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Remembering Oscar Kempthorne (1919–2000)

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Abstract. On November 15, 2000 the statistics community was saddened by the death of one of its most prominent members and leaders, Oscar Kempthorne, who had given over 50 years of his life to statistical science as an educator and researcher. Obituaries and other accounts detailing aspects of and achievements during his personal and professional life have appeared elsewhere (*IMS Bulletin* 30 (2), 2001; Bancroft, 1984; David, 1984). The purpose of this paper is different: it is to highlight his major contributions to statistical science, and to indicate how these ideas are still guiding statistical thinking today.

Oscar Kempthorne contributed largely to three major areas: to experimental design, to genetic statistics, and to the philosophy and foundations of statistics. These seem to be rather distinct areas, but his research shows a common thread in the form of his concern for acquiring scientifically sound data and interpreting such data. In this context he considered the analysis of variance as one of the most powerful statistical techniques, and it is therefore not surprising that much of his research, certainly in experimental design and genetic statistics, centers around this technique. This work established him very early on as one of the leading statisticians of our time.

Key words and phrases: Experimental design; randomization; genetic statistics; population genetics; inference.

1. CONTRIBUTIONS TO EXPERIMENTAL DESIGN

1.1 Randomization Theory

One of Kempthorne's lasting and most important contributions is, no doubt, his book *Design and Analysis of Experiments* which appeared in 1952 and has since become a classic. It is heavily influenced by developments in this area brought on earlier by R. A. Fisher (1935) and Frank Yates (1935, 1939), two men whose work he admired, albeit not uncritically. One of the main ideas expounded in great detail in the book is Fisher's idea of randomization, but unlike Fisher he puts it in a more mathematical, theoretical framework. Here we find the beginnings of the notion of *derived* linear models and resulting randomization tests as compared to *assumed* linear models and normal theory tests, which, among other things, sets his development apart from other design books. This view he expounded later (1955a) in what I think is one of his most important papers, and even more explicitly in 1975 when he wrote, "[But] I am of the opinion that experimental conclusions can be drawn *without* the use of parametric probability or likelihood functions. I realize that this view places me in an almost empty set of theoretical statisticians and can only say that my outlook is almost totally derived, perhaps unjustifiably of course, from Fisher." In fact, he "translated" Fisher's ideas about randomization into mathematical-probabilistic terms by introducing the notion of the design random variable, which is simply a (1,0) random variable indicating whether or not a particular treatment is assigned to a particular experimental unit. This concept enables one to explicitly incorporate the physical act of randomization into the formulation, that is, derivation, of a linear model using only the minimal assumption of unit-treatment additivity, which then leads directly to the analysis or, more precisely, randomization analysis. He used this to point out the

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sharp distinction between experimental and observational studies with respect to the properties of the underlying, and often identical looking, linear models and the type of conclusions that can be drawn from such studies, that is, causality versus association. Furthermore, he showed that this approach has important consequences with respect to testing hypotheses in the context of the analysis of variance table, namely that one cannot test hypotheses about the effects of blocking factors. This result is still not fully understood and appreciated and hence still debated. His ideas on these issues were further developed by several of his students (e.g., Wilk, 1955; Wilk and Kempthorne, 1955; Zyskind, 1962, 1963; White, 1975) to cover essentially all basic existing designs. I was greatly honored when he asked me to help him revise his book. In *Design and Analysis of Experiments, 1: Introduction to Experimental Design* (1994) we were able to present many of the earlier ideas and developments in a coherent and logically consistent way. I personally have put a great deal of emphasis in my teaching on the randomization ideas in an effort to increase the "almost empty set" to a larger set of statisticians informed on matters of experimental design and to carry on Kempthorne's legacy.

There is no question that most statisticians consider randomization to be an important concept, in particular—but not only—in experimental design (for exceptions see, e.g., Harville, 1975; Lindley and Novick, 1981). Yet, as mentioned above, in many cases the consequences of randomization are still not fully understood and hence ignored. Research and education in this area are vital. Kempthorne's approach (see also Kempthorne, 1977) provides an important mechanism, which has been extended to cover more complex experimental designs (Hinkelmann and Alcorn, 1998). Another, but yet related, approach was suggested by Nelder (1965a, b) by introducing the notions of orthogonal block structures and strata, a theme developed further by, for example, Bailey (1991). Both approaches are incorporated in the recent work by Calinski and Kageyama (2000) which provides evidence of active interest and research in this area.

