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A Conversation with Oscar Kempthorne

J. Leroy Folks

Abstract. Oscar Kempthorne was born on January 13, 1919, in St. Tudy, Cornwall, England. His schooling was in Cornwall. He received a B.A. with Honors from Cambridge University in 1940, an M.A. from Cambridge in 1943, and was awarded an Sc.D. degree from Cambridge in 1960. He worked at Rothamsted Experiment Station from 1941 to 1946 and joined the Statistics Department at Iowa State College as Associate Professor in 1947. He was promoted to Professor in 1951, and was named Distinguished Professor of Science and Humanities in 1964. He was President of the Biometric Society, ENAR, in 1961 and President of the Institute of Mathematical Statistics from 1984 to 1985. He is a Fellow of the American Statistical Association, the Institute of Mathematical Statistics, the American Association for the Advancement of Science and an Honorary Fellow of the Royal Statistical Society. He was awarded an honorary Ph.D. degree from the University of Ioannina in Greece.

This conversation took place at Dr. Kempthorne's home in Ames, Iowa on March 7, 1994.

Folks: Kemp, I have enjoyed watching your ASA videotape "From Observation to Inference" (ASA archives) and I know about the videotape of your lecture "The Adventures of a Cornish Farm Boy in Search of Statistics" (Kempthorne personal copy). I am very interested in your university days.

CAMBRIDGE

Folks: I have an old snapshot (Figure 1) of Clare College at Cambridge. Do you recognize the college from this picture?

Kempthorne: Yes, my room was that one—the third room from the corner on the third floor.

Folks: Tell me a little about your university background.

Kempthorne: I am to tell you about my university background. However, to do this, I should revert to earlier things; that is, how I got to the university and what my education was before that. I was born into a farm family in Cornwall. I think I grew to dislike the hard work in farming immensely. I went to a village school which had a headmaster who put me on my path. Then to a high school where I discovered that I liked mathematics. I found that the teacher of mathematics wasn't very good, so I learned mathematics myself. I wanted to go to Cambridge, so I took scholarship exams and various other exams and was awarded scholarships to go to Cambridge.

I had hoped to recapture at Cambridge some of the joy that I had received when I learned beginning mathematics in high school. It didn't really happen because I found most of the lectures at Cambridge were not at all inspiring. In fact, there was a curious thing. It seemed as though the most creative people were the worst lecturers. I have wondered about that ever since. I did a lot of mathematics—real variable and analysis. In the first two years I took courses on mathematics only. I have notebooks of the courses and I see from these that I had real analysis, measure theory, complex analysis, differential equations, linear algebra, theory of numbers, analytical dynamics, mathematical physics, hydrodynamics, electricity and magnetism, quantum mechanics and mathematical statistics. I didn't really cover the Lebesgue integration in great detail. There were several excellent lecturers: Cunningham, Ingham, Burkill and Goldstein. And a few very poor ones. I got turned off from pure mathematics because it didn't seem to be going anywhere, and then I stumbled into mathematical physics, but I didn't really dig that because I hadn't yet done much physics. Then I had a first course in statistics from John Wishart during my second year that seemed to lead somewhere.

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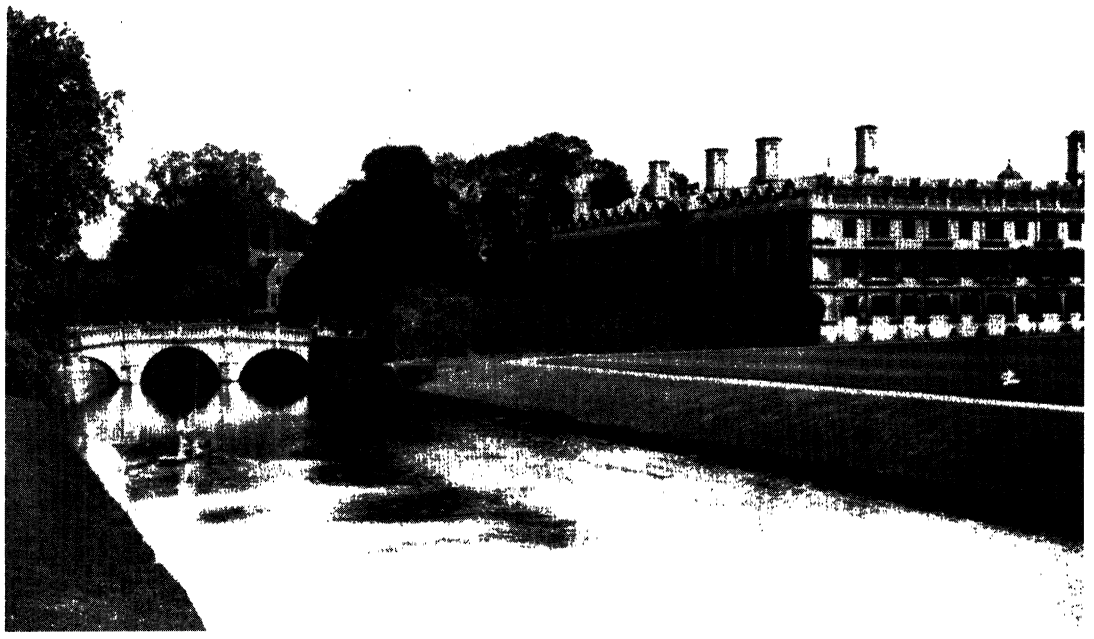

FIG. 1. *Clare College at Cambridge.*

Folks: Did Wishart use a textbook?

Kempthorne: No, he used notes. I still have a copy of the notes. He wasn't a bad lecturer, but he was somewhat uninspiring. I think that was because he didn't have any status at Cambridge. He was a reader in agriculture, and pure mathematicians didn't think much of him. Cambridge was pure mathematics and mathematical physics. That was it.

Folks: Going back to when you first went there on a scholarship, the biographical material which you provided me says that you had an exhibition. What is an exhibition?

Kempthorne: An exhibition? Well, let me describe my scholarships. I was in advanced school examinations and thus was a candidate for a state scholarship. There were three hundred in all subjects in England and Wales and I got one of those. Then, I got a couple of county scholarships, but I also tried for a scholarship at Cambridge. They had scholarships for 100 pounds a year, 60 pounds a year and then awards called exhibitions of 40 pounds a year. I got an exhibition, although the competition was very keen. Altogether I had enough to pay the whole bill. After a year, when I got a first class result on a first year exam, the College raised my exhibition to a scholarship. So, that's an exhibition. I was really very pleased.

I have already mentioned that I was very interested in mathematical physics, but I hadn't done the basic stuff. I couldn't really see the connection of their models to data. It seemed as though mathematical physics came out of pure thought. But, of course, it didn't. So, anyway, I stumbled into statistics, and it seemed as though statistics would be useful. I think it was particularly the case that Fisher, when he wrote about statistics, was talking about agricultural experiments. The idea that mathematics should be useful and could be useful in agricultural research really struck me. So I had a third year, two-quarter (two-term) course, from J. O. Irwin, who did more statistics, very much Fisherian, and that was my statistics.

