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A Conversation with John W. Tukey and Elizabeth Tukey

Luisa T. Fernholz and Stephan Morgenthaler

Abstract. John Wilder Tukey, Donner Professor of Science Emeritus at Princeton University, was born in New Bedford, Massachusetts, on June 16, 1915. After earning bachelor's and master's degrees in chemistry at Brown University in 1936 and 1937, respectively, he started his career at Princeton University with a Ph.D. in mathematics in 1939 followed by an immediate appointment as Henry B. Fine Instructor in Mathematics. A decade later, at age 35, he was advanced to a full professorship. He directed the Statistical Research Group at Princeton University from its founding in 1956; when the Department of Statistics was formed in 1965, he was named its first chairman and held that post until 1970. He was appointed to the Donner Chair in 1976 and remained at Princeton until reaching emeritus status in 1985. At the same time, he was a Member of Technical Staff at AT&T Bell Laboratories since 1945, advancing to Assistant Director of Research, Communications Principles, in 1958 and, in 1961, to Associate Executive Director, Research Information Sciences, a position he held until retirement in 1985.

Throughout World War II he participated in projects assigned to the Princeton Branch of the Frankford Arsenal Fire Control Design Division. This wartime service marked the beginning of his close and continuing association with governmental committees and agencies. Among other activities he was a member of the U.S. Delegation to the Conference on the Discontinuance of Nuclear Weapons Tests in Geneva in 1959, served on the President's Science Advisory Committee from 1960 to 1964 and was a member of President Johnson's Task Force on Environmental Pollution and President Nixon's Task Force on Air Pollution. The long list of awards and honors that Tukey has received includes the S. S. Wilks Medal from the American Statistical Association (ASA) (1965), the National Medal of Science (1973), the Medal of Honor from the IEEE (1982), the Deming Medal from the American Society of Quality Control (1983) and the Educational Testing Service Award (1990). He holds honorary degrees from Case Institute of Technology, the University of Chicago and Brown, Temple, Yale and Waterloo Universities; in June 1998, he was awarded an honorary degree from Princeton University. He has led the way to the fields of exploratory data analysis (EDA) and robust estimation. His contributions to the spectral analysis of time series and other

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aspects of digital signal processes have been widely used in engineering and science. His collaboration with a fellow mathematician resulted in the discovery of the fast Fourier transform (FFT) algorithm. Author of *Exploratory Data Analysis* and eight volumes of collected papers, he has contributed to a wide variety of areas and has coauthored several books. He has guided more than 50 graduate students to successful Ph.D.'s and inspired their careers. A detailed list of his students as well as a complete curriculum vitae can be found in *The Practice of Data Analysis* (1997), edited by D. Brillinger, L. Fernholz, and S. Morgenthaler, Princeton University Press.

John W. Tukey married Elizabeth Louise Rapp in 1950. Before their marriage, she was Personnel Director of the Educational Testing Service in Princeton, New Jersey.

On June 25, 1997, Luisa Fernholz and Stephan Morgenthaler talked with John and Elizabeth Tukey at their home in Princeton, New Jersey. The conversation ranged over various aspects of John's remarkable career and unique personality. A separate interview has been published in *The Practice of Data Analysis* (Brillinger, Fernholz and Morgenthaler, 1997). It was recorded on June 20, 1995, at the two-day symposium held at Princeton University to celebrate John's 80th birthday. Also shown at this symposium was a videotape produced by BellCore and the American Statistical Association in 1993, in which John and Elizabeth Tukey, in conversation with Ram Gnanadesikan and David Hoaglin, discussed a number of topics ranging from statistics to more general issues, including many personal insights. The present interview is intended to complement the two previous ones.

Elizabeth Tukey has been a driving force in John's life and her comments and anecdotes add a personal touch, complementing his statements. She had read and agreed to the publication of the present conversation. Unfortunately, Elizabeth passed away on January 6, 1998. This article is also a tribute to her memory.

In the following conversation, the questions, denoted by **Q**, were asked by Luisa T. Fernholz and Stephan Morgenthaler. Answers by John W. Tukey are denoted by **J** and answers by Elizabeth Tukey are denoted by **E**.

STATISTICS

Q: Let's talk about your view of statistics as opposed to the prevailing view when you were young. My impression is that the mainstream view was really this Fisherian one, where you had a probability model with parameters that you estimated and tested and so on. And you came along and proposed things that were looking much more closely at the data and letting the data guide what you do.

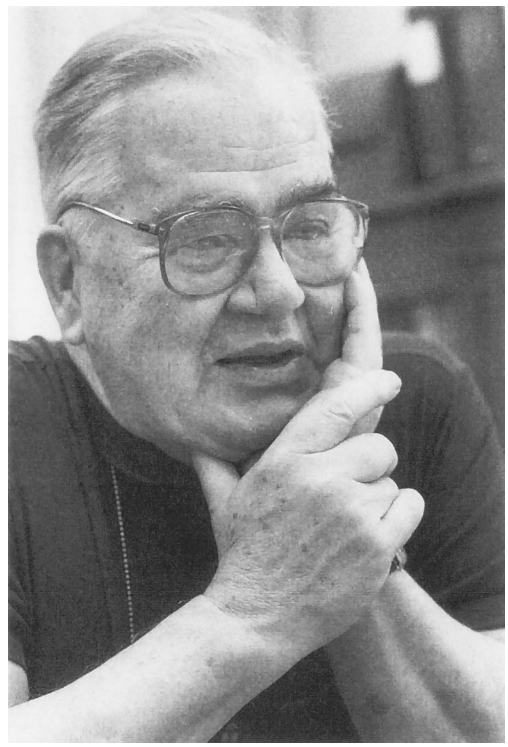

FIG. 1. *John Tukey, date and place unknown.*

J: I'm not sure that that's what happened early on. My first quasistatistical paper is probabilistic. It's the one about the fractional part of a statistical variable. I had read a fair amount of statistics because I read a fair amount of many of the things that were in the math library at Brown. I read them rather than studied them. Let me get a bibliography [gets a bibliography from the bookcase]. The first statistical paper is Scheffé and Tukey (1944) which is a very short note on sample sizes for population tolerance limits. Now, at that point I had my educational experience working on war problems, a large part of the time in double harness with Charlie Winsor. So, it was natural to regard statistics as

FIG. 2. *John and Elizabeth Tukey on the day of their wedding.*

FIG. 3. *John W. Tukey receiving the National Medal of Science from President Nixon in 1973.*

something that had the purpose of being used on data—maybe not directly, but at most at some remove. Now, I can't believe that other people who had had practical experience failed to have this view, but they certainly—I would say—failed to advertise it. I guess we are to take as our initial period the last part of the 1940s, from 1944 on. I don't really know how people thought generally. I know how Charlie Winsor thought; it was easy to discover that. (I also had some understanding of how Sam Wilks thought, which was quite different.) Charlie had a very brief engineering background and a much longer background working with Raymond Pearl in what might now be called biometrics—biostatistics, but not as highly formalized. So, for Charlie dealing with data was the natural thing.

Q: Without thinking of population parameters at all?

