

# After 50+ Years in Statistics, An Exchange

Jerome Sacks and Donald Ylvisaker

*Abstract.* This is an exchange between Jerome Sacks and Donald Ylvisaker covering their career paths along with some related history and philosophy of Statistics.

*Key words and phrases:* Research areas, design of experiments, data, applied statistics, research programs, Statistics departments.

Jerome (Jerry) Sacks was born in 1931 in the Bronx. He graduated from Cornell, with a 1952 B.A. and a 1956 Ph.D. in Mathematics. His dissertation, with advisor Jack Kiefer, was “Asymptotic Distribution of Stochastic Approximation Procedures.” From 1956 until 1983 he taught at CalTech, Columbia, Northwestern and Rutgers. In 1983–1984 he was Program Director for Statistics and Probability at NSF. He returned to Academia as Head of the Department of Statistics at the University of Illinois, until 1991, when he became Professor at Duke. At the same time Sacks became the founding Director of the National Institute of Statistical Sciences, a position he held until 2000. When he stepped down the NISS Board of Trustees established the Jerome Sacks Award for Cross-Disciplinary Research to honor Sacks’ service. In 2004 he retired from Duke. Sacks is a Fellow of the IMS, the ASA and the AAAS, and a recipient of the Founders Award of the ASA. During and after his work at NISS Sacks studied highly complex scientific problems such as circuit optimization, traffic simulation and air pollution measurement, using both design strategies and computer models.

Donald (Don) Ylvisaker was born in 1933 in Minneapolis. His B.A. in Mathematics and Economics was from Concordia College in 1954, followed by an M.A. in Mathematics from the University of Nebraska in 1956 and a Ph.D. in Statistics from Stanford in 1960. His dissertation, with advisor Emanuel Parzen, was “On Time Series Analysis and Reproducing Kernel Hilbert Spaces.” From 1959 until 1968 he taught at Columbia, New York University and the University of Washington. He then moved to UCLA where he was head of the Division of Statistics in the Department of Mathematics until his retirement in 1996. Ylvisaker has served both the IMS and the ASA on many committees and in many functions. He has done major editorial work for several of the leading statistics journals. Between 1990 and 2008 he was involved with advising the Commerce Department on Census adjustment and evaluation, he is a long-term advisor of state lotteries, and he has been involved in projects counting the homeless population and in the sensible use of DNA evidence in the criminal justice system. At UCLA he was instrumental in the 1998 establishment of a Department of Statistics, separate from Mathematics. Ylvisaker is a Fellow of the IMS and the ASA.

The conversation reported below is not a unique event. Sacks and Ylvisaker have been friends and collaborators for a long time, with a very distinguished list of joint publications, written over more than 30 years. Perhaps the most influential ones have been the papers on design aspects of regression problems, which started in classical mathematical statistics and eventually came to include calibration, response surfaces and computer experiments. As documented in the conversation below, we see the emphasis in the publications of both Sacks and Ylvisaker shifting from more theoretical topics, such as stochastic approximation and reproducing kernels, to papers using a more applied and computational approach, which are motivated directly by actual advice and consultation.

---

*Jerome Sacks is Director Emeritus of the National Institute of Statistical Sciences, PO Box 14006, Research Triangle Park, North Carolina 27709, USA (e-mail: [sacks@niss.org](mailto:sacks@niss.org)). Donald Ylvisaker is Professor Emeritus, Department of Statistics, UCLA 8125 Math Sciences Building, Box 951554, Los Angeles, California 90095-1554, USA (e-mail: [ndy@stat.ucla.edu](mailto:ndy@stat.ucla.edu)).*

Jan de Leeuw  
September 2011

**JS:** We met in 1959 at the Department of Mathematical Statistics at Columbia. You had just arrived from Stanford as a fresh Ph.D.; I had been there for two years. I don't think Departments of Mathematical Statistics exist anymore in the U.S. There are a couple in Australia and England, and maybe in some places nobody hears about. The Columbia department morphed into a Department of Statistics and, by now, there is a whole swarm of names in use: Statistical Science, Statistical Sciences, Mathematics and Statistics, Statistics and Operations Research and who knows what else. I'm not sure how the name Mathematical Statistics came about, but the signage certainly suggested that "data enter at their peril; theory is done here—for applications go elsewhere."

**DY:** We were trained as mathematical statisticians certainly (though I had managed two summers at Allison Division of General Motors, involved with data on the X-ray determination of stress in metals). In fairness to those times, there was a lot of interest among the mathematically oriented in fresh areas with considerable practical importance: reliability, queueing theory, inventory problems, flood/insurance risks and the like. Still, data didn't have much of a presence; the thinking was closer to "suppose there is a person with these data and this problem, we will solve this problem." It was really a question of matching for a time—getting theorists together with practitioners who had significant data issues.

So, maybe serious treatment of data was not prominent in our circles until we were middle-aged, but this is not to say that all such issues are absent today. Recall that when we were looking at nominations for the Mitchell Prize under the standard of "an outstanding paper where a Bayesian analysis has been used to solve an important applied problem," one could safely discard quite a few methodological works that had little direct connection with an honest problem. Too commonly one finds papers that propose a new technique and then tout its performance on a data set rescued from another time or place. Still, everyone has to operate at some remove from the data, lest there be nothing with which to go public.

