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Featured Article

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On becoming a Bayesian: Early correspondences between J. Cornfield and L. J. Savage

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Jerome Cornfield was arguably the leading proponent for the use of Bayesian methods in biostatistics during the 1960s. Prior to 1963, however, Cornfield had no publications in the area of Bayesian statistics. At a time when frequentist methods were the dominant influence on statistical practice, Cornfield went against the mainstream and embraced Bayes. The goals of this paper are as follows: (i) to explore how and why this transformation came about and (ii) to provide some sense as to who Cornfield was and the context in which he worked. Copyright © 2012 John Wiley & Sons, Ltd.

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1. Introduction

By the mid 1960s, Jerome Cornfield was arguably the leading proponent for the use of Bayesian methods in biostatistics. His Bayesian outlook was forcefully and systematically presented in three seminal papers [1–3]. The applications that motivated these papers were based on issues arising in the design and analysis of clinical trials, although his contributions both to Bayesian theory and practice were much more general [4]. It is interesting to note that Cornfield had not published in the area of Bayesian statistics prior to 1963. Yet, at a time when the works of Fisher, Neyman, Pearson, and Wald were the dominant influences on statistical practice, Cornfield went against the mainstream and embraced Bayes. The goals of this paper are as follows: (i) to explore how and why this transformation came about and (ii) to provide some sense as to who Cornfield was and the context in which he worked.

Cornfield joined the National Cancer Institute of the National Institutes of Health (NIH) in 1947. His early statistical contributions at the NIH were primarily in the development of methods for the design and analysis of laboratory [5] and epidemiologic studies [6]. He may be best known for his contributions during the 1950s on establishing a causal link between cigarette smoking and lung cancer [7] and for his perspectives on statistical thinking and practice [8]. In 1960, he moved to the National Heart Institute, and his attention turned to the design and analysis of randomized controlled clinical trials. Cornfield's methodological contributions were far reaching and, to this day, continue to be influential [9].

In 1977, when I was a graduate student at the University of Michigan, Cornfield visited the Department of Biostatistics. In a casual conversation, I asked if he had ever visited the Department before. He replied that he had, in the early 1960s to give a talk. I recall further that he mentioned that L. J. Savage was at Michigan at the time. I am not sure why I remember this conversation, but the suggestion of a connection between Cornfield and Savage stayed with me.

Leonard Jimmie Savage was a Professor in the University of Michigan's Department of Mathematics from 1960 to 1963. During the 1950s and 1960s, Savage advanced a Bayesian theory of statistics based on a subjective or personal definition of probability. In a memorial volume honoring Savage, Frank Anscombe commented that Savage was amazingly conscientious about correspondences. 'He took a keen interest in other people's ideas and activities' and 'he hated to leave a letter unanswered,

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for that would be to slight another person' [10]. Remembering my conversation with Cornfield about his earlier visit to the University of Michigan, I wondered whether there was a record of any correspondences between Cornfield and Savage during this period. Indeed, to my great delight, there was. In the Yale University Library, there exists a collection of Savage's papers and correspondences, including his correspondences with Cornfield. As we will see from a review of these letters, specifically during the 10-month period from May 1961 to March 1962, Jimmie Savage played a significant role in the development of Cornfield's Bayesian outlook.

2. Correspondences between J. Cornfield and L. J. Savage

2.1. May–June 1961

On May 17, 1961, Cornfield visited the University of Michigan to present a seminar in the Department of Biostatistics. The Department Chair at the time was Felix Moore. Cornfield and Moore had known each other at the NIH where Moore had been the Chief of the Biometric Branch of the National Heart Institute from 1948 to 1957. Figure 1 [11] is a copy of what appears to be the first correspondence between Savage and Cornfield, dated May 12, 1961, one week before Cornfield's visit to Michigan.

