to what happened at the end of the '30s and during the war? Would one of you like to comment on what happened?

Daly: I left in '39, so I can't really tell you what happened after that.

Eisenhart: I wasn't there either. I was first at Tufts and then at Columbia. But I know that Sam had those two groups, one group down there in Princeton and another group under John Williams up there in Columbia. The work of the principal Statistical Research Group at Columbia was written up quite fully [Statistical Research Group, Columbia University, *Selected Techniques of Statistical Analysis* (edited by Churchill Eisenhart, Millard W. Hastoy, and W. Allen Wallis), New York and London: McGraw-Hill, 1947], and they ought to get somebody—a combination of Mosteller and Anderson and Dixon—to write up the Princeton Group.

Aspray: I understand from your article that at some point in the '30s Wilks took over responsibility for editing the journal of the Institute of Mathematical Statistics.

Eisenhart: Yes, that's right.

Aspray: How much was this supported by the university? For example, did they provide secretarial help or office space?

Daly: I don't know how Sam did these things, he just did them.

Eisenhart: Carver had started it at Michigan. Then the Institute of Mathematical Statistics took over the ownership of the journal, and Sam was appointed the first IMS editor. He was the editor for ten years or so.

Daly: He hardly ever slept.

Eisenhart: He was editor for a long, long time, and no one else has ever been editor that long.

Daly: Sam would never give up. I mean, 5:30, 6:00, that wasn't the end of the day for Sam, he was just going good.

Aspray: Did he call on you, for example, to help him out at all with this.

Daly: No.

Eisenhart: He did an awful lot, and he must have had a secretary. Whether the Department provided it I don't know, but down in the basement in his house there were boxes sitting up on little boards across saw-horses. This was at least as long as this sofa, maybe longer. Ted Anderson and I once spent some time down there looking. We were hoping to find a lot of interesting archival máterials about the running of the *Annals* there, but we didn't find much. In the first place, Sam seems to have made a policy of destroying papers as soon as they were accepted, and I guess the rejected ones he threw out too. These boxes were full of editorial mechanics stuff; apparently he placed them there. These papers were going to be in this issue. He would apparently make a table of contents, with '00', you know, for the pages numbers. He'd apparently make that up and ship it off to the printer with the texts. These boxes were full of tons of those things.

Aspray: What I was fishing at here was the parallel with the *Annals of Mathematics*. At that time, as you know, the editing went on at Princeton, and there was heavy use of graduate students, especially advanced graduate students, even to referee.

Eisenhart: I think Sam had someone proof it.

Daly: I never did any of them, I'm sure.

Eisenhart: I wasn't there, so I don't know.

Daly: It could be that some of the guys later did, George [Brown] and Alex [Mood].

Eisenhart: Well, it's very useful to do. Because—though this is not Princeton—one of the most useful things that I did when I was in London was that Egon Pearson, I think it was, came in one day with the manuscripts of all of the articles for Volume 1 of *Statistical Research Memoirs*. He said, "You are the kind of individual to which this new thing was to be addressed. I want you to read through these things and check the typography and everything, but also tell us if there are any pieces you have difficulty following." I did this, you see. I learned a lot from reading those articles, and I did find some unclear places because those people there were so deeply imbedded in Karl Pearson's statistics. I don't even remember today what the relationships are between betas; you know there are certain equations involving beta two, beta six and beta so-forth. What happened was that equations would mysteriously shrink from being a foot long to being two inches. It was because they would use these relations without saying so. I spotted a few of these. So this was a useful experience. I don't know whether they had other graduate students assigned to do it, but I went through every page of the manuscripts.

Aspray: Can you give me some list of students that Sam had during this time? You have mentioned a couple people.

Eisenhart: I didn't take any course with him, you see; he just supervised my senior thesis. Then the next year, when I was doing graduate work, I did one little bit of work with him. A publication came out of it ["Statistical aspects of experiments in telepathy", a lecture given at the Galois Institute of Mathematics at Long Island University (mimeographed, copyright 1938 by H.C.L.R. Lieber), 18 pp.]. We did this study in the psychology department as to whether anybody had extra-sensory perception; everybody's petered out, as it usually does, I guess.

There was Bill Shelton, who was a year behind me; he did something with Sam. Then there was a fellow named Segal. Segal wrote a senior thesis or something that was of such note that it was published in the *Proceedings of the Cambridge Philosophical Society*. I don't know what's happened to Segal. [He is a mathematics professor at MIT.]

Daly: The principal people who come to mind are Alex Mood, George Brown, Will Dixon, and Ted Anderson. Carl Allendoerfer, Fred Ficken, and all the people that had any interest whatever in applied mathematics used to sit in on the courses. The only people that really didn't follow them were the confirmed topologists like Ralph Fox. It didn't take much time out of anybody's day; it was only two or three hours a week. It took more time to sit through the teas in the afternoon that it did to sit through Sam's course.

Aspray: Of course, it took a while to digest the material outside the class. Isn't that right?

Daly: I don't remember that. It just seemed to seep in. I didn't spend any time on it.

Aspray: Taking a larger view, how would you assess the contributions of Wilks and of Princeton in the overall development of mathematical statistics?

Eisenhart: I guess next to Hotelling he turned out the most outstanding students. There was no reason they should be more outstanding, but he had more of them.

Daly: Yes, I don't think it was anything he did or any of his articles or any theory that he developed. It was just that he got some good people interested in statistics. I'm not sure that it was Sam as much as it was the war. It was the thing to do something with applied mathematics when the country got into the war. This looked a whole lot more important at that point than fiber bundles and things like that. It was the right time. I believe that was partly it, because there was plenty of activity in the field of statistics. I guess Abraham Wald and his people did more theoretical statistics than Sam, and probably some of Neyman's people did too. But as far as pulling in good workers, like Dixon, Mood, Mosteller ...

Eisenhart: I don't know how Sam's students were sponsored. I suppose that they had graduate fellowships.

Daly: Yes, there were plenty of fellowships around at that time.

Eisenhart: The only time I ever heard Sam griping about anything was that apparently up there in New York Hotelling had access to these Carnegie grants and various other things. I guess we didn't have that access down at Princeton. Sam turned out an amazing amount of stuff anyway, but Sam would sometimes gripe about people like Bill Madow and Abe Girshick and some of those people. "Damn, those guys are unemployed, and so they're being paid to sit there and write stuff, while I've got to do a job besides." He got a little bit annoyed. These guys were being subsidized to turn out papers, when he managed to turn out his papers in addition to doing a job.

Daly: Pretty hard to get mad at Girshick, though.

Eisenhart: Yes, I don't think he got mad at Girshick. He got mad at Bill Madow. He was a colleague of yours later.