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A Conversation with Churchill Eisenhart

Ingram Olkin

Abstract. Churchill Eisenhart was born on March 11, 1913, in Rochester, New York, but was raised from infancy in Princeton, New Jersey. He majored in mathematical physics, as an undergraduate at Princeton University, received an A.B. degree in 1934, and an A.M. in mathematics in 1935. During 1935–37 he was a doctoral candidate in Egon S. Pearson's Department of Statistics, University College, London, with Jerzy Neyman as his thesis advisor and was awarded his Ph.D. by the University of London in 1937.

From 1937 to 1947 he was employed by the University of Wisconsin in Madison, rising from Instructor to Associate Professor in the Department of Mathematics and serving concurrently as Statistician, Biometrician and Head of the Biometry and Physics section of the Wisconsin Agricultural Experiment Station. On World War II leave from the University of Wisconsin, he was a Research Associate on a Navy project at Tufts College from January to March 1943, then a Research Mathematician, Applied Mathematics Group, Columbia University, 1943–44 and a Principal Mathematical Statistician, Statistical Research Group (SRG), Columbia University, 1944–45.

He went to the National Bureau of Standards (NBS) in October 1946, on leave from the University of Wisconsin, to take charge of a small statistical consulting group in the Office of the Director. He became the first Chief of the NBS Statistical Engineering Laboratory in mid-1947, a position he held until 1963 when he was appointed a Senior Research Fellow. He retired from the NBS in 1983. He is now a Guest Researcher in the Computing and Applied Mathematics Laboratory of the National Institute of Standards and Technology (successor in 1988 to the National Bureau of Standards).

He received the Bullitt Prize in Mathematics in his junior year at Princeton, a U.S. Department of Commerce Exceptional Service Award in 1957, a Rockefeller Public Service Award in 1958 and the Wildhack Award of the National Conference of Standards Laboratories in 1982. He was President of the American Statistical Association in 1971 and received the Association's Wilks Memorial Medal in 1977.

The following conversation took place at the Cosmos Club in Washington, D.C.

Olkin: Churchill, thank you for agreeing to have this interview for *Statistical Science*. I know that you have been interviewed and videotaped for other journals. Could you give us the details on these other interviews?

Eisenhart: In 1984, I gave the Pfizer Colloquium lecture at the University of Connecticut at Storrs. The lecture was videotaped, but unfortunately the associated sound record was almost unintelligible. I gave essentially the same lecture again in 1989 at the National Institute of Standards and Technology (successor to the National Bureau of Standards). A videotape of this lecture is available through the American Statistical Association. The only other time I was taped was inside the Bureau for the Bureau's own history, and I'm still editing the transcript of that.

PRINCETON DAYS

Olkin: Churchill, I'd like to put on record something about your background, in particular, how you got into statistics and your collegiate background.

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FIG. 1. *Churchill Eisenhart at age 63, as an NBS Senior Research Fellow.*

FIG. 2. *Churchill Eisenhart at age 2, Princeton, NJ.*

Eisenhart: I don't know just when I got into mathematical business. I was very poor at languages, and in my teens and twenties, I had an antagonism toward history, which is interesting since I've now made a 180-degree turn. I was antagonistic toward history largely because I had an aunt who was interested in genealogy and would always tell me, "Now Churchill, you know on your mother's side you're related inside, outside, right side, left side, upside, downside, to so and so. The daughter of so and so was married to Sam, who is your cousin." I never could remember all of this, and so I became antihistorical. I also didn't have much of an aptitude for economics it seems. Mathematics was one of the things I could do, so I decided to do mathematics.

I was an undergraduate at Princeton from 1930 to 1934. In my sophomore year, I took a course with Professor H. P. Robertson which we fondly called "relativity and poker." During the first half of the course we studied the theory of relativity using a little book by L. Bolton (*An Introduction to the Theory of Relativity*, Methuen, 1921), and in the second half we did probability using Thornton Fry's book *Probability and Its Engineering Uses* (Van Nostrand, 1928). In Princeton there is a mathematics prize called the William Marshall Bullitt prize that is given in the junior year for an essay on a mathematical topic. I've never been a mathematician's mathematician. I have not liked mathematics for itself and have never had a great inclination to contribute to mathematical foundations. My interest has been in mathematics as a tool to do things with. And so I had not intended to participate in this essay contest. Dr. Edward U. Condon, who at that time was Associate Professor in the Mathematics and Physics Departments, came to me and said, "Churchill, Robertson tells me that you have shown quite a lot of enthusiasm for probability, and I would like you to participate in this essay contest." Well, I was unenthusiastic. He said, "What do you know about probability and statistical aspects of measurements?" I said that I had read Weld's little book (L. W. Weld, *Theory of Errors and Least Squares*, Macmillan, 1916) and it seemed fairly straightforward and not very interesting. Condon said, "Let me just tell you that what you just read is all old hat. It's out of date. Here, have a look at this," and he gave me his personal copy of R. A. Fisher's *Statistical Methods for Research Workers*. "Read that," he said. So I read it and pretty soon got very interested in it and said to him, "Dr. Condon, if this fellow is correct, if he knows what he's doing, then it seems to me that most of the physicists that I know, if they follow one prescription in these books on the theory of errors, they simply do not know how to handle small sets of measurements. They're proceeding on the basis that they have enormously large samples, so that the mean and standard deviation of their measurements are the same as those of the measurement process, whereas Fisher shows that with four or five measurements a computed standard deviation can be miles from sigma." Condon said, "That fellow knows what he's talking about. Now you've got your mission, go to it."

Olkin: That was the essay that won you the Bullitt prize. What did you do when you finished at Princeton?

Eisenhart: I graduated in 1934 and then stayed on for another year and got a Master's Degree.

Olkin: Was this also from Princeton?

Eisenhart: Yes, during this time I worked with Sam Wilks in statistics. Wilks was the one who suggested that I go to London to work with Egon S. Pearson. He had met Pearson at the University of Iowa when Pearson had come over in 1931, and he worked with him during his 1932–33 visit to England as an International Research Fellow.

Olkin: At that time in your graduate years, who were some of your compatriots at Princeton? Were there any other statisticians?

Eisenhart: No. There were no other statisticians. There was Adrian S. Fisher who was a fellow mathematics major. He later went to Harvard Law School and subsequently became a lawyer of considerable note. He was Chief Counsel for the Atomic Energy Commission at one point; later he was Chief Counsel for the Department of Commerce and then Dean of the Georgetown Law School. Another was Phil Phoenix. After graduation he worked as an actuary for a while and then he went into the ministry. Another mathematics major whom I remember was Howard White, who also then studied law. Nathan Grier Parke was the only one who stayed in mathematics. He published a *Guide to the Literature of Mathematics of Physics* (McGraw-Hill, 1947) and later founded a mathematics consulting firm.

FIG. 3. *Churchill Eisenhart at age 21, upon graduating from Princeton University.*

Olkin: So the development of statistics at Princeton in terms of students came considerably later than your era.

Eisenhart: That's right. The next student that Wilks had before he really got into teaching statistics courses was Bill Shelton.

FISHER VERSUS NEYMAN

Olkin: What did you do after your Master's degree?

Eisenhart: Then I went to University College, London.

Olkin: With whom did you study?

Eisenhart: Jerzy Neyman, and also I attended lectures with R. A. Fisher.

Olkin: Did you get a Doctor of Science degree there?

Eisenhart: No, it was a Ph.D. from the University of London. Those were very interesting days at University College, London, because Neyman was there again. He'd been there earlier in 1925–26, and was there again in 1934–38. He and Egon Pearson developed their theory of testing statistical hypotheses. I took courses with Pearson on techniques of statistical inference and with Neyman on probability. I attended Fisher's course on experimental design. It was very unfortunate that there was a definite antagonism between Neyman and Fisher. In 1934 Neyman had given his famous paper on sampling methods at the Royal Statistical Society that brought out Fisher's wrath. And this wrath continued at University College during my time there (1935–37).

Olkin: How did it manifest itself?

Eisenhart: Well it manifested itself in the following fashion. Fisher's approach to teaching and writing on methods was: I'll tell you what to do, and you leave it up to me what the basic theory is. But then he wouldn't always tell you all the relevant facts of the theory. He would be lecturing, say, on factorial design, and would never mention the importance of additivity. Someone would tell Neyman about this, and in Neyman's next lecture on probability he'd digress and give a bitter discourse on Professor Fisher and his factorial design. Neyman would deliberately come up with some experiment involving combinations of fertilizers or other treatments such that if used in small doses everything worked fine (that is, the effects were additive), but if you used too much, the things interacted to form an undesirable compound or something worse. Then he'd analyze the data strictly according to Fisher's factorial technique and show that you got a ridiculous result.

Olkin: It sounds as if Neyman was goading Fisher.

Eisenhart: Neyman would also take issue with Fisher's idea of randomization, which he didn't object to entirely. But he pointed out that the trouble with randomization is that it mixes the heterogeneity of the material in with the experimental error, so you get a bigger experimental error to use as a yardstick. Consequently, if you're doing a field trial and you have some knowledge of the pattern of the fertility gradient, what you should do is to fit the fertility surface with a two-dimensional family of polynomials, subtract out that part and then use the rest as error. Well, when Fisher heard about that, he'd come up with some experiment where there wasn't a smooth change in fertility but there was nearly barren land up here and very fertile wet land by the river, so that there was a step function in fertility at the junction, and he would proceed to fit this with some sort of polynomial surface and often would obtain a ridiculous result. This went on and on. It was most unfortunate.

Olkin: It is an English tradition to have tea at the university in the afternoons. Did Fisher and Neyman take tea in the same room together?

Eisenhart: No they did not. What happened was that the Pearson group would go into the tea room at 3:30 and we would have India tea and then we'd all get out by 4:00 or 4:15. Then Fisher's group would come in and they would have China tea.