1.2 Factorial Designs

Another area where Kempthorne left his mark is that of factorial designs. Again, following earlier ideas of Fisher and Yates, he was able to put this whole area on a more mathematical basis and present it in terms of a general theory. As a consequence, his derivations led to a new parametrization of the individual observations in terms of main effect and interaction components. For example,

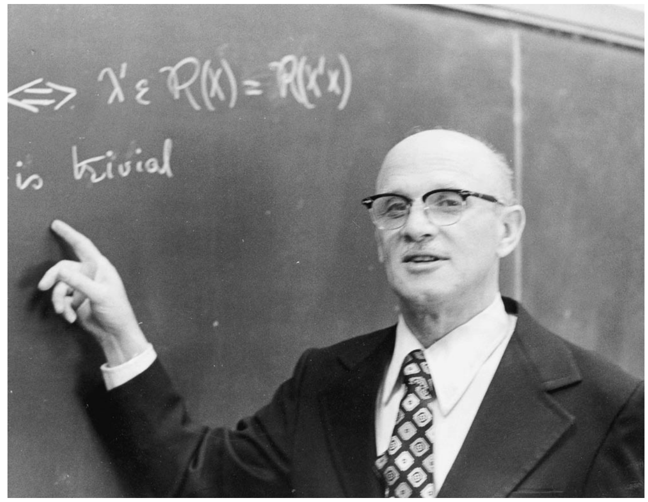

FIG. 1. *Lecturing on his 65th birthday (January 31, 1984) during sabbatical at Virginia Polytechnic Institute and State University, Blacksburg, Virginia.*

rather than writing the model for a 3^2 factorial in a completely randomized design in the customary form as

$$Y_{ijk} = \mu + \alpha_i + \beta_j + (\alpha\beta)_{ij} + e_{ijk},$$

where $\alpha_i (i = 1, 2, 3)$ represents the effect of the ith level of factor A, $\beta_j (j = 1, 2, 3)$ represents the effect of the jth level of factor B, and $(\alpha\beta)_{ij}$ represents the interaction between those levels, Kempthorne's (not of full rank) parametrization would lead to the model

$$Y_{ijk} = \mu + A_i + B_j + AB_{i+j} + AB^2_{i+2j} + e_{ijk}$$

(Kempthorne, 1952), where A_i $(i = 0, 1, 2)$ and B_j $(j = 0, 1, 2)$ denote main effect components of the factors A and B, respectively, and AB_{i+j} and AB^2_{i+2j} (with $i + j$ and $i + 2j$ reduced mod 3) denote two-factor interaction components. This parametrization is particularly important and useful when considering setting up the identity relationship for constructing systems of confounding and fractional factorials, as well as for evaluating treatment contrasts other than main effects and interactions. Developed originally for symmetrical factorial experiments, it has been extended also to asymmetrical factorial experiments (Hinkelmann, 1997).

Kempthorne's work on fractional factorial designs began as early as 1947 and culminated in his work with Addelman on orthogonal main-effect plans for asymmetrical factorials (Addelman and Kempthorne, 1961; Addelman, 1962a, b). This research, using and generalizing methods for symmetrical factorials, resulted in the idea of collapsing higher- level into lower-level factors and expanding combinations of lower-level symmetrical factorials into higher-level factors, using the result that proportional frequencies preserve orthogonality. This gave rise to an elegant representation of a catalog containing a large number of such main-effect plans. This work is basic for current research in the general area of fractional factorial designs and, specifically, in the area of off-line quality control (e.g., Wu and Hamada, 2000).

