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JOHN TUKEY

(with ALBERT TUCKER)

This is an interview with John Tukey at Princeton University on 11 April 1984. The interviewer is William Aspray, assisted by Albert Tucker.

Aspray: Could you tell me something about your pre-Princeton days and how you came to Princeton?

Tukey: My parents were teachers. My father ran the Latin department and at times other departments at the New Bedford [Massachusetts] high school. My mother was a substitute. They claim, probably correctly, they taught every course in high school, except shorthand and typing. Having them I did not go to school, which they thought a good idea, except for a semester of French, chemistry laboratory, and mechanical drawing. I got into Brown on College Boards and so on.

New Bedford had a very unusual public library. They had both the *Journal of the American Chemical Society* and the *Transactions of the American Mathematical Society*. I think I became a chemist because I could read *JACS*, but I could not read *Transactions*. I got a lot of calculus out of the way before I went to Brown, so by my sophomore year I was taking graduate courses from [C.R.] Adams and [Jakob] Tamarkin and others. Near as I can remember, I fell over the fence into mathematics in August '37. So when I arrived in Princeton in September of that year it was pretty clear to me that I was going to be in mathematics, at least for awhile, rather than chemistry. So beginning in '37 I spent a lot of time in Fine Hall.

**Tucker:** What were your first impressions of Fine Hall and its inhabitants?

**Tukey:** My first impressions of Fine Hall were all these closed solid doors. The inhabitants, I don't think, were strange enough to me to make that strong an impression. I picked Princeton on account of chemistry; that was settled six months before I decided on mathematics. So the mathematical community, the department and the Institute, was a very large bonus.

**Aspray:** How did you move over into mathematics?

**Tukey:** Well, at the end of my first year, I took prelims in math. That accomplished the move.

**Tucker:** I can add that he did so well on those prelims that the following year he had the Jacobus Fellowship, which was the top fellowship in the graduate school.

**Aspray:** I see.

**Tukey:** If I had stayed in chemistry, I would have undoubtedly worked with Henry Eyring. Henry went to Europe, I think, on sabbatical, the second semester. He used to say how he felt when he came back and found out somebody had stolen me away from chemistry. Henry was the salt of the earth. I think that everyone who ever knew him felt that way.

**Aspray:** In what area of mathematics did you begin to work after your prelims?

**Tukey:** Topology. I decided long afterwards that the sequence of my interests is explainable by the fact that my natural interest is in methodology. Chemistry could be very methodological. Topology exists to provide methodology for large hunks of the rest of mathematics. Statistics exists to provide methodology for a very diverse set of things. From my point of view, the migration is perfectly sensible. This is after-the-fact rationalization, but it might be right.

**Aspray:** Did you have an interest in statistics at this time also?

**Tukey:** When I was at Brown I read a very diverse set of books from the math library. I said read, I did not say study. There was some statistics there, and I did have a little tin-case with 3x5 cards in it for recording what seemed to be interesting statistical techniques. But it was not in any sense a major concern of mine until the war. About May 1941 I went to work for what was called Fire Control Research, which Charlie Winsor had joined in February of that year. I worked there until early 1945, when things were pretty well wound up. I migrated to Bell Labs [at Murray Hill, New Jersey].

**Tucker:** I had forgotten that you were already working there when I joined Fire Control Research. I was away from Princeton in '40-'41,

and it was not until I had returned in September '41 that I joined the outfit.

Tukey: I had about a four-month lead on you, and Charlie had a four-month lead on me.

Aspray: You retained a connection with Princeton all of this time, is that right?

Tukey: Well, in the beginning of '45 I went on leave because I was full time at Murray Hill.

Tucker: But when you ceased to be a graduate student, you were appointed a Fine instructor. You were already an assistant professor, weren't you, by the time the war broke out?

Tukey: It is possible. I would have to look at the records.

Tucker: Then you became involved with Fire Control Research in the spring of '41, and you were involved until '45.

Aspray: Where was that work conducted?

Tucker: 20 Nassau Street, right here in Princeton, a very big building at the corner of Chambers and Nassau.

Tukey: We had a field laboratory at Fortress Monroe, because in the early days we were working with anti-aircraft height- and range-finders. That was where the coast-artillery school was; it was a place where people were trained on height finders. Brock McMillan got involved with that. Are you going to interview Brock?