Olkin: Who were some of the other students or faculty there at that time?

Eisenhart: Among the other faculty at that time was Florence Nightingale David. She had just come over to the Department of Statistics after Karl Pearson's death in April 1936. Karl Pearson's death was a great blow to all of us in his son's department because none of us ever got to see him while he was alive. This was most unfortunate. It was really Egon Pearson's fault. We'd say that we wanted to go over to see his father, and he'd come back and say, "My father is very testy today. Today's not a good day." And after Karl Pearson died, I mentioned this to Florence David. She said that each of us should just have gone over to see the professor. "He loved students. He would have given you a half hour of his time even if it made him late for supper." None of us ever got to see Karl Pearson before he died, and that was one of my great disappointments.

Other students who were there at the time were R. W. B. Jackson from Canada, M. D. McCarthy from Ireland, C. Chandra Sekar, U. S. Nair and P. V. Sukhatme from India, P. C. Tang and Mrs. Tang from China and H. V. Allen, J. M. C. Scott and E. Tanburn from the U.K.

Each of us was expected to give a seminar. Neyman asked me to work with Miss Tanburn and to have her practice her presentation before the seminar. This I did. So she got up and began giving her seminar, but before she could get more than a couple of sentences into her talk, Neyman asked her a question that she was going to cover later. She said, "Well I'll come to that in a minute, Professor." And then she went on and he asked her another question. I knew that she was going to cover that soon too. I said, "Professor Neyman at your request I have rehearsed Miss Tanburn. I know that she can discuss this well, so please just keep quiet and let her give her talk." Well, for a student to tell a professor to shut up just wasn't done. All hell broke loose. Later I overheard F. N. David castigating Neyman.

Olkin: Who were the other faculty members?

Eisenhart: In our group there was B. L. Welch. M. S. Bartlett had been there, but he had left. Upstairs with Fisher, there was W. L. ("Tony") Stevens, Professor Paul Rider (of Washington University, St. Louis) and Professor George Rasch (Copenhagen). There was some intercourse between the two floors among the students. But you had to change your language when you went from one floor to the other. You would talk about inductive behavior when you were with Neyman, you talked about fiducial inference when you were with Fisher.

WISCONSIN IN 1937

Olkin: After your stay at University College, did you go to Wisconsin?

Eisenhart: I did go to Wisconsin in 1937. I had a joint appointment in the Mathematics Department and in the Agricultural Experiment Station. During the academic year I taught half time in mathematics and the rest of the time at agriculture; in the summer I was 100% with the agriculture group.

Olkin: What statistics courses were taught in the mathematics department during this period (1937 to 1940)?

Eisenhart: I gave a course, "Introduction to Statistical Methods in the Natural Sciences," in the Mathematics Department. It was a 100-level course, which meant that both undergraduates and graduates could take it for credit.

Olkin: At that time were there any books for such a course?

Eisenhart: Yes, I used Rider's book (P. R. Rider, *Introduction to Modern Statistical Methods*, Wiley, 1939) as a textbook and supplemented it with other material.

Olkin: How long were you at Wisconsin?

Eisenhart: On paper I was at Wisconsin from '37 to '47, though I was on leave from January '43 to the fall of '45. During January–March '43 I was Research Associate at Tufts College working on a project sponsored by the Special Devices Section of the Navy. Then I went to Columbia University where I first worked in the Applied Mathematics Group. There I worked on aerial gunnery, which I'd been doing up at Tufts also. In November 1944 I moved to the Statistical Research Group (SRG) through September '45.

Olkin: The history of the Statistical Research Group at Columbia is well documented by W. Allen Wallis in the *Journal of the American Statistical Association* (1980 75 320–330) and in his 1991 interview in *Statistical Science*.

Eisenhart: It was also discussed in the book *Selected Techniques of Statistical Analysis* (McGraw-Hill, 1947) edited by "Eisenhart, Hastay and Wallis." It should have been "Wallis, Eisenhart, and Hastay," because Wallis was the editor-in-chief. However, because my name was first alphabetically I received all the correspondence and requests for copyright permission to copy or adapt things from it.

Olkin: After the war, did you go back to Wisconsin?

Eisenhart: Yes, and I was there until September '46.

THE NATIONAL BUREAU OF STANDARDS

Eisenhart: What had happened in the meanwhile in Washington was that the same Dr. Condon who in 1933 got me started in statistics at Princeton had become the Director of the National Bureau of Standards (NBS) in 1945. He realized that there needed to be modern statistical and mathematical programs at the Bureau. At that time there was no mathematics per se at the Bureau. There was one person, Chester Snow, with the title mathematician, who was engaged in the application of mathematics to problems arising in electrical research, but there was no formal NBS mathematics program. Condon, aware that "applied mathematics was on the threshold of revolutionary developments which would permit numerical answers to be obtained to physical problems at hitherto undreamed-of speeds," considered a strong, federal applied mathematics center to be a necessity in the national research program and proposed to establish such a center as a unit of the NBS. In his Foreword to the Prospectus for "The National Applied Mathematics Laboratories," he wrote: "In these days when so much emphasis is properly being placed on economy in government research operations, it is important to take advantage of the substantial savings which can be effected by substituting sound mathematical analysis for costly experimentation. In science as well as in business, it pays to stop and figure things out in advance."

The question then became: Whom should he get to organize and head up this new applied mathematics activity? Condon talked to his physicist/statistician friend, Ed Deming, who told him that there was a young man named John H. Curtiss, a first-rate mathematician who had been doing good statistical work in the Navy Department during World War II. Condon interviewed Curtiss and set him up to start a program in applied mathematics and applied statistics in the Office of the Director. But before Curtiss was able to get very far with this, the Bureau of the Census and the Office of Naval Research asked the Bureau to provide specifications for, and oversee the procurement of, electronic computers for them. Condon asked Curtiss to head up this new venture. Curtiss told Condon that he could not do this and develop an applied statistics program too. Then Condon remembered his Princeton student of 1933 and sent for me. I visited Washington sometime in September. The job offered appealed to me. Many colleagues thought that I would never leave Wisconsin because my setup there was so good. It was good except in one respect, namely, that I never was allowed to have some of my own students. I had repeatedly asked for permission to have some funds to enable a postgraduate to work with me in statistics towards a Master's degree or maybe even a doctorate and also be an assistant to me at the Agriculture Experiment Station. This never happened because the Mathematics Department was strong on algebra and the fellowships usually went to algebra candidates. I never got one. I was very lucky in that I did get a couple of people whom I hadn't recruited. One of them was Wilfrid J. Dixon. The other was Robert J. Hader. But except for those lucky "finds," I had no luck. So when Condon told me that I could recruit a staff of my own, this really thrilled me.

Olkin: When did you go to the Bureau?

Eisenhart: October 1, 1946.

Olkin: And you've been there ever since. Tell me about the early days at the Bureau. What were some of the projects that you worked on?

Eisenhart: When I first came to the Bureau, John Curtiss had already recruited Lola Deming (Mrs. W. Edwards Deming). I began with a small staff of Lola Deming as my technical assistant, Helen Herbert (now Mrs. Gerald J. Lieberman) as secretary and myself. Bill Cochran recommended Joe Cameron. He came in the spring of '47, and we started to work on statistical problems at the Bureau. As a result of wartime activities, sampling inspection had gotten a lot of impetus. There was a Testing and Specifications Section at the Bureau and one of the first things we did was to look at their sampling and test procedures.

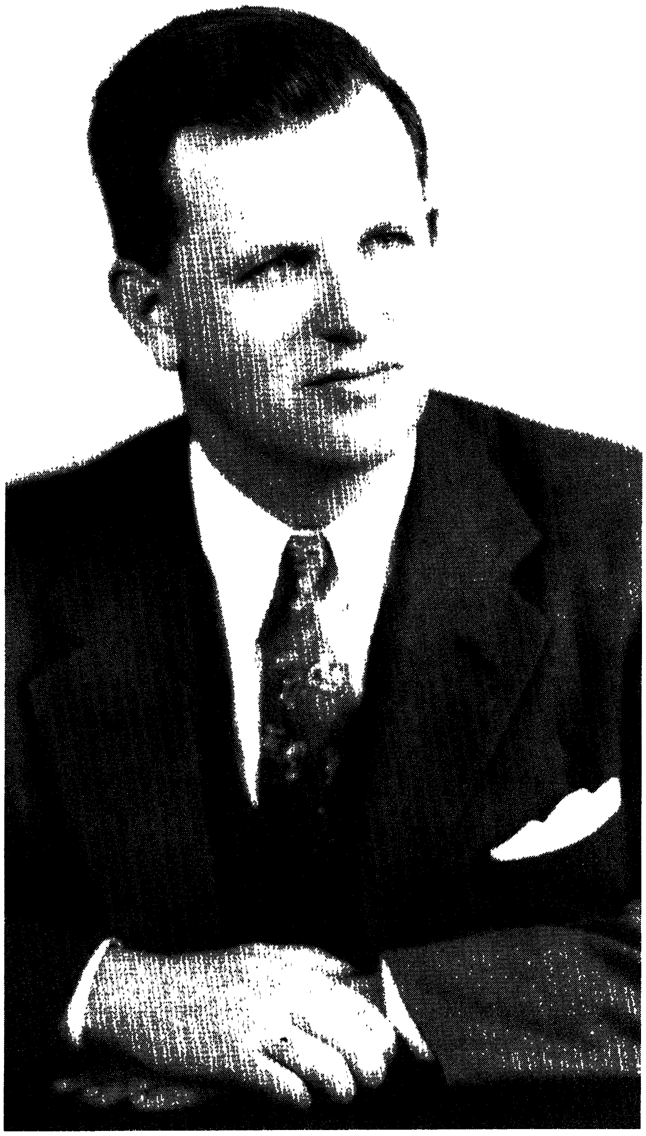

FIG. 4. *Churchill Eisenhart at age 34, upon becoming Chief of NBS Statistical Engineering Laboratory.*

Olkin: Did the Bureau have a stated mission?

