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Miscellaneous Reminiscences

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1. JOHN TUKEY

My first encounter with a statistician was memorable for me even though neither of us was a statistician at the time. It occurred in my first year of graduate work 1935–36 at Brown University. In a course in complex variables there was John Tukey, the youngest member of the class by two or three years, giving the professor fits by pointing out that his proofs did not quite hold water and explaining how they could be fixed up. I was accustomed to being at the head of the class and it came as quite a shock to me that I could be outclassed by a classmate—and such a young one. I was a physicist in those days. Several other physics students and I complained to the Mathematics department that this esoteric course was too unrelated to our needs. That was a mistake. The department moved us out of the frying pan into the fire of a Russian czar, J. D. Tamarkin, who literally swamped us with applications of complex variables to physical problems.

My next encounter with John, a fast mover, occurred at Princeton when he was one of the faculty members on my oral exam committee. I know of no other person who can concentrate on two things at once or who can read as rapidly as John; he reads at about the same rate as he can turn the pages. When I was supervising a large survey of educational institutions while I was at the U. S. Office of Education, I asked John to be on the advisory committee. He came to the first meeting with a suitcase full of books and plowed through several of them during the course of the meeting while participating fully in the proceedings. Afterward a committee member said to me "I wonder what that fellow was looking for thumbing through all those books?" Before explaining, I suggested that maybe John was trying to find a $50 bill he had hidden one of those books. The inquirer thought that was a pretty good guess.

2. SAM WILKS AND PRINCETON

My statistical career began accidentally when I applied to the Princeton Mathematics department in 1938 for student assistance after serving as an instructor of Applied Mathematics for a couple of years at the University of Texas. I had interrupted my graduate studies to marry the lovely Harriet Harper and had switched from physics to teaching mathematics because that was the best paying job I could find. Most of us were opportunists in those depression years; as an undergraduate I was a Chemistry major until the moment the Physics department offered me a student assistantship. The only assistantship available at Princeton was with Sam Wilks; I took it without really intending to specialize in statistics but was soon persuaded that this was a very promising field and a most interesting one because of its roots in probability.

Sam was a slave driver. Fred Mosteller has remarked on several occasions that Sam turned him into a workaholic. Certainly that happened to me; I was fairly easygoing until I fell into Sam's clutches. The trouble was that Sam never did anything but work and it was clear that his students were supposed to follow suit. In any case, it would have been uncomfortable for us to take an afternoon off for tennis or an evening off for a movie when we knew that Sam was toiling away while we were indulging ourselves. But he didn't even give us a chance to feel uncomfortable. If he couldn't find us at work at home or on the campus, he went into action telephoning around town and firing off telegrams if the telelphone failed him. Of course, it was possible that he was just worried that some disaster had befallen us, but we didn't think so. Two years of that treatment ingrains the habit and you are ruined for life.

Sam was a true mathematician in that he always strove for elegance in proofs and was always most careful about details—a perfectionist. One unfortunate result was that his beautiful book (Wilks, 1962) on mathematical statistics was published about 20 years too late. A reasonably complete version was ready in 1942 and issued in lithographed form in 1943 for limited distribution. Sam kept tinkering with it year after year; then Harald Cramér published his book (Cramér, 1946) which covered much the same ground. So Sam decided to make his much more comprehensive and tinkered with it another 15 years. Cramér's book enjoyed great prestige—something Sam's book could have had if he had been a little less concerned about impeccabillty.

But that is a minor matter. Sam's greatest accomplishment, even greater than his ingenious research or his development of *The Annals of Mathematical Statistics,* consisted of his students. The leaders of many of our best statistical departments and laboratories were launched by Sam Wilks. His research and steady stream of outstanding students brought him many nice offers from other universities but as far as I know he never considered leaving Princeton which had offered him a position in the depths of the depression when he badly needed one.

Sam's professional interests were very broad. He promoted the application of statistics in many fields and the cause of statistics generally by serving on myriad commissions and committees and by accepting practically all invitations to lecture—even to school children. His professional life seemed to be almost his entire life; I never heard anything from him about any other subject.

