

Rejoinder

I. Introduction

I am honored and grateful to have so many interesting and challenging comments on my paper. I want to thank the discussants for their willingness to jump back into the thorny quagmire of Birnbaum's argument. To a question raised in the paper "Does it matter?", these discussions show the answer is yes. The enlightening connections to contemporary projects are especially valuable in galvanizing future efforts to address foundational issues in statistics.

As long-standing as Birnbaum's result has been, Birnbaum himself went through dramatic shifts in a short period of time following his famous (1962) result. More than of historical interest, these shifts provide a unique perspective on the current problem. Already in the rejoinder to Birnbaum (1962), he is worried about criticisms (by Pratt 1962) pertaining to applying WCP to his constructed mathematical mixtures (what I call Birnbaumization), and hints at replacing WCP with another principle (Irrelevant Censoring). Then there is a gap until around 1968 at which point Birnbaum declares the SLP plausible "only in the simplest case, where the parameter space has but two" predesignated points (1968, 301). He tells us in Birnbaum (1970a, 1033) that he has pursued the matter thoroughly leading to "rejection of both the likelihood concept and various proposed formalizations of prior information". The basis for this shift is that the SLP permits interpretations that "can be seriously misleading with high probability" (1968, 301). He puts forward the "confidence concept" (Conf) which takes from the Neyman-Pearson (N-P) approach "techniques for systematically appraising and bounding the probabilities (under respective hypotheses) of seriously misleading interpretations of data" while supplying it an evidential interpretation (1970a, 1033). Given the many different associations with "confidence," I use (Conf) in this Rejoinder to refer to Birnbaum's idea. Many of the ingenious examples of the incompatibilities of SLP and (Conf) are traceable back to Birnbaum, optional stopping being just one (see Birnbaum 1969). A bibliography of Birnbaum's work is Giere 1977. Before his untimely death (at 53), Birnbaum denies the SLP even counts as a principle of evidence (in Birnbaum 1977). He thought it anomalous that (Conf) lacked an explicit evidential interpretation even though, at an intuitive level, he saw it as the "one rock in a shifting scene" in statistical thinking and practice (Birnbaum 1970, 1033). I return to this in part IV of this rejoinder.

II. Bjornstad, Dawid, and Evans

Let me begin by answering the central criticisms that, if correct, would be obstacles to what I purport to have shown in my paper. It is entirely understandable that leading voices in a long-lived controversy would assume that all of the twists and turns, avenues and roadways, have already been visited, and that no new flaw in the argument could enter to shake up the debate. I say to the reader that the surest sign that the issue is unsettled is that my critics disagree among themselves about the

puzzle and even the key principles under discussion: the WCP, and in one case, the SLP itself. To remind us [Section 2.2]:

SLP: For any two experiments E_1 and E_2 with different probability models f_1, f_2 but with the same unknown parameter θ , if outcomes $\mathbf{x}^*$ and $\mathbf{y}^*$ (from E_1 and E_2 respectively) determine the same likelihood function ($f_1(\mathbf{x}^*; \theta) = cf_2(\mathbf{y}^*; \theta)$ for all θ), then $\mathbf{x}^*$ and $\mathbf{y}^*$ should be inferentially equivalent for any inference concerning parameter θ .

A shorthand for the entire antecedent is that $(E_1, \mathbf{x}^*)$ is an *SLP pair* with $(E_2, \mathbf{y}^*)$, or just $\mathbf{x}^*$ and $\mathbf{y}^*$ form an *SLP pair* (from $\{E_1, E_2\}$). Assuming all the SLP stipulations, we have:

SLP: If $(E_1, \mathbf{x}^*)$ and $(E_2, \mathbf{y}^*)$ form an SLP pair, then $\text{Infr}_{E_1}[\mathbf{x}^*] = \text{Infr}_{E_2}[\mathbf{y}^*]$.

Bjornstad

According to Bjornstad: "The starting point is *not* that we have an arbitrary outcome of one single experiment, but rather that two experiments have been performed about the same parameter resulting in proportional likelihoods." I do not think Bjornstad can actually mean to say the SLP cannot be applied until both members of the SLP pair are observed. So, for example, if in the sequential experiment one is able to stop (with a .05 p-value) at $n = 169$, resulting in $\mathbf{y}^*$, one may not regard it as evidentially equivalent to $\mathbf{x}^*$, the SLP pair with n fixed at 169, until and unless $\mathbf{x}^*$ is actually generated? The universal generalization of the SLP asserts that for an arbitrary $\mathbf{y}^*$, if it has an SLP pair $\mathbf{x}^*$, then $\mathbf{y}^*$ is equivalent in evidence to $\mathbf{x}^*$.

