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CHURCHILL EISENHART

This is an interview on 10 July 1984 with Churchill Eisenhart in his office at the National Bureau of Standards, in Gaithersburg, Maryland. The interviewer is William Aspray.

Aspray: Why don't you start by telling us something about your father?

Eisenhart: My father was apparently a very good teacher. He loved teaching, and, contrary to what is the case with many people who become deans of one sort of another, he continued to give an undergraduate course all through his career. I think he alternated between a freshman course and a sophomore course, or something like that. When they built the new Fine Hall they built this fancy Eisenhart Lounge on the top, which reflects I think my Uncle Herbert's taste more than my father's. My father was a modest and non-flashy individual; this fancy lounge up on the top is a tribute to Dad from his younger brother.

Aspray: I had it reported to me that your father went out of his way to teach a course at the freshman or sophomore level each year, because he wanted to set that as a role model for many of the other people in the department who were not very interested in teaching lower-level undergraduate-courses. Can you comment on that?

Eisenhart: I think that this was probably the case. The idea of having senior faculty teach underclass courses goes back to Woodrow Wilson. Dad wanted to teach a freshman course even when he became Dean of the Graduate College in order to get a feel for what the new generation of students was like. He felt it important to keep your hand on that pulse. And he was very much against the idea of what you might call a research mathematician who doesn't do any ordinary teaching.

Aspray: So in a way he was the balance to Veblen in the department.

Eisenhart: Yes.   He had great respect for Veblen, and when Dad got married again when I was five, I spent the summer with the Veblen's in their house on, I guess it's called, Battle Road, beyond the Eisenhart Arch.   They were a very nice couple.

Well, to go back to one of the things I was saying before, there is still, I think, in [the present] Fine Hall, down on one of the lower floors, a little room which was the original Eisenhart Room.   It is a small room for students to study in, and it has a table by the window where my wife and I, when we look in there, can sort of see Dad sitting, turned toward some students he is saying something to.   It is right next to another room that is I think dedicated to Sam Wilks. That room brings out, at least to us, the man my father was, more than the lounge on top.   To John Tukey I was complaining one day about the fancy room on top, and he said, "Churchill, you've just got to remember that those of us who knew your Dad appreciate this little room, but your father had a big impact on the university and on the department.   If there were only that little room, future generations would see it and say, "He couldn't have been a very important guy if there was just this little room for him.""

There was Veblen and Wedderburn, and later Lefschetz and Tracy Thomas.   I think Veblen was the one Dad was the closest to professionally; they were both geometers.   In 1933 Dad appointed Sam Wilks to the Department over, I understand, the opposition of the rest of the Department.   He got away with it, I guess, because he was the chairman of the department, Dean of the Faculty, and chairman of the Research Committee.   So you can't very well argue with that person. I've often wondered, though, whether he would have done it if Veblen had still been in the department not over at the Institute, which was housed in the same building.   I don't know.

Aspray: Your father was close socially as well as intellectually to Veblen.   Isn't that correct?

Eisenhart: Yes.

Aspray: Can you tell me how your father felt about getting so involved, and for so long, in administration at the University, and how he reconciled that with his strong interest in teaching?

Eisenhart: I don't know.   He never commented on it.   He seemed to take to it.   I think it was a thing that he felt should be done, and he didn't let it interrupt him very much.   He had a remarkable feature that I have not inherited from him—a zero delay-time.   If I am working on a manuscript and I get interrupted every fifteen minutes, I get nothing done at all.   But Dad could get a lot done.

When we were living in the Dean's House, which is on the campus right there on Nassau Street, and I was going to school at Lawrenceville, sometimes after school I would visit him in his office,

and then we would come home to dinner together. There were a number of times, I remember, when it was toward the end of the day and dean appointments were over, but he was still in his dean's office there on the first floor of Nassau Hall. He would be sitting there working on a mathematical manuscript, and the secretary would buzz him and say that Professor So-and-so was there about the fellowship program or some such matter. Dad would spin around, get up, and welcome the man in. They would talk about the thing for a while—what seemed to my father a long while—and then the man would go.

