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A Conversation with Wilfrid J. Dixon

Nancy Flournoy

Abstract. Born on December 13, 1915, in Portland, Oregon, Wilfrid J. Dixon recognized very early in his childhood that numbers fascinated him. One of three sons, he knew it would be his responsibility to finance any advanced studies. So, with his parents' encouragement, he sold magazines and delivered papers from age 8 to finance his college education. Dixon received his B.A. in mathematics from Oregon State College in 1938, his M.A. in mathematics from the University of Wisconsin in 1939, and his Ph.D. in mathematical statistics from Princeton in 1944 under Samuel S. Wilks. He was on the faculty at the University of Oklahoma (1942–1943), the University of Oregon (1946–1955) and the University of California at Los Angeles (1955–Emeritus since 1986). During 1940–1942 and 1944–1945 he was an operations analyst at Princeton University and on Guam for the Office of Scientific Research and Development.

At UCLA, Dixon had a joint appointment in the Department of Preventive Medicine in the School of Medicine and in the Biostatistics Division in the School of Public Health. He was the first tenured statistician in each of these schools. In addition, Dixon initiated the Biostatistics Division, started its graduate program and served as its first Chief. He organized the Department of Biomathematics in the School of Medicine and served as chair of this department from its inception in 1967 until 1974. In 1973 he was appointed Professor of Psychiatry. As a member of the U.S.–U.S.S.R. Joint Working Group on Computer Software (1974–1980), Dixon served as liaison to the Kolmogorov Laboratory at the University of Moscow.

Many of his over 120 publications result from long-term collaborations in pharmacology, physiology, surgery, neurology, cytology and psychiatry. His commitment to statistical consulting, coupled with his idea to parameterize computer programs in 1960, led to the development of one of the first general statistical software packages, BMD, Biomedical Computer Programs, that has evolved into BMDP Statistical Software.

Dixon organized the Statistical Computing Sections of both the American Statistical Association and the International Statistical Institute. He made major contributions to nonparametric statistics, serial correlation, adaptive (up-and-down) experimental designs, robust statistics and the analysis of incomplete data. He is a Fellow of the American Statistical Association, the Institute of Mathematical Statistics, the Royal Statistical Society and the American Association for the Advancement of Science and received the ASA's 1992 Wilks Medal. While at the University of Oregon (1951), Dixon coauthored with Frank Massey a first-of-its-kind statistical textbook for nonmathemeticians that sold over 300,000 copies.

EARLY DAYS AT OREGON STATE AND WISCONSIN

Flournoy: Hello Will. I'd like to start by saying that I am very pleased to be able to conduct this interview today, and I thought we'd start with some of the early times. I understand that you received your Bachelor's degree in Oregon?

Dixon: Yes, at Oregon State.

Flournoy: What was happening at Oregon State at that time?

Dixon: It was the bottom of the depression, so I went to Oregon State for economic reasons. There were

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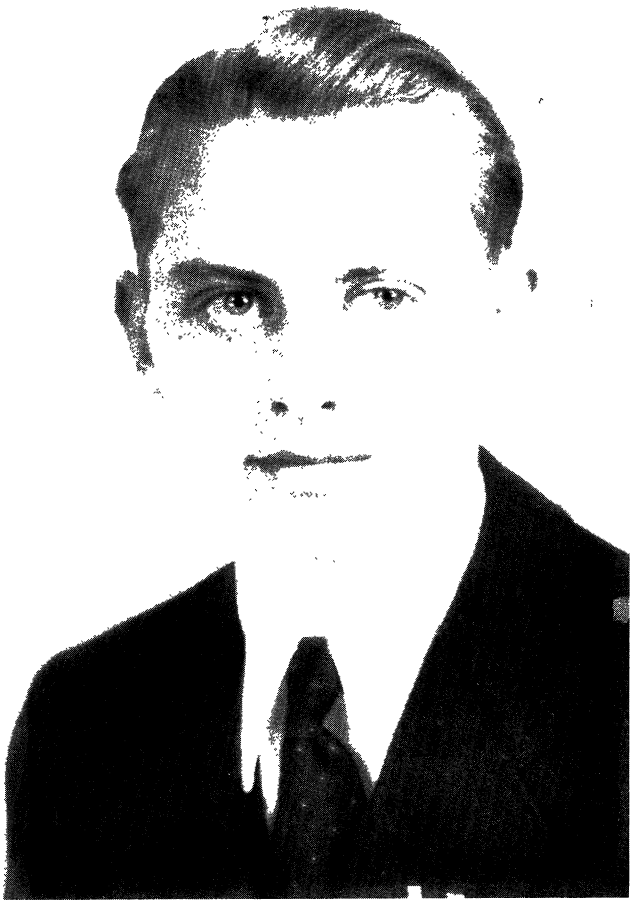


FIG. 1. *Will Dixon in 1933.*

about 2000 students; it was not a large school. With the money I had saved since childhood plus a $150 scholarship given to me by the principal upon graduation from Jefferson High School in Portland for use at one of the Oregon schools, I chose Oregon State. My older brother had gone to UC Berkeley.

Flournoy: Were you exposed to statistics at that time?

Dixon: Yes. Paul Hoel joined the faculty at Oregon State in my last year or two. I took a statistics course from him out of what is referred to as Sam Wilks' Orange Notes. They gave a basic survey of what was known mathematically at that time, but were put together in a very integrated way. It was the sort of thing that Wilks should have published, but he was always refining it, trying to perfect it. He was very concerned about being mathematically acceptable.

Flournoy: Was this an undergraduate text? What kind of material was in it?

Dixon: Wilks' notes were not elementary. I would describe them as being of intermediate mathematical level, but no text with similar material existed. We were able to cover the material quite well at the senior level with mathematics as it was developed in the '30s. The level of mathematics demanded of mathematics majors at that time was less than it is today.

Flournoy: After your graduation in 1938, you went to Wisconsin. What led you there?

Dixon: I was accepted for graduate school by Brown, Minnesota and Princeton as well, but Wisconsin's fellowship required no teaching, and so allowed me to progress more rapidly to my degree. The reason I didn't go to Princeton right away was because Paul Hoel felt that Oregon State did not provide sufficient mathematical preparation; I would need to take too big a step. He was probably right because at Wisconsin I got a very firm foundation from a number of very capable mathematicians, and in particular, I got the chance to learn statistics from Churchill Eisenhart. He was on the faculty, spending at least half his time handling the statistical consulting at the agricultural experiment station on campus. So I not only had early training from Paul Hoel, but I had quite a different training from Churchill Eisenhart, who had studied in England. Churchill's father was Dean of Princeton's Graduate School and had been head of the mathematics department. He encouraged his son to study statistics in London.

Flournoy: Did Eisenhart's father think of statistics as proper training at that time?

Dixon: Dean Eisenhart was supportive of statistics at Princeton. Churchill taught me a great deal about applications. I think my interest began to increase then, because he gave me indications of the breadth and depth of problems that could be solved. When I talked to him about giving a course the second semester, he told me he didn't think he could because the mathematics department felt that teaching statistics was all right as long as it didn't interfere with his standard teaching load of mathematics.

Flournoy: So he actually had to volunteer additional course time in order to teach statistics?

Dixon: Yes. I don't recall who was chair at that time. Of the other faculty, William Langer impressed me most. I was able to get a Master's degree and take a full set of solid courses: algebra, real and complex analysis and statistics. Churchill returned to Princeton the next year and then went to Washington D.C. to the National Bureau of Standards where his talents were appreciated.

GRADUATE STUDENT LIFE AT PRINCETON

Dixon: I reapplied to Princeton and Iowa State, and was accepted by both. I informed the faculty, and Langer, who was my advisor, said "You were accepted at Princeton? That's an awfully good school." I don't know whether it was because I showed an interest in

statistics that he thought I wouldn't be qualified for Princeton, but he did wish me well. I accepted the Princeton offer, although I did have some serious interest in going to Iowa State to work with Gerhardt Tintner, who was a leading economic time series researcher of the period.

Flournoy: Did Iowa State have a statistics department by then?

Dixon: As long as George Snedecor was there, there was a laboratory or department. He had been there ten or fifteen years before that time. However, I went to Princeton on a fellowship provided by Sam Wilks. Wilks expected us to work for our fellowships.

Flournoy: Did you go specifically to work with Wilks?

Dixon: That's right.

Flournoy: What sort of course work did you have to take for a degree in mathematics?

Dixon: Princeton required no specific course work for the Ph.D.

Flournoy: Just the dissertation?

Dixon: Yes, and a very thorough written and oral general examination that lasted several days. It was the responsibility of the student to present himself for the examinations when he thought he was prepared. Failures of these exams were frequent and there were even occasional failures of the "final" exam. There was a certain amount of washout at Princeton. There were no restrictions, such as years of residency, on when to take an exam, and because there were no specific requirements, sometimes someone would take these exams before he was ready. I took Wilks' statistics courses. I also took an algebra course, but I learned much more freom reading the text than I did from the course. There was a Calculus of Variations course that I took from Tommy Tompkins, who later moved to UCLA. John Tukey at that time was not a statistician; he taught topology. His degree from Brown was in chemistry and he then shifted to topology.

Flournoy: So while you were attending Princeton, Tukey, Wilks and Tompkins were on the faculty.

Dixon: Yes. Other principal faculty were Solomon Bochner, Solomon Lefschetz and Albert Tucker. Further, since Princeton's Fine Hall was coexistent with the people at the Institute for Advanced Study, all had their tea there. Included among the Institute visitors there were Albert Einstein, John von Neumann and others. At one of these teas Paul Halmos saw a group of statisticians talking to each other and commented on how all these statisticians were spending all their time investigating a very restricted set, that is, a set with total measure one. Which, of course, is true.

Flournoy: Halmos would probably say the same thing today.

Dixon: I did proofreading for Sam Wilks because he had taken over the editorship of the Annals of Mathematical Statistics. There weren't many mathematically oriented papers in the Journal of the American Statistical Association (JASA) at that time. It was a very applied journal. The Annals was expected to be more mathematical. However, it was, in general, less mathematical than JASA is now, which I believe was good. The extensive proofreading finally allowed me to see errors on a page immediately and automatically. Sam saw to it that papers were proofread before they went to the typesetters, proofread afterwards, proofread in page proof and so on. Inheriting that responsibility, Fred Mosteller, Ted Anderson and Phil McCarthy were also drafted into proofreading service.

Flournoy: So you were students with Fred Mosteller, Ted Anderson and Phil McCarthy?

