Comment on “Bayesian Analysis of Pentaquark Signals from CLAS Data”, with Response to the Reply by Ireland and Protopopsecu

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The CLAS Collaboration has published an analysis using Bayesian model selection. My Comment criticizing their use of arbitrary prior probability density functions, and a Reply by D.G. Ireland and D. Protopopsecu, have now been published as well. This paper responds to the Reply and discusses the issues in more detail, with particular emphasis on the problems of priors in Bayesian model selection.

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I. INTRODUCTION

The CLAS Collaboration [1], citing the pioneering work of Harold Jeffreys [2], has published a Letter [1] that claims to illustrate a general method that “could be applied to any data set where a search for a new state has been carried out”, providing a “quantitative measure” for judging potential discovery results by using the formalism of Bayesian model selection. My Comment [3] criticizing their use of arbitrary prior probability density functions, and a Reply [4] by D.G. Ireland and D. Protopopsecu, have now been published as well. As I believe that the Reply did not satisfactorily address the points raised by my Comment, and that it was not informed by other points in the articles I cited, I elaborate in this post to the arxiv.

All Bayesian calculations, and in particular model selection results, are potentially sensitive to the choice of prior pdf. My short Comment [3], reproduced in Sec. II, focused on the statement in the Letter [1]: “We assume that each prior is a uniform distribution between a lower and upper limit since this represents the least initial bias.” This statement goes against the entire thrust of Jeffreys’ book and subsequent research: Jeffreys explains in convincing detail the contradictions one reaches by such use of Laplace’s idea of a uniform prior. Of course, a lot has been learned in the nearly half century since Jeffreys’ last edition appeared in 1961, and my Comment included references to some more recent key papers and discussion, notably by José Bernardo and Jim Berger and collaborators, and the review by Kass and Wasserman.

My Comment focused on problems in prior specification which are present already in Bayesian parameter estimation. In Bayesian Model Selection, there is an additional major concern arising from computing a ratio of two integrals that are evaluated in parameter spaces of different dimensionality. In the CLAS Collaboration’s Letter [1], one has a four-dimensional prior in the denominator, while the prior in the numerator has the same four dimensions plus three additional ones. Specification of the prior in these extra dimensions without arbitrarily affecting the answer is a difficult problem which is not addressed by the uniform priors described in the Letter.

To spell out these objections further, in Sec. III I respond to some specific statements in the Reply [4]. Sec. IV explains why the evidence ratio in the Letter has a steep dependence on the arbitrarily-chosen upper and lower limits referred to in the above quote. Indeed the method advocated in the Letter, in which “The prior parameter ranges were established by performing an initial fit and setting the limits to be ±50% of the values found” [1], is of a type cautioned against in the Bayesian literature. I conclude in Sec. V.

II. MY COMMENT AS PUBLISHED IN PRL [3]

The CLAS collaboration, in presenting a Bayesian analysis of two searches for pentaquarks [1], suggests “an alternative means of quantifying the evidence for discovery. What is specifically required is a quantitative comparison between the two hypotheses…”, and concludes, “More generally, this method could be applied to any data set where a search for a new state has been carried out, and can provide a quantitative measure with which to judge whether or not a result represents a discovery.” Especially given the repeated emphasis on “quantitative”, one must challenge
III. Response to the Reply to my Comment

I do not believe that the Reply [4] by Ireland and Protopopsecu adequately addresses my criticisms with respect to the priors. There is also no study presented of the frequentist sampling properties of their result. Thus the scientific conclusions one obtains by following the general method as presented in the Letter [1] are without proper foundation, and cannot be interpreted in the “quantitative” fashion claimed in the Letter.

Below are some italicized quotes from the published Reply and my further comments.

“The choice of a Gaussian function to represent a peak is a standard one, since the three parameters required to specify it are related to quantities with physically meaningful interpretations: centroid position, detector resolution and signal strength. The limits on the possible value of centroid position correspond to the mass range for which the experiment is sensitive, so it is a location parameter for which a uniform prior is a reasonable choice.”

In this (August 2009) version of this note, I modify my comment on this point. My earlier comment contained the following two paragraphs:

“The fact that a parameterization is a standard representation in physics formulas has nothing to do with whether or not the are the metrics in which the prior should be flat. The claim that mass is a location parameter for which a uniform prior is reasonable is false. The prototypical example that Jeffreys used to show the non-universal applicability of uniform priors is in fact a physical parameter with a semi-infinite range of possible values: the charge of the electron (pp. 104-105 in the 2nd edition, pp. 119-120 in the third edition). His analysis applies equally to mass. Those pages are in the part of the book on estimation, and as noted in my Comment, one can be led to different priors in model selection; but in neither case is mass a location parameter with uniform prior.”

