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A Conversation with Cuthbert Daniel

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Cuthbert Daniel was born in Williamsport, Pennsylvania on August 27, 1904. He received his B.S. and M.S. degrees in chemical engineering from the Massachusetts Institute of Technology in 1925 and 1926, and spent a semester during 1928–1929 studying physics and mathematics at the University of Berlin. From 1944 to 1946 he worked in plant design and operations analysis at the Heyden Chemicals Corporation, and during 1946–1947 he did general statistics at the Gaseous Diffusion Plant of Union Carbide in Oak Ridge, Tennessee. From 1948 until 1985 he served as a consultant in industrial statistics, data analysis and the design of experiments for Procter and Gamble, Bakelite, U. S. Steel, New Jersey Zinc, M. W. Kellogg, E. R. Squibb, Itek, American Oil, Pan American Petroleum, Okonite, Consumers Union, Interchemical, and Technicon Instruments. He is a recipient of the Samuel S. Wilks Memorial Medal presented by the American Statistical Association and an Honorary Fellow of the Royal Statistical Society.

The following conversation took place in New York in January 1988.

"I CAME INTO STATISTICS BY LUCK"

Tufte: How did you become interested in statistics?

Daniel: It's an interesting story although I can't make much sense of it. I tend to describe the things I can't analyze as being due to luck. I've lived a life of luck. I have depended on luck all my life and have been wonderfully lucky at every stage. I came into statistics by luck. My wife Janet—whom I'd met by luck—was at Harvard taking her degree in biochemistry.

Tufte: When was this?

Daniel: 1933. One day she brought home a book she had been told to read by her supervisor, William Crozier. It was called *Statistical Methods for Research Workers* by a chap named R. A. Fisher. I picked it up and looked at it and after about an hour I said this is great. That's how I came to statistics. Of course I had a past. I was then nearly 30 and had been wandering in various wildernesses—teaching science in high school, serving on a committee that wrote a couple of books in progressive education as it was called then, doing nothing, thinking I was a physicist, et cetera. I notice that my 1927 passport reads "Occupation Physicist." A completion delusion. That's what I thought I was going to be. I had B.S. and M.S. degrees in chemical engineering from MIT. I hadn't practiced as a chemical engineer at all at that point. I decided I must get into a field where I could apply these things. It seemed to me that they must apply. So this statistical thing changed the whole situation.

Tufte: The reading of Fisher.

Daniel: The reading of a single book. I then found another book, *The Design of Experiments*. I said well this looks like a cinch which was, of course, what I was looking for. I read those two books and then wondered how I would find a job because there were no jobs listed for designers of experiments in those days. So I had to wait quite a while. I came to New York to teach some more and get more contacts, and taught physics at Sarah Lawrence College, a progressive women's college in Bronxville for four years.

Tufte: Did you do any more statistical reading during that period?

Daniel: Yes. One or two books. One by Rietz on mathematical statistics. Hardly worth recording. It had no impact. But there was an important book—Snedecor, *Statistical Methods*. It wasn't easy for me to see how to get the rats and tomatoes out of the text and get some chemical terms into it. Churchill Eisenhart said to me some time afterwards, "That's the whole problem. You've got to get their nouns and put them together with our verbs and try out the sentences on the consultee, or victim, and see if they make sense."

Tufte: And when did you start looking at statistical data?

Daniel: It must have been in 1942. I took no courses in statistics and haven't ever. I couldn't get any closer hitch on it until I took a chemical engineering job in the Heyden Chemical Company's penicillin plant in Penn's Neck, next to Princeton. It was the first penicillin plant in the country and it was interesting to work there because there were giant tests—they couldn't be called experiments—going on in the plant all the time. I had no impact on them and realized, correcting what I said earlier, that I needed some training in statistics. I went down to Princeton, a mile or two away, walked into Old Fine Hall, was directed to the statistics department, which was in the basement, to find two young men working side by side. I told them I wanted to learn some statistics. I had no more definite idea than that, and they looked at each other. I said I needed tutoring and that I would pay for it. They looked at each other and I went out of the room. When I came back a few minutes later, one of them whose name happened to be Wilfred Dixon referred to the other whose name happened to be Alex Mood, "Alex will teach you." I think when I was out of the room they tossed a nickel and Mood lost.

Tufte: And when was this?

Daniel: 1943. Mood was a wonderful teacher. Each time I came into his office there were giant diagrams—they were blueprints—all over the table. He would say "Just a minute." He would fold them up, all of them, and put them in a giant vault, and lock it, rather ostentatiously, I thought, before he talked to me. One day he didn't fold them up and put them away and I said, "Alex, don't you want to fold those maps up?" and he said, "No. Those are the blueprints of the deck armor of every major ship of the line of the United States Navy. They were rather secret, but we just found out that the Japanese have a duplicate set." So there was no need to be quite so careful. He was a very severe and valuable teacher. So I shouldn't have said I never had a course. I always start with Alex.

Tufte: What statistical material did you cover with him?

Daniel: Little problems with the plant. Started with the *t*-test and went to the analysis of variance. Never really got to large scale multiple regression because there were no large scale computers. Otherwise it was entirely what it is to this day, multiple regression. And some planning of experimental work, if I may use the word experimental to mean all kinds of statistical tests.

Tufte: Did he have data for you?

Daniel: No, I supplied the data because I got it from the plant. And I learned my first lesson in consulting beyond *content*, which is *manner*. It is often more important than content.

As an example, in penicillin production you have a 10,000-gallon tank which has filtered air running through it for a day or two, you then have to assay and see how much penicillin you've got. The assay was a slow thing, a bacterial agar plate which took 18 hours. I had noticed, as everybody else had but didn't see what to do about it, that when the mat, which is the penicillin mold, is very heavy one has a good yield. Also if the sugar, corn steep liquor, which is mainly sugar that is in the tank, is largely gone, you'll have a good yield. So I devised this brilliant idea to see if I couldn't get an equation that predicted the yield from the weight of the mat and the percent of sugar. And I could. I got a little two-term equation which gave me an answer in about 18 minutes. I fear I didn't push this very tactfully because I got fired in about 3 weeks. I said this was the way to do it and implied that anyone who didn't see this was an idiot.

