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A Conversation with John Hartigan

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Abstract. John Anthony Hartigan was born on July 2, 1937 in Sydney, Australia. He attended the University of Sydney, earning a B.Sc. degree in mathematics in 1959 and an M.Sc. degree in mathematics the following year. In 1960 John moved to Princeton where he studied for his Ph.D. in statistics under the guidance of John Tukey and Frank Anscombe. He completed his Ph.D. in 1962, and worked as an Instructor at Princeton in 1962–1963, and as a visiting lecturer at the Cambridge Statistical Laboratory in 1963–1964. In 1964 he joined the faculty at Princeton. He moved to Yale as Associate Professor with tenure in 1969, became a Professor in 1972 and, in 1983, became Eugene Higgins Professor of Statistics at Yale—a position previously held by Jimmie Savage. He served as Chairman of the Statistics Department at Yale from 1973 to 1975 and again from 1988 to 1994. John was instrumental in the establishment of the Social Sciences Statistical Laboratory at Yale and served as its Director from 1985 to 1989 and again from 1991 to 1993. He served as Chairman of the National Research Council Committee on the General Aptitude Test Battery from 1987 to 1990. John's research interests cover the foundations of probability and statistics, classification, clustering, Bayes methods and statistical computing. He has published over 80 journal papers and two books: *Clustering Algorithms* in 1975 and *Bayes Theory* in 1983. He is a Fellow of the American Statistical Association and of the Institute of Mathematical Statistics. He served as President of the Classification Society from 1978 to 1980 and as Editor of *Statistical Science* from 1984 to 1987. John married Pamela Harvey in 1960. They have three daughters and three grandchildren.

This interview was recorded in John's office at Yale on August 15th, 2002. The following day more than 30 of John's former Ph.D. students—from all over the United States and from as far away as Japan—arrived in New Haven for a special celebration to mark John's 65th birthday. Readers unfamiliar with John's sense of humor should be warned of his tendency to support his arguments by outrageous and over-the-top statements, most of which are delivered with tremendous energy and accompanied by mischievous laughter. I have attempted to retain in the text as much of that side of John as I could.

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CHILDHOOD IN AUSTRALIA

Barry: John, you were born in Sydney in 1937. Can you tell me a little bit about your childhood?

Hartigan: We moved to Canberra when I was 9 months old. At that time, the population of Canberra was only 8000 people, but the government was transferring large numbers of people there and it was growing rapidly. Today it's a metropolis of 250,000. When we first moved there, we lived in the outskirts, but by the time I left Canberra, our house was right in the middle of the city. It was a little country town then, small numbers of people and rather small schools. I went to a parochial school. Canberra was very safe. We never had to worry about crime.

Barry: What job did your father have?

Hartigan: My father left school when he was about 14 years old. He got a job working as a telegraphist which, in those days, was a person who operated the teletype machine in the Post Office and received telegrams. He did that for a number of years while I was young. Later on, he got a job as a telegraph operator in Parliament House in Canberra. He saw a lot of the action in Australian politics as it was happening. He was a big Labor supporter. He knew the prime minister, Ben Chifley, for instance.

Barry: Was he active in the Labor movement?

Hartigan: Yes, he was. He was president of Labor South in Canberra, which was the unit of the Labor party that the Prime Minister happened to belong to, so he was nominally the Prime Minister's boss. (Laughter.)

Barry: Did he have strong left wing views?

Hartigan: I wouldn't think so. He had the usual lower middle class labor view that the government should provide pretty well for people, but certainly no idea that they should confiscate all the riches of the wealthy and distribute them amongst the poor.

Barry: Were your political views influenced by him do you think?

Hartigan: No, maybe slightly in opposition to him. My political views were scarcely developed until long after I had left home.

SCHOOLDAYS

Barry: Were you particularly interested in mathematics at school?

Hartigan: Not particularly. I was always good at mathematics, even when I was very young. At primary school I liked geography and history and art. The only thing that I did not do very well in was religion. I could never quite grasp it. I have never got a failing grade in any class in my whole life except in religion once in sixth grade. I could not understand how I got a failing grade, but there it is.

Barry: We won't go into that topic any deeper I think! (Laughter.) Do you remember any of your schoolteachers?

Hartigan: I had a very good high school mathematics teacher called Brother Ligouri. I went to a terrible boarding school known as St. Joseph's College. Football was the school's main interest. By accident they had a fabulous mathematics teacher who also found himself stranded there. Just how fabulous he was is indicated by the fact that, in the 40 years he was teaching there, 10 of these country boys came first in the state of New South Wales in mathematics. I was one of them. I always remembered his style of teaching and what he taught and, in fact, most of the things that I do day to day are things I already knew by the time I left high school. I could do moderately advanced calculus already. We did all kinds of things like solid geometry and projective geometry—really a huge variety of interesting mathematics.

THE UNIVERSITY OF SYDNEY

Barry: In 1955 you left Canberra to study at the University of Sydney. Can you tell me a bit about your time there?

Hartigan: I loved the University. I was in the Science Faculty and took lots of mathematics courses. It was wonderful, but I also enjoyed the other classes. In geology class, we would go on field trips with hammers and knock away. It was great fun. On the other hand, I hated chemistry. Like many of the courses in experimental science, it was not at all as experimental science should be, but rather that you should get the result that the lab instructor wanted you to get. They did not want to see any real experimenting going on, they just wanted you to go through a very rigorous routine and get a certain result in a certain range. It was very unimaginative and I stopped taking chemistry as quickly as possible. I liked physics, but again I never much liked the experimental part. The physics experiments were the same as the chemistry experiments. They would have this big theory about what was happening. There was hardly any resemblance between the actual lab apparatus and the theory but you still had to get results in accordance with the theory. I hated that, but I had some good friends who assisted me for a while until everyone found out just how bad I was. (Laughter.)