Personally he was a very pleasant person—quite friendly and without an iota of affectation. He regarded his graduate students as absolutely his equals socially; that was refreshing as we were accustomed to having professors consider themselves a notch above barely subsisting graduate students. It helped that Sam was not much older than we were. Another thing I particularly liked about Sam was that he, like me, was strictly a utilitarian. We often enjoyed inveighing in chorus against pure mathematicians. We had both been exposed to one of the worst of the species, R. L. Moore, at the University of Texas who was the favorite target of our scorn. R. L. was an elitist who regarded himself as an artist vastly elevated above the mundane affairs of the workaday world. There were two mathematics departments at Texas then: Pure Math and Applied Math. An enterprising dean once suggested that, in the interest of administrative efficiency, the two departments should be combined. R. L., who was something of a power on the campus, quickly quashed that idea by asking, "Why mix wine with dishwater?" I happened to be part of the dishwater at the time.

The fact that Sam was editor of the *Annals* contributed greatly to our education. We were his handiest referees and did practically all of the refereeing as I recall. I got the impression that Sam loved to reject manuscripts because the more I marked them up the more lavishly he praised my work. Of course that inspired me to pore over the offerings of these unfortunate authors letter by letter. Later it occurred to me that Sam was merely insuring that I didn't do a careless job of refereeing.

Sam was extremely conscientious about doing what was expected of him and that led him astray in one respect. Before World War II there was a widespread prejudice in academia against employing Jewish faculty members. In those days when Sam was writing a recommendation for a Jewish student he mentioned that the student was a Jew because he was expected to do so. He himself was not antisemitic and in fact believed that Jews were superior students; he was happy to get them. If it had been pointed out to him that there was something antisemitic about reporting the religion of these students, he would have recognized that there was and that might have put him in a bit of a quandry. Fortunately the quandry would not have lasted long because the prejudice evaporated rapidly after the war.

One thing that made statistics appear to be a promising line of work was the fact that Sam was in such demand as a consultant. He was continually off to New York or Philadelphia or Washington, especially Washington, where several government agencies kept him knee deep in work. Perhaps his conscientiousness did not permit him to turn down these demands on his time. He became absent-minded at an early age also, perhaps because his head was so full of other things. When I was working in Washington two years after graduating Sam would call me up now and then for lunch. On more than one occasion he had forgotten to go to the bank and needed to borrow 10 dollars for cab fare to the airport. On one occasion he came to the lunch in a cab because couldn't find the car he had rented for the day.

Sam's early students became my lifelong friends. His first student was Joseph Daly who was already at Princeton when Sam arrived. Joe went on to a fine career at the Bureau of the Census. Harriet and I rented the Daly home in Washington for a couple of years while Joe was on Navy duty. George Brown and I were the first students that Sam recruited, and we have been like brothers ever since even, to the extent of moving from one job to another together. Sam's next two students, Fred Mosteller and Will (with Eva) Dixon, arrived during my second year; all became marvelous friends. Will and I collaborated now and then on a paper. Later three other of his students, Ted Harris, Mel Peisakoff and John Walsh became good friends at the RAND Corporation.

John Williams, though not a statistics major, was one of our circle. He, Fred Mosteller, George Brown and I enjoyed frequent Saturday night bridge games. John was an extremely bright, somewhat self-indulgent student interested in astronomy, particularly meteors, which is why he wanted to know something about statistics. He had no intention of getting a degree; I don't believe he even had a bachelor's degree; he was merely taking courses that attracted him. He came from a well-to-do family and was the envy of all of us impecunious graduate students with his elegant Packard sedan and always a large roll of bills in his pocket. He was good company with a sharp sense of humor and a wickedly realistic view of the world.

3. R. A. FISHER AND MY THESIS

I had a most unhappy encounter at long distance with R. A Fisher while at Princeton. I had chosen as a thesis topic determination of the sampling distribution of the roots of the second moment matrix although Sam had warned me that Fisher had been working on the problem for years. Sure enough no sooner had I derived the distribution than the *Annals of Eugenics* issue arrived containing the papers by Hsu and Fisher detailing *my* thesis. The disaster was even worse than I thought. Sam had been delighted that I had worked this out but his demeanor suddenly changed. He said that it was most important that I have a publishable thesis and that I had better get to work on another problem. He suggested the distribution of runs which luckily turned out to be a very straightforward problem so that my graduation was not delayed.