Tufte: So this was the first non-content lesson in consulting.

Daniel: I learned it fairly quickly. Not that losing the job broke my heart but I saw how fast those people could act if they were offended. They were pharmaceutical people who had been in the business for their whole lives and they were not about to be told by somebody else how to do their work. Getting the checks that I got with my little regression equation against their results was worse than ineffective; it was offensive.

"THERE WILL BE VARIABILITY IN THIS PLANT THAT SHOULD BE STUDIED, MASTERED AND CONTROLLED"

Tufte: And then you went to Oak Ridge?

Daniel: I had heard about the A-bomb project in a talk at the Museum of Natural History by a *New York Times* science writer; a James O'Neil if memory serves. This lecture was on something he called the atom bomb and it was an indiscretion and a mistake. It didn't give anybody an idea of how to make it but it let people know of its existence. It was the first public revelation. The SAM project, as it was called then, was, if I remember rightly, on the seventh floor of the Woolworth Building. Well how do you get into the seventh floor of the Woolworth Building? This is one of the few times when I did the completely naive thing and it was the right thing. I went there, asked for the seventh floor and talked my way through several guards to get to the seventh floor, talked my way through four more guards to get to the head of the process engineering department, a dear man, a brilliant man—Manson Benedict, nearly a saint. This must be 1944 by now. The A-bomb project had been going for quite a while, no bomb yet. I had no idea of how the thing was made or what it was. I had a contention, an *a priori* contention. There will be variability in this plant, said I, and that should be studied, mastered and controlled, preferably with the help of somebody who can think statistically. He was shaking his head the whole time I was talking about variability

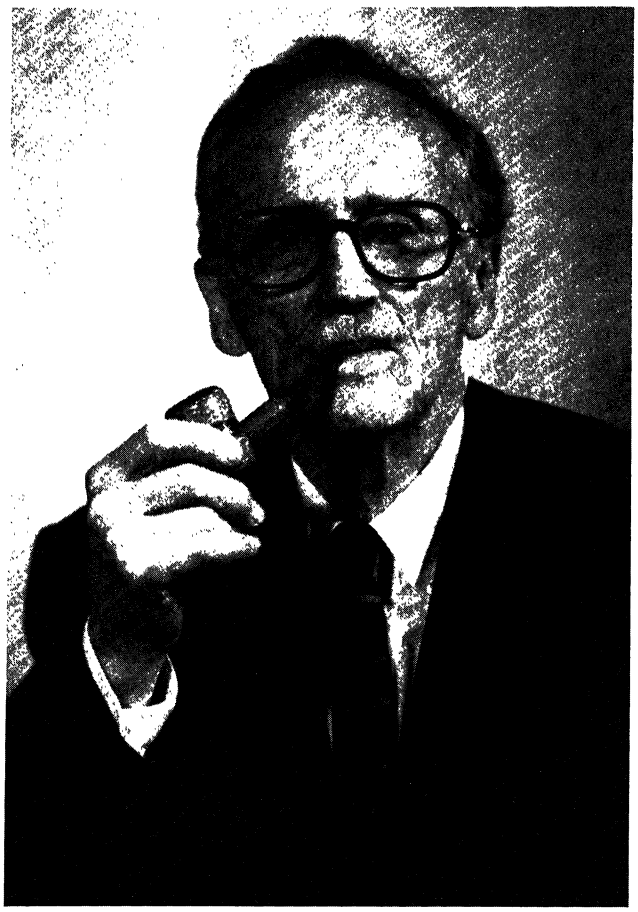

FIG. 1. *Cuthbert Daniel.*

in this plant. I remember his saying because of teasing about it later, "Mr. Daniel, this plant was not designed by engineers, it was designed by physicists and chemists and it's not going to have any variability."

It wasn't my qualifications that got me the job. What experience? One equation with two variables. He was broadminded and saw that I was quick and might find more and that might go somewhere. I got to Oak Ridge a few months later, taking my family, and started a statistics section. I had to recruit some local engineers and mathematicians down there. I had 7 to start and 25 to finish—something like that.

Tufte: Who were your colleagues there?

Daniel: One of the young men who came to work in my section was Robert Bechhofer, now and for the last 30 or 40 years professor of statistics and operations research at Cornell and still a good friend. There are others but they are not known to statisticians.

Tufte: And you wrote one of your first papers at Oak Ridge.

Daniel: Yes, with Nickolas Heerema a couple of years later, on data and thoughts that came entirely from Oak Ridge and from Bechhofer. It was, or so I thought then, the first paper in statistics on extrapolation—linear extrapolation. We had a great time there. Because the name "statistician" meant something different to management there from what it meant to me. I had to do a lot of interruptive talking because they *knew* what statistics was. For them it was a complete record of what the plant was doing at all points, at all times. That was their idea. But I managed to get a few statistical jobs in edgewise. The plant was taking the natural mixture of isotopes U238 and U235, and extracting U235, so that a graph of the U235 concentration versus Building Number from 1 to 47 was a curve that went from 0.7% (where it starts) to 90+. One of my jobs was to keep a record week by week of the average measurements made by mass spectroscopy in each building. After a month I noted that while the graph went steadily up there was one building near the top, therefore at high enrichment, which showed a flatness instead of a step up. You can see how much I had developed in politics because instead of now rushing to the management and saying there is something wrong in building 41 I went to one of the smartest of the process engineers and said, "If a building wasn't working at all, how would it show?" He said, "Oh, that's easy. Valve F43 is open, bypassing the barriers." So then I was alarmed and went to the plant manager and said "Valve F43 has been open for the last month." That is the only time I ever got any attention from him and I got it then all right. He was in my office in about 20 minutes. He asked, "How did you know that?" I started to laugh. Absolutely childish, but it had a big impact. The story was put about that this guy Daniel really knows what he's doing. It was a graph. There were no exact numbers. There was no mathematics. There were only weekly building averages.

Tufte: This exemplifies Yogi Berra's principle.

Daniel: "You can observe a lot by just watching." Certainly. Of course you do have to know what to watch, and you do have to keep watching.