Bjornstad's problematic reading results in his next remark: "So Birnbaum does not enlarge a known single experiment but constructs a mixture of the two performed experiments". What is constructed in Birnbaum's experiment E_B is a *hypothetical or mathematical mixture*, based on having observed $\mathbf{y}^*$ (from E_2). This is part of the key gambit I call *Birnbaumization* (Section 2.4). We are to consider the possibility that performing E_2 (which gave rise to $\mathbf{y}^*$) was the result of a θ -irrelevant randomizer (deciding between E_1 or E_2). Now I grant Birnbaum that we may imagine all the SLP pairs are "out there," each pair assumed to have resulted from a θ -irrelevant randomizer, ripe for plucking whenever a member of an SLP pair is observed. (See Sections 2.5, and 5.1). Yet even granting Birnbaum all of this, we still may not infer SLP (nor does it follow in the case where the mixture is actual.)

Bjornstad also criticizes me because he claims the SLP "is simply *not about method evaluation*." His position is that there is an evidential appraisal, and quite separately an assessment of long-run performance. For a frequentist, or one who holds Birnbaum's (Conf), evidential import is inseparable from an assessment of the relevant error probabilities. Not because we regard evidential import as all about long-runs, but because scrutinizing a given inference is bound up with a method's ability to have alerted us to misleading interpretations.

Bjornstad does "not find any *new* clarification of Birnbaum's fundamental theorem in this paper" because he assumes I am channeling the attempts of Durbin (1970), Kalbfleisch (1975) and Evans, Fraser, and Monet (1986), all of whom restrict or modify either SP or WCP to block the result. While I stand on the shoulders of these and other earlier treatments, a crucial difference is that unlike them, I do not alter the principles involved. If one is out to demonstrate the logical

flaw in an argument, as every good philosopher knows, one should scrupulously adhere to the premises, and generously interpret the machinations of the arguer. This I do. Bjornstad's opinion is that "one may regard the paper by Mayo as actually not discussing the LP at all." Or, alternatively, one may regard the position held by this critic to be mistaken about the SLP and Birnbaum's argument.

Dawid

Professors Dawid and Evans disagree about the key principle invoked in Birnbaum's argument, the WCP. Dawid views it as an equivalence relation, Evans says it is not. I follow Birnbaum in regarding the WCP as an equivalence, but, unlike both Dawid and Evans, I pin down what is to be meant in regarding WCP as an equivalence, or, for that matter, an inequivalence (see Section 4.3). First Dawid.

Dawid maintains that my WCP differs from the principle of conditionality Birnbaum uses in the SLP argument. Not so. I am working with the (WCP) stated in Birnbaum (1962, 1969): the very same one defined by Dawid:

The evidential meaning of any outcome of any mixture experiment is the same as that of the corresponding outcome of the corresponding component experiment, ignoring the over-all structure of the mixture experiment.

Dawid's definition is a portion of the one found in Birnbaum 1962 (271). It assumes, of course, all of the other stipulations, e.g., we are making "informative" inferences about θ , it is a θ -irrelevant mixture, the outcome is given, and all the rest. It is the definition used in countless variations of the SLP argument, and it is clearly captured in my 4.3. Perhaps I should have abbreviated it as CP; WCP comes from Berger and Wolpert's (1988) manifesto, *The Likelihood Principle*. My intention was to underscore Birnbaum's emphasis that the WCP concerns mixture experiments, and is distinct from many other uses of "conditioning" in statistics (Birnbaum 1962, 282-3).

I wondered why Dawid thought I denied that WCP asserts an equivalence, until I noticed that Dawid lops off the end of my sentence from Section 7: I do not say "the problem stems from mistaking WCP as the equivalence" simpliciter, but rather it stems from the incorrect equivalence! The incorrect equivalence equates the inference from the given experiment with one that takes account of the (irrelevant) mixture structure. This is what Dawid is on about in describing invalid construals of WCP, so he can scarcely object. (See Section 5.2.)