**JS:** Of course applications and data were the stuff of concern for many in those years—sampling was always there, serious quality control problems were being attended to, designs for engineering and agricultural experiments were on the table, as well as many other issues in, and especially outside, academic circles. But in the rarified climate of Cornell Mathematics (where I got my degree), Stanford Statistics (where you

### COLUMBIA IN 1959–1960

*The regular faculty were Ted Anderson (chair), Howard Levene, Herb Robbins and Lajos Takacs. Anderson had been there since 1946, as had Levene; Robbins arrived in 1953 from the North Carolina, following a year at the Institute for Advanced Studies. In 1959 Takacs came from England, a way station following the Hungarian revolution. Other faculty appointments were Ron Pyke, Jerry Sacks and Don Ylvisaker, while Joe Gani and Harold Ruben were, ostensibly, visitors. More widely at Columbia, Rosedith Sitgreaves was at Teachers College and Cy Derman was in Industrial Engineering.*

*The department had offices scattered over three floors of Fayerweather Hall, abutting Amsterdam Avenue at 117th Street. Helen Bellows handled the entire administrative load and did the technical typing as well. Full-time students may have had a common room, but they mostly appeared for classes and seminars. Among them were Ester Samuel-Cahn, Gideon Schwarz, Joe Gastwirth, Ted Matthes and Lakshmi Venkateraman. There was, as well, a healthy traffic in "night school" students, notably Peter Welch, who came down from IBM.*

*The younger faculty and visitors interacted a great deal, in and out of seminars, often joined by Benoit Mandelbrot, Y. S. Chow and Dave Hansen from IBM. Short-term visitors (Kai-Lai Chung and Aryeh Dvoretzky) and seminar speakers (Alan Birnbaum, Tom Ferguson, John Hartigan, Cuthbert Daniel) added to an already spirited atmosphere. Most memorable was Sir Ronald Fisher: as cantankerous as rumored and into the tobacco/cancer debate.*

*The level of activity centered around the department that year led to several long-term alliances, that of Chow and Robbins, for instance. At the end of the year, following a variety of misunderstandings between senior and other faculty concerning personnel and future plans, Pyke went to the University of Washington, Sacks headed to Cornell and then Northwestern, Ylvisaker went to NYU and then Washington, Gani returned to Australia, and Ruben became head of the statistics department at the University of Sheffield. The wholesale exodus seemed an unfortunate outcome at the time.*

got yours) or Columbia Mathematical Statistics (where we met), what mattered more was the ability to advance the basic theory of statistical reasoning.

I think that reflected the post-World War II era, a time when structure, ambiguity and abstraction were major forces guiding intellectual movements. Even if the dynamics of abstract expressionism, jazz, “chance-composed” music of John Cage, beat poetry, theater of the absurd or new wave cinema might not fit neatly in that box, their characteristics of randomness, uncertainty and subjectivity might resemble those forming the attraction and development of statistics. Statistics was caught up in efforts to seek structure (Wald’s *Theory of Statistical Decision Functions* in 1950, Savage’s *Foundations of Statistics* in 1954), while, of necessity, pursuing the “jazz” of data analysis. It would be nice if some intellectual historian could explore and analyze these connections—I’m not equipped to do that.

The 1950s were exciting times for those of us who came to “life” then. The tension between theorists and practitioners, new departments and expansions driven in part by Sputnik and the (overly optimistic) hope that decision theory would resolve all philosophical (and practical) disputes helped foster an environment that enabled statistics to flourish. Jack Kiefer’s optimal design paper in the 1959 *JRSS*, along with the ensuing discussion and rejoinder, provides an interesting snapshot of that statistics world.

**DY:** For me, it was coming to “life” in the microcosm of the times that was statistics at Stanford in the mid-1950s. Statistics was then regarded with some interest by mathematicians for its game theory and probability connections (Sam Karlin came to statistics for a while, for example), as well as by economists and others (Kenneth Arrow and Pat Suppes were often seen around the Stanford department, for instance). These were heady, energetic times for Statistics, suggestive of an era of great progress. Yet these good feelings seemed to flag in the early 1960s; overall respect for statistical problems waned as mathematical statistics was found too hard and items like Inventory Theory were rather easily “resolved.”

**JS:** Math departments seemed eager to hire statisticians in the 1950s, albeit the more theoretically inclined. Certainly the increasing demand for teaching statistics was a factor, and the proximity of interests in statistics and probability at that time was another. Though this alliance of interests weakened in subsequent years, it provided a measure of acceptance for statisticians within mathematics then (after all, Kolmogorov was everybody’s “daddy”), and a number of prominent probabilist/mathematicians dabbled, and more, in statistics, for example, Joe Doob, Mark Kac, Kai-Lai Chung and Sam Karlin.