Barry: Did you have any memorable teachers in mathematics or statistics while you were in Sydney?

Hartigan: The level of instruction was quite good. There was a Professor Tim Wall who was very good. He did group theory and did it really elegantly. Group theory is one of those areas of mathematics where everything is well defined and it's clear what you are doing. It's like a nice simple game. We also covered things like real analysis, which I did not find so attractive and didn't like as much, but I realized later on that on you have to know everything about real analysis and measure theory and functions.

Barry: Did you do much statistics while you were there?

Hartigan: Only a little. Harry Mulhall was the teacher that got me interested in statistics. He was a nice fellow and had very carefully designed lectures. He wasn't a real statistician though. He was a mathematician who knew a bit of statistics but he didn't really do much applied work, which I regard as a basic qualification to be a statistician, but what he did know he spelled out carefully and he was very helpful to me. I somehow liked statistics even though it was not a subject of high repute in mathematics. I did special projects over the summer in probability and statistics. I read Jeffreys which made a big impression on me. It was very different from all the other mathematics that I had ever done. For one thing, Jeffreys was quite inclined to say one thing in Chapter 1 and something else in Chapter 3 entirely contradictory to what he had said earlier. Didn't worry him at all! (Laughter.) I thought that sounded like a subject that could use some help. It still can!

Barry: Just before we leave Sydney, was it at Sydney that you met Pam?

Hartigan: It was. Her name was Harvey and so she was right next to me in the chemistry lab. (Laughter.) We had many classes in common and it was inevitable that, after two or three years, we would eventually get together.

GRADUATE STUDY AT PRINCETON

Barry: In 1960 you went to Princeton. How did that come about?

Hartigan: In those days the Australians mostly went to England. They would go to Cambridge and study the Tripos and then get a Ph.D. in the English style which was really that, after you'd completed your undergraduate education, you'd just work on something. Then they would come back and get jobs in Australia. That was a standard pattern for a lot of guys before me who had gone through the Mathematics Department in Sydney. I was first in mathematics in Sydney over the years that I was there and I got the gold medal in the last year and so I was one of the people who would normally have gone through that route. And I was considering that, but, on the other hand, I was married by then and that made it very difficult for me to go to the U.K. They did not give you enough money to live on, whereas it wasn't so difficult in the U.S. and, at that time, people were starting to think of the U.S. as a better alternative. One of my professors, T. G. Room, was a geometer and he had just spent a year in Princeton and knew some people over there. He wrote to them and they wrote back offering me a fellowship that was enough for poverty level living for a graduate student with a wife.

FIG. 1. *John in Fine Hall, Princeton, about 1965.*

Barry: What was Princeton like in 1960 when you got there?

Hartigan: It was a very interesting place. They offered a wonderful graduate program in mathematics which was more in the European style than in the American style in that the professors taught whatever courses they liked. There were no exams, there were no grades, and to continue to your Ph.D. you had to take an oral exam after you had been there a couple of years. The faculty would all attend and if they were satisfied with you, they would let you continue, and if they were not, they would throw you out. At that time there was a little statistics group there which consisted of John Tukey, Frank Anscombe, Sam Wilks and usually one or two visitors every year. It sounds small, but it was an active group doing a lot of interesting things. We would take some courses in statistics and some in mathematics in general and some in analysis and probability. I went to a course that Feller gave on probability. It was a pretty rotten course in my opinion since he kept changing his mind about what he wanted to say. He was writing his second book at that time and I think he was working through the ideas of that book, but it was not a particularly good way to learn probability especially since he didn't believe in measure theory. I learned all my probability by myself by reading books and, at Princeton, that is what you were expected to do. That does not seem so unreasonable to me. It encourages independence and I like independence.

Barry: How did you end up working with John Tukey as your Ph.D. advisor?

Hartigan: I had joint advisors, Frank Anscombe and John Tukey. Frank had a bit of a Bayesian aspect and I was interested in Bayesian theory then having read Jeffreys and I thought that it seemed to me the only reasonable way to formulate statistical problems. On the other hand, John Tukey was probably the leading person there and I thought that he might be interesting to work with.

Barry: What was your thesis about?

Hartigan: I wanted to work in the foundations of probability, but as I've usually done, rather than do something serious on foundations, I did something technical instead which was to try to develop objective prior distributions. These were a little bit similar to things that Jeffreys had done but were based on groups. Don Fraser had used his group theoretical method of inference to develop certain classes of invariant prior distributions. My innovation was to construct these classes of relatively invariant priors which were slightly larger than the ones considered by Fraser. I could see that these group theory priors were not really sufficient because, for too many problems, there are no relevant group transformations around. I wanted to look locally in the parameter space and use some kind of local group operation that would specify the invariant local prior. So I made various assumptions and came up with a class of locally invariant prior distributions called asymptotically locally invariant priors. The interesting thing is that years later I did another paper (Hartigan, 1998) asking which prior most closely matched maximum likelihood asymptotically. Whenever I do research I rarely read the literature until afterward because I don't want it to influence me too much. That can be a very embarrassing policy, because sometimes it turns out that you have just reinvented the wheel. So I was quite concerned that I was going to go back and find that this maximum likelihood prior had already been found somewhere else. And it had been. I did it myself (laughter)—40 years ago.

Barry: Was Tukey interested in this kind of stuff?

Hartigan: Not really. He has never been really interested in Bayes theory, but it didn't matter because he could give good advice even if he wasn't particularly interested. And he was very good. He wasn't saying, "Oh, what if you do some data analysis?" He was looking at what I was trying to do on its own merits and making comments. One of his great skills was to listen to what the person was saying and to talk about the problem from the person's point of view. On the other hand it was very hard to get to talk to him because he was always somewhere else. Frank helped me too. They both helped me by not interfering too much. I regard that as help.

Barry: Did anyone you met there have a major influence on your attitude to statistics?

