Some discussions on the Read Paper “Beyond subjective and objective in statistics” by A. Gelman and C. Hennig

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1 Seconder discussion (Ch. Robert)

While I fully agree with the authors’ perspective that there are more objectivity issues in statistics than the mere choice of a prior distribution in Bayesian statistics, I first doubt switching terms as proposed therein will clarify the subjective nature of the game for everyday users and second feel there are deeper issues with the foundations of statistics that stand beyond redemption. While surprised at seeing a paper entirely devoted to (necessarily subjective) philosophical opinions about statistics I obviously welcome the opportunity of such a discussion.

Indeed, “statistics cannot do without data” but the paper does not really broach upon the question whether or not statistics cannot do without probability. Although this may sound like a lieu commun, let us recall that a statistical analysis almost invariably starts with the premise that the data is random. However, the very notion of randomness is quite elusive, hence this aspect truly fits within the paper topic—without even mentioning the impossibility of establishing randomness for a given phenomenon, barring maybe instrumental error—. This query extends to the notion of a probabilistic generative model and it relates more directly to the repeatability assumption that should not be taken for granted in most realistic situations.

The central message of the paper is that statistical analyses should be open about the many choices made in selecting an estimate or a decision and about the forking paths of alternative resolutions. Given that very few categories of statisticians take pride in their subjectivity, but rather use this term as derogatory for other categories, I fear the proposal stands little chance to see this primacy of objectivity claims resolved, even though I agree (i) we should move beyond a distinction that does not reflect the complexity and richness of statistical practice and (ii) we should embrace and promote uncertainty, diversity, and relativity in our statistical analysis. As the discussions in Sections 2 and 5 make it clear, all statistical procedures involve subjective or operator-dependent choices and calibration, either plainly acknowledged or hidden under the carpet. This is why I would add (at least) two points to the virtues of subjectivity to Section 3.3 that is central to the paper message:

1. Spelling out uncheckable assumptions about data collection;

2. Awareness of calibration of tuning parameters.

while I do not see consensus (item 2) as a necessary virtue.

In fact, when going through the examination of objectivity claims by the major threads of formalised statistical analysis, I get the feeling of exploring many small worlds (in Lindley’s words) rather than the entire spectrum of statistical methodologies. For instance, frequentism seems to be reduced to asymptotics, while completely missing the area of non-parametrics. Frequentist inference is mostly equated with the error-statistical proposal of Mayo (1996), despite the availability of other and more mainstream
perspectives. In particular, except for the reference to Davies (2014), the M-open view seems to be missing from the picture, despite attempting to provide reasoning outside the box. From a Bayesian perspective, the discussions of subjective, objective, and falsificationist—missing empirical—Bayes do not really add to the debate between these three branches, apart from suggesting we should give up such value-loaded categories. I came to agree mostly with the subjectivist approach on the ground of relativity, in that the outcome is always relative to a well-specified Universe and that it can solely be measured in terms of that reference. I further find the characterisation of the objectivist branch somehow restrictive, by focussing solely on Jaynes’ (2003) maxent solution (which itself depends on many subjective choices). Hence, this section is missing on the corpus of work about creating priors with guaranteed frequentist or asymptotic properties. Furthermore, it operates under the impression that an objective Bayes analysis should always achieve the same conclusion, which misses the point of an automated derivation of a reference prior construct. That many automations are feasible and worth advocating nicely fits with the above relativity principle. I also find the defence of the falsificationist perspective, i.e. of essentially Gelman and Shalizi (2013) both much less critical and extensive, in that, again, this is not what one could call a standard approach to statistics.

In conclusion, the paper is appealing in calling for an end to the “objectivier than thou” argument, but harder to perceive as launching a move towards a change in statistical practice. On the positive side, it exposes the need to spell out the inputs—from an operator—leading to a statistical analysis, both for replicability or reproducibility reasons and for “objectivity” purposes, although solely conscious perceived choices can be uncovered this way. It also reinforces the call for model awareness, by which I mean a critical stance on all modelling inputs, including priors, that is, a disbelief that any model is true, applying to statistical procedures Popper’s critical rationalism. This has major consequences on Bayesian modelling in that, as advocated in Gelman and Shalizi (2013), and Evans (2015), sampling and prior models should be given the opportunity to be updated when inappropriate for the data at hand. A potential if unrealistic outcome of this paper would be to impose not only the production of all conscious choices made in the construction process, but also through the posting of (true or pseudo-) data and of relevant code for all publications involving a statistical analysis. On the negative side, the proposal is far too idealistic in that most users (and most makers) of statistics cannot or would not spell out their assumptions and choices, being unaware of or unapologetic about these. This can be seen as a central difficulty with statistics as a service discipline, namely that almost anyone anywhere can produce an estimate or a p-value without ever being proven wrong. It is therefore hard to fathom how the epistemological argument therein—that objective versus subjective is a meaningless opposition—could profit statistical methodology, even assuming the list of Section 2.3 be made compulsory. The eight sins listed in the final section would require statistics expert reviewers for all publications, while it is almost never the case that journals outside our field call for statistics experts within referees. Apart from banning all statistics arguments from journals, I am afraid there is no hope for a major improvement in that corner.

