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A Conversation with Myles Hollander

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Abstract. Myles Hollander was born in Brooklyn, New York, on March 21, 1941. He graduated from Carnegie Mellon University in 1961 with a B.S. in mathematics. In the fall of 1961, he entered the Department of Statistics, Stanford University, earning his M.S. in statistics in 1962 and his Ph.D. in statistics in 1965. He joined the Department of Statistics, Florida State University in 1965 and retired on May 31, 2007, after 42 years of service. He was department chair for nine years 1978–1981, 1999–2005. He was named Professor Emeritus at Florida State upon retirement in 2007.

Hollander served as Editor of the *Journal of the American Statistical Association, Theory and Methods*, 1994–1996, and was an Associate Editor for that journal from 1985 until he became *Theory and Methods* Editor-Elect in 1993. He also served on the editorial boards of the *Journal of Nonparametric Statistics* (1993–1997; 2003–2005) and *Lifetime Data Analysis* (1994–2007).

Hollander has published over 100 papers on nonparametric statistics, survival analysis, reliability theory, biostatistics, probability theory, decision theory, Bayesian statistics and multivariate analysis. He is grateful for the generous research support he has received throughout his career, most notably from the Office of Naval Research, the U.S. Air Force Office of Scientific Research, and the National Institutes of Health.

Myles Hollander has received numerous recognitions for his contributions to the profession. He was elected Fellow of the American Statistical Association (1972) and the Institute of Mathematical Statistics (1973), and became an elected member of the International Statistical Institute (1977). At Florida State University he was named Distinguished Researcher Professor (1996), he received the Professorial Excellence Award (1997), and in 1998 he was named the Robert O. Lawton Distinguished Professor, an award made to only one faculty member per year and the University's highest faculty honor.

Myles Hollander was the Ralph A. Bradley Lecturer at the University of Georgia in 1999, and in 2003 he received the Gottfried E. Noether Senior Scholar Award in Nonparametric Statistics from the American Statistical Association. He was the Buckingham Scholar-in-Residence at Miami University, Oxford, Ohio in September, 1985, and had sabbatical visits at Stanford University (1972–1973; 1981–1982), the University of Washington (1989–1990) and the University of California at Davis (Spring, 2006). The following conversation took place in Myles Hollander's office at the Department of Statistics, Florida State University, Tallahassee, on April 19, 2007.

Key words and phrases: Nonparametrics, Bayesian methods, Dirichlet process, biostatistics, ranking methods, reliability theory, stochastic comparisons, system signatures, writing, editing, administration.

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WHY STATISTICS?

Samaniego: It's a real pleasure to be back at Florida State, Myles. I spent my first postdoctoral year in the Statistics Department here, and I have many fond memories. Though we've been friends for over 35 years, there are many details of your life and career that I'm looking forward to hearing more about. Let's start somewhere near the beginning. I know that you began your college career at Carnegie Mellon as an engineering major. Can you tell me how you got interested in Statistics?

Hollander: I came to Carnegie Mellon, it was Carnegie Tech when I entered in 1957, with the aim of becoming a metallurgical engineer, but all the engineering students took more or less the same curriculum, including calculus, chemistry, English, history of western civilization. As the year progressed I found I liked math and chemistry the best so near the end of the year, I went to see the heads of metallurgy and math. The metallurgy chair was informative but laid back and said it was my decision. The math chair, David Moscovitz, was much more enthusiastic. He said, "Hollander, we want you." Well, I was only 17, impressionable, and I liked being wanted so I became a math major. I didn't encounter a formal course in statistics until my junior year. That year, Morrie DeGroot (who had come to Carnegie the same year I did—1957—he with a Ph.D. from the University of Chicago) taught a course that I really enjoyed. It was based on Mood's "Introduction to the Theory of Statistics." DeGroot wrote some encouraging comments on a couple of my exams and I began thinking I might become a statistician. Then in my senior year, I took two more excellent statistics courses from Ed Olds. Olds at that point was a senior faculty member who had actually done some work on rank correlation but was, I think, more known for his consulting with nearby industry, Westinghouse, U.S. Steel and others. In the afternoon he taught a statistical theory course from Cramér's "Mathematical Methods in Statistics." In the evening he taught a course on quality control. I liked the juxtaposition of beautiful theory that could also be useful in an important applied context. I would say those three courses, those two teachers, sealed the deal for me. Carnegie wanted me to stay on and do my Ph.D. there in the math department but the lure of California, Palo Alto, Stanford's statistics department, was too great, so I headed west.

Samaniego: Let me ask a quick question about the books you mentioned. Cramér is even today thought of as a very high-level book mathematically. It's surprising that it was used in an undergraduate course.

Hollander: In retrospect it is surprising but Olds taught a beautiful course and it helped me later on in my studies. I still have the book in my library and I look at it from time to time.

Samaniego: I see it and it's clearly well worn.

Samaniego: You were attracted to math and science in your early years. Was that your main focus in high school?

Hollander: I was on an academic track in high school and studied mostly math and science. I attended an excellent public high school, Erasmus Hall, in the heart of the Flatbush Avenue section of Brooklyn. It was a three-block walk from my apartment house. Naturally, I also took other types of courses, English, social studies, history, mechanical drawing, and Spanish. Math was my best subject and that seemed fortunate for a kid who wanted to be an engineer.

Samaniego: How did a kid from Brooklyn end up choosing to go to a private college in Pittsburgh? I suppose that once the Dodgers left town, you felt free to leave, too.

Hollander: I could have stayed in Brooklyn and gone to Brooklyn College, thereby saving a lot of money. I could have stayed in New York State and gone to Rensselaer Polytechnic Institute, where several of my close friends chose to go. I wanted something different, and Pittsburgh, despite its reputation then as a smoggy city, due to the steel industry, appealed to me. That the Dodgers were leaving Brooklyn the same time I was (1957 was their last season in Ebbets Field and also my senior year of high school) didn't affect my thinking. I did get to see them play a few times at Forbes Field in Pittsburgh during my years at Carnegie. Forbes Field was actually a short walk from Carnegie and you could enter the ball game for free after the seventh inning.

Samaniego: Tell me about your parents and their influence on your academic development.

Hollander: My mom and dad were committed to education, wanted me to go to college, and worked hard to make it happen. My dad had one year of college. He was at Brooklyn Polytechnic Institute in the 1927–1928 academic year majoring in civil engineering. Then the following year the Depression hit and my father, as the oldest of three siblings, went to work to help support his family. He never got back

to college. My dad went on to open a sequence of haberdashery stores, mostly selling pants and shirts, in the boroughs of Manhattan, Queens and Brooklyn. My mother did not have college training but worked as a bookkeeper, mostly for a firm that managed parking lots throughout the city. They both left early in the morning and came back at dinner time. I was a latch-key kid before the term became popular.

I lived on the first floor of an apartment house on Linden Boulevard, directly across the street from a branch of the Brooklyn Public Library. The library was a good place to study and in my senior year I would thumb through books on engineering. Civil, mechanical, electrical, aeronautical were the popular areas but metallurgy appealed to me: the chemistry labs, blast furnaces, protective masks, etc. I looked for schools that offered it and I also thought that by applying to a less popular field, I would increase my chances of being accepted, and getting a scholarship.

Samaniego: I know you had scholarship support from the Ladish Forging Company while at Carnegie Mellon, and also worked for them in the summers. What was the work like? Did it play a role in your decision to go to graduate school?

Hollander: When I switched from metallurgy to math at the end of my freshman year, I contacted the Ladish Forging Company. They said that was fine, they would still support me, which I obviously appreciated. Then in the summer of my junior and senior years I worked for them in Cudahy, Wisconsin. I estimated the costs of drop forgings using the costs of materials, the geometrical shapes of the parts, labor costs. I did some of that each summer and also wrote some programs in Basic for the IBM 1401. My supervisor told me on the parts I estimated for which the company was low bidder, the company lost money. I was biased low. But he said it was fine because the workers needed the work. Ladish actually wanted me to work for them after graduation but I wanted to study statistics and my heart was set on Stanford. Ladish wasn't my last position in the private sector. In the summers of 1962–1963, after my first and second years of grad school, I worked for the Sylvania Reconnaissance Laboratories in Mountain View. There I did get to use some of the material I was learning at Stanford, particularly Markov chains and stochastic processes. In the summer of my junior year, I had an internship at the Presbyterian Medical Center in San Francisco.