### THE 1960 BERKELEY SYMPOSIUM

*For six weeks in the summer of 1960, an extraordinary group of statisticians and probabilists met at the fourth of six symposia, held at five-year intervals at UC-Berkeley. This symposium marks a high point of the widespread interest in the more mathematical aspects of statistics and probability. In the preface Jerzy Neyman notes that “the present Proceedings are much richer than those of the earlier Symposia because of the several contributions from members of the great Russian school of probability.” In four volumes, the Proceedings of the Symposium contained over 100 works, including such classics as “Nonincrease, Everywhere of the Brownian Motion Process,” by Dvoretzky, Erdos and Kakutani, and “Estimation with Quadratic Loss,” by James and Stein. Linking via <http://www.lib.berkeley.edu/math/services/symposium.html> details the dimension and character of all the symposia and speaks to the special nature of the fourth one.*

*Of countless unrecorded memorable moments at the meeting, the comment by Harold Hotelling following Doug Chapman’s lecture on “Statistical Problems in Dynamics of Exploited Fisheries Populations” stands out. Hotelling gave a lengthy, erudite exposition, “Fish, as Symbol” with stress on the mythic and religious to complement the secular content of Chapman’s talk.*

**DY:** Whatever nuances one places on the research interests of the era, and despite the excitement generated by its seminal results, it is now a time that seems not all that well remembered for its people. Erich Lehmann died recently at 92, and there are long-established researchers around who have not much sense of him and his work, as just one example. Perhaps it was the timing of serious, innovative statistical work in the post-war years that brought out what were to us the huge personalities of the 1950s; one can compile a pretty long list, and one had the feeling that there were many chiefs and not so many Indians around. While their personae remain vivid to those around at the time, statistics has now gone off in so many directions that there are now few “giants” to be readily discerned.

(As a footnote to research in the 1950s, I heard a computer scientist give a talk the other day in which sufficiency and Rao-Blackwell entered without further ado—and we thought those topics were goners after data analysis and robustness came to the fore.)

**JS:** Looking back to those times makes me reflect on how (I'm afraid to ask why) we got interested in statistics and what influenced our directions. In my case I was an undergraduate mathematics major and became curious about statistics from an offhand remark of my brother who had come into contact with Cuthbert Daniel while working at Oak Ridge and was impressed enough to suggest that statistics was onto something (little did I know). Then, in my senior year, Kiefer and Jack Wolfowitz joined the faculty at Cornell and, between course work and paper grading, I became more involved and was encouraged to stay on as a graduate student.

The mathematics department at the time was not very large (maybe 20–25 faculty). There were two statisticians (Kiefer and Wolfowitz), a few probabilists (Chung, Gil Hunt and Kac) and fewer students (Bob Blumenthal and I were the only first-year students interested in statistics or probability; Dan Ray was finishing his Ph.D. at the time). The small and close atmosphere in the department had two effects on me: it forced a fair amount of independence on me, and it provided a strong intellectual influence. At the same time, I shouldn't slight the fact that there was a strong group of statisticians across the Cornell campus, some of whom had been there before I started graduate work (Walt Federer, Iz Blumen and Phil McCarthy), others who had just arrived or were visiting (Bob Bechhofer, Charlie Dunnett, Lionel Weiss and Milt Sobel) and probably others that I fail to recall. It was a pretty intoxicating atmosphere with such a variety of statisticians around and an array of year-long or summer visitors (Feller, Kakutani, Bochner, Dvoretzky and Erdős), but mainly was so to me because of the dynamism of Wolfowitz and Kiefer.

**DY:** In my case, it was always natural to take mathematics courses when in school; everything else seemed mundane by comparison. While shoring up my undergraduate background in a master's program at the University of Nebraska, I gravitated to Fred Andrews, a recent statistics Ph.D. from Berkeley who had also spent some time at Stanford. All to the good, he got me to work hard and then encouraged me to go on to a Ph.D. program. In those days (1956), one applied to North Carolina, Berkeley and Stanford, and then had a choice among them. I was taken by the thought of heading west to Stanford and became part of their first large statistics class—ten full-time students came to campus that year (Bill Pruitt and an older Frank Proschan among them), joining two continuing full-time students—Don Guthrie and Rupert Miller.

Student camaraderie, an engaged and approachable faculty, streams of visitors and related faculty passing through Sequoia Hall, with its trafficked corridors and unpretentious offices, contributed to the exciting place Stanford was in those years; you can well imagine the effect this had on a student from a small Minnesota college. The lively research topics were sequential design (Herman Chernoff), admissibility (Charles Stein), total positivity (Karlin), and reproducing kernel spaces and time series (Manny Parzen). Manny agreed to take me on as his first student, and I made it through school.

The serious mathematics/theoretical statistics training we got served the two of us well throughout our careers. I have always attributed the "Mathematics as a secret weapon" thought to Art Owen—a point well made. At the end of the day, the whole Stanford experience was great for me, but then it was time to set out for life as a "grown-up," to Columbia in the Fall of 1959.

**JS:** It is interesting that it was the combination of our backgrounds that led in 1959 to our collaboration: you were close to the innovations by Manny Parzen in formulating time series analysis, and I was aware of the seminal work of Kiefer and Wolfowitz on optimal experimental designs. It sure didn't hurt to have had the mathematics training that enabled easy communication between us, especially of the function space ideas.

**DY:** I recall that you gave a seminar on Kiefer and Wolfowitz's *Annals* regression design paper, from the June '59 issue, at the start of the school year, and I wondered aloud to you about the possibility of doing something related when errors were correlated. Guess we got past that question after some 15 years.