This brings me to another aspect of the A-bomb project that most of us, including me, would rather forget. We were all motivated by the fear that the Nazis would get the bomb first. This fear was entirely (repeat, entirely) unwarranted. There was never any evidence that the German physicists were working at a pace that could produce a bomb. Nor was any effort made by Allied intelligence to produce such evidence. Only in 1952 in Goudsmit's book, *Alsos*, was the lack of evidence fully documented. To summarize rudely, we were all working frantically (and alas successfully) under a gross misapprehension.

There are, to be sure, things to be admitted and things to be learned. To be admitted: I spent too much spare time in Oak Ridge suspecting scientific colleagues of fellow-traveling (some were) and not enough time thinking about why we were there and what we should do about it. To be learned: Although I had always thought myself a thorough skeptic, I was not skeptical enough.

In 1946 Arthur Squires and I published a piece in the *Bulletin of the Atomic Scientists* titled "The Zero-Zero Plan." It was a plan to initiate a *dis*-armament race. It produced no resonance. It would never get off the ground. Not practical. We (Squires and I) fought for a year or two but obviously not hard enough or cleverly enough. We lost. The Principle of Unripe Time won.

Tufte: Do you draw a moral from this?

Daniel: Yes. Unwarranted fear is one of the worst possible motives for doing anything. And that is what drove us in the Bomb project. And what was used on us. Everyone on my level and above was drawn to the project, recruited to the project, by the warning that we were in a race to the death. The war would end very suddenly indeed if they got this first. That was wrong. There wasn't a shred of evidence that they were within 7 years of the end.

Tufte: Does this suggest to you that the United States shouldn't have pursued this or pursued it more slowly?

Daniel: We needn't have pursued it. I suppose a third of the scientists of the country were in the SAM project, another third were in the radar project, as you know. But this one was not needed. Now there are all the standard arguments. If we didn't discover it somebody else ultimately would. The most polite thing I can think of to say about that argument is "the later the better." That's my feeling. We were in it needlessly. You'd think the high order of intelligence of the leaders—Bethe, Oppenheimer, Einstein—might have sufficed to avoid the calamity. They were all motivated by the same fear from Einstein down. Einstein has said so any number of times. Unwarranted fear. One can't say just fear. There are things that it's rational to be fearful of. This was not one.

"I FELT THAT I HAD ENOUGH EXPERIENCE TO OFFER MYSELF AS A CONSULTANT"

Tufte: You wrote your first statistical paper out of the Oak Ridge work. When was that published?

Daniel: In 1950, in the *Journal of the American Statistical Association.* On linear extrapolation. [C. Daniel and N. Necrema (1950). Design of experiments for most precise slope estimation or linear extrapolation. *J. Amer. Statist. Assoc.* **45** 546–556.]

Tufte: Did you start doing consulting after you left Oak Ridge?

Daniel: Yes, exactly, with no interruptions and no further preparation. I felt that I had had enough experience there to offer myself as a consultant. In 1947 I wrote to the vice president in charge of research at Procter & Gamble. He wired back, "Come out." That was it. I was launched. I had a client and I had some experience. I still only knew something about the design of experiments and something about the *t*-test and something about the analysis of variance and very little about multiple regression. But these things were mainly still just dreams of mine. There were no usable books on multiple regression; there were a few more advanced texts. So I went to Procter & Gamble having again no idea what the problems might be like. Within a year I had found many statistical problems, from developing acceptable perfumes for the washing products, to doing serious studies using washing machines and artificially soiled cloths to rank detergents. They were very careful experiments because the results had to be widely applicable.

Tufte: How long did you work for Procter & Gamble?

Daniel: For a few years. It tapered off. I wasn't needed anymore. One of the things I learned is that when you are no longer needed, you do not conduct a rearguard action. You go quickly. Your reputation is enhanced if, when the next client asks where did you work before and is told P&G, then P&G says "he's great." But if P&G says, "He says the same damn thing over and over," that's not great. I was somewhere in between these two cases, I think, when I left Procter & Gamble. I went then to U. S. Steel. So far as I can see, no advertising except by word of mouth has ever helped me. Technical papers did not help me in the least bit. Written papers in those days were simple—they were examples of analysis of variance on some tabulated data. That kind of thing. Or I came out flatfooted for quality control. Some brilliant idea like that.

Tufte: Did you have more than one client at a time?

Daniel: Oh, yes, usually two or three. Union Carbide, Procter & Gamble, U. S. Steel, M. W. Kellogg, General Foods, E. R. Squibb, New Jersey Zinc, American Oil (later Amoco Chemicals), Itek, Okonite, Interchemical, Consumers Union and Technicon Instruments Corp.

Tufte: Tell me about those who helped you develop as a statistician.

Daniel: I have been centered too much on my own actions as if no one has played a major part in my career or helped me. That is unfair to a good many people. The major one was surely Henry Scheffé. I was on good terms with K. A. Brownlee, who was first at E. R. Squibb and then a professor of statistics at Chicago. He already had experience and had written singlehandedly during the war the first edition of his book *Industrial Experimentation*, I think it was 1945.

When I got to it, probably in 1946, it was a revelation. He and I were doing parallel processing. But he was generally ahead, so I learned more from him, I think, than he from me.

Tufte: What about Scheffé?

Daniel: Scheffé was wonderful. I met him at Princeton when I was being tutored by Alex Mood. He and I took immediate liking to each other. We had supplementary gifts. Henry was extremely shy. He dreaded going into a strange group and telling them what statistics could do. He lacked the gall for that. And it turned out that I could. He was very polite even in explaining to me what the equation for a straight line was, what each mean square really meant. We supplemented each other very nicely. He was kind to me in his book and in his acknowledgments because I supplied him with a missing link in his background. If he had only had influence solely through his classic book for the mathematicians and teachers, that would have been a very serious diminution of his power and of his history. He was influencing industry very widely through me and others. Each of my clients knew what Scheffé had done. I circulated photocopies of his memoranda, always handwritten, never typed.

I was strict about never working for two clients in the same field. Or even with related problems. That requirement has since been somewhat relaxed. Many companies realize nowadays that work done on their scientific problems can be widely useful throughout industry, and most of them want that kind of influence and reputation.