J: No. No, no no! I'm trying to cast my mind back. No, because the counterexample in a sense is the 1947 paper by Hastings, Mosteller, Tukey and Winsor "Low moments for small samples: a comparative study of order statistics," which was a low-power version [of computations for inference purposes], but not confined to the Gaussian. We also had the rectangular and one reasonably stretched-tail distribution. Now, you don't get involved in that if you're abhorrent of population parameters. And Charlie was involved in the working of that. That's not just a decorative appearance on the list of authors.

Q: Would you say that you read most of the literature that was published? As it came out, you read it?

J: I don't know. What was maybe more important was that I read *Series B*, then called the *Supplement to the Journal of the Royal Statistical Society*—read, again, rather than studied—from volume 1 on. And I read through *Biometrika*, so that I had a reasonably good feel for what people were doing or had been doing—for 40 years in the case of *Biometrika*.

Q: Interestingly enough, those are two British publications. So, are you rather a statistician in the British sense of the word? Was it the Americans who brought in the more theoretical stuff?

J: No, not necessarily. John Wishart, for example, was all mathematical as opposed to data-oriented. I suspect I never worried as much as some people would have thought I should about "what people were doing."

Q: Talking about these more data-oriented approaches, what surprises me is why nonparametrics, which I think also came out around that time, did not have a bigger impact than it had. That people didn't say: "that's the thing we have to do."

J: Well, it came about—I don't have the history clearly in mind—but some of the things of that sort go back probably pre-World War I. Mainly isolated things in the social science areas. But there are two requirements that are important, with varying in-

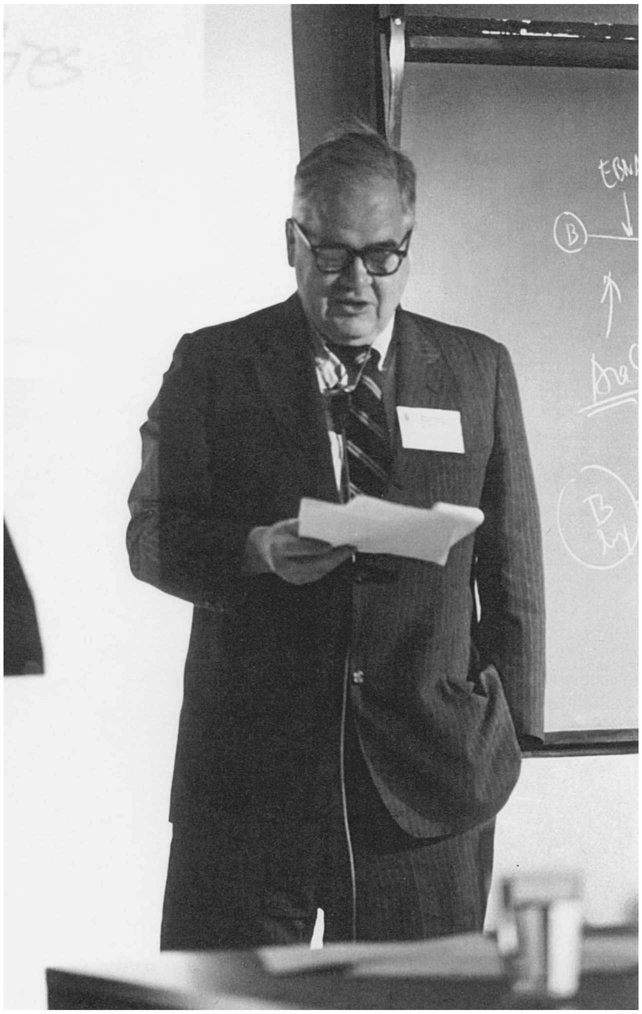

FIG. 4. *John Tukey, date and place unknown.*

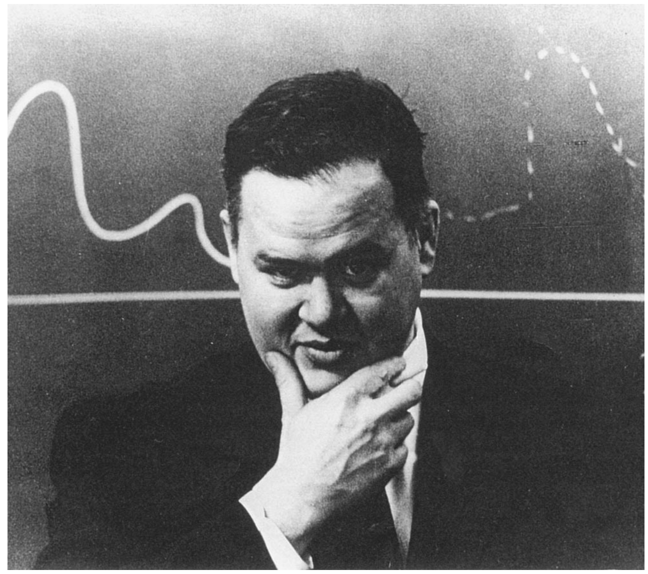

FIG. 5. *John W. Tukey, Princeton University (early years).*

tensity in varying times and places, about something that's going to be an active field. One is: it has to look mathematical enough so that you are protected from criticism from your mathematical colleagues. And, second, there have to be enough thesis problems around to keep the trade running. Now, as far as I'm concerned, I would want to add a third to that and say that it ought to have a useful impact on the analysis of data in due course. I guess there is a corollary to the first two that says: it's a strong plus if it appears as a coherent body of thought, with common principles and so on. There is a paper of Fisher's that I can't cite accurately offhand (Fisher, 1929) in which he essentially says that "it is obviously impossible for there to be a set of statistical inference techniques for different assumptions as to what the populations are like, one for each alternative." Now, 50 years from when he said this, this might still be right. But I think we now recognize that it doesn't have to be right from now on. Nonparametrics was good to protect the flanks from attacks by people who wanted to go in other directions—I mean in terms of a particular application. If you had a conventional least squares Gaussian–normal theory sort of thing, then an obvious attack is to say every body of data really isn't Gaussian. And if one could show that the results were also significant by nonparametrics, that blunted that sort of attack very considerably. Nonparametrics didn't lend itself much toward a subtle and ramified analysis of things. If you have a situation where the median makes good sense, then it's fine to have good properties of things that are median-based. But if you need to complicate the analysis a little, it may not be nearly as clear where to go, as by doing some classical regression sort of thing. Not that I'm arguing that classical regression is ideal and wonderful, but it's often a natural way to try to go deeper. That's one thing I think that held nonparametrics back. I think another thing is the fact that you ended up trying to prove things for all possible kinds of inputs. But, you knew enough about the world to know that all possible inputs weren't really needed. Now, this I don't think bothered people explicitly, but I think it had to come into the feelings that you had about things.

Q: You could do better by fixing yourself a framework.

