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Frederick Mosteller and John W. Tukey: A Conversation

Moderated by Francis J. Anscombe

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Anscombe: Good afternoon. My name is Frank Anscombe. I am Professor of Statistics at Yale University. I was formerly a member of the Committee for Filming Distinguished Statisticians of the American Statistical Association. This afternoon we have a discussion or conversation between two very famous figures of the statistical world—John Tukey and Frederick Mosteller. John Tukey is Senior Research Statistician and Donner Professor of Science Emeritus and Professor of Statistics Emeritus at Princeton University. He was also, until his recent retirement, Associate Executive Director of Research at the AT&T Bell Telephone Laboratories. Frederick Mosteller is Chairman of the Harvard Department of Health Policy and Management. He is Roger I. Lee Professor of Mathematical Statistics in the Harvard School of Public Health, and Professor of Mathematical Statistics in the Harvard Department of Statistics and in the Harvard Department of Psychology. He is a member of the faculty of the John F. Kennedy School of Government and of the Medical School of Harvard University.

John Tukey and Frederick Mosteller have known each other for a long time, since, I believe, 1939. They have collaborated in a number of major research projects and also over the years have had numerous informal contacts and discussions. We are now going to hear from them reminiscences and reflections on developments in statistics during their long association. We will hear of their early days at Princeton, of their collaborative studies and of their attitudes on statistics and the current state of statistical science. Let's begin with the early days at Princeton. John, you were there a little before Fred. Would you care to begin?

Tukey: Thank you. Well, I came to Princeton as a chemist in 1937. I took prelims in math at the end of that year and a Ph.D. in topology a year later. I didn't know as much about the state of statistics in the mathematics department then as I learned later. It was a difficult time. If it hadn't been for Luther Pfahler Eisenhart, I don't think there would have been any statistics in Princeton. And I'm sure that Sam Wilks would have done his important work somewhere else. In May of '41 I went over to war work and for most of the war, except when they were away, I spent a lot of time with Charlie and Agnes Winsor, with Charlie both day and night and at meals. I learned a lot from real data but I think I learned even more from Winsor. So I came out of the war a statistician, not a topologist. A change I've never regretted.

Mosteller: I was sent to Princeton as a graduate student by my own mentor, Edwin G. Olds. And I was astonished when I got to Princeton and met Wilks because he looked so much younger than I did. Of course you see me now, but then I looked very young and he looked younger yet. I could hardly believe that he was going to be my teacher. Also, he was a very different man from Edwin Olds. When Edwin Olds wanted you to do something you didn't have any misunderstanding about that job. He just told you. When Wilks wanted you to do something, it was hard to catch it. You had to be alert for it. He would hint around, "It would be nice if somebody did something of a certain kind." You had to grasp that he meant you. Coming from Olds it was a little hard for me to make it out sometimes. Wilks was very busy. He was in Educational Testing Service work, sample survey work, working for the Navy, working for the National Research Council and a member of the Social Science Research Council—a very busy man. And although tea was a sacrosanct institution at Fine Hall when I was there, Wilks almost never appeared except to make an appointment with somebody or to settle something, some kind of business. He almost never

Frederick Mosteller

John W. Tukey

picked up a coffee cup and almost never ate a cookie. There were few statistics students there at that time. Alexander Mood, George Brown, they were both ahead of me. Wilfrid Dixon was in my class. Phil McCarthy, who is now at Cornell, was in my class, and a young man named Ernest Villovaso. The next year Ted Anderson and David Votaw came. The total graduate statistics curriculum consisted of one course which Sam taught throughout the year. We met lots of statisticians because after Wilks built his home famous statisticians were always visiting him at Princeton. All the graduate students met Neyman, Hotelling, A. T. Craig, Cecil Craig, Wald, Koopmans, Deming, Shewhart, Dodge, Romig and so on.

Tukey: I don't have too much to add to this. Before I got involved in other things and started to become statistical, I'll have to say that not only was Wilks not stopping at tea but I very rarely saw him in any other way. He was hard at work, but not visible on the surface.