1.3 Statistical Consulting

At a more practical level and in the setting of a land grant university, Kempthorne helped many researchers design and analyze their experiments over the years. He enjoyed combining theoretical and applied aspects of statistics in an effort to understand and promote sound principles of obtaining and interpreting data. In some sense this activity reflected his strong belief that "almost all of the statistical ideas we find valuable grew out of the needs of science and technology and general human needs to understand the real world" (Kempthorne, 1983), and that as statisticians we could and should contribute to this effort. Thus, occasionally he would involve his students to challenge them and give them the benefit of applying what they learned—or should have learned—in their classes. I still remember when he called me into his office and showed me the data from what appeared to be a well-designed genetic experiment. It had not taken him long to recognize that this was not your ordinary experiment. "Something funny is going on here," he said in his typical way, "you go and figure this out." It turned out to be a wonderful learning experience with a conclusion satisfying to me, but not to the researcher (Hinkelmann, 1963b). There is no doubt that Kempthorne's love and deep understanding of experimental design carried over to his students. I know, because throughout my own career it has given me great satisfaction to help researchers in various academic fields design and analyze their experiments (Hinkelmann, 2000). And I see the same in our own students who are involved in intramural statistical consulting.

2. CONTRIBUTIONS TO GENETIC STATISTICS

According to Kempthorne's own testimony (Folks, 1995), his interest in genetic statistics is founded in the combination of a strong agricultural environment at Iowa State and his extensive study of Fisher's work. He often referred to Fisher's 1918 paper as one of the most important papers in statistics, laying the groundwork for analysis of variance and genetic statistics at the same time. It should not come as a surprise that Kempthorne saw an intrinsic connection between factorial effects in experimental design, such as main effects and interactions, and genetic effects, such as additive, dominance and epistatic effects.

2.1 Covariances between Relatives

This led to his important work on covariances between relatives (e.g., Kempthorne, 1955b) for various forms of mating schemes as they would be encountered in plant and animal breeding research. These concepts became influential in quantitative genetics, in particular in the evaluation, that is, estimation, of genetic parameters, such as heritability or general and specific combining abilities, using various mating designs in plant and animal breeding. The driving force in this development is reflected in his own words in the preface of his groundbreaking book *An Introduction to Genetic Statistics* (1957): "It is part of the outlook of the author that the analysis of variance is not a tool to be used blindly with the picking of linear models 'out of thin air.' The inputs for the interpretation of the analysis of variance must if possible be based on genetic theory." In fact, he showed how the combination of experimental and mating design would give rise to appropriate linear models and hence to analysis of variance tables. In such a table the expected mean squares can then be expressed, through the intermediary of covariances between relatives, in terms of genetic parameters, such as additive, dominance, and epistatic variance components, in an effort to test hypotheses about different forms of gene action in a population under consideration (e.g., Matzinger and Kempthorne, 1956).

Again, we see the close connection between experimental design and genetic statistics. His way of thinking determined to a large extent the development of the statistical aspects of quantitative genetics and led the way towards successful applications. In his own words (Kempthorne, 1978) he regarded his book as providing "as its major thrust this reconciliation [between "continuous variation" and "discrete" Mendelism] and the use of the reconciliation for the improvement of organic stocks." In this connection it is important to note that Kempthorne, based on Malécot's (1948) work, was the first to present the calculation of the inbreeding coefficient in terms of genes being identical by descent. This treatment of the inbreeding coefficient had a profound impact not only on researchers in quantitative and population genetics, but also on users of this research, namely plant and animal breeders.

Even though the emphasis in statistical genetics has changed due to recent advances in molecular genetics, the concepts of covariances between relatives (for a general formulation, see van Aarde, 1975) and the underlying concept of genes being identical by descent (and its extension) remain of critical importance in current research, in particular in connection with quantitative trait loci research. In fact, with the introduction and implementation of new methods to estimate variance components, such as REML, the use of covariances between relatives has become very important in the estimation of genetic variance and covariance components in QTL mapping (e.g., Guo, 1995; Grignola and Hoeschele, 1997; Wang, Fernando, van der Beek and van Arendonk, 1995).

2.2 Population Genetics

For much of his professional life Kempthorne was interested in questions of natural selection. The basis for this interest was the desire to understand what R. A. Fisher had written about this subject, in particular in his book *The Genetical Theory of Natural Selection* (Fisher, 1930) which contains what Fisher himself called the "fundamental theorem of natural selection." This theorem, derived in terms of Malthusian parameters of fitness, states essentially that the rate of increase of fitness of a species is equal to the additive variance in fitness. In his own book Kempthorne showed that the theorem is applicable only in very special situations requiring assumptions that certainly do not hold in most biological populations. One of the problems, he pointed out (see Folks, 1995) is that it is not known what fitness really is.