May 12, 1961

Mr. Jerome Cornfield
National Institute of Health
Bethesda, Maryland

Dear Cornfield:

I see that you are to lecture here at Michigan at the seminar of the Department of Biostatistics on May 17. The announcement that you might possibly touch in your talk on a talk I gave there a few weeks ago increases my natural desire to share scientific and philosophic thoughts with you. I am, therefore, lending you herewith a dittoed version of a lecture that I gave in London two years ago and that perhaps summarizes my current ideas about the foundations of statistics as well as any one lecture could.

I do hope that while you are here we shall have good opportunities to talk about the foundations of statistics and other subjects, either before or after your talk.

Most cordially yours,

Sincerely Jimmie Savage

Leonard J. Savage

LJS/dld

Enc.

BC: Felix Moore
Leslie Kish

Figure 1. May 12, 1961, letter from Savage to Cornfield.

Although there is no record of the title or topic of Cornfield's seminar, we see from Savage's letter that the seminar most likely had a Bayesian theme. Even to a casual acquaintance, Cornfield was known as Jerry, so the salutation, 'Dear Cornfield', and the absence of personal inquiries suggests that Savage and Cornfield did not know each other personally. The topic of Cornfield's talk obviously piqued Savage's interest, apparently overlapping with the topic of a talk Savage had recently given in the Department. Savage appears hopeful that he and Cornfield will have a chance to talk during Cornfield's visit. The notes he shared with Cornfield appear to be a written version of a lecture Savage had given in London in the summer of 1959, entitled 'Subjective Probability and Statistical Practice'.‡ These notes would eventually be published with discussion in a monograph entitled *The Foundations of Statistical Inference* [12]. [NB. For younger readers, 'dittoed' refers to a method for reproducing documents that was commonly used prior to the xerox machine.]

This letter from Savage establishes that Cornfield was thinking about Bayesian inference as early as 1961. His talk was likely motivated by a problem he had encountered in practice, and it likely would have been something on which he was currently working. He liked to speak publicly about things he was working on even if they were incomplete – he considered this to be part of the research process, helping him to sharpen his understanding of the problem he was working on and highlighting what the open issues were. This was a period when the NIH began a program supporting investigations of therapeutic interventions using randomized controlled clinical trials and the National Heart Institute, in particular, was initiating several large multisite trials. Cornfield would have played a key role in the planning of these trials [13].

Cornfield does not write back to Savage until the end of June (Figure 2) [14]. His note is brief. Those who knew Cornfield can easily imagine the twinkle in his eye as he addressed his letter 'Dear Savage'. Significantly, there is no indication that Cornfield and Savage met during Cornfield's visit to Ann Arbor or that Cornfield was previously familiar with Savage's 1959 notes.

2.2. *December 1961*

The annual meeting of the American Statistical Association (ASA) was held on December 27–30, at the Hotel Roosevelt in New York City. This meeting would prove to be a watershed moment not only for Cornfield and the development of his Bayesian outlook but also for the field of statistics in general. At these meetings, Cornfield presented a paper entitled 'An Objective Bayesian Calculus', the goal of which, according to the abstract, was to derive 'a general method for obtaining prior distributions based on a principle designed to explicate the idea of initial impartiality among all possible states of nature' rather than, for example, from prior information. He called this principle the 'impartiality principle' [15]. It is very likely that Cornfield's ASA talk was a version of the seminar he gave in Ann Arbor the previous May. I will say more about Cornfield's paper after commenting on a truly historic event that took place at these meetings.

At a special evening session, Allan Birnbaum presented a paper on the likelihood principle entitled 'On the Foundations of Statistical Inference' [16]. The likelihood principle states that all evidence obtained from an experiment about an unknown quantity θ is contained in the likelihood function of θ for the given data. Although the likelihood principle follows directly from Bayes' rule and was always implicit in the Bayesian approach to statistics (e.g., [12]), the significance of Birnbaum's paper was that he showed that the likelihood principle was a consequence of two basic principles accepted by almost all statisticians – the sufficiency principle (that a sufficient statistics summarizes the evidence from an experiment) and the conditionality principle (that experiments not actually performed should be irrelevant to conclusions) (e.g., [17]).