FIG. 1. *Myles Hollander at age 6, Brooklyn, New York, 1947.*

Gerry Chase and I rode the Southern Pacific Railroad from Palo Alto to San Francisco two or three times a week and worked on medical data. Nevertheless, even though I liked these summer jobs, as my years in graduate school increased my inclination to join the private sector decreased.

GRADUATE SCHOOL AT STANFORD

Samaniego: Your graduate study at Stanford heavily impacted your career choices and the statistical directions you have taken. Tell me about your cohort of students at Stanford.

Hollander: It was a terrifically talented cohort. Brad Efron, Howie Taylor, Joe Eaton, Carl Morris, Grace Wahba, Barry Arnold, Jim Press, Paul Holland, Jean Donio, Galen Shorack, Gerry Chase, and many more. I should really name them all. We were all excited about the material. We wanted to learn what our professors taught and we wanted to learn how to do it ourselves. We were very cooperative and friendly among ourselves. I have many memories, Howie Taylor working on (and talking about) a probability problem at the blackboard in our office in Cedar Hall, Carl Morris and I talking about Pitman efficiency at a blackboard in an empty classroom in Sequoia Hall and Carl shedding light on what was going on, Barry Arnold and I discussing a mathematical statistics problem in Cedar—many,

many such instances. Brad Efron was a senior student to our group who interacted with us and helped us in many ways, including discussing geometrical interpretations of theorems. We typically took the qualifying exams in the middle of our third year. To help us prepare, we would each choose a topic and write a 10–12-page focused summary with solutions to problems, theorems, key ideas. I did one on nonparametrics, Howie Taylor did one on advanced probability, and so forth. We put the summaries together, made copies and passed them amongst ourselves. When we took our orals we were pumped, prepared, and, to the extent that one can be for such a momentous test, we were confident. Also, of course, we were nervous. My exam committee was Lincoln Moses, Rupert Miller, Charles Stein and Gerry Lieberman and I see them sitting there today just as I am looking at you and I remember most of the questions to this day.

Samaniego: Give me an example of a question that was asked.

Hollander: Well, Lincoln Moses asked about nonparametric tests for dispersion and I decided to mention one of his rank tests. Then Gerry Lieberman turned to Lincoln and said in mock surprise, "Lincoln, you have a test?" They were close friends so Gerry could tease him in this way but Lincoln wasn't particularly happy about my answer and then he threw a tough question at me about the asymptotic distribution of the Kolmogorov–Smirnov statistic. Charles Stein asked me about decision theory and I was ready for that. I went to the blackboard and outlined the framework of a decision theory problem just like he did at the beginning of many of his lectures.

FIG. 2. *Myles Hollander, with his parents Ruth and Joseph Hollander, Catskill Mountains, New York, 1954.*

FIG. 3. *Myles Hollander with the graduating class of the Mathematics Department, Carnegie Institute of Technology, 1961.*

Samaniego: He didn't ask any testy inadmissibility questions, did he?

Hollander: I had covered the blackboard and used a lot of time but he did ask about the relationship between admissibility and invariance. It had been covered in his course so I was ready for it.

Samaniego: Which faculty members at Stanford had the greatest influence on you, personally and professionally?

Hollander: Lincoln Ellsworth Moses had the greatest influence. I was lucky at the start because my first TA assignment in fall quarter, 1961, was to be a grader in the elementary decision theory course he was teaching out of Chernoff and Moses. He gave the main lectures and five or six TAs graded papers and met with sections to go over homework. I got to know Lincoln through this activity and he also encouraged me to attend the biostatistics seminar

that he and Rupert Miller were giving in the medical school. I would also be invited to his home in Los Trancos Woods and got to know his wife Jean and their children. I was close to him throughout and after he married Mary Lou Coale, Glee and I remained very close with them. Beginning in the fall of 1963, Lincoln taught a two-quarter course on nonparametric statistics. It was a beautiful contemporary sequence and there was lots of nonparametric research in that period, particularly by Erich Lehmann and Joe Hodges at Berkeley, Lincoln, Rupert Miller, Vernon Johns at Stanford. Lincoln named me the TA for that course even though I was taking it at the same time. There I was, grading the papers of my really talented fellow students, like Joe Eaton—and so I had to be good. I was determined to excel, to be one of the best if not the best in the class. Later, motivated by this course, I wrote a thesis on nonparametrics under Lincoln's direction.

Lincoln became my role model, the statistician I most admired and tried to emulate. He showed me how to be a professional, the joy of statistics, and the great pleasure of being a university professor. In my career I have tried to do for my students what Lincoln did for me.

Samaniego: What you say about Lincoln Moses rings very true. From my own few interactions with him, and from things I've heard about him over the years, he was both a fine teacher and scholar and a true gentleman. Tell me about your interactions with other Stanford faculty.

Hollander: I was also strongly influenced by other professors from whom I took courses. Rupert Miller via the biostatistics seminar, Ingram Olkin through the problems seminar he co-taught with Shanti Gupta, who was visiting in 1961 (they started out assigning problems in Cramér's book and that was a break for me as a beginning student because I had seen most of the problems at Carnegie). Ingram also taught multivariate analysis which I also took. I took Charles Stein's decision theory sequence and Manny Parzen's time series sequence. Kai-Lai Chung taught the advanced probability sequence. They were all dedicated to their subjects, made them come alive, each had his own style, and each was at the top of his game. Then on two sabbaticals at Stanford, working in the medical center, I became friendly with Bill and Jan Brown and reinforced my friendship with Rupert and Barbara Miller. Bill and Jan became the godparents to our children. One special bond that existed between Rupert and Barbara and Glee and me: Jennifer Ann Miller and Layne Q Hollander were delivered the same day, October 29, 1964, at Stanford Hospital, and Glee and Barbara shared the same hospital room for three or four days. Over the years, I've grown closer to Ingram through the various international conferences on reliability that you and I have attended and to Manny through his work with the nonparametrics section of ASA.

Samaniego: All of the people that you've mentioned have written very good books in probability or statistics. I'm wondering, since you've co-authored three books yourself, whether these people and the way they wrote influenced you?

Hollander: I did put a high premium on clear writing in the three books I've co-authored. I think the person who influenced me the most in that regard was Frank Proschan, who insisted on clear writing. When I took the course on stochastic processes, it was based on Manny's notes (his book was not yet out) and it was taught by Don Gaver. When I took Ingram's multivariate analysis course, he used his notes and Ted Anderson's book. I used Rupert's book on multiple comparisons for research, but I didn't take that subject as a course. Kai-Lai used the notes that would become his beautiful book on advanced probability. Certainly Manny, Ingram, Rupert and Kai-Lai wrote in clear, captivating ways.

Samaniego: You met your wife Glee at Stanford and the two of you were married in the Memorial Church on the Stanford campus. Many of your friends feel that your bringing Glee into the extended Statistics community is your greatest contribution to the field! Tell me how you met Glee and how you managed to persuade her to marry you. (Laughs)

Hollander: I was sitting in my office on the second floor of Ventura Hall at Stanford in October, 1961. It's a spacious office and even though it had four desks, only two students would come regularly, Jon Kettenring and me. (A year later Pat Suppes would take over that office.) I was working on a hard problem and I paused to look out the window. I saw a young girl walking briskly, determined, in high heels, with blond hair, bouncing along with remarkable energy (past Ventura, maybe to the Computer Center). A California girl! Clearly I could never even approach a person like that. She passed out of my view and I went back to my homework, probably a waiting time problem in stochastic processes. The expected waiting time for me to approach the girl I had just seen was no doubt infinite.

Eight months later, in June, 1962, my friend Heinz, an engineering student from Germany, and I decided to go on a double date. We decided to meet on a Friday night at El Rancho, a restaurant on El Camino Real, in Palo Alto. In addition to dinner, El Rancho also had a dance floor and a lively band. When I arrived I realized that Heinz's date was unmistakably the girl I had seen when gazing from my Ventura Hall office in the fall—Glee.