I now think the issue is not as clear-cut as I portrayed it, for very interesting reasons. The above argument by Jeffreys is an argument based on considerations of the physical parameter itself. This is in contrast to the argument (in the same book) leading to Jeffreys’s Rule for priors that are known as “Jeffreys priors”, and that are generalized to
the Reference Priors of Bernardo and collaborators. Jeffrey’s Rule for priors is completely based on the measurement process, i.e., the specific experimental setup one uses in order to “make the measurement”; this is what statisticians typically call “the model”. This is distinct from considerations (such as the scaling argument for the electron charge) about the physical parameter itself! That is why, for instance, the Jeffreys prior for the binomial parameter is different in the binomial model than in negative binomial model, in violation of the strong likelihood principle, even if it is the same physical parameter. So when using Jeffrey’s Rule, it is indeed the case that simply measuring a parameter with a Gaussian resolution function implies a uniform prior, independent of one’s notions about the physical parameter itself!

As to the issue of a restriction on allowed values of the parameter, Berger [17] addresses exactly this point:

“One important feature of the Jeffreys noninformative prior is that it is not affected by a restriction on the parameter space. Thus, if it is known in Example 5 [in which $\theta$ is a location parameter] that $\theta > 0$, the Jeffreys noninformative prior is still $\pi(\theta) = 1$ (on $(0, \infty)$, of course). This is important, because one of the situations in which noninformative priors prove to be extremely useful is when dealing with restricted parameter spaces (see Chapter 4). In such situations we will, therefore, simply assume that the uninformative prior is that which is inherited from the unrestricted parameter space.”

My above-quoted comments on this point were thus wrong from the point of view of objective Bayesianism; the scaling argument of Jeffreys which I quoted is eclipsed by Jeffreys’s Rule. On the one hand this might seem to be a compelling argument against such “objective” priors from Jeffreys’s Rule, but on the other hand it is worth recalling arguments in favor of such objective priors, namely that they “let the data speak the loudest”, and that since the time of Welch and Peers they are associated with probability matching to a certain order.

This discussion seems to make quite clear how so-called objective priors are different from subjective priors. Subjective priors encode an individual’s prior beliefs about the parameter, while objective priors encode properties of the measuring apparatus (including the stopping rule, in violation of the likelihood principle). The arguments I quoted advocating a sensitivity analysis would seem to take on even greater force in these circumstances!

“The width and height of the Gaussian are, strictly speaking, scale parameters for which a Jeffreys’ prior may be appropriate. However, for the calculation of evidence integrals, we require normalized priors, so a Jeffreys’ prior $[f(x) \propto 1/x]$ needs to be normalized between two limits. Limits on the width of the peak are naturally suggested: the minimum by detector resolution and a maximum such that a peak is not confused with a background shape.”

The uniform prior is also unnormalizable over all space. The authors do not explain why they chose a truncated uniform prior rather than a truncated version of some other unnormalizable prior, if that was the concern. But the larger issue is that if one is in a situation where truncation is needed in order to make the prior normalizable, then the result of the model selection calculation will depend on the endpoints used for the truncation. I come back to this crucial point in Sec. IV below.

“For the signal parameter, we must include zero, since the posterior probability density function shows that, even in cases where the maximum likelihood is non-zero (i.e. a peak is most likely), there is still a significant probability that the results are consistent with zero signal. The Jeffreys’ prior is undefined at $x = 0$, so an alternative could be to use a gamma distribution, which is normalized, has a similar decay as $x \to \infty$, and is defined as $x \to 0$.

The Bayesian literature on model selection (starting with Jeffreys) has a lot of discussion about the situation, as we have here, where the crucial scientific question is whether or not a particular parameter is zero (i.e., corresponding to the classical case of a point null hypothesis). There are various ways to approach this, for example concentrating some prior in a delta function at the point, and spreading out the rest. (This is already in Jeffreys’ 2nd edition.) A more comprehensive look at the professional literature is needed to understand the subtleties which have been ignored here.

“We studied the problem with these alternative priors, and found no significant difference in the results obtained; the use of uniform priors was thus motivated by using the simplest form appropriate to the problem.”

Without a quantitative exposition or knowing the scope of the sensitivity analysis, one cannot evaluate the claim that their method provides a “quantitative” result. Were the limits of the mass range changed? What range of shapes of gamma functions was explored?

“In addition, the sensitivity of the results to the parameters of the background is minimal. The evidence ratios (or Bayes Factors) are in the form of (logarithms of) ratios of integrals. Any slowly varying prior in the space of the background parameters will thus result in approximately constant factors that cancel.”
Again, whether something is “minimal” or “approximately constant” is in the eye of the beholder, so the authors should provide the list of alternatives explored and the numerical results.