Eisenhart: The Bureau's basic mission of course is to serve as the custodian of national standards of measurement, together with the research activities necessary to maintain, improve and disseminate them throughout the country. But Herbert Hoover, when he was Secretary of Commerce got the Bureau involved in a great deal of support work for industrial activities, so that by the time we got there, the Bureau was authorized to carry out research for other agencies.

Olkin: Were these only government agencies or was industry included?

Eisenhart: Industry too. That's an interesting point. We couldn't assist a particular company. But if there was a problem that transcended particular companies, then as an industry problem it was all right to help. For example, in 1948 we worked on core sampling of baled wool. This was done through an ASTM Committee. ASTM was called the American Society for Testing Materials then, and now is called the American Society for Testing and Materials. A Task Group on the Sampling of Packaged Wools by Core Boring was set up under ASTM Committee D-13 on Textiles to help the U.S. Customs Laboratory in Boston with the sampling of shipments of baled wool for determination of a shipment's percentage of clean wool fiber. The Task Group consisted of Louis Tanner (Customs Laboratory), Ed Deming (Bureau of the Budget), Jack Youden, Joe Cameron and myself (NBS).

One of the Task Group's findings was that the ratio of the standard deviation of between-bale variability to the standard deviation of between-core variability within bales varied from less than 0.5 to over 2.5 depending upon the county of origin of the wool. Another was that the ratio of the cost of positioning a bale for core boring and the cost of boring one core from a bale varied from 1 (when the sampling takes place as the bales are being unloaded from a ship or are being put in a warehouse), to over 25 (when the bales have been stored in piles, and some are to be selected and positioned for core boring). The end result of the Task Group's work was an ASTM tentative standard for core sampling of wool (D1060-49T). The relevant theory and essential findings were summarized by Tanner and Deming in *ASTM Proceedings* (1949 **49** 1181–1186) and by Cameron in *Biometrics* (1951 **7** 83–96). The number of cores per bale must, of course, be an integer. The customary formula usually yields a non-integer value between k and $k + 1$. Cameron includes an algorithm he attributes to me [but is often cited as "Cameron (1951)"] that indicates whether k or $k + 1$ is the correct optimal integer. This study calls for an aside about our financing that I'll comment on later. Back to the early part again.

One of the problems that we had when we first began working on test methods and sampling techniques—and you always have this problem when a group of statisticians move to a new place—is that the people there, whether diabolically or in hopes of help, bring you all their little chestnuts that they haven't been able to crack, so you have a big risk of failure. The relevant experiments are long past; the data are old; there's usually not much you can do. So you really have to venture out to find new problems to work on where a little statistics will go a long way toward obtaining better results. Joe Cameron and I had just started working on NBS problems when we had a great stroke of luck. What happened was that I was invited to a symposium on statistical methods in experimental and industrial chemistry at the 1947 Atlantic City meeting of the American Chemical Society to discuss a paper by Sam Wilks. There was a man there who just fascinated me. His name was W. J. Youden. He presented a "Technique for testing the accuracy of analytical data." I felt that this man would be just wonderful at the Bureau. I was reluctant to ask him to join the Bureau because he had been working at RAND where he received a salary in the neighborhood of $15,000. The NBS Director's salary was $9,000 then. I just didn't see how I could ask a man like that to come to the Bureau. I mentioned this to Forman Acton, and Forman said to me: "Churchill, never answer another man's questions for him. Ask him." I asked Jack. "Oh," he said, "the Bureau of Standards has been 'Mecca' to me my whole life. I'll come." And come he did. Right away, he and Joe set out to find problems where they could use a small bit of statistics to make a whopping big effect.

When Jack came, in the Spring of '48, we were still in fiscal year 1948. Fiscal year '49 would start on July 1. We hadn't known he was coming so we hadn't put any money for him in the budget for fiscal year '49. Condon was out of the country. He had told Curtiss that he had given to the Applied Math program all the money that he could: "Don't go asking me for more." So, when Youden came, I hunted around to see whether we could get some help from other divisions. I asked Dr. Wichers in Chemistry. Youden was already well known to some of the chemists there because he solved some chemical problems by skillful use of statistical techniques. Using statistics, he had found flaws in certain famous chemical papers, and they were very much impressed with him. Dr. Wichers said he was glad we were getting Dr. Youden. If he could have Youden's services free of charge, then he would be glad to have them, but, if he'd have to pay for them, the Chemistry Division would carry on without Youden's help, as it had in the past. I told this to Dr. Condon when he got back; Dr. Condon got angry. He threw his pencil down on the table and said: "I'll fix those fellows. I'll put your consulting services operation on Bureau overhead. Then it'll come out of all funds of the Bureau, and the Bureau staff can scramble to get your help." That's what he did. From that point on, the Statistical Engineering Lab (SEL) was on overhead, half and half. All of our consulting and advisory work was on overhead. Our mathematical research, our presentations of courses on methods at the Bureau, our preparation of manuals on statistical methods, our work on experiment designs and things of that sort were Applied Mathematics Division activities, and for these I reported to Curtiss. For the overhead part, I reported to the Director.

Olkin: That was actually a very fortunate event because it meant that you didn't have to scramble for every penny; it came off the top of the pile, so to speak.

Eisenhart: That's right. After Dr. Condon left, Dr. Astin maintained that arrangement, which continued while Joe Cameron was my successor as Chief of SEL. A few years after Joan Rosenblatt succeeded Joe, a bureauwide effort to reduce overhead led to conversion of SEL's consulting and advisory services to a regular technical program of the Applied Mathematics Division with funding commensurate with their cost in recent years. She then reported to the Applied Mathematics Division Chief for the entire SEL operation, he reported to an Institute Director, who reported to the Deputy Director. When I reported directly to Dr. Astin and Dr. Condon, our consulting and advisory services budget was settled for the "next year" in about fifteen minutes. They would call, we'd discuss SEL's needs, we'd settle the overhead part, and that was it.

SOME BUREAU PROJECTS

Olkin: I remember when I visited the Bureau, you once told me a story about sizing of young women's clothing, and I thought that was interesting problem. Perhaps you can review this problem.

Eisenhart: I can comment on it. But it was an absolutely anomalous project for us to be working on because our focus was on statistical methods applied to measurements in the physical sciences and engineering. The way that we got involved was this: In 1947, the Mail Order Association of America (MOAA) asked the Commodity Standards Division (CSD) of the NBS to develop new standard body-measurement size designations for teenage girls, inasmuch as the number of teenage garments returned for nonfit had become a serious problem for its members (Sears Roebuck, Montgomery Ward, Spiegel, etc.). They approached the CSD because it had published recently a number of Commercial Standards on clothing sizes, for example, on "boys' pajama sizes" (CS106, 1944), "women's slip sizes" (CS121, 1945) and "men's shirt sizes" (CS135, 1946). The MOAA wanted the CSD to issue a new Commercial Standard for teenage girls based on the 37 body measurements taken on approximately 7,000 school girls some years before by trained anthropometrists of the Textile and Clothing Division (TCD) of the U.S. Department of Agriculture (USDA) Bureau of Home Economics (BHE) and still available on punched cards. Those data were a portion of those obtained in the pioneer TCD study of body measurements of 147,000 American children for which the late Meyer A. Girschick (1908–55) was responsible for the statistical design and analyses and that saw publication in USDA Miscellaneous Publications 365 (1939) and 366 (1941). The Textile and Clothing Division was not willing to make these punch cards available to the NBS Commodity Standards Division. This refusal was unacceptable to the Bureau of the Budget inasmuch as these punched cards stemmed from a U.S. tax-supported study. A compromise was reached; the punched cards would be loaned to the NBS Statistical Engineering Laboratory (SEL), which would be responsible for the requisite statistical analyses; the necessary numerical computations would be carried out for the SEL by the NBS Computation Laboratory; but no personnel of the Commodity Standards Division were to have access to the punched cards. Hence, a "Teen-Age Girls' Body Measurement Study" was established in the SEL, with Lola S. Deming and myself as project managers.

Olkin: What statistical procedures did you use for that?

Eisenhart: Well one of the first statistical procedures we used was a frequency diagram of stature versus hip girth for girls of ages 12 through 17. The Mail Order Association had told us that for teenage girls they wanted us to have five sizes. Now, a size can be thought of as a two-dimensional rectangle of sides 2 inches and 1 1/2 inches in the stature and girth directions with the upper right-hand corner called the grading point, since this will denote the maximum height and maximum hip girth for that "size." The reasoning was that you could always shorten garments and you can always narrow them, but you can't lengthen or expand them. Well the first thing Lola Deming did was to take some of these correlation plots and try to place five rectangular "windows" of the aforementioned dimensions on the stature–hip girth frequency diagram so as to include the maximum number of girls. And she found that with five such "windows" she couldn't capture more than about 46% of the girls. So we had to go back to the mail order people and say we can't do very well with only five, how about letting us have seven or eight? They said, "Ok, try seven." So then, Lola placed 7 optimal "windows" on the diagram, and found that by having a "talls" and "shorts" corresponding to each of the three central hip girths we could cover over 85% of the population. Now that was just the beginning.