As with any equivalence, there is an implicit inequivalence as a corollary [See 4.3 (i) and (ii)]. Typically, in saying the evidential import of two outcomes are the same, one would not add "and be sure to ignore any features that would render them inequivalent". Birnbaum adds this warning because some treatments do not ignore the mixture structure. To put this another way, WCP includes the phrases "is the same as" as well as "ignoring". The problem is that Dawid is ignoring the word "ignoring" in the very definition he proffers. There is no difference between the phrases

- ignore the over-all structure of the mixture experiment

and

- eschew any construal that does not ignore the over-all structure of the mixture experiment.

I also refer to this as irrelevance (Irrel) (4.3.2) because Birnbaum describes the WCP as asserting the “irrelevance of (component) experiments not actually performed.” (1962, 271).

Dawid opines that I am using the WCP in David Cox’s (1958) famous weighing example, which he does not define; I am guessing he means to suggest I must be limiting myself to actual mixtures. That is to miss the genius of Birnbaum’s argument. Birnbaum, quite deliberately, intends to capitalize on the persuasiveness of conditioning in Cox’s famous example, but his ploy is to extend the argument to mathematical or hypothetical mixtures. (I am not saying it is an innocuous move, but that is a separate matter.) Even if Dawid chooses to view Cox’s WCP as a non-equivalence, it is irrelevant; I am following Birnbaum in construing it as an equivalence, permitting, for example, y^* , known to have come from a non-mixture, to be evidentially equivalent to the appropriate θ -irrelevant mixture as in 4.3. (Irrel) protects against illicit readings that Dawid warns against. SLP still will not follow.

So Dawid, Birnbaum, and I are using the same definition of WCP. The onus is on Dawid to pinpoint where my characterization deviates from Birnbaum’s. The only difference is that I have shown one cannot get to SLP, and Dawid gives no clue how to get around my criticism. Curiously, Dawid maintains that if we restrict WCP to actual mixtures, then he agrees with me that SLP does not follow. Why? (He should test his intuitions with the example of Section 5.4).

For Dawid to simply pronounce that “Birnbaum’s theorem is indeed logically sound” and that therefore my argument “must itself be unsound”, is question-begging, and will not do. Demonstrating unsoundness of my argument should be accomplished straightforwardly, as I have done regarding Birnbaum. That said, I fully agree with Dawid that one can view [(SP and WCP), entails SLP] as a theorem, but in order to *detach* the SLP, as is mandatory for Birnbaum, he is left with a “proof” that is either unsound or question-begging. Perhaps those who are long wedded to Birnbaum’s argument are comfortable with merely assuming what was to have been shown. It is part of the mysterious “path of enlightenment followed by conversion” that Dawid mentions. That is no reason for others to allow “trust me, it is sound” to take the place of argument.

Evans

Given that Evans largely agrees with me, it may seem ungenerous to focus on apparent disagreements, but there is too great a danger in leaving some misimpressions regarding a problem already beset with decades of misunderstanding. Notably, it seems I have not convinced Evans of the logical error that Birnbaum makes. Instead Evans thinks the problem is with the conditionality

principle WCP, and claims that frequentists need to fix it somehow. But it is not the principle, it is the “proof”.

I have at least convinced Evans that there are cases where SP and SLP and not-SLP hold without logical contradiction (in Mayo, 2010). These cases may be called “counterexamples” to the argument whose conclusion is the SLP. They are also counterexamples to [WCP entails SLP], using the weaker notion of mathematical equivalence of Birnbaum (1972) that dispenses with (SP). Evans will take those counterexamples to show that WCP is not an equivalence relation, assuming a frequentist standpoint. Now it is true that any such counterexample may be seen to warn us against mistaking WCP as asserting the incorrect equivalence, noted in my rejoinder to Dawid. But that does not preclude WCP from asserting a correct equivalence. A more general issue I have with Evans’ treatment is that it does not show where the source of the problem lies in arguments for SLP.

Introducing his set-theoretic treatment into a simple argument, I am afraid, does not help to pinpoint where the argument goes wrong, but in fact leaves us with a very murky idea even as to his definition of WCP. The argument for the SLP begins with: We are given y^* from E_2 , a member of an SLP pair. Will Evans block introduction of the mathematical mixture in Birnbaumization? This would seem to cut off Birnbaum’s argument too quickly. Were that sufficient, the debate would have surely ended with Kalbfleisch (1975). Note too, unlike Evans, my argument in the paper under discussion does not rely on assuming a frequentist principle at all, though obviously I avoid a formulation that rules it out in advance. To sum up this part, Evans uses my counterexamples to show a restricted WCP may be applied, while blocking SLP. Left as it is, it opens him to the criticism (the one Dawid raised!) that he is altering Birnbaum’s WCP, and restricting it to actual mixtures.