Mosteller: The war brought lots of statisticians to Princeton. Cochran came. And as you've already mentioned, Winsor, Paul Dwyer, and R. L. Anderson. Tukey, you were working at Fire Control Research and my wife, Virginia, worked there as secretary to Merrill Flood. With the large number of statisticians there in Princeton at that time, a seminar began, an extremely active one. My recollection, John, is that we had many fine speakers but whenever we ran out of speakers you always had something to say.

Tukey: Well, I can't confirm or deny that. And maybe I should clarify the term Fire Control Research; many people in Princeton thought we controlled fires. But really what we were working on in the beginning was stereoscopic height and range finders with an experimental set-up down at Fortress Monroe. Then we moved on into a variety of less obviously related topics including things like armored vehicle fire control and ballistic behavior of rocket powder. And then eventually we finished off the war as the coordinating group for a thing called AC92, which was trying to find out how to make the B29 bomber more useful. But I think the important thing for the present was that there were a number of statisticians there. Particularly toward the end of the war, after Cochran and Mood had set up their two families in a large house (that is now occupied by my lawyer). On Sunday afternoons the statisticians would gather over there. Some people would do *The New York Times* crossword, some people would do this and that. I first met

George Snedecor in the garden of that house, for example, and other people of interest.

Mosteller: There was a fair bit of teaching of quality control at this time. Holbrook Working and E. G. Olds and Paul Clifford came and helped develop a quality control program that was going throughout the nation. And Sam, of course, cooperated to set up such an enterprise at Princeton. I taught in the program at Princeton and later in Newark and in Philadelphia.[1] I think that experience was one of the reasons I was later involved in NBC's Continental Classroom. Sam edited *The Annals of Mathematical Statistics* and I was his assistant. It was amazing how he got the *Annals* out. On Sunday evening when we would finish pouring some visiting fireman onto the train from Princeton, usually to New York or to Washington, he would turn and say, "I wonder if we shouldn't spend just a little time getting out the *Annals* tonight." And that was always the beginning of a long, hard session because Sam enjoyed the all-night effort. And about five or six in the morning we'd pull out of there and go down to the post office and mail off another issue of the *Annals*. He also got me a job working with Hadley Cantril. Hadley was a social psychologist who pioneered in using survey research for social science. And as a result of that effort, I ultimately met Sam Stouffer and worked as a sampling consultant to the War Department in Washington. Finally, that led to my coming to Harvard University in the Department of Social Relations after I completed my degree at Princeton.

But the war work that I did was partly done in New York with John Williams and Cecil Hastings and Jimmie Savage. We had a very small group; we called it the Princeton Statistical Research Group, Junior because the main group was in Princeton. There was also a Columbia group in the same building. Indeed my wife, Virginia, went to the Columbia group and served as a secretary for Allen Wallis, who headed that group. They had an enormous collection of statisticians including Harold Hotelling, Abe Girshick, Churchill Eisenhart, Albert Bowker, Jimmie Savage, Wald, Wolfowitz and so on. This was a very important move for me, partly because I got to work with Jimmie Savage and partly because I learned to work with problems—real problems—quickly. Problems came in and they always had a deadline on them, and I learned that you had to do what you could, and then write it up and send it off because it wasn't going to be of any value later. That was a marvelous experience for me. Until then most of my statistical research wasn't quite finished or quite good enough to display. After that I learned to bundle it up and send it off.

Tukey: What Fred didn't mention, because it never struck him as much as it struck other people, is that just after that experience if you heard somebody speaking in the next room at a party, you couldn't tell whether it was Jimmie Savage or Fred Mosteller. They had worked together so much that their accents and intonation and everything were really very close. He also didn't mention Milton Friedman, and it would be a mistake for me to let that omission get by. If I were going to give a paper on statistics anywhere in the ten years after 1945, the one person whose presence in the audience would make me most careful would have been Milton Friedman. Not anybody who claimed to be a statistician, but somebody who knew an awful lot of statistics and was very sharp to boot.