Dixon: Yes, and also with Alex Mood, George Brown and John Williams, who were there when I arrived. John left before completing his Ph.D. and started the statistical activity at Rand when it was first organized. Charles Winsor, Henry Scheffé and Bill Cochran were also resident working on research programs.

Flournoy: Wasn't R. L. Anderson there too?

Dixon: R.L. came in later when there was a research project, after receiving his degree at Iowa State. His thesis was on the same problem of serial correlations on which I worked. I would guess that the ideas for R.L.'s thesis came from Gerhardt Tintner, but I think Bill Cochran was his thesis supervisor. Others at Princeton were Mel Peisakoff, Ted Harris, David Votaw, Richard Bellman and Leo Goodman.

Flournoy: Tell us about Sam Wilks.

Dixon: He didn't seem any older than we students. Rather he seemed very young. He wasn't comfortable with the formal Princeton social scene. He was from Texas. Although he had been trained in England, he was still a Texas boy. He was very friendly and outgoing, but he rarely engaged in the social milieu at Princeton. Instead, he enjoyed inviting graduate students to his house. He kept an "open" refrigerator filled with an assortment of drinks and we would spend hours sitting round and talking.

I was married at the time, and he invited us to go out to the beach with him. He would take his boy and the two of us, and we would head out to a beach south of Atlantic City. And then when he would go on trips, he would have me tend his furnace for him to see that the coal kept burning when it was cold in the winter. He didn't want the house to get cold and freeze the pipes. It was a very close relationship. I don't know whether the others were as close.

The formality at Princeton was truly impressive and took some adjustment. I know Sam's background in Texas was no preparation for the high teas organized at the Graduate School by Mrs. Luther Eisenhart. It

took me a while to learn the protocol of a formal tea, that is, to learn who sat at which end of the table and when. The lady of highest status could be determined by noting who was serving tea at five o'clock; second in status was serving coffee at five. Number three served tea at four while number four served coffee. This is probably a British tradition. When I was at Princeton I learned some of the rules; I'm not sure I can quote them correctly. The soirée would last for two hours.

Flournoy: Did you have graduate student housing? Were you all together in some physical location?

Dixon: No, I was married and married students were not permitted to live in university housing.

Flournoy: You mentioned that statistics did very well in the mathematics department. We don't think of statistics as being big at Princeton today. How did things change?

Dixon: There were university fellowships that Wilks could allot. I don't know how Sam achieved such status, why he was recruited and put on an equal level with the mathematicians. But Fine Hall had corner offices that were for senior faculty. Sam had a corner office as did Solomon Lefschetz and other "prominent" professors of the time. Sam seemed to be able to draw his fraction of the graduate appointments. I don't know if he brought in a large amount of additional funds. He did for me. He had to find that money elsewhere because the university did not award fellowships to married students. I may have been funded by the Educational Testing Service.

Flournoy: What was the reason for this ruling about married students.

Dixon: It was a male school, and I think it was thought that anyone who was married couldn't possibly be spending their total energies on their studies. This was probably another British tradition. I don't know whether there were rules at the source of the funds that made the restriction. There well may have been.

Flournoy: So what would happen if a student married while they were studying. Would they lose their fellowship?

Dixon: Yes. The fellowships also qualified the student for housing in the graduate dormitory. Princeton had purchased a number of apartment houses off campus for University staff and we were able to arrange for that. Being off-campus, I was hard to reach. Sam always wanted to be doing things rapidly, efficiently; "Let's get on with it." His agitation would move from foot shaking to reaching for the telephone. But telephones were expensive in those days, and at first, I didn't have a telephone. So he would send me telegrams. Alex Mood in his Miscellaneous Reminiscences (1990, *Statistical Science* 5) referred to this and other anecdotes about Sam.

Flournoy: Telegrams across town!

Dixon: Yes, if he wanted me to come over to the office before 5:00 p.m. and I wasn't around the office at 2:00 p.m., then he'd call the telegraph office, and the telegraph boy would come around a half hour later and give me the telegram: "I want you to get over here to the office before 5:00."

Flournoy: He would have loved fax machines.

Dixon: Wouldn't he have!

Flournoy: Your dissertation was on serial correlations. Would you give some details?

Dixon: Yes. Sam gave me a paper to write before that because I don't think he thought anyone should start on their thesis until they had published something. So he would pose smaller problems to the graduate students to see how they would do. I think it had two functions. Not only was it a test of the capabilities of the graduate students, but also, I think a good many graduate students get mired down if their first study is too big and complicated. They get so tired of the topic that they don't ever publish their thesis. I think it is a good device, if it can be done. The problem Wilks gave me was a nonparametric test for the equality of two distributions (1940, *The Annals of Mathematical Statistics* 11 199–204). Abraham Wald and Jack Wolfowitz at Columbia University were working on this problem at the same time (1940, *The Annals of Mathematical Statistics* 11 147–162). Their results have received a great deal of attention, but I think my solution has advantages over theirs.

The thesis topic came later. I think, in part, the problem arose because Tjalling Koopmans was also at Princeton. (He later received a Nobel Prize for his research.) He had just arrived from Holland and was spending a year at Princeton. He was a combination statistician, mathematician and economist. He interacted with Wilks on his own research, and he interested Wilks on the time series problem.

It turned out that von Neumann was working on it too, which scared the hell out of me. I didn't want to lose my thesis topic. I assumed that if von Neumann was working on a problem, he would solve it in the next six weeks, if not in the next week, whereas it would take me six months or a year to solve it. But he went down a different route. His problem arose from the Aberdeen Proving Grounds where he had been called in to work on mathematical problems associated with weaponry control: How do you operate with artillery, in terms of estimating the drift over time? As the artillery warms up, the trajectory drifts, and you may need to estimate the corrections needed to maintain accuracy. He was working on the problem of the distribution of the mean square successive difference. The square of the difference includes the cross-product of successive observations, so the problem can be made essentially the same as studying the serial correlation.

You have the same information. I was working on the correlation and he was working on the difference. But the end terms in the sum differ and so the problem is slightly different. I discussed the two approaches to the problem with von Neumann. He said he was seeking the precise answer based on the differences. This made it clear that we were working on different parts of the problem. So I relaxed.

Flournoy: Were there others at Princeton who influenced your life?

Dixon: Yes. John Tukey, Charles Winsor and William Cochran had a lot of influence on me. One example concerned a difficult point that I hadn't completely resolved in my thesis. I was convinced that I had a true statement but the mathematical proof needed sharpening. I showed it to Cochran, and he said "Well that looks like something that Laplace wrote and so let's get out his book on Celestial Mechanics (P. S. Laplace, *Traité de Mecanique Celeste*, 2, Paragraph 21) and look at it." We did, and there was the development that I needed to finish my thesis. I was impressed that Cochran had this sort of information at his finger tips.

Flournoy: Is there anything else you can think of about Princeton that impressed you?

Dixon: Before leaving Princeton, I worked on a weapons control research project run by Merrill Flood. We also had a project studying how you could organize school transportation in West Virginia. But I didn't spend much time on the latter except to help introduce them to computer activities.

Flournoy: So you had computer activities at Princeton at that time?

Dixon: Yes, even before I went to Oklahoma.

Flournoy: What form did these computer activities take?

Dixon: This weapons control work was to find the altitude of an airplane using theodolites. Theodolites are instruments trained on an object that measure the angles of altitude and azimuth producing numerical output. If a plane is tracked by two theodolites placed a given distance apart, their data can be used to estimate the location of the plane. We repeatedly solve the set of triangles, plot the course and construct a statistical trip in three-dimensional space. The individual computation was simple triangulation. The military were computing a tremendous number of these flight tracks at Fort Monroe. For analog computation they used a barracks floor about 40 feet square. The triangles were constructed in miniature and the results were measured with a tape measure. They were getting an analog solution for the altitude.

We decided it would be much better to have a computer solution. The angles were measured in mils, and since they weren't looking up the trigonometric functions but just solving them in analog form they didn't need the tables. So we obtained the Army tables in mils for the trigonometric functions and decided that we could do it by computer. There was an IBM 600 at that time that had keypunch card feed and keypunch card output. The machine would chew on a problem for a little while after it was read in, and punch the answers out. Since the triangulation problem involved only products and extensions, a machine built for accounting would perform these functions. It was a machine that Western Electric had built to do their electrical computations using complex numbers. IBM modified the machine for making the extensions done in accounting. It was a remarkable machine for its time. It could look up prices from a second card file, and this ability to reference a file was just what we needed. We would take an angle, look up its sine, cosine and tangent and do some multiplications.

To verify our keypunching of the table we computed sine-squared plus cosine-squared to compare with 1.0 and then multiplied the cosine by the tangent to obtain the sine, a few check computations like that. Some of these did not check and the difficulty was more than errors in keypunching. The Army's table had many errors, not just errors in the fourth or fifth place, but some in the second place. Throughout World War I, the Army had been using this table with such errors. We then corrected the entries.

Flournoy: Did you let the Army know?

Dixon: No, I don't think we did. Doing the computations was very slow because the machine would do only five or six multiplications per minute. But it was faster and more accurate than doing them by hand. So, that is how I got started in computers.

Flournoy: Was this computer available for any person working at Princeton?

Dixon: No, we used the machine in the IBM Service Bureau in Trenton to demonstrate that it could work. We then leased a machine for our project. The project was located in Princeton but not a part of Princeton University. We had a keypunch, a verifier and a collator.

Flournoy: This was about 1940?

Dixon: Yes, 1941 and 1942, before I went to Oklahoma. That was the extent of computing at that time. von Neumann was designing computers then, and John Mauchly frequently visited Princeton. He was a developer of the Univac in Philadelphia.

TO THE UNIVERSITY OF OKLAHOMA AND BACK TO PRINCETON

Flournoy: Where did you go from Princeton?

Dixon: You may understand better where I went if you understand my background. I don't know how much of it stems from coming from the blue collar or white collar, or whatever collar background. My father

was a clerk who never earned as much as one hundred dollars a month with a family of three boys. I didn't have people to help form reasonable goals in life.

Flournoy: We call them role models today.

Dixon: Yes. I thought the postman had a better job than my father, and so I thought I'd be a postman. Then I thought high school teachers were doing better than postmen, so I had that goal. Since it required more training I decided to go to college. I took practice teaching my first year or two of college. I enjoyed mathematics and economics and since school was easy I decided to finish college. The next step was to round out my education in graduate school. After all, you get a degree, find a teaching job somewhere, and try to move up the salary range to better support a family. By then I not only was married, but I had a child. I guess the best teaching job available at that time was at Oklahoma. I had no notion that I was starting down a path where I would be as good as I could be. I had limited goals, always. But I did finish my thesis while teaching at the University of Oklahoma in Norman. Later, returning to Princeton for war-time research, I began to feel that there were other goals in life too.