Then came the big correlation study. The mail order people figured that they couldn't expect a mother to take more than three measurements on her child, so the question was: What three measurements? These three measurements had to be ones that correlated well with all the measurements she didn't take. For example, it seems unlikely that you were going to use arm length as one of the measurements. But you probably are going to use stature. You're probably going to use some girth measurement, such as hip girth, chest girth, waist girth, or bust girth. Now what you wanted to do, is to get three measurements such that when you knew the values for those three measurements for a size-twelve girl, say, you could calculate good estimates of arm length, across-back width, back neck-to-waist length, waist girth, crotch height and so forth. So we did a lot of correlation analyses with regard to the teenage girls, and, as I recall it, we selected the measurements for stature, bust girth, and hip girth as the three best for teenage girls.

Olkin: So basically there were just several measurements?

Eisenhart: There were several measurements, but there was this interesting correlation study to find out what three to use. The end product of our teenage-girl body measurements analyses was "Commercial Standard CS153-48, Body Measurements for the Sizing of Apparel for Girls."

Olkin: For men's shirts, for example, they use only two measurements, neck size and arm length.

Eisenhart: Right.

Olkin: Men who are broad shouldered have trouble in compromising between those.

While we're on the subject of different studies at SEL, would you review the famous Automobile Battery Additive Study that created such a controversy.

Eisenhart: This study really helped the Statistical Engineering Laboratory be accepted around the Bureau, because the feeling was that we had helped save the Bureau. What happened was that the Director was pressured by various senators and the battery additive producer into agreeing to run a demonstration test of the additive. To do this test, 25 heavy-duty six-cell twelve-volt batteries of the type used in buses were obtained from the Navy. It was agreed by all concerned that nine of these were unfit for the test. The six cells of each of the remaining 16 batteries were then separated and reassembled to form 32 three-cell six-volt batteries, numbered serially from 1 to 32 for identification. The manufacturer wanted to put all of the 16 batteries that were to be treated on one charging line. The Bureau said, "No, you can't do that, because, if the batteries that received the treatment performed better or worse than the controls, you never could tell whether the difference was due to the treatment or the charging lines, or both." We insisted that they have both kinds of batteries on both charging lines, and that the assignment of the treatment be blind. In other words, nobody participating in the test was to know which were the treated and untreated batteries. Furthermore, we insisted that the selection of the batteries that were to get the treatment be done formally at random, because then we could use randomization test procedures and wouldn't have to pretend that the battery performance was normally distributed. We didn't want to be subjected to that possible criticism. We

FIG. 5. *T. W. Anderson presents to NBS Director, Allen V. Astin, congratulations from the Institute of Mathematical Statistics on NBS's 50th birthday in 1951, with John H. Curtiss, David H. Blackwell, Churchill Eisenhart and Erich L. Lehmann watching.*

wanted to be able to analyze the resulting data both by what you might call traditional normal theory and by using Fisher's two-sample randomization test where you permute all the actual numbers you got.

Dr. Youden proposed a design with the 32 batteries grouped in pairs on three charging lines. On lines 1 and 2, both batteries of a pair would be treated or both would be untreated. On line 3, one battery of a pair would be treated and the other untreated. The actual selection of the batteries to be treated was made by the Director using the table of random numbers [Table 1.2] in Snedecor's *Statistical Methods*. Line 1 contained 3 trays of untreated pairs and 2 trays of treated pairs, connected in series. The converse was the case on line 2. The 6 trays of line 3 each contained one treated and one untreated battery. [For the actual layout, see p. 557 of *Hearings before the Select Committee on Small Business, United States Senate, Eighty Third Congress, First Session, On Investigation of Battery Additive AD-X2*, Washington, 1953.]

Shortly before the electrical test was to be run, Dr. Youden came running up in a state of panic to state that he had not realized that besides a battery performance test, that is, an electrical test, there was also going to be a visual inspection of the plates of some of the batteries after all the electrical data were available. In other words, some of the batteries were going to be taken apart and their electrical plates examined and compared, and we needed a design for that. The plan was that 5 treated batteries and 5 untreated batteries would be taken apart after the electrical tests, and the plates of each battery would be compared for their condition with the plates of each of the other 9 batteries, by each of the judges—a total of 45 paired comparisons by each judge.

Fortunately Joe Cameron found just the design that we needed in Cochran and Cox's book. The experimental design was going to have to be an incomplete block design to handle all 45 paired comparisons, and we needed to have an incomplete block design in which the 45 "blocks" of comparison pairs were grouped into replications to facilitate analysis of the data. It just happened that one of the designs in Cochran and Cox's book (*Experimental Designs*, John Wiley & Sons, 1950) was arranged that way [Plan 11.14 on page 331]. We used that in the visual comparison test. But this experience scared the life out of us because, if Joe hadn't found this plan, somebody would have had to sit down with Cochran and Cox and see whether under the pressure of the urgency they could group the 45 blocks into replicates. When I mentioned this to agricultural people they always said, "So what. If it can be done, you can do it." Our point was, "Maybe you can do it, but could you do it in a hurry, and could you do it correctly under pressure." We felt that it would be desirable to have the designs in Cochran and Cox's book arranged in replicates whenever possible. We had a summer student named Millicent Rupp. Under the guidance of W. S. Connor, she grouped the blocks in Cochran and Cox's plans into replicates wherever possible. Their results were collected in an NBS Report, which we gave to Cochran and Cox. In the next edition of their book they arranged their tables of incomplete block designs in replicates whenever it was possible, so this became a contribution to the literature. Using this design we ran the test.

Olkin: What year was the test actually run?

Eisenhart: June 1952. Now just before the electrical test was run, we had another crisis. Dr. Youden came from the test site and went into the Director's office. I happened to be there at that time. Youden told Dr. Astin that he was frightened. Although he had the greatest confidence in the competence of our battery experts, this particular test was to be witnessed by the press, by representatives from the Army, the Navy, Air Force and Atomic Energy. It would be a real spectacle, and he was terribly afraid that our experts might become nervous under these circumstances and possibly make a mistake. He felt that it would be desirable for Dr. Astin to administer this test. To my horror, Dr. Astin picked up his telephone, put in a call to the test site and said "Dr. Eisenhart will be down to serve as administrator of the test on my behalf." Ouch! So I went to the test site and served as administrator. Luckily for me, no problems arose for me to resolve.

Olkin: What were the conclusions?

Eisenhart: The conclusions were that the treatment had very little or no effect. [The full details of the "June 1952 Test," including the analyses of the data by the Statistical Engineering Laboratory are to be found in Exhibit 9 in the Senate Hearings Publication cited above.]

Olkin: Whereas the companies producing these had said they had an effect?

Eisenhart: Yes. One of the difficulties in evaluating any battery additive is a characteristic of a lead acid storage battery with which most people are not familiar. Suppose that you have a good battery. Further, suppose that it is a cool morning, and you've tried to start the engine but have cranked it several times, so that the starter won't turn over anymore. Now, if you let the car sit idle for a half an hour or so, and then try to start it, it will probably turn over. The point is that the battery rejuvenates itself. Most people don't realize this, and it is one of the things that helps what a battery additive demonstrator will do. He will take a car, turn the ignition off, and run the starter until it won't crank. Then he will put the additive in the battery, after which it is necessary to wait 20 minutes to half an hour to let the additive work. Then he would get in the car, step on the starter and it would turn and start engine. However, it generally would have started without the additive!

Next I want to tell you about a comparison test based on ranks that I carried out for the members of the National Academy of Science Committee on Battery Additives. I wanted to persuade them of the fallacy of applying a treatment only to what appeared to be the poorer batteries in a preliminary test. In order to demonstrate this I took a deck of cards with me. I said: "Now I want to show you the fallacy of treating the worst. I took cards bearing the numbers one to ten and put numbers 1, 2, 3, 4, 5 on the left, and 6, 7, 8, 9, 10 on the right. Let us consider these to be the rankings of 10 batteries on a pretest, with '1' the best and '10' the worst." I said: "Now I'm going to apply this magical treatment to these." I waved my hands over the right-hand group, saying that I had just applied treatment X to these five worst. To rank their performances again, I took other cards 1 through 10, gave them to somebody to shuffle, and then I put the top five by the cards on the left and the next five by the cards on the right. Now we summed the ranks of the "treated" group on the right before and after "treatment" and likewise for the control group on "pretest" and "test." The sum of the ranks of the "control" group on the left was 15 on the pretest, and on the test, was 26, say, a "deterioration" of 11 ranks. The sum of the ranks of the "treated" on the pretest was 40; on the test, 29; a rank "improvement" of 11. The Battery Additive Committee enjoyed this immensely. Wow, that treatment had a huge effect! Joan Rosenblatt calculated the probability distribution of the rank "deterioration," D, of the "controls" under such randomization and found the probability that $D > 11$ is 0.655 and that $D = 21$ is needed ($P = 0.048$) for "significance" at the 0.05 level.

We tried to carry out a similar analysis with random normal data using the two sample t-test. The mathematics of the resulting t^* is difficult because there's a correlation between the numerator and denominator, and we were never able to do the mathematics. But Carroll Croarkin simulated the distribution on a computer, and, if I remember correctly, we found that for the case of five in one group and five in the other group, the probability is 0.564 of t^* exceeding the upper 0.05 probability level of Student's t for 8 degrees of freedom. I gave a talk on these findings at the 1962 Annual Meeting of the Virginia Academy of Sciences with the title "On the Fallacy of Treating the Worst." An abstract was published in the *Virginia Journal of Science* (1962 **13** 311–312). An analysis of the case of the paired sample t-test has appeared recently in *The American Statistician.*

Olkin: Was the battery additive study written up in the statistical literature?

Eisenhart: No. The details and findings of the "June 1952 Test" were published as an exhibit in the Senate Hearings, as mentioned earlier. All of NBS's work on AD-X2 was issued in a large two-volume NBS Report 2447 in April 1953. There will be a discussion of the "Battery Additive Controversy" in a history of the NBS covering the period 1950–1958, now in preparation.