After he left my father would say, "These fellows are all long-winded." He'd sit down, and to my great astonishment he would pick up his pen and start to write right away, apparently right where he'd been. There was no reading over the previous page or previous paragraph, which I would have to do in order to get back in my stride. Now I could never do that, and I've seen him do that many, many times, with telephone interruptions and so forth. Apparently he had what he was going to write clearly in his head, and it just sort of treaded water there until he got a chance to get to it again.

Aspray: It is clear, just from the kinds of appointments that he got throughout his career, that he was well respected by the rest of the faculty and that they thought he was an able administrator. Do you have any idea whether he enjoyed this administrative work or undertook it just as a duty?

Eisenhart: I really don't know. He must have enjoyed it, but I really can't tell you whether he thought it was a duty or not, because he didn't talk about these things very much. To give you an illustration. I don't know what year this was, but the mathematical society was meeting somewhere. Dad said to his wife, "Katy, I'll have to be gone the next couple days, because I have to go to the mathematical meeting." "Oh Luty," she said, "you know you really need a rest," and so forth. "Why don't you take a few days off?" He said, "I can't. I'm the president. I have to give a talk." That was the first time that she knew he was the president. [He was president of the American Mathematical Society, 1931-1932.]

Aspray: I see. Do you remember his being particularly troubled by any decisions that he had to make? Do you remember any things that weighed upon him?

Eisenhart: No, I was never aware of his being weighed upon by the decisions he had to make as a dean or as the chairman of the department. I do know that he was aggravated at the way the University handled the Fine Hall situation. First, when the building was given in memory of Dean Fine by Thomas D. Jones—I can't remember whether it was Jones himself or his niece—it was also endowed so that it would cost the university nothing, not in maintenance, not in repairs, nothing. Dad felt very strongly that that endowment should be kept distinct for Fine Hall, but the fiscal people at the University just made a note of the magnitude of it and dumped the funds right into the regular university maintenance funds. Dad

felt that that was dishonest. He was also very much annoyed when they essentially evicted the mathematicians from Fine Hall. He felt this had been built as a mathematics building: it had mathematical symbols in it, it had the fireplaces, it had the blackboards that came out, it was made for mathematicians. And he felt that the excuse that the students couldn't go to classes from there because the campus had moved down, with the athletic field being eliminated and turned into an engineering unit, that this was a pretty weak excuse. I remember being over the house when he apparently signed something accepting the change. "It's a lost cause. I might as well go along," he said.

Aspray: This story is entirely new for me. I'd heard only that the math department was required to move because of the growing size of the department. You're suggesting that there were a number of other factors as well.

Eisenhart: What I understood as the basic reason given was that the students couldn't make it in the 10 minutes between classes from Fine Hall all the way down to that new Engineering Quadrangle. When they went from there to McCosh Hall they could do it.

Aspray: Can you tell me something about the role that your father played in the development of Princeton's mathematics department as a place of graduate instruction and research? The story that I have received so far has emphasized the role that Veblen played in these developments. I suspect, just because of the personalities of the people and such, that your father played a much more important role in these events than has come out so far.

Eisenhart: I really don't know. I have no knowledge on that at all.

Aspray: What about in the appointments of people. We will talk this afternoon, in great detail I assume, about Sam Wilks and the development of statistics. He certainly played a major role in the growth of statistics.

Eisenhart: Yes, that's right.

Aspray: What about the appointments of some of the other people, say Einar Hille or later on Albert Tucker? Do you know what role he played in the hiring of them?

Eisenhart: I have no idea what role he played in the hiring. When Tucker came was he already interested in operations research?

Aspray: He was primarily interested in geometry.

Eisenhart: I really wouldn't know. I know that Dad, in spite of being a geometer, always considered himself an applied mathematician.

Aspray: Maybe we should investigate that some more, although we'll talk about it this afternoon. When people look to Princeton, they think about all of this pure mathematics. If they talk about applied

mathematics they look closely at a couple of areas because of the work of von Neumann and Einstein and that group. What about the rest of applied mathematics? Was it lost at Princeton? Did it ever get taught?