I think in Princeton we didn't have the same sort of short fuse experience, but we certainly had the real problems experience and the real data experience and different kinds of experimental design than they teach you in courses. For example, Eastman Kodak was building an experimental height finder and when it was time for us to close down our heightfinder work, I sat down and dictated 101 dictaphone cylinders. That was the first draft of the report on the M1E9 heightfinder. So Fred got one thing which he needed, I must have got another good thing.

Mosteller: Well, Milton Friedman gave both Jimmie Savage and me a very, very important lesson. He took a one hundred page manuscript that we had labored over long and carefully and wrote all over it on both sides of the pages. This was at a time when it was hard to get extra copies of anything because they had to be made from scratch. So we were very indignant that he would mess up a manuscript this badly. So we took his 1,000 corrections and explained to him the 22 that we thought were mistaken. He then fought us into the ground. He wouldn't give on even one. Finally, at the end, he said of two of them, "Well, those may be matters of taste." But what was important was that when he got finished he said, "Well, you fellows really have a lot of good things to say, and you ought to learn how to say them. There are some ways to learn how to write. Here are a couple of books that would do you a lot of good." He sent us off with these books, and Jimmie and I for many months worked very hard on this problem. It made a big difference, I think, to both of us. It was the first time I think anyone ever took the editing of any writing that I had done or that Savage had done seriously enough to give us some motivation for learning how to improve our writing. It was a very important occasion.

I learned something else at this time, John, and that was that everything didn't have to be done by direct

[1] These Princeton Quality Control Courses (short courses we would call them now) were so well liked that participants asked to convert them into an annual event that still continues under the auspices of the Metropolitan Section of the American Society for Quality Control.

mathematics. It was possible to do things by example, by approximation and by simulation. I had done some simulation with Wilks, and with Olds before I had left Carnegie Tech, but simulation seemed to be very often needed in the wartime problems that we had. And indeed, many of them probably couldn't be done even today by any mechanism other than simulation although it was fairly slow. We didn't have a lot of heavy calculators available.

One of the wartime problems that I had that was rather interesting was this: We had special bombs that could be aimed. Two were called Azon (azimuth only) and Razon (range and azimuth only), and there were also heat-homing bombs. These bombs could be aimed while they were in flight so that they would be likely to hit a road or even a point target. The trouble with them was that they seemed to have a very large standard deviation around their aiming point. And a question before the house was: How was it possible that special bombs could be useful when they had a larger aiming error than standard bombs? So I was called to go to Washington to explain how that could be. The answer was essentially this: The radios that controlled the guidance systems in these bombs would break in a substantial proportion of them. And when they broke, they tended to lock their fins in extreme positions with the result that the bombs would just sail off, far, far from the aiming point. On the other hand, when the radio didn't break then it was possible to aim them very precisely. With Azon you could get a substantial proportion of hits on a long rather narrow target, say thirty or forty feet wide from an altitude of 15,000 feet, even though the overall standard error would be of the order of 2,000 feet. So because John Williams was away I was required to go to the National Academy of Sciences in Washington to explain this to a very distinguished panel of scientists. It was the first important experience I had testifying about a statistical problem.

John and I didn't get to know each other until he came into statistics, though we saw each other at meals at the graduate school and at Fine Hall. But we did work together some in the war on a number of little problems, one of which had to do with the low moments of small samples. I had written a paper that gave the means and the variances of the order statistics of small samples but didn't give the covariances. When I showed it to Wilks, he felt that if we didn't give the covariances the paper wasn't satisfactory. It was very difficult to get the covariances. Getting accurate covariances for order statistics turned out to be a problem that defeated calculating machines for quite a few years even after they became fairly good. So getting those covariances wasn't something that was going to come easily. Cecil Hastings, who worked for this Princeton group in New York with me, was very good at this kind of calculation so he worked hard on getting the covariances. Meanwhile, John Tukey and Charlie Winsor were with me one of those afternoons, perhaps a Sunday afternoon, I'm not quite sure, and I explained to them that it would be nice if we had features of these order statistics, not just for the normal distribution, but for some other distributions. I had them, of course, for the uniform distribution but wanted them for other distributions. And John and Charlie agreed to get the corresponding things for some other distributions. John, how did you do that?