Probably my only claim to semi-immortality is through Henry's paper on multiple comparisons. [H. Scheffé (1953). A method for judging all contrasts in the variance. *Biometrika* **40** 87–104.] I am proud of the fact that my copy is inscribed "To the guy who hounded me into this." That was a wonderful tribute. And that was really all I did. Kept asking him. One night—Henry was a daytime man and I was a nighttime man—but one night he called up at 11 p.m. He said, "Guess what," and I said "This is Henry isn't it?" And he said "Yes, I have found it; it's the *F* distribution." He couldn't sleep and so he had to call me. That was a great experience. I take credit of course just for hounding him, that's all. I helped not at all with the mathematics. Henry did many, many other things. The whole set of papers on experiments with mixtures, by which we mean now not mixtures of distribution but mixtures of composition, multiple-component systems, where concentrations of all ingredients are forced to add to one, much of that was done under my hounding. And keeping him on the track so he didn't get into too great generality too soon. Keeping one foot lightly on the ground was my general assignment in those days.

Tufte: Did you read his book manuscript?

Daniel: Oh, yes. But that doesn't mean I had a big influence on it. Henry knew in deeper ways than I which ways to go on many parts of it. In the last chapter he talks in some detail without detailed solutions about the effects of failures of the assumptions of the analysis of variance. I spent a great deal of time telling him we really need to know about this or that. I think I had some influence on that part. But I had nothing to do with the lengthy first and second chapters which no teacher known to me has ever gotten through in any introductory course. It remained an impenetrable matrix wall which I could not break through or discuss at all. He wanted them to give dignity and some degree of rigor.

Tufte: I faced that same wall as a graduate student, in a course on analysis of variance using Scheffé's book as taught by Charles Stein.

Daniel: Oh, my. Stein would naturally want to use that book. In the 1950s Scheffé felt an obligation to raise the mathematical level of statistics, particularly of the analysis of variance, above that of the many introductory texts that were appearing. And as the *Citation Index* shows, the book has had very wide influence indeed.

"HALF MY CONTRIBUTIONS CAME FROM FANTASY"

Tufte: And other colleagues?

Daniel: I have had luck in finding sympathetic statisticians to help me or at least to listen, again and again. Not that I can always bring the problem close enough to a mathematical formulation to enthuse them. It is that they see that something is there from the words I mumble. Allan Birnbaum was a wonderful help, patient and tolerant of statements that didn't hold with the full generality that I was claiming and often making some sense out of what was left. He did it again and again. Who else? I have talked as if my education consisted mainly of having my clients teach me. And that has often been the case. It shows in some papers. But I cannot say that all published work came directly out of problems associated with industry. That isn't what's happened. I have a long history of loafing which to make respectable one calls thinking, lying in bed or taking a walk and thinking, "Now what could be wrong with this part of the field?"

Half my contributions came from fantasy. I had no examples. I just said this might be worth looking into. The trend-free paper, for example, with Frank Wilcoxon [C. Daniel and F. Wilcoxon (1966). Factorial 2^{p-q} plans robust against linear and quadratic trends. *Technometrics* **8** 259–278] and the paper on parallel fractional replicates [Parallel fractional replicates. *Technometrics* **2** (1960) 263–268], which is unknown. Nobody has ever quoted the latter. I showed how to

FIG. 2. *Cuthbert Daniel, age about 5.*

use the same fractional design to estimate the effects on two or more different *responses*. So it is real multivariate design of experiments. Not just wiggling around in factor space so that you get an estimate of the effects of these factors on one response yield but on another one at the same time without conflict, it turns out. I give in that paper the conditions under which you can do that. There needn't be two, there can be more response variables.

Sometimes one just has a hunch. No client is necessary. For example: statisticians have a long record of deploring standard "one-at-a-time" experiments. So I tried to think of situations in which that would be the *right* thing to do. My results are in the Fisher Memorial Lecture for 1971. [One-at-a-time plans. *J. Amer. Statist. Assoc.* **68** (1973) 353–368.] I can detect no effect of this work whatsoever in any other statistician's or mathematician's thought. So now there are 13 more years of deploring. W. G. Cochran often said that it takes about 20 years for a good paper to have an effect. Perhaps I must hang on for another 7 years. I can, but will not, give other examples of works I thought useful which have had no visible effect. Nor do I see any essential difference between those that get some attention and those that were lost.

Tufte: I was thinking about the last few pages of your *Industrial Experiments* book. [*Applications of Statistics to Industrial Experimentation.* Wiley, New York, 1976.] It is very nice.

Daniel: Most people thought that was very unprofessional. I am glad to hear you differ. You are the first person to do so.

Tufte: Maybe it is suitable for exactly this occasion.

Daniel: It is more like that. When one gets through a book one doesn't have to pick up the English Book of Common Prayer to realize that "I have done a lot of things which should not have been done and I have not done some things which should have been done and there is no good in me." That's what that chapter is really about.

"MY EXPERIMENTS WERE 16 TIMES AS GOOD AS THEIRS"

Tufte: Could you describe a difficult problem with a client where statistics did make a difference?

Daniel: Yes. Let me describe the first experiment—I think the first in the literature, certainly the first in my experience—which was bivariate, had two responses. This is the paper with Riblett. [C. Daniel and E. W. Riblett (1954). A multifactor experiment. *Indust. Engin. Chem.* **46** 1465–1468.] Eight factors were thought to influence the two obvious properties of a catalyst—selectivity and yield. Kelogg had been working for many years on these platinum catalysts. They were big producers of the catalysts. They felt they might be missing something and should therefore try varying the actual production conditions a little to see which of them, if any, or any combination of them made an improvement in either selectivity or yield. Eight factors appeared to be worth testing, minimally first at two levels. So we had a two *response* situation. Two to the eighth, 256 trials, is too big; the trials were expensive. I found finally, and Riblett agreed, a 32-run fraction which would give us good estimates, that is to say with each main effect clear of all two-factor interactions. There were 28 possible two-factor interactions. One can't conceivably estimate them separately but we can estimate them in sets, and there are two or three in a set. Those experiments were carried through with very sad conclusions. We found only two main effects that could by the most charitable interpretation be called detectable. With 100% efficiency we were having effectively 16 full replications on every one of those factors and we failed to find effects. They had been running for years chasing one will-of-the-wisp after another, never two at a time, but always ending up with something quite close to where they started. Forever came they out by that same door wherein they went. They were sure they had it.

