

On the Relevance of First Order Asymptotic Theory to Economics

Esfandiar Maasoumi

Dept. of Economics, SMU, Dallas, TX 75275-0496, maasoumi@mail.smu.edu

On the occasion of its 100th issue, the founders of the Journal of Econometrics, as well as its successive co-editors and editorial boards deserve our gratitude and thanks. It is easy to forget where we were at in econometrics publication in early 1970s. If you had something to say, you had to either please statistical outlets, or the few barons controlling *Econometrica*, or you shelved your work. Things have hardly improved with *Econometrica*, to say the least, but thanks to pioneering work of this journal, most of the real action is reflected elsewhere. There are numerous other outlets that are widely read and cited.

Looking back and to the future deserves our attention, well beyond a few pages available here. But this collection will have great historical value, both for what is said and what is not. I will focus on only a few points. The first is, what have we achieved since about the publication of Bill Taylor's "On the relevance of finite sample distribution theory", Taylor (1983, *Econometric Reviews*)? The second is an informal set of where the most consequential developments may come from.

There is a widespread belief, an official position of sorts, that we now have the "inference tools" to deal with much more complicated models and processes. Laws of large numbers (LLN) and central limit theorems (CLT) have been given under largely unverifiable and unlikely conditions for nonlinear, dynamic models/processes. Almost every function of the data you are likely to consider is consistent for something, is normally distributed, and gives rise to Chi-squared type statistics for testing any desired hypothesis. Any casual consideration of the rich tapestry of the random phenomena suggests that there is something wrong with this picture. It is practically tautological to say that most estimators are neither normally distributed nor even symmetric, and most test statistics are not Chi-squared. Finite sample distribution theory may not have given us a workable inference theory, but it gives us enough to assert the previous claim. Science is about approximation, to be sure, but the burden of proving adequacy and accuracy is with those proposing any approximation. In sciences it is widely accepted that the first term of Taylor series approximations, already merely locally valid, can be very poor. Asymptotic expansion, already less solid as an approximation concept, delivers even less. Still worse, in Maasoumi and Phillips (1982) a rather chilling characteristic of first order asymptotics was demonstrated. In the practically relevant "misspecified models" the limiting distribution may even fail to exist! This has recently been shown for GMM estimators by Alastair Hall and others. Monte Carlo evidence, though badly needing systematic compilation, provides frequent embarrassing evidence against first order asymptotic results. There are some experimental situations that are better than others. But where is the characterization work that may guide the users and empirical economists as to what to expect, when, where, and how? It is important to appreciate that most models are local approximations. Because of this we will often find evidence of "regime change" in the data. It is not a little comical then to pretend that larger samples serve a fundamentally small sample context. How can infinitely many observations pertain to an approximation valid for three points in time?

It is no wonder to witness a widespread cynicism, and even outright rejection of "econometrics" in some quarters. See Jim Heckman's account in this issue, even though I am less worried about pre-renaissance economics rejecting econometrics, or trying to reinvent it to support an impressionist movement. But we have been warned: with or without econometrics, a good empiricist, familiar with his/her data and substantive context, can look at the square of her errors and decide that it is "relatively" large or not. A main claim of statistician/econometrician to usefulness in this process is the potential ability to provide a reliable probability level to such statements as economists need to make. That is, an inference theory with adequately characterized approximation levels and performance guarantees. We are not immune to laws of nature, even if we cook up the rules of the game so as to preserve the status quo. We need to change the incentives and the reward structure, may be a little like the mechanisms we teach our students and publish in elite journals, or face even greater irrelevance to the exciting economic world around us.