There was another interesting aspect to the battery additive work, which I think should be mentioned. There was this great big report to be prepared. When the Director was fired, we felt that we had to get all of the basic data and analyses to date written up before the Department of Commerce told us to stop. So people were typing night and day. We had a lot of volunteers from the Computation Laboratory; many were middle-aged and elderly women who were proficient at comptometers and desk calculators. They asked how could they help? One of the big tasks is proofreading. So Joe Cameron came up with the novel idea: "We'll take each page of the original manuscript as it comes out and have one of these volunteers add up every number on that page. If the number is a date, or if it is page number or if it is a measurement that doesn't make any difference, add it." Every number on a particular page would be added up and the sum would be written in a corner. And the same would be done for each page. Then, when the final typescript came out, a copy of it would be made, with an indication of where each manuscript page began and ended. Somebody would again add the numbers on a manuscript page. If they didn't get the same sum, then we looked to see whether there was a transposition, a copying error or omission. It was a wonderful idea. In addition, it gave these desk calculating people an opportunity to contribute. It also helped find discrepancies.

Olkin: That's an interesting proofreading device. Did you mention that Astin was fired?

Eisenhart: Yes, he was fired in March, 1953. He was actually off the payroll. But fortunately he was reinstated at the insistence of the President of the National Academy of Sciences before the end of next pay period, and so he didn't lose salary.

Olkin: Who fired him?

Eisenhart: Craig R. Shaeffer, who was the Assistant Secretary of Commerce for Domestic Affairs at that time, and he'd felt that Dr. Astin had not given adequate recognition of the "play" of the marketplace in his judgment of the additive. So Dr. Astin was off the payroll on March 31. We at the Bureau didn't realize that he was gone. He continued to sit in his office, and Dr. Wallace Brode, who was the Senior Associate Director, was Acting Director. Astin continued to run the Bureau as if he were the director, but he couldn't sign anything official. So anything official had to be signed by Dr. Brode. Many of us thought that the Director had been "placed on ice" so to speak, momentarily suspended, not fired. But, no, he was really gone. Then the Secretary of Commerce asked the National Academy to set up a committee to review the Bureau's work on the additive. Dr. Bronk, President of the Academy, refused to do this unless Astin were at least temporarily reinstated. And so Astin was temporarily reinstated before the end of April. That was lucky for him because in the federal government the check that you get in March is really the check that you earned the month previously, in February; and the April check is the check you earn in March and so forth. Astin got back on the payroll before he missed a check.

Olkin: And then the Academy reviewed this?

Eisenhart: The Academy established a Committee on Battery Additives, chaired by Zay Jefferies, a retired vice president of the General Electric Company. Bill Cochran and Sam Wilks were members of the Committee. The Committee's conclusions were that the Bureau's battery additive work was first rate and the Bureau's conclusion from the data that the product was without merit was valid from its analyses.

Olkin: Was that the end of the politics?

Eisenhart: Yes.

Olkin: And Astin then stayed on?

Eisenhart: Yes.

THE STATISTICAL ENGINEERING LABORATORY

Olkin: How long were you the head of the statistical group at the Bureau?

Eisenhart: I was head of the statistical group in the Bureau from October 1, 1946, when it was a small group in the Director's office. Then, with the birth of the Applied Mathematics Division on July 1, 1947, I became the chief of the Statistical Engineering Laboratory and I was in that position until 1963, when I was appointed a Senior Research Fellow. I was in that position until 1983 when I retired.

Olkin: Who were the people in the Statistical Engineering Laboratory (SEL) at the Bureau during your tenure?

Eisenhart: As I indicated earlier, the people in my group in the spring of 1947 were Lola Deming, Helen Herbert and Joe Cameron. When the Applied Mathematics Division was born on July 1, 1947, with the Statistical Engineering Laboratory as one of its units, our staff consisted of just those people. I told you earlier about our great success in acquiring Dr. Youden in 1948. Early in 1947 a recruitment opportunity arose that was a success for the NBS but not for the Statistical Engineering Laboratory. Curtiss came into my office one day with a Personnel Form 57 for a man by the name of John Mandel, who was applying for a desk calculator job at the Mathematical Tables Project in New York City. Curtiss showed me this Form 57 and said, "Look at this. This fellow has a degree in chemistry from Belgium, and he has been studying mathematical statistics with Abraham Wald at Columbia. Wouldn't it be a waste for him to work as a desk calculator? Shouldn't he be in statistics?" I said that he certainly should. Curtiss asked me to see whether we could get him for SEL. I called Mandel on the telephone and he said that he would very much like a statistician job at NBS but there was a small problem: His wife was a diamond girdler, and she brought in a separate income by diamond girdling, which she could do in their flat; and as far as he knew they didn't do any diamond girdling in Washington, so she'd have to give that up. Hence, our offer would have to include enough increase in his salary to make it worth their while. Curtiss told me that as we were about to begin as a part of a new division on July 1, our funds for fiscal year 1948 were set: "We don't have any money in our budget to hire Mandel, so see whether you can get help from other divisions." I went around and began asking questions of other divisions. Dr. A. T. McPherson, who was head of the Organic and Fibrous Materials Division, sounded very interested. SEL had been helping them with road testing and fabric testing experiments. I told him that here was a man who was ideal for this sort of work. What I intended to convey was that, if McPherson could pay for Mandel for the first year, then we could get him on board, and he could be an SEL missionary there. What I had intended was that he'd be our missionary, that he would belong to us, but Dr. McPherson would pay for him. Well, when the time came, I found that the old Scot, Dr. McPherson, had taken the view that, if he was paying for Mandel, he was going to own him. So Mandel became a member of the staff of the Organic and Fibrous Materials Division, and SEL never got him. We tried a number of times to get him to transfer to SEL but without success. John got involved in projects that continued from one fiscal year to the next and said that he could not leave his colleagues.

Olkin: In the early fifties a lot of statisticians were at the Bureau. I recall that Marvin Zelen was there as well as Frank Proschan. Who else was at the Bureau?

Eisenhart: The others who joined SEL early were Julius Lieblein, Mary Natrella, Bill Connor, Richard Savage and Willard Clatworthy. The first person who came in mid '47 was Julius Lieblein. We'd expected him and so we had money for him in the fiscal year '48 budget. At that time Emil Gumbel, who was an expert in extreme values, was out of work. At Dr. Condon's suggestion, we invited him to give a series of lectures on the theory and application of extreme values. The lectures were written up and published by the Bureau under the editorship of Lieblein (NBS Applied Mathematics Series No. 33, 1954), together with a set of tables to implement these tools (NBS Applied Mathematics Series No. 22, 1953).

Frank Proschan came in 1951. He was our link with the engineers at the Electronics and Ordnance Division before the battery additive controversy broke and the Bureau's Ordnance Divisions were transferred to the Army in September 1953. He did very good work for them; he also wrote a number of manuscripts that shocked the NBS Editorial Committee chairman because they were humorous and written in dialogue. The chairman said: "What are we coming to?" It turned out that Hugh Odishaw, Assistant to the Director, was quite a scholar and commented that Galileo wrote in dialogue. He said: "Let's read the manuscript and see whether it's well done." So these manuscripts did get published ("Use of Random Numbers," *Industrial Quality Control* 9 32–33, July 1952; "Control Charts May Be All Right, But–," *Industrial Quality Control* 9 56–58, May 1953).

Bill Connor joined us in 1951. He and Youden developed new families of experimental designs that were "tailor made" for the physical sciences and engineering, where only two or three replications of a particular measurement are often sufficient to assure adequate precision of final results, and the performance of instruments, products or treatments can be compared using distinct "plots" (e.g., feet, wheels, positions) within "natural" sharply defined and physically independent "blocks" (e.g., persons, cars, boards). Furthermore, in calibration work the domain of inference is limited to the objects (resistors, thermometers, weights, etc.) whose properties are measured–one is interested in the resistance of a particular resistor, not in its resistance as a specimen of resistors of that type or brand.

For "blocks" of 2 objects, they recognized three distinct cases:

1. the same property is measured for each of the objects (e.g., readings of two thermometers in a common temperature bath),
2. measurement of only the *difference* of the two objects with respect to some property (e.g., the difference in length of two length line standards, such as meter bars),
3. measurement of only the *sum* of the amounts of this property for the two objects (e.g., weights, length end standards such as gauge blocks).

In "New experimental designs for paired comparisons" (*J. Res. NBS* 1954 **53** 191–196), Youden and Connor considered case (1) explicitly and in footnotes gave the necessary modifications of the formulas for analyzing the resulting data when only differences (case 2) are measured. The evaluation of the lengths (or masses) of n objects by measuring the sum of their lengths (or masses) two at a time is evidently very old. Ferdinand Rudolph Hassler (1770–1843) discussed both the method and its advantages in his 1832 "... account of the means and methods employed in the comparison of weights and measures, ordered by the Senate of the United States. ..." The measured combinations in such instances are special cases of the spring-scale (or single-beam-balance) weighing designs considered by Mood (1946), Banerjee (1947) and others. To take advantage of some of the special circumstances of spectrographic determinations of chemical elements carried out by the comparison of spectrum lines recorded on photographic plates, Youden developed *linked blocks* (1951) and (with Connor) *chain blocks* (*Biometrics* **9** 1953 127–140). Mandel developed chain block designs further, providing a scheme for two-way elimination of heterogeneity.

The statistical literature is full of chemical balance and spring scale "weighing designs." It turns out that these "weighing designs" are of no value in calibrating a graded series of laboratory weights (e.g., 100, 100, 50, 30, 20, 10, 10, 5, 3, 2, 1, 1, ... grams) in terms of one or more mass standards (e.g, a reference standard 100). What you need for calibrating graded series of weights are similar to what are called solutions to the bridge tournament problem. R. C. Bose and Cameron developed (*J. Res. NBS* 1967 **71B** 149–160) a series of bridge tournament designs for comparing groups of objects that are nominally of equal total mass (or length). These are what are actually used for calibrating graded series of weights.