Tufte: Indeed they repeatedly confirmed their past good judgment.

Daniel: In doing it slowly enough, you don't find anything different. I was not sneering at them, but I could say that my experiments were 16 times as good as theirs. Part of the reason for that was that they didn't waste any time on doing any duplicates ever. "We've done that; what the hell do we want to do it again for?" They had no idea of their standard deviation. I remember the bafflement when I finally did produce an error estimate because I had plenty of degrees of freedom left over for error, not from replication but from higher order interactions. The error for selectivity was 1 or maybe .96 and one of the smarter men came to me and said, "I don't see how it always comes out one. Every time I look at a statistics book they say sigma equals one. How do they know that?" It was coming out that way for their data. They were regularly assuming that the standard deviation was on the order of 0.2. Now there must have been some experiments done in which they got a very good agreement, what you and I would call chemist's duplicates or bang-bang duplicates; if you do things close enough together and fast enough, you can get very good checks because nothing has had time to move; none of the uncontrolled factors has had time to change its impact so you get apparent small error. That is the reason, as you know, for doing experiments close together. If you can manage to use that, if you can vary some factor quickly, while you may lose on the effect of some other factor, you can get very small error by doing some things close together.

So that experiment was not an immediate success for industry. It was for me and it was for them in the long run since they now knew something about the error standard deviation, for runs of that type, doing them the way they were doing them. We went a little further and found out what are some of the components of that error. The chemical analyses were not close enough. You didn't have to do the whole experiment over to get a better result; you could analyze it repeatedly, chemically, and get a better result. So something was learned. They were not particularly satisfied. I don't think I could call them one of my most pleased clients; they had me there for 2 or 3 years. I got some respect, but not enough. And that was my fault more than theirs. I didn't have any way of getting closer to the company.

Sometimes things worked out very well indeed. The simplest one that worked out well was an experiment in extrapolation at Oak Ridge. One wanted to know the critical mass of the material one was making. But it is not a thing you test by going on both sides of the critical mass and seeing which one explodes. Such experiments don't work very well. You use up too much staff. Instead one adds to a nearly spherical mass small extra masses and watches how the neutron flux changes. But reasonably far away from the critical mass, of course. Something like $1/n$ plotted against flux turns out to give a straight line, but it is a straight line you really must extrapolate to get the critical mass. So I got into the problem of extrapolation quite early down there. I was too ignorant, perhaps fortunately so, to know anything about Wiener's work which was going ahead then. I stayed with the usual assumptions, independent observations, etc. and derived a formula which showed how, depending on whether the extrapolation was remote or close by, one either had to determine the slope of the straight line precisely or one had to determine the *height* at the near and most influential X-value. One of these designs is very skew; the other is very even. But one case fades smoothly into the other. The very simple design, or rather formula for allocation was used down there and seems to have been useful. With all the compartmentalization, mutual distrust and competition that were going on at that point I never learned the detail. "Very good" they said.

"INDUSTRIAL SECRECY WAS NOT A SERIOUS IMPEDIMENT"

Tufte: Another example of a difficult success?

Daniel: Yes, I've had a few of those. One has lasted about 15 years. The automatic blood measuring equipment that Technicon Instruments and other companies manufacture give nearly exact straight lines when the concentration of an analyte is plotted versus the voltage of a colorimeter or whatever they read; it is different for every analyte. Because of the machine's extreme sensitivity and versatility, one finds drift. Also in the fluid flow machines, because the same actual channel is used to pass one sample after another, there is carryover from one sample to the next. So one wants designs, i.e., allocation-sequences, which simultaneously measure and eliminate drift as well as carryover and also measure the linearity and *quadraticity* (a word not in most dictionaries; it's in mine—I have written it in). So one requires a schedule that measures four parameters—linear time drift, carryover, linear relation between the input and the output and curvature, if any. There is nothing that does that in the literature. At least I found nothing. We were able to make plans which give all four estimates with high efficiencies. By 1974 we had designs that were, at their worst, 97% efficient. That's since been pushed to 99.7%.

Most of the designs were published as the Youden Memorial Lecture for 1974. [Calibration designs for machines with carryover and draft. *J. Qual. Tech.* **7** (1975) 103–108.] One of the nice things about work at this time compared with work 30 to 40 years ago was that industrial secrecy was not a serious impediment. Technicon was glad to publish this design in the open literature, without, as far as I know, any active propaganda on anybody's part. These plans had very rapid spread. This happened partly through the government, too, which should be spoken well of here. The Food and Drug Administration saw the advantage of these plans immediately and now a large proportion—I can't say all—but a considerable proportion of the calibration results which they want to have, to see how the instrument works—how much drift, how much carryover, how do you allow for it and how straight is the calibration line—come from these designs. So that was a success, I guess. It took several years of real cooperation with fellow chemists and statisticians as well as with the people above me to get those designs straight. They had to see the advantage at every moment to allow chemists any computer time. I always checked final designs by hand. I wanted to get them exact. You know how that is done. If you have a matrix you want to invert, don't ever divide by anything, just multiply. And that, even in these little designs, takes you up into the ninth place. Nobody in their right mind would, I think, try to get 100% efficiency.

Tufte: Statisticians who did a great deal of hand calculation talk about its virtues.

Daniel: Because it slows you down.

Tufte: Do you believe that?

Daniel: Fisher said so. I think he said so because he had a relatively small machine and couldn't do otherwise. His was a multichannel mind anyway. I imagine that it is good for people with multichannel minds like Tukey's or Fisher's. You are thinking about lots of things and the more time the better. For me, even though I work on a per diem basis, I don't think that is true.

Tufte: Most of us are slowed down enough anyway.