I had my three years at Cambridge, and that was the total of my university education. I calculated once that I had a total of 864 lectures—three years of three eight-week terms, each with 12 lectures per week.

Folks: Did you have any courses that interested you in philosophy?

Kempthorne: No, I didn't really understand what philosophy was about or that there were such people as philosophers. And I am not much better off now about that. But how is one to get some understanding of what causes growth in agriculture and plants or animals? The idea that mathematics and statistics could be useful in that really impressed me tremendously.

It is relevant to note that my experience at Cambridge was greatly marred by the occurrence of World War II. It is important to understand the period of my attending Cambridge. When I went up

to Cambridge at first, in September or October of 1937, the world was very much uncomfortable about the activities of Germany and Italy, going back to the civil war in Spain, and then the invasion of Abyssinia and, of course, the activities of Japan. Anyhow, I went to Cambridge with no problems and studied rather hard for the first year. At the beginning of the second year, peace-time conscription was introduced. I was conscripted in 1938. I was not comfortable; I felt that war was coming and I saw little future for myself because I thought I would end up in the trenches. However, that did not happen, but I didn't study very much. Then in my third year, war broke out but I was told to finish my university studies. I finished those in May of 1940.

In my first year at Cambridge, I worked very hard and obtained a first class pass in mathematics. In the second year, I was less comfortable, and did not work very much. But I managed to obtain a first class degree, called a Wrangler, in May 1939. Now in the third year, with war being on, I really didn't work at all. I went to various lectures, and spent about five hours a day for five days a week. So I finished that year and then took the mathematical tripos, part three.

Some time in my third year, I went to a recruiting board and I was given a dirty piece of paper saying that I was reserved to do technical work. Consequently, I did not get involved in the armed forces at all. After three years at Cambridge, I spent a term working with J. O. Irwin on a collaborative drug assay. In December, I joined the Ministry of Supply in the Department of Tank Production.

FIG. 2. *Kempthorne at Cambridge University in 1939.*

Folks: Where was that?

Kempthorne: It started in London, but then in December of 1939 the group in which I worked was evacuated to a place called Bromsgrove near Birmingham. I didn't do much there. This was a Ministry of Tank Production and the idea was to keep track of how many tanks were being produced. The production was pathetic. The thing that saved the day was American tank production. They could produce tanks by the hundreds or thousands. While there, I really didn't do much work, but spent a lot of time arranging bus trips to Stratford-on-Avon to watch Shakespearian plays. Then, I guess in May of 1941, the man who was running the Ministry of Supply group suggested that I might like to look for a job.

ROTHAMSTED

Folks: How did you happen to go to Rothamsted?

Kempthorne: Well, I don't know really, except that perhaps Yates was looking for someone. Fortunately, I heard from the University of Cambridge appointment board that there was the possibility of a job at Rothamsted. Because of the courses that I had taken, I had been very much attracted to statistics, and the idea of going to Rothamsted had great appeal. I went to Rothamsted and worked on experimental design for agricultural experiments and I worked on the field plot committee for Rothamsted.

Folks: What did they have you doing? Did you do some of the computations?

Kempthorne: I did some computations, but I was scared stiff. Someone gave me a randomized block experiment to analyze. I didn't know what was going on. But I did the computations according to Fisher and Snedecor. That was okay; they accepted that. But then I got involved rather heavily in the analysis of a farm survey. A national farm survey of England and Wales yielded a massive amount of data that had to be summarized. I was in charge of that. I learned that those survey results were summarized by using Hollerith (British Tabulating Machine Company) equipment. I began to learn a little about semimodern computing. There were six part-time clerks and a person who had a bachelors degree who supervised them and I supervised her. I also worked under Yates, who was working with Solly Zuckermann on operations research associated with the war effort and, in partic-

ular, with analyzing the effect of bombing on Italy and Germany. This work was done under the direction of Zuckermann, who continues even until the present day to be a leader of thought in Great Britain.

THE GREEK MISSION

Kempthorne: My relief from Rothamsted came as a result of the civil war in Greece. It is ironic that the foundations of democracy were laid down apparently by the Greeks some 2000 years ago—when there was a small population of educated people and a big population of slaves. The Greeks had trouble resolving their political life. In the thirties Greece had been governed by a dictatorship. Then with World War II and the initial conflict of the Western world with communism and Russia, there was a considerable development of a communist thrust in much of Europe. During WWII the Greeks worked hard to stop the Nazis—the Germans—and a left wing party developed. Now this was in conflict with the right wing party, and after the conclusion of WWII, there was a civil war —a fight between the right and the left. The allied governments (that is, the United States, Great Britain and France) decided that there should be an election and that this election should be monitored by them, which is really a rather interesting political move.

The United States State Department decided to form a technical mission to aid in the observation of the Greek elections. This technical mission consisted of political people, but critically it depended on a technical staff of statisticians. Now, for some reason that I do not understand, the United States pushed the idea of having a sampling group. So a technical statistical group was made up to assist in the observations and the evaluation of the election. It was led by Ray Jessen (a leader in real work in sampling) of what was then Iowa State College, and it included on the staff W. Edwards Deming, Jerzy Neyman, Joe Daly and Arnold King from the United States and me from Great Britain. There should have been also a person from France, but I believe the conclusion, erroneous or not, was that there wasn't anyone in France who could really lend technical support. So off we went to Greece. We—Jessen and the team—set up a sampling scheme in which a sample of precincts was drawn and observations were made on the sample by army officers. The survey was done, the report was made and it was concluded that the election result was fair. The result of the election was that the right wing group won the election and the left wing group lost. An outcome of this, which I heard about only recently, was that the left wing people were badly treated by the right wing people, and that many of them had to leave Greece in order to survive. The election was a very important part of the development of politics in Greece.

I spent perhaps three months in Greece working on this mission. A paper on this whole plan was written by Jessen, R. H. Blythe, Deming, and me [Jessen, Blythe, Deming and Kempthorne (1947)]. It is of interest that Neyman was dismissed by the mission for actions contrary to its policies.

Folks: Was your experience with the national farm survey in England one of the reasons you were chosen to go on the Greek Mission?

Kempthorne: I think so. I don't know exactly how that happened. Maybe Cochran put my name forward to the people at Iowa State. But, anyhow, one day I got this letter from the foreign office to go on this allied mission.

Folks: Thinking about it now, it seems like it might have been dangerous.

Kempthorne: Well, I suppose! I lived in Athens and the fighting was away from Athens, so I wasn't really worried about it. After the work in Greece, I went back to England and looked at one or two jobs in Great Britain, but they didn't appeal. Then I got this offer from Iowa State.

IOWA STATE

Folks: I have heard you say that you had planned to be at Iowa State two years. Was it a temporary appointment?

Kempthorne: Oh, no! This was a permanent appointment as Associate Professor. To some extent I tried to wiggle out of it because I was concerned about paying the fare to come, but Iowa State put up six hundred to a thousand bucks to pay my transportation. So I arrived in Ames.