**JS:** One of the first reactions to your question was in thinking about how we might optimally sample a Brownian motion. I don't think we concluded much at the time, but, after a few feeble starts, we managed to come to grips with the issues in the early 1960s. By then both of us had left Columbia—you were in Seattle (U. of Washington's Mathematics Department) and I was in Chicago (Northwestern's Mathematics Department). I continue to be surprised that we were able to make any headway on the problem at all, and even more surprised that the asymptotically optimal designs we found for polynomial regression with Brownian motion errors were intimately connected with optimal designs for numerical integration. This even gave us some street cred in the applied math/numerical analysis world.

We carried on this collaboration at long distance and with some visits (mostly you to Chicago). Sometimes the distances were extreme, with me working

on the beaches in Acapulco while you were “sweating” it in Seattle. Later on I was “amused” when the well-known mathematician Stephen Smale had research funds withheld by the NSF for saying he did his best work on the beaches of Rio (actually, I think it was his outspoken support of the Free Speech Movement at Berkeley and the attention of the House Un-American Activities Committee that led to the loss of funds). Still later, in 1985, Smale had an encounter with some numerical analysis problems (dubbed Computational Complexity by Joe Traub and company) and rediscovered some of our results on numerical integration in an article in the *Bulletin of the American Mathematical Society*. I sent him a note along with one of our reprints; he never answered.

**DY:** It was indeed surprising to make progress with the (infill) asymptotics of our design problem. You taught me a lot in that process and, it can be added, forced me to work awfully hard when it came to many parameter extensions. The structural things that I knew more about, such as the connection of splines with Brownian Motion (or, more generally, processes with their kernel sections), were hardly deep, but it took a long time to understand that they could be posed in a way that would be interesting to people. Thus, a short distance from our regression problems to quadrature, but several years before we wrote it down in that fashion, and more years before quadrature surfaced as one of the basic problems in what had come to be called complexity theory.

Getting back to the state of Statistics in the late 50s, expansion showed up in various ways. IMS meetings were in those days, for example, written up in the News and Notices section of the *Annals*, with the full list of attendees. Imagine attempting that for a JSM today. There is an interesting history in how departments emerged and grew, one that Alan Agresti and Xiao-Li Meng are now compiling at <http://www.stat.ufl.edu/~aa/history/>.

By 1960 the mathematical statistics of the 1950s had lost some of its attractiveness, but Tukey’s call to data analysis, and the related follow-up in the form of the more theoretical robustness questions sparked a new path for the 1960s. None of this was entirely new, but who could forget Tukey’s talk of 1961 and paper of 1962, or Huber’s thesis of 1964? Are there other landmarks of the decade if one sticks to the central thread we are on? True, there were deep admissibility results that continued the “statistics as math” thing, but I have the sense that people were searching for things to do

with themselves after the basic theoretical problems that remained were found to be too tough.

**JS:** Looking back at the 1960s, I get a sense that while the world around us was blowing apart (civil rights movement, Vietnam war, assassinations of John and Robert Kennedy and Martin Luther King), the profession and its activities were moving independently in the way you described with, I think, one exception: Bayesian thinking was emerging more strongly and creating some tension with frequentists. I didn’t feel much of this, possibly because I had less sympathy with “fundamentalism” and more interest in responding to questions raised in the context of existing or developed structures, such as those we addressed in the work on designs when errors are correlated. At any rate, I had trouble in determining who was in charge of fixing the prior distribution.

**DY:** I have always had trouble with being lectured on the right way to think, and the Bayesian evangelists of the 1960s were very active. I have no problem with Bayesian ascendancy (do you recall the review we got some years back, to the effect that “It’s nice to find an intelligent Bayesian paper again . . . but . . .”), yet am stuck in the belief that the statistical issues in a problem precede a philosophical/methodological stance on its treatment. Some situations, generally highly complex settings, demand priors (Toby Mitchell persuaded me of that in his gentle, nonpedantic way), but my applied work has most often been close to sampling and design, and correspondingly far from posing a need for, or justification of, a prior distribution.

Thinking ahead then to the 1970s and beyond, life in research and teaching broadened for each of us; there were added administrative jobs and more involvement with applied statistics. While this began to get serious in the early 1980s, the question I would raise is, did something happen in the 1970s? Early on, at least, these were “between” years for me, personally and professionally. Perhaps it was also something of a forgettable decade for statistics generally. By its end, one could point to Empirical Bayes, the wholesale onset of smoothing problems in their modern guise, the Bootstrap and, more generally, to the increasing rumblings of the computing revolution; a quiet interlude nonetheless?

**JS:** I suppose there are many who will argue that a lot happened, but I agree with you that the decade of the 1970s was more important as a prelude to the more explosive developments of the 1980s. It is convenient to claim so as a generalization of what was happening

personally, but I think, rather, that it was a more general phenomenon.

In the mid-1970s I was drawn into statistical issues directly spurred by applied problems. I suspect the estrangement between reality and my work was due to living in a mathematics department, or maybe was a hangover of the spirit of the 1950s. In any case, it was around then that I came into contact with Bob Boruch and Don Campbell, social psychologists at Northwestern. Interestingly, it was Cliff Spiegelman, getting his Ph.D. in the math department, who pulled me into that milieu and into Campbell's work on quasi-experimental and regression discontinuity designs. (Never underestimate the power of an imaginative graduate student to light the paths of senior professors.) Though I never published anything directly, Spiegelman did. You and I also produced some work on smoothing methods that grew out of these problems. The more significant thing, for me at least, was being part of a conversation that stimulated my thinking (and maybe some of theirs) about the critical statistical issues faced in evaluating social policies and innovations (e.g., Head Start). It also opened me up to influences from people who had modest technical expertise, yet had an incisive intuitive understanding of the statistical nature of their data.