In an attempt to come up with a useful definition of fitness he, jointly with Pollak, published a series of papers on this subject. They introduced different measures of fitness by incorporating viability of individuals and fecundity of matings and reaffirmed that Fisher's theorem and variations of it (e.g., Kimura, 1958) are valid only under very restrictive assumptions.

For the special case of a self-fertilizing diploid population Pollak and Kempthorne (1970) actually show that "in the long run the mean of the Malthusian parameter increases at a rate approximately equal to the *total* variance of the Malthusian parameter," thereby contradicting the results by Fisher and Kimura. The difficulty in dealing with this problem in general terms or using Malthusian parameters and doing so for general populations under general conditions leads to their conclusion that

> It seems quite clear that mathematical arguments which use an algebraic variable, say W, to denote fitness without relating this at all even to loosely identifiable aspects of fitness such as viability and fecundity have marginal merit. It is essential to consider what is observable with a real biological population. Naïve consideration suggests that one can observe the fecundity of matings and possibly the probability that an infant survives to adulthood. Our view is that the need is for a theory incorporating *at least* these observables, because there must be a way of relating theory to observation or else the theory has no relevance to the real world.

(Kempthorne and Pollak, 1970).

More recently, Fontdevila (1995), for example, alludes to the problems of such an approach in natural populations, and more recent texts on population genetics deviate not at all or only slightly from Fisher's classical argument (e.g., Smith, 1998; Nagylaki, 1992), perhaps because of the difficulties pointed out by Kempthorne and Pollak. Charlesworth (1994) writes.

> "The earlier papers on this subject were rather casual about the nature of their assumptions concerning the mating process and mode of selection; Pollak and Kempthorne (1971) were the first to emphasize the need for clarity on these points."

It appears that the field of developing the theory of natural selection is still a fertile, albeit difficult field. The great difficulty is that a completely general treatment leads to unworkable mathematics, forcing researchers to make simplifying assumptions, which in turn lead to unrealistic results. There is clearly a great need to take a comprehensive look at this subject.

2.3 Nature-Nurture Controversies

Kempthorne was interested in and made important contributions to the nature–nurture question. This topic goes back to Galton (1869), and has led subsequently to a great deal of controversy in connection with questions about the relationship between IQ measurements and intelligence, and to what extent intelligence is due to nature, that is, genetics, or due to nurture, that is, environment. Burt and Howard (1956) and Jensen

(1969) as proponents of one school assert that the heritability of IQ is of the order of 0.80. Such a position is supported, if not explicitly, but implicitly, in the best-seller *The Bell Curve* by Herrnstein and Murray (1994). Even though there is no question that intelligence is affected by both heredity and environment, the viewpoint that heritability is paramount is strongly opposed by others, among them Kempthorne (1978, 1990), and more recently (in response to the book by Herrnstein and Murray) by Devlin, Fienberg, Resnick and Roeder (1997), Kincheloe, Steinberg and Gresson (1996) and others.

Although Kempthorne discussed this issue, he put it clearly in the larger context of causality, data analysis and observational versus experimental studies. Apart from his view that observational studies cannot lead to causal inferences, but only to associations, the problem here is much more complicated in that one is dealing with the joint causality of two forces, namely, genetics and environment. He shows in his paper in terms of a simple example that one cannot partition such joint causality in a meaningful way. The basic problem is that there is dependent association of genotypes and environments, as compared to independent association in experimental populations. This fact calls into question the customary notion of heritability. Based on arguments initiated by Kempthorne, Emigh (1977) shows that even if one introduces an alternate measure of heritability, based on components of commonality (rather than on components of variance), such a measure lacks the ability to explain how the association between genotypes and environments is produced. Kempthorne's discussion of this topic illustrates once again his way of providing powerful arguments by stripping away unnecessary complexities to prove a point, but then restoring the complexities to put the question in a much larger framework. We can see this type of argument in much of his writings.