Flournoy: How did your return to Princeton come about?

Dixon: I asked to be re-recruited because I was being inducted into the army. I got my draft notice, and I asked the draft board if they didn't think I would be better doing research rather than drafted.

Flournoy: If it hadn't been for research opportunities during the war, you might still be teaching at Oklahoma.

Dixon: Yes, or some other school. It was a major turning point. I had close work with Alex Mood, R. L. Anderson and Bill Cochran, and even Al Tucker was involved with a project. He'd been chairman of the mathematics department at Princeton at that time.

Flournoy: Did the fact that the war was going on alter the atmosphere at Princeton?

Dixon: I don't know that it did. Wilks had a lot of status anyway, and by bringing in federal research money the group was a little larger. But I don't know that the war increased his status.

Flournoy: I was wondering about feelings of excitement in terms of statistics. Was there a group involvement or did you work one on one with Wilks.

Dixon: Wilks was it, although Tukey would soon become a statistician through weapons control work. I was at Princeton working with this group and Sam saw that there was a need for a statistician in Washington. I was chosen. Wilks loaned me to the Joint Army-Navy Target Group that was doing statistical analysis of Japanese commerce and manufacturing. Charles Hitch headed several research divisions of this group in Washington; he later became President of the University of California system. One of his divisions was a mathematical division headed by Carl Allendoerfer, who later became chairman of the mathematics department at the University of Washington. I worked with Allendoerfer for a while, and then I was assigned to continue the work in Guam.

Flournoy: What did you do there?

THE 20TH AIR FORCE IN GUAM

Dixon: The 20th Air Force in Guam (under General Curtis LeMay) asked for special kinds of assistance from Washington and wanted their target analysis to be done in Guam rather than in Washington. They requested my transfer to Guam, and I started working for Princeton University out in Guam. I worked on the reliability of aerial photos and their relationship to economic analyses. Much of Japanese industry was done in private homes and in backyards, and you could tell pretty well what they were doing from aerial photographs. I also worked on the analysis of Nagasaki's commerce and on the reliability of economic estimates in cases where they had photographic verification.

Flournoy: Were you in Guam when the war ended?

Dixon: I was there five or six months before the war ended. Then I came back to the job at Oklahoma for a year and was recruited from there to the University of Oregon. I had strong feelings about going back to Oregon because that was where I had grown up, and I had ties to the state. The Joint Target Group had recruited Carl Kossack, who was then in Oregon, and when he knew that he was not going back to Oregon, he had suggested me. Kossack went on to Georgia and was in the Department of Statistics there for many years.

Flournoy: So Kossack went from Oregon to Washington to Georgia, and you went from Guam to Oklahoma to Oregon.

Dixon: You remind me of decisive moments, and how one thing led to another. There was a very interesting pivotal point at the end of the war. I suffered from sleep deprivation and developed some nerve damage; it got so that I could hardly walk. Because of this I couldn't stay with the Statistical Research Group that went to Tokyo to do some studies for the occupying command. Without this medical problem, I no doubt would have spent a year in Tokyo and followed a different professional path. Or perhaps ended up somewhere else, not Oregon. That was a turning point. Once back in the United States with the cool weather, I regained my capacity to walk.

Flournoy: Was the sleep deprivation quite extreme?

Dixon: Yes it was. The military camp was noisy until 11 or 12 o'clock at night and the sun was up at 4:30. With the sun beating on a tent, it was no place to sleep. When you don't get enough sleep your nerves or tissues may swell and pinch the nerves and interfere

with the nerve transmission. Getting more rest and a cooler environment solved the problem. It was amazing to me. A young physician in the Guam headquarters, who didn't have much to do, plotted out the whole system on my body as to what nerves would work for hot and cold and what nerves would work for muscle twitch. He drew diagrams on my body. He would say the difficulty is right in this spot on your spine between this vertebra and that vertebra. So I learned what the situation was.

THE UNIVERSITY OF OREGON AND THE DIXON-MASSEY TEXTBOOK

Flournoy: After the war you moved to Oregon. Who were the other statisticians at Oregon?

Dixon: When I came to Eugene there were no other statisticians, so I attempted to recruit someone immediately. I tried to hire Frank J. Massey, Jr. who was graduating from Berkeley, but he had accepted a job at Maryland. I continued to recruit him and the next year he came to Oregon.

Flournoy: How did you come to know Massey at Berkeley?

Dixon: I didn't know him at Berkeley, but I asked Jerzy Neyman who his students were. I had talked to Neyman earlier, but I don't think he thought Oregon was a place for his best students. I got a list of his doctoral students and I corresponded with one or two of them in addition to Massey. We were the two statisticians at Oregon, and we started to train students. Three of the graduate students are shown with Frank Massey in a 1950 picture and four more students in another picture taken in 1952. Other students were Pearl Van Natta, Howard Nielson, Hjalmar Anderson, Richard Link and Earl Yost.

Flournoy: Tell me about the department at Oregon.

Dixon: Oregon had recruited Andrew Moursund, and to build up the mathematics department, he recruited three new faculty. In the second post-war year, he apparently had enough funds approved to bring in three new faculty, me, Paul Civin and Ivan Niven. Both Civin and Niven had outstanding research records, and so we became a little group trying to build a graduate program. We were given fairly free rein to do it. Yet, once again, my belief that statistics should be a full-scale field of considerable breadth using mathematics only as its language became a point of contention. Traditionalists were of the opinion that statistics was a narrow nonessential subset of mathematics. It was similar to the situation at Wisconsin: you could have graduate students in statistics so long as they accepted the rule that statistics was an add-on to the regular mathematics graduate program.

In some cases, the difficulties were so intense that some students who disagreed were, in my opinion, persecuted. That bothered me deeply. For example, we would go into a doctoral exam and these students would be quizzed by the mathematicians, who seemed determined to demonstrate that the choice of statistics for a major implied second rate mathematics ability. It was a humiliation that was painful to watch. It wasn't a matter of ensuring that a proper level of sophistication was required of someone holding the Ph.D. degree. It was specific to the statisticians. I believe that, as a result, a number of students were not allowed to achieve their full capacity. It concerned me and when I encouraged the students to deal with it the best way they could and to try to live through it, some could manage it, some couldn't. Unfortunately, we are dealing with a disease that affects certain mathematicians.

Flournoy: So you taught a full mathematics course load as well as a statistics course load? Or were you just teaching statistics at that time?

Dixon: No, at Oregon there was allowance for the statistics courses. The difficulty was with the students in their degree programs. For example, the chair wanted to build the department and he was willing to take a student studying statistics on the same level as a student studying algebra, as far as building his program was concerned. However, his quota was only two statisticians for the faculty. So the more statistics courses we taught, the more basis he would have to add another faculty member who was not in statistics. I tried to build a Statistical Laboratory to get the campus much more involved, but met with increased resistance and additional obstacles and I decided to leave.

Flournoy: By statistical laboratory, do you mean a consulting group?

Dixon: In part. The research component was aided by support from the Office of Naval Research. I also wanted to develop joint courses with other departments where the more statistically minded faculty would participate in the course. I never achieved this. I also wanted to see that the vocabulary and the examples were appropriate for the department. I had an agreement from one of the professors in the psychology department to do that, but it was a lost operation because it no longer counted as mathematics if a psychology professor was doing some essential part of the teaching. Inevitably, my attempts to branch out failed. The branching was good as long as all the credit would go in the general pot. That was the chair's goal, not mine. At one point, the chair sensing my dissatisfaction said "You are an individual who is really going to succeed and you'll be doing better than you can do here at the University of Orgeon." He may have felt that I was becoming too big a nuisance, and he was right.

Flournoy: He may not have wanted to compete with you. Was the Dixon and Massey book (*Introduction to Statistical Analysis*, McGraw-Hill) published at this time?

Dixon: The book was written because of these courses. We felt that there should be a new book because the available books were inadequate.

Flournoy: The first edition of Dixon and Massey appeared in 1951. How long did you work in preparing it?

Dixon: We published several editions in advance of that, where we typed the manuscript and had it printed by Edwards Brothers in Michigan and distributed the books ourselves. I think we must have written the book in less than a year to begin with and then continued revising after that until our last revision in 1983.

Flournoy: I was interested in how you selected the material to be included. For example, there are a large number of tables that many subsequent statistics texts have adopted. What prompted you to include so many tables?

Dixon: More than the tables, what prompted the book in general was that almost none of the books we tried had uniform notation. At Oklahoma and at Oregon, the point I found most annoying to the statistics students was nonuniformity of notation. The nonmathematician is bothered by this. The common complaint is: "I learned what that is and now you call it something else. Why do you call it something else?" Well, mathematicians are not concerned about changing notation for local convenience. But this is a point that is troublesome to the nonmathematician. There was scarcely any book that had uniform notation. Some progress had been made by Helen Walker at Teacher's College, Columbia University, whose book (1943, *Elementary Statistical Methods*, Holt) eased the students' progress with the essential mathematics. She organized the notation but did not develop great statistical depth. I tried Walker's book and could see improvement in the students' response. So I decided to carry it further, to make it more modern, more complete and move the uniformity of notation even further. Another difficulty with most books at that time was that students were required to do a variety of computations in order to complete their analyses that could more efficiently be done by use of tables. Also the tables should be available in a single source rather than be obtained from several external sources. That made the study of statistics unnecessarily complicated for students from other disciplines. I thought we should provide a self-contained text with tables that are easy to use. I taught some of the graduate students to use a computer, and we verified existing tables and created new ones. Random numbers were generated from other tables of numbers. I was fascinated by computers, and so we prepared many of the original tables. Originally, we wrote separate chapters. One colleague said "Well, I know who wrote this chapter and who wrote that chapter." But since he had them reversed we thought that was good. As we wrote and rewrote, the text became more uniform. The book was published and because it was adopted so widely in the best schools, it generated increased enthusiasm for continuing. We had immediate adoptions in the best schools in the East and in the West. For example, both Berkeley and Stanford had multiple adoptions.

Flournoy: I do recall your mentioning that four departments at Berkeley adopted the book.