**DY:** With age comes wisdom, or the times demanded it? I got involved with legal work, lottery consulting, census and various other matters that became increasingly interesting to me and worth the time spent.

**JS:** Practice seemed to me to come in (at least) two forms. There were applications within the scientific research world, and others that stemmed from sources like those you mentioned. The former applications were relatively easy to transition to, in principle, but the others brought different issues that depended very much on personality and politics. I did get caught up in some employment discrimination cases, and later had an extensive involvement with voting rights cases in the 1980s. Sorting out and explaining statistical subtleties, or even crudities, to a mixed bag of intelligent but quantitatively semi-literate clients, lawyers and judges, while being challenged and scrutinized by opposing experts of varying degrees of sophistication, forces one to have a firm and critical view of just what statistics is about. It's comparatively easy to prove a theorem by imposing the right assumptions; it's another story to justify assumptions to a suspicious antagonist or decision maker. I suspect we can regale each other with stories of "experts" unable to do elementary arithmetic, judges willing to admit probabilities of 1.14,

lawyers engaged in Bayesian dialogue and so on. (The last named actually took place in a deposition in a voting rights case: Sam Issacharoff, an able lawyer and currently a professor at the NYU Law School, fenced with me about why I wouldn't do or accept a Bayesian analysis. I forget who won.)

**DY:** I always thought that a principal reason for being involved with legal matters was the need to keep Bayesian analyses out of the courts. Mannered subjectivity was even to be foisted on jurors in the form of "choose your own prior probability" so that the proffered crank could be turned—evangelism of the 1960s now brought forward for the masses.

In the 1980s, statistical testimony was regularly offered as to questions of discrimination by race, gender and age in such areas as employment, wages, housing, jury selection, sentencing and voting rights. I was involved in several cases during this time and, yes, once in a voting rights case with David Freedman, Steve Klein and you. In that instance, the opposition employed an ecological regression that had Stockton's diverse citizenry composed of two politically cohesive racial groups—blacks and Hispanics on the one hand, whites and Asians on the other!

The atmosphere changed somewhat with the landmark 1987 decision in *McCleskey v. Kemp* in the wake of the Baldus death penalty study, for it brought heightened standards for "statistical" relief: the "racially disproportionate impact" in Georgia death penalty sentencing, indicated by a comprehensive scientific study, was not enough to overturn the guilty verdict without showing a "racially discriminatory purpose." One commentator had it as "the Dred Scott decision of our time." However viewed, the statisticians' discrimination landscape underwent a considerable change.

Another active area began in the 1970s with the reporting of blood and tissue typing tests as evidence of culpability. DNA analyses were then a leap forward in this same vein when introduced in 1987, shortly after they became available. Statistics enters the discussion only through population genetics, and in a rather cursory fashion. The tests, as evidence in court, brought out a fierce battle when DNA analyses were first invoked, prematurely in my opinion. This was played out quite publicly in the 1995 O. J. Simpson trial. Things subsided a good deal after the second of two NRC reports on its use, in 1996. Unfortunately, to me, common sense had lost out and the "product rule" is now practiced with little added thought; it likely will continue to be used until genetic advances finally eliminate the need for silly calculations.

**JS:** The changes in attitude and involvement with practical issues we both experienced left me, by 1980, restless and dissatisfied with the same old same old academic pursuits in a mathematics department. I began looking around to change circumstances, and things moved at a rapid pace. Pivotal for me was a decision to go to the NSF as a program director in 1983.

Jack Kiefer's untimely death in 1981 had had a profound effect. Not only was he a close friend but a man who shared his ideas and encouraged others to take on new challenges. He had previously suggested that I go to the NSF to take a hand in advancing the cause of Statistics in Washington. After his death I thought more about doing so, and in the early spring of 1983, Ingram Olkin and Don Rubin pressed me to take on that job. One thing that drove me was the perceived chance to affect the future development (read funding) of statistical design of experiments. With Kiefer's death the leading figure in the field was gone and it was unclear how and in what way future efforts would proceed. In fact, as you may recall, at the Neyman-Kiefer Symposium in 1983, and again at the annual JSM meetings in the summer of 1983 in Toronto, several of Jack's friends and collaborators (Ching-Shui Cheng, Toby Mitchell, Henry Wynn, you and I) talked about where the field was going. None of us saw a strong direction at that time and we thought it valuable to pursue ways to energize thinking about this. You and Ching-Shui followed up by putting together a proposal to stage a series of four workshops on design. These were funded by NSF and held at Berkeley and UCLA in 1984–1986.

**DY:** It is hard to properly account for Jack's influence on us all, but one could start by bringing out his ideas and technical strengths, his personal warmth and generosity. He managed the combined role of mentor and friend with remarkable grace, and there we were, lost for both his leadership and his companionship.

Working with Ching-Shui, laconic but with much to say, on the planning and implementation of the workshops, was a new experience for me, and a joy. The first, held at Berkeley in the summer of 1984, brought together researchers with a fairly broad spectrum of interests; the summer workshop in 1985 at UCLA hosted a truly wide array of interesting people (among them Rosemary Bailey, Grace Wahba and Don Rubin) and topics (climate research, survey design and nonresponse, and product and process design for manufacturing, as examples).