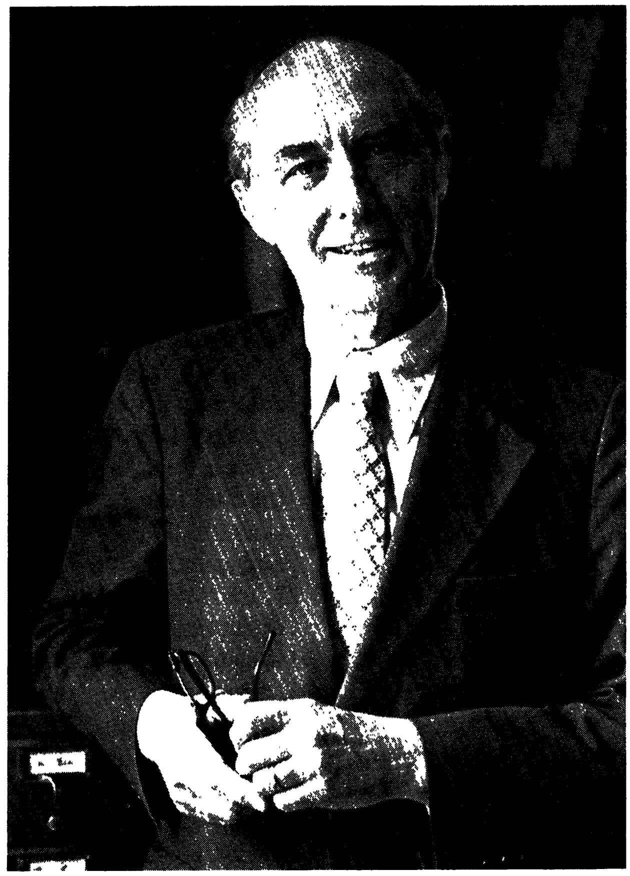
Tukey: We went for a distribution where the chances of doing it were good.

Mosteller: Cheating?

Tukey: No, no, not cheating. The main point is that we don't have to go as far as simulations sometimes to be useful. If you look for a case in which the representing function, the inverse of the cumulative, has a nice algebraic behavior, you can end up doing things of this sort in reasonably closed form. And so we did some of this. So when the paper (Hastings, Mosteller, Tukey and Winsor, 1947) finally came out, it had some uniform information, some Gaussian information, some asymptotic Gaussian information and some information for the special distribution. All this did a fair amount to illuminate how these things varied as you went to distributions with a longer tail. The special distribution was only a little bit longer-tailed than the logistic. We weren't very brave in those days.

Fred mentioned a couple of things that he got into as a result of wartime problems. Maybe I should mention a couple. I got into a robustness problem because when we were working on the B29 we were supposed to be looking at the precision of machine gun fire from the aircraft. Well, everybody knew then as everybody knows now, regrettably, that if you have samples from a Gaussian distribution you should calculate the standard deviation in order to judge how broad it is. It turned out we weren't getting perfect Gaussian distributions.

It turns out that if you add 0.1% of a Gaussian three times as spread out as the basic one, then already the mean deviation has passed the standard deviation as a way of measuring the scale. How you detect whether someone's been by with a hypodermic needle and put in one part in a thousand of a slightly broader distribution is a very, very difficult point. Indeed, by the time you put in 10% you can be pretty thoroughly ruined. And I think that this was essentially what got me started thinking about robustness—a topic to which I have come back more than once. I worked for Fire Control Research through roughly the end of 1944, then that was sort of winding up. I ended up going to Bell Laboratories and I worked there full time for a while. Then in September 1945 things were



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turning down a little so I came back to Princeton half-time and worked on Sam Wilks' project. Not at Fire Control Research where I'd been, but I saw much more of Fred.