Dixon: Yes. Perhaps every department teaching statistics but the statistics department. Ironically, it was a book that was never completely acceptable among mathematical statisticians. John Wiley & Sons were already staking out the statistics field and so we sent it to them because we thought they had the best distribution. They turned it down. The comment from the reviewer was that it was very poorly organized. Most importantly, placing the material on probability at the end of the book rather than the beginning was entirely unacceptable. As far as he was concerned it would make every statistician in the world, both living and dead, turn over in his grave. We didn't want the living ones turning over in their graves, but Wiley insisted that we reorganize the book into a "proper" statistics text. After a while with no success with Wiley, we decided to go to McGraw-Hill. They accepted it immediately, and we did very well with them.

Flournoy: I used the Dixon-Massey book as an undergraduate at UCLA. That would have been 1968 or 1969. Later, as a Masters student, there was some consideration of changing the book. I was taking the graduate seminar, and they assigned each of us to do a book review on different basic statistical texts. What was interesting was that since everybody had had Dixon and Massey, it was easy to criticize. It was a real eye-opener to the students in the seminar that it didn't matter what we thought was wrong with Dixon and Massey, there was a lot more wrong with the other texts. Of course, at that time I didn't realize that Dixon and Massey was the first of its kind. How quickly did others start producing books?

Dixon: It was not quick. Of course, at that time the number of statistics courses taught was not large. An early large adoption was at West Point. Recently, the citation index people contacted me on two different matters. They said they occasionally take surveys to find unusual citations, and to ask the author to write about them. With their help we studied the 22-year period 1961 to 1982 and found that only 26 works received more citations (three were statistical) than our book. There was a very slow start when the book was first published with a steady increase over a long time period. The total number of citations to 1988 is

over 4000. Few text books ever reach 120 citations per year. The book sold, to date, 300,000 copies.

Flournoy: Was the Dixon-Massey book translated into other languages?

Dixon: Spanish, an official one, and also Thai. Others were Assamese, Korean and just this past year Indonesian.

Flournoy: How did the translations into Thai come about? That is not a common language.

Dixon: Dr. Bundhit Kantabutra was trained in America and has tried to keep in contact with as many world leaders as he could in order to put Thailand, and himself, on the map. He and his wife were statisticians for the government in Bangkok. He asked and received permission to translate the book, and so he did.

THE MOVE TO UCLA SCHOOL OF MEDICINE AND SCHOOL OF PUBLIC HEALTH

Flournoy: You mentioned you were dissatisfied at Oregon because of the treatment of statistics students, and when they wouldn't let you start a statistical laboratory, you came to UCLA to the Medical School. How did that happen?

Dixon: I took a sabbatical leave after six years and spent half the year at the University of North Carolina at Chapel Hill and Raleigh and half the year at Rand. John Williams had left Princeton when I was a student there to start the statistical activity at Rand. He then recruited a number of the Princeton graduates to Rand. Since Alex Mood and George Brown both went to Iowa State from Princeton, and then were recruited from there by Williams, several other people from Iowa State also went to Rand (or at least to Los Angeles). These included Ray Jessen and Paul Homeyer. In any case, the influence at Rand was from Williams, through George Brown, Alex Mood and the people at Iowa State as well as at Princeton. There were also invitations for Tukey and Mosteller and various others to spend time at Rand.

Flournoy: What year was your sabbatical at North Carolina and Rand?

Dixon: 1952 to 1953. There was discussion of my moving to North Carolina. While I was at Rand I taught part of George Brown's course at UCLA, and Olive Jean Dunn was a student of mine. Since I was only part time at Rand I started consulting with Planning Research Corporation there during my sabbatical. They invited me to join their firm. They had three or four principals and I seriously considered it. I asked Alex about other possible opportunities. He was preparing to start a company of his own. In addition, he was consulting with a large project at the UCLA Medical School and suggested that they needed full time faculty in statistics. At the end of my sabbatical I went back to Oregon and continued under a contract with Rand for the following year. UCLA inquired about my availability and by November or December UCLA's offer was pretty well in the works.

Flournoy: Did you talk to the people in the mathematics department or was the negotiation completely separate?

Dixon: There was no indication that the mathematics department was in any way involved with the recruitment. A selection committee was appointed in the Medical School and I came down for an interview, and all went well. The selection committee was the main selection committee for the formation of the Medical School: the five founding fathers of the medical school, Stafford Warren, William Longmire, Charles Carpenter, Andrew Dowdy and John Lawrence. Medical schools were required to teach some elementary statistics, but this committee expressed the need for a professor to serve their research needs as well as their teaching needs. The first offer was short of the Professor II level that I thought would facilitate the development of the group I wanted to build.

In any case, the department at the University of Oregon wanted a guarantee that I would return the next year. At that time I didn't have an offer from UCLA; I had the commercial offer. But I decided to risk leaving Oregon, and resigned my position. After I moved to Los Angeles, I received the desired offer from UCLA. That was a nice turning point because if the UCLA offer hadn't materialized I'd be in the commercial research business. After moving to Los Angeles, I did consult for Alex Mood's General Analysis Corporation for several years. When I started at UCLA I also held a joint appointment in the School of Public Health as their only tenured statistician.

Flournoy: Was the School of Public Health new, as well as the School of Medicine?

Dixon: In 1955, the School of Public Health at UCLA was run as a southern branch of the School of Public Health in Berkeley. It was not a full-fledged school. They answered to a dean through Berkeley, but were budgeted through UCLA. Dr. Wilton Halvorson was Dean of the School of Public Health at UCLA and chairman of the Department of Preventive Medicine. He recruited Steven Goerke in Health Administration, John Chapman in Epidemiology and me. My appointment came through in June, then Goerke's and, a few months after that, Chapman's. Harry Bliss was teaching Sanitation at UCLA, but later transferred to Berkeley. So of those who stayed, I was the first recruited, with Goerke and Chapman coming very quickly afterwards. We three were given appointments in the Medical School as well, but Goerke's chief interest was Public Health rather than the Medical School. Therefore, as the years went by, he paid less and less attention to his appointment in the Medical School and instead focused his attention on the School of

Public Health. My first priority was to the Medical School. Dean Halvorson was enthusiastic about statistics; he had come to UCLA from his position as head of the California State Department of Public Health because of a change of administration in the state. He supported and gave attention to building statistics in the Medical School. He thought it was important. He believed that there should be a statistics unit in the dean's office of the Medical School that would review all research proposals leaving the school and that it would increase their probability of funding by NIH, and so on. He wasn't in good health and had to retire soon. But he gave us a good start.

Flournoy: This was before or after you had established the Ph.D. program in biostatistics?

Dixon: This was before.

Flournoy: So you were working in both Schools?

Dixon: My appointment was in the Medical School with an additional title in Public Health. But I did all I could to build the Biostatistics group in Public Health, recruiting Frank Massey immediately, and then Olive Jean Dunn and Virginia Clark.

Flournoy: Were all their appointments in Public Health? It gets a little complicated. Your interests were in medicine, but you had no objection to a statistics program in public health and so you helped build it. Is that accurate?

Dixon: Yes. I didn't see the possibility of a graduate program in the Medical School at that time, so we built a graduate program in the School of Public Health. I think I spent sufficient energy in public health to do the job. I believe Dean Goerke would have liked my main efforts to be in Public Health rather than in the Medical School, and yet he was responsible, as chairman of Preventive Medicine, for the Medical School activities.

Flournoy: That is very common with cross-disciplinary research; none of the groups feel you are spending enough energy with them.

STARTING THE UCLA BIOSTATISTICS GRADUATE PROGRAM, 1955

Dixon: The School of Public Health was expanding with additional billets and a graduate program, and I had strong feelings about carrying the torch on to the next generation, so it was an opportunity to build a strong graduate statistics program. I was a full participant in searching for the Biostatistics faculty, appointing them and getting them approved. After two or three years, we received approval for the Ph.D. program in biostatistics. The School of Public Health was growing rapidly and they required all their students to take statistics. About a year after Frank Massey got here, he took over the management of the graduate program. Initially, I was Chief of the Biostatistics Division; then it was turned over to Frank, and subsequently to Jean Dunn. Abdelmonem Afifi later assumed the position and now he is Dean of the School of Public Health.

Flournoy: How did the Mathematics Department react to this development?

Dixon: All of these steps took much effort. I don't know whether any group on the main campus was interested in the development. I thought it was important to have the degree labeled a Ph.D. in Biostatistics, rather than a Doctorate of Public Health (Dr.PH), so that the students would have more status.

Flournoy: Weren't Nancy Mann and Virginia Clark among the first students?

Dixon: Yes. With National Institutes of Health (NIH) training grant support the doctoral program got underway. The first graduates in 1963 to 1965 were: Virginia Clark, Michael Tarter, Richard Kronmal, John Varady, Tony Lachenbruch and Nancy Mann. In the first ten years there were 39 Doctorates and 62 Masters. The second ten years saw 28 Doctorates and 117 Masters. The UCLA Biostatistics doctoral program has graduated many students who are now in academic positions. Clark stayed at UCLA and Mann and Lachenbruch returned to UCLA.

I also supervised or worked closely with Edwin Chen (Illinois), Paula Diehr (Washington), George Economos, Karen Yuen Fung (Windsor), Richard Kronmal (Washington), Tony Lachenbruch (UCLA), Michael Tarter (UC Berkeley) and later Stan Azen (USC), James Knoke (North Carolina), Annlia Paganini (USC), Jim Palmersheim, S. Raman (Ottawa) and Harlan Sather (USC).

When I moved to California we were able to find a large house in Santa Monica so we could continue to have students over frequently. This was a tradition that Wilks had started for me and does not seem to be a common practice with the coming generation of statistical professors. George Box at Wisconsin is an example of someone who encourages this level of socialization. We entertained visitors to UCLA as well. I believe the Russian visitors took the prize for enthusiasm and late night hours.

It was interesting helping students through the process. I also wanted the degree to be free of constraints by other departments. But the founding agreement had a requirement that the Mathematics Department had to pass approval on each of these students for them to pursue the Ph.D. For some reason this restraint has persisted beyond its usefulness. The Biostatistics faculty have the professional competence to enforce high quality standards internally. Mildred Mathias, after whom the UCLA botanical garden is named, strongly supported the development of the Biostatistics program because she felt that Biology needed a statistics presence on the campus that was not satisfied

by other departments. She lent assistance when it was needed.