J: You ought to be able to do better. Maybe you didn't know how. We were prerobustness at this point. You could tell in those days about a book in numerical analysis whether the author had done numerical analysis or not. It was the question of which of the simple quadrature formulas he mentioned, because some of them work much better than others. This isn't necessarily a theorem, but it's sort of well known in the trade. And there was a corresponding low-grade seal for statistics books which emphasized the variance of the arithmetic mean as compared to the sampling distribution of S^2. One of these works and the other doesn't. And while it was really rarely said that the other didn't, not mentioning it was a sign that things were being taken seriously. I don't know when it was, it may not be quite this far back. But there was some paper being discussed—probably at an Institute of Mathematical Statistics meeting—and Harold Hotelling and I were involved and I was pointing out to Harold that whether people in practice turned out to use a statistical technique or not would offer good evidence as to whether one really wanted to use it. And he got up and said he had never thought of such a thing. I think to the extent that you like to identify anything, you have to identify Charlie Winsor. He was data-oriented. I well remember walking up past by old Fine Hall and hearing Charlie say: "Well, Sam Wilks trains good mathematical statisticians, and it's surprising how soon they become good statisticians." But, associating with Charlie and living in the data-rich environment where what we were doing was trying to make sense out of the data left me with an ultimate data-orientation.

Q: If one looks at your biography, one identifies other reasons. You had a nonstandard education for a statistician.

J: Well, in those days most people did. Frank Yates was a surveyor, I think, originally (in Africa!). Charlie didn't have a conventional education. Cochran had a quasiconventional education. I'm trying to think of people who had strong data linkages and visible positions. I don't know how much the extensive chemistry and general physical science education that I had was nonstandard. I took a year's freshman English and everything else I took was on a diagonal across the campus from geology to math and physics and chemistry. Had this been all chemistry, it probably wouldn't have been as good as that. Have you read the paper about the education of a scientific generalist (Bode, Mosteller, Tukey and Winsor, 1949)? That's what seemed to make sense at the time, but it isn't something that happened. Dick Link had an aphorism that a statistician had to be a schizophrenic because he had to deal with mathematics, which was the most rigid of anything, and with the data, which is the least rigid. Now, I was willing to use mathematics to produce possibly impractical things, but I was also interested in techniques for which one could feel they were doing well whether or not there was any proof of it. There's a science fiction story by a lady named Katherine Maclean called *Incommunicado* which is set around one of the satellites of Jupiter or Saturn. The difficulty had to do with the senior man at a working group there who was an analog type when everybody else was digital. Now, I think as far as data analysis goes, maybe I'm the somewhat lonesome analog type. I expect to "feel" about whether something will work or not. And I don't expect to find this out by proving things.

Q: But you do understand people who say "feeling it is not enough."

J: Sure. Feeling is individual. Now, I would want to respond to Fisher's feelings very strongly although his basis might be very different from mine. But, the set of people whose feelings you can trust is going to be smaller than the set of people who can decide whether a proof is correct or not. So, there is a legitimate reason for natural selection against people who offer it on feeling.

Q: It seems to me also that the evidence that you accept for demonstrating the usefulness of something is also different from what other people want. You do not seem to expect a mathematical proof—casting it into some theory of optimality.

J: No, because I know too much about the anomaly of what is constructible in such a way to want to go that way. On the other hand, I think I've always been willing to take the mathematical structures and mathematical proofs as part of the story, and to expect that there were situations where one wouldn't have a feel for how to understand.

Q: Did you coin the word data analysis? Or does it come from earlier ages?

J: It is not one that I would recognize as a particular entity. You would have to talk to Steve Stigler or somebody about this, to see if he can answer the question, but not me.

Q: Do you think you ever gave a talk on the box plot? I ask this because I wonder whether you thought of the exploratory data analysis (EDA) methods as a research project.

J: Well, I guess I thought about writing EDA as sort of a research project.

Q: Because you tinkered a lot with it.

J: Yes. I tinkered with some of it. Some of EDA had existed for a while and some of it was put together during the writing of the book. And that had its evil features, because there are some things in there that are more complicated than what people are likely to use and that was an aftereffect of trying to do things and trying to do a good job on them. The position that I would have liked to have met, or would like to meet, is that the techniques were at least 50% efficient. If they were 80% efficient, that would be wonderful. So, the attempt to try to squeeze things as thoroughly as you can could be overdone. We'll see what the revision of EDA looks like. We should bear in mind the title of one of my papers, which is called "We need both exploratory and confirmatory" (Tukey, 1980). And it's not that EDA is the whole story, but if you took 1,000 books on statistics when EDA came out, there would be 999 on confirmatory. So, it was right and proper to push EDA moderately hard so that it would be recognized in parallel. And it probably still needs this.

Q: But what do you exactly mean by confirmatory? Do you mean model-based inference?

J: A situation where the questions could be specified in advance and quite a lot of technique picking done. That the overall logical position was that there were some questions. And these questions had been suggested. And roughly the only mechanism for suggesting questions is exploratory. And once they're suggested, the only appropriate question would be how strongly supported are they and particularly how strongly supported are they by new data. And that's confirmatory.

Q: In a strict Neyman–Pearsonian approach to confirmatory analysis you are not even allowed to look at the data beforehand. That was always a bit controversial with the Bayesians. How do you feel about Bayesianism?

J: Most of the time I don't feel I want to use it, but I have no feeling that says that I will never use it. If I get to a problem where that's the best to do, I hope I would be sensible and use it. In terms of things in the last very few years, I think that the most serious criticism of Bayesians is that they believe that there should be a single answer and in particular that you shouldn't stop with "if–then" statements that appear as alternatives. But it seems to me that there are problems in the real world which are going to have to be answered "if–then." If AIDS infections behave in a certain way, then so and so. And if they behave in another way, then something else. And try to resolve that into a single answer. Now, a Bayesian would argue that because he wants an *a posteriori* distribution for an answer, he isn't taking a single answer. But the idea that there's one framework that you have to take, and somehow you summarize into it all the relevant data in the world, and then when you've done this you are willing to accept this answer rather than to have alternatives, is I think a very serious thing. Now, classical least squares, general linear models dah-dah-dah-dah have a large dose of this. But, they usually leave some alternatives and usually you are not necessarily constrained to pick alternatives directly from the data before you. What you pick for a weight function for biweight can be picked on for other reasons. So, it's not taking the perspective that "the only good thing is perfectly focused a priori." It's not nearly as bad as the Bayesians from that point of view, although the way it's used it's often close.

Q: Do you think of the EDA book as a kind of a theory of data analysis.

J: No. No.

Q: You wouldn't want a theory of data analysis?

J: No! Colin Mallows has been working on this from time to time, and I'm pleased to see what he does. It doesn't follow that I like exactly all the formalizations he's put forward. But if we're going to understand what goes into data analysis—not of a formalized sort—it's almost certainly to our good if people try to formalize things and you find out which pieces can be formalized away and what's left that hasn't been touched yet. So, I wouldn't mind at all "a" theory of data analysis. I guess I would mind "the" theory of data analysis.

Q: But, I think in the preface to the EDA book you do say some words about the importance of vagueness—in concepts, etc. And you said it before also that you feel anybody who come and says "I have the answer to something" is probably making a mistake. This seems to be one of your principles.

J: Well, this is science as opposed to mathematics. Looking at things historically, in science the only things you can be sure about is that some substantial change will probably come along in the particular fields you're thinking about. This doesn't happen in mathematics.