3. CONTRIBUTIONS TO THE PHILOSOPHY OF STATISTICS

Throughout most of his career Oscar Kempthorne was interested in and concerned about the foundations and philosophical aspects of statistics. It is perhaps fair to say that many of his writings and discussions arose out of his constant effort to understand and interpret the writings of R. A. Fisher, whom he considered to be the father of statistics and to whom he referred often as "big daddy." This does not mean that he always accepted Fisher's viewpoints, but he insisted on a critical evaluation of those viewpoints and he gave Fisher credit where credit was due. He was also the first to admit that it was not always easy to interpret and understand Fisher's writings, but that we must never stop trying to figure out what Fisher really meant. This is a reflection of Kempthorne's sense of history: We must recognize and acknowledge historical foundations and developments, but we must not do that without critical examination.

3.1 Significance Tests and Data Analysis

Kempthorne pointed out that the wide use of P-values in statistical inference and data analysis goes back to Fisher and Snedecor (Kempthorne, 1976a; for some historical comments, see also David, 1995), leading to significance testing as compared to hypothesis testing as introduced in a decision theoretic framework by Neyman and Pearson (1928) and later Wald (1939). He took issue with much of the latter approach, suggesting among other things that the theory of hypothesis testing might more appropriately be called theory of falsification of hypotheses (Kempthorne, 1984c). In particular, he held the view that Wald's idea that every statistical activity is some sort of decision making had an almost negligible impact on applied statistics, similar to the view held by Good (1956) who argued that "the application of decision theory to scientific research is controversial." Many of Kempthorne's ideas on this subject are exposited in the book *Probability, Statistics, and Data Analysis* (Kempthorne and Folks, 1971) in an attempt to tie statistical inference closer to the problems of real data analysis, as compared to hypothetical situations. Based on similar ideas expounded by Fisher (1956), Kempthorne and Folks write

> We think about a test of significance because we wish to form an opinion of whether the data conform to the hypothesized distribution or model. We may not, in general, wish to form an opinion of whether the hypothesized distribution or model is the *true* one. Instead, we address the question of whether data conform to a particular model, and that is intrinsically an operational question.

It is in this context that one must also understand Kempthorne's remark, "The future of statistical methods lies in the appreciation of the investigator-data interaction process and the implementation of this process by means of the modern computer" (Kempthorne, 1972).

This, again, is symptomatic of much of Kempthorne's work; he felt that statistical work must be driven by data, whether it relates to their acquisition or to their interpretation. It also underscores his view that "the two pillars of science are observation and data analysis," to which he added that "data analysis = statistics" (Kempthorne, 1984b). To him, understanding statistics meant understanding the processes of science and technology. This involves asking often difficult philosophical questions such as "What is science?" or "What is a theory?" or "What is an experiment?". Some of his thinking on these issues is exposited in Chapter 1, "The processes of science," in Hinkelmann and Kempthorne (1994).

3.2 Consonance Intervals

Just as the inversion of tests of hypotheses leads to the notion of confidence intervals, Kempthorne and Folks (1971), acknowledging "that there is no unique way of summarizing the inferential content of data," strongly advocate the use of what they call consonance intervals as the logical inversion of significance tests. The goal is to show to what extent the data are consonant with a specified model as described by its parameters. This can be put in the context of goodness-of-fit tests suggesting that an interval estimate may be constructed by including all parameter values for which the goodness-of-fit test would not result in rejection (see also Easterling, 1976). The basic idea, using a very simple example, can be described as follows: suppose we consider a random sample $x_1, x_2, \ldots, x_n$ from a normal distribution with mean μ and variance unity. Further suppose that the significance test for $\mu \neq \mu_0$ results in the significance level P. Then, according to Kempthorne and Folks (1971), "to decide what values of μ are consonant with the data, it is reasonable to determine the hypothesized values for μ for which the significance level exceeds specified values." With $z_{1-P/2}$ denoting the $1-P/2$ upper percentage point of the standard normal distribution, this leads to the $1-P$ consonance interval

$$\left[\bar{x} - z_{1-P/2}\sqrt{1/n}, \bar{x} + z_{1-P/2}\sqrt{1/n}\right],$$

which means that μ values in this interval are consonant with the data at the significance level P.