Folks: Was that by train?

Kempthorne: By train. Now that was a memorable journey: New York to Chicago was okay, but from Chicago to Ames, that was exceptional! The only thing that I can think of that is comparable is in the movie, *Dr. Zhivago*. Did you see that film? In the winter!

Folks: So you came to Ames by train in January of 1947.

Kempthorne: In January, yes. The Chicago Northwestern railroad came straight through and the people here made me somewhat comfortable.

Folks: I came here in 1955, so I have an idea of what Ames was like in the winter of 1947.

Kempthorne: There was not much rental of apartments. I got a small apartment on Forest Glen right opposite where Snedecor lived. I was there for

FIG. 3. *Kempthorne with R. A. Fisher, Ames, Iowa, 1952.*

quite some time and was living there when I met my wife Val. We went up to Vancouver and got married and came back and rented an apartment that Art Dutton had, so that started me off. We lived in that apartment for a time and then rented a duplex and finally bought a house on Friley Road.

Folks: Who were the faculty members in statistics when you came?

Kempthorne: Let's see. Bill Cochran had gone to North Carolina State. Snedecor retired in 1947 and then Jessen was made Acting Head of Statistics. Arnold King worked on sampling. Then there were Gerhard Tintner, Lee Crump, Paul Homeyer, Alex Mood and George Brown here when I came. But George Brown and Alex Mood left after a year or so and went up to Rand. So the staff was very slim.

After they left, we all pitched in and did our part. In 1947–48 I taught seven courses in three quarters. Not only design of experiments and linear models, but also inference and Cramér-type stuff.

Folks: I know that Snedecor had external grants very early in statistics here. How were things funded then?

Kempthorne: Well, the big support was for the Master Sample of Agriculture that was supported by the U.S. Department of Agriculture. That was the big thing.

Folks: Tell me about the Master Sample.

Kempthorne: I would say that Snedecor's contract with the U.S. Department of Agriculture provided the foundation for the Iowa State Stat Lab. It provided funding for a number of staff members, clerical staff and graduate assistants. The work that was accomplished went a long way in establishing random sampling as an accepted method.

The Master Sample of Agriculture was a joint project of the U.S. Department of Agriculture, the U.S. Bureau of the Census and the Statistical Laboratory and was used in the 1945 Census of Agriculture by the Census Bureau. Of course, this was before I came to Ames, but I have been told that there were 150–200 people working on the Master Sample. I don't know exactly where they put all of these people. They drew an area sample of the whole United States and had interviewers going out to determine the status of agricultural production.

To draw an area sample of the United States, they had to obtain or hire a group of people working on maps to draw the samples. Then they had to arrange for interviewers. This was a very big mapping operation at the Stat Lab and it has been going on ever since.

Folks: About how many students were here when you first came? Who were the students you remember right at the beginning?

Kempthorne: I suppose about a dozen. Mostly Ph.D. students. Bob Steel and Osmer Carpenter. I was on Bob Steel's examination, but I didn't have any role in directing his thesis. I think he was directed by George Brown. So then there were Lee Crump and Walt Federer; Paul Homeyer was pursuing the Ph.D. degree for a time.

Folks: Who were your first Ph.D. students?

Kempthorne: Let's see. I have mentioned Bob Steel, but I just did his final exam only. Then Osmer Carpenter and Bernie Ostle, Art Dutton and Frank Graybill, Max R. Mickey, Virgil Anderson and Dan Horvitz. Martin Wilk, John Hofmann, and then you, Leroy.

Folks: I'm one of your earlier students actually.

Kempthorne: Oh yes. Well, you finished your Ph.D. degree in 1958 and you were my eleventh Ph.D. student.

Folks: It's sobering. From 1947 to 1989 you were the major advisor of 42 Ph.D. students.

Kempthorne: Ah yes! Thinking back on this, I realize that I benefited greatly from association with some very fine students.

Folks: How did the department function in the early years? At many universities, departments function much differently than they did just a few years ago. Of course, there are federal procedures and so forth. Was there something like a council of senior members or...?

Kempthorne: Well there was a graduate faculty here and I became a member of that at once. I don't

know why, because now the young people come in and they have to satisfy requirements, but I was a member of the graduate faculty immediately. The department was supported partly by Agriculture, and there was a department in Arts and Sciences, and there was a statistical laboratory that got some funds, so there was a tripartite operation very early.

Folks: Were there monthly meetings or frequent meetings of the department faculty or did you ever vote on anything?

Kempthorne: No. We didn't vote on anything. But, of course, it was a small group, so we just solved our problems. We were all capable at these things. Now we have staff meetings and vote on everything.

Folks: After I left here as a student and came back a few years later, you had a group of your own.

Kempthorne: Well, a small one. Yes, and I believe it was a significant group. Of course, there was another group in sampling.

Folks: Did it have a name? How did you refer to this group?

Kempthorne: The Experimental Design and Genetic Statistics Group. Eventually the department gave me a suite of offices in the west end of the building. I don't know where I had an office when you first came here. I think it must have been in the east end of the building in the main office area.

Folks: Then, you were in the main office next to Bancroft.

Kempthorne: Next to Bancroft, yes. But some time there, some year somebody got some funds and remodeled when computing came in. You know Mary Clem? She had a whole group of people there in that part of the building. Then her group went out and they remodeled that end of the hall with a suite of offices for me, George Zyskind and a genetic statistician.

GENETIC STATISTICS

Folks: I know from what you told me previously, and also from the tape that Noel Cressie did for ASA ("From Observation to Inference"), that in your first years you were involved in lots of things, but that is when you did the genetics.

Kempthorne: It was obvious that there was a deep interest in genetics at Ames. The School of Agriculture was very strong, with J. L. Lush in animal breeding and G. F. Sprague in plant breeding. These men were world leaders in their areas. The strength of Iowa State at that time was that it was the leading school of agriculture in the United States and the world. This was traceable, I think, to Snedecor and his teaching of statistical methods. It was natural that, having had to learn some genetics from reading Fisher, I became deeply involved in teaching genetic statistics. It is of interest that Lush and Sprague attracted good students from all over the world and I interacted with all of them. Because of this interaction, I wrote my book on genetic statistics [Kempthorne (1957)]. The people in animal science used statistics heavily, and so I was a consultant to that group. I found that they really didn't know enough statistics, so I started this course on genetic statistics and started working with people in plant breeding. Of course, I was aware from say, age five, of the problems in agriculture, on plant management, etc. It was obvious that genetics was a major force and this was recognized at Iowa State. The problem was to decide which individuals to select and the regression of the offspring on parents was a source of information. I got involved in the covariance of relatives and I ran that for quite some time. I had to get through Fisher's (1918) paper which was about random mating and some assortive mating, and with some faculty—L. N. Hazel, B. Griffing, A. W. Nordskog—I did something about selfing and then autotetraploids, and so I wrote papers on those things. Correlations of relatives and random mating populations, correlations of relatives in tetraploids, then covariances of relatives under selfing. I was trying to understand what was going on in this business.