While I was at Murray Hill, we had an engineer named Budenbom who had been building a new, especially good tracking radar for tracking aerial targets. He wanted to go to California to give a paper and he wanted a picture to show what his tracking errors were like. So it was a question of calculating the spectrum which was done by the best conventional methods (by ladies using desk calculators). Dick Hamming looked at this and said, "Well, if you only smooth it (¼, ½, ¼) it will look much better." So the first thing that we did was to smooth it (¼, ½, ¼) and sent Bud off with the picture. And the other thing was that Dick and I spent some months trying to understand why it was a good idea to smooth it (¼, ½, ¼). And that's where my spectrum analysis education really got started.

Mosteller: When I came back from New York with Virginia, she again worked for Merrill Flood who then had his own research company. I buckled down then, at last, to finish writing my thesis. It hadn't been going too well when I left for New York but when I came back it was going fairly well. The only difficulty was I could rarely see Sam Wilks for the reasons that John has explained earlier. And therefore, John became essentially the advisor on the thesis. I went around to John from time to time and asked him for some suggestions. He always did two things: Took a pass at the problem I asked him about and then he'd always suggest something else, something entirely different to work on. And I gradually got an important idea out of that experience which was that it's important to get out of ruts and into some new activity that may turn out to be more beneficial than the ruts you are already in and can't handle. So I owe John a great deal for that. The thesis got finished finally (Mosteller, 1946) and though the thesis was one that Wilks had originally suggested, John did a great deal of the final advising on the topic. At the end of that time, I was going to go to Harvard but we were both invited to Lake Junaluska by Gertrude Cox for a conference following a summer teaching program that they had given down there. And John offered to drive me there in his famous stationwagon. So we drove together.

Anscombe: Would you explain why this stationwagon was famous?

Mosteller: I suppose John should explain why it was famous.

Tukey: It wasn't famous to me, so it has to be Fred.

Mosteller: It was the oldest living stationwagon: perhaps the most disreputable looking stationwagon at Princeton.

Tukey: It was a 1936 wooden stationwagon and this was only 1946.

Mosteller: At any rate, it was a stationwagon, and he was going to drive us to Lake Junaluska in North Carolina for the meeting. We hadn't been on the road half an hour before he pointed out that he had ideas for a paper that we would write on the way to North Carolina and back. And so we did work away on it. He had an idea for developing something called binomial probability paper which was very good for plotting certain kinds of binomial information. And we wrote the paper (Mosteller and Tukey, 1949), not finished exactly during the trip, but we did a lot of work on it on the trip and shortly thereafter.

Junaluska had a lot of exciting people. Wolfowitz was there, Fred Stephan, Phil Rulon, David Duncan. I think Charlie Winsor was there, Bill Cochran, R. A. Fisher, Gertrude Cox, of course, and many others. Fisher was in very good humor. The first day everyone was asked to say what they would like to hear somebody talk about. And then after that list was put on the board, people were asked to volunteer to give talks on the topics. The thing that interested me very much was that one of the topics requested was Bayesianism and R. A. Fisher volunteered to give that talk. Indeed, I might say it was one of the best talks I ever heard him give in my life. He was always with us in the evening, drinking beer, especially with the younger people. He had a good time at that meeting, and we all had a good time too, both socially and intellectually. I gave a talk about pooling data which was sort of a Bayesian talk (Mosteller, 1948). The idea is that you have means from two different sources and the question is: Can you estimate one of those means better by using information from both these sources instead of only one? Fisher talked to me quite a bit about this idea. He never exactly told me he didn't like it but he didn't ever tell me he did like it either. He cross-questioned me very carefully in private for about three-quarters of an hour about it, and at the end we parted, and as far as I could tell, we were still very good friends. So apparently on some occasions, at least, Fisher was very interested in Bayesianism and comfortable with it. Do you remember anything about that, John?