Twelve years later, thanks to a brief conversation with Hotelling, I did get a short publication out of the first thesis. He was commiserating with me about my bad luck and I told him I was disappointed that Fisher and Hsu had omitted their derivation of the normalizing constant for the distribution because I had much more trouble with that than with the functional form. In fact, I never quite got it and had to get help in carrying out a complicated induction from Barkley Rosser who was spending a postdoctoral year at Princeton. Hotelling urged me to publish my derivation of the constant and armed with that recommendation I took the idea to Sam who agreed to put it in *The Annals of Mathematical Statistics.* But it was not my convoluted derivation; I had not made any record of Rosser's helpful insight and I couldn't recall it so I had to get help again—this time from John Nash, who cleverly sidestepped the whole induction by regarding the integers as variables and sending them to revealing limits.

4. WORLD WAR II

At the beginning of the second World War, I was recruited by the Office of Price Administration to help develop levels at which prices should be fixed. The task was immediately contracted to the Bureau of Labor Statistics and I was transferred there. One of my most valued colleagues there was Seymour Jablon, a Wald student, who later became the foremost expert on radiation damage to humans as a result of his years of tracking the survivors of Hiroshima and Nagasaki and their descendants for the National Academy of Sciences.

About halfway through the war, Sam Wilks was given a sizable contract by the National Defense Research Council to provide statistical assistance to the armed forces. He called on his students to join him in this effort and most of them came. They included Will Dixon, Ted Anderson, Phil McCarthy, David Votaw and me in Princeton. There was a New York branch of the project which included Fred Mosteller, John Williams, Jimmie Savage and Cecil Hastings. George Brown was already in Princeton on another military project directed by Merrill Flood who also had John Tukey and Charlie Winsor on his staff.

Sam recruited also for his project Bill Cochran and Cochran's student, R. L. Anderson. The Cochrans (four) and Moods (three) arrived in Princeton at about the same time when there was a stringent housing shortage. There was available, though, a big place with six bedrooms, and resourceful Sam put both families in it. There was ample room; all rooms were huge; we used the butler's pantry for a dining room. With all that floor space the furnace had a massive appetite for coal; Betty and Harriet were frequently stuck with feeding it as Bill and I traveled. The single kitchen might have have posed a problem but the families turned out to be completely congenial.

Those military projects were my first experiences with real applied statistics—actually gathering some data and trying to make something out of it. I was delighted that my first report nailed down the issue it was supposed to settle. Soon after it was distributed Charlie Winsor and John Tukey came to my office with a copy in hand and proceeded to tell me how much they liked it but—. I was surprised at how much improvement was possible and immediately issued a revised edition. The main difficulty, as nearly as I can recall now, was that I had run headlong in the direction the data pointed. Charlie was an old hand at dealing with data and knew the pitfalls of that course; judicious hedging was called for all along the way.

Those calamitous war years were nevertheless rewarding ones for the statisticians around Princeton; we were hard at work on interesting problems that seemed to be of real and immediate practical consequence. I will recall one that we had to do by simulation. Knowing the distribution of bomber aiming errors and the distance at which a bomb was $P\%$ certain of detonating a mine, how many bombs must be dropped on a mine field of given dimensions to be $Q\%$ certain of clearing a path through it F feet wide? We could think of no way to attack it except to draw scads of randomly located circles on sheets of paper until at last the required path appeared. People who could do that sort of work were extremely difficult to come by during the war. R. L. Anderson saved the day by persuading two of his sisters who were teaching school in Indiana to spend a summer at this tedious work. A modern day computer would have been a godsend for that one.

Occasionally I visited the New York branch of Sam's project at Columbia. Another military project directed by Allen Wallis was in the same building and we sometimes attended seminars given by that group.

There I met for the first time such luminaries as Allen himself, Harold Hotelling, Abraham Wald, Albert Bowker, Milton Friedman, Abraham Girshick, Harold Freeman and Jacob Wolfowitz. It was on this project that Wald carried out his elegant development of sequential analysis. Allen has written most interestingly about this remarkable group and the origins of sequential analysis (Wallis, 1980).

The Cochran-Mood household held open house every Sunday afternoon where statisticians could gather to have tea, work on the New York Times crossword puzzle, play chess and my favorite game, kriegspiel, an inferential form of chess in which a player doesn't know the position of his opponent's pieces and must attempt to locate them by trying to make impossible moves. John Tukey and Charlie Winsor with his wife Agnes were our most regular guests. Charlie made several nice contributions to statistics, but I expect his greatest was his role in helping to move John from mathematics to statistics. Charlie was extremely widely read—a true Renaissance man; he knew something about everything. Among the occasional guests at the teas were two of my sisters-in-law who lived nearby in New York City. They were attending the lectures of P. D. Ouspensky at the time and had fallen head over heels for him. He was a Russian philosopher who took some of the ideas of astrology and reincarnation seriously. One afternoon the sisters wondered whether Charlie had read Ouspensky's book *Tertium Organum.* Charlie admitted acquaintance with it. What did he think of it? He thought it was utter balderdash. In trying to probe further they found that Charlie had not gotten beyond page one. Naturally they were outraged that such a blanket condemnation was based on no knowledge of the book. "Well," said Charlie, "How much of a quart of sour milk do you have to drink?"