Hartigan: Oh yes, John Tukey certainly had a major influence on me. He convinced me that it was worthwhile to pay close attention to data analysis, statistics applied to real problems. That to be a statistician, it was not sufficient to publish papers in *The Annals* with theorems and proofs. He didn't ever say that, but he persuaded you that it was true by the way he taught and by the way he argued in seminars. I'm sure that he persuaded me that statistics is more than a subset of mathematics.

Barry: Did any major figures visit Princeton while you were there that had an influence on you?

Hartigan: A huge number of people came through. R. A. Fisher came through once and gave a lecture. D. R. Cox came; and Akaike, the developer of AIC (Akaike information criterion). Apart from seminar speakers, there would be one or two visitors who would spend a semester at Princeton. Art Dempster visited for a semester and gave some courses as did Don Fraser.

A YEAR IN ENGLAND

Barry: You were to stay at Princeton as a faculty member until 1968, but first you spent a year in England. Tell me about your year in England.

Hartigan: At that time I was planning to go back to Australia, because my Fulbright scholarship required that I should do so, but I thought that it would be a good idea, while I was over here so to speak, to spend some time in England as well. Sam Wilks spoke to David Kendall about this and they offered me a job replacing Morris Walker who was on leave for a year. At that time, I had job offers from various places; I had an offer from Berkeley for instance. These offers would just come out of the blue; I never applied for them. That is the way that it was done in those days. If you had a position, you would call around the various departments and ask if there were any people they would like to recommend and they would just give you a list of names. So it was done very differently before affirmative action. It was done by an old boy network.

Barry: What did you actually do in England?

Hartigan: I was at the Cambridge Statistical Laboratory and I taught some courses that Morris Walker would have otherwise taught, and then I advised some students regarding their project work. They were excellent students, better than our graduate students. The people there were David Kendall and Morris Walker, who was away on leave, and Violet Cane, who was an applied statistician, and John Kingman, who was a probabilist, and some younger people who were research associates. It was an interesting group, but it was very probabilistically inclined. They worked on queues and things like that and I remember saying once that there wasn't much statistics going on in the Statistical Laboratory. (Laughter.) David Kendall said, "Oh we take a wide view of statistics here." He did take a very wide view of what he regarded as statistics, but I don't think you can have a Statistics Department without actually having some statisticians in it. There are people like David Kendall and John Kingman who do beautiful probability and occasionally they do some statistics, but you feel that they are doing it out of duty rather than desire. I don't think it helps the development of a place to have it too heavily oriented that way.

BACK TO PRINCETON

Barry: You must have changed your mind about going back to Australia because you ended up going back to Princeton.

Hartigan: I was planning to go back to Australia and, in fact, Ted Hannan had already offered me a job at the Australian National University, but Sam Wilks died suddenly and John Tukey didn't have anyone to teach certain courses in Princeton that year. So he called me up and offered me a job. I really wasn't so anxious to accept, but it seemed as if they actually needed me seriously. It is not a good idea to go back to your own department or for departments to hire their own students. It's better to bring in a breath of fresh views. Nevertheless, I felt I owed something to the department and to John Tukey, in particular. This was a way of partly paying him back. Also, having seen what the English scene was like and knowing what some of the Australian scene was like, I thought that it might be the best place for me.

A YEAR IN LOS ANGELES

Barry: What happened in 1968 then?

Hartigan: I had a year's leave from Princeton and went out to Los Angeles, to the Health Science Computing Facility which, at that time, supported the BMD package. BMD was probably the best statistical package available before SAS came to life. It was developed with NIH funds because it was felt that doctors needed good statistical software. The people there wrote excellent Fortran programs and put out a package of programs with a relatively simple interface and, of course, no graphics interface. It was certainly a little bit primitive, but the programs were based on good numerical routines.

Barry: What were you doing there?

Hartigan: I liked computing at that time. When I'd just finished my Ph.D., I spent a year teaching at Princeton and during that time I did a lot of computing. I had not done much before, but then I learned to do serious Fortran and I liked writing programs. I thought going to BMD would be a good way to get some free computer time. It was pretty expensive in the universities to get computer time. So I went out there and wrote some programs for them on clustering.

YALE

Barry: In 1969 you went to Yale. How did that happen?

Hartigan: Frank Anscombe asked me to come. Princeton had established a separate Statistics Department in 1965 and that produced a considerable increase in administration, but, apart from me, Mike Godfrey and John Tukey were the only other people in the department. Mike Godfrey was half in Economics and John Tukey was half in Bell Labs, and so I was doing a lot of administration of the sort that you really shouldn't be doing when you are just starting out. John Tukey was a wonderful colleague, very smart, the smartest person in the world, but he didn't want to spend his time on administration. So it seemed to me that I should get away, because otherwise I was going to be doing a lot of administration, which was something that I didn't want to do.

Barry: And why did you choose Yale?

Hartigan: Well they asked me. Jimmie Savage was here of course and he was the great Bayesian of the world, so I thought it would be interesting to learn from him, and Frank Anscombe had always been a very helpful person to me.

Barry: Did you have much interaction with Jimmie Savage when you were here?

Hartigan: Well, he died after a couple of years so it wasn't very lengthy, but while he was here, we talked a lot and he was very good. Actually I have a story about that. John Tukey was a very truthful person and very objective about things. I asked him once what he thought of Jimmie Savage. This was long before I thought of coming to Yale. He often said memorable things. The memorable thing he said then was that, at the Eastern Regional Meetings of the Biometrics Society, Jimmie Savage was the person in the audience who asked the second best questions! (Much laughter.)

Jimmie had a very good broad grasp of things. His eyesight was terribly poor, so he did a lot of things visually and in summary. He didn't like to do a lot of detailed algebra, but I can remember asking him once about a problem in clustering, a certain convergence problem. He didn't do any algebra and he didn't do any formulation. He wrote down one line which said it will go like this, and it took me a year or two, but it did go like that. He was a great guy.