I should mention the NIH training grant. Other schools of public health that had biostatistics divisions all had training grants from NIH, and so I thought we should have one too; it would enable us to recruit more faculty, to support students and to recruit better students. There was an organization of the heads of the biostatistics programs of the schools of public health that met with their NIH budget officer every six months or so to see that it was a successful program. There had been a statement that schools of public health were not operating at full capacity for the health of the nation because they needed upgraded statistical expertise. There was a general program to increase support to biostatistics, and the heads were having a meeting at Berkeley, but when I went to Berkeley to see how they were doing and to get some information about how I could get a training grant, Jacob Yerushalmy from Berkeley told me that I couldn't attend. So, I stood around out in the hall for some time until Bill Cochran came by and said "What are you doing here?" I replied, "I wanted to attend this meeting and Yerushalmy says I can't." Cochran went into the meeting and soon after, the project officer from NIH came out and said that they would like to have me join the meeting, that there is nothing confidential. Later, I made an application for a training grant that finally was approved. I was principal investigator on the UCLA Biostatistics Training Grant. We immediately started recruiting students. Sometimes persistence is necessary.

In transferring the responsibilities of the training grant to others in the Division, the deadline for renewal was missed. That gave an opportunity to those who weren't in favor of a training grant for UCLA to say, "Well, those are the rules. You didn't get it in. There is no way you can make a proposal now." After twenty or more hours on the telephone, I got the application reinstituted. The program has successfully grown from that time.

Flournoy: Well, I can remember a similar occurrence at the Fred Hutchinson Cancer Center and how shocked Dr. E. Donnell Thomas, who had just won the Nobel Prize, was; he had delegated a grant renewal to a senior investigator, and the deadline came and went and Dr. Thomas had to jump through all kinds of hoops to pull it back out of the abyss. I guess it happens.

MATHEMATICS AND STATISTICS

Flournoy: I know that in more recent years various individuals at UCLA, such as Don Ylvisaker, have tried to form a Statistics Department. I guess you are not very optimistic that it will come to pass.

Dixon: I don't know. I am not sure it is good for UCLA. I'm tempted to launch into the business about mathematics versus statistics.

Flournoy: Why don't you explain some of that now?

Dixon: Wilks convinced me that statistics could be a full-fledged part of the mathematics community, and it could gain strength from that, which it did for him. But after what I had seen at Wisconsin, Oklahoma and Oregon, I was not prepared to believe that I had bought the right package from Sam. It was true for him and for Princeton at that time, but it was not true elsewhere. I could sympathize with people in other departments, struggling to get the statistics they needed. It was biology and a physiology group at Oklahoma; it was the psychologists and biologists at Oregon. When I got to UCLA, I could see that the medical school was relieved to have their own statistician, as was the Biology Department. Many came and got their advice from me. I could see how statistics was stifled at the University of Oregon.

I thought it was too risky to have a statistics division within the mathematics department. I was convinced that would be death. To have a separate statistics department was, also, not necessarily good. Around the country, I had observed that almost all of these statistics departments soon resembled mathematics departments. As soon as they separate, the mathematical statistician (who, since he doesn't consult, has time for politics) starts building the department with statistical purists, and therefore not doing what I think is important for a statistician. That is, to be, as I was, thoroughly immersed in fields of application.

There are good examples of this phenomenon. Biological faculty at a nearby UC campus tried to get approval to hire a statistician in the embryo-medical school that they were building. The mathematics department said, "No matter; just give us the billet and we'll recruit a good guy for you." They recruited somebody that had top billing as a probabilist. This embryo group returned to the Regents a year or two later to solve the problem that wasn't solved. This time they didn't want to repeat the same procedure. They were overruled. The mathematics department claimed they would recruit an applied statistician. Another faculty member is added, but, of course, the main problem was not solved since the department's definition of "applied" is irrelevant to the need that still remains unmet. I doubt that this is an isolated case. Of course, it happened at Berkeley too. Neyman was a good applied statistician, but few of his students developed a similar capacity for applications, unless it was in astronomy. There are examples in other institutions. I wasn't convinced that UCLA could profit from a statistics department. To get a statistics department at UCLA, one could have had to have the support of the mathematics department and therefore yield to

its influence. Also, I thought it would weaken the biostatistics program because the biostatistics program sits halfway between a school of public health program in biostatistics and a statistics department and would have less freedom for its program. A number of students who were graduated here could not have been trained in a biostatistics department if a statistics department were on campus. The absence of a statistics department meant that UCLA had the status of turning out the best doctoral students in the United States, as far as schools of public health were concerned. However, a statistics department might work now because the other departments have well enough established statistical groups.

Flournoy: I guess that the current group is looking towards a behavioral and social sciences connection. My impression is that some statisticians are perfectly content to be a mathematical statistician for some time, but that they may become frustrated by not having connections to applications. So where do statisticians in the mathematics department go?

Dixon: Well, I have heard mathematicians say that pure mathematicians are over the hill by about age thirty. Perhaps they are referring only to their colleagues. The development of applications allows one to continue to grow. Some of them do it with grandeur, like Mark Kac, for example. He was a great probabilist, and became interested in a variety of applications.

I've seen articles on "What is statistics?" I guess my feeling is that it should be considered a science, a nonmathematical science; somewhat more mathematical than biology or physics. It has an abstractness to it, so I want to call it a theory, but it is a science in itself, not a branch of mathematics. A recent book *The Emperor's New Mind* by Roger Penrose (1989, Oxford University Press) examines artificial intelligence. This book stresses the importance of human intuition in scientific advancement that will not occur by mathematics alone. My belief is that there is a construct of invention that has to do with scientific proof which in some way can be distilled in an observational science, somewhat independent of applied fields such as chemistry, physics and biology. There is a scientific proof algorithm, but what do we call it? The term "applied mathematics" was introduced more than 100 years ago, but it now means mathematical physics. The term "mathematical statistics" has lost its distinction from mathematics, and so we may need to drop the use of the name mathematics. Theoretical science is a possible title since the statistician thinks more like the biologist or chemist or some of the more pure medical fields that touch the applied. The thought processes of the statistician's mind much more closely match those who work in these fields than that of the mathematician. Some of the current mathematical statistics types want to, and do, become applied statisticians.

Flournoy: An applied "wannabe"?

Dixon: Yes. I think it is difficult for the mathematical statistician to leave the security blanket of mathematics and go forth into the real world where levels of significance are larger than zero and where power can be less than 100%. How do I know whether the assumptions are correct? What if the experimenter has made some errors in recording his observations? What if his conclusion is wrong? It can be overwhelming.

Flournoy: Talking about statistics departments makes me think of places such as Harvard that have both biostatistics and statistics departments. It has a large biostatistics department and it has a statistics department.

Dixon: The statistics department there grew out of social sciences. They were really willing to do the applications.

Flournoy: Minnesota is another university that has statistics, applied statistics and biostatistics.

Dixon: Apparently, they are not inferior to the mathematics department and all have similar status. That can work.

Flournoy: That's right. They have a School of Statistics that houses the statistics and applied statistics departments.

Dixon: Kentucky has done that too. I think it can work that way. The mathematically minded statistician who joins the statistics department, as far as I can see in general, eventually heads the department, and therefore it continues to move to the more theoretical area. It is a loss.

CONSULTING AND STATISTICAL SOFTWARE DEVELOPMENT IN UCLA's MEDICAL SCHOOL

Flournoy: Tell us who was involved in the initial statistical software development, and describe the ideas behind it.

Dixon: While commenting on the development of the statistical software it is important to recognize that the facility that supported the original software development also supported development of systems software for the computer operations, the biostatistical consulting service for the Medical School and the computer support service for the entire UCLA Health Sciences community as well as for many other institutions around the country. There is a full bookshelf of the history of this facility in the reports that were prepared each year for NIH. These include details of the research areas of project staff, the research of all the users of the facility, computer time, analyses of any innovative use of the computer and abstracts of the more interesting projects. A full bibliography of the papers based on computations using the facility is available covering the years from 1961 through 1978.

Leading up to the software development were my

interest in computing that started at Princeton and my fascination with applications. I am more interested in the science of the application than in the mathematics. While at Oregon, I visited Carl Hopkins at the University of Oregon Medical School and saw the large variety of interesting research problems in the medical environment. When I became a professor in UCLA's medical school and learned more about the computational needs of medical research, I soon found that the major deterrent to expanded involvement was the computational difficulty that was associated with problems with many variables and many observations.

The statistical software development arose from my consulting interests. First, with some support from the dean's office and from the National Cancer Institute (the project officer was Marvin Schneiderman, Chief Statistician), I started a consulting unit in the UCLA Medical School. In 1959, there was a flyer from NIH regarding available funds for technical support of medical schools, including computers. Dean Goerke asked me if I'd consider putting in a proposal. I said, "That sounds like a lot of administration; what I want is support to develop the applications here." He kept urging me and finally I turned in a proposal. With the resulting support we hired programmers and wrote the first software packages using an IBM 650 of the Western Data Processing Center (George Brown from Princeton days was Director). I was disappointed to find that the programmers insisted on writing a new program every time they did a different regression problem. I suggested what appeared to be a new idea to them, that is, to parameterize the program. For regression, we first parameterized the sample size, and then we parameterized the number of independent variables and then we parameterized a number of other aspects. This meant that anybody who could punch cards could set this program up and use it without the help of a programmer. With this success, we requested support from the Program of Technical Assistance in the Research Resources Division of NIH and were awarded support for an IBM Model 1410 in 1962.

Things began moving very fast. We had good programmers who were producing programs for a number of statistical procedures. In a short time, we issued a manual with 20 to 25 programs with instructions for the programs in fixed format code. Parameter language followed later. People heard about our software, and by 1969 we were shipping programs and manuals on request. We submitted a supplement to our NIH grant that was several times the size of the original grant. When that was awarded, we upgraded to the IBM Model 7094. This was the first Model 7094 delivered by IBM (later we received the first Model 91). A large computing facility within a medical school was unusual and received a lot of attention. Visitors came from all over the world. From the beginning UCLA was a leader

FIG. 2. *Dixon explaining the network of support to other facilities in California (and the Northwest) in 1967. Soon there were 185 remote stations operating out of the computer at HSCF.*

in computing because the Standards Western Automatic Computing (SWAC) from the Bureau of Standards was located here; this computer was a duplicate of the one at the National Bureau of Standards (now renamed the National Institute of Science and Technology) in Washington. Since the computing successors to the SWAC were also being developed on the main campus, UCLA had two large computing centers.

Flournoy: How do those computers compare with today's computers?

Dixon: I will compare them to the IBM 7090, a popular machine for many years. Today's PC (or laptop) has memory capacity 50 times, memory access 300 times and disk storage 5000 times the 7090. Also the 7090 required a large air conditioned room. The 650 was about a factor 10 less than the 7090 in terms of memory, storage capacity and access time.