Q: New things are being added.

J: And old things are being changed.

Q: No, I mean in mathematics. All that can happen is that new things are added. Old things, if they were correct, they're correct.

J: Yes. Although the question of what is correct is not as trivial as one would think. Herman Weyl's comment that the only mathematics that he clearly trusted was intuitionistic mathematics, but since he wanted to do mathematics, he didn't confine what he did to that. A very wise man.

BELL LABORATORIES

Q: When you started working at Bell Labs was your experience somewhat similar to what happens today?

J: When I first went to work for Bell Labs the war was still on. We were winding up what we'd been doing at Princeton and I went to Bell Labs with the specific thought that I was going to be involved with the NIKE program (antiaircraft missile), which was just then sort of being partly tooled up in terms of thinking and so on.

Q: That means there was a group of people working on that?

J: Walter McNair and Hendrick Boder were the two key people. Walter had done odd things for the telephone company. His group had built the first weather machine. When you called up, it told you what the weather was going to be. He sort of came from the acoustic side of things. And Hendrick was a mathematician and a circuit man, feedback type.

Q: You were supposed to design this missile.

J: Well, we were supposed to do what needed to be done to produce a prototype design for the whole system. Bernie Holbrook, who was a switching engineer by origin, and I more or less jointly ended up doing trajectory, aerodynamics and warhead. We ended up doing this quite empirically. We had some ladies turning hand calculators who were doing the differential equation integration. And the question is what path would the missile take to get the farthest out possible and still have enough speed to maneuver. And I sat down and did the variation on this—got four sets of coupled equations which if you integrated all those out you found out what the small variations were like. This didn't help. We did much better by seeing what we had done so far and then changing the lift profile a little and seeing what happened if you did this and that. Supersonic aerodynamics was in a very preliminary state. The only thing that got done analytically was the incompressible flow version which at that time predicted that if you went through Mach times square root of 3 then the controls would have the opposite effect of what you thought they would. This did not happen in the wind tunnel or in the atmosphere. And warhead, well, we did what we could with what people knew about vulnerability and got a reasonable sort of answer. So, out of that came a report. Other people were doing things about the computer that would be needed to steer the missile. And Walter McNair and some people in Whippany produced a wholly new type of radar to do the tracking. And all of this got put together in a report and it was decided to try to go ahead, and so a small gang of us flew out to the coast to try to persuade Douglas to be a subcontractor. I wasn't in the meeting where this took place, but it didn't take long for the word to get out. Walter was pushing on the Douglas people a little, and they were saying "but we make airplanes, we don't make missiles," and Walter said "and what do you think we make?" Which ended that one. So, anyway. I was full-time on this for maybe a year or something of that sort. Then things gradually whittled down a little. But I kept going to White Sands for the shoots of protomissiles, or missiles or what have you, and got used to sitting around the table with a small collection box. The rule was if anybody mentioned Reynolds's number, they had to put some change in the box. The general impression was that saying the missile's performance was different than it was in the wind tunnel because of the Reynolds number was a cop-out. But I got involved in other things from that point on.

Q: That was during the war and Bell Labs was basically subcontracting for the government to do this kind of work?

J: Well, Western Electric did the contracting and Bell Labs was a nonprofit subcontractor.

Q: Then after the war this armament research stopped or it still went on for a while?

J: Well, things like radar research stayed on. And Western [Electric] I'm sure kept the NIKE development. I don't know what the contractual arrangement was for the later things, because the whole development went on. NIKE became NIKE Ajax followed by NIKE Hercules, which was a much bigger and longer range missile.

E: I remember that, after we were married, you were still going out to White Sands every once in a while.

J: Sure. And going on "boondocks" expeditions to see if you could find any of the pieces somewhere.

Q: To know where it hit?

J: Well, and maybe to recover some pieces.

Q: Was Shewhart still at Bell Labs when you were working there?

J: Yes, yes.

Q: Was there a statistics group?

J: Well, Walter was always in the quality control side. And the key people as of that date were Shewhart, Dodge and to a lesser degree probably Romig. They had a lot to do with quality control. They weren't even in the research department. Later on, for the last few years, Walter did move out to Murray Hill and got into Research. But there wasn't a statistics department for some time. Paul Olmstead, who was a Princeton physicist originally, was involved with applications of statistics. But, there was an informal network and I spent a little time getting a distribution list—a list of people with statistical interests sort of—to lubricate things a little. Eventually they hired Milton Terry, he was the third person who was looked at hard and the first one where all sides sort of agreed to go ahead.

Q: And he was a statistician?

J: He was a statistician.

Q: What about people like Shannon. Was he still there?

J: Yes.

Q: And he was more a mathematician?

J: Yes. He was definitely. But a mathematician interested in practical matters. He wrote a paper whose title bothered some of the laboratories people; it was called roughly "How to do things reliably with crummy relays" (Moore and Shannon, 1956).

Q: That was the title?

J: The title had "crummy relays" in it. They didn't like that. There was a question of how did you hook things up so that if you only had a few failures it did what it was supposed to. And then of course the information theory stuff, which to a degree was in parallel invented by intelligence analysts. Shannon was a very reasonable person, but he wasn't a data analyst.

E: John, how was it then that he turned up at the Center for Behavioral Sciences the year we were there?

J: Well, probably information theory, which people thought was an important thing in the psychology etc. area. There were always a few anomalous people, even like me, at the Behavioral Sciences Center.

Q: But he was considerably older than you, Shannon, was he not?

J: Don't know; don't think so. Had you seen him at the final dinner at the Center, in which he appeared riding a unicycle, with Betty sitting on his shoulders, you would not have thought he was an old old man.

Q: Then I think we should talk a little bit about the time series analysis and your book with Blackman. Who was he?

J: He was a communications mathematician. Now, I'm trying to see when we ought to start this story. [John checks the bibliography while tea is being served.] Well, the origin of the later time series work probably comes from a number of practical problems, one of them being the measurement of the irregular motion in the atmosphere which causes an airplane with fixed controls not to fly a straight line—which was interesting to the boys in Whippany because one wanted to understand sort of what is the least unpredictability that might be in the airplane track. And this turned out to get Cornell Aeronautical Laboratory hired to fly airplanes along the lake, because there was as uniform a nearby surface as you knew how to find.

Q: And then you analyzed radar data?

J: You record what the controls are doing, you record what the accelerations are and so on and then you try to make sense of it. In this case, it didn't work at first, because people had been trying to read averages for, say, a second over each second on the record. And when we got them to read exactly what the trace said at the mark, then the analysis started to make sense. But this involved fairly complicated multivariate time series where some of the regression coefficients you know from the wind tunnel behavior, maybe some of them you don't. And so, this is one of the reasons why the first time series paper I find in the bibliography is Press and Tukey "Power spectrum methods of analysis and their applications to problems in airplane dynamics." That's 1956. The Blackman and Tukey paper "The measurement of power spectra from the point of view of communications engineering" is 1958. There were always things going on around Princeton with Hans Panofsky from Penn State, who had been bringing measurements of low-altitude atmospheric turbulence to be tried on Johnny's new computer. [This was weather data?] It was atmospheric, but not weather. In particular there were Brookhaven tower measurements on wind component velocities in all directions. So that had got involved. That's probably earlier than the other. It didn't produce anything that I published that was directly related. Another seminar problem was H. T. Budenbom's data about the performance of a new radar that he had obtained in a certain format and wanted to get it into another format so he could take it to the coast to talk to a classified meeting. And Dick Hamming and I discovered, one way or another, that if you smooth data series with a quarter, a half, a quarter, things get appreciably better. So, Dick and I took off a considerable amount of time to try to understand why this would be, and this produced the measuring noise color memoranda (1958; see Tukey and Hamming, 1984). Blackman and Tukey was an exposition of our combined work. Blackie had been teaching things to engineers. He knew a lot about what was going on. Between us we managed to put that together.