To me Cochran was Bill. Willie was okay in the U. K. but over here it was a child's name and he soon dropped it in favor of Bill. Those who knew him when he first came to this country stuck with Willie. I think I changed one of Bill's work habits during those years. When a new problem came in he would go poking around the library to see whether he could turn up something related to it, whereas I would sit down and go right to work on it. Inevitably I would get somewhere with it before he would and he soon decided those library searches were not paying off. He was a delightful person with a ready laconic wit—a real pleasure to work with and associate with even at the rate of 24 hours a day seven days a week. He was most happy with his new homeland where he was just as good as the next person. Occasionally he would say jokingly, "The trouble with this country is that you don't know who your betters are." He would have been hard put to find any betters anywhere.

5. IOWA STATE UNIVERSITY

Bill had been turning down offers from John Hopkins because he didn't want to leave Iowa State in the lurch. At the end of the war, he easily persuaded me to come with him to Ames assuming that when I was well settled there he could leave in good conscience. When I arrived in 1945 at the Stat Lab, as it was known, I found myself in the alps of applied statistics in the United States. There were plant breeders, animal breeders, food researchers, crop estimators, psychologists and engineers using elaborate experimental designs (such as incomplete block designs embedded in lattices) routinely. It was rewarding to see that all those mathematical statistics journals distributed around the world were not just building castles in the air. Researchers from other agricultural colleges came to Ames in droves to learn how to upgrade their experimental work.

It was here that I soon learned that applied statistics is more difficult than mathematical statistics. Abstractions are unfailingly well behaved; data seldom are. The Stat Lab was the creation of George W. Snedecor who almost singlehandedly kept the United States abreast of developments in applied statistics. He was a tall, spare, modest, matter-of-fact man with an ever-inquiring mind and an insatiable appetite for helping others. He was so busy with that that he never did much for himself. We know him in writing only through his textbook, *Statistical Methods*, and that was written for his students; he expected nothing from it as a publication of The Iowa State University Press. Over the years it slowly and deservedly became a best seller, now in its eighth edition, with William G. Cochran. Cochran had become a coauthor in a previous edition when Mr. Snedecor asked him to write the chapter on sampling.

Mr. Snedecor (he was never called anything else) became involved with statistics in the early 1920's when Henry A. Wallace of the Pioneer Hi-Bred Corn Company (later Vice-President of the U. S.) came to the college seeking help in analyzing data from the company's program to develop hybrid seed corn. Mr. Snedecor, a young man in the mathematics department, decided to give it a try and found his life's work. He soon discovered Rothamstead and that became his fountain of knowledge. He studied Rothamstead's publications; he wangled money from the administration to invite Rothamstead people to Ames for lecture series (among them R. A. Fisher and Frank Yates); much later he lured Bill Cochran and Oscar Kempthorne to the Stat Lab. Thus, while Britain with R. A. Fisher was blazing the trail, the United States kept up to date thanks to Mr. Snedecor.

Besides the design and analysis of experiments, another major activity of the Stat Lab was survey sampling. The Department of Agriculture and particularly the Bureau of Agricultural Economics were continually seeking data about farms, crop yields, advance estimates of harvests, impact of farm programs on the farm economy, the viability of that economy and so on. True to form, Mr. Snedecor became an expert in sampling by his own efforts and by bringing experts from abroad to help; among them were P. C. Mahalanobis and P. V. Sukhatme from India which was also in the vanguard of the development of applied statistics thanks to its intellectual connection to Britain. When I arrived in Ames, the resident sampling experts were Arnold King and Ray Jessen. There was a large sampling project going on under the direction of Jessen, who had developed the idea of area sampling as a way to introduce true randomness into the sampling of human and geographical populations. He was thus able to calculate correct measures of sampling error for such surveys. Ray says the idea was not original with him, but he took it and ran with it to make it what is now the standard sampling procedure used by the Bureau of the Census and the Department of Agriculture as well as the corresponding agencies throughout the world.