Flournoy: Were there political problems for UCLA's Health Sciences Computing Facility?

Dixon: With the expansion of the facility and its level of support, the political concerns grew on campus. Why is a facility for statistics and computing needed

in the Medical School? What sort of "imbalance" does this produce in orderly campus development? Is it appropriate for a grant of this size to have a single principal investigator? Is it appropriate for a computing facility to be headed by a statistician rather than a computer scientist? etc., etc. To the best of my ability, I dealt with these problems one by one. The problem of space was solved by asking for an unfinished area of the medical school and obtaining external support to finish the area.

Flournoy: Was there support outside the Medical School for your software development program from the Mathematics Department or the School of Public Health?

Dixon: The Mathematics Department generally was not interested in computers, but Professor Tompkins of that Department was director of the other campus facility. The School of Public Health was supportive from the beginning with Massey and Dunn supporting the software program's foundation.

Flournoy: So how did you go about deciding what statistical routines to pursue and at what level to pursue them?

Dixon: Well, we had a consulting clinic that faced a variety of questions from the medical researchers.

Flournoy: Was this consulting clinic housed in the School of Medicine?

Dixon: Yes. As much as possible, the participating statisticians were closely involved with the medical investigators, not only working to achieve solutions to the specific scientific problem but also looking for the generalities of software that could answer many questions. On the initial contact with an investigator, we would help the individual start collaboration by answering some questions and giving some initial support. Many of the investigators included us as colleagues and co-investigators so that we gained a deeper and deeper medical education as time went on. Some of the biostatistics students acquired experience in the consulting clinic in the Medical School.

A large number of the questions we faced in the consulting clinic could be formulated as a regression problem. Thus, the first computer program we wrote was for regression and others followed. Since I was particularly interested in discriminant analysis, we soon produced our first discriminant program. At the same time, we were improving the data entry and editing routines. New programs initially grew from the consulting demand.

Later, programs grew from the individual research interests of the consultants. Mort Brown, Alan Forsythe and Ray Mickey are among those consultants who developed academic research records of their own. Individual publications are a requirement to advance properly in the university environment. I never won the battle of getting proper credit in the review process

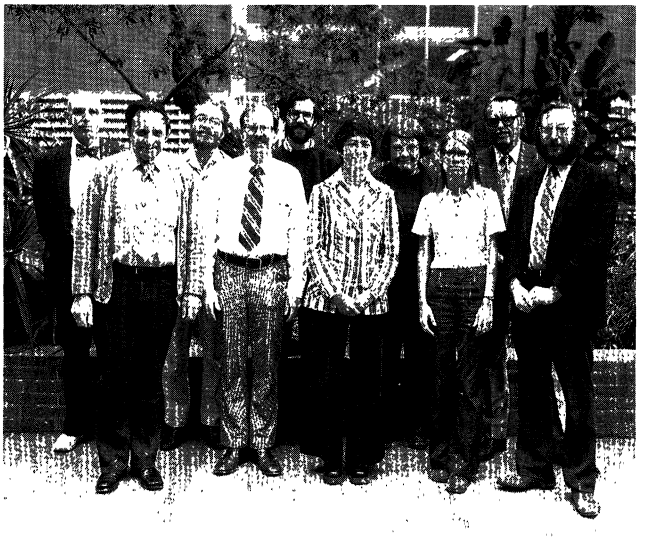


FIG. 3. *Biomathematics Department, UCLA, 1977. (Front) Robert Elashoff, Norman Johnson, Jackie Benedetti, Mary Ralston, Michael Fox, (rear) Ray Mickey, James Frane, Kenneth Lange, Carol Newton, Will Dixon.*

at UCLA for the demonstrated art of bringing deep insight and truly creative contributions to applied research projects that were not in the form of articles in standard journals. But here I was not only dealing with the theoretical mathematicians and statisticians, I was up against the entire University of California system. They recognize creativity in music and the arts, but they do not recognize statistical consulting, which I believe can be as imaginative and creative as any artistic endeavor. Ray Mickey revolutionized the organ transplant industry by developing some brilliant procedures. But that, to some, was an invention, rather than research. So although organ transplant research was greatly advanced by his efforts, there was little recognition in the University for such a clear contribution to science.

Flournoy: I am aware that Mickey contributed many highly creative and outstanding solutions to histocompatibility problems that arose from Paul Terisaki's research on organ transplants.

Dixon: Yes. Productivity, however, is not necessarily optimized by writing papers in the generally accepted format at every step. A problem we have not solved is how to evaluate professional competence, research results and their effect on society. I don't think we are doing a very good job.

Flournoy: Many young people in statistics departments across the country are facing the same issues. That is, we have a computer-literate young population that is undertaking topics that none of their professors – being more pure mathematical statisticians – have any idea how to judge for themselves.

Dixon: Doctoral committees can be a self-perpetuating system.

Flournoy: When I was at the Southwest Educational Research Laboratory early in the 1970's, I didn't think the BMDP programs had that much of a medical flavor. They seemed very generic to me, so I questioned, "Why is this biomedical?"

Dixon: The breadth may not be surprising because the medical disciplines cover a broad range of applications and these applications come from all areas of basic science. Perhaps a general aspect is that variability is large within experimental units as well as between, and problems are almost always multivariate. This characterizes much of biological research, but is also true in other fields. At first, the demand for the BMD programs outside of UCLA was greatest in the pharmaceutical industry and in the oil industry. As it became more clear that the programs were received widely the name drifted from BIMED to BIMD to BMD, which many people call the package still. The official name is now BMDP, with the P indicating parameter generality.

So the development was a matter of trying to understand what was going on in a multivariate medical application and adding statistical know-how to the imagination of the investigator.

Flournoy: I'm thinking now of the relationship between the applications with which you were directly involved and the BMDPs. You had a team of people that were consulting. They were all feeding problems into the software development at the same time you were consulting, and problems came out of your consulting as well.

Dixon: By easy stages I worked my way through applications from surgery to psychiatry. Psychiatry, I think, has the most complex problems. Early on, I was attempting a discriminant analysis for Sherman Mellinkoff in Internal Medicine. He said, "Some people with liver disease have an appetite and some don't," and he wanted to know why. On the assumption that the answer lay in the configuration of the 32 amino acids that he was measuring, we faced the problem of creating a program that could handle more than the eight variables that could be accommodated by hand calculation. Results from this analysis suggested the need for a factor analysis. Both discriminant and factor analyses call for matrix computations and stimulated some of the first important contributions from Robert Jennrich.

Flournoy: What was the result for the patients with no appetite?

Dixon: The discriminant analysis of the amino acid data indicated the chemical pathways that might be involved, but the factor analysis made the answer clearer since the factors grouped variables together corresponding to specific chemical subpathways of the amino acids. The first factor was recognized as a well-known interrelationship, but the second gave a clue to the process that was then verified as the culprit. Thus, we exhibited a discovery process using factor analysis (S. M. Mellinkoff et al. (1957) *Gastroenterology* **32** 592-598).

Another collaboration dealt with cancer of the cervix with Elizabeth Stern, an expert cytologist. Cancer was classified in stages I, II, III, IV. I suggested using a finer classification for research although the clinicians may find four levels adequate for their practice. She stated that there was concern that the classification have as little error as possible. However, as a result of my suggestions, she reviewed her large collection of slides using a much expanded set of descriptors of the cytology. By studying the transition matrix of these many cell anomalies from time series for the individual women it was possible to form a finer ordered set of progressive stages defining a number of levels of dysplasia that precede the cancer. Her publications with me and other statisticians helped revolutionize cervical cancer cytology. Publications appeared in 1961 (*Cancer* **14** 153-159), in 1967 (*Cancer* **20** 190-201) and in 1974 (*Cancer Research* **34** 2358-2361). There is a great urge for the clinicians to have a small number of categories for diagnosis of individual patients. Research will usually require expanded scales or scores.

Flournoy: I noticed that in Seattle. When I joined the Bone Marrow Transplant Team up there in 1973, I found that everything had to be chopped into groups of four. It didn't matter whether the measurement was discrete or continuous or whatever; four was as much as medical scientists could handle at that time.

Dixon: Some researchers are concerned about not being wrong, and as John Tukey once said, "If you don't want to be wrong, don't make any decisions." Using four diagnostic groups keeps the number of necessary decisions small.

Flournoy: You also spent some time looking at cholesterol and atherosclerosis.

Dixon: I served as statistician for a large Veterans Administration study of the effects of diet on cholesterol. Veterans were randomized to standard and low cholesterol diets. See S. Dayton et al. (1962, *New England Journal of Medicine* **266** 1017-1023). The study was performed at a VA domiciliary. The results received worldwide attention. We showed that you could reduce cholesterol easily, but that it didn't change the death rate.

Other studies had similar outcomes. Richard Peto called me several years later to say "We have all these studies, and about half of them go one way and half go the other way. The VA study seems to be a much better study. Can we get the data? Can we do something with it?" Unfortunately, the researchers discarded the data when they lost their funding. This and

other studies seemed to indicate that some reduction in death rate for heart problems was associated with an increased cancer death rate.

Flournoy: Were there particular problems that came up in either the cervical cancer or the fat research that ended up in the BMDPs?

Dixon: The research topics included these and ranged from acne and asthma on through the whole alphabet. Research contacts developed with every department in the Medical School. Data ranged from a patient's clinical record to real time electroencephalograms. Bob Jennrich, Mort Brown, Alan Forsythe, Ray Mickey and John Hartigan were active in designing the contents of programs to supply what the applied community needed.

Concerning the cervical cytological categories, there are several aspects that were never developed. For example, how can one estimate precisely the transition matrix from one cytological category to another, recognizing that transitions are variable and are observed with error and irregular interval censoring.

Flournoy: Is there a Markov chain model there?

Dixon: No doubt.

Flournoy: What attention was given to missing values?

Dixon: From the beginning we wanted to do something regarding the fact that standard software programs drop the whole case if any one of the independent variables is missing. Expanded computer power made this problem more acute since one now had the power to handle problems with many variables. It is easy to see that some solution was necessary because with random entries missing, say 5% of the time for each variable, we can expect less than half of the cases to be complete if we have 15 variables. An early attempt of ours was to develop a stepwise regression program that at each step evaluates the trade-off between adding a variable that was fairly complete versus a variable that was highly predictive but less complete. That was one of the unfinished projects at the time the funding from NIH was terminated. As yet, no released package attacks the missing value problem in this way.

Flournoy: What happened to the BMDP grant? I had assumed there was some policy decision.