Tucker: I don't know. When I proposed this oral-history project to Charles Gillispie, it was because I began to feel that the people who had been here and had seen what went on at the founding of the Institute for Advanced Study were going to die off before their recollections were recorded. I have for years been trying to interest somebody, but the answer is always "You do it." I wasn't eager to do it, but I thought it ought to be done, and finally Charles Gillispie worked out a way to do it. The Sloan Foundation put out money to pay expenses and it is being done and that is what started me on this. The Sloan Foundation, which pays expenses, stipulated that it was not to be me alone, but as many other people as possible. But since Brock did not come here until after the period that we are considering ...

Tukey: When did he come?

Tucker: Oh, that is right. He was here as a Fine instructor.

Tukey: He and I were in the grad college.

Tucker: Oh yes. I forgot.

Tukey: So he was here at the end of the '30s, for a good part of 1939.

Tucker: He is living in Maine.

Tukey: He is living somewhere downriver on the east bank below Damariscotta. [Probably near Camden or Blue Hill. J.T.] We can get an address out of Murray Hill if we need it. He and I were both interested in audio and one thing and another. We actually bought a set of parts with the intention of putting up a one-bit's worth of what would now be a central processor, since the world was drifting towards computers.

Aspray: When was this?

Tukey: Probably late '39. It was never assembled, which shows we were not deeply committed to it.

Aspray: Can you tell us something about your program of study leading to your mathematics degree. Who did you talk to most? Who did you work with?

Tucker: The answer is, just about everybody.

Tukey: I went to probably more sets of lectures than a rational person should. But it did not seem to do any harm. I can report two anecdotes from what I think must have been spring '38. J.H. Van Vleck was here, a visiting physicist on sabbatical. He gave a course of lectures on electric and magnetic susceptibility, which was attended by three to twelve people, depending on the interest of the day's topic. I went to that most of the time, just out of curiosity. The group went once to Philadelphia. We took three different trains between Philadelphia and Princeton Junction. Van Vleck was a railroad buff.

The other is that Marston Morse was giving a seminar on symbolic dynamics. I was highly interested in that. It took place in the old Fine 113, the small lecture room. What happened is that Norman Steenrod sat in one back corner, and I sat in the other back corner, and we did our best to keep Marston Morse honest. Halfway through the semester, at the afternoon tea—which was where, you know, all the interesting interaction took place with people—he asked me if I was at the University or at the Institute. Since I was a chemist, I regarded that as an interesting question. So I went to a little of everything that was going on.

Tucker: That is one of the special attributes of graduate studies in mathematics at Princeton. There is no fixed program other than that you have to pass the generals, you have to submit a thesis, and you have to take a final oral examination. Oh yes, you also have to pass the language exam, but no one says you have to go to so many courses or lectures, or what the courses are that you must take. Nothing of that sort.

Tukey: In those days at least, there was a core for the prelims. You were certainly supposed to do algebra and real and complex variables.

Tucker: But it did not matter how you prepared yourself.

Tukey: No. With the other two subjects you had a lot of freedom. It was a little hard to avoid geometry or topology, but I am sure that there were a lot of people who did. I guess I prepared myself for my thesis by existing and letting things come in through the skin.

Aspray: What was your thesis topic and how did you arrive at it?

Tukey: The principal part of it was a formulation of uniformity in topology, which got published as an *Annals of Mathematics* study. Strangely enough it was plagiarized and reprinted in Taiwan.

Tucker: They do an awful lot of that. If there is somebody who wants to give a course they ...

Tukey: The chance that anybody gave a course out of that, I regard as small.

Tucker: I was in Taiwan three years ago and saw two different plagiarized copies of my son Alan's *Applied Combinatorics*. Two separate editions.

Tukey: There was some other material in my thesis, about partially ordered sets and things of this sort. I would have to lay my hands on the thesis to be sure what the stuff was that did not go into the *Annals* study. I did the thesis because it seemed interesting and relevant at that point. The only discussion of uniformity in topology that was around, I think, was a very abstract formulation in Bourbaki, and that was only for metric spaces. There was also Eliakim Hastings Moore's general analysis, but you had to be a (skilled) connoisseur of multiple superscripts (and subscripts) to understand what was going on. What I was trying to do was to do a convergence version, because if I was in anything by nature it was analysis or point set topology. This didn't attract large numbers of geometers.

Aspray: That is how I learned topology. My impression so far is that the grad students at Princeton at the time were an exceedingly independent lot who pretty much chose their own thesis-topics and did a lot of the work on their own without much direction from the faculty. Would you say that is a correct assessment?

Tukey: I think there was a broad spectrum. There were, at one extreme, grad students like that. At the other extreme there were graduate students who once they passed prelims went off somewhere to teach college having a thesis topic. Those people, it seems to me, depended upon a lot of guidance.