Daniel: Yes. I was bedeviled by these and crawled through some of them in a spirit of annoyance. By 1974, I was able to use the computing equipment at Technicon by phone. I didn't program anything or even see the computer, I called up somebody and said I want this inverted and want the efficiencies for each of the estimates, and in half an hour Cathy Conley or some other dear girl would call me back and give them to me. All I needed was five numbers.

There's lots more to it of course. One has to have all kinds of control over the random variability which inevitably appears in these high speed machines. The machine is feeling its pulse all the time in 50 different respects, but some things get away from it. One of the commonest things of course is the aliquot that has been mispipetted, that sort of thing. The machine analyzes something like 25 components for something like 46 patients in 10 minutes, but there is still human input and even the four people watching it cannot be completely infallible. While bad results are rare, they are disastrous if you don't find them. So the program does more. How do you find various kinds of bad values? How do you check for common constant variance and so on?

Tufte: And didn't your consulting lead to your paper on the half-normal plot? [Use of half-normal plots in interpreting factorial two-level experiments. *Technometrics* **1** (1959) 311–341.]

Daniel: This was a *forced* discovery; I *had* to solve a problem of my own making. With Mavis Carroll's staunch help, I had persuaded cake-makers at General Foods to do a giant fractional replicate to settle in all generality a large number of questions about cake mixes and baking conditions. The lab result was a 256-trial set of data. About 100 were whole-plot effects. The largest contrasts were easy to spot but it seemed irresponsible to just christen the others as random error. (By the way, one of the *smallest* contrasts produced a controversy. It measured with unheard-of precision the average difference between cake flour, which is expensive, and ordinary flour. Every cake man knows that cake flour is better. Our failure to confirm this very nearly discredited the whole enterprise. G. F. had a whole mill devoted to bolting it. I did happen to notice, though, about 4 years later that the company had closed down their cake flour mill.)

First I tried plotting all the contrasts on a normal grid. Then I noticed that the contrasts with signs removed should have the distribution of the normal *range-of-pairs*, given in Hald's tables. I showed this to Allan Birnbaum who immediately said "That's the half-normal distribution." He showed me how to convert half of a normal grid into a half-normal one. We got a smooth curve but it did not quite go through the origin and it had a bend about half way up. The nonzero intercept meant a bad value and the bend meant some sort of nesting. So it turned out that the half-normal plot was quite informative.

I tried this on every 2^n set of data I could find. The plots were often nearly perfect, sometimes unexpected, and occasionally mysterious. The results were published in 1959 and came into wide use. It seems to me now that these grids are of little use for the 2^4, of considerable use for the 2^5, and are very informative for the 2^6.

Douglas Zahn published in *Technometrics* 1975 [**17** 184–211] a valuable analysis of the power of the grid

FIG. 3. *Cuthbert Daniel about 1940.*

using several methods of deciding which contrasts were improbably large.

"I DON'T LIKE TEACHING THE WAY MOST TEACHING IS DONE"

Tufte: Let's go back and discuss the other statisticians that were helpful to you.

Daniel: Yes, I've had wonderful luck here. W. G. Cochran took many hours to straighten me out on matters I was not straight on. Fred Mosteller has gone through manuscripts of mine over and over again. Oscar Kempthorne with his directness has been valuable in showing the shortcomings of the drafts and so on. Barry Margolin who did his doctoral thesis with me has been a lifelong statistical friend and helper. Hugh Fairfield Smith—no longer living alas—has been as direct as only one Scot could be to another in criticizing my work. Fortunately, I was able to thank him at length. John Tukey helped me many times, beyond count, directly and indirectly. Sometimes I feel that John must have me in mind when he is writing. He must say to himself, "There, even Cuthbert will understand this." In the 73-page article in the book *Design, Data, and Analysis* [ed. by C. L. Mallows, Wiley, New York 1987] he has taken several notions of mine and carried them very far indeed. There is, of course, much else in that article.

Let me talk a bit about another kind of cooperation, i.e., with engineers. At the research laboratories of American Oil, I had the luck to meet John Gorman and Fred Wood. They were only somewhat interested in experimentation but were continually confronted with masses of multifactor data already taken but never balanced, never randomized or even planned. Much of it was just a detailed record of the operation of a pilot plant. Analysis was of course by multiple regression. (I try not to use that term: It sounds too much like the name of a disease.) Very good computer programs for standard multiple regression were already available. But John and Fred knew that there must be many untested assumptions behind the printed results. Working together we listed, ranked and studied those assumptions. It turned out that most can be checked, or denied, or constructively modeled, by careful examination of the existing data. We wrote a book, *Fitting Equations to Data* [C. Daniel and F. S. Wood (1971), Wiley, New York], summarizing our results and their limitations. The original title was fitting equations to industrial data. But the unfortunate acronym moved us to change to the present title.

After Fred's development of partial residual plots, we had a second edition in 1980. Even though I keep telling people that it is now hopelessly out of date, it continues to be quoted. There must be something in it.

Tufte: This sounds like the University of Cuthbert here.

Daniel: That's the university I went to.

Tufte: I wanted to ask you about that. Most statisticians who publish work for universities. And you were quite an exception in having a major academic career so to speak but formally outside the academy. How did you work that out?

Daniel: It's a character defect of mine. I don't work well under close supervision. I don't work well in the early morning. I don't like teaching the way most teaching is done. I have refused to give people an A just because they're in a graduate course. I have a great many handicaps that keep me from being effective and well loved in universities. That's part of it. I have given courses at Berkeley and at Columbia, but I don't thrive well under the academic discipline. And since I was already 40 when I discovered statistics there was really no time and I was not about to go back to school and take elementary courses. I could read books. I am an experienced reader. So I read. It has grown to be an almost religious contention of mine that one learns by reading. Not any other way, except writing!

Tufte: Not by listening?

Daniel: Well, some people learn something by listening and I do too. There is proof that people do. But it isn't the common way nor the most useful way. One has to go back over things. If the lecture has more than one idea in it and has a great deal of content which one must master, a very small part of the present student body can get multiple points even out of a good lecture, by a good man, by listening to him once. They *have* to read. But if he's written it down very well and some have, then you know what he's about to say and you can go back over it and study it the way the Lord meant us to study, by reading. I don't think my mind is peculiar. I love a good lecture especially when I am about 95% ready for it anyway. What I cannot do is go to a lecture in a new part of the field, a wonderful lecture, and come away able to give a coherent outline of what was said. I cannot do it. And I don't think many others can either.