I very much subscribe to the points raised in Jim Stock's account in this issue. He has the courage to mention the "C word", as in Alfred Cowles. The market crash of the 20s led to Cowles closing down shop and confessing that he and his contemporaries couldn't predict. He threw his resources and support to the "correlation" crowd, the early econometricians who made up the Commissions and Foundations, with journals that would promote a scientific quantitative economics such as "has come to dominate in the natural sciences". It is fair to say that we have completely adopted the scientific language, the image, and the formalisms. But any one that thinks we can forecast better than in the 1930s is in denial. Actually, I have known a "secret" on this topic all my professional life! In Michael McCarthy's work, in my thesis work under the late Denis Sargan, as well as in a couple of seminal pieces by the latter, it was made clear why good forecasts were not likely with the standard methods and assumptions in use then and now. Almost all predictors have a Cauchy-like characteristic even, or especially, if the model is correctly specified! See Maasoumi (1977, 1978), and Sargan (1976, 1988). We also proposed some frequentist and Bayesian solutions that were noted by very few. But at the 1980 World Congress of the Econometric Society, Malinvaud acknowledged the problem and the promise of its solutions when he sounded the first alarms about where macroeconomics was headed. Some aspects of the Bayesian solutions and reduced form modeling philosophy which I thought would work did find a welcome place in Bayesian VARs, at least for a while. Perhaps more could be done now, given the very exciting numerical implementation methods described in John Geweke's comments here.

Indeed, our increased computational capabilities hold great promise. Perhaps there will be a development toward more realistic models and inference techniques, accommodating more sophisticated adaptive behavior and learning, and less beholden to production of closed form formulae. Perhaps even model specification will be recast into a learning and adaptive procedure, perhaps with versions of Neural Nets (NN), which will truly acknowledge that all models are approximations/misspecifications.

I predict a return to Distributed Lag models, preferably flexible ones! The currently popular dynamic models in which the own past history dominates, seem rather barren transformations of more economically interesting distributed lag processes. They are useful and economical curve fitting media, but they don't reveal much about behavior or working of economies. The fact that the multi trillion dollar quantities like the US GDP appear to move like a random walk is a numerical artifact, not a causal model which can play a role in policy analysis. The residual is too "small" to allow non-fragile inferences about additional variables and processes. A piece-wise linear model, the so called "structural break", as though we could distinguish it from an approximate modeling phenomenon, does equally well! As for the "unit root thing", more and more researchers will put the Bayesian hat on, and acknowledge that, for a continuous and unknown variable, statistical probability of any particular value is zero. Never have so many depended for so much on so little probability.

Another aspect of our newfound computational wealth offers a promise of more accurate and more reliable small sample inference. Resampling techniques, such as the bootstrap and simulation, are wonderfully simple tools for mimicking our sampling inference methods. They have an *interpretation* as estimators of higher order approximations to unknown distributions of econometric statistics. A modern account of this is given by Hall, Tibshirani and others. An older one for Monte Carlo is in an appendix in my thesis, Maasoumi (1977). Sargan (1982) is an expanded account. This was all inspired by some titillating Monte Carlo evidence I produced for expansions of the moments of estimators that had no finite sample moments. Of course, Edgeworth expansions are valid under less stringent conditions, and bootstrapped distributions can produce estimates of the commonly neglected second and higher terms (the first term giving rise to first order asymptotic theory). But bootstrap methods may do well even if the Edgeworth expansions don't (and they can do very badly, especially in the tails, and for a lot of interesting cases like the first order autoregressions). And they can do badly, both when the Edgeworth expansions do badly, and otherwise. Edgeworth expansions are not the only ones available, of course, but they are currently the best known to us. It would be a deep misunderstanding of statistical approximation to over extend this otherwise useful "interpretation" of the bootstrap. For instance, contrary to a common claim (e.g., see Joel Horowitz in this issue), asymptotically pivotal statistics are, in general, neither necessary nor sufficient for good bootstrap performance. Indeed "pivotalizing", for instance by dividing to poor variance estimators, can lead to a deterioration of bootstrap performance. This is not unlike the phenomena that can diminish the accuracy of Edgeworth expansions.

A word to the wise. The fate of an economic "science" is inextricably bound to that of econometrics. The subject matter is profoundly empirical, and if econometrics fails, economics will be relegated to an interpretative field not as it has come to dominate in the natural sciences. This will not be the end of the world since the economy has a way of surviving without us! But our ultimate goal of wanting some systematic and reliable method of enhancing societal welfare will be in jeopardy. One constructive way to hedge, given the probability of an economic science, is to teach serious "economic statistics" in order to produce "economists" who know something about the phenomenon they wish to theorize on. If you think this is not necessary, start an informal "on the spot" survey of your colleagues by asking, "what is the current GDP?"; "what is the current budget deficit?"; Total consumption?, etc. You might be alarmed.

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