The evening was going well and I was totally enthralled by Glee, her brightness, her wit, her energy, her enthusiasm, her bounce. After about an hour the band played "It's Cherry Pink and Apple Blossom White"—a cha cha. I asked my date to dance but she said she didn't cha cha. I mustered the courage to ask Glee. She said, "I'll try." Of course she was and still is a great dancer and I was on cloud nine. I thought I'd made a good impression. A week later I called her on the phone and said, "Hi Glee, it's Myles Hollander." She said, "WHO?" Obviously I did not impress her as much as she had impressed me. Clearly I needed to go into high gear. I took her sailing on San Francisco Bay. I took her horseback riding in the foothills behind Stanford. I took her skiing at Heavenly Valley. Eventually my persistence triumphed. We hit it off over a period of about a year, and got married at Stanford Memorial Church on the Stanford campus in August, 1963. We went on to have two fine sons, Layne Q and Bart Q, who, with their wives, Tracy and Catherine, also gave us five wonderful grandchildren—Taylor, Connor, Andrew, Robert and Caroline. Glee earned her Ph.D. at FSU in an excellent clinical psychology program and worked in private practice, and also at Florida State Hospital in Chattahoochee. I like to say it all started with the cha cha and we're still dancing after all these years!

Samaniego: On the statistical front, you published a major portion of your thesis in a pair of *Annals* papers. What was the main focus of this work?

Hollander: My thesis was devoted to rank tests for ordered alternatives in the two-way layout. Lincoln Moses, in his nonparametric sequence in the third year of my graduate work, had covered ordered alternatives in the one-way layout and that suggested to me some ideas for randomized blocks. I proposed a test based on a sum of overlapping signed rank statistics that is not strictly distribution-free but can be made asymptotically distribution-free. Kjell Doksum at Berkeley was also working on closely related problems at the same time and in the end our

FIG. 4. *Myles Hollander as a graduate student at Stanford, 1963.*

two papers were published adjacently in the 1967 *Annals* (Doksum, 1967; Hollander, 1967). In my thesis I also pointed out a certain multiple comparison procedure, thought by Peter Nemenyi (Nemenyi, 1963) to be distribution-free, was not, but could be made asymptotically distribution-free. I published the asymptotically distribution-free multiple comparison procedure in the 1966 *Annals* (Hollander, 1966).

A CAREER AT FLORIDA STATE UNIVERSITY

Samaniego: You've written quite a few papers on classical nonparametric testing problems. Give us an

FIG. 5. *Myles and Glee Hollander, starting life together after being married at Stanford Memorial Church, August 17, 1963.*

idea of the range of problems you have worked on in this area.

Hollander: In my early years at FSU I wrote nonparametric papers on bivariate symmetry, regression, uncorrelated nonparametric statistics, and did a little more on ordered alternatives. I also worked with my first Ph.D. student, Ron Randles, on a paper that was decision-theoretic rather than nonparametric. We developed T-minimax procedures for selection procedures and it was published in the 1971 *Annals* (Randles and Hollander, 1971). Ron took my class in nonparametrics and even though his thesis was not nonparametric in character, he did excellent work, went on to be a leader in nonparametrics and set a very high bar for my subsequent Ph.D. students.

Thus in the beginning I was working on my own and with students. That was the way the senior leaders in the department, Ralph Bradley and Richard Savage, wanted it. Work on your own, prove your mettle, and move away from your thesis topic. Later on, when I began to collaborate with Frank Proschan and Jayaram Sethuraman, two great statisticians, my scope of topics vastly increased and my research got better! Whenever I received an offer or feeler from another place, I had to ponder whether I could find and establish working relationships with such superb collaborators at the next stop. I always doubted it.

Samaniego: Your research over the years has been distinctly nonparametric, including, of course, interesting and important contributions to Bayesian nonparametrics. You and your doctoral student, Ramesh Korwar, were the first to develop inference procedures for the hyperparameter of Ferguson's Dirichlet process, establishing the foundations for an empirical Bayes treatment of nonparametric estimation. I see that it's an interest you've sustained up to the present time. How did you get interested in this latter problem area?

Hollander: My interest in the Dirichlet process arose from Tom Ferguson's seminal paper (Ferguson, 1973). That was the principal motivation. I had obtained a preprint before its publication. I had read some earlier papers at Stanford on Bayesian nonparametrics but Ferguson's paper was the most tractable, the most promising. I can't remember the exact timing but I went to a Bayesian nonparametric conference at Ohio State where Tom was the principal speaker. He was also aware of some of the results by Ramesh Korwar and me and mentioned them in his lectures. His wonderful lectures got me further fired up and I went on to do more Bayesian nonparametrics with Ramesh, and then later with two more of my Ph.D. students, Greg Campbell and Bob Hannum, and more recently with Sethu (Campbell and Hollander, 1978; Hannum, Hollander and Langberg, 1981; Hannum and Hollander, 1983; Sethuraman and Hollander, 2008).

Samaniego: Which ideas or results in your Bayesian nonparametric papers seem to have had the most impact?

Hollander: Ramesh Korwar and I had several interesting results in our 1973 paper in the *Annals of Probability* (Korwar and Hollander, 1973). We showed that when the parameter α of the Dirichlet process is nonatomic and σ-additive, $\alpha(\mathscr{X})$ can be estimated from a sample from the process. The estimator we devised is $D/\log(n)$, where D is the number of distinct observations in the sample. We proved that estimator converges almost surely to $\alpha(\mathscr{X})$ where α is a finite nonnull measure on a space $\mathscr{X}$ that comes equipped with a σ-field of subsets. We also showed in the nonatomic and σ-additive case, that given D, the D distinct sample values are i.i.d. with distribution $\alpha(\cdot)/\alpha(\mathscr{X})$. This result has been used by others. For example, in an *Annals* paper Doksum and Lo (Doksum and Lo, 1990) considered Bayes procedures when F is chosen by a Dirichlet prior and used the result to study consistency properties of posterior distributions.

Another result that Ramesh and I had in that 1973 paper gave the joint distribution of the indicators that tell if the ith observation is distinct from the previous $i-1$. The indicators are independent, but not identically distributed, Bernoulli random variables. Diaconis and Freedman (Diaconis and Freedman, 1986) used this result in their study of inconsistent Bayes estimators of location. In our 1976 *Annals* paper (Korwar and Hollander, 1976) Ramesh and I used the Dirichlet process to define a sequence of empirical Bayes estimators of a distribution function. One interesting consequence of that paper was a result reminiscent of the famous James–Stein result on the inadmissibility of multivariate $\bar{X}$ when the dimension is ≥ 3. Ramesh and I showed that if there are at least three distribution functions to be estimated, one could do better than estimating each distribution by its sample distribution.

In a 1981 *Annals of Probability* paper (Hannum, Hollander and Langberg, 1981) Bob Hannum, Naftali Langberg, and I studied the distribution of a

random functional $\int Z\,dP$ of a Dirichlet process. We related the cumulative distribution of that functional evaluated at x, say, to the distributions of random variables T^x and we obtained the characteristic function of T^x.

It has been surprising and gratifying to see some recent uses of this result. For example, it is used (Cifarelli and Melilli, 2000) to study the distribution of the variance functional. The result is also used (Regazzini, Guglielmi and Di Nunno, 2002) to study the probability distribution of the variance of a Dirichlet measure and the probability distribution of the mean of a Dirichlet measure. Thus the result is getting a little play in the Italian school.

Samaniego: You've been at Florida State for 42 years! I'd like to ask you about your extensive and fruitful collaborations with some of your colleagues here. Tell me about your first joint paper with Frank Proschan. It was, I believe, one of the first papers in which tests were developed to detect particular nonparametric (NBU) alternatives to the exponential distribution.

Hollander: Frank came here in 1971 from the Boeing Research Labs. He was very open, very dedicated to his research. Our offices were close and we became friends. One day he walked into my office and said, "Let's write a paper." I said, "Great." I was excited he asked. His main area was reliability and mine was nonparametric statistics, so we aimed to work in the intersection, namely nonparametric methods in reliability. The first paper we wrote covered our NBU (new better than used) test (Hollander and Proschan, 1972). The test is based on a U-statistic, partially reminiscent of the Wilcoxon–Mann–Whitney statistic. We enjoyed working on it and there was a mild surprise. In calculating the probability that the statistic assumes its maximum value, the Fibonacci sequence pops up. The sequence had not arisen in Frank's longer research experience, nor in my shorter one. It is nice to have a mild connection with a famous pre-Renaissance mathematician. I believe the paper stimulated more research in testing and estimation for the various nonparametric classes arising naturally in reliability, including more research avenues for us.