Tukey: Well, I would suspect that you weren't trying to sell it as a matter of high principle. As a practical device I think he could think about it. Well, there were various things that Fred has not mentioned like the large organized game of hearts that took place up in the third floor dormitory. It was part of that meeting.

Mosteller: Well designed too.[2]

Tukey: Yes. I think we ought to mention for the record the problems of keeping Fisher supplied with beer when we were meeting at a Methodist camp meeting. Fisher treated me very gently. I gave a talk about analysis of covariance from a somewhat non-standard point of view. I thought he was really quite gentle in pointing out to me how much of it really became the standard one if you just twisted it slightly. I might have expected to be much more roughly handled.

Mosteller: After we finished that paper on graphing binomial counts we got involved in some others including a set of papers on industrial quality control, on "quick and dirty" methods (Mosteller and Tukey, 1949–1950). But I think the next joint event probably was the work on the Kinsey Report. Sam Wilks was President of the American Statistical Association and he was requested, I believe by the National Research Council, to appoint a committee to review the statistical methods of the Kinsey Report on sexual behavior in the human male. He asked Cochran to chair the committee and he asked John and me to join on that committee, which we did. It was a substantial effort and we did actually produce a book (Cochran, Mosteller and Tukey, 1954b), and from that book some articles (Cochran, Mosteller and Tukey, 1953 and 1954a) were produced. I think John wrote some very original material on sample surveys in that book, and we got some new thoughts about how social science and behavioral science were being carried out at that time. Dr. Kinsey was a self-reliant scientist and liked to do everything himself. Consequently, when he studied issues like variability he did it essentially by simulation. He was not aware of the substantial statistical work on variability. So some of his work was criticized because he did not use the published literature.

Kinsey developed a special method of interviewing that allowed the interview to flow over its many topics in whatever order matters emerged from the respondent. Interviewers needed extensive training to handle this approach. He also had special coding methods to preserve the confidentiality of his respondents. A major problem for his studies was that his respondents were volunteers, although he had some good ideas for getting around this.

[2] Tukey was having fun with the design of experiments just then and he set up the investigation so that the players sat in all possible arrangements and took account of the changing position of the dealer. My recollection is that there were some differences among players but not in effect of position of dealer. The players included David Duncan, Fred Stephan, Charlie Winsor, J. W. T. and F. M.

He tried to gather all material about subjects he studied, and we saw an enormous collection of books and articles in his library on gall wasps, an insect he studied extensively. He also had an enormous library on sex.

Tukey: Fred may or may not remember the time when the three of us were going to the train at Princeton to let Fred go back to Harvard and Wullie (Cochran) go back to Johns Hopkins. I think I can still quote Fred very accurately saying, "They couldn't pay me to do this. They couldn't pay me to do this." It was really a labor of love!

Mosteller: The visits to Bloomington, Indiana, required a lot of work and we had many long conferences there. We also had to go to Baltimore and work with Cochran, and Betty acted as hostess. We had to read a great number of critiques of the book. We took each critique and cut it up into little pieces and tried to write a response to each of those critiques. It was a really massive effort but we did finally get it all done. We published both the original statement and our comment in our book.[3]

Tukey: The implication is, if you can do that you can do anything?

Mosteller: [Laughs] I don't know. We later had an opportunity to work together on the National Halothane Study (Bunker, Forrest, Mosteller and Vandam, 1969). Halothane is an anesthetic and the question was whether halothane could be killing people from massive liver necrosis because it had four halogens in its chemical composition and chemicals with halogens often cause liver damage. We got to working on this enterprise with John Bunker, Lincoln Moses, Bill Brown, John Gilbert, Yvonne Bishop, Morven Gentleman and a host of physicians for the Committee on Anesthesia of the National Research Council. We met almost anywhere in the country, wherever John was. John, I think was on sabbatical that year. At any rate, he was traveling a good deal, and the statistical team would just pick up and go and visit John—Phoenix, or New Orleans, or wherever. We attacked the halothane study with many different methods. There were three main methods, two being statistical and one being a study of the actual tissues from deaths in surgery.