Tucker: Do you remember some of the people who got their degrees the same year you did?

Tukey: Well, I remember the three people that stood up on the platform, because that was the time that Harold Dodds started to say the Latin to give us an honorary degree, instead of a Ph.D. degree. I had to stop him in the middle. I think there were a total of five people that actually showed up; Ralph Fox and Joe (Joachim) Weyl were the other two mathematicians.

Tucker: Do you remember with whom Joe Weyl did his thesis?

Tukey: No. He wasn't around very much while I was here. [Solomon] Lefschetz was the chairman of my committee. That is a demonstrable fact.

Tucker: That is the only thing that really gets recorded. There was never anyplace where someone was designated as the supervisor of the thesis. There were first and second readers, and there was a chairman of the examination committee.

Tukey: Who was normally the first reader.

Tucker: But there were cases where it was the senior person on the examination committee who served as chairman. For example, I was the one who had the most to do with the writing of Shaun Wylie's thesis, but Lefschetz was committee chairman.

Tukey: Similarly, Fred Mosteller would say I had a lot to do with his thesis, but so did Sam Wilks who was chairman of the committee. Nobody cared who was listed as chairman.

Tucker: I would very much like to hear you talk about the early development of graduate work in statistics, starting with Joe Daly and going on to the end of the war period. It seems to me that that was a remarkable period in the development of statisticians: R.L. Anderson, Mosteller.

Tukey: I don't know first-hand the early history from Sam Wilks's arrival in '31, but I heard enough. Atch Duncan was sent off to learn statistics by the economics department. They had to recover the investment. So Sam couldn't teach undergraduate statistics.

Tucker: He came here in the fall of '33. He was here for three years before he taught his first undergraduate course in statistics.

Tukey: To get into the Princeton mathematics department you had to be good, and that was even true of statisticians. So you might worry about where you were going to get enough statisticians, but you did not have any reason to be particularly concerned about the quality. That's true on average. There were a few losses, but those were the days when we lost a few grad students after the first year, but seven out of the eight who remained wrote Ph.D. theses one way or another. The barrier was getting in, plus some loss at the end of the first year. Now, Joe Daly came through before the war. I've never known Joe intimately. I hardly saw him when he was here.

Tucker: I saw a lot of him because after Ed Titt left, I got the Joe and Charlotte Daly to come in and help me with the house that I had in Princeton. Joe Daly did not actually come here to study statistics. He came here to work with [Luther] Eisenhart, but although Eisenhart was very approachable, he had pressing problems of some sort the year Daly wanted to work with him. I don't remember what the problems were, but the result was that Daly had to cast around for someone to work with. Happily he got on to Sam Wilks. I feel that it was not really until the start of the '40s that Sam's school ...

Tukey: And I did not really get involved directly until '45, because I was full time across the street [at Fire Control Research]. I emphasize 'directly'. Fred Mosteller was a case of somebody of great ability, but not necessarily mathematical. Since he is now, to the best of my belief, the only man who has been chairman of four different departments in Harvard, I think we might say his reputation is secure. I may also mention that among the graduate students his first year it was predicted that he would not last to the second year. If he had been a pure mathematician, they might have been right, but having done all the things that he has done at Harvard and other places, having been president of the AAAS, etc., he has demonstrated his quality. All this says is that mathematical ability is quality on only one coordinate-axis.

Ted Anderson was a lot more mathematical. Then there was David Votaw, who was a fellow Texan, not as strong as Anderson, but a very useful person. He went to Yale in industrial engineering and then to Mitre Corporation. These people were really all on Sam's side of the hall. There were two quasi-statistical operations. The one that Sam ran at Fine Hall was known as SRGP, Statistical Research Group at Princeton. There were also some people who worked at Columbia, almost as part of SRGP. These were John Williams, who some people might have thought was an astronomer; Fred Mosteller, who was still a grad student; Leonard Jimmie Savage, who had got his degree in topology a couple of years before; and Cecil Hastings, who was the man who wrote the late '40s book on rational approximation of functions that was published at Princeton. They were mainly concerned with bombing problems.

Tucker: The quick and dirty.

Tukey: Well at least the quick. At the end of the war, if Jimmie Savage and Fred Mosteller were in the next room talking, you could not tell which it was, because they talked exactly alike from working together so much. Then there was the Fire Control Research Group.

Tucker: That we were a part of.