Q: And the intended audience was engineers?

J: Well, the intended audience was people who could live with mathematics but not necessarily too sophisticated. Including engineers. I don't know whether the Dover publication of our work is still in print or not. The last I know, it was. In which case it's been in print since 1959.

Q: It added a fair amount to the statistical literature on time series.

J: And there were other things going on in parallel that didn't necessarily get written into that. There are two volumes of collected papers on time series and related things.

Q: The interesting aspect of all this is that you say you did it at Bell Labs and one would think that it's signal processing, but it wasn't really that—it was atmospheric data.

J: No, we just happened to mention atmospheric data. I'm not sure what all it was used for. But, for example, after Mike Healy and Bruce Bogert and I got involved with cepstra (see Bogert, Healy and Tukey, 1963), one of the people there used more-or-less cepstra-related things to produce the first machine that would really give a reliable account of the pitch of your voice. And, radar tracking errors is not an area that was devoid of interest for the Laboratories. More recently, there've been people who have been doing underwater geophysics, where spectrum analysis was crucial. Et cetera. The Budenbom data caused our perception of "a quarter, a half, a quarter" and eventually led us to the understanding that a Viennese meteorologist named von Hann had liked to do this. It was not atmospheric data; it was radar performance.

PERSONAL

Q: Let's leave statistics behind temporarily, John, and let's talk about your work habits. We are all impressed by the enormous amount of work that you have produced and we wonder how a person can produce so much. Did you have a very disciplined way of doing things? How many hours of sleep do you need?

E: I can talk about that. It varies at various times, but you can tell what the stress level is by how little sleep he gets. If the stress is high, the sleep is low and I think that one of the most stressful times that I saw him have was when he was working on the test ban talks and the detection of underground testing. John had pulled some rabbit out of the hat that made it clear that nuclear underground testing could take place and it would not be noticeable up on the surface, which people thought it would be. John, am I correct in this?

J: I don't remember it that way, but I don't remember it well enough to make a loud negative.

Q: So, little sleep means what?

J: Yes, how about some numbers for the sleep?

Q: Five hours?

E: Yes!

Q: Over a longish period of time?

E: Yes, that was sort of the worst. There was another time when you did go back to the five hours again, John. You had said to me at that time, this was 1959, that if you hadn't taken off weight when all of this nuclear testing stuff came up, that you would have been sick, because the stress was so great.

J: Well, anyhow, I think I conventionally had an eight-hour target. Whether I got it or not was another matter.

E: How often do you start to work when you have your snack in the middle of the night, whenever that is?

J: Yes, well, the snacks in the middle of the night are a relatively recent phenomenon.

E: But you used to get up at the same time whether you had a snack or not. About at three thirty.

J: But, by and large, the efficient time for me was early, not late. I typically didn't try to work after supper.

E: And he didn't like to talk about what went on during the day at dinner or after supper. He said it was enough to get through the day without thinking about it when he came home. He reads mystery stories at night to get to sleep. And that varies, I think, depending on what the story is and what his sleep position is. He always (or almost always) had gotten up sometime about three thirty and gone downstairs to get a snack. He would come upstairs again, maybe read a little more, go back to bed and then wake up at various times. But if he woke up at five a.m. and started to work, I knew that life was tough. And that happened for a number of years when he was trying to get the statistics department established. What he said to me, at the time, was that if he hadn't had that writing to do, which was EDA essentially, he would never have gotten through all the emotional trauma of getting the department started at Princeton. At the same time, there were also some growing pains at Bell Labs. When Ram Gnanadesikan came in as the head of the statistics department at Bell about the mid-sixties—I can't tell you exactly when it was, but it made a tremendous difference to improving John's life and mine.

J: One of the statistics departments, there were two for a long time. And they operated with a very weak barrier between them.

E: When he was working on his own research, John would come down at breakfast and work in his study. He would be in there from breakfast until sometime in the afternoon and always, always, playing classical music loudly. I can't tell you how many times I heard Mozart over and over and over. And also those sixteenth-century singers who did contra singing.

J: I'm not sure just which one you're being worried about.

E: I wasn't worried; I just think it's funny.

Q: But this was just background; it didn't really enter your brain.

E: He had to do that to keep out extraneous things that might have been diverting. He closed the door, put on the music as loud as he could and blocked it all out.

J: "As loud as he could" is a slight exaggeration.

E: Well, I did have the power to apply the breakers.

Q: Now, they have these Walkmans with earphones. You think that would have worked as well?

J: Well, what is it, three Christmases ago or two Christmases ago, the New Haven relatives gave me a Discman for a Christmas present. It's been parked on the bed ever since while I'm in town, so if I feel like it while I'm lying in bed, I can just reach over and turn it on.

E: How often do you do that?

J: Three to eight times a week.

Q: What's the other secret of your immense capacity for work? Quick absorption of ideas I think is necessary; a very good memory is necessary.

J: And maybe quick generation of ideas as well as absorption.

E: Well, there is one little story I'll tell you. At Brown, one commencement time, John and the Dean of the Faculty were talking with each other. The Dean was a physicist and he was complaining that he never got the chance to do any work because of his administrative duties. And he had more or less brought this up with John a couple of times. And John said to him, "I think that what you really need is a place where you can get away from everything and write or do your research." And John didn't specify anything about it, but he said it should not be at the office. And so I asked John where he did his work and John said, "Why of course I do it at home." And you know, I hadn't realized that. It hadn't penetrated. He never went to the office and did anything.

J: That's a slight exaggeration, but not very much.

Q: When you went to the office, you did not go there for research work. You went there for specific things: meetings, giving classes and so on.

E: This was one of the key things.

J: Probably a fair amount of work went on in Murray Hill—because that was probably a lot less subject to distraction.

Q: Better protected.

E: Well, there's one other thing that does make a difference and that is what secretarial support you had. In 1968 or 1969 John interviewed three different people to fill a secretarial job that was vacant at Bell Labs. He picked Mary Bittrich and Bell Labs never knew what hit them, because he also moved the bulk of the secretarial work being done at Princeton. I think that move was providential because Princeton never had adequate secretarial support.

UPBRINGING AND EDUCATION

Q: John, we know you have been raised in New England. How important was your cultural New England background in your life? Do you think it shaped you in a certain way? Do you think that things would have been different had you been raised in another part of the country?

E: That's the kind of question you never can answer for yourself, I think. John, what do you think?