Barry: You are still at Yale now and so you must have been happy here. Is that a fair assumption?

Hartigan: Yes, I do like Yale. It's not like Princeton, which is a scientific university in which the people are locked away in their own labs and there is not much communication between departments or between parts of the university. Yale is a humanist university and the connections between the different parts of the university seem much more extensive and encouraging. The humanist attitude toward learning is different from the scientific attitude in that it tends to be more broad-minded and tolerant. I have been on a lot of committees at Yale and have found the other people on the committees often to be very interesting people to talk to even though they might not know any calculus. That's got nothing to do with it. They are very persuasive. They listen to arguments. They produce correct counter arguments. It's good arguing with them and I like that.

Barry: You were Chairman of the Statistics Department at Yale for quite a number of years. Did you find that a very onerous task?

Hartigan: Yes. I was Chairman during some hard times. A few years ago, while I was Chairman, Richard Savage and Frank Anscombe had retired and so it was down to David (Pollard) and me, and we lost a couple of our juniors in one year—they went off to good jobs elsewhere. At that time, the University decided to enter on a retrenchment policy and other departments were eyeing our positions with interest. It was tough for us to defend ourselves. There were many meetings with the administration concerning the future of the department. Eventually they allowed us to keep our positions and to do what we wanted to do. I remember that as being quite a tough time. Generally speaking, however, it wasn't so difficult being Chairman. At that time, we had a very wonderful business manager named Barbara Amato now Barbara Kuslan. She was very wise, took care of everything that was routine and many things that were not routine.

PUBLIC POLICY

Barry: From 1987 to 1990 you were Chairman of the National Research Council Committee on the General Aptitude Test Battery. What was that all about?

Hartigan: The General Aptitude Test Battery (GATB) is a battery of IQ tests and mechanical diligence tests that was adopted by the United States Employment Service to test all of its applicants. Performance on the test was used to determine which candidates should be sent out for particular jobs. However, it turned out that African–Americans did poorly on these tests and therefore they were not being sent out for any of these jobs. They corrected this problem by adding 60 to the score of every African–American in order that the average score for African–Americans, after they added the 60, was the same as for everyone else.

Barry: What did you think of that scheme?

Hartigan: There were questions about it at the time. People at the Equal Employment Opportunity Commission, who didn't normally take this side of the argument, were arguing that it was discriminatory to adjust the test scores in this way. Our job was to address the political problem of these adjustments to the test scores and also to look at the validity of this test for predicting performance on the job. The committee met for three years. It had one or two statisticians on it, some educational psychologists and a number of industrial psychologists who do a lot of this testing and who were very much in favor of the test.

The designers of this test, Schmidt and Hunter, were not on the committee. They had, in fact, agreed to the adjustment because otherwise the test was not going to be used widely. They felt that the use of the test to select people was going to cause a large increase in productivity in the United States.

There was a huge amount of data. The test was based on around 500 different surveys for different jobs, how it predicts this job, how does it predict that job and so on. The committee ended up recommending a full reevaluation of the test. The difficulty is that the test has relatively low correlation for many jobs, but it's certainly very highly correlated with race. So, if you want it so that a black person who has a reasonable chance to do the job also gets to the job, you can't use this test as it is.

We suggested some changes to the test procedure which would improve the chances of minority group people, but at least some of the members of the committee felt that the claims of productivity were very dubious because they essentially acted as though the only

FIG. 2. *John with Peter Hall at University of Pittsburgh, Bump Conference, 2001.*

people that you ever reported to jobs were people who did well on the test. The fact is that almost everyone is going to get a job one place or another.

In general, you don't want a whole government operation, the United States Employment Service, employing fruit pickers in California and lathe operators in Detroit, all operating off one little intelligence test. It puts too much weight on that one intelligence test. There is certainly correlation, there may be some gains achieved by using this test, but a general recommendation was that it shouldn't be the only way to assign workers.

Barry: Did people listen to what the committee had to say?

Hartigan: There was a big outcry from people who felt that our suggested adjustments to the test scores for minority groups was an example of racial norming, but we were suggesting adjustments simply because this is a test which isn't very highly correlated with job performance but is very highly correlated with being black and, if you use it, you're definitely going to hurt blacks a lot. It just didn't seem like wise policy to do that, but the Equal Employment Opportunity Commission people felt that you cannot take account of race in employing the test; you have got to employ it as it is. As a result of our report, they did not use the GATB. I do not know what they do now, but not using the GATB is not a bad outcome.

Barry: Did you enjoy being involved in public policy?

Hartigan: Yes, I enjoyed it. It takes a lot of time, but it can be useful and I have done some other things like that which I liked.

Barry: What other things?

Hartigan: I worked for a while on a committee dealing with the Dictionary of Occupational Titles and the notion of comparable worth. There is an idea of comparable worth whereby you can look at each job and work out what the correct salary is for that job as a function of the skills required, and then if women, for example, weren't being paid that salary, they could be awarded a similar salary. If you look at jobs that are occupied mainly by women, you find that they are paid less than what seem like comparable jobs that are occupied mainly by men. An example that is often used is nurses and plumbers. Nurses in those days used to be paid a lot less than plumbers and I know a joke about that. (Laughter.) A plumber went to visit somebody's house and sat down, looked under the sink, twiddled for a while and 15 minutes later said that will be 75 dollars. The guy who was paying said, "I'm a lawyer and I don't get paid that kind of money," and the plumber said, "Neither did I when I was a lawyer." (Laughter.)

However this is one of those issues which is very political. They need committees to talk about these things because the politicians don't want to take any sides, any sides they take are very bad for them, and it was interesting to learn about those things.

EARLY PUBLICATIONS

Barry: What were your first few publications about?