Tukey: Yes, and where Charlie Winsor was, except for the year-and-a-half mistake he made of going to Washington to work with the mine-warfare people, who were operating under constraints that didn't let anything useful be done. Our group did work on the height- and range-finders for NDRC, and then worked for Frankford Arsenal

and later for Colonel Trichel, who then moved to the Pentagon and worked on rocket powder and such things.

Tucker: Henry Eyring worked on the B-29.

Tukey: Then we went back to NDRC and were the coordinating group for the improvement of the B-29, which was nicely diversified work. Irving Segal was there for a while.

Tucker: He is a distinguished professor of math at MIT.

Tukey: With views on cosmology and so on.

Tucker: Who studied topology at the Institute when he graduated from Princeton.

Tukey: Then George Brown was there for a while. He later went to Ames and then to Irvine. Paul Dwyer was there for a while running the computing facilities. We had an IBM multiplying punch of the original version. It had a frame in it about 36" wide by 18" high by considerable thick which made a 1" transverse motion each cycle to make the connections to transfer the information. The manual said that you could not do A times B times C; you could only do A times B at a single pass. We at least were able to do it a little better than A times B. Paul Herget, who was running something of this sort at the Naval Observatory, ended up with a little multilith document showing 29 things to do with such a thing. It could compute payments from the amount of the mortgage and the interest rate, because the officer commanding the observatory wanted to do his income tax. That was advanced computing for the time. We did true target positions for various things at Fortress Monroe, including for the initial test of the SCR 268 (the first aircraft radar to be deployed), which we only knew was a mysterious thing called Mickey. This led to, among other things, trying to design new (optical) range finders. There was an M1E9, a modification at Kodak of the M1 that must have originally been built at K&E.

Tucker: It was really the man-machine combination that we were working on. Besides improving the mechanical operations of things, we were trying to improve the operator. We had a psychologist, William E. Kappauf working with us and a man from the Ames Institute on binocular vision.

Tukey: Leo M. Hurvich, who now I believe is in the Academy, at least I see him at Academy meetings. Both he and his wife are in the trade, and it is clear that at least one of them is in the Academy. I think they both are. That is out of your era, but it is an indication of what mathematicians did during the period. Many of us did whatever came to hand. When things wound up at 20 Nassau Street, I went to Bell Labs and ended up with the group doing the paper-and-pencil study for what became NIKE, along with Bernie Holbrook, who was roughly a 9 and 1/2 th cousin, but we did not know it at the time. He and I did aerodynamics, trajectory, and warhead for the paper-and-pencil study.

He was supposed to be a switching engineer, and I was supposed to be a computer topologist. Supersonic aerodynamics at that time was in such a mess, people even took the linearized theory seriously. The linearized theory predicted the controls would start to work in opposite directions after it went through mach root three. That was of course nonsense, but interesting. But you probably want to hear more about the feeling of the time before the war.

Aspray: Yes.

Tukey: The first year I was here I was soon drafted into what was known as Fuhrocracy.

Aspray: Fuhrocracy?

Tukey: Yes. This was a group of people who sat at the near end of the first table on the right as you went into the dining hall in the graduate college. Lyman Spitzer, who is retired as chairman of the astronomy and astrophysics department here, was the Fuhrer. He sat at the head of the table. The rest of the group was probably 3/4 mathematicians and 1/4 physicists with a single Romance linguist, who was granted the authority to put people in Klein bottles (which have only one side). Frank Smithies, who was a functional analyst from Cambridge and here post Ph.D., and Ralph Boas, who was a slightly more conventional analyst and here as a National Research Fellow, and I tended to hang out together more than with the others. So while I never had much personal contact with Bochner, I heard lots about him from Ralph. His view was that if you came in and told Bochner about something new, there were two possible answers. Either such a thing "is impossible" or it "is trivial".

Aspray: In a way you have already told me the story of statistics here, but I wonder what the attitude of the math department was towards the teaching of statistics, the granting of degrees in statistics, and the recognition of statistics as a legitimate subject.

Tukey: Well, it must have been awful in Sam's time. In September '45 I came back to Princeton with a half-time position. I was working on war work for a while, with Sam's group. That closed down and I was back to being a teacher again. By that time I was a statistician instead of a topologist. But I had a union card. I think it was, in general, harder for mathematicians to be statisticians.

Tucker: The principal friend that Sam had in the department was Eisenhart.

Tukey: Right, friend and protector.

Tucker: Eisenhart was the chairman of the department until 1945. He was all in favor of Sam Wilks and what he was doing. He was somewhat influenced in this regard by Churchill Eisenhart, his son, but nevertheless, I think he himself ...