J: I'm glad to associate myself with your remark. Now, what would you tell them about this?

E: He's a New Englander through and through. I met John after I had been in Princeton for two years. But, the important thing was that I had lived in New England when I worked at Wellesley College and when I went to graduate school at Harvard, so that I found the New England people, their values, everything very compatible. More compatible than with the Middle Atlantic states where I grew up. But, eccentricity is not considered eccentricity in New England; that's just the way people are and they have a right to be that way. And you don't think about it. The air is just different there; it's independent air. It may be that it's the effect of having so long been a maritime community they have solved a lot of problems and they are not averse to taking on a problem and making a decision about it, which I like. Because, usually, you're never in any doubt about where a New Englander stands on something. They don't turn out necessarily to be copycats of each other, but the individuality is essential and I think that's one of the things that John has in spades. My family were shocked when they first met him because he was so unconventional. Yet, I grew up in a family that was very conventional in one sense, that is, Episcopal church; you know, what things you did and what you didn't. But I was also very unconventional because of my mother's background. She was from a pioneer family that had been in Virginia for over 200 years. And they sort of made their own lives just the way the New Englanders did. My grandparents were Baptists—either Baptists or Methodists is what you were in the South. But, on the other hand, there was a lot of eccentricity in my family that had been accepted. I finally realized later on that was one of the things that appealed to me about John. One of the first times he ever appeared in my father's and mother's house he came to pick me up.

He was wearing a very old Teddy bear overcoat, you know the kind that was like Teddy bear stuff. It was a fake fur, and it had seen a lot of wear and, when he got ready to go out, he pulled out a hat. It was a broad-brimmed hat—like a fedora or something similar—only it was unrecognizable. He had squashed it up so that it would fit in his pocket. So, if it got cold, he'd have it then. That hat was absolutely a howl. I told my mother early on that he didn't say much, but what he said was bang on and I still will stick by that.

Q: I think it is clear that the New England background is quite essential.

E: And he hasn't lost it. It's a value system, too. And I adored John's father because he had this wonderful way of bringing out people and he had a wonderful sense of humor—better than John's actually—he was John's role model and very quiet, nonaggressive. He had his Ph.D. in the classics, had gotten it from Yale and was at the American School in Athens for a year, etc. And he taught at William Jewell College out in Liberty, Missouri, as his first job. It was a men's college; it was a Baptist college to prepare ministers, among other things. When World War I came along, you know, all the young faculty resigned in mass, so that the older faculty would not any of them lose jobs when they had family and children to support.

Q: And his mother?

E: His mother! Well, let me start by saying that Sir William Pepperell is a relative of John's. He was the American that won the battle against the French at Louisburg. It was Sir William Pepperell's sister who married John Frost, who was a direct line down to John's mother. And when we were first married, John had told me that his father and mother were number one and two in their class at Bates College (class of 1898). And I asked which one was number one? And he said he never knew, and I said, well, then it has to have been your mother—which it was. When I asked her about that, all she said was "I was just a flash in the pan, my husband was the scholar." But never mind; the flash in the pan did very well and she was a superb teacher. Both of John's parents were teachers who came from a long line of teachers. They realized that they had something different in their son very early on. I'm sure that their schoolteacher training and experience would have facilitated this. They made up their minds that they would educate him at home. So, as he said, chemistry and mechanical drawing and French were what he took at school.

E: You said you were educated at the New Bedford Public Library, right?

J: Yes.

E: John's father said to me that if John came to him with a question, they wouldn't necessarily answer it, but would give him the clues to go and look it up and to dig. And I think that is another very characteristic thing, that he's not afraid to jump in and look for something. When he went to Brown, he had not been any place where he had been with other school children his age. He'd had neighborhood friends but he was not part of a group. And I thought about this in my later years, because it's pretty outstanding that he is a lone figure in a way. There are plenty of people who know him and there are plenty of people who like him, but he remains—I think—just that. And I think it's because of the fact that he never went to school till he got to Brown. He commuted to college for two years and then he lived on campus. He was actually the class of 1937 but he graduated in three years, class of 1936. Brown looked at this record and said "Oh gosh, you know, why don't you just go ahead (this was the spring before commencement)—why don't you take your degree now?" And then he stayed the extra year for the master's degree. His mother was very active in the community; she was head of the YW when I was first married. She taught school in Quincy, Massachusetts, and in Bridgton, Maine. In Maine, she said that when she woke up in the morning she had to break the ice on her bowl and pitcher set in order to get water to wash herself. But, she very shortly got a very good job at Quincy High School and then from there was hired for the New Bedford High School. She and John's father had met while they were at Bates College in the same class. John's father was teaching in Liberty, Missouri. They got married in 1912. John's mother had to give up her job when she married because the state of Massachusetts law declared that no two people in one family could work.

J: I think that's wrong. I think the state law was that married women didn't teach.

E: Oh, all right. That's even worse.

J: I don't think it was just nepotism. She couldn't be a full-time teacher.

E: She could substitute and she did substitute in everything from typing to Portuguese. I'm just trying to think. I think that sums up the main things about his father and his mother.

Q: What are the things you like to do, John, to relax?

E: Read mystery stories. Number one.

J: What do you think would come next—listening to classical music?

Q: Do crosswords?

E: Yes, but not in a big way. If there's one around he'll do what he can on it.

J: Actually double crosstics are more my thing than crosswords, but that varies over long stretches of time. There would be years when I did more of it and years when I did less.

E: There's one other thing that I have to mention about your work habits, John. My father asked John whether, while he was waiting for me at the altar, he would whip out a yellow pad and not waste any time! This has been a wonderful characteristic for me because I had a very impatient father. If he was waiting for me, or my mother, or somebody else, he was always chomping at the bit. John has always had something to do that kept him from being impatient.

Q: He didn't mind waiting?

E: Not a bit. In fact, I think he's a saint in that, because he's always had something to do and he's never been a nagger or anything like that.

Q: Do you have to read mystery stories twice sometimes? If you reread them, have you forgotten the plot, or do you always remember it?

J: I certainly do not always remember it. But reading it only twice would leave me in very bad shape. All the good ones would be used up.

E: Let me tell you another story. Some of my father's relatives read aloud each night. It is sort of a tradition in the Rapp family. So when John and I were married I said "how about if we get a book and read it aloud before bedtime?" And a very pained look went across his face and I said but what is the matter. And he said, you know, because I can read a book in an hour, spending an evening reading aloud would be an awful drag. So I saw the point of that immediately.

Q: So, he's a quick reader.

E: Yes, really.

J: Not as quick now as I used to be.

E: John, how much fishing did you do? We had both grown up at the shore and fishing was something that we both liked to do and have often done.

J: I think the best term is "some." Not like cousin Chick.

E: No, I know. But that was a pastime. We ought to show you the picture we got that was taken down in Key West when we were there one winter. I'll go get it. It's kind of fun.

Q: You were deep sea fishing?