**JS:** The last workshop in January 1986 was an important one. In fact, at the end of the workshop I enlisted Henry Wynn, over sushi at a restaurant near the

UCLA campus, to help draft a research proposal to formulate and attack problems on statistical issues in computer experiments. This helped start a whole program of research at Illinois and elsewhere—the workshop had some real influence.

**DY:** It was the most focused of the four workshops. In setting up the program, I was able to rely on Toby Mitchell, who had been thinking of these things for some time. This was my first opportunity to work with (and appreciate) Toby, and the resulting program was an early and distinguished entry in what was soon to become a central research area.

**JS:** There is something, less obvious, to be learned from that experience. The NSF was, probably still is, most often regarded as a source of funding for ideas generated within the discipline. What is perhaps less noted is the catalytic effect of NSF; the stimulus provided by NSF to you and Ching-Shui is a nice example of that.

**DY:** It does seem that the NSF is presently far more involved in the pushing of broader research agendas than in the years prior to your tenure there—thus, cross-disciplinary areas might be identified for specific grant monies as opposed to the classic method of soliciting individual research proposals. Surely, as with upstream design for a manufacturing process, this is a sensible method for shaping and facilitating research programs. In this vein, it is crucial that statistics is suitably championed and, in the complex and shifting statistical research environment one currently sees, that its sub-areas come under a wise focus. Easier said than done.

**JS:** The year at NSF put me on another trajectory: I went to Illinois in 1984 to lead the establishment of a new department of statistics, and also became increasingly involved in subject matter issues. A lot of my time at NSF was spent with scientists from outside statistics and mathematics and I began to sense, along with some others, that our field could and should be energized by serious interest across disciplines. This was not fully appreciated by all, but it did resonate at UIUC with some enthusiasm for joint appointments and enterprises. Also, in 1984–1985, I helped start a study about cross-disciplinary research in statistics that led ultimately to a recommendation of an Institute devoted to that purpose.

**DY:** Coincidentally, 1984 marks an effort toward cross-disciplinary statistics at UCLA that began with a proposal by social scientists to hire some six statisticians in their division. The statisticians then located in the mathematics department sought some involvement

in that process, at the very least. The dean of Physical Sciences was not sympathetic to us, but the dean of Social Sciences shortly set up a statistics division in the social sciences program, one that brought some cohesiveness to the process. Most importantly, Jan de Leeuw was recruited to run it from a joint appointment in Psychology and Mathematics. Much energy having been generated, a division of statistics was formed within the Mathematics Department in 1986, leading eventually to the birth of the Statistics Department in 1998. In this last development the (then) dean of Physical Sciences was highly instrumental, being persuaded that Statistics was an honest endeavor of great interest to many in the university. Of course, he was right.

**JS:** The cross-disciplinary theme emerged more gradually as an influence in the 1980s when compared with that of computational developments. Both continue to underpin attitude and focus in the field. Strangely though, some advances in computational power, like supercomputing, were slow to be recognized by our colleagues, and the rapid, innovative adoption of statistical methods and ideas by computer scientists (and others) was not quickly digested. To a degree, this gave rise to some thinking of the need to push the field. I became especially aware of these things when I was at the NSF.

**DY:** I was no monitor of the changing times, certainly, but can offer before and after pictures from UCLA. When I was a vice chairman of undergraduate affairs in the math department in 1971–1973, we proposed an undergraduate degree in applied mathematics to sit alongside the one “pure” math option available to students. The proposal was promptly laughed out of the faculty meeting, probably without a vote being taken, for it would have allowed some students to graduate without a differential geometry course! There were only two or three applied faculty to defend or implement things at the time, and they had to vie with the slightly more numerous statistics group for respectability in the department. It was several years before such an applied major was instituted.

Fast-forward to the present to find just over 200 majors in each of pure and applied math, and 90 students enrolled in the new undergraduate statistics major. Since the 1970s, the applied mathematics group in the Math Department has grown considerably, is awash with money and prestige, and is now ranked about third in the country. The Statistics Department dates to 1998, the FTE count has roughly doubled since then, and the student population has gone through the roof.

Of particular note, there has been a considerable movement of mathematicians into problems we have thought of as statistical, at least to some degree. For example, for the years 2009–2011 at the Institute for Pure and Applied Mathematics at UCLA, one finds programs on “Model and Data Hierarchies for Simulating and Understanding Climate,” “Mathematical Problems, Models and Methods in Biomedical Imaging,” “Statistical and Learning-Theoretic Challenges in Data Privacy,” and “Navigating Chemical Compound Space for Materials and Bio Design.” Some statisticians have shown up in these programs, but not many.

Of course the big news is, and should be, the data themselves: huge increases in availability, much improved recognition of the need for the understanding of basic statistical concepts in “everydata” problems, and the astonishing growth of analytic tools mindful of new age data sets and rapid computational improvements.

In this expansion, the design and analysis of computer experiments has been a special interest of ours. The early framework papers that grew out of the workshops in the 1980’s, already with an eye toward various engineering problems, are now heavily cited in many areas in which the simulation of complex systems is practiced. One would like to think that the ideas in them, and beyond citations, are put to use in the kind of experiments that get written up as internal company or laboratory reports on specific projects. Since the late 1980’s you’ve been a lot closer to the “factory floor” in this regard than I have.