Hartigan: I did a publication on my thesis (Hartigan, 1964) and then another publication on asymptotically unbiased priors (Hartigan, 1965) which was a development beyond that. I had only two publications on prior distributions. I have never really believed in doing a whole lot of publications on one thing. What I tend to do is to have several things going at one time. Then one of them goes ahead, while the others stay fallow, and when I'm sick of one, I've got something to turn to.

Barry: You then went on to "Probabilistic completion of a knockout tournament" and "Estimation by ranking parameters." Are these two papers related?

Hartigan: No. They are different things. The first paper (Hartigan, 1966a) considers a set of people who go through a knockout tournament and you want to use the results of the games played to figure out how to rank the people. The paper is based on a model and produces a ranking given the tournament results. The ranking parameters paper (Hartigan, 1966b) was a different matter entirely. Suppose that instead of using the data to estimate the parameters or to compute confidence intervals, you want to use the data to produce a ranking of all the parameter values. The paper contains some theory and suggestions as to how you might do that. As far as I know, I'm the only one who has ever written on that subject and it obviously hasn't gotten deeply into practice!

SUBSAMPLING

Barry: Starting in 1969 you have a series of papers dealing with subsampling (Hartigan, 1969, 1971, 1975a; Forsythe and Hartigan, 1970). What were those papers about?

Hartigan: Tukey had produced the jackknife a couple of years before that. I produced a Bayesian version of the jackknife in which, given n observations, the next observation was selected from each of those with probability $1/n$ and the next observation selected from each of the previous $n + 1$ with probability $1/(n + 1)$ and so on. So you generate a new sample and, in fact, you can generate an infinite new sample in this way. That gets you a certain distribution which is a Bayes distribution. In fact, it's a limiting case of the Dirichlet priors that Ferguson produced later on. Using this new distribution you can get estimates of variance and so on. In about 1980, Don Rubin produced something like this called the Bayesian bootstrap, but I had it in some of these subsampling papers as well. I actually did this when I was in England in 1963. I sent it to the *Journal of the Royal Statistical Society* and naturally they rejected it. (Laughter.) So they put me out by about 10 years.

Barry: Efron introduced the term bootstrap in the 1977 Rietz Lecture. How does your work on subsampling differ from the bootstrap?

Hartigan: It's in the same area. Mine is a rather particular resampling scheme. It takes the data and it does a recomputation on a rearrangement of the data in which each of the original observations is included a number of times, varying between zero and n. What you actually need is a sampling rule such that the number of times each of the original n observations is included has expectation 1 and variance 1. Then, provided you are computing a mean, the resampled mean has the right variance. That is the basic result. So for means it gives you about the right variance. However, unless the statistic is really meanlike, it doesn't work. I have stopped working on it because I decided that it really only works for statistics that are normally distributed and for those you don't need to do a lot of calculation to work out the distribution. You just need the mean and variance. By resampling, you are only working out the variance in a complicated way and usually jackknifing will do that just as well. So I feel that all the immense amount of calculation done in bootstrapping is more than is necessary for the problem.

Barry: Do you think that the bootstrap gets misapplied to statistics which are not meanlike?

Hartigan: Yes, I know it does. You can use methods which are more robust. For instance, if you take 1000 observations and compute a statistic based on subsamples of size 100, that's 10 of them, that will get you a valid distribution of the statistic at least for samples of size 100. You've just got repeats. It's a valid distribution even if it's not normal. All the other stuff mixes everything up together and unless it's normal, that's not valid. It seems to me to be more robust to just take straight subsamples. Of course, you might want the distribution for sample size 1000. If you don't have several samples of size 1000, then no robust solution to that problem is available.

CLUSTERING

Barry: You published your book on clustering in 1975 (Hartigan, 1975b). When had your interest in clustering begun?

Hartigan: It actually started during my Ph.D. at Princeton. I was interested in doing a thesis using a different notion of probability based on similarities and these similarities really come out of identifying objects as similar, which is a clustering kind of problem. Of course, there is the other aspect of clustering—just having algorithms that produce clusters—which is how it tended to be in the 1960s. There was a lot of classification going on in biology which was serious classification based on subject matter. Then there were some ad hoc algorithms being developed with very little theory. I gradually became interested in that—trying to produce some theories which would explain why these clusters might be better than some other clusters. My basic interest in clustering is not to produce algorithms, but rather because I think that classification is necessary as a foundation to probability.

THE MEANING OF PROBABILITY

Barry: You said that you talked a lot with Jimmie Savage during the couple of years that you were at Yale with him. What did you think of his theories of subjective Bayesianism?

Hartigan: I didn't like them much. I don't think a personal probability theory is sufficient for probability to have a proper impact. If you say that the probability that this drug will improve treatment is 0.8, you don't want to have to qualify that by saying, “Well, that’s just what I think.” You want it to have some public value.

Barry: What do you mean by probability then?

Hartigan: I think that probability should be a basis for action. If you say that the probability is 0.5 that the coin lands heads, it means that you regard it as a reasonable action to bet on either heads or tails. It is correct to translate into action in that way, but I don’t regard that as a definition of probability; that is just an alternative way of expressing what you mean. A probability has to be a considered statement based on all the things that you know and a statement that other people would also make if they knew the same things. It depends on your knowledge certainly, but two people with the same knowledge (yes, it is hard to know what you mean by that) should have the same probability and therefore find the same action reasonable.

So probability should be a recommendation for action, not a report on your own interior beliefs. Someone with different knowledge can have a different probability, but you can’t have someone with a deck of cards asking you to shuffle the deck and then saying, “My probability that the ace of spades is on top is $\frac{1}{3}$.” Am I supposed to sit there and not grin? That person is not entitled to his own probability, not entirely, not at all. So I think that probabilities should be prescriptive rather than descriptive.

Barry: But if somebody disagreed with you about the probability of an event, how would you engage them in argument?

