

The Subjectivist Response

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It is a rare opportunity to have one's research discussed in print by as eminent and contemplative a group of researchers as my three discussants. To each I wish to express my gratitude for his thought-provoking comments. I appreciate their praise for my proficiency as a "preacher," but I do not think this is due to any special literary skills. Rather it reflects the intellectual quality of the existing scripture upon which my "sermon" was based.

I will address the discussants' comments in an order which I believe reflects a decreasing degree of common ground in our views. Given this ranking, it should not surprise the reader that I first address the like-minded comments of John Geweke.

Geweke dispels the widespread idea (for example, see John Rust's comments) that Bayesian methods are too hard to be practical. His comments are pragmatic rather than pedantic and I believe they will strike the hearts of many readers more forcefully than my theological sermon ever could. I wish to emphasize Geweke's main point: often Bayesian methods are easier. Geweke is a "doer" and his examples illustrate his main point dramatically.

Moving on to John Rust, let me say that he disarms me of any venom in my reply when he admits to being a Bayesian "in spirit" and that he acts "as if" he is a Bayesian when doing empirical research. Rust also throws me off stride when he argues (as Adrian Pagan does) that the internal logical consistency of Bayesian methods places the researcher in an undesirable "straightjacket" which inhibits creativity. I must confess that it never crossed my mind to consider "logically inconsistent" approaches.

I do not find Rust's suggestion that "measurement" serves as a primary purpose of statistical analysis to be very convincing. I am hard-pressed to think of examples in

economics where the parameters of primary concern are “real physical quantities.” If I could think of such examples, however, then I would proceed in an “objective” Bayesian fashion fully consistent with the LP.

I was happy to see that Rust also views the behavior of many researchers who employ Bayesian agents, but who eschew Bayesian techniques, as “schizophrenic.” Of course I am also sympathetic to the difficulty of performing any sensitivity analysis, be it with respect to a prior in a Bayesian analysis or with respect to some other aspect of the model in a frequentist analysis. Good applied researchers of any persuasion are always concerned with the “robustness” of their results.

One issue raised by Rust that warrants special attention is the increasingly fashionable topic of semi-parametrics and nonparametrics. I believe he views the results of Dianconis and Freedman to be more damaging to the subjective Bayesian position than the authors themselves do, or I may add, than do many of the authors’ discussants. Readers should note that Rust’s characterization of the entire matter is very much in an objectivist tradition: there is something (like a conditional mean) about which we wish to learn and about which we wish to make few assumptions. In such a setting semi-parametric and nonparametric techniques appear quite enticing. As an economist, however, Rust knows there is no such thing as a free lunch, and more than most he understands the limitations of the asymptotic results that exist in these areas. (I trust the reader appreciates that, in order to judge the quality of the asymptotic approximations to a particular finite sample setting, one must make most of the assumptions one was trying to dispense with in the first place.) What I do wish to address, however, is the importance of Dianconis and Freedman’s results to the subjectivist.

Once one adopts the subjectivist position that the likelihood is merely a window through which to view the observable world, the fear in making bold assumptions is lessened. The question of whether the “true” regression is linear, say, is not necessarily a meaningful question to the subjectivist. Recall that I have argued that the subjectivist window is intended to be attractive and useful. It is chosen in the hope of capturing the interests of a wide audience who will agree to disagree over the most appropriate priors to impose on the parameters which define the window. If the subjectivist fears that the professional audience is so diverse that no finite-dimensional window will ever bring about agreement over even this initial matter, then nonparametrics becomes a possible alternative. The importance of Dianconis and Freedman’s results in this setting is that there may not be a tractable prior available which can guarantee that as data evidence accumulates, any two researchers need reach agreement. In effect, there is so little structure on the problem that there is no guarantee that the data will eventually swamp prior opinion. In such instances the subjectivist must be content with inevitable persistent disagreement among researchers. Casual observation of the economics profession suggests to me that this need not signal the end of research among like-minded subgroups.

Let me go further out on a professional limb (assuming this paper leaves me any further limb upon which to journey) and predict what I see as the possible future for semi-parametrics and nonparametrics in econometrics. On the positive side, we may

find that after all the sexy mathematical problems have been resolved we are left with techniques that aid in choosing simple parametric windows through which to view the world. If this turns out to be the case, then it may prove to be all worthwhile. I fear, however, that the end result may be that some of the most technically-talented econometricians around may have entertained themselves for a few decades without leaving behind much that is useful for the blue-collar empiricists.