Samaniego: In a subsequent paper, you and Frank discovered an interesting new context in which the total-time-on-test statistic arose. I'm sure that was a pleasant surprise.

Hollander: Frank and I wrote a testing paper on mean residual life (Hollander and Proschan, 1975) that was published in *Biometrika*. We considered the decreasing mean residual life class, the new better than used in expectation class, and their duals. We defined measures of DMRLness and NBUEness based on F, plugged in the empirical for F, and used those plug-in statistics as test statistics, standardized to make them scale-invariant. In the NBUE case, we obtained the total-time-on-test statistic. Up to that time it had been viewed as a test of exponentiality versus IFR or IFRA alternatives. We showed its consistency class contained the larger set of NBUE distributions, thus broadening its interpretation and applicability.

Large nonparametric classes of life distributions captured our attention for awhile. For example, we co-directed our student Frank Guess on a project where we defined new classes relating to a trend change in mean residual life. In our 1986 *Annals* paper (Guess, Hollander and Proschan, 1986) we considered the case where the change point is known. Later (Kochar, Loader and Hawkins, 1992) procedures were given for the situation where the change point is unknown.

Samaniego: On a personal level, what was it like to collaborate with Frank Proschan? Give us a feeling for his sense of humor, his work ethic, the "Reliability Club" and his overall influence on you.

Hollander: Frank, as you know, had a deadpan sense of humor. He would often remind me of the comedian Fred Allen who was very funny but never cracked a smile, never laughed at his own jokes. When he gave a lecture Frank would adroitly use transparencies, and there was always a parallel processing taking place, the material in the lecture, and humorous asides. He was dedicated to his research. He would come to the office very early, work for a few hours, go to the university pool for a swim, go home for lunch, then come back and work again. We both would come in on Saturday mornings, talk about what we wanted to show, go back to our offices, try to get a result, write up the progress, then put a copy in the other person's mailbox. This went back and forth. Some mornings we would come in and do this without talking face to face. The results would accumulate, and then we would have a paper. Later, Frank started the Reliability Club which met on Saturday mornings to present and discuss topics on reliability. Many students, several faculty and visitors would attend, and it would lead to dissertations, joint work, research grants, more papers.

I had the habit of working some weekends (including some Sunday nights; Glee and I lived very close to campus then, about an 8-minute drive) before Frank arrived but Frank solidified it and showed me I was not crazy doing it (or else we were both crazy). Without trying or fully realizing it, Frank's style and work ethic became a part of mine.

Samaniego: You've nicely integrated the parallel processing of material and humor into your own presentation strategy.

Hollander: Frank, I've always tried to be funny. It's both a strength and a weakness. I like to make people laugh but every once in a while it's not the time to be funny. Over the years I've become better at resisting the temptation to try to say something funny. But I still like to make witty remarks. I like to present to people the notion that statisticians have pizazz.

Samaniego: I've found myself that in teaching our subject, a little bit of well-timed humor—not the stand-up comedy type but the things that actually have something to do with the material we are talking about—helps people stay aboard; most people listen and enjoy it.

Hollander: At this point your advice on how to teach gets much higher marks than mine because you have just won an outstanding award at the University of California, Davis. I won't even mention the figure here; otherwise people will come by your house at night and break in.

Samaniego: Well, Myles, I've always enjoyed your presentation style and have probably stolen more than I care to acknowledge from the talks I've heard you give.

Samaniego: You've written a good many papers with Jayaram Sethuraman. What would you consider to be the highlights of that work?

Hollander: Even before Sethu and I worked together, I would go to his office for consulting. He is a brilliant statistician and he can often point you in a direction that will help, or lead you to a breakthrough when you are stuck. His entire career he has been doing that for all those wise enough to seek his assistance.

My first paper with Sethu is also joint with Frank. It is the DT (decreasing in transposition) paper (Hollander, Proschan and Sethuraman, 1977) and is certainly a highlight. It is a paper on stochastic comparisons which yields many monotonicity results. Among the applications in that paper were power inequalities for many rank tests. Al Marshall and Ingram Olkin later changed the DT term to AI (arrangements increasing). In a later paper (Hollander and Sethuraman, 1978), Sethu and I gave a solution to a problem posed to us by Sir Maurice Kendall during his short visit to Tallahassee in 1976. It was "How should one test if two groups of judges, each giving a complete ranking to a set of k objects, agree, that is, have a common opinion?" We proposed a conditionally distribution-free test using the Wald–Wolfowitz statistic.

Samaniego: Tell me about your more recent work with Sethu.

Hollander: Sethu and I have, on and off, been working on repair models in reliability for the last 15 years. Our interest was sparked by your groundbreaking paper with Lyn Whitaker (Whitaker and Samaniego, 1989) in which you developed what is now called the Whitaker–Samaniego estimator of the distribution F of the time to first repair in imperfect repair models. With Brett Presnell, we considered the problem in a counting process framework (Hollander, Presnell and Sethuraman, 1992) and also developed a simultaneous confidence band for F as well as a Wilcoxon-type two-sample test in the repair context. Many other important parameters, such as the expected time between repairs, depend on F and the nature of the repair process, so the problem of estimating F is important.

Five years later, with Cris Dorado (Dorado, Hollander and Sethuraman, 1997) we proposed a very general repair model that contains most of the models in the literature. We also introduced the notion of life supplements or boosts, so not only could the repairman move the effective age of the system to a point better than, say, minimal repair, he could also boost the residual life.

Recently we finished a paper on Bayesian methods for repair models (Sethuraman and Hollander, 2008). For example, if you put a Dirichlet prior on F in, say, the imperfect repair model, and take two observations, the posterior distribution of F is no longer Dirichlet. Thus there is, for these complicated repair processes which induce dependencies, a need for a broader class of priors which are conjugate. We introduced partition-based priors and showed they form a conjugate class. Beyond repair models, we believe this new method for putting priors on distributions has potential in many other areas.

Samaniego: One of my favorites among your papers is a JASA paper you wrote with Chen and Langberg on the fixed-sample-size properties of the

Kaplan–Meier estimator. It was based on a simple but very clever idea. Can you describe that work and how it came about?

Hollander: I was interested in the KME's exact bias and its exact variance. Brad Efron (Efron, 1967), in his fundamental article on the two-sample problem for censored data, had given bounds on the bias. Proportional hazards provided a clean way to get exact results. Earlier, Allen (Allen, 1963) proved that when the cumulative hazard function of the censoring distribution is proportional to that of the survival distribution, the variables $Z = \min(X, Y)$ and the indicator function $I(X \leq Y)$ are independent, where X is the time to failure, Y is the time to censorship. In his 1967 paper, Efron used this result for obtaining efficiencies for his generalized Wilcoxon statistic in the case when the censoring and survival distributions are exponential, and he thanked Jayaram Sethuraman for bringing the result to his attention. In the KME setting we (Chen, Hollander and Langberg, 1982) obtained an exact expression for moments of the KME by conditioning on $Z = (Z_1, \ldots, Z_n)$ and using Allen's result. Getting exact results in this setting was a natural consequence of my interest in rank order probabilities. Erich Lehmann really planted the seed with his famous work on the power of rank tests (Lehmann, 1953) where he obtained exact powers against what are now called Lehmann alternatives. My natural tendency is to first try hard to get exact results, then move to asymptotics.

Samaniego: You've done extensive joint work with some of your doctoral students. Perhaps your collaboration with Edsel Peña is the most varied and most productive. Tell me a little about that work.