**JS:** You are right to point out that the development of computer experiments coincided with the attention to cross-disciplinary work. Statisticians don’t “own” computer models and dealing with computer experiments means collaborating with the subject matter people who use the models. It was natural to be engaged simultaneously with the computer experiment research and the efforts that led to the establishment of NISS with its mission of fostering and doing cross-disciplinary statistical research.

**JS:** When I look back at the history of NISS’s creation, I am struck by the number of leaders of our field, and outsiders as well, willing to engage in and support such a venture. Of course there wasn’t unanimity, but the story does reflect a willingness of leadership to push boundaries despite low odds of eventual success (even in retrospect, investing with Bernie Madoff might have been a safer bet). That characteristic, surely not unique to our field, may have some roots in our having to claw our way into the consciousness of



established authorities (see your experiences at UCLA and everybody's everywhere else).

**DY:** I suppose only the older persons among us have the time to fret over the status and stature of statistics, the young are hard at work on "doing it." Still, the pushing of institutions like NISS needs incisive goals, thorough planning and plenty of clout. There, it seems, one needs age, experience and foresight.

**JS:** And a measure of luck. Little happens from just plain intention—help is needed from many sources. We typically focus on the advances in the intellectual arc of statistics and pay less attention to the politics affecting us and others. The "local" politics exemplified in the creation of NISS is minor compared to the connections statistics has with the serious economic, social and political matters of our time. These connections need much more attention. There are some books and occasional articles, but I don't think they capture the bigger and critical picture of what we are about.

**DY:** A political case in point is the census adjustment controversy that began in the early 1980s, peaking over the 1990 and 2000 censuses. The pro-adjustment view was the dominant one: a viable method was in place to better the process of counting every person. There was a good deal of informed opinion in this direction, and much uninformed support in the statistical community—the capture/recapture story is readily recounted, but its use in the census context is far more complex than that. The other side emphasized, among other things, the heterogeneity of the post-strata that were central to the adjustment methodology. The discussion of the technical issues was greatly complicated by the Democrats' support of adjustment and the Republicans' opposition to it. The media had a field day over the matter, and little of benefit accrued to the Bureau of the Census, or to Statistics.

My own involvement with the Census Bureau—contracts, contacts and NRC panels—lasted close to twenty years, beginning with the 1990 census. I was in the camp that held the nonadjustment decisions of the 1990 and the 2000 Censuses to be proper; in the latter case, the bureau agreed with that position at the last moment, especially given problems with duplicates. In all, the more one is around the bureau, the more respect one has for the tasks it is given, and for the host of talented people who work toward its goals.

**JS:** The census issues, along with the DNA and voting rights experiences we talked about earlier, hit a nerve. I don't think it too idealistic to want statistics to appear in these contentious settings as objectively as possible. The rush to employ sophisticated, or not so sophisticated, methods under tacit

## ORIGINS OF NISS

*At the 1984 annual IMS meeting in Tahoe, California, discussions about the future of the field among David Moore, Ingram Olkin, Ron Pyke, Jerry Sacks, Bruce Trumbo and Ed Wegman led to a plan for a report on cross-disciplinary research in statistics. Money was obtained from the NSF, and a panel was formed with Olkin and Sacks as co-chairs. At a meeting in 1987 Olkin proposed the establishment of an Institute to implement ideas around which the panel had coalesced; in time, the proposal became the key action item in the report.*

*How to bring the recommendation to reality began with discussions among Nancy Flournoy, Olkin, Sacks and, most critically, Al Bowker. These discussions led, with the help of Flournoy and Murray Aborn (NSF), to the financing of a feasibility study carried out through the ASA, culminating in a plan to seek proposals from groups around the country (mostly located in the East). Proposals competed not for dollars, but to receive blessing from a committee of statisticians (chaired by Bowker) the proposers had to commit real dollars themselves!*

*A consortium from the Research Triangle area of North Carolina made the winning proposal, committing start-up money, academic positions, land and funds for a building. A host of North Carolina people were involved: university provosts, department chairs, executives at the Research Triangle Institute and others. Two people were critical for the initial effort and for the early stages of growth of NISS. One, Dan Horvitz, had stature in the statistical world and, as retired vice-president at RTI, had significant political contacts. The other, Sherwood Smith, CEO of Carolina Power & Light, had great interest in furthering the development of Research Triangle Park, and his political savvy and connections were instrumental in ensuring the initial commitments for NISS and, a few years later, a renewed commitment by the state to build a "house" for the institute.*

assumptions—Hardy-Weinberg in DNA calculations, ecological regression in voting rights, independence of capture/recapture in census adjustment—that may lack adequate justification is harmful, even when used to advance laudable causes.

**DY:** We do have a PR problem at all levels. The much-improved early training of students in probability and statistics notwithstanding, reaching the public is not easily managed. There is the constant barrage

of social science findings, medical recommendations and the like. Of late, the often-fleeting nature of study results gets a good deal of attention, but it is hard to see how the system and the media will ever reward patience in such matters; we show up as would-be custodians of the peace in these settings, sharing the fate of commentators on rare illnesses, earthquakes, climate change and the like.