Another star of the Stat Lab was Paul Homeyer, the perfect consultant. He was a friendly, gregarious, outgoing man who immediately immersed himself in whatever problem was brought to him to the extent that he often became a collaborator before he or the researcher realized because he took such a great interest in what the researcher was doing. Paul knew experimental designs backwards and forwards and had an intuition about analyzing the data that baffled me. I couldn't partition the sums of squares of these complicated designs without first working through the algebra. Paul just sat down in front of a Monroe calculator and reeled them off; he had a complete understanding of exactly what the design was supposed to be doing. The same talent made him an excellent bridge player.

Some years later in 1955 when I founded a consulting organization, I lured Paul away from Ames as soon as I could afford it; that is, as soon as I could land a contract that would support him. The contract was with the U. S. Army Chemical Corps at the Dugway Proving Ground. Dugway soon found itself depending more and more on Paul; the contract grew in scope every year; it was not long before we had to establish a branch office in Salt Lake City to handle this thriving business.

After I had been at Ames for a year, Bill Cochran moved to Johns Hopkins and that made it possible for me to bring my good friend George Brown to Ames as his replacement. Mr. Snedecor, though, was not altogether sure that these two mathematicians were just what he needed. He went back to his perennial source of knowledge and expertise to bring Oscar Kempthorne to Ames from Rothamstead. Kempy really was a marvelous replacement for Cochran—he knew the theory as well as George and I did and he was extensively experienced in applied statistics—just what Ames needed. Not only that, he was an expert in genetics which made him all the more valuable to Ames. Oddly enough, Kempy's arrival simplified a decision for me; a year later John Williams persuaded George and me to move to the RAND Corporation; I could not have abandoned Ames if Kempy had not been there.

The Stat Lab was a particularly prestigious unit of the campus. In the first place, it was much valued by the other departments because their researchers were confident that they were using the most efficient experimental designs thanks to their ready access to the Lab's experts and the generous cooperation of those experts. Furthermore, they didn't have to analyze their data unless they wanted to. The Lab maintained a computation section consisting of a room full of Monroe and Marchant calculators manned by people familiar with analyzing data, inverting matrices, partitioning sums of squares, conjuring up formulas for supplying missing data and so on. The section was a handy way, incidentally, to provide employment for graduate students.

In the second place, the Stat Lab brought a significant amount of contract money to the campus. It was the favorite place for the Department of Agriculture to carry out work related to crop estimation and the advancement of technology at agricultural experimental stations. The Bureau of the Census depended on the Stat Lab to assist its gathering of agricultural statistics and later surveys of the human population. When I arrived there Mina Rees, the angel of mathematics at the Office of Naval Research, was persuaded to allocate money to the Lab for research and the support of graduate students.

In the third place, Stat Lab personnel were world travelers. When some less developed nation asked for help in setting up a national census or a program of agricultural research, the U. S. State Department knew where to go for help. As a result, Lab people were regularly going off on 6 or 12 month tours of duty in the most pleasant of circumstances as VIPs with the full and enthusiastic support of the government in power.

How many departmental seminars regularly draw professors from other departments? The Stat Lab seminars were attended by Jay Lush, head of the plant breeding department, John Gowen, head of the genetics department, several plant breeders from the agricultural experiment station and occasional visitors from the engineering experiment station, the psychology department and the home economics department.

It was by far the most cosmopolitan regular seminar I have ever attended.

Statistics courses were taught in the Mathematics department by Stat Lab people who also had appointments in the Mathematics department. The department was totally cooperative and ready to go along with whatever the Lab suggested in the way of courses or personnel but, for reasons that don't make much sense to me now, I thought that we should have a separate Statistics department. No one in the Lab or the department objected, so I undertook to plod though the administrative procedures for getting a new department set up. This waste of time was much lengthier than I expected. There was never any opposition; it was just that much documentation was necessary and many people and committees had to sign off on it. One required piece of documentation was a list of the courses we planned to teach and a description of each. I became carried away with this assignment and put together what I am certain was the most comprehensive statistical curriculum ever seen on the planet. My reasoning was that while I was going through all this rigamarole I might as well anticipate everything and avoid going through it again. I would be surprised if half of those courses were ever taught. Arnold King became the first head of the new department.