Hollander: Edsel is an amazingly dynamic and energetic researcher. He loves to do research and his enthusiasm is infectious. He is also very talented. We have worked on a broad range of problems. We started (Hollander and Peña, 1988) with obtaining exact conditional randomization distributions for various tests used to compare treatments in clinical trials that use restricted treatment assignment rules, such as the biased coin design. We have also worked on confidence bands and goodness-of-fit tests in censored data settings. For example, in our 1992 JASA paper we (Hollander and Peña, 1992) defined a goodness-of-fit test for randomly censored data that reduces to Pearson's classical test when there is no censoring. We considered the simple null hypothesis and later Li and Doss (Li and Doss, 1993) extended

FIG. 6. *Myles and Glee Hollander in their flower-child style on sabbatical at Stanford, 1972.*

it to the composite case. Thus, although not ideal, there are secondary gains in not solving the more general problem straight out. You inspire others and your paper gets cited.

Edsel and I have also worked on interesting reliability models. For example, in Hollander and Peña (1995) we used a Markovian model to describe and study system reliability for systems or patients subject to varying stresses. As some parts fail, more stresses or loads may be put on the still-functioning parts. We use the failure history to incorporate the changing degrees of loads and stresses on the components. Shortly after that (Hollander and Peña, 1996) we addressed the problem about how a subsys-

FIG. 7. *Myles Hollander and Jayaram Sethuraman at the Hollander's home, Tallahassee, 1978.*

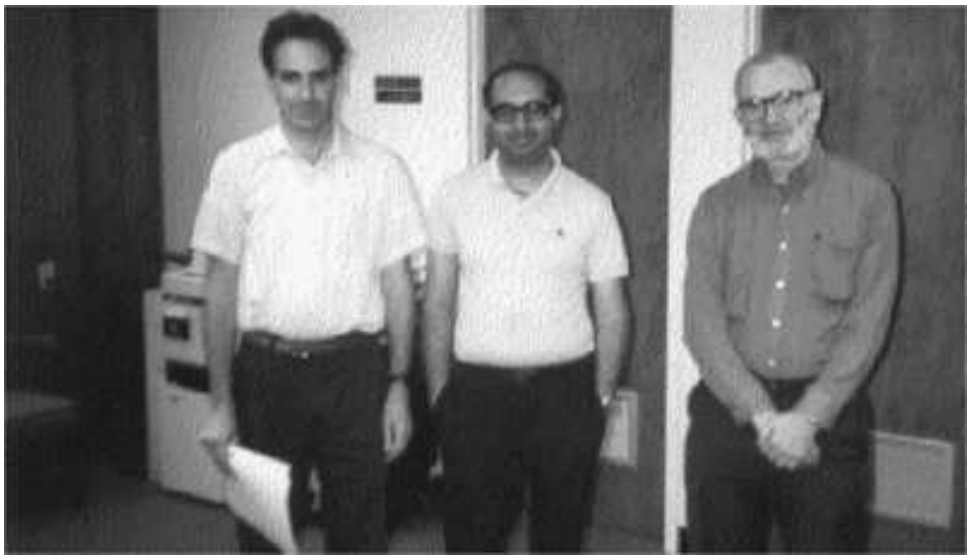

FIG. 8. *Myles Hollander, Subhash Kochar and Frank Proschan in the Department of Statistics, FSU, 1983.*

tem's performance in one environment can be used to predict its performance in another environment. Another idea that may attract some interest is our class of models proposed in 2004 in the *Mathematical Reliability* volume (Peña and Hollander, 2004). We introduced a general class of models for recurrent events. The class includes many models that have been proposed in reliability and survival analysis. Our model simultaneously incorporates effects of interventions after each event occurrence, effects of covariates, the impact of event recurrences on the unit, and the effect of unobserved random effects (frailties). Edsel and his colleagues and students have been studying asymptotic properties of the estimators and also applying them to various data sets.

OTHER WRITING

Samaniego: Tell me about your three books.

Hollander: The nonparametric books with Doug, the first and second editions, were very successful (Hollander and Wolfe, 1973, 1999). One important feature of these books are the real examples from diverse fields. It helped us broaden our audience beyond statisticians. Doug and I also taught a short course for about nine years, mid'70s to 80's, at the George Washington University Continuing Engineering Education Center. The audience at those courses consisted mainly of people in government and industry so again, in a way, we were bringing the nonparametric ideas and techniques to a different audience. Wiley has sought a third edition, but Doug and I have not yet committed to it.

Bill Brown and I began writing the medical statistics book (Brown and Hollander, 1977) in 1972 when I was on sabbatical at Stanford. We also featured real examples and it was adopted at many medical schools. I also used it for many years at FSU for a basic course on statistics in the natural sciences. Wiley always wanted a second edition, but Bill and I never got around to it. Wiley is now going to publish the original book as a paperback in its Wiley Classics Library series.

The book *The Statistical Exorcist* with Frank Proschan (Hollander and Proschan, 1984) was great fun to write. The book consisted of vignettes that treated

FIG. 9. *Myles Hollander with Mary Lou and Lincoln Moses, Tallahassee, 1985.*

a variety of problems. We wrote in a way to explain to the readers what statistics does, rather than give a formulaic approach on how to do statistics. In fact, we didn't use any mathematical formulas or symbols. One interesting feature is the cartoons, about half of which were drawn by Frank and Pudge's daughter Virginia and half drawn by Glee. Frank and I described the scenes and supplied the captions and Ginny (Virginia) and Glee did the drawings. We also opened the vignettes with epigraphs, relating to statistics, from novels. Some of the epigraphs are real and some were created by Frank and me. In an appendix we informed the reader which ones were from our imagination. For a text, however, students found it difficult without a few formulas upon which to hang their hats, for example, when to multiply probabilities, when to add, and so on. Marcell Dekker also wanted a second edition and it is not beyond the realm of possibility.

This semester I've been teaching an advanced topics course. The material was an eclectic mixture of survival analysis and reliability theory where I focused on some of the parallels between the two subjects. The course title is "Nonparametric methods in reliability and survival analysis." Whenever I look at the syllabus, it occurs to me that the material would make a good monograph. The problem is that most books on reliability are not big sellers although some are beautiful and informative. When I write, I do it not so much to make a few extra dollars, but to be read and thus a potentially large audience is the draw.

Samaniego: Did any specific examples in *The Statistical Exorcist* come out of your joint research with Frank?

Hollander: Some of the subject matter was motivated by the joint research. For example, we had vignettes on reliability which are unusual in an elementary book. We also had vignettes on nonparametric statistics, so the vignettes were influenced to some extent by our favorite subjects.

Samaniego: Myles, *The Statistical Exorcist* is, I would say, unique in the field as an introduction to statistical thinking. The book is distinctive in a variety of ways including its general content, the humor of its cartoons and epigraphs and even the titles of some of its sections. There is one entitled, "A Tie is Like Kissing Your Sister." Tell me about that section.

Hollander: There was a time when college football games could end in ties; that time has long passed and now they play extra sessions to determine a winner. But the conventional wisdom of most coaches was that a tie was no good. It leaves everybody frustrated and unhappy, the players and fans on both teams. Some coach coined the phrase "A tie is like kissing your sister." Which meant, you love your sister but you don't get much satisfaction out of kissing her. The vignette considered an optimal strategy for near the end of the game, taking into account the chance of making an extra point (one-point) play, the chance of making a two-point play and the relative value of winning the game versus the relative value of tying the game.

ADMINISTRATIVE WORK

Samaniego: With all these activities, plus your teaching and the mentoring of your graduate students, one would think that there might have been little time for other responsibilities. But, in fact, you served for nine years as the chair of your department. What were the main challenges you encountered as chair, and what achievements are you proudest of?

Hollander: A chair obviously has many priorities: the faculty, the students, the staff, the administration. They are all important and you have to serve and contribute to the well-being of each. However, in my mind the top priority is to recruit well, get the best people possible. Then everything desirable follows: a stronger curriculum, research grants, better students, and so forth.

In my first term, 1978–1981, my most significant hire was Ian McKeague who, in 1979, came from UNC, Chapel Hill. He stayed 25 years, participated in grants, became an expert in survival analysis, and served a three-year term as chair. We co-directed Jie Yang on a topic on confidence bands for survival functions and have two papers that emanated from that work and related work on quantile functions with Gang Li (Hollander, McKeague and Yang, 1997; Li, Hollander, McKeague and Yang, 1996). In my second and third terms, 1999–2005, among the tenure-earning people I hired, Flori Bunea, from U. Washington, Eric Chicken from Purdue, Dan McGee from University of South Carolina Medical School, and Marten Wegkamp from Yale, seem the most likely to contribute and hopefully stay at FSU for a long time. Each filled an important gap in our curriculum, taught new courses, got involved with grants. I recruited Dan as a senior biostatistician and he has

been a driving force in establishing our new M.S. and Ph.D. programs in biostatistics. He also succeeded me as chair.