**JS:** Books like *How to Lie With Statistics*, *Foiled by Randomness* and *The Black Swan* (at first I thought the last was a late review of an old Tyrone Power, Maureen O'Hara pirate movie—a movie with much more pertinence to the economic catastrophes of 2008 than Weibull distributions) too often leave a sense of villainous activity by statistical practitioners.

Just what can be done to further a nonwarped public perception has been evasive. It is impossible to shut off the supermarket tabloids. And while useful efforts to bring some public sense to vexing reports like those about mammogram screening have appeared in such places as the *New York Times*, none of the journals or newspapers of record has actually undertaken to spotlight the ubiquitous nature of uncertainty and the efforts to cope with it. Individual instances pop up now and again but a coherent discussion, perhaps in a series of articles, would be useful.

**DY:** Beyond public respect, there is the issue of the proper understanding of statistics as a competitive discipline in the new age. Is it clear, for example, what core knowledge should be required of our graduate students? Are there standards for this that would have wide appeal? If not, are there consistent answers to the question of what we are all about? There is a decent sense of where we've come from, maybe much less of where statistics heads.

Which again brings up our history. It would seem that a lot more could be preserved of the story of the growth of Statistics over the past 100 years, and a sense of the people who propelled this. On the positive side one sees a growing interest in doing something about it, the Agresti and Meng project is just one example of this.

**JS:** Thinking about the future of the field should be done periodically, even if lamely. I am struck by the sudden emergence of books and articles (e.g., *The Information* by James Glieck; the special edition of *Science* dated 11 February 2011) about the data flood threatening to drown us or drive everybody nuts. Apart from the need to physically manage the data, the issue of how to analyze them has enormous implications for developments in the field, many of which, of course,

have been in progress for some time and in critical ways (one example: false discovery rates to manage multiplicity in bioinformatics). Still, there is so much going on now that, say, doubling the number of practicing statisticians would still leave unfilled needs.

**DY:** What concerns me then is not so much the progress of statistical technology, call it the Benthamite school as described in your Hazelwood paper with Paul Meier and Sandy Zabell, but the well-being of the “strict constructivist” agenda that claims the other end of the spectrum. Proceeding on the basis of “what is useful is good” allows much latitude for producing new procedures in the light of mushrooming data sets and increased computational power, but at the same time evaluation and validation remain as understaffed pursuits.

Methodological advances clearly outpace their justification. Accentuating this problem, from my perspective, is the flood of research papers that look and sound the same: “Here is our new procedure and these are its asymptotic properties; we have run some simulations and analyzed some ‘real’ data, all of which goes to show that our procedure is better than the other procedures of this type.” All too often, the data set employed is dated and well worked over, and the immediate contribution to overall understanding of the main issues is not demonstrably nonignorable. Against this, one finds that model validation is important but hugely difficult, and evaluation of large and continuing issues of public welfare that rely on statistical information is nowhere near what it should be. Do many graduate programs give serious attention to validation and evaluation? These are tough problems, but when the going gets tough . . . .

You likely think in terms of a broader agenda for statistics that gets toward public policy. Does this fit in such a descriptive framework, as an extra leg perhaps?

**JS:** Yes, most definitely yes, an expanded engagement in evaluation and validation should be part of the field's agenda. Though model validation has surfaced as a critical area in several communities, with programs of “Uncertainty Quantification” that bring out the usual suspects as well as whole varieties of engineers and scientist, uncertainty is as uncertain as ever. Related are issues of evaluation: “just what does this series of studies/analyses imply?” Engagement with these questions (whether in health, environment, education, etc.) is not for the faint-hearted and needs many replacements for David Freedman with the ability and energy to tackle such problems.

Beyond these needs, your comments raise, I think, an issue about how statistical evidence and “proof” are evolving. In the past, mathematical proof and assessment was primary. Today, computer simulations, in some contexts where mathematical argument is unavailable, offer a less austere route; perhaps “preponderance of evidence” versus “beyond a reasonable doubt.” The tendency you note of producing a method and assessing its utility by applying it to a shopworn data set ought to lead to the case being tossed out of statistical court on grounds of insufficient evidence. Presumably, the weight of evidence is increased if the application is made to a spectrum of data sets buttressed by simulation studies. How to devise the spectrum and studies for a “prima facie” case is not apparent but surely worth thinking about.

**DY:** A lot of issues on the plate, but we seem better equipped to look back at this point. Maybe we could reminisce a bit, we’ve covered a lot of ground in 50 plus years. What do you think of in terms of the good and, perhaps, the bad for you?

**JS:** As with everybody there were triumphs and disappointments, wins and losses. Still the feeling that lasts and continues to drive me is that I had, and still have, a part in an exciting trip over meaningful terrain, accompanied by good people (and a couple of scoundrels). I sometimes feel sorry for colleagues in other disciplines who don’t have the opportunity to swing in whatever style comes up, whether it be education, materials science, genomics, lottery draws, climate, baseball—you get what I mean.

**DY:** I found that working on statistics problems of every sort was natural and pleasurable for me. In general, though, the profession itself has served as a comfort zone, and the good of this starts with the people—mentors, students (and especially one’s Ph.D. students), colleagues, collaborators, friends. The list is so long that it is best left unrecorded. OK, you. But seriously, why would one choose to be something other than a statistician?