Hartigan: Consider the probability that President Bush will be reelected. I wouldn’t say “it’s 0.3 and that’s the end of it.” I would say it is 0.3 for these reasons and, if he wanted to disagree with me, I would expect him to produce reasons, and I should listen to those reasons and maybe change my probability. That’s alright, because probability should depend on knowledge and, in fact, one of the reasons that we engage in argument is to extract knowledge. So I would go through what I regard as all the relevant facts for the possibility of President Bush being reelected and I would try to weight them. Now how you do that exactly is pretty hard I agree. Nevertheless in principle I think that is the way you should go about it. Now this person might come and say, “No it’s 0.7 and that’s the end of it.” (Laughter.) I would say, “No, it’s not the end of it. Tell me why,” and if he refuses to tell me why, then I regard that as an inadequate argument. I would expect him to say that these are my reasons and I might say, “Yes, I didn’t think of that. I will change my probability,” and I would expect him to address my reasons, to say, “No, you shouldn’t take any notice of those reasons.” I can’t see why there shouldn’t be a reasonable argument about these things.

Barry: Do you think that if the two people exchanged all the relevant information and still don’t agree, then one of them is stupid?

Hartigan: No, I don’t think one of them is stupid. If they still don’t agree, it means perhaps that they have a different idea of what probability is, which is certainly fair enough. In the *Foundations of the Theory of Probability*, Kolmogorov did a great disservice to probability because he said “It’s just a measure.” So everyone thought, “Oh thank God, we’ve solved the problems of the foundations of probability.” Of course we haven’t solved anything at all! That’s just mere technicalities. Countable additivity and measure theory and whether or not the axiom of choice matters and things like that constitute a total distraction from an understanding of what probability is. I regard Kolmogorov’s work as a great step backward, at least in the foundations of probability. In the mathematics of probability, it was a great step forward.

Barry: Do you think it’s important that we get a better grasp on dealing with probability?

Hartigan: Yes, I think so. I think that we should know what we are talking about. Most of the definitions in the books seem to be circular. It’s “the coin has probability one half if it’s a fair coin.” (Laughter.) Oh good that’s the way you tell it’s going to be a half, if it’s a fair coin. I think that we need to know what we mean by probability and that probabilities have to come out of something that is not probabilistic. That is how to get a noncircular definition of probability, and you have to be able to argue that this or that probability is right.

If you can have two well-reasoned people and one comes up with a probability of 0.2 and the other comes up with 0.8, I would say that there is a fairly unsatisfactory world of probability out there. I know that respected experts do come up with a wide range of probabilities and it is partly because they are not used to arguing about them.

There is an example in the report on the Challenger disaster, where Feynman wrote an addition to the report disagreeing with some aspects of the report. He wrote a number of things about probability there. In one of them he asked some engineers what the probability was of the Challenger falling out of the sky and they said one in a hundred thousand. Then he asked them what the probability was that one of the main engines would fail and again they said one in a hundred thousand. He said, “Where did the one in a hundred thousand came from?” and they said, “Oh it’s just that two or three years ago, in order to launch a rocket in Florida which was carrying a plutonium reactor, the state of Florida said we couldn’t launch it unless we had done an environmental damage study to show that the chance of it falling down and completely contaminating the state of Florida was really low. And they tried to think of a number. They thought one in a million sounds far too low but on the other hand one in a thousand sounds a bit threatening so they chose one in a hundred thousand.” In other words a purely public relations probability. I think a lot of probabilities are like that and we really need to do better than that. It would be good to have some established way of doing it.

The way that the statisticians do it is utterly feckless. They say, “Let’s make a probability model.” By that they mean “We don’t really believe it so therefore we don’t have to defend it and therefore you shouldn’t believe it either to start with. We will discover whether it is true or not by looking at the data.” That’s just crazy. You cannot discover the probabilities by looking at data. You may discover that you are grotesquely wrong in some direction, but that is all.

In a true Bayesian world there is no real point to estimation, testing or decision theory. You start with a prior distribution, make some observations, produce a posterior distribution and that’s the end of the story, and a true Bayesian world is one in which the probabilities are believable. We don’t have believable probabilities, so we are falling back on a Fisherian world.

I think that Fisher was mistaken to think that he could handle uncertainty without having probability distributions to express the uncertainty. He thought that he could make up a model for the probability distribution and that the data would tell him by a significance test whether or not it was correct. That’s surely absurd. No one would say, “Oh look. Here is a probability distribution in two dimensions and I have got a point from it. Is the probability distribution right or wrong?” People would think that you must be mad. Things don’t improve by being in 50 dimensions. You might be able to say that the point is out in the tail of the distribution, but you certainly cannot say whether the model is right or wrong. Anything you say is going to be tremendously dependent on the details of the probability distribution that is just being assumed in there as a model.

So I just regard that Fisherian program as totally feckless. It is not even effective because it turns out that when you follow through the various methods, you have to do things that are consistent with a prior distribution anyway. So here is a program that introduces these models which excuse people from presenting real probabilities and allow them to present fictitious probabilities which at some future time are going to be settled by the data. No chance whatsoever! The data are not big enough to settle the question of what the probabilities are. The probabilities have to come out of something else. There’s got to be a real way of assessing uncertainty and being persuasive about uncertainty, and that is not achieved by multiplying the amount of data by a hundred or by collecting more data. It’s done by having a proper philosophical approach to statistical methods and concepts.

THE FOUNDATIONS OF STATISTICS

Barry: In 1983 you published your book *Bayes Theory* (Hartigan, 1983). I have an impression that you are deeply interested in the foundations of statistics, but that you find none of the existing approaches entirely satisfactory. Would that be fair?