Samaniego: Tell me a bit about how you tried to broaden the department's focus and reach. What are some aspects beyond recruiting?

Hollander: In Fall 1999 I called Ron Randles, who was statistics chair at the University of Florida (UF) at that time, and suggested we create an FSU-UF Biannual Statistics Colloquium Series. Ron liked the idea and after getting approval from our faculties it began and continues today. The idea is that it provides the opportunity for the recent appointees of each faculty to get some outside exposure by giving a talk in the other department. Thus in one semester UF comes to Tallahassee and a UF person talks, and the next semester FSU goes to Gainesville and an FSU person gives the colloquium talk. I also hope it leads to some joint research. Some people have had discussions, but to my knowledge it hasn't happened yet.

When I was chair, I was a mentor to all of our students, many of whom I recruited. I tried to teach them how to become professionals. I helped them get summer jobs and of course wrote reference letters for them. When I was younger I played intramural basketball and softball with some of them. I've gone to some of their weddings. Many students still stay in close touch with me. Of course you don't have to be the chairman to engage in these mentoring activities, but as chair one gets many opportunities to give extra advice at, for example, orientation and frequent student visits to the chair's office.

Here's a chair's story that goes into the highlight category. Ron Hobbs, an M.S. graduate of our department in 1967, and his wife Carolyn Hobbs, who earned a B.S. in Recreation Studies from FSU in 1965, endowed a chair in our department. It worked like this. Each year for six consecutive years, Ron and Carolyn contributed $100,000. Then after six years, the state contributed $400,000. Then the university had one million dollars to help support the chair. One year, in early December, Ron attended a meeting with me in my office and handed me an envelope with roughly $100,000 worth of America On Line shares of stock. I thought for a moment, there's a Delta jet with connecting flights to Hawaii leaving in about an hour. I could promptly turn the envelope over to the university's chief fundraiser at the time, Pat Martin, who was also attending the meeting. Or I could excuse myself, take the envelope with me ostensibly to return in a moment with the shares in a more carefully labeled envelope, and instead catch that jet.... Later that morning I noticed the sky was blue and clear as the engines roared and we took off to the west.

Samaniego: Myles, that could be the beginning of the next great American novel!

EDITORIAL ACTIVITIES

Samaniego: You served as the Editor of the Theory and Methods Section of JASA in 1994–1996. I know this is an extremely labor-intensive job. You seemed to thrive on the experience. What did you enjoy most about it?

Hollander: I had a great board of associate editors, including you, and you gave me the luxury of three reviews per paper. I liked working with the board. I also enjoyed reading the submissions—one year I had 503!—and the reviews. I tried to encourage authors, and with the reviews, improve the papers. Even if a paper was declined, I wanted the disappointed author to feel his/her paper was treated with respect and got a fair shake. I helped to get a page increase and in some of my issues I had over 30 papers in Theory and Methods. I also increased the T&M acceptance rate to around 30%. I suspect it is significantly lower now. It was just a great experience. Many nights and weekends I would bring a stack of folders home. If an AE was very tardy, I threatened to send in a SWAT team or toss him in a dark cellar until I received the reviews. One of my main goals was to make the papers readable and understandable. I insisted the authors write for the readers. I believe that was a mark of my editorship and your editorship, Frank, as well.

I enjoyed being JASA editor and a JASA AE before the editorship. I served on the boards of Paul Switzer, Ray Carroll and Ed Wegman, learned a lot from them, and was grateful for the opportunities. I've also continued with editorial activities after my JASA term ended. In 1993, the first volume of the *Journal of Nonparametric Statistics*, founded by Ibrahim Ahmad, appeared and I have been a board member since then, with one break. In 1995, Mei-Ling Lee launched *Lifetime Data Analysis*, and I have been a board member since the beginning. Both of those journals publish important papers and the profession should be, and I believe is, grateful to Ibrahim and Mei-Ling for their visions and dedicated work.

THE FUTURE

Samaniego: In 2003, you received the Noether Senior Scholar Award for your work in nonparametric statistics. That must have been extremely satisfying. What do you see as the important open problems that current and future researchers in this area might wish to focus on?

Hollander: The Noether Award is very special to me. The list of awardees consists of distinguished people with major accomplishments in nonparametrics and I am very grateful for the honor. The awardees thus far are Erich Lehmann, Bob Hogg, Pranab Sen, me, Tom Hettmansperger, Manny Parzen, Brad Efron and Peter Hall.

Stephen Hawking, the great physicist, says you cannot predict the great innovations in the future; that's partially why they are termed great innovations. If, however, Dennis Lindley is correct about this being a Bayesian century, and it seems to be going in that direction, then I would like nonparametrics to play a major role. Thus I would wish for new, important innovations in Bayesian nonparametrics. In my department we have at least three faculty members, Anuj Srivastava, Victor Patrangenaru and Wei Wu, working in image analysis, target recognition, face recognition and related areas. I would like to see nonparametric developments in these areas which are obviously important in many arenas including medical diagnoses and national security.

As a field, I'm glad we are pushing hard in interdisciplinary work, and it's good for our future role in science. It's valuable for the quality of research in the outside areas with which we participate and for scientific research overall. I'm hopeful statisticians will contribute significantly to many of the important open questions in other fields and many already do. In academic settings, it's critical that university administrations recognize the importance of strong statistical support raising the quality of research.

I want to be surprised in the future but, like Hawking says, it's hard to guess at the surprises. What do you think, Frank?

Samaniego: In the 20th century, especially from say, 1940 to 1990, the mathematical aspects of statistics were emphasized in both teaching and research. Mathematical statistics was prime. The power of computation changed that considerably. Then, applied problems, real applications with large and complex data sets, changed it even more. Today, there are areas like data mining that are of great interest and importance but haven't yet been mathematized. I wonder if it's just too early to mathematize challenging problems like these. I'm guessing that some sort of theory of optimality, some sense of what's good and what's better than something else, will be part of the future development of these evolving problem areas. It's just simply too hard to do this with tools we have available now.

Hollander: It is true that you can do a lot of things now with computer-intensive methods and not worry about getting the exact results. It's a little reminiscent of when Karl Pearson was classifying curves. There are a lot of data-based methods, but the mathematical foundations may have to be solidified. I think now that we are pushing applied stuff, computer-intensive methods, we can get results relatively easily, for example, nonparametrics with bootstrapping and Bayesian methods with MCMC. We may have to go back a little bit and shore up some of the methods, study their performance and properties as you suggest. But I think that will be considered only by theoretical statisticians. The computer-intensive surge is of course going to keep rolling, yield many new discoveries, and is great for the field.

OTHER INTERESTS

Samaniego: You have many collateral interests, not the least of which is baseball. You once told me that you were as pleased with your published letters to the Editor of *Sports Illustrated* as you were with many of your professional accomplishments. Tell me about your interest in the Dodgers and in sports in general.

Hollander: I was just kidding about the importance of the SI letters. Getting a statistical paper published is much more satisfying and represents a long-term and dedicated effort. However, the letters arose this way. My friend Bob Olds, a psychiatrist in St. Augustine, used to live in Tallahassee and write columns for the local newspaper. His future wife, Ann, took a few classes from me when she was an undergraduate at FSU. Bob sent a few letters to SI and they were not published. He is a wonderful writer, much better than me, but just for fun I submitted two and, surprisingly, both were accepted. The first was about Dodger pitcher Fernando Valenzuela during a period of Fernandomania in LA. The second was a comparison of the Stanford and Florida State

FIG. 10. *Myles Hollander flanked by Dodger pitching coach Ron Perranoski and Dodger pitcher Fernando Valenzuela, Dodgertown, Vero Beach, Florida, 1986.*

marching bands. The latter was prompted by that bizarre play in November, 1982, at the Cal–Stanford Big Game. You may recall that the Stanford band prematurely went on the field near the end of the game thinking Stanford had won and they inadvertently ended up as blockers on Cal's game-ending touchdown.