One badly needed course at Ames was one that grounded our first year graduate students in the basic theory of statistics. I undertook to provide it in the first year of the new department. There being no adequate textbook and being the workaholic that Sam made me, I resolved to write one and parcel it out to the students section by section during the year. Of course, I had to stay a few pages ahead of the class and the work was naturally done at the last minute, so the book was mainly written between 9:00 p.m. and 2:00 a.m. on Monday, Wednesday and Friday so the secretary could type up mimeograph stencils and run off copies on Tuesday, Thursday and Saturday. My recollection is that dreaming up the problems required more time than writing the text. My first year at the Stat Lab in the milieu of applied statistics made it a very different book from what it would have been otherwise; I think that influence was a major factor in its success.

The next year a second expanded edition was produced in about the same way. It was much improved thanks to an excellent student in the first class, H. D. Block, who found all manner of glitches in the first edition and who worked through all the problems to make many suggestions for improvement. He went on to become a fine mathematician. About this time, I heard that Paul Hoel had written a similar textbook for Wiley. While that was discouraging I went ahead and sent my manuscript to McGraw-Hill as planned, even though I doubted that the book could amount to anything. My estimate of the gross sales potential was the worst I ever made; it was off by a factor of a hundred. I counted five or six statistics departments in the United States at the time, with perhaps five or six graduate students each for a gross potential of perhaps 35 copies. But Hoel's book would be ahead of mine and likely monopolize the sales. So I was a little surprised and quite pleased when McGraw-Hill decided to publish it. However their representative said the manuscript would have to be typed (I had sent them a bundle of mimeographed sheets), and when I asked who would pay for that he told me that would be my responsibility. Immediately I was able to declare that I preferred to drop the whole project; it was evident to me that several years of royalties would be needed to recoup the $200 or so that the typing job would cost. "Gee," he said, "you're a tough negotiator. All right we'll pay for the typing." That is when I inadvertantly learned the secret of negotiation. I later discovered that this representative was the president of the book company. After the book had been in print several years McGraw-Hill began pushing me to revise it. I couldn't get very excited about that project because the book was performing far beyond my wildest dreams and, in any case, I was extremely busy getting my firm under way. Finally they cited clauses in the contract and I found a coauthor to do the revision. However, months went by with no perceptible progress, so McGraw-Hill found another coauthor who was willing to get right to work on it. Thus I can claim no credit for the fine coauthor, Franklin Graybill, who kept the book up to date.

6. THE RAND CORPORATION

John Williams spent the war years as a dollar-a-year scientist with the military and the National Defense Research Council. Afterwards he was influential in persuading the Air Force and particularly General Hap Arnold to create the RAND Corporation. The argument was that warfare was becoming more and more scientific, and therefore that the military needed first-rate scientists. But it would be hard to recruit them for strictly military work, and the civil service pay scale was too low to attract them anyway. The solution was to form a nonprofit research corporation which was founded on the basis of the promise of large continuing contracts with the Air Force and the Atomic Energy Commission. The Ford Foundation donated one million dollars for working capital.

John became head of the RAND Mathematics department and at once urged many of his old associates to join him. George Brown and I were among those who did; George became head of the computer division where he immediately set about building for RAND a duplicate of what we called the Johnniac, a computer John von Neumann had built in Princeton and which was by far the most advanced computer of the day. RAND's copy was used mainly for Monte Carlo studies of nuclear radiation problems. I became John's deputy. He tried hard to persuade Fred Mosteller to come but Fred could not be dislodged from Harvard; I think that was one of the greatest disappointments of John's life. He was also very disappointed that Jimmie Savage would not come either. Both Fred and Jimmie did spend a few summers at RAND.

In one of those summers Jimmie learned that George was a believer in personal probabilities. That struck Jimmie as a very unscientific notion and he was at some pains to straighten George out on the matter. Of course Jimmie wound up being converted himself with the result that we have his *Foundations* (Savage, 1954). Strangely, the book does not mention George at all, but George probably didn't notice; it was always his habit to broadcast his ideas freely as simply another mode of scientific communication. Any graduate student who chose George as a thesis adviser had it made; whenever he or she encountered a roadblock George was right there with a way around it and on the spot; he had a very quick mind for sizing up a problem.