Willard Clatworthy joined us in 1952. He had worked on partially balanced incomplete block designs at the University of North Carolina in Chapel Hill, and continued this work at NBS, with publications in the *NBS Journal of Research* and the *NBS Applied Mathematics Series*.

Olkin: When did Joan Rosenblatt join you?

Eisenhart: She joined in 1955, after the battery additive controversy had ended.

Let me continue for a moment on experiment design. In agricultural field trials, the setting in which the art of statistical design of experiments was born, a trial begins early in a year, when all plots are planted with seed of the varieties to be compared, treatments (e.g., fertilizers, liming, etc.) are applied to selected plots at designated times during the growing season and all results (i.e., yields of the respective plots) are obtained together at the end of the growing season. Everything is set throughout the "experiment." If this year's results suggest possibly beneficial changes, these are incorporated in the plan for next year's trial. In laboratory experimentation, on the other hand, the measured "yields" of individual combinations of factors (e.g., substances, instruments, operators, temperatures, etc.) are usually obtained sequentially; promising or unpromising combinations may become apparent to the experimenter early, and he may (and often will) drop further measurement of combinations of poor "yields" and try others of greater promise. To do this efficiently one should employ *fractional factorial* experimental designs. To this end, booklets of *Fractional Factorial Designs for Factors at Two Levels* (*NBS Applied Mathematics Series* No. 48, 1957), . . . *for Factors at Three Levels* (*AMS* 54 1959) and . . . *for Factors at Two and Three Levels* (*AMS* 58 1961) were prepared under the general direction of Bill Connor and Marv Zelen. It is pleasing to note that these three publications have been reprinted as Appendices 1, 2 and 3 in Robert A. McLean and Virgil Anderson, *Applied Factorial and Fractional Designs* (Marcel Dekker, 1984).

Some of the designs were constructed by summer students: Hugh N. Pettigrew, Robert C. Burton and Forest L. Miller. Rehearsed by Connor and Zelen they gave talks at the December 1955 ASA/IMS meeting in New York City. They were trained so well by Connor and Zelen that after the meeting somebody mentioned that these kids were much better expositors than many of the other speakers.

STATISTICAL TABLES

Olkin: Churchill, one of the projects of the Bureau was the preparation of tables.

Eisenhart: Yes, that was one function of the Computation Laboratory.

FIG. 6. *NBS Director Astin watches as Secretary of Commerce, Sinclair Weeks, presents Exceptional Service Award to Churchill Eisenhart in 1957.*

Olkin: That was a massive undertaking. How did this activity come about?

Eisenhart: The Computation Laboratory was a separate group. That was one of the ways in which the statistics group at the Bureau differed from what was common at places such as the Statistics Laboratory at Ames. The statistical group at Ames has all sorts of computing machines and engages in both small- and large-scale statistical analyses. At the Bureau, small analyses were done in the Statistical Engineering Laboratory, and large computations were done by the Computation Laboratory. The Computation Laboratory was the successor to the New York Mathematical Tables Project.

Olkin: Was that a Works Project Administration (WPA) federal project?

Eisenhart: Yes, but it was always under the scientific sponsorship of the NBS from its start in 1938. It ultimately was moved to Washington and was a separate unit of the Applied Mathematics Division (AMD) that prepared tables for the Bureau. It also did a lot of calculating for the rest of the government. When Condon set up the Applied Mathematics Division, he visualized that this math division would be a central mathematics unit for the entire federal government. That's why originally it was called the National Applied Mathematics Laboratories, and for a time it operated that way. We were the only agency that had the SEAC, and we had the Computation Laboratory, so we did computations for various other agencies. That's where we had some troubles in funding the Computation Lab. In the government you're not allowed to charge a profit, so that if someone in the Air Force wanted special tables, we could only charge them for what it had actually cost the Lab. We couldn't charge anything extra to cover the pay of the Lab people while they were waiting for the next job. One year John Curtiss told the Mathematics Executive Council that the Applied Mathematics Division (AMD) was $300,000 short with only one month to go, and he didn't know where he was going to find funds. A colonel from the Air Force turned to all the other members of the Council, called them bad names and said that he had expected them to leave the AMD underfunded, so he had carefully hidden some Air Force funds to bail the AMD out because he knew that "you rascals weren't going to do a thing to help." He said he was only going to do this once.

The Mathematics Executive Council was something that John Curtiss felt he had to have. We had the Computation Laboratory, we had the SEAC, and John Curtiss could not take upon himself the responsibility of deciding between the Air Force, the Navy, the Atomic Energy Commission, the Army and so forth, who was going to get what time on the SEAC and in what order computations would be done by the Computation Lab. The Council met once a month and resolved these matters: who was going to go next on the SEAC, and who was going to get their table(s) made next by the Computation Lab. So the Computation Lab made a lot of tables that SEL had very little to do with.

When the Computation Lab was in New York as the Mathematical Tables Project, they made tables of the error function (*MT* 8, 1941) and the normal distribution (*MT* 14, 1942), the latter with a Foreword by Harold Hotelling. These were later reprinted in the NBS Applied Mathematics Series (*AMS* 41, 1954 and *AMS* 23, 1953, respectively). Gertrude Blanche, of the NBS Institute for Numerical Analysis at UCLA, prepared very extensive tables of the bivariate normal distribution for various values of the correlation coefficient, which were published in the Applied Mathematics Series (*AMS* 50, 1959).

Olkin: What about the binomial distribution.

Eisenhart: Well the binomial table (*AMS* 6, 1950) is an interesting one. The individual values of the binomial distribution table were actually tabulated by another government agency that never permitted itself to be mentioned. They had this table and one of my friends there came around with a copy and said that we could use this table in our sampling inspection work and that it ought to be more widely available but his agency couldn't publish it and in fact "can't even tell anybody we did it." I wrote an introduction, and NBS published it.

Actually, it wasn't a new table, it was simply a rearrangement of the entries in Karl Pearson's Tables of The Incomplete Beta Function. If you've ever used these incomplete beta function tables to obtain a binomial probability, you know that the notation is misleading because Pearson's "x" corresponds to the "p" of a binomial probability, his "q" to "$n - r + 1$," where n is the sample size and r the number of occurrences of the event of probability p. When I wanted to use the Incomplete Beta Function Tables to evaluate the probability of at least r successes, it would take me 15–20 minutes to get the first answer out, then after the first I could get other values quickly.

Olkin: Those normal distribution tables are outstanding from the point of view of number of decimals and accuracy. Who were the numerical analysts who helped on this?

Eisenhart: I think Milton Abramowitz, Ida Rhodes, Herbert Salzer and Gertrude Blanche were the principals.

Olkin: Was George Forsythe at the Bureau at that time?

Eisenhart: No, it was before his time. It was Jack Laderman who also helped.

Olkin: Were the calculations done by hand calculators?

Eisenhart: Yes they were all done by hand calculators. There was also a table of the power function of F-tests in the fixed-effects case. When you set your sights too high, you sometimes fall down on your face. That's what happened in this case. Neyman, Hotelling and Wald proposed, and Tukey and H. O. Hartley designed, a table of the power function of the F-test for the fixed (*not* random) effects case. This table was computed in the Computation Lab in the early fifties. Milton Abramowitz supervised the work at the start. But the table never got published because Irene Stegun was not sure of the sixth or seventh decimal place. For most statistical applications three significant figures are enough. We in SEL urged her to get it out. She said "no." She felt that she had the reputation of the New York Mathematical Tables Project and the NBS Computation Lab to uphold. I said just publish them as they do in *Biometrika*. If there are some minor errors they will eventually be corrected, but she wouldn't do that, so it never got published. Various people borrowed it to prepare F-test power function curves in their books, but it has never been published.

Olkin: Morris Hansen in his interview mentioned that the Bureau helped with the creation of FOSDIC (Film Optical Sensing Device for Input to Computers) for census data.

Eisenhart: That was done in the Electronic Instrumentation group. As I understand it, FOSDIC is still used by the Bureau of the Census and the National Weather Service; an NBS alumnus, Leighton Greenough, updates it from time to time.

WILKS, EINSTEIN AND VON NEUMANN

Olkin: Let me go back to your life at Princeton. When you were living in Princeton, your father was a Dean at the University, and you must have met lots of well-known people as a young person, before you got into the mathematical business. Tell us about these people, for example, Sam Wilks.

Eisenhart: Sam did his undergraduate work at the University of Texas under E. L. Dodd and his graduate work at the University of Iowa where he and Allen Craig were students of H. L. Rietz. Wilks wrote his doctoral thesis on small sample theory for answering a number of questions arising in the use of matched groups in experimental psychology. Then he sort of outgrew Rietz. Rietz suggested that Wilks go to work with Hotelling at Columbia, and that's where he really got into his stride. While there (1931–1932) he wrote or completed four distinct papers on multivariate analysis, one being his great paper, "Certain generalizations in analysis of variance" (*Biometrika* 1932 24 471–494). While Sam was in Cambridge (early in 1933), John Wishart suggested to him that there was some dissatisfaction about Fisher's geometric proof of the independence of the component sums of squares in the analysis of variance of Latin squares and randomized blocks and put this problem to Wilks. Wilks wrote a paper giving analytic proofs of the independence of the respective sums of squares. It's not an easy paper to read. It was submitted on his behalf by G. Udny Yule to the Royal Society (*not* the Royal Statistical Society). Apparently Fisher was a referee for it, and it seems to have infuriated him because he felt that it gave the impression that he hadn't really proved the results. While awaiting a verdict from the Royal Society, Wilks received a letter from the Royal Society saying that his manuscript had been lost and asking for a second copy, which he sent. In due course, the second copy was returned *rejected*.