Tukey: He never would have chaired the NRC (National Research Council) Committee on Applied Mathematical Statistics if he did not have a real personal interest in things. You know he would not do something for decoration.

Tucker: So he was the one who was supporting Sam Wilks in the department, being the chairman of the department ...

Tukey: And being Eisenhart. I mean Eisenhart and Lefschetz were the poles of power in the department, without any doubt.

Aspray: Who opposed statistics in the department?

Tukey: Well, I never saw anything overt. One issue might have been my salary, although I was not complaining. Another might have been about having another statistician, which would not have been discussed at my level until I was a full professor. By the time I was, I think around 1950, the pressures were reduced, so I am not the one who has the war stories. But if Eisenhart had not been here, Sam would have left, no doubt whatsoever.

The most sensitive issue was which graduate students got admitted. That has enough different aspects to it that it does not necessarily come to an out-and-out confrontation. I feel very bad, though, that the day before the night that Sam died there was a confrontation of this sort and I was not there because I had to go to Atlantic City to give a talk to meteorologists. To what extent that confrontation brought on Sam's death nobody knows.

Aspray: You started to tell ...

Tucker: I was going to say that I was myself quite friendly to statistics partly because as an undergraduate I studied something called actuarial science, which prepared me to pass an examination in statistics. I qualified to become an actuary, but I never did any actuarial work. Indeed I had to make a choice when I graduated from the University of Toronto. Was I going to go on in mathematics, which meant pure mathematics, or was I going to become an actuary? Becoming an actuary seemed to offer more financial compensation. But it did not seem to me to offer very much in the way of intellectual compensation, so I chose to go ahead in mathematics.

Here at Princeton in the 1930's, because I was an associate member of the actuarial society, I was given the job by Eisenhart to help the juniors and seniors who were majoring in math and hoping to get actuarial positions. Indeed I had already taught statistics. As a teaching fellow at the University of Toronto I ended up teaching nine hours a week. It was the biggest teaching load I ever carried in my life, and I was just a teaching fellow having just completed my bachelor's degree.

One of the things I taught was mathematics for economists. In those days there were no teaching materials available. I had not had a

course in economics. Fortunately it was just a small group. We worked our way through any mathematical economics I could lay my hands on. But it was a year course, so after a couple of months I decided the more sensible thing for these students was to study statistics. So I used a textbook on statistics by someone named, I think, [C.H.] Forsyth. Have you ever heard of such a book?

Tukey: I might have.

Tucker: Anyway, I learned statistics in a formal sense at that time. You can't teach something without learning it.

Tukey: Some people can't help it, but it is possible.

Tucker: It's a good way to learn. So that I felt a definite bond to Sam from the time that he came here. We actually started out as instructors the same year.

Tukey: I know Sam recognized this support.

Tucker: So while I did not have power, I was Sam's friend and defended him time and time again, especially when Lefschetz became chairman of the department. But I never could persuade Lefschetz to let Sam have any of the money from the research fund, although I tried almost every year to do that.

Aspray: What was the intellectual reason behind Lefschetz's attitude?

Tucker: Statistics was not mathematics.

Tukey: It was not good mathematics. And he was right, but that was not the issue. My view is that I don't have to feel happy about mathematicians who feel this way, but it is a common feeling. Once one of our distinguished British colleagues, a well known statistician—this went on one summer out at Stanford—argued that something could be good mathematical statistics without being either good mathematics or good statistics. I was not prepared to take that, and to hear it from a British statistician was shocking.

Tucker: Because in Britain, statistics has much more honor in mathematical circles than it has here.

Tukey: And also at that time it was much more firmly applied statistics.

Tucker: The country where statistics is particularly honored is Australia.

Aspray: Was this attitude towards statistics in the 1930s fairly common across the U.S.?

Tukey: I was not there, but I think so.

Tucker: I hesitate to speak about it because I did not have much knowledge of it even at Princeton.

Tukey: There were very few places where serious research was going on. George Snedecor had things going on at Ames, Iowa. They had their 50-year celebration last spring: 50 years of the statistics laboratory. The department has a much shorter life than that; for a long time it was only the statistics laboratory.

Charlie Winsor's father founded the Winsor school for boys, north of Boston; it's a well-known school. Charlie went to Harvard, but left at age 16 to drive an ambulance for the French service in World War I. He came back when he was 18 and went into the army, to OCS. He went back to Cambridge. He was always interested in military affairs. He ended up an engineer—those were the days when Harvard had an engineering school. He went to work for Longlines (A.T.T. Longlines Division). He decided that was not for him, so went to Baltimore to work for Raymond Pearl, who was a famous nutritionist. He met his wife, Agnes, there. He stayed there for a number of years and then decided he should get a union card, that is, go back to Harvard in physiology.