Eisenhart: Well, it doesn't ever to seem to be discussed. The thing that Princeton seemed to do was geometry. My recollections of Princeton was that geometry was the thing. I don't know just when algebra came in. You see I took algebra early at Princeton—I have probably seen the subject the same way as a result—they improved and I never did. We learned algebra out of Bocher's *Higher Algebra*, which is no way to learn algebra. You want to learn it out of Birkhoff and MacLane if you want to be able to do something with it besides just contribute to algebra. It seems to me that the attitude of at least the later teachers at Princeton was that they were teaching mathematics to people so that they could contribute to mathematics, rather than so that they could use it to do other things. In contrast to that attitude we have the fact that my father brought down to Princeton Thornton Fry from Bell Labs. Fry gave a course in differential equations, real analysis, and applied mathematics.

Aspray: I think that's about right.

Eisenhart: Incidentally, I don't know whether the copy that you saw of my Wilks thing had been corrected or not. The printed version is incorrect as to when I took the two courses, the one with Jim Smith and the one with Atch Duncan. My memory was that I had taken the Smith one in the spring of '33 and the Duncan one in the fall of '33. Atch Duncan, who is still alive and over at Hopkins, said, "That may very well have been possible the first time, but the courses were usually given in the fall and spring, respectively, and you would have taken mine in the spring of '34. Why don't you check with the University?" I called the University, and to my great astonishment the girl said, "You're Class of '34, and you want to know when you took economics?" I gave her the course numbers, and she read the dates to me just like that. I said, "That's amazing. I thought that you were going to say that you had to go to the archives and that you'd write me in two weeks." She said, "Anything else you want to know?" I said, "When did I take Robertson's course on what we called "relativity and poker"?" She said, "You took that in the spring of your sophomore year."

Aspray: Did Robertson have any interests in applied mathematics, broadly conceived?

Eisenhart: Yes, he did. I took this course with him; the books are actually sitting on a shelf at home. We took relativity for the first half of it and used the book by Bolton [L. Bolton, *An Introduction to Relativity,* New York: E.P. Dutton]. We looked at some of Einstein's little books, too. We worked mostly on the special theory of relativity and a little bit on the general theory of relativity. Then the second semester was probability and its engineering uses out of Fry.

Now the following comment may be significant. There was this William Marshall Bullitt Prize coming up. I was not planning to enter it, because I liked mathematics to do things with it. I was not a mathematician's mathematician, who liked to do mathematics for its own sake. In other words, I wasn't interested in developing some new theorem in algebra or analysis or something else just so that you'd have a new theorem. If I had a new theorem of analysis or algebra that enabled me to design gear ratios or something better, that would be great. So I wasn't going to enter the contest at all.

It was Condon who, on Robertson's recommendation, persuaded me. Robertson had told Condon, who was in charge of the contest, that I had shown an interest in the theory of errors. So Condon took me aside and said that Robertson had told him this and asked about my writing something on the theory of errors. My response, as I recall it, was that the theory of errors seemed to be a very dull subject, that it had already been done by Gauss a hundred years ago, and that there was nothing more to it. He said, "Oh, you're wrong," and loaned me his copy of Fisher's *Statistical Methods*. I read this, and I replied to him "My gracious. If this guy is correct, then it seems to me that most of the physicists that I know who teach us in the laboratory courses don't know how to handle a small number of observations." Condon said, "Okay, you've got the theme. Get going." That was how I got started, you see.

Then, as I always like to say when I tell this to people, I had the good fortune to have the right father at this point. I said to Condon, "You say this fellow's book is sound. It's very hard to tell because there are no theorems in it. He talks a lot about the t-distribution and gives a table for it, but he never says what it is. You have to take everything on faith." He said, "It's good, it's sound." So I was complaining about this to my father one day at home. Dad said, "Did you say Fisher?" I said, "Yes." He said, "You know, there was a guy with a beard and thick glasses at the mathematical congress at Toronto. He gave a paper there in 1924. I was one of the editors of the proceedings, which came out about 1928. I think he said in that paper that it was the paper for which some of the reviewers of his book felt a need." So he goes to the shelf and takes down a great big blue book and thumbs through it, and, sure enough, there is Fisher's basic paper "On the distribution yielding the error functions of several well known statistics". So that's why I said I had the good fortune to have the right father at that point.

Aspray: You'd said a few minutes back that—I am quoting you roughly—later mathematicians at Princeton were interested in teaching mathematics only for arriving at further mathematical theorems. By "later" when do you mean, and who do you have in mind?