Dixon: Yes. After 16 years of successful operation of the facility with NIH support, the full-scale facility ended. In 1977 to 1978 we weathered two site visits, each resulting in priority scores well within the funding range; but each review ended with the same decision in Council, that the project lacked relevance because it was expected that software companies such as SAS would react to the market and provide users with state-of-the-art software. Of course that didn't happen and few of the proposed research projects have resulted from efforts elsewhere. After various efforts at different levels within National Institutes of Health, we were

FIG. 4. *Will Dixon, UCLA Chancelor William Young and Pat Britt at the IBM 1410 Console in 1963.*

permitted to have a three-year phase-out contract so that it wouldn't be such a catastrophe. We then proposed a grant to follow the contract, a small one, to pursue some of our goals. The Research Resources Division proposed sharing support with the National Institute of Mental Health. They each decided to fund one-half of it, but there was an administrative decision later not to fund, likely related to the same faction in the same council. So the Research Resources half was dropped, but NIMH funded their half. I continued for six years with NIMH, and the grant is now in its third three-year period. It's a nice small grant.

Now the reason for all that preamble is that I still wanted my missing value problems solved. Therefore, in this NIMH grant we funded the research part. We persuaded the post-doc Mark Schluchter, working with Bob Jennrich, to develop a missing value analysis of variance program which we call BMDP 5V. So it will, to the extent that you can estimate more and more parameters, estimate the parameters for each missing value. Maximum likelihood estimates are used for an analysis of variance design, and it handles a number of different choices for the assumption of the matrix of the repeated values, unstructured, autoregressive, etc. Clearly, it is a very powerful program for repeated measures analyses.

Flournoy: Repeated measures was one of the places where BMDP excels. A Master's level statistician who worked for me always swore that, as far as repeated measures were concerned, SAS would do anything BMDP would do. She came in sheepishly one day with a repeated measures design that SAS wouldn't handle,

FIG. 5. *Alan Forsythe (Amgen Corp.), John Hartigan (Yale University), Bob Jennrich (UCLA), Michael Fox (unknown), Carol Newton (UCLA), Will Dixon in the IBM Model 1410 days.*

and I said "Well, let's see if BMDP can do it." And, in fact, BMDP would do it.

Dixon: The NIMH grant continues with Rod Little as Principal Investigator.

Flournoy: Is Rod Little's joint work with Don Rubin on missing values work connected with BMDP development?

Dixon: Yes, I met Rod Little working with Sir Maurice Kendall, Director of the World Fertility Survey in London, and recruited him here. He was a student at Imperial College with David Cox.

Flournoy: And he was interested in missing values before you recruited him?

Dixon: Yes, he was. I don't know how deeply he had gotten into it, because he was the chief statistician for the World Fertility Survey. So they, of course, faced missing values. He was developing inventive ways of dealing with missing values. He then joined forces with Rubin. They produced a book (1987, *Statistical Analysis with Missing Data*, Wiley) on the topic that has created wide interest.

Flournoy: Well, we are covering a combination of areas in which you have worked: the BMDP programs, applications and the statistical areas. We still have gaps in all three of those, and yet they undoubtedly are tied together. We just talked about missing values and the applications that spawned your interest in missing values. What about psychiatry and schizophrenia? You spent quite a bit of time with these applications.

Dixon: Our consulting clinic worked from the beginning with Phil May, a professor of psychiatry. He was very active. He believed in statistics and saw to it that he learned the fundamentals of why the statistic was doing what it was, not mathematically, but conceptually. In fact, he worked his way through some of the mathematics books in an attempt to understand it. By understanding what statistics were doing in a fundamental way, he was always questioning, "Well, why can't you do this? Why not do that?" Well, we didn't have the programs for it and hadn't thought about it before. Consequently he strongly drove development. He worked with all the people in the clinic, or almost all of them. He worked with me, and then Alan Forsythe, with various others, and some junior people helped. He was the one who insisted that we get a regression program for missing values, and was disappointed that we didn't produce one.

Flournoy: But it has come along now.

Dixon: Unfortunately he died recently and didn't get to see it. He was deeply involved in research on schizophrenia and a treatment side effect, tardive dyskinesia. The interesting measurement problem is the measurement of tremor and other muscle movements at various frequencies as related to various drug regimens.

Flournoy: What were some of the ideas that determined the type of article you would publish?

Dixon: I think they were topics that would simplify applied research work, general problems, like the sign test. No great mathematical or statistical talent was required to base the test on the binomial distribution. But we felt that applied research would be aided by a simple reference table rather than the answer being searched from binomial tables.

Flournoy: And yet that was published in the *Journal of the American Statistical Association* in 1946.

Dixon: Yes.

Flournoy: Times must have changed. What was behind your up-and-down procedure?

Dixon: It came out of Bruceton Laboratories, testing explosives, and it was developed under a contract during World War II with the Princeton research group. They were dropping weights on explosive caps and primers, and Alex Mood and I obtained the probit analysis type of solution for data resulting from this special design (1946, W. J. Dixon and A. M. Mood, *Journal of the American Statistical Association* **41** 557–566). It turns out that our up-and-down technique uses the minimum number of subjects to get the most efficient estimate of the median effective stimulus. The application of this design in biology is especially important since the number of animals required is minimized. This requires less handling and management of the animals and for research on scarce genetic strains or rare animals such as primates, it gives maximum results for minimum subjects. In most applications, the animal requirements are about 20 to 25% the number required for the traditional probit design. Later I published a paper indicating that for small samples one could obtain estimates of the median effective stimulus with constant variance by using a "nominal" sample size (1965, *Journal of the American Statistical Association* **60** 967–978).

Although this technique was introduced before 1950,

forty years passed and people continue to use animals in wasteful numbers. Therefore, I sought funding from NIH to develop documentation and write programs, to help convert toxicologists to use a more efficient research design. They funded it. It went through the first six months at the $50,000 level, and now we are preparing to carry it further. A very similar movement is coming from the engineering side, although no engineer refers to the biology literature and no biologist refers to the engineering literature. Even though the up-and-down technique is more than forty years old, it is used effectively in less than 5% of the test facilities where it could be used.

Flournoy: I would agree completely on the need for investigators to use resources more effectively. I have been doing some related research. My motivation is experiments with humans, where they couldn't afford the chimpanzees and so they just study people.

Dixon: That was my original motivation.

Flournoy: You said you got a six-month grant. What were you able to accomplish?

Dixon: We really were just able to get started. It was a Small Business Innovation Research, Phase I grant. Now I am seeking further funding.

Flournoy: Are there other major areas that you are tackling at this time? For instance, I think you have been involved with sample size issues.

Dixon: I was more a facilitator with regard to sample size issues. We put a number of better diagnostics into the BMDP program 5V, but I don't think the beginning user understands easily what's going on when there are multivariate missing values.

Flournoy: That's what I would be afraid of, that the people using packages that don't have missing value estimation procedures available would not know that they are missing.

Dixon: In fact sample size concerns should probably be addressed in the edit programs that say how many cases are read in and how many cases are used. Indications should be made at this point of reduced power associated with situations in which only a small fraction of data can be used in a computation.

Flournoy: What about your paper on outliers?

Dixon: This was based on the distributions of the ratio of ranges of extreme order statistics. The goal is similar to that of Frank Grubbs (1950, *The Annals of Mathematical Statistics* 21 27–58) who used variance estimates rather than ranges. Power under normality for the two approaches differs very little for a variety of outlier models.

NATIONAL AND INTERNATIONAL ACTIVITIES

Flournoy: How did national activities in statistical computing get started?

Dixon: The American Statistical Association had formed a committee to consider the role of the computer in the Association's activities. After a year or more with no action from the committee and continuing difficulty in getting program slots for topics in statistical computing, several of us from UCLA with the help of others, circulated a petition at a national meeting. The petition with hundreds of signatures, far more than the minimum required, was persuasive and the Section on Statistical Computing was established in 1972. Churchill Eisenhart asked me to serve as the interim Chair. We obtained the desired program assignments for the meetings and the section soon became one of the largest. Internationally, Mervin Muller and I worked together to initiate a statistical computation section in the International Statistical Institute. As an international organization, it is important to support communication and I sought unsuccessfully to have the ISI allow a separate journal for articles dealing with computing. This proposal was turned down with the usual comment. That is, if the articles are of high quality they will be published in the Review. Of course, this did not happen since new things are never of high quality to the establishment. Several private journals soon appeared. Finally, after many years, one of these journals (*Computational Statistics & Data Analysis*) has become the home of the ISI Statistical Software Newsletter.

Flournoy: Were you involved in the Interface conferences as well?

Dixon: The Interface also grew out of the impatience of computer-minded statisticians with the establishment. The development of the BMDP package had created interest in charting the future of statistical computing here in Southern California. Nancy Mann and Arnold Goodman were the chief movers forming the statistical computing activities in the Southern California Chapter of ASA and the Los Angeles chapter of the Association of Computing Machinery. The first four meetings of the Interface were held in Southern California 1967 to 1971. A history of the Interface appears in the initial issue of the Interface Bulletin (1991).

Flournoy: Tell us about your involvement in international affairs.

Dixon: My first trip to the Soviet Union was to an International Cancer Congress in the late fifties. This occurred because I was serving as statistician to several national cancer clinical trials. These were combined University and Veteran's Administration trials of chemotherapeutic agents for gastric and colon cancer. During the period of the major research activities of the UCLA Health Sciences Computing Facility, I lectured at numerous meetings in Europe and Asia on statistical and graphics software.

Flournoy: I understand that you made several trips to the Soviet Union.

Dixon: Yes. Nixon and Brezhnev instituted a program of scientific exchange. Don Aufenkamp of the

FIG. 6. *In 1975, facing Moscow State University, Vladimir Morozov, Laszlo Rakoczi (Tymshare), Interpreter, Will Dixon, Interpreter, Don Aufenkamp (NSF), Virginia Klema* (NBER), *Dan Teichroew (University of Michigan). Ben Mittman (Northwestern University) took the picture.*

FIG. 7. *Will Dixon with Andrei Kolmogorov in Moscow in 1975.*

National Science Foundation was placed in charge of the exchanges that focused on the use of computers and economic planning. This topic area was later divided into the areas of economics, statistical computing and computer systems for later exchanges. I led the group in statistical computing and made additional trips to the Soviet Union visiting computer activities in Leningrad, Kiev, Minsk, Tbilisi, Tashkent and Novosibirsk as well as those in Moscow. In return, groups from the Soviet Union were provided with tours of American facilities, even Disneyland.