What a surprise, then, to hear Evans allege that “many authors, including Mayo, refer to the [WCP] which restricts attention to ancillaries that are physically part of the sampling”. I do not know on what grounds Evans wants to distinguish actual and mathematical mixtures, but Birnbaum’s argument for the SLP concerns mathematical or hypothetical mixtures. Birnbaum calls an experiment a mixture “if it is mathematically equivalent” to a mixture (1962, p. 279). Further, Birnbaum (1962) emphasizes that earlier proofs [that WCP and SP imply SLP] were restricted to actual mixtures. “But in the above proof” he is able to get a result relevant for all classes of experiments, by using an ancillary “constructed with the hypothetical mixture” [E_B] (1962, 286). So, I am not sure what Evans is alleging. In one place, Evans worries whether the WCP “resolves the problem with conditionality more generally”, but this is a separate issue from Birnbaum’s argument. Here the focus is on WCP solely for purposes of arriving at the SLP.

Although there was not space to discuss this in my paper, it is worth noting why merely blocking the SLP with a modified WCP fails to make progress with a further goal required of an adequate treatment. Consider how, in discussing Durbin’s modified principle of conditionality, Birnbaum notes that “Durbin’s formulation (C'), although weaker than [WCP], is nevertheless too strong (implies too much of the content of [SLP]) to be compatible with standard (non-Bayesian)

statistical concepts and techniques" (1970b, 402). Birnbaum (1975, 264) raises the same problem with Kalbfleish's restriction to "minimal experiments" to which Evans' treatment is closely related. Evans does not show his modified conditionality principle avoids entailing "too much of the SLP". (This relates to Dawid's point about stopping rules in his comment.) For a frequentist account to satisfy Birnbaum's (Conf), all cases corresponding to misleading interpretations with high probability should still show up as SLP violations.

To this end, my argument shows that any violation of SLP in frequentist sampling theory necessarily results in an illicit substitution in the formulation of Birnbaum's argument. To put the problem in general terms, $p = r$ does not follow from $p = q$ and $q = r$, if q shifts to q' within the argument, where $q \neq q'$ (fallacy of 4 terms). For specifics see Section 5. Thus, ours is in no danger of implying "too much" of the SLP: what was an SLP violation remains one. Now Evans may not be concerned with retaining those frequentist SLP violations, given he makes it very clear he embraces Bayesianism, but that is irrelevant to what an adequate treatment of Birnbaum's argument demands. I have seen some statistics textbooks leave the details of the SLP proof to the reader; I think it is time to give full credit to students who found it impossible to make the valid substitution in general. I explained why.

III. Fraser, Hannig, Martin and Liu

Let me turn to the second group of discussants. It is an honor to be "strongly commended" by Fraser for emphasizing the importance of "principles and arguments of statistical inference"; and I feel my efforts are worthwhile with Martin and Liu's noting my "demonstration resolves the controversy around Birnbaum and LP, helping to put the statisticians' house in order". I entirely agree with them that the "confusion surrounding Birnbaum's claim has perhaps discouraged researchers from considering questions about the foundations of statistics", at least, from appealing to those foundations that reject the SLP. Let me underscore Fraser's point that the need for an inferential variation of (N-P) theory "reached the mathematical statistics community rather forcefully with Cox (1958); this had the focus on the two measuring instruments example and on uses of conditioning that were compelling". Cox's (1958) example also appears in Hannig's discussion, and I will borrow his simple description of the case where the measurement but not the instrument M is observed. In that case, inference is based on the convex combination of the mixture components, consistent with WCP. This allows me to succinctly put an equivocation that I suspect may enter, in the case of SLP pairs, between the irrelevance of the mixture structure, given (E_i, z_i) , and the irrelevance of the index i , given just the measurement. This equivocation may be behind the puzzle.