“In the calculations, the likelihood functions for each data model must be integrated over all parameter space. The effect of priors is to weight the likelihood functions. In practice, the likelihood functions for the study in the letter are significantly non-zero in only a small region of parameter space. It would thus take a rapidly varying prior in this region to make a noticeable difference to the integrals, and one would of course have to justify this rapidly varying prior.”

A rapidly varying prior in their chosen metric will be a uniform prior in another metric; what is the criterion for choosing the preferred metric? As I discuss below, the location of the endpoints of the uniform prior can affect the answer, even if the likelihood is zero near the endpoints.

With seven parameters, the authors may be surprised to find that the volume effect (most of the prior probability located near the boundary of their 7-D hypercube) distorts the posteriors.

We used the term "least bias" to indicate that, as far as possible, we wanted to see what information one could extract from the data, whilst introducing only minimal prior prejudice.

As shown by Jeffreys [2] and reviewed by Kass and Wasserman [10], using uniform priors in the manner of the Letter [1] does not satisfy this desire. This is also extensively discussed by Bernardo in the “dialogue” cited in my Comment [11].

How one achieves this is an open question, and it should be noted that the debate within the statistics community appears far from settled, as attested by the papers cited in the Comment [2,3], and subsequent contributions in the same publication.

This was the reason I wrote the Comment: The Letter’s statement that their uniform priors had the “least bias” was without foundation. It appears that the authors of the Reply now agree.

“To conclude, the use of alternative priors makes little difference to the results of our study. The measured data in this case therefore contain sufficient information to dominate the calculation of the probabilistic quantities of interest.”

The reader should be provided with details of the alternative priors and numerical results in order to judge what “little difference” means, since Sec. IV below indicates that this is not the case.

Actual claims of discovery would require a more detailed examination of evidence than presented in the Letter. We therefore fully agree with the author of the Comment that, in general, a sensitivity analysis of results to the choice of priors (and data models) is essential.”

The main scientific point of the Letter [1] was to claim that the first CLAS result was actually “inconclusive” (and if anything weak evidence against a peak), contradicting CLAS’s earlier published claim of $5.2\sigma$ “observation” of a peak. It would seem that such an extraordinary situation should be backed up with the sort of “detailed examination” that the Reply authors agree is required for a discovery. This should include the dependence on the limits (endpoints) of the prior as discussed in Sec. IV below.

IV. THE PROBLEM OF DIFFERING DIMENSIONS IN MODEL SELECTION AND THE DANGERS OF ARBITRARILY TRUNCATING THE PRIOR

A difficulty in the present model selection calculation is a common one in Bayesian analysis: Model A has four parameters, and Model B has the same four parameters, plus three more. If the priors for the three additional parameters are unnormalizable (e.g. a uniform prior extending to $\infty$), then the answer is completely arbitrary: multiplying the prior by a constant in the numerator but not the denominator (where it does not appear) will change the evidence ratio. The Reply alludes to this, and in the Letter the uniform prior was truncated at a location well outside the range where likelihood function is non-negligible. This truncation is perhaps plausible-sounding since such a truncation may be made without severe effect in estimation problems. However, the effect in model selection is unfortunately that which often occurs in physics when dealing with an infinity by introducing a cutoff: one simply replaces the infinity with a dependence on the cutoff, which is arbitrary.
The Letter set the endpoints to be ±50% of the best-fit values. (“The prior parameter ranges were established by performing an initial fit and setting the limits to be ±50% of the values found” [1].) The Reply [4] appears to contradict this statement from the Letter, by saying that zero was included in one interval; but as this statement in the Letter has not been explicitly retracted, we can take it as the generally applicable method advocated by the CLAS Collaboration. Then let $f$ denote this percentage, i.e. $f = 0.5$ in the Letter, and let $\hat{\xi}$ denote the best-fit value of a parameter $\xi$. Then the prior is:

$$P(\xi|M) = \begin{cases} 
1/(2f\hat{\xi}), & \hat{\xi} - f\hat{\xi} < \xi < \hat{\xi} + f\hat{\xi} \\
0, & \text{otherwise.} 
\end{cases}$$

(1)

Thus, even if the likelihood functions are all negligible near the endpoints of the uniform prior, after all integrations are performed the arbitrary factor $(1/f\hat{\xi})$ will appear in the numerator of the evidence ratio, and not be canceled be a factor in the denominator. *Thus the arbitrary constant of an unnormalizable uniform prior is just replaced by an arbitrary constant determining the height of the normalized uniform prior (via the arbitrary specification of the width and the normalization condition).*

As the Letter has one such factor $f$ for each of its three extra parameters in the numerator, the evidence ratio goes as $f^3$. That is, if $f$ is varied from 25% to 75%, the evidence ratio changes by a factor of 27, and its logarithm, $\ln(RE)$, changes by 3.3 units. This is enough to change the evidence by two categories of strength in Jeffrey’s scale!