As John's deputy, I was kept fairly busy with nonscientific work because John didn't care much for administration. For example, he didn't trust airplanes, so I made the briefing trips and the recruiting trips; his recruiting instruction was "Just ask the department head who their brightest student is and make him an offer." It mattered not that the prospect's field might be topology or number theory; the important thing was to get quick learners. The offers often failed to land the man even though RAND's salaries were considerably more generous than academia offered; Ph.D.s in mathematics were often dedicated to an academic career. But I had no difficulty in filling out slots with the best of people; I have already noted that we enlisted three Wilks students. One of them, Mel Peisakoff, was pretty much diverted from a statistical career but John Walsh and Ted Harris were not. Ted was recently honored by becoming a member of the National Academy of Sciences. George Brown remained a statistician all his life, but his RAND experience and some earlier work with von Neumann led him to put his major emphasis on computer science. His contributions to both fields were much larger than the written record shows because he preferred talking to writing. Some of our other stars at RAND were Richard Bellman, J. C. C. McKinsey, Abe Girshick, Merrill Flood, Sam Karlin and Olaf Helmer.

One year John abandoned his administrative duties altogether in order to write a book for laymen about game theory (Williams, 1954) which enjoyed considerable success. In my copy he wrote, "For Alex who did my work while I did this." I didn't get any credit for doing his work later while he pursued the most astonishing project in RAND's history. It is omitted from the roster of RAND accomplishments. John loved fast automobiles, especially his Jaguar roadster. But why not make it still faster? Why not double the horsepower by putting a Cadillac motor in it? Cadillac and Jaguar mechanics assured him it was impossible but he was not easily separated from this captivating idea. After making some careful measurements of the motor and the car he decided it could be done and that he would do it himself. RAND had a well equipped machine shop; all he needed to do was make a shorter drive shaft, cut some new gears for a slightly smaller gear box, trim the clutch down a bit, reinforce the frame a bit and presto—the sizzlingest Jaguar in the world. He did it. There seemed to be no end of unforeseen difficulties and there were the usual beginner's errors on the machine tools, so that the project wound up taking about a year of full time work in the machine shop, but he showed what a stubborn and able person can do if he sets his mind to it. RAND's president grumbled to me about it several times, but so far as I know he never grumbled to John, who was not an easy man to argue with.

7. GENERAL ANALYSIS CORPORATION

After seven years at RAND (1948–1955), I decided to go into competition with it and mortgaged my home to the hilt to found the General Analysis Corporation. George Brown, who had meanwhile moved to UCLA to head a new computer center subsidized by IBM, gave me all the support he could without actually giving up his professorship. The firm's business eventually consisted primarily of two sizable ongoing projects: one with the U. S. Army Signal Corps to evaluate communications systems, which was directed by Lester Ford, Jr., and one with the U. S. Army Chemical Corps to evaluated chemical agents and weapons, which was directed by Paul Homeyer. There were a variety of one year contracts, mainly with government agencies; Sam Wilks was helpful in our getting a contract with the National Security Agency. Besides George, Paul and me, our statisticians included Mel Peisakoff, Ray Jessen, Ray Mickey, Paul Sommerville, Don King (son of Arnold King), Scott Krane, Robert White, John Penquist and James Yarnold full time, together with Will Dixon, Al Bowker, Herbert Solomon and Harry Romig as consultants—a distinguished group that I was most proud of.

The firm was a great success technically, but less so as a business because it lacked a hard-nosed manager who would let people go when there was no contract to support them. I could not do that because these people were my friends. I worked hard at trying to match the onset of a new contract with the ending of another but failed too often, with the result that we carried people too long without work. That ate up the profits and enlarged the overhead. There came a time when one of our two major contracting officers disallowed a large chunk of overhead wiping out our slender working capital. It was at moments such as this that I wished that I had settled permanently in that statistical oasis at Ames, Iowa.

Only days after this disaster, a white knight appeared in the form of Herbert Robinson, president of CEIR, Inc., who wanted to buy the company and made a generous offer for it. He had a dream of setting up a nationwide computer network and operating it as a public utility. Our choice was either to sell or embark on an uncertain quest for new capital which would substantially reduce our equity; all the stock at the time was owned by employees. We decided to take the bird in the hand and thus ceased to be an independent entity just five years after starting out. That was a big disappointment to some of us, but it was the only way to go.

8. U. S. OFFICE OF EDUCATION

My last big statistical project was with the U. S. Office of Education; Francis Keppel, its commissioner, asked me in 1965 to join the office to organize the National Center for Educational Statistics by bringing together miscellaneous statistical activities scattered through the Office. A second task was to bring a computer to the Office which was distributing large sums of money each year in various programs and keeping track of it by hand. Keppel thought there ought to be some punch cards or something involved in all this accounting. Then I had my own agenda which was to develop some production functions relating school inputs to educational outcomes. Mr. Keppel thought this was a fine idea, but of course the other two tasks had the priority at the moment.

