Memorial Article

ALLAN BIRNBAUM 1923–1976

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It is a measure of the importance and profundity of Allan Birnbaum's contributions to statistics that a belated obituary notice can still be of current research relevance in spite of the continually accelerating rate of publication in this area. His tragic death in July 1976 continues to engender pangs not only of sadness at the passing of a great friend but also of regret that he is no longer with us to help in resolving the many issues that continue to underlie the foundations of statistical inference.

After completing a premedical program at the University of California at Los Angeles he took graduate courses in sciences, mathematics and philosophy, some of them from Hans Reichenbach. Allan went to Columbia for his Ph.D. where he planned to work with A. Wald. As a result of the tragic accident of Wald's death he asked Lehmann, who happened to be spending a semester at Columbia, to accept him as a student. His Ph.D. was taken under E.L. Lehmann's guidance. His early papers on Poisson processes are concerned with applying sequential ideas to the standard conditional procedures and already exhibit his special ability to concern himself with problems of real practical import while rigorously examining and carefully expounding the principles involved in the solutions proposed. The main part of his doctoral thesis [B6]* is concerned with admissibility and complete classes of tests in the multiparameter exponential family of distributions, and shows that the situation illustrated in his [B5], where a test that is locally best can, for distant alternatives, be worst, cannot arise in this case. His careful note [B4] on combining independent tests of significance proves an optimality property of Fisher's procedure, while pointing out that this procedure is not, in general, uniquely optimal. This work brought him into association with the 1954 Polio Vaccine Trials (1955). All of this early work is in the spirit of, and is referred to in, E.L. Lehmann's classic book, *Testing Statistical Hypotheses.*

After completing his doctorate Allan lectured at Columbia. He continued his mathematical research and acquired other interests. Professor T.W. Anderson writes,

"In 1954 Herbert Solomon, Rosedith Sitgreaves, and I organized a program of research on classification and discrimination. It was located in Teachers College and was supported by the School of Aviation Medicine. (It continued into the 70's.) The project involved many statisticians and social scientists at Columbia, including Herbert Robbins, Howard Raiffa, Paul Lazarsfeld, and Irving Lorge. Allan was also in this program and developed statistical methodology for the logistic model; he preferred the logistic to the normal (probit) because of the existence of a sufficient statistic. The rest of us published our work in *Studies in Item Analysis and Prediction* edited by Herbert Solomon, 1961, (Stanford University Press). I think Allan's research was reported in [B25] and [B27].

"In the early fifties, Lazerfeld, Solomon, and I organized the Behavioral Models project at Columbia under a contract with the Office of Naval Research. The purpose was to develop mathematical models and related statistical techniques for the social sciences. Duncan Luce eventually became director. (The Luce-Raiffa book came out of this project.) Allan worked on this project, too.

"A third activity at Columbia was the University Seminar on the Method and Content of the Social Sciences. When it split into two, we participated in the University Seminar

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* The numbers [B1], ... refer to the list of Birnbaum's publications at the end of this article.

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on Mathematical Methods in the Social Sciences. Included in these seminars was Ernest Nagel. I think it was through these seminars that Allan and Nagel became friends. Lazarsfeld always had an interest in latent structure analysis and tried to involve the statisticians, including Allan."

He went on to lecture at Stanford and Imperial College before being appointed full Professor at the Courant Institute of Mathematical Sciences of New York University, a post he held until 1975. He spent the years 1971-1974 on sabbatical leave and, for part of that time, as a Visiting Fellow of Peterhouse, Cambridge University. In 1975 he accepted the Chair of Statistics at the City University, London, England, where he remained until his death. Meanwhile, from 1974 on he was a Visiting Professor at the Cornell University Graduate School of Medical Sciences, and from 1975 a Consultant to the Open University. He was elected a Fellow of the Institute of Mathematical Statistics, the American Statistical Association and the American Association for the Advancement of Science.

The review [B7] and the paper [B8] continue Birnbaum's development along earlier lines; but in [B9] he makes a careful attempt to formalize, and thus to deepen our understanding of Cuthbert Daniel's intuitively appealing device of the half-normal plot. It illustrates one aspect of what continued to preoccupy him from then on—the task of moulding into a coherent, adequately formalized whole, the collection of intuitively appealing methods in current successful use among applied statisticians. There followed three more technical papers [B10], [B11], [B12], on essentially standard lines, except that in [B12] Bayes's Theorem is used at a time when such applications were less popular than now. Birnbaum then returned to general issues with [B13] and [B14], in which an attempt is made to develop a theory of estimation without reference to loss functions, combining "point estimation" with "interval estimation" and "confidence curves", and using what are now called "pivotal quantities" or "estimating functions" under the name of "quasistatistics". Fisher's score function was shown to be an admissible quasistatistic and in that sense a nonasymptotic justification of the method of maximum likelihood is given. This finite sample justification is distinct from that provided by Godambe (1960).