Kempthorne and Folks consider it valid, and useful to actually display the entire family of consonance intervals; that is, the intervals associated with different probability values. It is interesting to note here that statistical software packages generally perform significance tests, that is, give P-values, but then give confidence intervals instead of consonance intervals. This obviously represents a philosophical disconnect and calls for corrections. For some, hypothesis testing and significance testing may refer to the same thing, as do confidence interval and consonance interval, but Kempthorne points out on many occasions (e.g., Kempthorne, 1976a) that there is, indeed, a deep philosophical difference between these concepts.

3.3 Critique of Bayesian Approach

Kempthorne never made a secret of his anti-Bayesian, or better anti-neo-Bayesian stand, and he expressed his opinions and ideas very forcefully (e.g., Kempthorne, 1984a). It is, however, fair to say that he had no objection to the Bayesian argument for purposes of developing a theory. His objections were directed towards what he considered to be more or less arbitrary choices of prior distributions in connection with *real* data analysis. To give a concrete example, we shall refer to a review article on Bayesian experimental design by Chaloner and Verdinelli (1995). They consider, among other things, what they call a Bayesian design for the one-way analysis of variance model, specifically, for comparing $t-1$ treatments with a control. The process is a decision-theoretic approach and begins with the choice of an appropriate utility function, which, in turn incorporates prior distributions of the treatment effects. Chaloner and Verdinelli then derive expressions for the number of replications to be used for the control and for the treatments for an A-optimal design. In discussing their result, they emphasize that "in implementing such a design, the assumption is clearly critical that the prior information really does *represent accurate information* about the experimental units in this particular experiment." (The emphasis is mine.) This is, of course, the very point to which Kempthorne always

FIG. 2. *At Iowa State Statistical Laboratory breakfast in Brookside Park, Ames with wife, Val, and children, Jill and Peter, Spring* 1961.

strenuously objected, namely, that we do not have accurate information and that, besides, it is impossible to translate this information formally into "objective" prior distributions, or in his own words: "Obviously, you do not know the proper prior to use except in certain technical situations, where there has never been disagreement." (Kempthorne, 1984a), or an another occasion "...there is no acceptable deductive way to force a choice of a prior. A prior must come out of the mind of the investigator..." (Kempthorne, 1984c). At the same time he always maintained that "informal Bayesian ideas" in the form of prior information should be used to decide on an error-control design when setting up an experiment, as described by Hinkelmann and Kempthorne (1994, Chapter 2) even though the word "Bayesian" is not mentioned explicitly. Also, returning to the theme of experimental randomization, Kempthorne (1975) makes a similar point when he says, "It seems to me that the role of randomization is to overcome the nonexistence of a prior that one can apply with confidence."

As if to emphasize the points mentioned above and extend them to questions of data analysis and decision making, he states in a more conciliatory tone

> The controversy between traditionalists and Bayesians does matter. It is necessary that there be interaction, rather than the essentially complete absence of interaction that now exists. On the one side I see the complete absence of Bayesian thinking and on the other side a complete absence of contribution to the routine of scientific exploration and the building of a scientific world picture. Because the activities of the working scientist involve the making of decisions, on what ideas to pursue, how to pursue them, and on some idea of personal degree of belief in models, some use of Bayesian ideas, even informally, is involved. We have to note that the books on statistical methods do not address problems of decisions, and they should.

(Kempthorne, 1984a).

Perhaps in recent years there has been some movement in this direction, but there seems to be a long way to go to satisfy Kempthorne's criticism of the status of real data analysis.

His concerns about and preoccupation with philosophical issues and his challenge to us can best be summarized in his own words: "I plead with the statistics profession to take cognizance of philosophy, philosophy of science, and the actual processes that have occurred in science" (Kempthorne, 1975). Clearly, this challenge is as true today as it was then.

4. THE MENTOR AND FRIEND I KNEW

Obviously, the discussion of his work given above presents only a partial and, hence, incomplete picture of what Oscar Kempthorne has meant to the statistics community. To fill in some details and other aspects of the man, I shall now turn to some personal experiences and impressions.