Tufte: What should universities do then?

Daniel: Learning is a two-way street. Learning is a dialogue. Learning is a continual probing by the teacher of what the student has retained and what he hasn't retained. And a continual probing by the student of what the teacher really meant. These cannot take place by standardized lectures followed by applause and a final exam. A teacher who gives a beautifully prepared lecture covering all the points in his curriculum usually feels he has done his job and that the rest is up to the student. He usually does not want to learn how little he has transmitted. I have done a lot of teaching and only a little evaluation because I do not like to do it either. It's discouraging to find out how little of what I have said has been retained for as much as an hour. And to discover what they have retained by the end of the term is not really practical. By then it's too late. In my judgment a proper course has to be permeated with dialogue not only in the sense that students can feel free to ask questions (which they almost never ask) but also in that the teacher takes pains to find out just how much got into the student's head. Even into the temporary memory.

The same is true the other way, of course. Even mature students do not want it made abundantly clear that they didn't get a quarter of what was said. And I believe that is often the case. Now taking time for evaluation and testing interferes disastrously with current coverage. I look at the scheduled curriculum of one of the most energetic teachers I know. He covers a grotesque amount but he doesn't take any time for tests. He does not have the time and it is not his problem to find out how much of it went in.

"ONE CLIENT REGULARLY HELD A 'WHAT DID HE SAY?' MEETING THE DAY AFTER I LEFT"

Tufte: Next topic. Shall we go back to publications and ideas that you especially treasure?

Daniel: Yes. But the way you cut me off on this business of quizzes and tests makes me think I should spend a little more time on it.

Tufte: No, you were ranting.

Daniel: I want to rant a little longer. What does one do about this? How can we be constructive about this? Of course less work can be covered in an elementary course if one is to take the time that is needed to give each student some time to express himself, to reduplicate a formula, to answer a simple question like "What is the fundamental assumption underlying the t-test," or to solve tiny problems. And we don't have time. We are all pressed for time. We have a curriculum and we are in competition we think with other people who have an even longer curriculum with even more detail in it. Those are the wrong people to be in competition with. We have to slow the pace at which we advance through the curriculum. Not just saying every few minutes, "Any questions?" and looking around the room ready with your horsewhip for anybody who asks questions. I haven't even taken up the question of stupid questions which are one of the hardest things to handle. One way to handle this, at least at the graduate or seminar level, is through a teaching assistant. Students are more at ease and are more likely to ask an assistant a question that reveals that they did not "get" some point. I remember one client who regularly held a "What did he say?" meeting the day after I left. The lecturer must of course make sure that the assistant understood the lecture! It is the student or auditor who has to get through the curriculum, not the teacher. Otherwise, to put it simply, the teacher doesn't know what he's doing. Now I will come down off that particular high horse and go to any high horse you say.

Tufte: Part of what you describe about the dialogue is like the British system of education. And some of the problems that you see here are because of mass education in America. One possibility is that there might be a dialogue, not just between teacher and student but, for mass education, a dialogue among students, so that some students are working with others. We just have too many bodies out there to have a tutorial dialogue.

Daniel: The system doesn't seem to me to match the British system in one important respect. If by the British system you mean the Cambridge tutorial. That's only for elite people anyway. Only for a tiny minority who know how to take notes or who have wonderful memories and who know the book and will read the whole thing through and be sure they understand it. Those are not the conditions under which teaching is done in this country and that doesn't depend primarily only on the volume of them. The fact that there are 60 million students or whatever there are makes this more of an issue, not less. It means that one has to teach *less*, but *better*. And one has to be able to demonstrate that, to justify the whole enterprise. The gigantic expense that goes into American education, not only the buildings and amenities, but into everything else, is not justified if all you have done is covered the subject.

Tufte: Now let's go back to your publications and ideas. Are there things that you particularly treasure that we haven't discussed?

Daniel: Yes, a few. Some of them apparently I am treasuring quite alone. These are buried treasures in the sense that while I've written on them I don't see any response to them. I don't feel hurt, I'm largely resistant to external criticism. If somebody says "don't publish that," or "that's not any good," I require strong proof.

As an example of a lost gem, take the issue of "one-at-a-time" plans. It seemed to me that there must be some experimenters who have only *one* piece of equipment, one working system. And they, or at least one of them, might be interested in the logical consequences of carrying out trials in some particular order. Well, I am, so far, wrong. Evidently no one who reads the *Journal of the American Statistical Association* is interested in this. I hope not many are as *dis*interested as the referee who wrote "If this is the sort of thing the Fisher Lecture is to become, then we should consider discontinuing the series."

I do not want to go through the details of this again. As we say in New York—it stands written. Clearly it does not interest statisticians. I must seek a different audience or readership. Perhaps an ad in the *New York Review of Books* would do it. Even so simple an experience as (1), a, ab, abc, bc, c, which will measure the effects of A, B, C, free of two-factor interactions is not acceptable. Perhaps the requirement of a fixed order, without randomization is too obnoxious.

"SOME WILL FEEL THAT IT'S JUST ANOTHER STATISTICAL CHEAT"

Tufte: Any other treasures?

Daniel: Yes. Trend-free designs. Experimenters in many areas have to work with a system that is drifting slowly. Whether it be a lactating cow or an aging catalyst, he must study his problem (i.e., vary his environmental conditions) in a drifting system. Most design statisticians have preferred to randomize the time order of sequential trials, so as to include the drift in the error estimates. But some experimenters, working with a familiar system, may prefer to fit an equation with one or two terms for linear and quadratic trends rather than allocating drift to error.

To take the simplest case, for a single two-level factor, and a system with linear *and* quadratic drift, only one order of varying the factor is possible, viz. high, low, high, low (or, of course, its reverse).