Folks: Your genetics book is still used, isn't it?

Kempthorne: Oh, it's still used a bit today. Actually, I am still very proud of it. But people don't look at it enough. So, anyway, I did that stuff and served as a statistical consultant.

Folks: But I don't remember that you were teaching genetic statistics when I came as a student.

Kempthorne: No, I taught it sometimes, but then we hired Ted Horner and he was teaching it.

Folks: After you started working on these papers, did that change some of the practices here in plant or animal breeding?

Kempthorne: Well, I suppose it had some impact. In animal breeding the leader was Lush and he thought in terms of path coefficients, strictly the Sewall Wright stuff; so I had to wean these people away from that with these covariances of relatives and components of variance.

Folks: We may want to come back to this later, but I still see new books in which the authors use path coefficients.

Kempthorne: Oh yes. Well, it's a way of adjoining to observed correlations a model of causation. And I described this fairly well in the genetic statistics book.

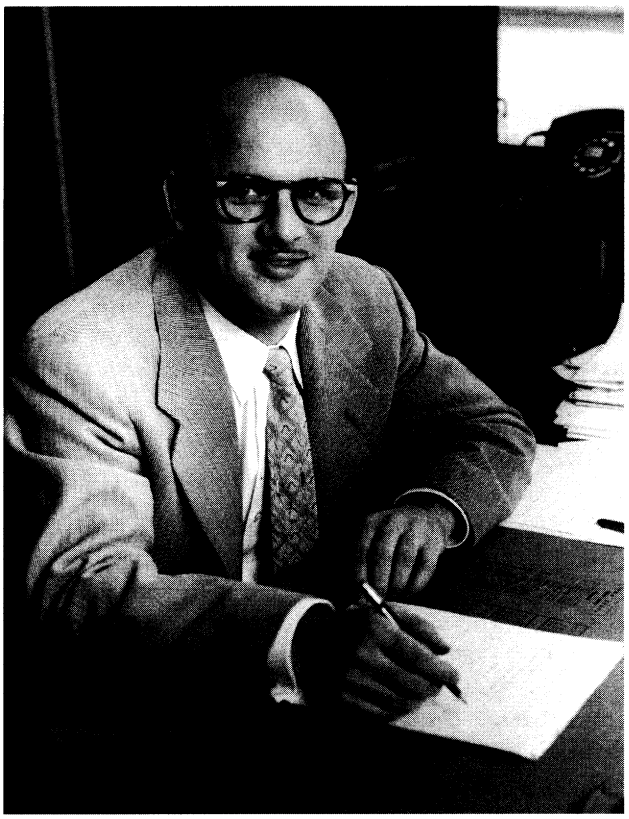

FIG. 4. *Kempthorne in 1969.*

Folks: What do you regard as your most important contribution to genetic statistics?

Kempthorne: In genetic statistics? Well I suppose this work that I did on covariances and relatives was very important, but then the whole game has been altered extensively by Watson–Crick; though, in fact, the ideas of Mendelian genetics are still there. The whole business is based on the theory of genetics. I got involved in Fisher's fundamental theory of natural selection and tried to understand that and then wrote a whole lot about fitness. Biologists write about fitness without ever saying what it is, and mathematicians write reams about "fitness."

You know, why did I get involved in genetics? Ah! Because, the ideas of Mendelism are quite deeply established and it is very much like factorial experiments, so if one has messed around with factorial designs and so on, then to consider genetic variation is sort of a natural step.

Folks: Isn't it true that in recent years there have been questions raised about the inadequacy of the idea of natural selection based on survival of the fittest?

Kempthorne: Well, you see the problem with that is we do not know what fitness is. Ed Pollak and I messed around with that back in the early seventies [Kempthorne and Pollak (1969, 1970, 1971)].

Folks: So, the survival of the fittest model explains things, if we know what fitness is.

Kempthorne: Yes. I guess that's it. But there are obscurities about how to handle overlapping generations and mating and stuff like that.

Folks: We look at two sea shells. One is spiraling clockwise and the other one counterclockwise. If we had the whole story on fitness, would we understand the reason for the direction of spiralling?

Kempthorne: Yes. I wrote an evaluation of current populations genetics theory in 1983 [Kempthorne (1983)]. That went down like a lead balloon. I don't think anybody takes any notice of it. I thought and still think it was a good paper.

Folks: I heard your invited paper at the 1979 ASA program in Washington, "Where is the Fault, Dear Brutus, Genes or Environment?" I know that you served as an expert witness in the Shockley versus Witherspoon trial. What was that about?

Kempthorne: Well, William Shockley is a world-famous physicist. He and two other scientists shared the 1956 Nobel Prize in Physics for invention of the transistor. But Shockley ventured into scientific matters beyond his depth. He was quoted in *Time* magazine as saying that "because of heredity, blacks as a group suffer intellectual and social deficits!" Shockley went so far as to advocate selective breeding on the basis of IQ.

Roger Witherspoon claimed that Shockley's program was tried out in Germany during World War II. Shockley said that Witherspoon's article identified him as the "author" of "The Holocaust" and sued Witherspoon. I was called as a witness to testify on Shockley's incompetence in genetics, which I did because he equated correlation and causation. The jury agreed that Shockley had been libeled and awarded him one dollar in damages which may be compared with his claim of one million dollars in damages.

Folks: Well, Kempie, did you reap any critical comments from your talk about the matter of genetic differences between different ethnic groups? Did you experience any unfavorable results?

Kempthorne: Oh no! I wrote this paper that came out in *Biometrics* [Kempthorne (1978)]. That thing went down very well, very well indeed, and I still get requests for it, but whether these people who want it really read it is another question.

Folks: A different matter?

Kempthorne: I saw somewhere the comment that I wrote about genetics as if I don't know the front end of a horse from the hind end. Stuff like that. That's why I emphasize my origins on a Cornish farm.

Fisher was, of course, concerned with the genetic quality of humans while he wrote this book. Let's see, what was the title of that? *The Genetical Theory of Natural Selection* [Fisher (1930)]. He was trying to develop a mathematical theory of natural selection. And it is obvious that evolution proceeds by natural selection, and that the way it operates is by fitness, which is viability, variability or reproductive variability. So, if individuals die young, they don't contribute as much to the future generations. Then, if they don't have children, that is the end of their contribution. So Fisher produced this theorem he called the fundamental theorem of natural selection. This theorem is very statistical and it says that the rate of increase of fitness is equal to the additive genetic variance in fitness. I have been concerned about that and I wrote some stuff in the genetics book in 1957 about this theorem, which I guess nobody has looked at very much. I have come to the conclusion that this fundamental theorem just doesn't wash. This theorem is very much related to the "additive genetic variance of fitness." But then when I looked at selfing, for instance, the additive genetic variance just doesn't enter in because the covariances of relatives do not depend on the additive genetic variance, which is defined by least squares fitting. The additive genetic variance is the amount of variance in fitness that is explained by a gene model which is additive in regard to gene effects. This idea does not work in genetic populations that reproduce by selfing and it doesn't work except for single locus populations that reproduce by random mating. So, I came to the conclusion that this theorem just doesn't hold.