Birnbaum's discussants at the session included statistical luminaries such as Savage, Barnard, Cornfield, Bross, Box, Kempthorne, Dempster, Pratt, Good, and Lindley. The reception was mixed, ranging from Savage who proclaimed, 'Without any intent to speak with exaggeration or rhetorically, it seems to me that this is really a historical occasion. This paper is a landmark in statistics...' [18], to Irwin Bross who derisively stated, 'The author here proposes to push the clock back 45 years, but at least this puts him ahead of the Bayesians, who would like to turn the clock back 150 years' [19].

‡*In subsequent letters, Cornfield and Savage refer to sections in a document they call 'Subjective Basis'. This could be the original name of Savage's London lecture or refer to notes that Savage was preparing for a textbook also written in the summer of 1959 entitled 'Subjective Basis of Statistical Practice'.*

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22 June 1961

Dr. L. J. Savage
Department of Mathematics
University of Michigan
Ann Arbor, Michigan

Dear Savage:

I am returning your Subjective Probability and Statistical Practice which you were kind enough to loan me. I have had a copy made and hope to make good use of it.

Best regards,

Jerome Cornfield

Jerome Cornfield

Encl.

OK - DRD
6/26/61

National Cancer Institute   National Heart Institute   National Institute of Allergy and Infectious Diseases   National Institute of Arthritis and Metabolic Diseases
National Institute of Dental Research   National Institute of Mental Health   National Institute of Neurological Diseases and Blindness   The Clinical Center
Division of Biologics Standards   Division of Business Operations   Division of Research Grants   Division of Research Services

Figure 2. June 22, 1961, letter from Cornfield to Savage.

Cornfield's discussion [20] is concerned with trying to better understand a practical implication of the likelihood principle – the irrelevance of the stopping rule, which he and many other statisticians at the time were having difficulty accepting. From the frequentist perspective, an experimenter's intentions for stopping a study could affect the conclusions of the study because inference could depend both on the data realized and the stopping rule leading to the data. Thus, it would be possible to reject a *true* hypothesis with probability one by collecting data sequentially, performing a non-Bayesian fixed sample size test of the hypothesis and stopping when the departure from the hypothesis is significant at some preassigned level [12, 21]. In contrast, the stopping rule principle states that once the observations have been obtained, the reason for stopping an experiment (the stopping rule) should be irrelevant to inferential conclusions. The stopping rule principle is believed to have been first stated explicitly by Barnard [22] and was shown formally by Birnbaum to be a consequence of the likelihood principle. We know from Cornfield's later writings that his interest in these issues was strongly motivated by practical problems that he and others were confronting in the design and analysis of clinical trials. For example, in 1966, he wrote, 'Clinical trials have a much more complex structure than is assumed in the classical theory of hypothesis testing. It often happens that unforeseen essential complications arise, or that detailed analysis of results disclose interesting, but unanticipated, relationships so that the most appropriate statistical analysis . . . [is not] the test of some pre-specified hypothesis' [23].

We now return to Cornfield's ASA talk. In the Savage archive, I found a written version of the talk Cornfield presented at these meetings. Savage had read the paper and marked it up with comments. There is evidence that he did this the day after Birnbaum's talk, returned the marked-up copy to Cornfield, and discussed it with him in person. Savage's written comments were in the tradition of the first discussant at a Royal Statistical Society meeting – he began with 'Sincere congratulations for ingenuity and thoroughness' and then noted 'five or so serious defects'. Savage's comments are direct, insightful, and constructive. Recall that Savage at this time was advancing a subjective view of probability for statistical practice and Cornfield's goal in his paper was to propose an objective program of Bayesian inference, two approaches that were not necessarily compatible. Finally, I note that it is clear from the manuscript that Cornfield was familiar with Harold Jeffreys' work [24] and makes a clear distinction between his approach and Jeffreys' objective perspective, which Jeffreys took to be based on the principle of insufficient reason (see, e.g., [25]).