I'm pretty sure the villain in this, if any, was Fisher, not J. O. Irwin as Sam believed. In his early years at Princeton (1933–), Sam complained repeatedly that Irwin had stolen his stuff. Irwin had published a paper in the *Supplement to the Journal of the Royal Statistical Society* (Vol. 1, 1934) that made use of Helmert's transformation to prove "the independence of the constituent items in analysis of variance." The exposition was at a very elementary level. When I went to London in 1935, I could understand Irwin's exposition, and I'm sure that at that time I would not have been able to understand Sam's.

Ted Anderson, I think, found the rejected "second copy" in the cellar of the Wilks house and with Mrs. Wilks' permission we donated it to Wilks archives at the American Philosophical Society in Philadelphia. I'm quite sure that J. O. Irwin is an innocent victim, so to speak. I've talked to Irwin about Sam's 1933 manuscript. He had never heard anything about it, didn't know it existed. Irwin told me what had happened: "Fisher came in to see me one day and said it seems that some people are having difficulty with my geometric proofs, and its very easy to do algebraically, you know. He sat down and sketched to me what to do. I wrote my paper in complete innocence." I believe him. I'm sure that's what happened.

Now came the exciting part in the spring of 1933. Sam's Fellowship was about to run out. He wrote to Hotelling. He wrote to Texas. He wrote to Rietz. He wrote to Harry Carver at Michigan, the founder in 1930 of the *Annals of the Mathematical Statistics*. He wrote to everybody to try to get a job. He had a new baby, and soon he was going to be without money. No university to which he had applied would give him a job.

Hotelling had been corresponding with my father for some time. Hotelling told my father this man was one of the leading lights in the field and he didn't have a job. So my father over the opposition of the entire Math Department, I understand, appointed Sam to an instructor position in the Math Department.

In 1924 Dad had been on a committee of the Mathematical Association of America to review some objections that had been made about the College Board's processing of the data from the Scholastic Aptitude and other tests that they gave. Carl Brigham who headed the research office of the College Board in Princeton later talked to Dad about the fact that the Board needed a statistician who could handle multivariate problems. So when Dad offered Sam a job he really handed him two jobs. He offered him a job as an Instructor in the Math Department and arranged a separate job with the College Board to work on multivariate problems. Dad was a hard person in those days to overcome. He was not only the Chairman of the Mathematics Department but also Dean of the Faculty and the Chairman of the University Research Committee. You don't really argue with that kind of fellow.

Olkin: So Sam did go to Princeton?

Eisenhart: Yes, in September 1933, and Sam was everlastingly grateful. Later he got wonderful offers from Texas, but out of loyalty to Dad he would never leave. He said the Dean went out of his way to rescue him when he was in need, and he should be loyal to him to his death, and he was.

Olkin: Well he certainly built up a tremendous group of graduate students. They populated the profession for many years. What was your relationship with Sam?

Eisenhart: It was very good. But a very awkward thing happened at the beginning. What happened you see was that Princeton didn't know Sam was coming. There had been no statistics of the modern sort taught at Princeton up to that time. There was a course, traditional economic statistics, taught in the Economics Department by Professor J. G. Smith. In those days you computed a lot of index numbers, and least-squares regressions by the Doolittle method in the so-called laboratory session. Acheson Duncan was our laboratory instructor. It was a traditional course in economic statistics. Now, it so happens that the Head of the Economics Department had come to see my father around about 1931. Charles F. Roos had been at Princeton in 1927–28 and had aroused interest in mathematical economics along the lines of G. C. Evans of the Rice Institute and Henry Schultz of Chicago. The Head of the Economics Department came to my father and said, "We have nobody in the department who is capable of giving this course, we suggest that we send one of our younger men off to study with Henry Schultz, come back and give a course on mathematical economics." My father, as Chairman of the Research Committee said that he would agree to this with one understanding; the young man will spend half a year with Schultz at Chicago and half a year with Hotelling, who at that time was at Stanford. The victim was Acheson Duncan who was busy at work on a doctoral thesis on South African gold and monetary policy. He was yanked off his thesis and shipped off to Chicago to study with Schultz during the first half of the academic year 1931–32. He never got his trip to Stanford because by the time he had finished with Schultz, Hotelling had moved to Columbia, so he studied with Hotelling there. The fall that Sam arrived, Duncan was giving for the first time the course that he had been sent away to learn how to do. He used Hotelling's mimeographed notes on Statistical Inference for the text. And so here was Sam at Princeton, and he wasn't giving a statistics course. There was a lot of criticism about the fact that Sam didn't give a course in statistics until about 1936. The reason for that was that statistics courses were the prerogative of the Economics Department. To have a new course you had to obtain the approval of the Faculty Course Committee. The Depression was still on, and funds were scarce, so it wasn't until 1936 that the Faculty Committee approved an introductory course in statistics in the Math Department by Sam and an economics statistics course in the Department of Economics. In the meanwhile, Professor G. C. Chambers who had been giving a graduate course in modern statistical methods at the University of Pennsylvania had died, so Sam gave such a course at the University of Pennsylvania in 1935–36.

Olkin: You mentioned that your father gave Sam a job over the objections of the Math Department. Princeton had one of the most distinguished math departments ever. I gather that statistics did not fare that well in terms of the Math Department?

Eisenhart: The Math Department was strong on algebra, analysis, geometry, mathematical physics, and so on, and statistics was just not their thing. [For a fuller and crisper account of the foregoing, see Churchill Eisenhart, "S. S. Wilks' Princeton Appointment, and Statistics at Princeton before Wilks," pp. 577–587 in *A Century of Mathematics in America*, Part III, American Mathematical Society, 1989.]

Olkin: In fact they didn't have anybody really, did they?

Eisenhart: No.

Olkin: This may get us away from statisticians—but did you know von Neumann during that time?

Eisenhart: Yes. Well I could tell you about Einstein too. I'll tell you about Einstein first. I was taking a course with H. P. Robertson that we students called "relativity and poker." A short time after that the *Scientific American* wrote to Einstein, said that there was a big controversy going on about "the ether," and wanted Einstein to write a paper about this. Einstein was always reluctant to get into controversy. He apparently mentioned the *Scientific American* request to Robertson, saying that he was not anxious to write this and asked whether Robertson had any students who could do it? Robertson said "yes" and asked me to do it. So sure enough, I did it. I sent it to Einstein. He made a few changes and sent it off. It came out in the November 1934 *Scientific American*. That sort of told me that the *Scientific American* was a nice place to publish papers. So I dug up a paper that I had written when I was a student in Lawrenceville on the impossibility of trisecting the angle while you abide by the rules. I sent that off too. They gave it a title of their own: "Trisecting the Impossible, Or Why the Angle Trisector is Wasting His Time." It appeared in the April 1936 issue.

That had repercussions many years later. When Dr. Condon was the NBS Director, he came to me one day and said Dr. Clemence of the U.S. Naval Observatory has a letter from a quack who thinks that he has trisected the angle. He said, "You've written on that subject. Would you write a reply for Dr. Clemence?" So I wrote a reply. Dr. Condon approved it and shipped it down to the Naval Observatory. A few months later, Dr. Condon came in with a smile and said, "How are you on trisecting the angle?" I said "Why do you ask?" He said, "The same fellow has now written to the Secretary of the Commerce, who has sent the letter up to me. So would you write another essay on the subject?" I said, "Well, I guess I can." So I wrote a second essay on the matter. By this time I was beginning to get respect for those people who write theses on the same topic for several students. I thought that that was the end of the matter, but no: The fellow wrote to President Truman. I was asked if I could do a third reply. I did a third one. All were different, and I hoped that I wouldn't get one from a king or the Pope. I didn't believe that I could write a fourth different essay on this subject.

Olkin: Did you meet Einstein much?

Eisenhart: I met Einstein rarely. On one occasion, my wife and I took him to the University's McCarther Theater to hear the Philadelphia Orchestra. It was interesting: When I put my hat under my seat, he put his under his seat; if I crossed my legs, he would cross his legs, and so on. During the intermission the orchestra invited him up on the stage. The curtain was down, but we could hear a violin playing. Very nice fellow.

There's an amusing story I like to tell about him. When IBM was going to dedicate its card control calculator on Madison Avenue, they sent an invitation to Dr. Einstein and didn't hear from him. They sent a second letter and didn't hear from him. One of my father's students who worked for IBM wrote to my father and said that they'd asked Professor Einstein a few times and he hadn't answered. My father replied, "That's very unusual. Professor Einstein rarely attends such functions, but he always replies. There must be some mistake here." So, I followed Dad over to see Einstein. My father told Professor Einstein about the IBM letters. Professor Einstein had a wastebasket that stood about three feet high. He tipped this over, spilled the things in it on the floor; got down on his hands and knees and began crawling around, and then all of a sudden he came up with the letter. He said, "Hah, here it is; but you see it looks like it's printed, and I never pay any attention to printed materials." It had been typed on the IBM Executive typewriter.

Olkin: That's amusing. What about Einstein's role during the atomic era? Were you there at that time?

Eisenhart: No.

Olkin: By then you had left?

Eisenhart: Yes, but I attended one seminar in which the gentle character of Einstein was evident. This particular seminar was given by one of the other German professors who was there. He gave what he claimed was a much simpler proof of some result of Einstein's. He was sort of arrogant about it, and after it was all over, everybody looked to see what Einstein was going to say. Einstein got up and said he was delighted to hear the presentation of this proof, and he was sure that the speaker was aware the assumptions underlying his derivation were different from the assumptions that he had made, and he was pleased to discover that his results were of greater generality than he had realized. Everybody felt that Einstein had won the day.

Olkin: That's a good story.