This is a very curious situation, because Fisher made two contributions which he thought were important. The first of these great contributions was fiducial inference and the second was on the fundamental theorem of natural selection. At one time I just didn't buy either. I sort of have a quasi-open mind on fiducia because Fisher said that the sort of inference that one can do is based on significance tests and then the inversion of these tests gives fiducial inference. I found this all very mystifying because it wasn't at all clear to me what the Fisher definition of significance test is, and I never really understood fiducial inference. The only comfort that I have is that I don't really think anybody else has understood it. People write about it and don't make sense.

Folks: We will get to fiducial inference later, but let us talk about it right now. Sometimes I thought that Fisher's fiducial distribution was simply a way of getting a posterior distribution without using a prior. But I couldn't quite understand the logical process that Fisher went through. But maybe it doesn't matter if it yields a distribution. Do you suppose Fisher made a mistake?

Kempthorne: Fisher felt that inference was based on significance tests. He claimed that the fiducial distribution results from an inversion of a significance test. This makes fiducial intervals very much like confidence intervals which result from inversion of accept–reject rules. So there we are, I guess. Obviously I should address the matter of what Fisher did about inference. He realized the defects of the Bayes process and felt that he had made a significant advance by putting forward fiducial inference.

EXPERIMENTAL DESIGN

Folks: Let's come back to that one. I am getting out of order in my thought process of how things went here at Iowa State. You worked in genetic statistics and developed a course and published a book on it, but, of course, you did other things. The experimental design book, [Kempthorne (1952)] came before the book on genetic statistics. When did you start working on that?

Kempthorne: I started working on that soon after I came here.

Folks: Had you thought of writing the book before you left England?

Kempthorne: I guess so. I started writing that book in 1948, and sort of got it finished by late 1949, and sent it to Wiley in 1950. Then it was a year and a half getting it published. I guess it came out in January of 1952.

Folks: You used a matrix algebra presentation of linear models. Had you learned it that way previously?

Kempthorne: No, I suppose that really came because George Brown had been teaching linear models here and I took over that course. And I found that he had used matrix algebra, so I took it over.

Folks: I spent a lot of time reading through that book when I was a student. Not reading it! Studying it and correcting errors and adding little details that were missing as far as I was concerned. I really liked the subject. What's going on today? Is experimental design still a vital subject?

Kempthorne: Most of the basic designs are pretty much set and laid out.

Folks: Are you aware of any students doing dissertations in experimental design at Iowa State?

Kempthorne: No. Harville came on the staff in 1984 and he wrote a paper entitled "Experimental Randomization: Who Needs It?" [Harville (1975)]. And then Bob White gave a paper saying, "I Need It" (unpublished). The people who are doing the

basic level consulting and who are teaching statistical methods, still use randomization. So I don't know what is happening.

Folks: One area very much alive is the use of fractional factorials in industry. And, more broadly, the Taguchi methods.

Kempthorne: Well, fractional factorials! There is an interesting story on that. That was done at Rothamsted when I was there. Finney and I were talking about it on a Friday and he came in Monday and said, "Here is the way it is," and wrote a paper on it [Finney (1945)]. Then later I wrote a paper on fractional factorials [Kempthorne (1947)]. It led to a simple approach to confounding and fractional replication in factorial experiments. Finney hasn't done very much since on that.

Taguchi is a latecomer to the use of fractional factorials. He came here three years or so ago and gave a seminar. He reminded me of Ed Deming. Ed Deming gave many seminars on ideas that seemed well known, but, obviously, he had an enormous impact.

Folks: It surprised me that late in his life he achieved such...

Kempthorne: Such eminence. Yes.

Folks: But then some people do achieve prominence in another career after they retire.

Kempthorne: Oh, yes.

Folks: Aren't the orthogonal arrays of Taguchi just simple fractional factorials?

Kempthorne: They are just simple factorials. He has not discovered any new ones, as far as I know. Two to the fourth in eight observations, two to the fifth and so on, and two to the seventh in eight observations. Then two to the eleventh in twelve. Taguchi was pushing these very small fractionals and doing a lot of them. So, in that respect, I guess that was a contribution.

Folks: Now, in some of his orthogonal designs does he use things like combinations of a half and a fourth?

Kempthorne: No, I think not. He uses sort of regular designs like those of Plackett and Berman.

Folks: What do you think about his idea that not only the mean, but also the standard deviation, may depend upon factor combinations.

Kempthorne: That is a very interesting thing. What Taguchi was saying was that people manufacturing products should think about not just the mean of the output, but also the variability. People developing these things should think about their use. Their use would introduce variability. So he considered the mean value and the variance as intrinsic parts of the quality of the gadgetry. I believe that was an original idea. He was the one to push the idea.

THE NEW BOOK

Folks: When will your new book with Klaus Hinkelmann appear?

Kempthorne: It has just been published. Here is a copy [Kempthorne and Hinkelmann (1994)]. There is more in this book than in my first design book on the nature of the significance level under normal theory. Plot the randomization significance levels versus the normal theory significance levels and they line up very well. So, maybe Fisher had in mind that this sort of thing would happen.

Folks: Tell me more about this book. How long was it in the making?

Kempthorne: Oh, this book was in the making for quite some time. I remember asking Klaus if he would be interested in doing this, in Colorado, I guess, in 1967. Well, this is called Volume I. There will be a second volume and Klaus is working on it. You can see the chapters here on principles, experimental designs, linear model theory, randomization, completely randomized design and that stuff. Randomized blocks, Latin squares, factorial basics, response surfaces and the split block design. I don't know how this thing will go, but these graphs showing the agreement between significance levels based on randomization models and significance levels based upon normal distribution theory are rather remarkable things.

Folks: How could Fisher have guessed that it would happen?

Kempthorne: I don't know. Of course, in my first design book I did some expected mean squares and distribution of the treatment mean squares divided by the error mean square. Pitman (1937) and Welch (1937) did this mathematically in 1937. Fisher, somehow or other, worked out that there was an agreement between the randomization results and the normal theory results. I don't know how he did that. These graphs show that this works all along the range, and I don't know how he knew that. Of course, I did a very small example. In my course Stat 511 at Iowa State I had a class make up sets of data and compare the randomization significance level and the normal theory result. They correspond very nicely. Surprisingly so!

Folks: In the new design book, are there any other features or surprises in there?

Kempthorne: I would say not. Look in the table of contents. The treatment of linear models theory is better than in my first design book, as are randomization, completely randomized designs, randomized blocks, Latin squares, factorials and split plots. The topics are the same as in my first design book, but much better exposited.

Folks: I see that it contains mixed models. What do you make of Yates' objection in recent years to Eisenhart's models I, II and III?

Kempthorne: I think Eisenhart's table was okay, so I don't know what Yates' problem was with it. I remember studying the randomized block experiment years ago, and he sort of blew me out of the room once because I was worried about the testing of blocks. I guess he thought that the block-treatment interaction was the proper error for assessing treatments, which works only if there is additivity. So, apparently he didn't really understand randomization. However, the death of Yates very recently, at age 92, causes me to rethink about him.