It is thus my great pleasure to second the vote of thanks for this multi-faceted paper that helps strengthening the foundations of our field.

2 About Bayesian transparency (G. Celeux)

I congratulate Andrew and Christian for their much interesting and stimulating article. I agree with their proposition to bring to the fore the attribute transparency instead of the attribute objectivity. As a matter of fact, statistical models are not expected to explain or describe the world, but they can rather be expected to provide tools to act on it. For this very reason, transparency is desirable.

But, I am not sure that transparency is easy to be ensured in the Bayesian framework with complex models. Actually, the influence of hyperparameters could be quite difficult to be analysed in an informative setting and this task could appear to be even more difficult in a non-informative setting. In other words, in some circumstances, choosing prior distributions could appear to be a sorcerer’s apprentice game hardly compatible with transparency. Anyhow, transparency of a Bayesian statistical analysis requires in-depth (and expensive) sensitivity analyses as soon as the statistical models are somewhat complex.
3 Subjective Bayesian updating (J. Jewson)

I thoroughly enjoyed how this paper brings to light the subjectivity disguised as objectivity in statistical practise, and I relish the prospect that understanding the impossibility of objectivity will allow researchers greater freedom to experiment with their analysis.

Focusing on the Bayesian standpoint, I feel there is one major omission from the authors discussion, the methods for parameter updating. It is recognised throughout the paper that the model used in any statistical analysis is almost unavoidably taken to be an approximation of the decision maker’s true beliefs (or of the true data generating process depending on your perspective) (Bernardo and Smith, 2001). This results in statistics taking place in the M-complete or M-open world The authors regard Bayesian updating to be objective and transparent, suggesting that if a researcher is able to interpret their prior, then they will by implication, be able to interpret their posterior inference. In the M-closed world, I can believe this is the case. However, in the M-open world Bayesian updating is less transparent. It is a known result that Bayesian updating learns about the parameters of the approximate model that minimise the Kullback-Leibler divergence to the data generating process. Nonetheless, in practical terms I do not believe many statisticians understand what it means for two distributions to be close in terms of KL-divergence. The general Bayesian update (Bisiri et al., 2016) reinterprets Bayes’ rule as producing the Bayesian posterior attempting to minimise the logarithmic score (and as a consequence the KL-divergence to the data generating process):

\[
\pi(\theta | x) \propto \exp(- \sum_{i=1}^{n} -\ell(\theta, x_i)) \pi(\theta) = \exp(- \sum_{i=1}^{n} - \log(f(x_i; \theta)) \pi(\theta) = \pi(\theta) \prod_{i=1}^{n} f(x_i; \theta). \tag{1}
\]

This provides greater transparency to the Bayesian updating process, demonstrating that in combination with the prior, greater posterior mass is given to parameter values whose predictions via the model \(f(\cdot; \theta)\), achieve a low logarithmic-score on the observed data \(x\). Bernardo and Smith (2001) observe that scoring predictions based on the logarithmic scoring rule places great importance on correctly specifying the tails of the data generating process, as a large loss is incurred when an observation predicted with low probability is seen. Bernardo and Smith (2001) argue that tail specification is important for pure inference, but in applied problems the statistician may require their predictions to be accurate in some other region of the predictive distribution. The authors acknowledge that information concerning “how the results of an analysis are to be used or interpreted” should form an important, subjective part of the analysis. If the tails of the predictive distribution are important for the analysis, then the logarithmic-score should be chosen, and this decision should be documented. However, if the tail specification is not important, then blindly (implicitly) using the logarithmic-score under an approximate model, can produce predictive distributions that perform very poorly on the rest of the distribution. In this scenario the general Bayesian update provides the tools to produce a predictive distribution targeting an alternative loss function.

It is tempting to try and implement the general Bayesian update without using a model. I agree with the sentiments of the authors that using a model is important, it provides another tool to incorporate prior information into the analysis, and provides transparency in the way predictions are produced. Divergence functions and their associated scores, can therefore be used to produce model based loss functions allowing Bayesian updating to target aspects of the posterior predictive distribution away from the tails.

In agreement with the author’s recommendations concerning priors and tuning parameters, I advocate that Bayesian updating cease to be considered an objective black box and the room to impose subjectively is exploited and documented.