Fraser rightly reminds us that, "statistical inference as an alternative to (N-P) decision has a long history in statistical thinking" with strong impetus from Fisher. Still Birnbaum struggled to articulate a N-P theory as "an inference theory" (Birnbaum 1977), and my view is that we had to solve "Birnbaum's problem" before doing so properly. Finding Birnbaum's argument unsound opens the door to

foundations that are free from paying obeisance to the SLP. In this spirit Martin and Liu correctly view my paper as “an invitation for a fresh discussion on the foundations of statistical inference”. Yet there is more than one way of proceeding. Tracing out the mathematical similarities and differences between the approaches of Fraser, Hanning, Martin and Liu is a task for which others are better equipped than I. All are said to violate SLP.

It is interesting to note, as Hannig does, that “since the mid 2000s, there has been a true resurrection of interest in modern modifications of fiducial inference” which had long fallen into disrepute. Fraser’s has been one of the leading voices to persevere with innovative developments, and his own “confidence” idea is clearly in sync with Birnbaum. However, the differences that emerge in this group’s discussions should not be downplayed. Hannig says that “the common thread for these approaches is a definition of inferentially meaningful probability statements about subsets of the parameter space without the need for subjective prior information”, and Martin and Liu suggest that error probability accounts are appropriate only for decision procedures, as distinct from their “inferential models”. Some might view these attempts as building a concept of evidence as a kind of *probabilism* but without the priors. However, lurking in the background of these contemporary developments was a suspicion that their SLP violations were picking up differences where no purely inferential difference was warranted. So long as Birnbaum’s proof stood, this suspicion made sense.

Post-SLP, it is worth standing back and reflecting anew on these accounts. In this respect, this foundational project is just beginning because for 40 or 50 years, the questions of foundations were largely restricted to accounts that obeyed, or were close to obeying, the SLP. So, we have Birnbaum, along side Fisher, being catapulted onto the contemporary foundational scene, squarely calling on us to address the still unresolved problem: how to obtain an account of statistical inference that also controls the probability of seriously misleading inferences. Better yet, the two goals should mesh into one.

IV Post-SLP foundations

Return to where we left off in the opening section of this rejoinder: Birnbaum (1969).

The problem-area of main concern here may be described 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*. (Birnbaum 1969, 113)

Given Neyman’s behavioral decision construal, Birnbaum claims that “when a confidence region estimate is interpreted as statistical evidence about a parameter” (1969, p. 122), an investigator has necessarily adjoined a concept of evidence, (Conf) that goes beyond the formal theory. What is this evidential concept? The furthest Birnbaum gets in defining (Conf) is in his posthumous article (1977):

(Conf) A concept of statistical evidence is not plausible unless it finds 'strong evidence for H_2 against H_1 ' with small probability (α) when H_1 is true, and with much larger probability ($1 - \beta$) when H_2 is true. (1977, 24)

On the basis of (Conf), Birnbaum reinterprets statistical outputs from N-P theory as strong, weak, or worthless statistical evidence depending on the error probabilities of the test (1977, 24-26) While this sketchy idea requires extensions in many ways (e.g., beyond pre-data error probabilities, and beyond the two hypothesis setting), the spirit of (Conf), that error probabilities qualify properties of methods which in turn indicate the warrant to accord a given inference, is, I think, a valuable shift of perspective. This is not the place to elaborate, except to note that my own twist on Birnbaum's general idea is to appraise evidential warrant by considering the capabilities of tests to have detected erroneous interpretations, a concept I call *severity*. That Birnbaum preferred a propensity interpretation of error probabilities is not essential. What matters is their role in picking up how features of experimental design and modeling alter a methods' capabilities to control "seriously misleading interpretations". Even those who embrace a version of probabilism may find a distinct role for a severity concept. Recall that Fisher always criticized the presupposition that a single use of mathematical probability must be competent for qualifying inference in all logical situations (1956, 47).

Birnbaum's philosophy evolved from seeking concepts of evidence in degree of support, belief, or plausibility between statements of data and hypotheses to embracing (Conf) with the required control of misleading interpretations of data. The former view reflected the logical empiricist assumption that there exist context-free evidential relationships—a paradigm philosophers of statistics have been slow to throw off. The newer (post-positivist) movements in philosophy and history of science were just appearing in the 1970s. Birnbaum was ahead of his time in calling for a philosophy of science relevant to statistical practice; it is now long overdue!

"Relevant clarifications of the nature and roles of statistical evidence in scientific research may well be achieved by bringing to bear in systematic concert the scholarly methods of statisticians, philosophers and historians of science, and substantive scientists" (Birnbaum 1972, 861).

I agree with him.

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