My interest in the Dodgers came about naturally during my childhood in Brooklyn. During my summers in high school most of my friends were away at what was then called sleep-away camp. My parents could have afforded to send me, and I wanted to go, but I was an only child and they liked having me around. So I had summer jobs in the city and then on weekends, and on some evenings, I would walk to Ebbets Field, sit in the bleachers or the grandstand, and watch the Bums, as they were affectionately called. This was the era of Jackie Robinson who displayed tremendous courage when he broke the color line in baseball. Branch Rickey, the Dodgers' General Manager at the time, also deserves a lot of credit for giving Robinson the opportunity. I enjoyed talking baseball to strangers at the game, seeing Afro-Americans and Caucasians get along, and I loved the teamwork on the field. I've lived my life with respect for people from all walks of life, from different backgrounds and cultures, and the Dodgers played a role in teaching me that. During my years as chair, I tried to instill the same kind of teamwork in the department.

I liked playing sports, mostly basketball, baseball and tennis. In my childhood, on the streets of Brooklyn, I played city sports like punchball and stickball. I also played basketball in schoolyards and baseball at the Parade Ground in Brooklyn. I played some tennis in high school but didn't get reasonably skilled at it until the early '70s.

Samaniego: One of the things that I've noticed about you over the years is that you and Glee like to go down to Vero Beach to see some spring training games. How long has that tradition been going on?

Hollander: I would say it dates back to the '70s, almost the time we first came to Tallahassee. We came to Tallahassee in 1965. We used to go to see the Dodgers. It was a different era. We could actually go up to them and talk to them and chat about baseball, whereas today they're much more isolated. There are fences. I had some good conversations with players over the years. I remember once we went to Vero Beach and the game was rained out. It was a game against Boston. Fernando Valenzuela was practicing with his pitching coach, Ron Perranoski. They were tossing the ball on a practice field so Glee and I went up to them and started talking to them and they also posed for pictures. We have many pictures from those years. One with our sons Layne and Bart and Hall-of-Fame Dodger pitcher Sandy Koufax is here on the office wall.

Samaniego: Has any of your work involved sports in statistics?

Hollander: I haven't done serious sports statistics like the type that interests the sports statistics section of the ASA. In the early '70s, however, Woody Woodward, who had been a player on FSU's baseball team, came to my office for help on the design and analysis of a study on different methods of

FIG. 11. *Myles and Glee Hollander riding mopeds in Bermuda, 1994.*

FIG. 12. *Mathematics chair DeWitt Sumners, Dean Donald Foss and Myles Hollander on the occasion of Myles' Robert O. Lawton Distinguished Professor Award, Tallahassee, 1998.*

rounding first base. I helped him and it became part of his master's thesis. Later Doug and I put the example in our nonparametrics book. In appreciation for the consulting, Woodward sent me a baseball glove from spring training when he was a member of the Cincinnati Reds. I used it when I played intramural and city league softball at FSU and I still take it with me to spring training games and major league games, hoping to catch a foul ball.

Samaniego: I'm visiting FSU on the occasion of a conference honoring your contributions to statistics and your department and university and commemorating your upcoming retirement. I know that you're looking forward to spending more time with family.

FIG. 13. *Myles and Glee Hollander with their sons Layne Q and Bart Q after the Lawton Award Luncheon, Tallahassee, 1998.*

I'm sure your sons and your grandkids will soak up plenty of your freed-up time. Any special plans?

Hollander: You're right. Glee and I do want to spend more time with our sons Layne, and his children Taylor and Connor, and Bart, his wife Catherine, and their children Andrew, Robert and Caroline. One set lives in Plantation, Florida, one in Amherst, Massachusetts. That will prompt some traveling. Also, Glee has siblings in Hilton Head, South Carolina and Spokane, Washington and I have family in LA, so we will get around. I also hope to go to a few statistical events. I love the international travel to conferences. You and I often attend the ones featuring reliability with the usual reliability club, Ingram Olkin, Nozer Sinpurwalla, Allan Sampson, Nancy Fluornoy, Henry Block, Edsel Peña, Mark Brown, Phil Boland, Jim Lynch, Joe Glaz, Nikolaos Limnios, Misha Nikulin, many more.

Glee and I own a beach house at Alligator Point, Florida. It's about an hour drive from our home in Tallahassee. We expect to be there a lot, walk on the beach, take bike rides to the western end of the point where there is a bird sanctuary, read novels, and so forth.

Samaniego: I've got to believe that you have at least one more book in you. Do you hope to do some writing once you are officially retired?

Hollander: Possibly I'll write a book. Realistically, I think it's more likely I'll stay involved by writing a paper every now and then and recycling back to FSU from time to time to teach. Lincoln Moses said, "There are no facts for the future." Despite being a statistician, I can't predict.

FIG. 14. *Glee Hollander, Myles Hollander, Frank Samaniego, Henry Block, Nozer Singpurwalla, Refik Soyer, Elena Samaniego at the 10th INFORMS Applied Probability Conference, University of Ulm, Ulm, Germany, 1999.*

Samaniego: You've had a long and productive career as a research statistician. Looking back, what would you say is your "signature" result?

Hollander: I'll interpret the word "signature" literally and take the opportunity to say I greatly enjoyed the work we did together on your elegant concept of signatures in reliability theory during my sabbatical visit to UC Davis in Spring, 2006 (Hollander and Samaniego, 2008). For comparison of two coherent systems, each having i.i.d. components with a common distribution F, we suggested the distribution-free measure $P(X < Y)$ where X is the lifelength of system 1 and Y is the lifelength of system 2. We found a neat way to calculate the measure directly in terms of the systems' signatures and probabilities involving order statistics. Among other things, we resolved the noncomparability issues using stochastic ordering, hazard rate ordering and likelihood ratio ordering that you (Kochar, Murkerjee and Samaniego, 1999) encountered for certain pairs of systems.

In the bigger picture, my signature career quest was to promote nonparametric statistics, bring it into other areas, get more people to use it, and get students to study the subject and make contributions to the field.

Samaniego: It seems that, over the period of your career, nonparametric methods have become more and more important and pervasive. There is no question that your work has helped that direction significantly.

Hollander: Thank you, Frank. When I look in journals there are a lot of papers that are nonparametric in nature and the adjective nonparametric does not appear in the titles. It's just a natural way to start a problem now, letting the underlying distributions be arbitrary.

FIG. 15. *Myles and Glee Hollander with Ingram Olkin and Nancy Fluornoy across from the Winter Palace while attending the 5th St. Petersburg Workshop on Simulation, St. Petersburg, Russia, 2005.*

Samaniego: You've worked with some of the legendary figures in our discipline including Ralph Bradley, Frank Wilcoxon, Richard Savage. These colleagues, and others, have played important roles in your professional evolution. How did the general environment at Florida State help shape your career?

Hollander: I came to Florida State because of Ralph, Frank and Richard. They were three luminaries in nonparametric statistics and I wanted to do nonparametrics research. Frank and I shared an office; he and his wife Feredericka and Glee and I became

FIG. 16. *Myles and Glee Hollander with their grandchildren Caroline, Andrew, Taylor, Connor and Robert, Tallahassee, 2006.*

friends, but he died three months after I arrived. I never did research with Frank, Ralph or Richard. But I was close to them. Ralph and his wife Marion and Richard and his wife Jo Ann were always friendly to Glee and me. Although I didn't write with Richard, up to the time he left for Yale in 1973 he carefully read each one of my technical reports and often made valuable suggestions. I gave the Bradley lectures at the University of Georgia in 1999, and after Ralph passed away, I was asked by his family to deliver a eulogy at his memorial service in Athens which I did, with pleasure.

The environment at FSU was dedicated to research and I liked that. I came to a place where that was the top priority. Also I came in 1965, only six years after the department was founded by Ralph, so there was the excitement of building. As it turned out, I was there when the first Ph.D. graduated and thus far I have seen all of our Ph.D. students graduate.

Samaniego: My recollection is that there's a famous quote attributed to you about the discipline of statistics. Tell me about it.