Flournoy: What level of computer development did you find in the Soviet Union at that time?

Dixon: Our group estimated that the lag time was about ten years in hardware, perhaps a little less in software. To some extent the lack of hardware development stimulated imaginative software solutions.

Flournoy: What type of statistical software were they using?

Dixon: Each laboratory had some of their own software as was common in the United States in the early days. The only general package they were using was the early BMD software. This package had been modified to include largely cyrillic output and had been distributed throughout the country. In this way the lag was only about two years. Technology transfer was occurring, whatever the rules.

Flournoy: Was this a legal import to the Soviet Union?

Dixon: No. There seemed to be little delay in the import of later releases as well.

Flournoy: Did you become acquainted with Professor Andrei Kolmogorov while in Moscow?

Dixon: Yes, we visited with him on several occasions. He expressed great interest in computer support of statistics, particularly with regard to how graphical presentation could be a part of the analysis. I had the most remarkable impression that he shared similar opinions and interests to mine regarding applied statistics. This was in strong contrast to the general image most Americans have that Kolmogorov is mostly a strong theoretical mathematician. Recent biographical comments in our literature by mathematicians describe him as such. It appears that in his communications he stressed whatever was the interest of his interviewer. His interest in applications, however, went far beyond the conversational level. For instance, during our visit to research laboratories throughout the Soviet Union, when the researchers were questioned how they obtained statistical advice and guidance, they always credited Kolmogorov and his laboratory in Moscow.

Flournoy: What areas of applications were involved?

Dixon: All areas. Our contacts with research laboratories covered a great variety of applications. One of the groups that volunteered information about their contact with Kolmogorov was a social research group in Novosibirsk. Kolmogorov's informal publications on applications had a wide readership in the Soviet Union. Unfortunately, they were not exported. His publications, however, on elementary mathematical exercises and the teaching of computers in high school did reach this country.

During my visits with Kolmogorov, he introduced me to Professor Akiva Yaglom who is shown in one of the pictures. He also arranged contact with Professor Boris Gnedenko. When I visited with him in his office, I gave him copies of the latest editions of the Dixon–Massey text and the BMDP manual. He expressed a special thank-you since he said his earlier edition of the text had been heavily used and he appreciated the new edition. I was pleased to note Gnedenko's comment in his "Conversation" in *Statistical Science* (7 282) that "It is impossible to do good work in mathematics without application."

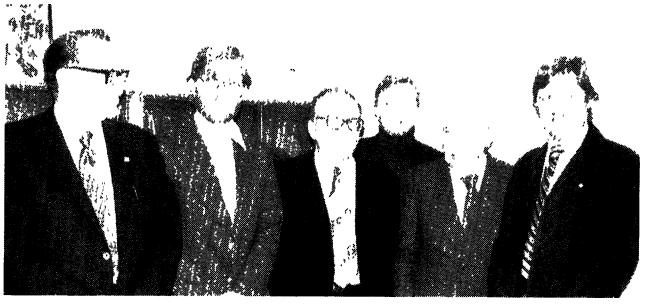


FIG. 8. *Will Dixon, Anthony Thrall, Akiva Yaglom, Igor Zhurbenko, Andrei Kolmogorov, Bert Davis in Kolmogorov's office in January 1978. Thrall and Davis were part of the visiting team.*

FIG. 9. *Dixon receiving the Wilks Medal at the 1992 ASA meetings from William DuMouchel with Arnold Zellner and Stu Hunter looking on.*

Flournoy: Did you have other international contacts?

Dixon: In 1979, I visited the International Institute for Applied Systems Analysis (IASSA) in Vienna on two occasions. They were heavily involved in computer applications for modeling economic, environmental and social systems. My contacts involved computer software. The Institute, in addition to its continuing members, had short-term visitors from both Western and Iron Curtain countries.

Flournoy: Other contacts?

Dixon: In 1952, I became acquainted at an American Statistical Association (ASA) meeting with Sir Maurice Kendall. He was a bridge enthusiast, and we enjoyed playing together. We formed a lifelong friendship. Sir Maurice was Director of the World Fertility Survey (WFS) that was done under International Statistical Institute sponsorship. He asked me to work for the WFS in the early 70's, and I served as consultant. So then I got more involved in ISI activities at that same time. It was always a pleasure to visit with Sir Maurice. He was continually creating new limericks and poems. He was also fascinated with puzzles and oddities. One example was his ever changing "alphabetisms": A for -ism (aphorism), B for pork (beef or pork), C for yourself. Later letters are L for leather, O for a beer, Q for the bus and British expressions R Faminute and W for quits.

Visits to WFS were occurring at the same time as the trips to the Soviet Union. I combined some of the visits to the WFS with those to the Soviet Union, as well as visiting my daughter Kathleen Dixon-Nebert at the Max Planck Institute in Berlin. It was always an experience to cross from West Berlin to East Berlin. Once, while bringing some 16-mm film to show Kolmogorov computer graphical statistical procedures, the East Berlin guard unrolled many feet of the film, perhaps hoping that it was pornographic.

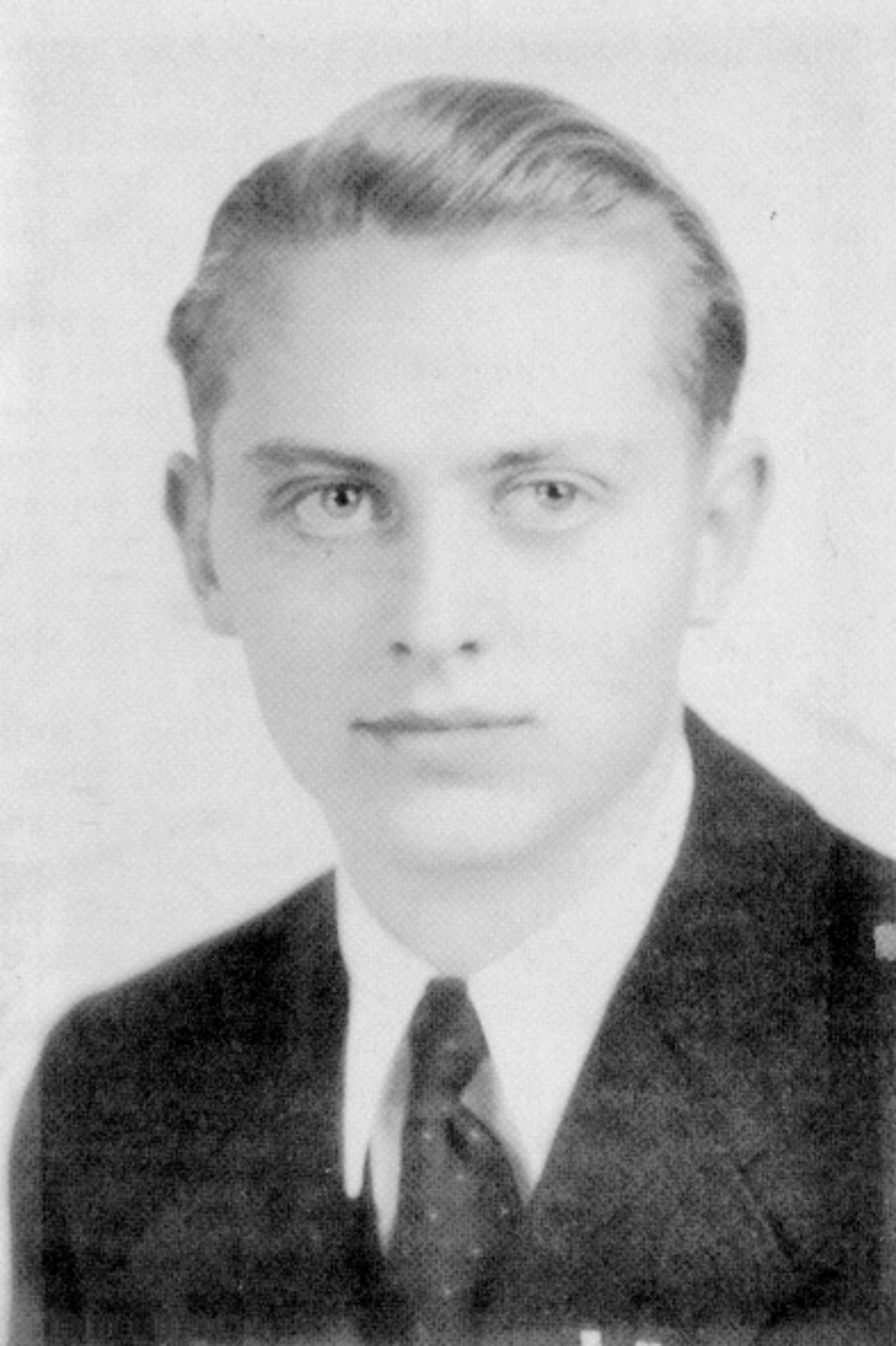
Flournoy: Are the World Survey Reports the books of data that my students find in the library when I send them looking for data to analyze?

Dixon: I believe so. There was a report for each country surveyed. The analyses were thorough and complete with discussions of the applicability of various models to the observed data.

Flournoy: What careers are your children following?

Dixon: Scientific discussions were a part of the dinner table conversation while my daughters were growing up and both are now pursuing successful professional careers. Janet Elashoff went to Stanford and then completed her Doctorate in 1966 with Fred Mosteller at Harvard. She is now Professor of Biomathematics in the Medical School at UCLA as well as the Director of the Division of Biostatistics, Department of Medicine at the Cedars-Sinai Medical Center in Los Angeles. Her son Michael is a graduate student in Biostatistics at Harvard and her son David is an undergraduate at MIT. My other daughter, Kathleen Dixon-Nebert, studied at Barnard and then obtained a Doctorate from the University of Rochester. She is now Associate Professor of Environmental Health and Head of the Division of Molecular Genetics at the University of Cincinnati Medical School. She and her husband, Daniel Nebert, specialize in research on molecular genetics and ecogenetics. They have four children, too young to categorize as to their professions.

Flournoy: What do you do for recreation? Are you slowing down at all?

Dixon: My wife Glorya and I keep contact with my two daughters and six grandchildren and her four children and five grandchildren. I continue statistical research activities at UCLA and consult with numerous research projects through the Dixon Statistical Associates. These projects include research on AIDS, drug effects, clinical trials of many types, etc. I continue as Chairman of the Board for BMDP Statistical Software Inc. However, I am working hard on slowing down.

Flournoy: Thanks so much for providing us with such a rich history of both your life and the development of much of statistics.