Hartigan: Yes, that’s true (laughter), but I suspect that nearly everybody would answer yes to that. I mean how can you find them satisfactory? I think that the division between Bayesian statistics and so-called frequentist statistics (I say “so-called” because no one really knows what they mean by frequentist statistics—it is very hard to find a definition anywhere that you can attack properly) has been healthy in that there are two ways to approach problems and, if they differ a lot, it makes you think about them both.

On the other hand, I do not find them sufficiently different. There is not enough of a difference between Wald’s decision theoretic view, which says let’s look at all the statistical procedures corresponding to all the prior distributions, and a subjective Bayesian point of view, which says let’s look at all the posterior distributions corresponding to all the prior distributions. The difference is not great enough to really give you a wide angle on what’s wrong.

What do I think is wrong? Well, for one thing, I think that the emphasis on prior distributions is totally wrong. Our problem is to determine a prior distribution? No, it’s not! That’s not our problem. Our problem is to determine any kind of a reasonable probability distribution whatsoever.

There is the belief that the prior distribution may not matter so much. We will learn enough in the future so that even if we did make a mistake with the prior, everything will be corrected. That is a totally false idea.

In fact, the original probability distribution we started with, that's the big probability distribution that's going to kill us. The assumption of independence that everyone makes gaily—how can that not be a dangerous thing to do? Fictitious probability based on models giving you permission to say anything you like, and therefore the answer is anything you like. That can't be a good way of reasoning.

Barry: What do you think about p values?

Hartigan: I find that I use them in data analysis. Fisher used them because he didn't want to have a full prior distribution and they have become set into statistical practice now. They look unnatural in a Bayesian light and many Bayesians reject p values out of hand for that reason. Most Bayesians reject frequentist ideas as being outrageous and the next thing that comes out of their mouths is, "Let's use these Bayesian techniques that match up with frequentist methods." Therefore, although they object to frequentist philosophy, they follow frequentist practice. I think that's the case with p values. To some extent you can convert them into Bayesian probability statements.

In fact, you can design a test procedure in a Bayesian framework that says you should do this test if you want to test a particular hypothesis against another. The real question is: Is that what you want to do? Often you don't and often you just contrive to do that because that's the only statistic available to you. It is certainly true that p values are very dominant in certain fields such as medicine and psychology. You don't get a 0.05, then we won't publish your paper. The really high class journals have to have a 0.01 for their results! Statisticians laugh when they hear that because they know that you can always get a good p value if a good statistician is at hand.

p values came out at a time when data were relatively scarce, but when you have a hundred million data points and a p value of 0.05, you suddenly want p values of 0.00001. When the p value is asking whether this particular hypothesis is true or false, the real answer is that this particular hypothesis is never true, but it might be good enough and the p value supposedly gives a hint about that.

Barry: You seem to have a very strange attitude, which is that you don't think that we have any real idea of what we mean by probability or probability models, that choosing a likelihood function is a very dangerous thing to do and so on. You sound very much like John Tukey and yet your publications are nothing like John Tukey's. The majority of your publications use probability models.

FIG. 3. *John with granddaughter Bia, 2002.*

Hartigan: It is true that I am almost two different people when I write for *The Annals* and when I talk to somebody in my office about a real statistical problem. (Laughter.) When you are doing mathematical statistics, you make certain assumptions and say what the consequences of these assumptions are, but there are the other questions as well. Is that the kind of assumption that applies to real life? Because that is what you better check when you are talking to the guy in the office.

I feel that when I am talking to the guy in the office, I certainly don't want to be saying, "Let's make this big complicated probability model and let's look at the law of the iterated logarithm to see what the convergence rate is." They're gone before I finish the sentence. When I'm talking to the guy in the office, I'm asking where did you get this data from, why did you collect it and is that measurement really accurate. Essentially asking about the data quality and about the relevance of the data. I'm talking about graphs and simple statistical methods that might help him to see the things that he wants to know about. You really have to be skeptical about assumptions, especially independence assumptions. Lack of independence often doesn't affect estimates much, but affects your estimates of error a lot. That's where people make their biggest errors: When they take seriously a model that somebody just dreamed up, act as if it is true and make predictions that are absurdly narrow. I have done it any number of times myself. I make a prediction based on a model. It turns out that I forgot something or they didn't tell me something and the estimate is over here and everything else is over there.

THE LAST 40 YEARS

Barry: What do you regard as the greatest contributions to statistics over the last 40 years? Say since 1960.

Hartigan: I think if I was forced to choose the greatest contribution, I would probably choose S-PLUS. I also think that the Internet has made a great contribution in that data that used to be hard to get hold of are much more available these days. As a result, people are much more realistic when thinking statistically. They pay attention to data much more and do try to find models that fit the data. These are both statistical computing things that have had a very big effect on statistics and will continue to have an effect.

Barry: What about on the theory side?

Hartigan: The paper that I regard as the most astounding is Stein's result on regression which shows that the usual estimates that everyone has been using since Gauss can be shown to be improvable using decision theory (Stein, 1956). Nobody thought that decision theory was ever going to do anything useful and so they were amazed when Stein actually produced this result. It's a result that just permanently amazes me. Decision theory tends to be regarded as a bit old hat these days among statisticians, but it's fundamental anyway. What else is really critical? I think the empirical process theory is important. I'm skeptical about a lot of statistical theory because it depends so heavily on independence.

Barry: It has been said that the use of Markov chain Monte Carlo methods has revolutionized the practice of Bayesian statistics. What do you think of MCMC methods?