He debated this with Pearl, and it was decided that it was all right as long as he did not work with [W.C.] Crozier. Of course he ended up working with Crozier. Crozier and Pearl were strongly antipathetic. Anyway, it was a shock to the physiologists to see their first graduate student who had never taken a course in biology. But he took his Ph.D. and stayed there as a post doc for a year. George Snedecor picked him up for Ames to teach biomathematics and to consult in the Agricultural Experiment Station. After probably two years at Ames, he came to Princeton. That was at the time I have mentioned, when the Fire Control Research Group was starting. But at that time Ames was a less mathematical place than Iowa City, which [H.L.] Rietz had founded. Iowa City still has a reasonably good group of statisticians.

Tucker: Was there anything at Michigan then?

Tukey: Well, Harry Carver, who founded the *Annals*, Cecil Craig, and Paul Dwyer. Theirs was a school that started before Sam's, less mathematical I would say.

Tucker: It was somehow tied in with actuarial ...

Tukey: They had an actuarial program.

Tucker: I think the department was mainly for students preparing to be actuaries.

Tukey: Gertrude Cox, who had been George Snedecor's right hand woman, together with [W.G.] Cochran and [Harold] Hotelling, ended up in North Carolina. It was Gertrude's operation. At Columbia things were rough until they succeeded in hiring Hotelling. At Columbia statistics was not part of the mathematics department. I don't think it ever has been at Columbia, has it Al?

Tucker: No.

Tukey: Harvard was a place where statisticians started anywhere except in math. There wasn't anybody at Yale in that era. At Berkeley, Jerzy Neyman, who had gotten there a little before the war, might have kept himself acceptable to mathematicians; I don't know.

Tucker: I think the department was large enough by the time Neyman came there, but they were breaking up.

Tukey: Neyman wanted to do mathematical statistics, and I think he had a better chance than most at that time. A better chance than I would have had as a statistician if I had had a Ph.D. in '33 and it had been in topology at Princeton. I would have had a rough life in the math department in those days. Let me make that perfectly clear.

Tucker: A quick note here. You and Harold Hotelling have something in common: You are both Princeton Ph.D.'s in topology.

Aspray: At Princeton, what graduate training was offered in statistics and when did it start?

Tukey: Well, something happened fairly soon after Sam came: he gave a graduate course in statistics.

Tucker: But at the beginning I don't think he was able to give it every year. Because there were not enough courses to go around at that time, and an assistant professor was low on the totem pole. He could give a graduate course only if his seniors had not taken up all the slots. One exception was [Solomon] Bochner and [H.F.] Bohnenblust, because the course in complex variables had to be given.

Tukey: The attraction of that course was not tremendous; as I said, I went to almost everything, but I never went to it. I taught a graduate course in statistics before I ever attended a statistics course at Princeton. In those days most important statisticians arrived by peculiar routes. Frank Yates went to Africa as a surveyor originally.

Tucker: Florence Nightingale was known for other things than being a statistician.

Tukey: If it had not been for R.A. Fisher's limited eyesight who knows what Fisher would have done. Charlie Winsor had a Ph.D. in physiology, Joe Berkson had an M.D. and a Ph.D. in physiology. In those days the abnormal was the normal as far as statisticians were concerned, and Princeton did not get there by what it provided in course work. It got there by getting good people and by giving them good direction, which in many cases meant not bothering them. For the ones who did not need to be bothered there was very little bothering. Mel Peisakoff is the outstanding example. He just came around one day and asked "How would this be for a thesis?" Neither Sam nor I knew just what he had been thinking about, but he had solved some things that Abraham Wald had been raising questions about. Other people were taken by the hand or pushed by the seat of their pants.

During the war, [A.M.] Mood and Cochran were both in Princeton and were living in the house over on Mercer Street, where my high-priced attorney now lives. At that time they were both working in Sam's group, as was R.L. Anderson, who was president of ASA [American Statistical Association] last year. The gang would turn up at the house on Mercer Street Sunday afternoon to do the *New York Times* crossword and possibly to play Go, something of that sort, and to talk about this and that. Winsor would be there, and I would be there. When Mosteller or Ted Anderson were in town, they would quite likely be there too. There was no supervisory connection between these people, but there was a lot of joint work. Sometimes official, sometimes not. There is a report on staircase methods of sensitivity testing which has a number of names on it, including Ted Anderson's and mine, for example. Then there is a later report to Votaw on a similar subject.