2.3. January 1962

On the 18th of January, Cornfield wrote to Savage [26]: 'I am returning the copy of your discussion notes [NB. Presumably, referring to Savage's written comments on Cornfield's ASA talk referred to above], for which many thanks. I have been mulling over our conversations, and I must say your remarks on the irrelevance of the stopping rule are beginning to take hold.' The remainder of the letter consists of three topics. In the first, Cornfield continues to explore from a Bayesian perspective his understanding of the irrelevance of the stopping rule, citing as his source the notes Savage sent him the previous May. The second is a question Cornfield had been thinking about and feels he understands better after having read Savage's 1959 notes. The question is 'Does a given numerical value of the likelihood ratio have the same meaning for any dichotomy?' Cornfield uses this opportunity to check his understanding of the evidential meaning of the likelihood ratio. Finally, Cornfield returns to his ASA talk. In Savage's written comments, he suggested that the principle Cornfield was advancing 'conflict[ed] with the likelihood principle forcefully defended by A.B. [Allan Birnbaum] last night'. In this letter, Cornfield proposes a modification of the principle that he believes does not violate the likelihood principle and asks Savage for his reaction. To be clear, it is not that Cornfield is wedded to this proposed objective approach, but rather, he is using it as a probe to explore his understanding of Savage's comments and, in particular, the full implications of the likelihood principle. As in his previous letter, Cornfield opens with 'Dear Savage' and signs it with his given name 'Jerome Cornfield'.

2.4. February 1962

Savage wrote to Cornfield on February 22 apologizing for the 'long delay' in responding [27]. Savage's letter is two pages long, single spaced. The letter opens again with 'Dear Cornfield'. Regarding Cornfield's understanding of the stopping rule principle, Savage confirms that he is on the right track and elaborates and generalizes a little more from Cornfield's statements.

With respect to Cornfield's question about whether the numerical value of the likelihood ratio has the same meaning for any dichotomy, Savage responds at length:

> There is a sense in which a given likelihood ratio does have the same sense from one problem to another, at least from the Bayesian point of view. It is always the factor by which the posterior odds differ from the prior odds. However, all schools of thought are properly agreed that the critical likelihood ratio will vary from one application to another. The Neyman-Pearson school expresses this by saying that the choice of the likelihood ratio is subjective and should be made by the user of the data for his particular purpose. As a Bayesian, I would say that a critical likelihood ratio for a dichotomous decision about a simple dichotomy depends (in an evident way) on the loss associated with the decision and on the prior probabilities associated with the dichotomy. I imagine that section 3.5 of "Subjective basis" [NB. Savage's 1959 notes] will, if necessary, clarify this idea for you.

> The fact that the reaction to an experiment depends on the content of the experiment as well as on its mathematical structure and whatever economic issues might be involved seems to me to be brought out by the following triplet of examples that occurred to me the first time I taught statistics, when I still had a completely orthodox orientation. Imagine the following three experiments: 1. Fisher's lady has correctly dealt with ten pairs of cups of tea [NB. Many readers will recognize this example to be from R. A. Fisher's famous lady tasting tea experiment; see [28, p. 11-26]]. 2. The professor of 18th century musicology at the University of Vienna has correctly decided for each of 10 pairs of pages of music which was written by Mozart and which by Hayden. 3. A drunk in a parlor car has succeeded 10 times in correctly calling a coin secretly tossed by you. These three experiments all have the same mathematical structure and the same high significance level. Can there, however, be any question that your reaction to them is justifiably different? My own would be: 1. I am still skeptical of the lady's claim, but her success in her experiment has definitely opened my mind. 2. I would originally have expected the musicologist to make this discrimination; I would even expect some success in making it myself; he, an expert in the matter, felt sure that he could make it. His success in 10 correct trials confirms my original judgment and leaves no practical doubt that he would be correct in substantially more than half of future trials, though I would not be surprised if he made occasional errors. 3. My original belief in clairvoyance was academic, if not utterly nonexistent. I do not even believe that the trial was conducted in such a way that trickery is a plausible hypothesis, and feel sure that the drunk simply had an unusual run of luck. Of course, these tests are not simple dichotomies, but I think you will find them germane to your question.