E: Yes, and I got a wahoo (a big game fish) and his got away. Those are the fish we caught, the two of us, one day. And the big one here, the sleek thing is down in my cellar, stuffed. And these two big ones, that one in the front, John got a citation in the Miami fishing derby that year. We got our picture in the local papers. And I said to one of my Rapp uncles who used to take me fishing when we were growing up, "see what your pupil has done." And he sent that picture off to my cousins with a caption saying "Why don't you do something like this!"

FIG. 6. *John and Elizabeth Tukey in Key West with fish they caught.*

Q: Gardening is another thing you enjoy. Is that correct?

E: The problem with gardening is that your knees at the back of your legs begin to go and you can go down to work on your knees but getting up is harder and harder. And so I think that's going to limit the gardening. But, he's been a fantastic weeder. He and his father used to go out and weed in the garden and talk and visit. And patience, again. For most people, weeding is the worst thing they do in the garden, but that's what he goes for first. So I've been blessed.

PRINCETON GRADUATE YEARS

Q: Can you tell us about the time when you were a graduate student at Princeton?

J: Well, I was a graduate student here for two years.

E: He didn't waste any time!

J: Because I came in 1937 and I got my degree in 1939. I lived in the graduate college both years.

E: You lived in the graduate college until we got married, more or less.

J: Now, there was a group, largely of mathematicians, who ate together and ate at the near end to the first table on the right as you went into Proctor Hall. And Lyman Spitzer, who died recently, was the official Fuehrer—this was far enough before 1941 so Fuehrer was not completely a bad word. He was responsible for dividing extra ice creams into the required number of slices, required for the people who were interested in things of this sort. He was an astrophysicist. There was also an astronomer or two. Frank Smithies, who had come from Cambridge as a mathematics postdoc, was part of the group.

E: Cambridge, England.

J: And we had one chap who was a graduate student in romance languages who had the special privilege of being able to put people in Klein bottles if he wished. (The Klein bottle doesn't have an inside.) That was the group that I hung out with the first year, and I guess my second year would have been with some of the same people. But Ralph (Boas) would have gone; he was a National Research Fellow—that was the time he went to Cambridge to spend a year with Besicovitch.

Q: Was Richard Feynman part of that group?

J: Well, one of the things that went on is that Arthur Stone from England (I don't think it was Frank Smithies) had to buy some paper for his looseleaf notebooks. Because he had British-sized notebooks and U.S.-sized paper he had lots of paper strips. So he started folding regular polygons and recognized when he folded a hexaflexagon that he had something different. In a hexaflexagon what you see are six triangles and, by folding in and out, a different face comes out. So, Bryant Tuckerman and Dick Feynman and myself got involved in doing flexagons. So that was an incidental activity. Another incidental activity was that Aurel Wintner was here at the Institute for a year—in those days the mathematics part of the Institute sat in Fine Hall—so he was giving something between a seminar and a course. And at the end of the course C. C. McDuffie, who was the only remaining attendee beyond the three of us, took everybody in his car for a trip up to North Jersey to celebrate. So, the notes for that course according to the Library of Congress entry were by Ralph Boas, Frank Smithies, John W. Tukey, with the sympathetic encouragement of Cyrus C. McDuffie. I was supposed to be a chemist that first year and I was an assistant in a sophomore analytic lab, which perturbed me a little when I first came here, because I'd been an assistant in a physical chem lab at Brown for a year and a half. But at Princeton you had to have a Ph.D. to be an assistant in physical. I was around chemistry some, but around mathematics a lot more. And I took the prelims in math at the end of the first year.

Q: I think Princeton math always had somewhat a reputation that you did your learning by yourself.

J: Well, there was a baby seminar tradition: that if one of the standard courses was not being offered, then the graduate students who needed that for prelims were supposed to get together and run a seminar on their own and learn it. But there wasn't an absence of courses. There just was an absence of complete coverage.

E: One of the interesting people who was around Fine Hall in those days was that fellow who broke the German code, Turing. You drove to North Carolina with him, didn't you?

J: We drove to North Carolina in his car; I don't think he was actually going. There was a meeting in North Carolina.

Q: He lent you his car?

J: Yes, the Turing car. I think that's it. I know Ralph Boas was one of them, because there is a picture somewhere of Ralph with his umbrella pointing to a street sign in Chapel Hill that says 12 and 3/4 Street N.W. I won't guarantee the prevailing accuracy of that.

ENVIRONMENTAL POLICY

Q: An important part of your involvement in public service has been the environment. Can you tell us something about this?

E: Well, let me tell you the story; it's very interesting. Rachel Carson had written her book in the 1950s. And in the early 1960s, people were beginning to really take all these environmental things to heart, people who were the avant-garde. And I can remember one of the summers—and I think it was 1962—when we were at the Behavioral Sciences Center and we got invited to a cocktail party over on the Stanford campus and there we saw Kai Lai Chung, a mathematician that John had known long ago. And so, we were circulating around having a ginger ale or something and Kai Lai spied John and he came over and told him that he personally, John, should do something about the environment, that it was absolutely intolerable what was going on as told by Rachel Carson, and he got all fired up. I think everybody knew at that point that John was very active in Washington, because this was while he was on the President's Science Advisory Committee (PSAC). And he said: "John, you got to do something about this!" Well, about two years later—John was off the President's Science Advisory Committee—but Lyndon Johnson became President and as part of the Great Society program one of the things he wanted to do was to look at the environment. Now, John, you can pick up from there.

J: Yes, well. Actually, things go back further than that. I was on a thing called the President's Air

Quality Advisory Board at one stage, which was interesting rather than important, I think. This was just as Ruckelshaus was taking his first term as EPA administrator. And he was still optimistic that if you told the polluters what they were doing, then they would stop doing it. So, I had been involved in things before I was on PSAC—I'm not sure; this might have been a year or two earlier. Then, on PSAC, I was involved in some environmental things, but the specifics I don't think are important. Now, I guess the first time that I got into the environmental things just tangentially was when there was a previous report on some environmental issues that was going through PSAC. This was when I was still on PSAC. It was very interesting. Elizabeth just mentioned *Silent Spring* (Carson, 1962). And the thought that PSAC would mention Rachel Carson had some of the people from the Agriculture Department practically weeping in their beer. Really, it was surprising how strong the feeling was.

E: It would interfere with them making money. DDT was still much used.

J: There was a committee on Impact of Stratospheric Change that was a National Academy of Sciences—National Research Council committee. This tied into the ozone problem. And I found myself running that now for the Academy instead of PSAC. And, there were a couple of go-arounds on that and then I was happy to see other people take over. We tried to say what we thought the scientific facts were. But we felt the only way that we could really communicate about how strongly we felt about some things was to actually go to practical recommendations. So, we were beaten on a little by the Consumer Products Safety Commission, who thought that that was their business. And then, still later, well, something that's an overlap with this was a report for PSAC called "Chemicals and health." Most of it was nonpollution but a fair amount of it was environmental. That arrived just at the time that Nixon had eliminated the President's Science Advisory Committee. So, that was about another year or so coming out. It came out through the National Science Foundation. OMB [Office of Management and Budget] didn't like it because it recommended that the administrator of FDA etc. should rely on at least having advice from a scientific committee on major issues. And they thought that if you tied him down that much you couldn't get good people to take the job. So, things sort of stalled for most of a year. But it eventually came through and it came through with a preface which indicated that the issuing authority didn't necessarily agree with everything that was in the report. But we didn't back down on it. And still more recently I was on the oversight review board for NAPAP, the National Acidic Precipitation Assessment Program.