His move towards detailed exploration of the consequences of "likelihood inference" is carried further in [B15], [B16], [B17] and [B18], of which the central paper is [B16]. Here he starts from Neyman's view, that the evidential meaning of the result x of an experiment E must be found in terms exclusively of probabilities definable in advance of knowing x. He uses the notion of "mixture" of experiments introduced by Cox (1958), along with the associated principle of conditionality. He shows that in testing a simple hypothesis H_1 against a simple alternative H_2 the only relevant quantity is the observed likelihood ratio $f_2(x)/f_1(x)$. He shows also that the appropriate long run interpretation of this is to be found in the "odds of error" $(1 - \beta)/\alpha$, in the usual notation, rather than in terms of α and β considered separately. Provided H_1 and H_2 exhibit appropriate symmetry, this long run interpretation is the standard one but in the absence of such symmetry reference is made to a sequence of conceptual repetitions in which the roles of H_1 and H_2 can be interchanged. Reference to these results was made by Kiefer (1977) in his important paper.

These papers lead up to Birnbaum's major contribution to the study of foundations [B19], further discussed in [B30] and more clearly and tersely restated in [B35]. The explosive impact of his proof that the widely accepted sufficiency and conditionality principles together implied the much less widely accepted likelihood principle can be judged to some extent from the discussion printed following [B19].

As reformulated in [B35], Birnbaum's argument takes as the model of an experiment E the triplet (Ω, S, f) where $S = \{x\}$ is the (discrete) sample space, $\Omega = \{\theta\}$ is the parameter space, and $f = f(x, \theta)$ is the probability function of x, for each θ. Then he takes the pair (E, x), when x is observed, to constitute a model of statistical evidence. The proposition that two such models with common parameter space represent equivalent statistical evidence is denoted by $Ev(E, x) = Ev(E^*, x^*)$. The statistics $h(x)$, $t(x)$ are defined, respectively, to be (a) ancillary, or (b) sufficient iff (a) $f(x, \theta) = g(h)f(x \mid h, \theta)$ or if (b) $f(x, \theta) = g(t, \theta)f(x \mid t)$. For given (E, x), (E_h, x) denotes the model of evidence determined by an outcome x of the experiment $E_h = (\Omega, S_h, f_h)$ where $S_h = \{x : h(x) = h\}$ and $f_h = f(x \mid h, \theta)$, while (E', t)

denotes the model of evidence determined by the outcome t of the experiment $E' = (\Omega, S', g)$, where $S' = \{t : t = t(x), x \in S\}$. Then the axioms of statistical evidence considered are

Conditionality (C): If $h(x)$ is ancillary, then $Ev(E, x) = Ev(E_h, x)$, where $h = h(x)$.

Likelihood (L): If, for some $c > 0$, $f(x, \theta) = cf^*(x^*, \theta)$ for all θ in Ω, then $Ev(E, x) = Ev(E^*, x^*)$.

Sufficiency (S): If $t(x)$ is sufficient, then $Ev(E, x) = Ev(E', t)$, where $t = t(x)$.

Weak Sufficiency (S'): If, for some $c > 0$, $f(x, \theta) = cf(x^*, \theta)$ for all θ in Ω, then $Ev(E, x) = Ev(E, x^*)$.

Mathematical equivalence (M): If $f(x, \theta) = f(x', \theta)$ for all $\theta \in \Omega$, then $Ev(E, x) = Ev(E, x')$.

With the above formulation Birnbaum proved that

(I) C and M jointly imply L,
(II) $L \rightarrow S \rightarrow S' \rightarrow M$, and $L \rightarrow C$,
(III) Each of the three implications $M \rightarrow S' \rightarrow S \rightarrow L$ is false.