I don't know how to give a feeling for the Fuhrocracy, but it was quite a strong group, an intellectually strong group. It had people you might not have expected. Gilbert W. King, who was a theoretical chemist, in the days when theoretical chemists were rare, who used to say "First I does my stochastics and then I does my statistics." Mort Kanner, who went to MIT and had a large hand in the microwave-radar business and died shortly thereafter. Henry Wallman, who was supposed to be a topologist.

Tucker: He was also other things.

Tukey: He was an audiophile among other things. People from Rad Lab [MIT Radiation Laboratory] said there were two people there that you could put a schematic for a new circuit in front of; they would look at it and say, "Didn't you leave out a resistor here?" One was a biologist, and the other was Hank Wallman.

Tucker: Wallman ended up as a professor of electric techniques at the Chalmers Institute in Gothenburg, Sweden, where he is now retired. He wrote a book with Witold Hurewicz on dimension theory.

Tukey: It was intellectually about as strong a group as you are likely to find.

Tucker: Wasn't Stone in this group?

Tukey: Arthur, yes. When did Arthur come? He must have been here by '39. Arthur, Dick Feynman, Bryant Tuckerman—who went to IBM—and I were the people who invented hexaflexagons. This came about because Arthur had an English-size notebook. Woolworth sold only American-size paper. He had to cut strips off the edges. He had to do something with the strips, so he started folding polygons. When he folded the hexagon he had the first hexaflexagon. Later came the Feynman diagram, the Tuckerman traverse, and so on.

Tucker: Was it that group that used the pseudonym "Pondiczery"?

Tukey: Yes, but with a somewhat broader reference.

Aspray: For what purpose?

Tukey: Well, the hope was that at some point Ersatz Stanislaus Pondiczery at the Royal Institute of Poldavia was going to be able to sign something ESP RIP. Then there's the wedding invitation done by the Bourbakis. It was for the marriage of Betty Bourbaki and Pondiczery. It was a formal wedding invitation with a long Latin sentence, most of which was mathematical jokes, three quarters of which you could probably decipher. Pondiczery even wrote a paper under a pseudonym, namely "The Mathematical Theory of Big Game Hunting" by H. Petard, which appeared in the *Monthly*. There were also a few other papers by Pondiczery.

Tucker: Moulton, the editor of the monthly at that time, wrote to me saying that he had this paper and the envelope was postmarked Princeton and he assumed that it was done by some people in math at Princeton. He said he would very much like to publish the paper, but there was a firm policy against publishing anything anonymous. He asked if I, or somebody else that he knew and could depend on, would tell him that the authorship would be revealed if for any reason it became legally necessary. I did not know precisely who they were, but I knew that John [Tukey] was one of them. He seemed to be in the thick of such things. John agreed that I could accept Moulton's terms. I sent a letter with this assurance to Moulton and he went ahead and published it. Which I thought was very flexible on ...

Tukey: Somebody with a high principle. Pondiczery's official residence was in Ong's Hat, New Jersey, which is a wide place in the road going southeast from Pemberton, but it does appear on some road maps. There is a gas station that has a sign out about Ong's Hat.

Aspray: But no sign for Pondiczery?

Tukey: No sign for Pondiczery. Spelled c-z-e-r-y, by the way. Not like the area of India, Pondicherry, which is spelled c-h. Anyway, this was a good group, and it enjoyed its existence. I learned a lot from dinner table conversations. What was the name of our algebraist friend, a quiet soul who was around at that time?

Tucker: You don't mean Ernst Snapper?

Tukey: I don't mean a student. I mean somebody who was at the Institute.

Aspray: Neal McCoy?

Tukey: No, McCoy is tall. This gent was not so tall.

Tucker: MacDuffee?

Tukey: Yes, Cyrus C. MacDuffee. I was interested to see some years ago that the Library of Congress has the notes of Aurel Wintner's course indexed under 'MacDuffee'. The reason his name got on the

notes was that he was the only other person (beyond Boas, Smithies, and Tukey, who wrote the notes) who stayed the course. When the seminar was over, MacDuffee, who had a big car, and the lecturer and the remaining group went off for a day in north Jersey to celebrate by having a meal and driving around. You might have supposed that I went to that course because of a budding interest in statistics, but I went as much because of Ralph and Frank as any other reason. I suppose Al has spoken to you about tea in Fine Hall.