At the end of the first year, my only accomplishment was to discover that I was never going to be able to make programmers and systems people out of my pencil operators. So I went to the Bureau of the Budget with my sad story and asked for fifty new positions. They gave them to me without batting an eye. Everyone in Washington howls about the way the Bureau slashes their budgets. Not I. They always gave me whatever I asked for.

Also at the end of the first year I was handed a third job by Keppel; Congress had appropriated a million dollars for a survey of United States schools and colleges in connection with the Civil Rights Acts of 1964. The main purpose was to determine how minorities were faring. It had languished for a year in another division leaving me only one year to carry it out. So I frantically telephoned a number of survey statisticians to see if one of them would drop everything and come take charge of this project. None would. But Phil Hauser suggested that James Coleman, a professor of sociology at nearby Johns Hopkins, would likely be interested in doing the study and wouldn't have to move. Coleman immediately agreed to take on the job.

Meanwhile I had been suggesting to Keppel that the survey include student achievement tests to get some data for my production functions, which I saw coming to life much sooner than I expected. He was doubtful saying the state and city school officers would resist it and might be driven to refusing to cooperate with the survey. But I had another argument. Senator Robert Kennedy had informed us that he expected us to carry out a good evaluation of the effect of all the Title I money we were beginning to distribute to disadvantaged school districts. Surely he would not be satisfied to learn simply that more teachers and equipment had been put into school buildings; we needed to show him that the students were learning more; the survey was a golden opportunity to get some baseline data. Keppel decided to go with it and was magnificent; the school officials did resist strongly and he had to use all his persuasive powers to bring them around.

Problems with the school officers redoubled when Coleman's sociological questions were added to the survey. That was going too far to pry into family secrets via the children. We had to whittle them back, but we got some in thanks to Keppel. In the end, those districts that wouldn't cooperate cited those questions—not the achievement tests as the reason. Despite these problems we finished the job on time: designed the questionaires, drew the samples, reached 570,000 students and 60,000 teachers and 4000 schools, gathered the data, analyzed the data, wrote the report and delivered the printed report all in one year—a remarkable accomplishment thanks mainly to the Herculean efforts of James Coleman. Fred Mosteller told me that as of that time this survey was the second largest social science study ever undertaken.

The first regression coefficients we cranked out showed us early in the game that these coefficients were going to be pitifully small and erratic. So I proposed that we forget about them altogether and lump our variables together into a few meaningful groups and calculate what each group did to the variance. Of course, that put us in the partition of variance quandry in which the first group gets most of the credit, but I had dreamed up the unique and common parts idea which I thought was original. Later I learned that two Englishmen (Newton and Spurrell, 1967) had proposed the same thing at about the same time or earlier. In any case, we dropped regression coefficients entirely and presented our results in terms of fractions of variance explained; fortunately they turned out to be somewhat consistent.

In retrospect, I was lucky that all the statisticians turned me down and I wound up with a sociologist. My thoughts about production functions included only school, teacher and principal attributes as independent variables. Without Coleman, we would not have put in the family and peer variables which turned out to be far more important.

Our results were published in a fat government report (Coleman et al., 1966) which stirred up quite a lot of criticism among educators because the primary finding was that schools and teachers did not seem to have as nearly much effect on educational achievement as the attitudes of parents and fellow students toward education. A happy consequence of the controversy was that Fred Mosteller and Daniel Patrick Moynihan were stimulated to set up a faculty seminar at Harvard to assess the survey and its implications; the seminar resulted in a very learned review (Mosteller and Moynihan, 1972) which generally supported the findings of the survey.

I thank Morris DeGroot for his flattering invitation to write this piece and close with another bit of evidence that wildly improbable events do sometimes occur. A Chinese family in Taiwan owns an electronics factory there which has a contract with the Pentagon. In 1987 a contracting officer went there to negotiate an extension of the contract. During the small talk before the negotiations began, a beautiful member of the family seated next to him was telling him about the game of go and how complicated it was. But he was not unacquainted with the game. He told her that he had once worked for a fellow who played go but he was a high-powered mathematician so the complications probably didn't bother him. Whereupon she bowled him over by saying, "It wasn't Alex Mood was it?"

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