4 About coding (J. Josse)

Although I agree with the authors that virtuous statistical practice involves justifying choices made during the analysis, I do not think that statisticians do not do it because it is subjective, but rather because no one cares enough. Even if such explanations are crucial, they are not valued by the community. It is not common to have an entire article on the topic of scaling (see. Bro & Smilde, 2003), and such arti-
cles are likely to be published in applied journals not perceived to be prestigious by other colleagues. The pressure to be published should be mentioned.

I do not think neither that there are endless discussions on the subject of objectivity, subjectivity, but the fact that there are many ways to deal with a problem will always lead to this impression of subjectivity. This debate seems more linked to the Bayesian literature, perhaps because it has at least the merit of questioning what information is incorporated into the analysis. This could explain why the ones who use it for mathematical simplicity, which is quite justifiable, may be seen as “opportunistic Bayesians”. It is crucial to make choices clear.

The choice of data coding is important. In sensory analysis, there is debate as to whether the Likert scale should be coded as quantitative or qualitative. To be “coding free”, some methods (Pages, 2015) consider a compromise between these two points of view and highlight the specificity of each. The example of clustering is striking. Callahan et al. (2016) also stresses the need to document analyses with a view to reproducibility. He has shown that there could be “more than 200 million possible ways of analysing these data”. Of course, there is no “good” solution, it “depends” on the characteristics of the data one wants to capture.

Even when a problem is well-characterised, two statisticians who make use of the same data will use different approaches. This is mainly due to their personal history, and the expression “when one has a hammer, one sees nails everywhere” often applies. This is not necessarily a problem, and experience gained must be used. As mentioned by the authors, collaboration should be encouraged, for example in the development of simulation studies.

“The best future is one of variety not uniformity”. John Chambers

In conclusion, this paper has the merit of promoting transparency, awareness of the limits of a study, and its context-dependent nature. The battle is not lost because the community is already encouraging the sharing of code and data. It is worth remembering that different points of view can be legitimate.

5 About randomness (J.-M. Marin, J. Josse and C. Robert)

We congratulate the authors on their exposition of the issues of modelling and experimenters’ input on statistical inference and welcome this opportunity to discuss some fundamentals on such neglected topics.

A first criticism is about the focus that is definitely set on (statistical) models and the ensuing (statistical) inference. We indeed wonder if this focus is de facto set on a completely inappropriate problematic, namely arguing between ourselves [meaning academic statisticians] about the best way to solve the wrong problems, while the overwhelming majority of users is more than ready to buy and exploit quick-and-dirty solutions, provided these carry a sufficient modicum of efficiency, i.e., ready to enforce imprecise and suboptimal inference. Taking, for instance, the perspective of an Internet ordering operator seems much more relevant than focussing on the statistical background for validating the existence of an elementary particle. In other words, there are many more immediate (production) problems that call or even scream for statistical processing than well-set scientific questions.

We adhere to the argument that the scientific realism position allows for a more workable modus operandi. This particularly applies to data analyses in social sciences and medicine, as opposed to hard sciences where (almost) all experimental conditions can be expected to stay under control or at least to be stationary across repeated experiments. Maybe not-so-incidentally, the three examples treated in Section 4 belong to the former category. These examples are all worth considering as they bring more details, albeit in specific contexts, on the authors’ arguments. However, most of them give the impression that the major issue in the debate does not truly stands with the statistical model itself, referring instead to a concept of model that only is relevant for hard sciences. This was further illustrated by the mention of outliers during the talk, a notion that is nonsensical in an M-open perspective. It is obviously illusory to imagine the all models are wrong debate settled but it would have been relevant to see it directly addressed.

As a side remark, the rather hasty dismissal of machine learning in Section 5 is disappointing, because there is at least one feature for which machine learn-
ing tools are worth considering, and it is that they avoid leaning too much on a background model, using instead predictive performances as an assessment criterion. The alluring almost universal availability of such tools, as well as the appearance of objectivity produced by the learning analogy, could have been addressed in the spirit of the paper, especially in a context of those techniques taking over more traditional statistical learning in many areas.

Finally, the powerful role of software should be mentioned. Indeed, the availability of methodology in software may explain why certain practices, even when flawed, are still in use. Even though it is difficult to imagine software without any default values for “tuning” parameters, solutions may be envisaged to force users to be aware of the underlying choices and to assume them. In addition, all-inclusive statistical solutions, used through “point-and-shoot” software by innumerate practitioners in mostly inappropriate settings, give them the impression of conducting “the” statistical analysis. This false feeling of “the” proper statistical analysis and its relevance for this debate also transpires through the treatment of statistical expertises by media and courts, as well as some scientific journals.

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