**Aspray:** I would like to hear another person describe it.

**Tukey:** It was a vital element, it seems to me, in pulling things together. Not that you could put your finger on what things. Initially there were mathematicians and physicists; later the physicists had a tea of their own.

**Aspray:** When did that happen?

**Tucker:** I think it was not until the '50s.

**Tukey:** Likely. There was always a lot of a game playing among the mathematicians. Go on the one hand, where Ralph Fox became a rated player, which is not an easy accomplishment. There were a lot of Kriegspiel people. Now Kriegspiel comes in at least two different versions. Kriegspiel played at Brown was not like Kriegspiel played at Fine Hall. At Fine Hall, opportunities to capture a pawn were announced—the existence was announced, but not the location. Then if you wanted to, you could then try, since the pawn capture move would otherwise be illegal. The normal philosophy would be to try all the places you thought it wasn't, so that you would at least know what was there before you made a move. There was a lot of Kriegspiel. Finally, there were a fair number of two-board referees, that is people who refereed perfectly well without middle board, just putting things together. I think the game playing was important. I think it contributed to a professional feeling. And graduate students had a lot of interaction, particularly in the early years, with a lot of people, because all the Institute people, including the junior people, turned up.

**Aspray:** What stories or impressions of the major faculty members do you have?

**Tukey:** Let's see. I think the students thought that Bohnenblust gave the best courses. They might say privately that Bochner was a better analyst, but they thought that Bohnenblust gave very, very good graduate courses. There was a Fine Hall verse for Hermann Weyl, which may have been written by Al for all I know, since he wrote lots of them.

**Tucker:** "Here we have a punning Aryan,/ Who likes to make groups unitarian".

**Tukey:** That is not the one I have in mind. It ends "The great, the noble, the holy Hermann."

Tucker: The last two lines are "He is that most saintly German,/ The one, the great, the holy Hermann." First I gave you the first two lines. C.B. Tompkins wrote it.

Tukey: The one about Lefschetz, about his ...

Tucker: "Here's to Papa Solomon L.,/ Unpredictable as hell,/ When laid at last beneath the sod,/ He'll then begin to heckle God."

Tukey: "Here's to Robertson, Howard Percy,/ On his soul, there will be no mercy."

Tucker: "Round of belly, and deft of toe,/ His forehead's high, but his mind is low."

Tukey: I always thought you could send somebody into a meeting of either mathematicians or physicists with these, and have a finite chance of coming out.

Tucker: Then, of course, there was the undergraduate verse for Eisenhart. "Here's to good old Luther Pfahler,/ In four dimensions he's a whaler./ He built a country club for math,/ Where you can even take a bath." This was the old Fine Hall, where there was a shower. You knew the one about Bohnenblust?

Tukey: Go ahead.

Tucker: "Here's to Bohnenblust, the Swiss,/ Who devils up analysis./ One day he had to lock his door,/ And now its Art for evermore."

Tukey: We have one more for your information, Bill, but off tape. Concerning Eisenhart, one doesn't think of any specific thing in mathematics, but somehow everybody knew he was an important part of the game. I never went to many lectures or seminars. I would occasionally go to tea at the Eisenhart's, where his wife was known as a person with a whim of iron.

Aspray: What role did the people at the Institute play?

Tukey: While the Institute was in Fine Hall there was really very little distinction, except that the University faculty had to see to it that grad students could pass prelims. The Institute people were not under the compulsion to teach, but a lot of them did. I was a timid soul and never tried to become acquainted with Einstein. He came in, saw his assistants, and that was about all, as far as the mathematicians went. Weyl was respected at least as much as anyone else, I believe. The remark of his that I remember the most was that the only things that he was sure of were those that you could prove in intuitionistic mathematics, but he wanted to do mathematics, so he didn't confine himself to these things. This is probably not verbatim, but I think exactly in the spirit.

Tucker: You can find that in the report of the Princeton conference.

Tukey: Yes, in '46 there was the Princeton Bicentennial here, and various things went on, such as meetings. It was the first time that I heard what my voice sounded like. They wanted to make a recording to play on radio stations, and they had Harald Cramer, Henry Whitehead, and somebody else. I got put on because there was supposed to be someone from Princeton.

Tucker: You were the reporter.

Tukey: Yes, I was the reporter for the conference, but that didn't mean that I necessarily got on radio. So that was my first experience being recorded and later hearing it over the air. I don't think you really hear your voice until you hear it together with the voices of people you know. As long as you only hear yourself played back, you don't have the comparisons that make it real.