RANDOMIZATION THEORY

Folks: We have talked a bit about randomization and randomization models. To me, the biggest thing in your first design book was the presentation of randomization models.

Kempthorne: Actually, that was not as big in 1952 as it should have been. I described it a bit and talked about randomization derived in linear models, but then we had Martin Wilk here and he really worked on randomization models. Then we had several people who worked on randomization models. After Martin Wilk came George Zyskind and Basilio Rojas. Then Neil Throckmorton in 1961 and Bob White in 1963.

Folks: Oh, yes, I knew all of them. Of course, Martin had already graduated when I came as a student. How did you happen to begin this work? I know you studied Fisher's papers and thought about randomization, but what started you on developing these randomization models?

Kempthorne: That is sort of curious because I wrote the first design book and it was all sort of linear models stuff, and then after it was written, I looked at it and felt that it was incomplete. And I went back and (at least as I remember it) fixed up the randomization a bit. But there wasn't as much randomization in the first book as there should have been and this new book with Hinkelmann has a lot more.

Folks: It probably also has more on testing. Randomization tests?

Kempthorne: Oh yes, there is more on randomization tests and stuff like that.

Folks: In the early book, the expected mean square for blocks under the randomized block design with the randomization model did not conform with the usual presentation.

Kempthorne: You know you can't test blocks.

Folks: The situation with respect to testing blocks and treatments is unsymmetric. I presume that not everyone accepted that.

Kempthorne: Oh, no! People didn't accept that. Somehow they regarded the testing situation as symmetric between blocks and treatments. The idea that you could test treatments but there was no error by which to test blocks just didn't go down at all.

Folks: What do you consider to be the most important results in that bundle of work?

Kempthorne: Well, I guess the work on randomization models was started by Martin Wilk, in his thesis on linear models and randomized experiments. He developed concepts and notation (identified by those who work in this area by the name cap sigmas) which expedited dealing with expected mean squares over populations of conceptual randomizations.

Folks: I will ask a question we have talked about before. If I actually carried out a random assignment of treatments, then I could consider a conceptual population of all possible assignments that could have occurred under other possible randomizations. Why isn't it valid to consider that same conceptual population even if I haven't randomized?

Kempthorne: Because if one has randomized and one considers the data in the randomization frame, then the probability that the significance level (called p-value by some) is less than or equal to α is, in fact, equal to α. It is sort of a concomitant of the randomization. The significance levels, given by randomization tests are, so to speak, believable. They are not based on some sort of prior. The thing that bothers me about people who write about this is that they produce basically the same linear models as derived from randomization considerations. But then they adjoin a prior—lots of priors—and it seems to me that they produce these out of thin air. Because I don't see people obtaining priors by looking at data in any way. That is the nice thing about fiducial inference. The "quasi-prior" is produced by data analysis and it is curious that Fisher and Jeffreys got along fairly well because the Behrens–Fisher test was the same as that produced by Jeffreys using his particular priors. So Fisher and Jeffreys were sort of in a position saying, "I agree with the results that the other one got, but I don't agree with the method of deriving it."

Folks: I know that Fisher in his books and papers argued against using prior distributions.

Kempthorne: Ah, yes! He was a nut about this. And he gave the argument that with the binomial,

if you use the uniform prior, you get one result if p is uniform, but if you say that $\sin^{-1}(\sqrt{p})$ is uniform, you get a different answer. There was the lack of invariance under transformations, so that is what Jeffreys, I guess, was trying to do—to find a prior by some mathematical invariance kind of argument.

HYPOTHESIS TESTING AND STATISTICAL INFERENCE

Folks: We have already talked about hypothesis testing. Do you think that Fisher ever really accepted the idea of alternative hypotheses?

Kempthorne: Well, a common thing said about him was that he did not accept the idea of the power. But, of course, he must have. However, because Neyman had made such a point about power, Fisher couldn't bring himself to acknowledge it. Neyman and Pearson tried to fix up significance tests; their interpretation was that tests were accept–reject rules and that errors of the first kind and errors of the second kind had to be considered. That is, they converted significance tests into accept–reject rules. I think Fisher's significance tests led up to fiducial inference and it sort of led up to things like "consonance intervals" that you and I talked about in the book we wrote together [Kempthorne and Folks (1971)].

Folks: It goes without saying that the expression "consonance intervals" didn't exactly sweep the field.

Kempthorne: Oh, that did not sweep the field at all.

Folks: In fact, how many people have ever mentioned the term to you?

Kempthorne: Well, I guess that nobody has taken much notice of that point.

Folks: In the first design book you had very little on testing.

Kempthorne: Yes. Just curiously, I think, Yates said there was too much on testing. But there wasn't really much about testing.

Folks: As a student I thought the reason there wasn't much on testing was because you were emphasizing the analysis of variance as a way of estimating things. Is that erroneous thinking on my part?

Kempthorne: Well, I guess you know the analysis of variance and then getting mean squares and then taking ratios of those was very much a testing

FIG. 5. *Kempthorne with President Indira Ghandi, New Delhi, India, 1974.*

game. There is a strong emphasis on estimating the proper error term. That, of course, was related to testing.

Folks: And randomization tests then were not possible. One didn't do them because of the computation.

Kempthorne: That's right. Yes. Now it is very different.

Folks: In the book that we worked on together for 10 years, we kept changing direction as we went along. But there are some nonstandard developments in there, like the strong emphasis on discrete models. The chapters on testing are very different. They are closer to the actual practice of using testing, not for decision making, but as a way of analyzing data. What is your current thinking about the way testing should be done?

Kempthorne: Well, I guess in the way we presented it. In the elementary books on testing, the authors talk about errors of the first kind and the second kind, and apparently that is an easy way to teach the stuff. However, I don't think it is the correct way.

Folks: And now we have computer software which almost does the test for you. You put some data in, and without your doing anything, it...

Kempthorne: It gives you p-values.

Folks: Yes. Without your saying what you want to use for an error, it is decided for you. Fisher always argued strongly against treating testing as an industrial decision-making device. What do you think he would say about these software packages?

Kempthorne: I don't know, except of course, he contributed a lot to it. Many of these tests that are used today derived from him. So I don't know.

Folks: In the elementary courses we tell people that statistical inference has three parts: estimation, testing and confidence intervals. Is that all there is to statistical inference? Is that it?

Kempthorne: There is the specification of the probability model, which is totally basic. There is the question about fiducial inference, which is related to confidence intervals. But I think statistical inference is up the creek.

Folks: Can you elaborate on what you have just said about statistical inference? What do you mean?

Kempthorne: Well, the way we developed inference in our book is a better way to do it. Did you ever try to teach out of that book? How did it go?