Last summer I presented a series of lectures on incomplete block designs to graduate students at the University of Dortmund, Germany, using draft chapters of the second volume of Hinkelmann and Kempthorne's *Design and Analysis of Experiments.* As an introduction and motivation I described to the students how I became interested in this topic. But it really is the story of how I met Oscar Kempthorne, first through his written work and then in person, and how a professional and personal relationship was born.

In 1958, after receiving my diploma in mathematics from the University of Hamburg, I accepted a position as research associate at the Institute of Forest Genetics at the University of Hamburg to work on developing incomplete mating designs (so-called partial diallel crosses) suitable for research in tree breeding. I was given two books to read as preparation for the project. One was on general principles of genetics, the other was Kempthorne's just published *An Introduction to Genetic Statistics* (Kempthorne, 1957). Needless to say, trying to read it presented me with many challenges, but at the same time it proved to be one of the most stimulating books I have ever read. It helped me understand and tackle the problem I was working on. When I hit another roadblock, which was of a purely statistical nature, I searched through much of the available statistical literature for help and came across an article "A class of experimental designs using blocks of two plots" (Kempthorne, 1953), a special case of incomplete block designs. I speculated then that these designs must have some relationship to partial diallel crosses, which enabled me to successfully complete my project (Hinkelmann and Stern, 1960). Only later did I understand and prove the strong relationship between incomplete mating designs and incomplete block designs (Hinkelmann, 1963a; Hinkelmann and Kempthorne, 1963).

Not surprisingly, this new experience kindled my interest in genetic statistics. I was told that if I

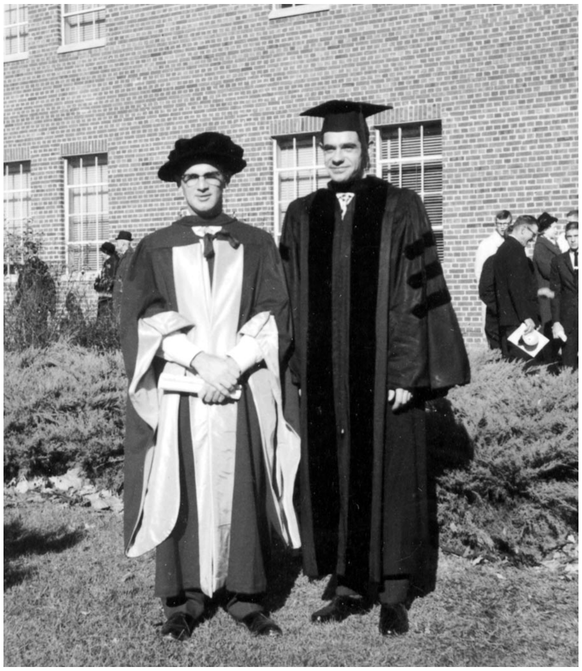

FIG. 3. *With former student Klaus Hinkelmann at graduation on ISU campus, Ames, Iowa, November* 1963.

wanted to learn more about this emerging field I would need to go to the United States and talk with the leading researchers in this area, among them Oscar Kempthorne at Iowa State University. Such a visit was arranged, and in the spring of 1960 I set sail for the U.S.

I was very nervous before my first meeting with Professor Kempthorne in his office on the Iowa State campus, but from the first moment it turned out that my worries were unfounded. He was very friendly and sympathetic to my request to learn more about genetic statistics. When I told him that I read (or, better, tried to read) his book, but did not understand many things, he replied that I was not the only one who had this problem and that even he himself did not understand everything (which, of course, I did not believe). He was quite interested in the work that I had done, and it did not take him long to recognize that my solution to the problem was isomorphic to the one he and a collaborator had arrived at just recently (Kempthorne and Curnow, 1961). As to my earlier request, I found it quite remarkable that he suggested that we meet every day and go over each chapter in his book.

Needless to say, I learned a great deal from this experience. It also established a bond between us that would last for 40 years and result in many interactions and collaborations, first as one of his many Ph.D. students, later as a colleague and even coauthor. During those years I became aware and appreciative of Kemp's (I could call him by that name after I received my Ph.D. in 1963) many and varied contributions to experimental design, genetic statistics, and philosophical issues in statistics, and I have tried to highlight some of those contributions above.