Tukey: Yes, I think there are still things for many statisticians to learn by going to see if they can find a copy of the report on the National Halothane Study and reading the statistical aspects in it. Although, if I don't say it Fred undoubtedly will, about its having led to the Bishop-Fienberg-Holland book on contingency tables (Bishop, Fienberg and Holland, 1975). Not all the techniques that were piloted in the study got taken to the book stage.

Mosteller: Right now there is renewed interest in this area because Congress, and also the executive branch of the government, especially the Health Care Finance Administration, want to compare hospitals in the quality of their care. This raises the problem of adjusting so as to have some fairness in the comparisons between hospitals that take severe cases and hospitals that take less severe cases. It's not clear to me now how that should be done even though I was very comfortable with what we did in the halothane study. There seem to be more political questions involved in the new effort than there were in the halothane study. Maybe I've become more sensitive, but I think not. I think that's a realistic part of the new study.

Tukey: Maybe you were technically happier than I was. I wasn't unhappy in the sense that I knew anything better to do. But if you start rating severity of cases on a five- or seven-point verbal scale, it's hard to believe that a verbally described severity in a hospital that only sees light cases is the same as that same verbally described severity in one that sees much worse ones. And it's very hard to understand how to change things so that the word "moribund" will have the same interpretation everywhere. And without that, you have some real problems. Certainly the adjustments that we used were good things. I think we know enough now about making adjustments for broad categories that we might want to take those things somewhat further, if we had all that effort to do again and all that data to work with. "All that data" being information on about 800,000 cases. That's a lot of data.

But carrying on the expositional tradition, there is always the green book (Mosteller and Tukey, 1977). And in that there is material on adjusting for broad categories (pages 240–257). The thing that people forget when they say, "Well, I'm going to cure the fact that there is this background variable. I'll dichotomize it, and then I'll look at the two halves of the dichotomy and sort of take what effect there seems to be in each half and pool them together." Forgetting that if the ratios of the fractions are different, if you sort of cut a distribution across under a knife, the centers of gravity are not going to be in the same place when the distribution is over here and cut there, as when it's over there and cut here. Dichotomizing helps, but it's not the whole answer.

Being sure you make this correction—or any correction—accurately is not likely in human affairs, but

[3] When you have lots of little slips of paper of irregular size, it is sometimes hard to keep them straight. I recall that one slip of paper was edited by us and entered into the text of our article as our own work. After its publication, W. Allen Wallis pointed out to me that the paragraph was from his own review of the Kinsey Report and that in his opinion it was better said before we had edited it. Let me take this opportunity to acknowledge that accidential bit of plagiarism without bothering to hunt up the offending paragraph.

you are a lot better off to make the correction than not. Correction for broad categories is one of the things that really did get into the green book, and I think it's right to direct people's attention to it, because it's a pretty widespread problem that people often sweep under the rug, saying: "Well, we'll divide it. We'll at least separate the people with low blood pressure from the people with not-low blood pressure and then not worry about the details anymore." We learned something about broad categories in the halothane study because the data was all collected on the 800,000 cases before one could get a hard look at it, and the ages had been coded in ten-year blocks. It turned out that the risk of death from surgery about doubled every ten years, so the distinction between sixty-one and sixty-nine, or seventy-two and seventy-nine, was a really important distinction. If we'd only had better age data, we would have been able to squeeze things a little more. And if we knew more about broad categories, we could have done a little better. I don't think it would have affected the overall conclusions,[4] but it would have been nice to come nearer to getting out of the data what was in there waiting for us.

Anscombe: Could I just ask, would you explain the phrase "the green book"?

Mosteller: Well, Gardner Lindzey had asked John and me to write a statistical chapter (Mosteller and Tukey, 1968) for the second edition of the *Handbook of Social Psychology.* John and I worked on that very hard and we wrote much too much. Whereupon Lindzey took a share of it and we were left then with a considerable extra bundle. So we decided to put the bundle together and create a book around it called *Data Analysis and Regression.* And it does include some information about robust methods as well as more classical kinds of techniques. It especially has a substantial discussion of regression and the difficulties and hazards associated with multiple regression. Essentially it says a lot about how little you can do with regression as well as how much. That's an important feature of the book.