Eisenhart: Von Neumann was just unbelievably bright and quick. In a certain sense he was for a while a bad influence on the graduate students of Princeton because Johnnie could go to parties and wine and dine and drink until 2 or 3 in the morning and show up the next morning at eight o'clock for lectures and just be clear as a crystal. Some of the graduate students thought that the way to be bright like Johnnie was to do all this partying and drinking. They just couldn't do it and be clear-headed the next day. My father recognized that Johnnie was bright and was quick, but also he thought that like many people of his ilk Johnnie had worked on a great many problems before so he could draw on this background. One day Sam was working on something new and was telling me about this. Johnnie wandered up and said, "Well what are the statisticians talking about today?" Sam told Johnnie what he was working on. Johnnie said immediately, "That's a very interesting problem. You're going to have some trouble because there's a severe discontinuity with that distribution. Have you considered trying to find the distribution of the reciprocal function in closed form?" Sam smiled and said "That's just what I've succeeded in doing."

Olkin: He knew what the point was.

Eisenhart: Just like that, you know.

Olkin: I gather that von Neumann was not antagonistic toward statistics?

Eisenhart: Oh, no, no.

Olkin: He was too broad.

Eisenhart: Yes.

Olkin: Who were some of the faculty at Princeton?

Eisenhart: H. P. Robertson was there. Later, he went to UCLA.

Olkin: Was Bochner there?

Eisenhart: Bochner was there, I took a course in complex variables with Bochner and that's how I learned German. I found it was easier to read Bochner's book in German than read Wiener's book in English, so that's how I happened to learn German.

The reason why I do not know anything about matrices is that I took a course on that with J. H. M. Wedderburn. I never would have passed his course without the help of Merrill Flood and Nathan Jacobson. Wedderburn was an absolutely unclear lecturer, and if you asked him a question, you thought he was going to have a heart attack. So you never asked him questions. Also, his book on matrices is not what you'd call easy reading. I just had no appetite for matrices after that course, which is a pity because matrices are great things in statistics. If I had studied matrices from Birkhoff and MacLane, I might have fared much better.

Olkin: Yes, it is a very useful tool. What are some of the other things you'd like to talk about. Are there any particular research areas? We have really covered quite a bit.

Eisenhart: Well there's just one more thing that I probably should mention because it's had a permanent effect. I used to lecture at the Bureau on statistical methods applied to measurements, and I used to talk about a measurement process being a production process and the use of control charts and so forth. Let me go way back for a moment. When I was in London as a graduate student (1935–37), I think probably suggested by Dr. Condon, I was asked by the editor of the mathematics section of a new *Handbook of Engineering Fundamentals* to rewrite the chapter on the theory of errors. Well, I did. But apparently I did such a good job that it was a failure. My chapter was a combination of Fisher's statistical methods and Shewhart's statistical quality control. The mathematics editor looked at this and said that it was too different from anything he had seen before, so he couldn't possibly accept it. So the mathematics editor essentially copied from earlier books on the theory of errors, and all that I got in was one paragraph that pointed out how unreliable a computed standard deviation is when computed from a small number of observations. That's all I got into that book (Ovid W. Eshbach, *Handbook of Engineering Fundamentals*, Wiley, 1936).

MORE ABOUT THE BUREAU

Back to the Bureau. At NBS I gave a course on statistical methods that had Fisherian statistical methods in it and also Shewhart's control chart techniques. General Leslie E. Simon always said that I was the one who invented the idea of measurement as a production process. I didn't know when I first said this, but I knew that I had the idea by the time I got to the Bureau (1946). Later, looking over some old committee-work files, I found that way back in 1939, I had apparently discussed this at a meeting of the Joint (ASA, AMS, ASME, ASTM) Committee on Statistical Methods in Engineering and Manufacturing of which Shewhart was the chairman and I was an ASA representative (1938–49). I apparently discussed this at some meeting, and Shewhart suggested that I write a little booklet on measurement from the viewpoint of quality control. Then World War II came, I got involved in war work and forgot all about it. About 1947 General Simon reminded me about it, and so that's when I began talking about it at the Bureau.

Then in 1962 we had a crisis. There was going to be a meeting of the National Conference of Standards Laboratories at Boulder, Colorado. There was to be a paper presented there on statistics and measurement by a fellow from an aircraft company. Youden came to see me with his manuscript and he said, "Church, you have got to get on that program otherwise there is going to be broad side misinformation." So I called the chairman of the conference, told him that I had lectured on statistics of measurement at the Bureau for some time, and he agreed to put me on the program. So I wrote a paper initially entitled "Realistic Evaluation of the Precision and Accuracy of Measurement Systems." It had to go through the NBS editorial review. It was highly endorsed by Forest Harris who had written the landmark book *Electrical Measurements* (Wiley, 1952). It was endorsed by some of the other people too. With a change in title to "Instrument Calibration Systems," it went through and it became an historic paper (*NBS J. Res.* 1963 **67**C 161–187; reprinted in *NBS Special Publications 300*, Vol. 1, 1969). It was that paper that inspired Joe Cameron and Paul Pontius to introduce their Measurement Assurance Programs, which are a very basic thing at the Bureau and top-echelon measurement laboratories around the country.

From its earliest days the Bureau had calibrated sets of weights, meter bars, gauge blocks, resistors, capacitors, thermometers and so on for industry, commercial laboratories and academe. Cameron and Pontius pointed out that this was not sufficient to ensure accurate measurements. For example, a laboratory might possess a set of weights accurately calibrated by the NBS, but some of its weighing staff might be inept, so the accuracy of the weights was lost in their application. What was needed was a means of ensuring that they were weighing accurately too. To this end, Pontius and Cameron introduced the concept and technique of a mass measurement assurance program at a seminar on mass measurement at the NBS in November–December 1964. Mass measurement was viewed as a production process. To determine whether it was in a state of statistical control, the mass(es) of one or more auxiliary standard masses were always determined as "unknowns" along with the calibration of any particular (e.g., customer's) set of weights. It was the control charts for the average and standard deviation of the repeated measurements of the auxiliary standard masses that indicated whether the process was in a state of statistical control, and, if so, its precision. [A lucid exposition of the procedure is Paul E. Pontius and Joseph M. Cameron, *Realistic Uncertainties and the Mass Measurement Process: An Illustrated Review*, NBS Monograph No. 103, U.S. Government Printing Office, Washington, D.C., August 1967.]

Olkin: Based on your experience, what are your views about statistics?

Eisenhart: When I started applying statistics in agriculture at Wisconsin, I used the experimental designs that Fisher and Yates had devised for field experiments. When I came to the Bureau, we had new designs that were tailor-made to the circumstances of physical science experimentation and engineering testing. The thrust of these designs was not only to have designs where you could separate out the various effects and their interactions but also such that the arithmetic would be easy. It was also very important to have things balanced as much as possible because, if experiments are unbalanced, there is a problem with evaluating some of the interactions. The trouble is that exactly what analysis is appropriate depends upon what assumptions are made about the interactions; and, unfortunately, the data may be such that they do not throw light on which of alternative possible assumptions is right.

A central idea of a lot of the designs we used at the Bureau was to make the arithmetic easy. Then, when computers came along, that wasn't so important. Now people do complicated experiments, which we would never have attempted before. The computer doesn't care about how complicated the arithmetic is. So the computer made a big difference. Also the computers have made it possible to analyze the real problem that the customer has. I fear that in our early days at the Bureau, we sometimes twisted the customer's problem around to some extent to one that we could solve. Today we could face his problem forthright, and analysis by simulation would show whether his solution was or was not a good one from its operating characteristics viewpoint.

Olkin: What about the health of the statistics profession?

Eisenhart: I think the statistic profession is probably getting to be healthier. When I first started out at NBS, it was very hard to find somebody—except from places like Ames, Iowa or Raleigh, North Carolina—who was any good for us. There were plenty of people who could write papers for the *Annals of Mathematical Statistics*, but hardly anybody who would look at some problem that a researcher had and say what you need there is simply a sign test. The *Annals* guy would come up with a proposed solution to the problem as a random walk in Banach space or something far out like that. Actually for a while at NBS I felt the recruits for our work were better if you hired people who had a Master's degree and hadn't been ruined by a doctorate.

Olkin: You think that's changed?

Eisenhart: I don't know whether that's changed or not, but there certainly are a lot of people who have practical experience now.

Olkin: You think that there is more emphasis now on problems in the training of doctorates in statistics?

Eisenhart: Certainly there are more people out in the field and industry who are doing problems; what degrees they have, I don't know.

Another big change has been the revival of Bayesian methods. When we started, Bayes was a forbidden word. To some extent, for some purposes, we still are skeptical of Bayes. I remember one of the administrative decisions I had to make with regard to a man who was going to give a paper up at Aberdeen after he had joined us. He had written this paper before he had joined us, and I had to tell him that he could not give it as a paper from the Bureau. The reason being that this particular Bayesian paper was not empirical Bayes or anything like that; it wasn't based on past experience; it was subjective Bayes. I was terribly afraid that this fellow's paper would result in some colonel somewhere telling the people who were testing that he knew where the answer lies with such-and-such probability, "Now build that into your analysis." I just didn't want the results of munitions testing to be subject to the personal opinion of a colonel.

One of the things I've always emphasized is: Don't do the Student's t-test if you can possibly avoid it. If, on the basis of many measurements of pretty much the same kind, you know that this instrument's sigma is about 3.6, then use normal theory for sigma equal to 3.6. Don't use the sample standard deviation that you calculated from a few measurements.

In cases where there is some kind of evidence you can use to work up a prior or empirical Bayes prior, I think you can do that. I'm just concerned about the subjective part of the Bayes approach. On the other hand in the social sciences, apparently subjective Bayes is the thing.

Olkin: We have come to the end of our interview. Thank you for providing so much history about an agency that has had such an important impact on applications.