5. MORE PERSONAL VIGNETTES

I will conclude with some other anecdotes that give further insight into what kind of person Oscar Kempthorne was, and why his work and his probing mind left such an impression on many statisticians.

Many remember Oscar Kempthorne by his presence at statistical meetings, where he was forceful in presenting his research, expressing his ideas and opinions and participating in discussions. And in doing so he invariably created a great deal of excitement. I still remember his talk "Can multivariate data be analyzed by univariate methods?" at the annual meetings in Minneapolis in 1962. The first reaction came from the chair of the session, Rolf Bargmann, when he said: "The answer is obviously no." End of session. Kempthorne, of course, thought so too, but he considered it to be important to comment on past mistakes that he had found in the literature, and he did it in his own personal style. Some agreed with him, others did not, and as a consequence the entire meeting was consumed by discussing his talk. For the most part, such engaging talks are absent from our meetings today.

Many will recall another annual meeting, in Detroit in 1970. Kempthorne, himself a former Fisher Memorial lecturer (see Kempthorne, 1966), was chairing the Fisher Memorial Lecture, presented by L. J. Savage, entitled, "On Rereading R. A. Fisher." Those who were there will remember the crowded (people were sitting on the floor) and hot lecture hall and one of the most memorable lectures ever given at a statistical meeting. After a thunderous applause Kempthorne got up and expressed his and everybody else's thanks to Savage and then said something like "After this talk any questions or comments would be anticlimactic. The session is closed." Fortunately, the talk was published posthumously (Savage, 1976), and in the discussion of the paper Kempthorne (1976b) acknowledged that "... [this] was the finest statistical lecture I have heard in my whole life". The paper also reaffirmed Kempthorne's admiration for Fisher's genius on the one hand and the ambiguities that surround some of his work on the other hand: "The mysteries of Fisher's thought arise as soon as one turns away from the purely mathematical work which has stood the test of time ...", mysteries he struggled to explain, write and lecture about throughout his career.

At an entirely different level, he was always aware of his students' struggles to learn about and understand the finer points of statistical ideas. He did what he could to help them along. On many Saturday mornings he would drop by the office and just say: "Do you want to talk?" Of course, who would refuse that offer? Or, on other occasions, I remember standing in his office in front of the blackboard, ready to be quizzed, and most of the time not knowing the answer. To my "I do not know; that's why I am here," his reaction was "That's fair," and he would proceed to provide the answer, and more. What valuable lessons! I learned early on that, above all, he valued and demanded honesty, and that included honesty in acknowledging results obtained by others, in particular in a historical context. He would expose quickly those who did not adhere to this standard, in particular those who pretended to know the answer.

Kempthorne was one of the cofounders of the statistical honor society Mu Sigma Rho, which was established at Iowa State University in 1968. The Virginia Alpha Chapter of Mu Sigma Rho at Virginia Polytechnic Institute and State University was established in 1979, and Dr. Kempthorne was invited to formally initiate the chapter. On that occasion he presented a talk on "The 2×2 table," which he referred to as a small table with big questions. These questions were, and still are, of a philosophical nature, touching on the origin of such tables, tests of significance, conditional P-values, Fisher's (1956) and Berkson's (1978) writings. In his eloquent way he shed some light on these questions and this topic, which he considered to be of great importance to science and technology (Kempthorne, 1979).

In my last letter to Kemp a few days before his death, I mentioned that I had just given my perhaps last colloquium in our department, "Statistics as a Science, Art, and Power—A Personal Account" (Hinkelmann, 2000). I thought that he might have enjoyed some of what I had said, and that I, in turn, would have liked to get his reaction and criticism. On second thought, however, and on rereading some of his publications, I should perhaps not have wanted his comments. It might have turned out that some of my pronouncements he would have labeled as "rubbish," a word he did not hesitate to use where appropriate, because some of my ideas deviate somewhat from his philosophical convictions. But even then, I am sure, it would have been an honest and sincere dialogue. More important, to this day Kemp remains, and always will be, my revered mentor and friend. I am certain I am not alone in my admiration for Oscar Kempthorne, and this alone will ensure that his work will live on, and that it will inspire new research and, above all, new thinking about old problems.

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