Hartigan: They're alright, but this is another one of these panaceas like the bootstrap. It solves our problems very well, just like the bootstrap does, when we have a normal distribution. If you have a normal distribution for your parameter and you generate it by MCMC, it will wander around in about the right way and the proportion of values that you get in any particular region will roughly correspond to what the normal distribution says it is. Of course, the trouble is you really only need to estimate the mean and the variance. MCMC suffers from the defect that in a very complicated situation with a zillion modes, it is too easy to spend all your time darting around a particular mode and to end up with a very poor estimate of the real distribution. So I think you have got to be very cautious in using it. On the other hand, it gives you a way to solve very hard computational problems that you cannot do otherwise. I avoid it if I possibly can. If I'm doing, for instance, a big regression problem with lots of possible predictor sets, rather than wandering around by MCMC, I want to report all the highly probable predictor sets, all of them. That seems to me to be a better report. In order to do MCMC, you really need to solve an optimization problem first, namely to identify the maxima. I think that MCMC is really a small sister of annealing. First you have to do a good annealing step, but, as everyone knows, it is in fact impossible to do optimization in general, so therefore it is impossible to do what I'm saying—unless it's normal and you know, for instance, that there are only two modes.

Barry: Do you think that the statistics profession is an effective research community?

Hartigan: Yes, I do, in a funny kind of way. If you compare statisticians and mathematicians in a university, then the mathematicians certainly have higher prestige, but the statisticians are the people that everybody is talking to. People think that statisticians can do them some good. The theory of statistics might be a bit of a shambles but the practice isn't such a shambles. Statisticians have seen a lot of data sets and they can help people with things, and there is a huge amount of statistical work out there and a lot of demand for the product. Statisticians have many useful tools, such as graphical methods and practical computational methods. The theory is at a level beyond that. The theory doesn't have to work perfectly for the applications to work not too bad.

THE NEXT 40 YEARS

Barry: What do you think researchers should be focussing on in the next 40 years?

Hartigan: I am sure that questions of data collection and data quality will become increasingly important. Designing, for instance, a web interview method so that when a person answers a question a certain way, you then ask them an appropriate different question. As far as I know, there is absolutely no method of analyzing data obtained in that way. None of the experimental design people seems to work on that. The census is going to be carried out differently 20 years from now. It is going to use the Internet one way or another. That is going to be very important and we have to begin thinking about how that should be done. In doing data analysis, I have become impressed with how important data quality is. It used to be that there were only 200 values that you had to check. Now it's 200 million and if it's

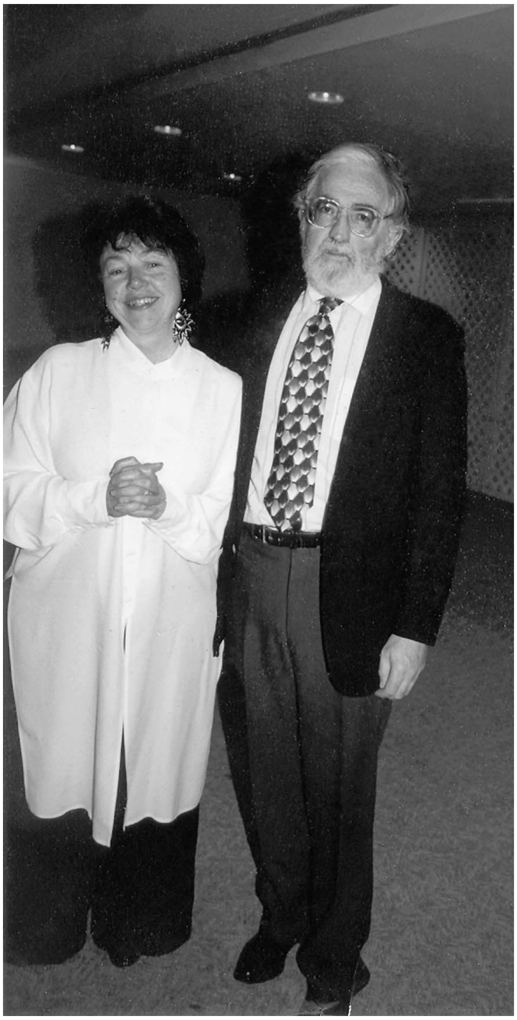

FIG. 4. *John and Pam Hartigan at John's 65th birthday celebration in New Haven, 2002.*

bad, you might not even notice it. So I think that statisticians will become interested again in experimental design, survey sampling and data quality questions in general.

My other big thing, of course, is that we must develop a believable theory of probability. That's what I regard as really important.

Barry: John, that was great. Thank you very much.

Hartigan: OK. Thanks, Dan.

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REFERENCES

FORSYTHE, A. and HARTIGAN, J. A. (1970). Efficiency of confidence intervals generated by repeated subsample calculations. *Biometrika* **57** 629–639.

HARTIGAN, J. A. (1964). Invariant prior distributions. *Ann. Math. Statist.* **35** 836–845.

HARTIGAN, J. A. (1965). The asymptotically unbiased prior distribution. *Ann. Math. Statist.* **36** 1137–1152.

HARTIGAN, J. A. (1966a). Probabilistic completion of a knockout tournament. *Ann. Math. Statist.* **37** 495–503.

HARTIGAN, J. A. (1966b). Estimation by ranking parameters. *J. Roy. Statist. Soc. Ser. B* **28** 32–44.

HARTIGAN, J. A. (1969). Using subsample values as typical values. *J. Amer. Statist. Assoc.* **64** 1303–1317.

HARTIGAN, J. A. (1971). Error analysis by replaced samples. *J. Roy. Statist. Soc. Ser. B* **33** 98–110.

HARTIGAN, J. A. (1975a). Necessary and sufficient conditions for asymptotic joint normality of a statistic and its subsample values. *Ann. Statist.* **3** 573–580.

HARTIGAN, J. A. (1975b). *Clustering Algorithms*. Wiley, New York.

HARTIGAN, J. A. (1983). *Bayes Theory*. Springer, New York.

HARTIGAN, J. A. (1998). The maximum likelihood prior. *Ann. Statist.* **26** 2083–2103.

STEIN, C. (1956). Inadmissibility of the usual estimator for the mean of a multivariate normal distribution. *Proc. Third Berkeley Symp. Math. Statist. Probab.* **1** 197–206. Univ. California Press.