Anscombe: Would you care to say something about what I think you call the "Cambridge writing machine" as a more recent work in collaboration?

Tukey: That's what I call it. I'll talk about it a little, and then let Fred talk about it. He wouldn't call it that. But there has to be some name for the collaborative volumes edited by Hoaglin, Mosteller and Tukey, of which two are out and more are in the mill. Somehow this has worked out very well. Fred and Dave have been very effective at persuading people around Cambridge, or who have been around Cambridge—and it's surprising how many of them were around Princeton before they were around Cambridge—to write chapters for these books. These are the things that are known in the trade as *UREDA* and *EDITS* at the moment: *Understanding Robustness and Exploratory Data Analysis* (Hoaglin, Mosteller and Tukey, 1983) and *Exploring Data Tables, Trends, and Shapes* (Hoaglin, Mosteller and Tukey, 1985). I'm not sure why this has been as good a solution as it has. The three editors have obviously been complementary and have beaten on the authors about very different things. The beating has been heavy enough so even though the chapters are separately authored, we've had compliments on coherence from reviewers, much to my surprise. Fred, what do you want to say about this in addition?

Mosteller: I think it's been helped enormously by some people behind the scenes, as well as by the collegiality of people who come and visit. First, the people behind the scenes: We've had marvelous secretarial support and two very helpful research assistants. One assistant was Anita Parunak, for part of this period, and the other, for a much longer period, is Mrs. Cleo Youtz. These people both have an important quality that this kind of work needs, and that is an eagle eye and an unwillingness to pass by something that they regard as possibly mistaken. They just keep after it until finally they force the authors to get it right. I wouldn't say we don't have any errors in the books, that would be silly, but the number of errors is fewer by, I would think, about two orders of magnitude than it would be if it were not for the help of these people. And they also redo all the examples and comb through the work. That explains one of the positive aspects of the work. Marjorie Olson manages the secretarial side of the productions with skill, imagination and organization. The second feature is that the number of people who are willing to participate in these enterprises is astonishing. I think they get some fun out of it and then when they leave Cambridge and go home to their own university, they usually have one or two more strings added to their bow which they may use with other students later or in their own work. Third, some of our own graduate students participate and this leads to training and publication for them. Fourth, sometimes Harvard faculty join in. We have had a sustained relation with John Emerson of Middlebury College, as well as Boris Iglewicz at Temple. So it's been a very productive enterprise and profitable, I think, for all participants.

[4] Based on mortality and on death from massive liver necrosis, the study found the anesthetic halothane to be as safe as nitrous oxide with pentothal and safer than cyclopropane; the distribution of the use of ether across hospitals made it impossible to get a reliable comparison between halothane and ether. It was also concluded that even after adjustment for several variables some difference in mortality of hospitals remained, and this ultimately led to a later study of institutional differences at Stanford.

Tukey: Well, maybe we should turn to a closing topic. I think there are a few lessons that I would believe in very thoroughly. I think Fred would too. The first is that real problems deserve realistic attention. Which implies it's better to have an approximate solution to the right problem than to have an exact solution to the wrong one. Second, that one should intend to learn from real problems, that they can be extremely suggestive over the long pull about both theory and techniques. Third, that the use of techniques is not confined to the instances that are covered by theory. If you had to have theory to cover every application, very few techniques would ever get used. And I think the corollary to this, or better, to the thing to which this is a corollary, is that statistics needs to be broad, not narrow.

Anscombe: Do you care to add any comments on that kind of theme?

Mosteller: No, I think that pretty well covers it.

Anscombe: Well, then I think we are about at the end of the time that is allotted for us. And I thank you gentlemen very much indeed for your marvelous series of thoughts and reminiscences, and historical insights. Thank you.

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