Folks: Pretty well. The part on testing went particularly well. There were some things in there that were left unfinished. For example, the difference between discrete and continuous models. You know that example of Birnbaum's? Of course you do! If you use continuous models and condition, you get an apparent contradiction. But you don't get that contradiction with discrete models. And then another matter. If you take the discrete version of the normal, the sample mean and the sample variance are not quite independent and they are not quite sufficient.

Kempthorne: So, you are dead right on that. I think that the profession has taken over continuous probability theory and used it incorrectly, so to speak.

I have to say something about statistical inference in general. I have been deeply bothered about this for many years; in fact, for the whole of my thinking life. When I went into statistics, I was concerned with experimentation and with the development of reasonable mathematical formulae. However, I became less happy with this line of work until I wrote with you, Leroy, this book. It seemed to me that the whole area had been taken over by mathematicians of the sort I had been exposed to at Cambridge. Just recently I have been trying to understand quantum mechanics. I was led then to look at a mighty tome by R. C. Tolman called *The Principles of Statistical Mechanics* [Tolman (1946)]. Now as I read this, as I looked at this book rather, I noticed that I had written in the beginning, on the flap, that the whole justification of statistical mechanics is embarrassed. Now I find that this is really worth talking about. I would like people to take this book and to look at pages 3, 4, 5, 7, 10, 11, 21, 22, 59, 60, 61, 62, 65, 68 and 70. What is said in these pages is very relevant to my concern about inference. The role of the prior is critical in the discussion on these pages.

Folks: In our opening chapters, we developed distributions and moments before discussing probability or random variables. But, of course, statistics would be hard pressed without probability models.

Kempthorne: I guess it would. I have made the point or expressed the view that there is pure sampling inference and then there is experimental inference and in both these cases probability is brought into the recipe with random sampling or experimental randomization. But then with regard to observational studies, there is nothing for it but to use probability models. I suppose it goes back to the old story. Maybe the thing is that one should get the data; look at the data; formulate a probability model; do a test of significance; then do an inversion on it to obtain a fiducial distribution. As you know, Fisher made this remark about the logical inversion of the notion of a random variable and he says somewhere that you can verify, not only obtain this thing, but verify it by repeated sampling. Oh yes! I had some correspondence with him about that!

Folks: I never could understand how to verify it by repeated sampling.

Kempthorne: Well you see. I've wondered about this. He seems to claim that the verification of the fiducial argument is equivalent to the verification of the significance test which can be done by repeated sampling. So if you've done that, you've verified the fiducial distribution.

Folks: When we were scrutinizing his words over and over, we were looking for just a little more, weren't we?

Kempthorne: Yes. That's right. I was meaning to write to him about what his idea of verification was. I was late, because he died.

R. A. FISHER

Folks: When did you first meet Fisher?

Kempthorne: Well, when I went to Rothamsted, he was there, but I was a boy; I didn't know enough to ask him a question or to speak to him about anything. He was there all the time that I was at Rothamsted. Of course I didn't have any contact with him then, and didn't really have contact with him until he came over to Ames in 1951. We have this photograph of him. I look over his collective works, five volumes in all, and I am amazed, strictly amazed at the amount of originality that Fisher was able to exhibit. It seems to me that he was a very great applied mathematician. I see his work on experimental design in which he apparently knew all about Galois fields. I see his work on statistical inference going back to his paper on the mathematical foundations of theoretical statistics. I see his work on stochastic processes. In 1922, he was the first person to introduce the Fokker–Planck differential equation into stochastic processes of genetics. Fisher had an excellent mind on combinatorics and Fisher's work on genetics is tremendous. And, in fact, writings on genetics occupy about half of his collective works. I wish workers in statistics would spend some time looking at these collective works, thus obtaining an idea of how statistical methods were of use in genetics. I gave a seminar here at Ames on Fisher once and announced the title as "R. A. Fisher = 1.4 of Gauss." This sounds sacrilegious to most people, but I think not. I look at the breadth of Fisher's intellect and the class of problems he worked on and, if I were consulting a statistician, I would like to consult Fisher.

Folks: One of my great memories was in going to Tintner's home after Fisher gave a lecture here in the ballroom in the Student Union, and Tintner held a reception.

Kempthorne: What year was that?

Folks: The Stat Lab Report shows that Fisher gave a seminar in October of 1957. Bancroft, Hartley and you were there. I came as a graduate student. In the front room Fisher was sitting on the sofa at the end of the room, and everyone else was seated a respectful distance away. Well, anyway, that was a great experience. There was a large crowd in the Student Union ballroom and Fisher talked about fiducial inference.

Kempthorne: I remember. Didn't Bob White ask a question?

Folks: Yes I believe so, ...brave soul! We had been studying Fisher's last book. Trying to puzzle out what fiducial methods were.

Kempthorne: That book [Fisher (1956)] was published in 1956. Yes. I remember when that book came. I thought, "Now we'll get the answer!" But we didn't. I think I must say something about R. A. Fisher. He was the major influence in my professional life. I regard him as by far the greatest statistician that humanity has produced. He was also the greatest worker—most productive worker—of genetics from a theoretical point of view. It was unfortunate that Fisher started working in statistics when Karl Pearson was the big person. Karl Pearson had a deep feeling that he was the leader and he was unable, as we all are, to give appropriate credit to younger people. So Karl Pearson's life was in a sense hurt by R. A. Fisher.

In the case of Sewall Wright, he did contribute quite a bit to mathematical genetics and to the theory of evolution. However, it was unfortunate for Sewall Wright that when he was doing his work, Fisher was also working on the theory of evolution. Wright did not have the ability to understand or compete with Fisher. So Sewall Wright was also unfortunate.

I have found in my later life that I am unable to accept much of what Fisher wrote, but this does not affect my opinion of the man. It seems to me that he could be creative on any mathematical problem. The point is that Fisher tackled the biggest problems of statistical inference.

BAYESIAN INFERENCE

Folks: Kemp, I've heard heated exchanges at statistics meetings about a variety of topics and one of them was Bayesian inference.

Kempthorne: Oh yes! If one could get some rational basis for obtaining the prior, then there would be no problem. But people have seminars these days about something where someone says, "I am going to use such and such a prior." Where does he

get the prior? It is not data based. It is a mathematical convenience or something like that. It is not even obtained by using Bayes' theorem. Why one should believe the outcome of using this seems to be a very moot point. I think Bayesian inference is just the use of conventional probability, and some way or another people have to wrap this up in the figure of Bayes, whereas I sort of doubt that people have even read the original Bayes paper.

Folks: Has Bayesian analysis really provided a solution to something like analyzing data from a randomized block design?

Kempthorne: Well, that is an interesting question whether there is some rationale by which randomization can be thought of as producing a prior. I don't know.

Folks: In the one-dimensional case the whole thing is very neat, but another problem with the Bayesian approach is the dimensionality.