The value of $f$ used for the mass parameter deserves special attention, since it controls the “Occam’s razor” effect due to where one is looking in mass: a firm prediction of the mass of the pentaquark, followed by a peak at that location, should result in an enhanced evidence ratio resulting from a small value $f$. It is not clear to what extent the analysis in the Letter in fact *reduced* the evidence ratio derived from the first data set by using an inflated value of $f$.

Given such strong dependence on an arbitrary parameter $f$, it is hard to comprehend the claim in the Reply that a sensitivity analysis was performed with “no significant difference in the results obtained”.

(The Letter was not clear as to whether or not the limits on the four parameters in the 3rd-order polynomial were the same in the numerator as in the denominator. If different “values found” by the fit led to different limits in the numerator and denominator, then this adds to the non-canceling dependence on $f$.)

This issue is of course known in the Bayesian literature that I cited in my Comment. For example, Berger and Pericchi [9] list “Difficulty 3. Use of vague proper priors usually give bad answers in Bayesian model selection”, with a specific example where the Bayes factor depends the arbitrarily chosen “large” value of $K$, which in their example sets the size of the region over which the prior is appreciable. They conclude, “The short story is never use vague priors for model selection...” (and in fact prefer an improper prior in that particular example). The limited-length uniform priors used in the Letter suffer from the same disease of the Bayes factor depending on the arbitrary $f$.

In his article in Bayesian Analysis cited by my Comment, Jim Berger [12] has a section entitled “Dangers of Casual Objective Bayesian Analysis” which is even more explicit in urging caution in use of “pseudo-Bayes” analyses: “...while they utilize Bayesian machinery, they do not carry with them any of the guarantees of good performance that come with either true subjective analysis (with a very extensive elicitation effort) or (well-studied) objective Bayesian analysis...and hence must be validated by some other route.” He specifically warns in a section entitled “Truncation of the parameter space” that truncation at large ±$K$ to avoid having an improper pdf must be done with care: “At the very least, this approach should only be used if a very careful sensitivity study is done with respect to these bounds (and with bounds for different parameters varying independently in the sensitivity study.” The context of these quotes is in terms of avoiding an improper posterior by truncating an improper prior, but the concern is exactly paralleled in truncation of the prior in the model selection problem of the Letter [1].

The last subsection in Berger’s section on “Dangers of Casual Objective Analysis” is worth quoting extensively: [12]:

**Data-dependent vague proper priors.** The second common data-dependent procedure is to choose priors that span the range of the likelihood function. For instance, one might choose a uniform prior over a range that includes most of the mass of the likelihood function, but that does not extend too far (thus hopefully avoiding the problem of using a ‘too vague’ proper prior). Another version of this procedure is to use conjugate priors, with parameters chosen so that the prior is spread out somewhat more than the likelihood function, but is roughly centered in the same region. The two obvious concerns with these strategies are that (i) the answer can still be quite sensitive to the spread of the rather arbitrarily chosen prior; and (ii) centering the prior on the likelihood is a quite problematic double use of the data. Also, in problems with complicated likelihoods, it can be very difficult to implement this strategy successfully... In conclusion, while these pseudo-Bayesian techniques can be useful as data exploration tools, they should not be confused with formal objective Bayesian analysis, which has very considerable extrinsic justification as a method of analysis.”
V. CONCLUSION

If we are going to use Bayesian techniques in our research, then we should read and understand a representative sampling of the relevant Bayesian literature. I urge anyone contemplating an objective Bayesian analysis to read Kass and Wasserman [10] before attempting to write down a so-called objective or noninformative prior in a desire to “represent the least initial bias”. If one wants to go beyond parameter/interval estimation and get into model selection, the article by Berger and Pericchi [9], also cited in my Comment, is a must for beginning to appreciate the difficulty of the subject and potential pitfalls; the volume has other valuable articles and commentary as well. For another perspective and pointers to a much broader discussion, see Chapter 6 (including the “Bibliographic Note” in Sec. 6.9) of the text by Gelman et al. [16]. Kass and Raftery give another brief synopsis in Sec. 5.1 of their article on Bayes Factors [15]. Of course Jeffreys’ classic monograph also still provides insightful reading and historical perspective.

The articles by Berger and by Goldstein and the ensuing discussion in Bayesian Analysis [12, 13] are a great introduction to the discussion within the Bayesian community. While there is quite a spirited discussion, it is clear that there is a consensus recommendation for a “healthy” sensitivity analysis in any Bayesian analysis used for scientific communication. In a Model Selection analysis, particular caution is needed when using priors in which arbitrary constants in the normalization do not cancel in the evidence ratio. The method advocated by the CLAS Collaboration [1], while applying the established Bayesian model selection formalism, used such arbitrary inputs and thus the “quantitative” output should also be regarded as arbitrary, until a “healthy” sensitivity analysis is displayed, and/or the sampling properties are understood.

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