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Rejoinder

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1. INTRODUCTION

Very sincere thanks to the discussants for choosing to enter a virtual minefield of disagreement in the development history of statistics. For we just need to recall the remark that fiducial is Fisher's "biggest blunder" and place it alongside the fact that fiducial was the initial step toward confidence, which arguably is the most substantive ingredient in modern model-based theory: the two differ in minor developmental detail, with fiducial offering a probability distribution as does Bayes and with confidence offering just probabilities for intervals and special regions. Statistics has spent far more time attacking incremental steps than it has seeking insightful resolutions.

As a modern discipline statistics has inherited two prominent approaches to the analysis of models with data; of course such is not all of statistics but is a critical portion that influences the discipline widely. How can a discipline, central to science and to critical thinking, have two methodologies, two logics, two approaches that frequently give substantially different answers to the same problems. Any astute person from outside would say, "Why don't they put their house in order?" And any serious mathematician would surely ask how you could use a lemma with one premise missing by making up an ingredient and thinking that the conclusions of the lemma were still available. Of course, the two approaches have been around since 1763 and 1930 with regular disagreement and yet no sense of urgency to clarify the conflicts. And now even a tired discipline can just ask, "Who wants to face those old questions?": a fully understandable reaction! But is complacency in the face of contradiction acceptable for a central discipline of science?

A statistical model differs from a deterministic model in having added probability structure that describes the variability typically present in most applications. So, in an application with a statistical model and related data it would then seem quite natural that that variability would enter the conclusions concerning the unknowns in an application: what do I know deterministically, and what do I know probabilistically?

And that is what Bayes proposed in 1763: probability statements concerning the unknowns of an investigation. Many have had doubts and said there was no merit in the proposal; and many have acceded and became strong believers. And then Fisher (1930) also offered probabilities concerning the unknowns of an investigation, but by a different argument, and the turf fight began! Bayes had hesitantly examined a special problem and *added* a random generator for the unknown parameter, and Fisher had worked more generally and used just the randomness that had generated the data itself.

But then a third person, Lindley (1958), from the same country said that the second person, Fisher, couldn't use the term probability for the unknowns in an investigation, as the term was already taken by the first person, Bayes. And strangely the discipline complied! Decades went by and anecdotes were traded and things were often vitriolic.

2. WHAT DOES THE ORACLE SAY?

Consider some regular statistical model $f(y;\theta)$, together with a lower β-confidence bound $\hat{\theta}_\beta(y)$, and also a lower β-posterior bound $\tilde{\theta}(y)$ based on a prior $\pi(\theta)$: What does the oracle see concerning the usage of these bounds? He can investigate any long sequence of usages of the model, and He would have available the data values y_i and of course the preceding parameter values θ_i that produced the y_i values; He would thus have access to $\{(\theta_i, y_i): i = 1, 2, \ldots\}$.

First consider the lower confidence bound. The oracle knows whether or not the θ_i is in the confidence interval $(\hat{\theta}_\beta(y_i), \infty)$, and He can examine the long-run proportion of true statements among the assertions that θ_i is in the confidence interval $(\hat{\theta}_\beta(y_i), \infty)$, and He can see whether the confidence claim of a β-proportion true is correct. In agreement with the mathematics of confidence, that proportion is just β.

Now consider the lower posterior bound. The oracle knows whether θ_i is in the posterior interval $(\tilde{\theta}_\beta(y_i), \infty)$, and He can examine the long-run proportion of true statements that θ_i is in the posterior

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interval $(\tilde{\theta}_\beta(y_i), \infty)$. Now suppose the long-run pattern of θ_i values just happened to correspond to the pattern $\pi(\theta)$; then in full agreement with the mathematics of the Bayes calculation, the Oracle would see that long-run proportion of true statements among assertions that θ_i is in the posterior interval $(\tilde{\theta}_\beta(y_i), \infty)$ was correct, was just the stated β.

But what if the long-run pattern of θ_i values was different from the introduced $\pi(\theta)$ pattern? Then in wide generality the long-run proportion of true statements among assertions that θ_i is in the posterior interval $(\tilde{\theta}_\beta(y_i), \infty)$ would *not* be β! In other words, the confidence procedure is always right, and the Bayes procedure is typically *wrong*, unless the prior was guessed correctly. Seems like a poor trade-off!

Now consider further what a prior actually does in producing parameter bounds or quantiles that are different from the confidence bound. From an asymptotic viewpoint a prior can be expanded as $\exp(a\theta/n^{1/2} + c\theta^2/n)$ to the third order, as mentioned but not pursued in Section 6(iv). This provides a direct displacement of the confidence bound in standardized units and produces an $O(1)$-shift away from the claimed β value, either up or down depending on the sign of a! Hardly an argument for using the Bayes procedure unless there was some very urgent need for a quick and dirty calculation.