In his 1962 paper the axiom M was not separately specified, and this has given rise to a widespread misapprehension, that he proved that C alone implies L. He regarded M as, in some sense, "obvious". Actually with Birnbaum's definition of a statistical experiment, namely, (Ω, S, f), axiom M is a "tautology" (Godambe, 1979). However a real statistical experiment would have many other identifying characteristics, for instance an additional mathematical structure. In fact it was the failure of M to be valid, in the presence of group structure, or other such structure with evidential relevance, that caused Barnard, Jenkins and Winston (1962) to support L only for experiments in which such structures are absent. Using a wider definition of a statistical experiment than Birnbaum's, the notion involved in M was formalized by Dawid (1977) in the "distribution principle" (DP): For any two experiments E_1, E_2 having the same (Ω, S, f), $Ev(E_1, x) = Ev(E_2, x)$, for all $x \in S$. Barnard et al. restricted the valid application of L to experiments for which DP is taken as true. A detailed discussion of the issues involved here is given in Godambe (1979), where it is shown, in particular, that M is not at all obvious, and that without M, C does not imply L. Further developments are provided by the "functional model" for statistical inference proposed by H. Bunke (1975), extended by O. Bunke (1976), and further discussed by Dawid and Stone (1982) and by the closely related "pivotal model" proposed by one of us (Barnard, 1977). Both these models contain elements additional to the triplet model considered by Birnbaum, and so the axiom M fails to apply to them. The same is true of Fraser's models (1968, 1979).

There can be no doubt that Birnbaum's [B35] represents a peak of clarity in discussions of foundations which has yet to be surpassed.

Although [B35] is mainly concerned with the case where the possible observations form a discrete set, two sections of the paper deal with the extension of the arguments to the continuous case. Here it is pointed out that S' may be needed to replace M in the derivation of L from C, and a very careful discussion is given of the complications that may appear to arise from the limited degree of arbitrariness that exists in the choice of density function. The fact that Schwartz's paper (1962), with the title "The Pernicious Influence of Mathematics on Science", is referred to indicates how well Birnbaum recognized the extent to which merely mathematical technicalities could get in the way of serious discussion of statistical issues. He fully understood the point, repeatedly emphasized by R.A. Fisher, that the process of advancing empirical knowledge of the natural world is quite distinct from, though it can be aided by, research in pure mathematics.

Having established, as he thought, that the process of interpretation of experimental evidence could be reduced to that of interpreting the likelihood function, Birnbaum soon found difficulty in cases other than those which could be reduced to problems of "pairwise comparison" of simple hypotheses. He spent a long time wrestling with the problem posed by Armitage (1961) (which had earlier been posed to one of us by Bartlett), based on a

sequence of independent binomial trials with unknown probability θ. For any true value θ_0 the law of the iterated logarithm tells us we can, by continuing observations sufficiently far, arrive at a result such that the standardized maximum likelihood pivotal $(\hat{\theta} - \theta_0)\sqrt{n}/\sqrt{\{\theta_0(1 - \theta_0)\}}$ exceeds any prescribed positive K. If, as L implies, the sampling rule is irrelevant to the inference, how can we reconcile this with the common interpretation of such a result with n fixed in advance, that θ_0 is implausible? Later he came to be equally concerned with difficulties such as those presented by a single observation x from a normal distribution with unknown mean μ and unknown standard deviation. The likelihood here is infinite at the point $\sigma = 0$, $\mu = x$, suggesting very strongly that small values of σ are more plausible than larger values, no matter what the value of x that is observed. We appear to have misleading evidence, with probability 1.

Birnbaum formulated the problem area of main concern as that "of determining precise concepts of statistical evidence, systematically linked with mathematical models of experiments, concepts which are to be non-Bayesian, non-decision-theoretic, and significantly relevant to statistical practice." [B28]. His reasons for excluding the Bayesian and decision-theoretic approaches were, essentially, that no conceptual problems remain to be solved with these approaches; with the Bayesian approach, for example, we have only one probability distribution, defined by the prior distribution of the parameters and the conditional distribution of the observations given the parameters; "inference" from the observations simply involves conditioning on the observed values. In [B28] he went on to say that major developments in his problem area have turned upon two kinds of criteria for such concepts: (a) judgements that certain aspects of statistical evidence are irrelevant to evidential meaning; (b) a requirement of adequate operational content, in terms of suitably controlled probabilities that evidential interpretations will be strongly misleading. This he described as the main feature of the "confidence concept". He concludes this discussion:

"It has seemed to some (including this writer) that any adequate concept of statistical evidence must meet at least certain minimum versions of both of the criteria just indicated. But the difficulties in developing such a concept have become increasingly apparent, and it now seems rather clear that no such adequate concept of statistical evidence can exist."