E: In other words, acid rain.

J: The purpose was to see that the review process for all the reports for NAPAP was properly conducted—not necessarily to do reviews themselves. But it was an interesting operation.

E: But you notice how acid rain has simmered down as a topic.

J: The situation is even more complicated than that. There was a law passed in Congress without waiting for the final report from NAPAP. There was officially a thing called the Committee of Joint Chairs, which is, roughly speaking, 12 people from 12 different agencies. And when it came time to getting out a final report, they wanted to have it so that everybody was willing to sign off on it. It produced things like the Director going up to Congress and saying that the continued effect of acid rain on the Adirondack lakes was to make one plus or minus 200 lakes more acid.

E: You were on the Committee on the Ocean and Atmosphere and another one, clean air and/or something.

J: It's true; I was on the NOAA [National Oceanic and Atmospheric Administration] Advisory Commission on Oceans and Atmosphere which was a very diverse, and on the whole moderately effective, committee. It represented quite diverse views. We had one man from the Seaman's Union in Seattle and one man who represented a large commercial shipping firm. You could hardly get much further apart on that particular point. It was unintentionally sabotaged by Nixon, who appointed two people who lost their races for the House of Representatives to it. This was when the Democrats still controlled Congress. Nixon's actions made Congress so mad that they disestablished the commission and put up a new one, so that everyone went off. I think the new one was a little bit more loaded with do-gooders and I have no idea how it functioned.

E: One of the people who was on that commission with you was Shirley Temple's husband, whose father was head of Pacific Gas and Electric Co.

J: He was active actually in the Middle East doing aquaculture. He was somebody who had a background that was appropriate to add to the crew.

E: Well, when we went to the first UN conference on the environment (and there's one going on in New York right now) there were a lot of interesting people that we met, such as Margaret Mead and Shirley Temple. Shirley Temple was the most effective delegate there, because she was recognizable by all the African states and she really did a terrific public relations job.

Q: How does the Health Effects Institute (HEI) tie in with your work on the environment?

J: It was set up to be concerned about the health effects of automotive emissions.

E: Then that's one that ought to be included, because you spent eight years working for them.

Q: Was it funded by the auto industry?

J: Funded fifty-fifty by EPA and by the auto engine manufacturers.

E: That's another thing Bill Baker got John involved in. Why don't you talk about it a bit.

J: Bill was on the HEI board of directors. It operated through two committees—a Health Research Committee and a Health Review Committee. I was on the Health Research Committee for a long time. This committee was responsible for planning a research program and selecting people to do it and chewing with them somewhat. Then the question was about what the report would look like and at that stage it went over to the review committee, so that people could be sure that the reports were going to be reviewed before they came out.

Q: That's up in Cambridge?

J: Yes.

ELECTION FORECASTS

Q: When did you get involved with calling elections?

E: 1960.

J: Yes, I guess I wasn't involved in anything before that; the 1960 election. Things might have started in 1959.

E: It was tied in with the development of the computer because RCA did this originally to advertise their new computer. They were separate from NBC, though they owned them.

Q: And the Kennedy election was relatively difficult.

E: Yes, it was. They locked all the analysts up because they didn't believe they were smart enough to have done the prediction. And they kept them there till eight the next morning.

J: You're thinking of a later election.

E: No, it was that one.

J: I'm sorry, my recollection is different. The one where one of our friends was on for one of the other networks and had to come up and apologize because it was turning out that he'd been wrong when he called it. One way or another, I was involved from 1960 to 1980, mainly in presidential elections. I think once or twice in the intervening ones. The techniques developed through the years. Some of the rest of us, especially David Wallace, had a lot to do with how they were developed. Initially, we were just looking at what the current return was in a state and the history of how the deviation from the final answer had behaved in terms of percent voting in previous elections, which was called an m-curve. There got to be more and more complicated calculational procedures that eventually ended up with two up-and-down stages. One calculation taking the estimated turnout up and then down again: the top, say, is the whole state and the bottom is individual precincts or groups of precincts; another pass, up and down, for what the vote percentage was going to be.

Q: And the input data were actual counts?

E: Yes, they had people calling in.

J: This varied historically through time. Initially things ran mostly on the sort of routine information handling, with a few special precincts taken singly and called in directly. But with the competition it eventually got to the stage where there were tens of thousands of precincts that were "strung" with somebody there, and when they got a result they called in. Doing this between three and five times in parallel for three networks and a couple of newspaper services was unbearable for the financial side. So, there got to be a News Election Service that collected this sort of information for all the networks. And the networks were only supposed to be able to do projections on the basis of what was available to them on a common basis. The typical NBC arrangement ended up with a statistical group doing this sort of thing and Dick Scammon paying careful attention to key precincts. The theory was that if these two agreed, it would be safe to call. But one time, when the statisticians were down in Cherry Hill (remember, this was RCA), we called the governor's race in New York and California. And for two hours, the results polled in went the other way and we sat there and didn't uncall. And after about two hours, things began to turn around and it was all right. But it wasn't guaranteed. This gave rise to a lot more pressure on the model.

E: How about the year when all the machines broke down and you had to do it just with paper and pencil and an adding machine?

J: Yes, there was one time, when things were in Radio City, the machines got in a state of mind and there were people on the floor cleaning the tape heads in the hope that that would make the program run. So, Dick Scammon and the statisticians used elementary methods as far as possible.

E: That was kind of tense, though. That first night, it was so close that the NBC management didn't believe they could trust the figures that had come from the statisticians and, instead of allowing them to go home, they locked them up there. They did not let the statisticians out until eight thirty in the morning. And you were all right, that's the other thing.

J: Yes, that's the election where the river wards in Chicago were crucial. And there was a question of one set of people holding up an equal number of areas for those the other one was holding up. And nobody wanted to come down and say for the benefit of the other side we twist a little to make Illinois come in. This is the nearest thing to real-time statistics that exists as far as I know. Because you're supposed to be fast, but not make any mistakes.

E: And you didn't. You all didn't. You never had any fiascoes.

J: We didn't have any fiascoes, but we probably called an occasional thing we shouldn't.

E: You mean like a senator.

J: Well, I just am not claiming for certain.

E: Well here we are. Never absolute. Always leaving the thing open for the unexpected!

Q: Were the statisticians ever interviewed on camera?

E: No. John may have been on once. The only thing that was interesting actually happened to me. John fixed me up with a computer screen so he could ask me some questions. I could answer right away, so I was sitting there beside him all these various times. And one night, it was about two thirty and they kept the studio stone cold because of the equipment so that you were almost frozen. And they were running the camera around the room and you know this was a minute or two they had when nothing was going on, so I'm sitting there looking at the screen—in my coat—and all at once, what did I see on the screen, but me. I fortunately got out of the picture, before I reacted. That was really kind of fun and so I am in the archives at NBC.

Q: Elizabeth and John, we thank you for your hospitality and for this most enjoyable conversation.

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