Frank Wilcoxon noticed that the particular sequence 01101001 for one factor, or even (1), a, b, ab, a, b, ab for two factors, A and B, permits the elimination of linear and quadratic trend from estimates of A, B, and AB. He and I showed the extension of this up to $N = 32$ for 14 factors, but again, I do not see any application of these plans by others.

Daniel: I have a few points on exposition that I skipped on this question of how to teach. May I come back to them now?

Tufte: Yes, that's fine. Yes.

Daniel: I have looked at only a few dozen elementary texts. There's a certain similarity to the way they introduce the subject to the statistically naive student. I would make a different suggestion. Either one starts with the best of the examples of the kind that are given in the books edited by Tanur and Mosteller, or they should start with examples that are so simple that they don't strain the attention span of a man who is spending the rest of his life looking at television. Each can be made interesting and plausible and unexpected in 5 to 15 minutes. I can give a few examples of extremely simple experimental situations which are bound to interest, I should say, *may* interest people who will not take a month to learn about expected variances and so on. The simplest is the earliest. It's Hotelling's example of how to weight two objects on a two-pan balance in only two weighings as precisely as if each had been weighed twice by itself. It only occurred to me a little while ago that this works just as well if you wish to measure the length of two hard objects assuming they have nicely polished ends and that you know where the ends are. Every statistician knows the answer, but most others do not. Do we have time to tell my tale about Fritz Lipmann and Severo Ochoa? I found myself by luck seated at a neighbor's picnic in the country where I live (my neighbor's name happens to be Fritz Lipmann, Nobelist, Biochemistry, 1953) between him and Severo Ochoa, also a Nobelist. I am saying nothing. Of course, under those conditions one doesn't try to control the conversation but finally in politeness Ochoa turns to me (and looking down at me, he's about three feet over me) and says, "Well, what do you do, Mr. Daniel?" My usual answer in the country is that I am just here for local color but this time I said, talking very fast—well I am just a man who tells people how if they have two objects they want to weigh in a two-pan balance, etc.—There was a silence so I knew I had his attention. He said, "don't tell me" and a few seconds later "let's see, you could weigh them both, oh, sure, and then you could put one of them on the other side and balance them out" and then he turned to me as if he had just seen me and said, "Well, that's a very good idea." On the other side of me is Fritz who says "Why would you want to weigh it more precisely?" So you can't win them all even with a two-pan balance. But it's my belief that one does not need to be a Nobelist to see the point of this. It's one of those things that gets better as it gets bigger, in the sense that four weighings can be done four times as well, etc. So that is a simple example and doesn't take 15 minutes. Almost anybody can understand it; it's something one hadn't thought of and that will meet some resistance, but many will be interested in it. Some will feel that it's just another statistical cheat.

"SIMPLE POINTS GET LOST IN THE REFINEMENT AND THE RIGOR"

Tufte: I am reminded of your other simple example, but at a higher level, the question you asked about the *t*-test.

Daniel: A little different because I an now riding the hobby of examples that are so simple that they can hardly fail to interest somebody. Thus the first lecture doesn't end up with expectations. What else? A trend-free plan. They're absolute necessities in some parts of the world. At Texas A&M you cannot fail to interest your audience if you tell them how the experiments are done in studying the effects of some new feed on the production of a milk cow. She is of course fresh and producing milk, but her yield tapers off in time so that you can't just give her one kind of food for a month and then give her the other kind. Her milk yield is dropping off anyway. Even if you are only studying the effect on yield let alone quality, you can't do it. How do you find out, how do you get around that inevitable trend? The minimal way is very simple indeed. After you have gotten through the second period you go back to the first feed and have her take that again. You take the difference between the average of the two ends and the middle and you have a much clearer picture of what the effects of the feed were in the face of a linear trend. Simple example. Anybody can see what is happening. And examples of drift can be adduced in almost any field. It was Grant Wernimont who pointed out to me that this may be valuable even if you have very good equipment. But if you don't have very good equipment this enables you to spend your time not getting the drift out of your equipment, but getting the drift out of your results.

Tufte: That's a very good point. Shall we do the *t*-test?

Daniel: At one of our major universities, talking with the head of the department, taking up the time between 11:30 and 12:00 when you are waiting to go to lunch and you can't think of anything but lunch, I asked him what he would say was the prime assumption underlying the *t*-test. Well he said normality and cons—no, it isn't that, what is it, oh, yes, oh, yes, it's independence, statistical independence, sure. He was annoyed though because he hadn't seen it instantly. So when one after another of his colleagues came in getting ready for lunch, he asked them, "What's the basic assumption underlying the *t*-test?" Well, normality and so on. They all failed it until the one woman on the faculty, a junior woman, came in and was asked the same question and said instantly, "statistical independence." The head of the department recovered rapidly and we had a good enough lunch, but this is not the kind of thing that gets emphasized so again it is one of these simple points although it doesn't come in the class of these little one-at-a-time experiments. Simple points get lost in the refinement and the rigor and the extensive time spent on making statistics respectable for mathematicians rather than on making it interesting for human beings.

Tufte: Tell me your future plans, Cuthbert.

Daniel: Well my future plans are to go ahead as I went before, waiting for things to turn up. I have one client or maybe one-and-a-half clients. They keep supplying me with interesting small problems. They haven't been supplying me with anything major since the carryover plans. Except one thing. There seems to be no entry in the *Cumulative Index* under *Weighted Designs* where you know beforehand that the precision cannot be the same at all the conditions that you propose to study. In fact there may be a range of conditions with the precision of a result varying by a factor of several hundred. How do you make a plan so that you can estimate your parameters best even though some of the observations have a thousand or maybe only thirty times the weight of others. I ask that at the moment as a rhetorical question. I hope you don't have the answer because I don't, and I want to think about it. I have some answers; I got some yesterday. Now all the ideas of balance don't seem to work. Things become quite different, in that new results are counter-intuitive. I really have nothing else on my mind except my private life which is very active and promising right now. I have a feeling that other things will turn up shortly. But that's the only one that I can be explicit about. I would be glad if anyone wants to precede me in this weighted design affair. It's intriguing but very slow. Some powerful mathematician may get general results quicker than I will get very particular results. That's happened before.

I want to thank you, formally, Edward, for having been so enterprising and so patient in these sessions.