Kempthorne: Oh yes. No problems if it agrees, more or less, with Student's t. When you have several treatment constants, both the prior and posterior distributions are multidimensional. Then it's the usual difficulty of comprehending what you have. But what are you going to do about the true treatment means of the k-variate prior? I was very interested recently to read the paper by Jim Berger entitled "The Present and Future of Bayesian Multivariate Analysis" [Berger (1993)]. Berger discusses the importance of the choice of prior and reviews the work done on choice of prior distributions. He says that it is often the case that the noninformative prior $\pi(\theta) = 1$ is satisfactory. It is not satisfactory to me because I have the general problem that I simply do not buy the notion of noninformative priors.

Folks: There are many people working on Bayesian statistics.

Kempthorne: I suppose so, though I don't know what they are doing. We just have a new situation, so you bring in Bayes. It is curious how Noel Cressie says you need Bayes when you have very little data. But people seem to use old-time statistical models and then "'obvious" priors for the parameters.

Folks: Do you believe that Bayesian statistics has a major role to play in the statistics profession?

Kempthorne: Of course, there are different opinions on this. Berger says that there is no longer any debate. I think there is no point in my hedging on this. I do not regard that the Bayesian process has a major role to play in the future of statistics. I have no doubt that, in the design of studies, the optimal design, or an optimal one, is itself, of course, a purely mathematical choice. The optimal design is dependent upon the unknown θ, and there is no choice but to invoke prior information about θ in choosing the design. I would like to say there has never been the slightest argument about this. In the design of experiments, one has to use some informal prior knowledge. How does one construct blocks in a block design problem, for instance? It is stupid to think that use is not made of a prior. But knowing that this prior is utterly casual, it seems ludricrous to go through a lot of integration, etc., to obtain "exact" posterior probabilities resulting from this prior. So, I believe the situation with respect to Bayesian inference and with respect to inference, in general, has not made progress. Well, Bayesian statistics has led to a great deal of theoretical research. But I don't see any real utilizations in applications, you know. Now no one, as far as I know, has examined the question of whether the "inferences" that are obtained are, in fact, realized in the predictions that they are used to make.

Folks: Kemp, likelihood plays a role in Bayesian inference.

Kempthorne: It is curious that the "modern" Bayesians seem not to see the problem of the choice of a parametric model. It seems as though they arrive at their likelihood by some essentially arbitrary process which they do not go into. I have been worried about this for "donkeys' years" because I have wondered about the question of the likelihood. Well, you see, that is an interesting point because what is likelihood? Likelihood should be the probability of the data in terms of parameters. In our book, we got into this question in the case of discrete data, and I think that likelihood is satisfactory in the case of discrete data. But I have been bothered about the use of likelihood in the case of continuous probability models. I discussed this matter in my Fisher lecture in 1965 [Kempthorne (1965)].

A completely basic idea in my thinking is that one has to do a data analysis to arrive at an appropriate probability distribution, which then is validated by a goodness-of-fit test. Now it is curious that according to the Bayesians there is no use for a goodness-of-fit test. I find it interesting that Fisher's solution for the unknown mean of a normal distribution by fiducial inference has been accepted to a considerable extent by Bayesians and that the Fisher solution, or the Behrens–Fisher solution to the Behrens–Fisher problem, based on the fiducial argument, was in agreement with the result obtained by Jeffreys using a Bayesian argument.

Folks: In one of your papers you analyzed one of Birnbaum's likelihood principles. The L and the C and the S. I did that with a group of graduate students many years ago. We never could get it to work. We either didn't understand one of the princi-

ples or the implication wasn't as it was stated. You had similar difficulties?

Kempthorne: Oh yes. Apparently, I was being an idiot, as happens all too nonrarely. I see the likelihood function with data as being written down with "continuous" data which one does not have. I think that the likelihood principle doesn't really work. Everybody seems to buy it. You know the question "How does the likelihood principle fit in to all this?" Next to that I write, "Where does the model come from?" One really can't accept the likelihood principle as being basic, because one has to get a probability model. You and I wrote about this, but apparently no one reads this.

Folks: Kemp, I am going to change the subject to ask about one of your achievements. I seem to remember that you have professed not being a mathematical statistician. But you were President of the Institute of Mathematical Statistics.

Kempthorne: Yes, I don't know how that happened. I subscribe to the journal, but you know I don't think that I am really a member of the IMS. I thought that I should go along with it because they had asked me. I wondered whether I could do anything positive and constructive, but I suppose nothing much changed.

PRESENT STATE AND FUTURE OF STATISTICS

Folks: Where do you think statistics is going from here?

Kempthorne: Oh, that is a very curious question. I guess it is not at all clear where it is going. I guess Statistics departments will survive and there will be plenty of work for statisticians, but I don't know where the profession is going. That is very curious, isn't it? The mathematicians have taken over. We have these seminars in statistics and they're all dominated by mathematics and so it seems to me they don't really contribute to our knowledge.

Folks: What is your view of the effect of computing upon statistics?

Kempthorne: Well, I think it is going to be massive. It is going to be very important for fitting models and then residual plots. A way to see if models are okay. So that's going to be very significant. More data analysis. Very difficult. Well, you know, we have all sorts of data recording devices now and statistical computing packages, so, it seems to me, there will be more and more situations where the data are collected and analyzed and you see a summary of the analysis before you ever see the data. Then you can go back and do something with it.

FIG. 6. *Kempthorne at retirement, June 1989.*

During the eighties I spoke often and wrote several papers about my ideas of where statistics was heading. Now I am not so sure. However, I will attempt to say where I would like for it to be going.

My view is that I would like for the profession to be involved in contributing to the important issues which confront us in the future. I believe that the Iowa State Stat Lab is a good model for what I would like to see. In 1987 [Kempthorne (1987)] I wrote that "The Stat Lab has been a huge force in the direction of applications for the whole of the USA for more than 40 years. ... It is evident from history of the past decades that the program in statistics at ISU with its overall philosophic background is the sort of program the nation (and state) needs."

What accounts for the success of the Stat Lab? I believe that it is because it was not driven by the mathematics, but by actual problems in biology, genetics, demography, economics, psychology and so on. To be sure, real problems give rise to abstract problems in statistical inference which have a fascination of their own. However, for statistics to remain viable, statistical problems should have their genesis in real, data-related problems.

Departments of Statistics will continue to exist for a long time. I hope that they do not exist simply as Mathematics Departments. We need to free ourselves of the notion that mathematical difficulty and statistical quality are the same thing. A large proportion of material which appears in textbooks bearing the name of statistics is tangential to the idea of applied statistics. I attempted to express my ideas along these lines in a talk I gave in 1986 entitled "The Future of the Department of Statistics at Iowa State University" (unpublished).

Folks: What do you see as the important problems of statistics?

Kempthorne: The problems are the choice of a family of distributions, looking at the likelihood function property, then statistical inference. It is curious that fiducial inference seems to work with single parameter models, but gets into difficulties with vector parameters, as does Bayesian inference. Finally, let me say that we will be all right as long as the students of the future continue to learn from the past, to read the important papers and to try, as others have done, to think deeply about the world of uncertainty. I feel that much of philosophy needs to be read—particularly the writings of the great American philosopher, C. S. Peirce.

Folks: Kemp, thank you for giving us your views of the concepts and the profession of statistics.

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