Eisenhart: Well, it may have been true when I was there, but I wasn't conscious of it. I wasn't conscious of it, yet from Wedderburn I didn't learn a thing. I managed to pass the matrix-theory course only because there were some people called Merrill Flood and Nathan Jacobson taking it who helped me through. When I said "later" I was

thinking of the time when I was a member of an advisory council, or whatever its called, of the math department. We kept getting these complaints from the students. One complaint concerned the contrast between the teachers in physics and the teachers in math, that the teachers in physics courses seemed to be enthusiastic about the subject and were trying to communicate that enthusiasm to the students and the math professors seemed to be just going through the motions of teaching in order to get it over with. The other complaint by the students was that each math professor seemed to want to make the students in his course creative artists in his specialty, so the stuff was turned out with such intensity that if you were majoring in the geometry/topology area and had an interest in analysis, you just hadn't time to take courses in analysis. The fellows resented this, you know.

Aspray: Was this undergraduate or graduate students?

Eisenhart: This was graduate students, first-year graduate students. Now the undergraduates—I don't remember them doing it in my day—complain about the fact that there not enough applied courses.

Aspray: So I get a rough dating, when was this "later"?

Eisenhart: Before, I guess, 1970. I could look to see when I first got appointed. [It was 1976. CE]

Aspray: I see, roughly 1970?

Eisenhart: I don't recall. You see when Robertson and Condon were there they gave courses in mathematical physics and such. Now I don't know whether those courses—I took some of them—are different from the courses of Wightman, say, who had a joint appointment with physics. I'm not sure whether Wightman's courses were mathematical physics with a big 'm', or with a big 'p' and a small 'm'.

Aspray: Can you comment on the impact that the European mathematicians coming into the Princeton community had, both in terms of the overall quality and in terms of the kinds of interests and topics?

Eisenhart: Of course the impact was great. Everybody was very much interested in their being there. It was a very stimulating thing, you know; Weyl was there, von Neumann was there, and of course Einstein. The only negative aspect I can remember was a feature of von Neumann. The Alexanders gave humdinger, wonderful parties. I don't know whether they would be regarded as outlandish today, but they were certainly regarded as far out in those days. The phenomenal feature of von Neumann was that he could go to these parties and party and drink and whoop it up to the early hours of the morning, and then come in the next morning at 8:30, hold class, and give an absolutely lucid lecture. What happened is that some of the graduate students thought that the way to be like von Neumannn was to live like him, and they couldn't do it.

Aspray: Did you see any change in the kinds of research problems, or in the kinds of problems that were talked about around the department, after the arrival of Weyl and von Neumann and Einstein?

Eisenhart: Yes, relativity and those things—it was the thing to do, to get in touch with these.

Aspray: Robertson had been interested in these subjects before they came?

Eisenhart: Oh yes. My recollection isn't too good, but it seems to me that there was a lot of interest in operators. I guess it was von Neumann. You see, I was there as an undergraduate and only one year as a graduate. The man who was an excellent teacher that I had both as a graduate and as an undergraduate was Bohnenblust. The difference between Wedderburn and Bohnenblust was absolutely extreme. Wedderburn apparently had his lectures prepared ahead of time. If you asked a question, it practically gave him a heart attack. He would stop dead in his tracks; he would be stunned. Now you needed to ask questions of Wedderburn, because in the course as Wedderburn taught it, each lecture stood on the shoulders of the preceding lectures. But each lecture had different shoulders, and what would happen is that Wedderburn would be coming along and would say something like, "By the theorem we had the other day, this reduces to so and so." Everyone in the class would scratch his head and say, "What theorem that we had the other day?" Then somebody would ask him the question, and this would appear to be catastrophic, almost stopped the lecture.

Whereas Bohnenblust when he lectured and came to a place where he was going to use a theorem of a preceeding day would say "Now using the theorem we had last Thursday, which said..." He would repeat it, you see. "We have got here and after reduction we get this," and so he tied it in very well.