Finally, let me turn to Adrian Pagan who appears to have enjoyed my pilgrimage down the Bayesian path, but who also remains unconvinced. I am happy that Pagan addresses the LP and he raises two points which deserve comment. Firstly, although his formulation of the score test statistic remains invariant for proportional likelihoods as in example 1, this is not the case when the parent formulation using the information matrix is used, since the computation of the latter involves taking an expectation with respect to the sampling distribution which differs for the two experiments. Secondly, his comments concerning Examples 1 and 2 never escape the frequentist assumption that one must always take into account the entire sample space, and not just the observed data. If one starts with this premise, then the LP will indeed seem peculiar. The issue I had hoped he would address is which of the underlying axioms he found disagreeable.

Pagan wisely throws me off guard by claiming that my PPMB's show that I am not a true subjectivist. I am not embarrassed by my eclectic and ecumenical approach. Indeed I welcome such labels as ammunition against what I perceive will be many readers who think my position is far too radical. Let me, however, attempt to recover my subjectivist purity.

I know of few one-prior Bayesians, certainly, none among serious empiricists. The decision making agent in Savage's axiomatic development is making personal decisions and is not assumed to be behaving in the setting of the research scholar. The alleged infrequency with which Ed Leamer receives invitations to the purists' altar are equally puzzling to me. History suggests, however, that prophets are not always treated kindly, and so his calls for sensitivity analysis may some day be more appreciated.

When implementing any philosophical position reasonable researchers may take alternative branches from the parent trunk. The Bayesian trunk to which my branch is attached, however, grows far away from any frequentist forest. If I have sinned in my admission that "priors" are sometimes better described as "post-data priors," then it is at most a venial sin of the empirical researcher, and not a "purer than thou" attitude of one who has never left the sanctuary of pure theory. Since both Geweke and Pagan appear disturbed about my PPMB 5 (Cromwell's Rule), I must indeed have caused some confusion and so allow me to try to clarify matters. I think my differences with Geweke are largely semantic; I believe assigning unitary probability to a window suggests that one would never consider expanding it. The main point of PPMB 5 is to serve as a reminder to avoid arrogance. Flagrantly poor predictive performance on an absolute scale can make it abundantly clear to the researcher that "something" needs to be changed without necessarily employing any statistical tests or knowing at first what that "something" is. While this situation shares commonalities

with those typically encountered when using a pure significance test, namely, the absence of a well-specified alternative window, I do not believe it implies the need for a frequentist-based pure significance test as I believe Pagan does. Nor do I believe it suggests one should abandon the old window before a new one is chosen. The creative act of choosing a new window is something I have attempted neither to describe nor explain. I suspect it will often be data-based. Once this new window is formulated, however, one can proceed in the standard Bayesian fashion to compare it to the old window as Geweke suggests. The practical importance of PPMB 5 is that the posterior probabilities for hypotheses that a Bayesian computes only measure relative performance, relative to the window in which all are nested. To assign a probability of unity to a window is to suggest these posterior probabilities measure absolute performance.

Finally, let me address Pagan's comments on pre-testing. I do not understand the argument that one should employ frequentist techniques which eschew conditioning on the data to pick a specification, and to then do the very unnatural frequentist act of conditioning on the chosen model. I agree with Pagan that people often behave this way. The rules of this "sometimes frequentist" and "sometimes conditionalist" game, however, escape me.

Pagan's characterization of the Bayesian pre-test problem also bothers me. I believe it differs from the frequentist analog in a very important way and that the LP plays an important role in this difference. For a LP proponent, how one arrives at a hypothesis does not matter for purposes of assessing the evidence provided by the data on unknown parameters. For a frequentist, two researchers who arrive at the same hypothesis following different paths will in general draw different inferences. The Bayesian view is that how one got the hypothesis should not matter in evaluating what the data have to say about the parameters given that hypothesis. The different paths, however, may reflect different prior probabilities assigned to the hypothesis and these nondata-based inputs may be quite important for testing.

Let me close by drawing the reader's attention to Pagan's closing quote of the eminent statistician David Blackwell for whom I have the greatest admiration. Pagan, Rust and myself have all engaged in religious metaphors, and given Blackwell's quote this indeed may be appropriate. Like religion, the statistical philosophy which we bring to bear on our research problems reflects to a great deal our early training, but in the latter case we receive it, not from our parents, but from the teachers of our introductory statistics courses. In this sense, not many of us are trained as Bayesians; indeed most Bayesians are converts, and we all know the evangelical tendencies of converts. Who becomes a convert is an intriguing question. I suggest a necessary condition is the willingness to read some of the Bayesian scripture, and if I have whetted the appetite of the reader to do so, then I consider my efforts here not to have been in vain. Many of those who actually do so will find it to be a "religious experience." I conjecture, however, that all who do so will find it to be an intellectual experience.

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1. Harald Uhlig. 1994. What Macroeconomists Should Know about Unit Roots: A Bayesian Perspective. *Econometric Theory* **10**:3-4, 645-671. [[Crossref](#)]