Hollander: The saying is: "Statistics means never having to say you're certain." I saw the movie "Love Story" in 1971. It was a big hit. It was based on a book of the same title by Erich Segal. I read the book after I saw the movie. As the title indicates, it's a love story. A wealthy Harvard law student, Oliver Barrett, falls for a poor Radcliffe girl, Jennifer Cavilleri, and eventually they marry. At one point, after a spat, Oliver apologizes and Jenny replies, "Love means never having to say you're sorry."

In statistics we give Type-I and Type-II error probabilities, confidence coefficients, confidence bands, false discovery rates, posterior probabilities and so forth, but we hedge our bets. We assess the uncertainty. With the movie fresh in my mind I transformed Segal's phrase to "Statistics means never having to say you're certain."

Samaniego: Thanks, Myles. This excursion has been most enjoyable!

Hollander: Frank, we have had a long friendship that has stood the test of a continental divide between us. I look forward to its future pleasures. Thank you for the conversation. It was highly enjoyable and I'm grateful for the opportunity to interact in this manner and offer my musings.

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REFERENCES

ALLEN, W. R. (1963). A note on conditional probability of failure when hazards are proportional. *Oper. Res.* **11** 658–659.

BROWN, B.W., JR. and HOLLANDER, M. (1977). *Statistics: A Biomedical Introduction*. Wiley, New York.

CAMPBELL, G. and HOLLANDER, M. (1978). Rank order estimation with the Dirichlet prior. *Ann. Statist.* **6** 142–153. MR0455198

CHEN, Y. Y., HOLLANDER, M. and LANGBERG, N. (1982). Small-sample results for the Kaplan–Meier estimator. *J. Amer. Statist. Assoc.* **77** 141–144. MR0648036

Cifarelli, D. and Melilli, E. (2000). Some new results for Dirichlet priors. *Ann. Statist.* **28** 1390–1413. MR1805789

Diaconsis, P. and Freedman, D. (1986). On inconsistent Bayes estimates of location. *Ann. Statist.* **14** 68–87. MR0829556

Doksum, K. A. (1967). Robust procedures for some linear models with one observation per cell. *Ann. Math. Statist.* **38** 878–883. MR0210256

Doksum, K. A. and Lo, A. Y. (1990). Consistent and robust Bayes procedures for location based on partial information. *Ann. Statist.* **18** 443–453. MR1041403

Dorado, C., Hollander, M. and Sethuraman, J. (1997). Nonparametric estimation for a general repair model. *Ann. Statist.* **25** 1140–1160. MR1447744

Efron, B. (1967). The two sample problem with censored data. In *Proceedings of the Fifth Berkeley Symposium on Mathematical Statistics* **4** 831–852. Univ. California Press, Berkeley.

Ferguson, T. S. (1973). A Bayesian analysis of some nonparametric problems. *Ann. Statist.* **1** 209–230. MR0350949

Guess, F., Hollander, M. and Proschan, F. (1986). Testing exponentiality versus a trend change in mean residual life. *Ann. Statist.* **14** 1338–1398. MR0868307

Hannum, R. C., Hollander, M. and Langberg, N. (1981). Distributional results for random functionals of a Dirichlet process. *Ann. Probab.* **9** 665–670. MR0630318

Hannum, R. and Hollander, M. (1983). Robustness of Ferguson's Bayes estimator of a distribution function. *Ann. Statist.* **11** 632–639. MR0696074

Hollander, M. (1966). An asymptotically distribution-free multiple comparison procedure-treatments vs. control. *Ann. Math. Statist.* **37** 735–738. MR0192612

Hollander, M. (1967). Rank test for randomized blocks when the alternatives have an a priori ordering. *Ann. Math. Statist.* **38** 867–877. MR0216692

Hollander, M., Mckeague, I. W. and Yang, J. (1997). Likelihood ratio based confidence bands for survival functions. *J. Amer. Statist. Assoc.* **92** 215–226. MR1436110

Hollander, M. and Peña, E. (1988). Nonparametric tests under restricted treatment assignment rules. *J. Amer. Statist. Assoc.* **83** 1144–1151. MR0997593

Hollander, M. and Peña, E. (1992). A chi-square goodness-of-fit test for randomly censored data. *J. Amer. Statist. Assoc.* **87** 458–463. MR1173810

Hollander, M. and Peña, E. (1995). Dynamic reliability models with conditional proportional hazards. *Lifetime Data Anal.* **1** 377–401. MR1371991

Hollander, M. and Peña, E. (1996). Reliability models and inference for series systems operating in different environments. *Nav. Res. Logist.* **43** 1079–1108. MR1426103

Hollander, M., Presnell, B. and Sethuraman, J. (1992). Nonparametric methods for imperfect repair models *Ann. Statist.* **20** 879–896. MR1165597

Hollander, M. and Proschan F. (1972). Testing whether new is better than used. *Ann. Math. Statist.* **43** 1136–1146. MR0348909

Hollander, M. and Proschan, F. (1975). Tests for the mean residual life. *Biometrika* **62** 585–593. MR0395119

Hollander, M. and Proschan, F. (1984). *The Statistical Exorcist.* Dekker, New York.

Hollander, M., Proschan, F. and Sethuraman, J. (1977). Functions decreasing in transposition and their applications in ranking problems. *Ann. Statist.* **5** 722–733. MR0488423

Hollander, M. and Samaniego, F. J. (2008). The use of stochastic precedence in the comparison of Engineered Systems. In *Proceedings of the 2007 International Conference on Mathematical Methods in Reliability.* To appear.

Hollander, M. and Sethuraman, J. (1978). Testing for agreement between two groups of judges. *Biometrika* **65** 403–411. MR0513938

Hollander, M. and Wolfe, D. A. (1973). *Nonparametric Statistical Methods.* Wiley, New York. MR0353556

Hollander, M. and Wolfe, D. A. (1999). *Nonparametric Statistical Methods*, 2nd ed. Wiley, New York. MR1666064

Kochar, S., Loader, C. and Hawkins, D. L. (1992). Testing exponentiality against IDMRL distributions with unknown change point. *Ann. Statist.* **20** 280–290. MR1150344

Kochar, S., Mukerjee, H. and Samaniego, F. J. (1999). The 'signature' of a coherent system and its application to comparisons among systems. *Nav. Res. Logist.* **46** 507–523. MR1700160

Korwar, R. M. and Hollander, M. (1973). Contributions to the theory of Dirichlet processes. *Ann. Probab.* **1** 705–711. MR0350950

Korwar, R. M. and Hollander, M. (1976). Empirical Bayes estimation of distribution function. *Ann. Statist.* **4** 580–587.

Lehmann, E. L. (1953). The power of rank tests. *Ann. Math. Statist.* **24** 23–43. MR0054208

Li, G. and Doss, H. (1993). Generalized Pearson–Fisher chi-square goodness-of-fit test, with applications to models with life history data. *Ann. Statist.* **21** 772–797. MR1232519

Li, G., Hollander, M., Mckeague, I. W. and Yang, J. (1996). Nonparametric likelihood ratio confidence bands for quantile functions from incomplete survival data. *Ann. Statist.* **24** 628–640. MR1394978

Nemenyi, P. (1963). Distribution-free multiple comparisons. Ph.D. thesis, Princeton Univ.

Peña, E. and Hollander, M. (2004). Models for recurrent phenomena in survival analysis and reliability. In *Mathematical Reliability*: *An Expository Perspective* (T. Mazzuchi, N. Singpurwalla, and R. Soyer, eds.) 105–123. Kluwer, Norwell, Massachusetts. MR2065001

Randles, H. R. and Hollander, M. (1971). T-minimax selection procedures in treatments versus control problems. *Ann. Math. Statist.* **42** 330–341. MR0282469

Regazzini, E., Guglielmi, A. and Di Nunno, G. (2002). Theory and numerical analysis for exact distributions of functionals of a Dirichlet process. *Ann. Statist.* **30** 1376–1411. MR1936323

Sethuraman, J. and Hollander, M. (2008). Nonparametric Bayes estimation in repair models. *J. Statist. Plann. Inference.* To appear.

Whitaker, L. R. and Samaniego, F. J. (1989). Estimating the reliability of systems subject to imperfect repair. *J. Amer. Statist. Assoc.* **84** 301–309. MR0999692