I would say that this is an aside, because it wasn't until I got to London that I encountered it. Bochner's book *Vorlesungen ueber Fourierische Integrale* I found easier to read in German, which I can't read worth a darn, than Wiener's book in English. The reason was that, though Bochner's style was very compact, as you read along Bochner always referred back to the preceding things, so you knew exactly what was going on all the time. As a matter of fact, Neyman invented a verb when he was correcting some of my writing. He said, "You've got to Bochnerize it."

Aspray: Coming back to this question of relativity and mathematics for a minute, I know that because of Veblen's interest in geometry, he was interested in relativity. Is that true of your father as well?

Eisenhart: My father I guess was interested in relativity because of the differential geometry that was there. Of course Einstein brought with him this fellow Walther Mayer, who was a geometer. Mayer died or something, and Dad used to help Einstein a bit with his geometry. Dad

I guess was a better geometer than Einstein. I never was good at differential geometry. Dad apparently was very good at the trick of separating variables. He would help people with that sort of thing. In his later years, just like Mr. Einstein, he was struggling with the business of getting a general field theory, which he didn't get.

That was one of the things that Dad was doing. If you ever get a chance to interview John Wheeler down at Texas, you might ask him about this, because I don't know how far along this had gotten. One of the things that Dad felt very strongly was that if a person was going to go into the relativity kind of physics it was an imposition on such a person to make him study all of mathematics in order to get just crucial bits from here and there. As I understand it, after he retired he and John Wheeler were working together at writing a book called *Mathematics Essential for the Theory of Relativity.* There was, for example, a book by Allen called *Mathematics for Economists,* or something like that. [R.G.D. Allen, *Mathematical Analysis for Economists,* London: Macmillan, 1938.] Dad and Wheeler, as I understand it, were bringing together in their book the mathematics, from here and there in the various branches of mathematics, you need for the general field theory.

Dad had a Gladstone bag—that's a certain kind of a suitcase, a leather bag. He pointed this out to me one day and said "If anything should ever happen to me, I want you to be sure that that is preserved." Well, my wife and I were in Princeton the day he died. We had gone up the day before and had not planned to stay. The nurse at the hospital told us that she thought it would be a good idea for us to stay. So we stayed overnight without any toothbrushes and that sort of thing, and just slept in our underwear and so forth. He died during the night. When we were over at the house, the Gladstone bag was there, beside his desk which was in the front bedroom in the house on Alexander Street. But my wife and I had to go back down to Washington to get more clothes and come back up again for the funeral. When we came back up the bag was gone.

It has never been found. My wife and I feel that mother had it destroyed. Mother got some sort of a psychosis at that point. She had plenty of money of her own, and why she thought of this I don't know, but somehow or other she had a feeling that somebody was going to get their hands on things that my father had handwritten, was going to learn how to imitate his handwriting, and then was going to draw up some documents that were going to do her out of her house and home. So she destroyed everything she could put her hands on that had his handwriting on it, and that went too. Now exactly how far they had progressed, I don't know. I know that he was working on it regularly up there in the front bedroom of the house, which he made his study. Now John Wheeler would know whether they had made any progress, or whether Daddy was in his dotage and John was just humoring him. I don't know.

Aspray: Would you believe that this extends to your father's attitudes about graduate education? If a student came in who was interested,

say, in relativity theory, would he insist on a strong, broad training in mathematics?

Eisenhart: I really don't know. I never saw him talking to any graduate students.

Aspray: The reason I ask that question is that someone— it was either Bargmann or Wigner—complained to me one time, "We wanted to bring in [as graduate students] these mathematical physicists, and the mathematicians wanted to train them in these half-dozen fields, and there was hardly any time left for them to learn mathematical physics."

Eisenhart: That's an interesting point. That sounds like the situation my father was trying to ameliorate by this book.

Aspray: While we're on the question of graduate students and education, do you want to say something about your father as a thesis advisor and producer of Ph.D.s?

Eisenhart: I have no idea who wrote theses under him. Unfortunately, I don't. He kept his professional act quite distinct, you know. He used to take theses up to Vermont to read.