3. RESPONSE TO THE DISCUSSANTS

Christian Robert

Christian presents a very committed Bayes viewpoint and quite correctly admonishes me for not distinguishing what Thomas Bayes did and what has followed in the same theme. But going beyond the minor detail, Bayes *added* a distribution for a parameter, a distribution that was not part of the binomial example under consideration and then used that distribution for probability analysis. And much of modern Bayesian statistics does precisely that: introduces an artifact distribution for expediency or convenience and then works reassuringly within accepted probability calculus. Indeed, this is the primary theme of the article: adding something arbitrary gives something arbitrary no matter how attractive the material labeled probability might or might not be, or no matter what might be available by other methods of analysis. If one faces a probability-type claim, it is fair enough to simulate and evaluate the claim, and that is what coverage probability is all about, as the invincible Oracle well knows.

The marginalization paradoxes do appear in the literature but are widely neglected and not "extensively discussed" as Christian suggests. They apply to any proposal for a distribution to describe an unknown vector parameter, whether obtained by the Bayes inversion of a density or the frequentist inversion of a pivot, such as fiducial, confidence structural or other. There is an immutable contradiction built into the hope to describe a vector parameter by a distribution. Curvature of an interest parameter has emerged as the critical source for this contradiction. Take a bivariate parameter, a data point and an interest parameter value: if the parameter is linear, the confidence and the Bayes values are equal; if then parameter curvature is introduced, we have that the confidence value and the Bayes value change in *opposite* directions! One has the coverage property and the "other" acquires bias at twice the rate of the departure from linearity. And the "other" uses the name probability with an assertiveness coming from the use of the probability calculus, conveniently overlooking that an artifact was introduced in place of the input needed for the validity of the probability calculus for the application.

Maybe it is time to address the Pandora's box and check for a Madoff pyramid: too good to be true.

Larry Wasserman

Larry presents a pragmatic view of the Bayes approach, acknowledging its rich flexibility but recommending coverage cautions. His five examples are most welcome concerning the wider spheres of application and he is to be complemented on the skillful innovations. I do quarrel, however, with his reinforcement of personality cults in statistics. It seems that statistics has suffered greatly from this externalization of the scientific method, as if there were different flavors of scientific thinking and mathematical logic and that these might gain concreteness when personalized.

Kesar Singh and Minge Xie

Confidence for estimation and exploration? It is deeply unfortunate that statistics chooses at many steps to malign its major innovators, for example, Fisher with his "biggest blunder" as a referent for the initiative that gave us confidence. What Fisher didn't do was present his major innovations in a fully packaged form ready to withstand a few centuries of challenges and modification: What? We still have to do a little bit of thinking! Tough! He clearly must generously have expected others to have his insight and wisdom!

Fiducial, confidence, structural or other? It is just pivot inversion with variation in context, conditions or interpretation: the big risk was described by Dawid, Stone and Zidek (1973) and curvature is now identified as the prime cause. To have different names to fine tune for different applications or different explorations would seem to take emphasis away from the proper calibration of the tool, as the primary concern for most applications.

Statistics routinely combines likelihoods as appropriate, so it is not correct to attribute this to Bayesian learning; perhaps the central sectors of statistics were just slow to glamorize the good things in their statistical modeling. Putting a prior on a likelihood is a different operation downstream from assembling the likelihood in the relevant broader context, although it does seem convenient for the Bayes approach to co-opt it as their own contribution when it was somewhat neglected by the "others."

Tong Zhang

Where does the pivot come from? Fisher's development of confidence or whatever attracted the mathematicians' criticisms, mostly because it wasn't proposed in a fully developed form. It was then shredded, fully ignoring the emerging recognition of its innovative genius. Certainly the need to clarify the origin of the key ingredient, "the pivot," is of fundamental importance, as Tong suggests: using all the data in an appropriately balanced way, respecting continuity and parameter direction from data, and more. Whether these should be bundled under a term optimality may be questionable, but doesn't diminish the importance of the individual criteria; for some recent emphasis on continuity see Fraser, Fraser and Staicu (2010).

4. SOME CONCLUDING INVOCATIVE REMARKS

An inference distribution for a vector parameter is inherently a contradiction. Information from two different sources can be reported separately, with combination *not* by principle. Combining likelihoods is a consequence of combining models, typically following from independence; the Bayes claim that it comes from the use of the Bayes argument is after the fact and disingenuous. Inverting a density and inverting a pivot are different except in the linear case, but the first can sometimes approximate the second.

The question was asked: "Is Bayes posterior just quick and dirty confidence?" And the case was made for "Yes": Bayes posterior is just quick and dirty confidence: quick in the sense of easier than using quantiles to determine how θ affects data; and approximate in the sense of a wide spread need to use approximation methods.

Not everyone liked the blunt question. One discussion expresses discomfort with such a direct confrontation to the Bayes approach; one discussion adds additional support examples; and the two remaining discussions speak more to methods and modifications of confidence distributions, overlooking the risks. But no one argued that the use of the conditional probability lemma with an imaginary input had powers beyond confidence, supernatural powers.

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