I included Savage's full response to Cornfield's question for several reasons. I believe this letter is a turning point in Cornfield's and Savage's relationship, and it is illuminating to read Savage in his own words. His response is instructive, supportive, and patient. Further, the anecdote in the second paragraph is not only entertaining but pedagogically brilliant. I am not sure it has appeared previously in print, but if not, this was a nice opportunity to make it more widely available.

As noted earlier, the third topic in Cornfield's letter was a proposed modification of his impartiality principle for generating prior distributions presented in his ASA talk. Cornfield concluded in his January 18th letter, 'This [modification] leads, I believe, to a system that does not violate the likelihood principle. Does it meet your objections?' Savage responds that it does not and briefly explains why. The statistical details are not central for the purpose of this paper. A discussion of this issue, however, will be the focus of their next several letters. Significantly, Savage signs this letter 'Jimmie'.

2.5. February 27, 1962

Cornfield wrote back in less than a week [29]. His letter begins 'Dear Jimmie'. Cornfield is puzzled by Savage's objection to his example and writes 'I don't see your argument'. In this letter, Cornfield elaborates with a detailed and clarifying counterexample. He does note respectfully, however, that 'If there is something wrong with this argument, please do not hesitate, etc.' Cornfield concludes the letter as follows:

> I hope you don't mind being pestered with these questions. I find that after having spent all my adult life in statistics, I'm finally on the verge of understanding the subject, and it is helpful to have somebody to turn to.

What an amazing statement. Cornfield is energized and feels that he is at an intellectual crossroad. He has clearly been thinking hard about Bayesian ideas for awhile, but the feedback and guidance from Savage have redirected his thinking. Savage's subjective approach seems to have provided Cornfield with a clearer understanding of the likelihood principle and the irrelevance of the stopping rule leading to an intellectual clarity about the foundations of statistics that had previously been missing. Cornfield signs this letter 'Jerry'.

2.6. March 1962

Figure 3 [30] is a copy of Savage's reply dated, March 2nd, to Cornfield's modification of the principle proposed in his ASA talk. Savage acknowledges that his initial response was 'all wet' and that Cornfield's modification does not, in fact, violate the likelihood principle. What follows seems to be characteristic of Savage's rhetorical style – restating the problem presented, offering some penetrating insights, and using it to illuminate some general principle. The final paragraph is personal, encouraging, and empathetic, acknowledging his pleasure in corresponding with Cornfield.

March 2, 1962

Mr. Jerome Cornfield
National Institutes of Health
Public Health Service
Department of Health, Education, and Welfare
Bethesda 14, Maryland

Dear Jerry:

The final paragraph of my letter of 22 February is all wet as you make clear in your kind letter of the 27th. Let me discuss afresh, and more cautiously, the final paragraph of your letter of 18 January. I would separate three points.

1. The method as proposed does seem in accord with the likelihood principle.

2. If several independent measurements were made of the same parameter but on the basis of different models $f_1(t_1/\theta)$, $f_2(t_2/\theta), \ldots,$ then the method suggested by your paragraph would not satisfy the likelihood principle. You doubtless never intended to use your method here; nonetheless the inappropriateness of such an extension may be a hint that the original idea is less val uable than it might at first seem. To put it differently, some general good might come of reflecting on how to handle a sequence of mixed observations.

3. For my own part, beyond the likelihood principle, I have a clear idea what I want initial and final probabilities to mean. Within this concept, there is no possibility that the initial distribution of a parameter should depend at all on the kind of instrument with which I undertake to measure it.

Pratt's argument about the likelihood principle is given, perhaps too succinctly, in the second complete paragraph on page 166 of his review of Lehmann, JASA 56 (1961), 163-66.

I enjoy corresponding with you very much. The subject has always been slippery, or even treacherous, and perhaps we only flatter ourselves that we are now on "the verge of understanding," but we do seem to be making at least a spurt of progress.