Such seems to have been Birnbaum's final view. One should here note the word "adequate" in the final sentence. He had, already in [B16] shown that one could satisfy both criteria, with the likelihood ratio, in the case of comparison of two simple hypotheses. And from verbal discussions we had with him it was clear that he was willing to admit the possibility that adequate concepts could be found in other specific cases, though he thought the domain to which such concepts could be applied would far from cover the major applications of statistical method. In the main, he concluded, the concepts of "statistical evidence" must be regarded as anomalous.

These later views are set forth in [B26], [B28], [B29], [B35], [B37], [B40], and his last paper [B41] (an important paper, printed with discussion in *Synthese*, after it had been rejected for reading by the Royal Statistical Society.). For the rest, in [B21], [B23], [B24], [B31], [B33], working mainly with E. Laska and V. Miké, he developed a theory of estimation of location using order statistics, coupled with Pitman's conditioning ideas, which has an important bearing on problems of robustness in this area. He strongly advocated the development of modern and historical case study materials to illustrate and clarify the roles of statistics in scientific research. He believed that this approach would constitute important research concerning the nature of theoretical and applied statistics, and that the results of this work might also be of value in the teaching of statistics. In this context he was interested in exploring the nature of statistical consulting, including its substantive, administrative and teaching aspects. (See [B34]). In [B38] he made a substantial conceptual contribution to the experimental science of genetics.

His long-standing interest in medicine found special expression in the work he undertook in 1974 as Visiting Professor at the Cornell University Graduate School of Medical Sciences and the associated Memorial Sloan-Kettering Cancer Center, where his former student V.

Miké was engaged in the establishment of a department of statistics. In the course of several visits to the medical center in New York each year, he began focusing on case study treatment of biomedical research problems, some involving the work of immunologist Robert A. Good and colleagues. He studied pertinent writings of Sir Peter Medawar, of special interest because the latter is unusual among biological scientists in emphasizing the significance of philosophy of science for a scientific theory or research program.

As a person Allan was gentle, sensitive, and kind. He had a subtle sense of humour that came across most effectively in private conversation. His only child, a son Michael, was born to him late in life and became to him a source of great joy. He spoke fondly of his plans to take an important part in the education of his son, as had been done by his father for John Stuart Mill.

His other interests included the theatre. He loved the seashore and spent summers near the ocean whenever he could.

A symposium was held to honor Allan's memory at Memorial Sloan-Kettering Cancer Center on May 27, 1977. The proceedings of the symposium: "Medical Research: Statistics and Ethics" were published in *Science* (1977) **198** 677-705. Two memorial articles are published about Allan, one by Godambe (1977) and the other by Lindley (1978). The latter article was written jointly by D. R. Cox and D. V. Lindley and contains comments on Birnbaum's ideas on statistical inference. One issue of *Synthese* (1977 **36** No. 1) is dedicated to Allan's memory.

Thanks are due to many of our colleagues who helped with many details included in this obituary. In this respect special mention must be made of T. W. Anderson, Valerie Miké and E. L. Lehmann.

THE PUBLICATIONS OF ALLAN BIRNBAUM

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[B2] 'Sequential Probability Ratio Confidence Sets', *Ann. Math. Statist.* **24** (1953), pp. 686, abstract.

[B3] 'Statistical Methods for Poisson Processes and Exponential Populations', *J. Amer. Statist. Assoc.* **49** (1954), pp. 254–266.

[B4] 'Combining Independent Tests of Significance', *J. Amer. Statist. Assoc.* **49** (1954), pp. 559–574.

[B5] 'Admissible Tests for the Mean of a Rectangular Distribution', *Ann. Math. Statist.* **25** (1954), pp. 157–161.

[B6] 'Characterizations of Complete Classes of Tests of Some Multiparametric Hypotheses, with Applications to Likelihood Ratio Tests', *Ann. Math. Statist.* **26** (1955), pp. 21–36.

[B7] Review of *Statistics in Research* by Bernard Ostle, *J. Amer. Statist. Assoc.* **51** (1956), pp. 385–386.

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[B32] 'Discussion of H.O. Lancaster's paper 'Statistical Bibliographies' ', *J. Roy. Statist. Soc., Series A* **133** (1970), pp. 458–460.

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