I do know that he did analytic geometry in new ways. For example, he used direction cosines in 2-dimensional problems—they are usual, of course, in 3-dimensional problems. Apparently there was an uproar in the preparatory schools, where people complained bitterly about my father sort of changing the rules of the game on them. You see, their students would get a preliminary course in analytic geometry at Exeter, Lawrenceville, Groton, or somewhere, and then they'd come down to Princeton and find they were no better off than the beginning freshman who was getting this new kind of analytic geometry. Also, a lot of them complained about some of the problems that are in it, because the sort of problems that they were accustomed to getting were things like "What is the locus of y equals x?" or "What is the locus of y equals minus x?". Dad would have a question like "What is the locus of points for which y is greater than minus x and less than x?" This would just throw the fellows into a tailspin, you see. They weren't accustomed to shading a region; they were accustomed to the line loci.

Aspray: What can you tell me about the effect that the Depression had on mathematics, in particular the mathematical community at Princeton?

Eisenhart: The Depression of course had an effect on the University as a whole. I don't know whether they had to take any salary cuts—I didn't hear about that—but they weren't able to hire any new people. That's why, for example, Duncan went off to take that course. But the University kept going, and I suppose that unless they had salary cuts many of the professors were relatively better off than they were earlier, because in those days university salaries were not great. On the other hand, they were great enough that customarily everybody took the summer off, you know, and went up into the hills where they did their research.

Aspray: What effect did it have on the number of students coming to school and on the placement of new Ph.D.s?

Eisenhart: I really don't know. You see I came in there right at that point, in 1930.

Aspray: What effect did it have on your own career?

Eisenhart: I don't think it had any effect. I wasn't really conscious of it. I don't think Princeton was very affected by it.

Aspray: Are there some stories that you would like to tell about your father, anecdotes that are revealing of his personality or style?

Eisenhart: Well, I don't think of any mathematical ones at the moment. The Eisenhart family in York were Lutherans. When we came down to Washington we went to a Lutheran Church for a while. We are Episcopalian now; my mother was Episcopalian, my father was Lutheran. There was, by the way, no Lutheran church in Princeton. When I got down here to Washington, the pastor of the Lutheran church in Washington that we went to gave me a book about the history of the Lutheran Church in America. To my astonishment I read about my father's grandmother's brother. His name was Schmucker, and he was the founder of the Gettysburg Theological Seminary, which was the Lutheran theological seminary. There was no Lutheran theological seminary in the United States at that time, but he had to get ecclesiasticized somehow. The only one that was theologically near was the Presbyterian theological seminary, which was right in back of our house. So I read along, and I read that when he attended that seminary he roomed as a student in a house two houses from us. Our house was 25 Alexander Street, and I suppose the next were 27 and 29. I said to my father one day, "How come you never said that your great uncle Schmucker once lived at 29 Alexander Street?" He said, "You never asked."

Aspray: I see.

Eisenhart: One other thing about Dad that is a great disappointment to the American Philosophical Society is that he had virtually no archival papers. Dad apparently grew up in the days when frugality was the norm, and he was always very economical. His style was to take incoming letters, turn them over, and write his reply on the back. This had the result that there was virtually nothing of this sort in his files.

I have given a few things to the American Philosophical Society. One of the things—I think I have given it to them already—is a postcard of something like 1929 or 1930. It is a postcard from Hardy, G.H. Hardy, to my father challenging him to a wager on the outcome of the World Series. Hardy was very interested in the World Series. The amusing thing about it was that he said that he was betting five dollars on it, and my father was expected to bet an equal amount. My father should record his choice of outcome. I guess Hardy said whom he

picked, and if my father was going to bet on the other side he should give his five dollars to a full professor in the department. To give it to anyone of lower rank than that would be unseemly or something. I don't know whether Dad ever did it or not.

When I found the postcard I showed it to one of our members of the math department, who was here at that time. He laughed and said, "You know, if your father had bet, he would have won." Because he knew what the outcome was that year. Whether Dad bet or not I do not know, but Hardy at least challenged him. As I said, Dad kept everything to himself. I have just recently found some correspondence indicating that he was involved in some international relief organization. I want to see if the Philosophical Society wants it.

Aspray: I was wondering if he was heavily involved in helping to place refugee mathematicians?

Eisenhart: He may have been. I am not as aware of his having done this; I know R.G.D. Richardson at Brown was very active in this. Of course, we took quite a few at Princeton.

Aspray: Because of the Institute there was opportunity to offer positions.