Best regards,

Jimmie Savage

LJS: jm

Figure 3. March 2, 1962, letter from Savage to Cornfield.

3. Discussion

Barabara Tuchman, writing about biography, noted that primary sources such as unpublished letters serve as a 'prism of history'. 'There is an immediacy and intimacy about them', she wrote, 'that reveals character and makes circumstances come alive' [31, p. 19]. The Cornfield–Savage letters provide a rare window into the professional and personal relationship between two giants in the history of statistics. From these letters, we not only get a glimpse of Cornfield working hard to understand Savage's subjective perspective on statistics but also Savage as mentor and teacher taking a keen interest in an obviously thoughtful and committed colleague. Cornfield did not pursue the objective approach described in his December ASA talk; rather, his Bayesian outlook was firmly rooted in Savage's subjective perspective. Cornfield and Savage continued writing to each other through the 1960s. The nature of their correspondences evolved to a more equal give and take of ideas, sharing problems and discussing solutions. The last letter from Cornfield in the Savage archive is dated April 27, 1970, approximately 18 months before Savage's passing at the age of 54 years.

It is now clear that Cornfield's interest in Bayesian methods began prior to 1961 and that the clarity of his Bayesian outlook began to take shape following Birnbaum's ASA paper on the likelihood principle and his subsequent discussions with Savage. Cornfield's frustration with the prevailing frequentist methods of the time grew out of a need for a theory of statistics that would truly help advance scientific discovery and would provide meaningful measures of evidence. In the early 1960s, these issues seemed to have come to a head around the design and analysis of clinical trials. For Cornfield, acceptance of the likelihood principle was the cornerstone to a coherent theory of statistics that addressed the logical limitations of significance tests. For him and others at the time, the analysis of sequential trials was ground zero. It is useful to hear Cornfield in his own words on this topic. In 1966, he wrote:

> I shall be concerned in this paper with a single question, the answer to which is of great importance to all those engaged in the sequential collection of data. The question is: Do the conclusions to be drawn from any set of data depend only on the data or do they depend also on the stopping rule which led to the data? In discussing this question I shall draw heavily upon the theoretical work of others, particularly L.J. Savage but also F.J. Anscombe, G.A. Barnard, A. Birnbaum, and D.V. Lindley. Biostatisticians have tended to regard the theoretical developments suggested by these names as unduly abstract and perhaps of no great relevance to statistical practice. . . . These newer developments, abstract or not, are, in my opinion of great relevance to biostatistical practice and their absorption into thinking, teaching and consultation is becoming overdue.
>
> . . . [T]o most scientists without previous exposure to statistics, as well as to most intelligent laymen, any dependence on stopping rules . . . seems like a violation of common sense. Those biostatisticians who defend sequential analysis on the other hand would argue that dependence of conclusions on stopping rules is required to preserve the critical level, i.e., the lowest significance level at which the hypothesis can be rejected for given data. If one accepts the importance of preserving the critical level, then clearly conclusions must depend on the stopping rule. But what is not immediately obvious is that the critical level provides an appropriate measure of the amount of evidence in the data for or against the hypothesis [1].

In 1966, the NIH sponsored a symposium on the role of hypothesis testing in clinical trials. At the NIH at this time, there was a growing appreciation based on experience for a disconnect between the theory and practice of clinical trials. Cornfield's presentation illustrated many of these challenges in the context of cardiovascular disease trials. It is fitting to close with Cornfield's closing comments from that symposium, which captures the essence of his statistical outlook:

> . . . [O]f course a re-examination in the light of results of the assumptions on which the pre-observational partition of the sample space was based would be regarded in some circles as bad statistics. It would, however, be widely regarded as good science. I do not believe that anything that is good science can be bad statistics, and conclude my remarks with the hope that there are no statisticians so inflexible as to decline to analyze an honest body of scientific data simply because it fails to conform to some favored theoretical scheme. If there are such, however, clinical trials, in my opinion, are not for them [23].

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