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Comments on: "Structural vs. atheoretic approaches to econometrics" by Michael Keane

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ABSTRACT

My comments on the keynote paper by Michael Keane.

There is little here I can disagree with. This should be no surprise since I am a structural econometrician, and thus a biased commentator. I have a great admiration for Keane's work, and also for the work of other fine scholars in this area — many of whom are at this conference.

I have only a minor quibble. Structural econometrics is not on the decline everywhere in economics. For example, the field of industrial organization still values empirical work that focuses on formulating, estimating, and testing models of firm and industry behavior. As a result, structural empirical IO papers are still frequently published in several leading economics journals such as *RAND* and *Econometrica*. It is mainly in public and labor economics where structural work has been on the decline, and my perception is that this has been partly due to antipathy to this work by the editors of journals such as the *Journal of Political Economy*, *American Economic Review* and the *Quarterly Journal of Economics*. Some structural econometric papers can still be published in these journals, but the odds are much better if the work is disguised as "behavioral economics" — a field that is now synonymous with theories involving some degree of "irrationality" or departure from dynamic expected utility maximization.[1] There are still a few leading economics departments where structural econometrics is taught and practiced, including Penn, Duke, and Yale. However structural econometrics has been virtually purged from most of the top ranked economics departments (MIT, Princeton, Harvard, Berkeley, and Chicago).

What explains this state of affairs? Keane identifies one important reason: structural econometrics is difficult. It requires several skills including a knowledge of economic theory, econometrics, an understanding of data and institutions, and especially, considerable knowledge of numerical methods and computer programming, since structural models rarely admit a convenient closed form solution that can be estimated via regressions or other standard statistical routines in popular statistical packages such as Stata.

Structural econometrics resulted from attempts to bridge theory and empirical work in economics. The Cowles Commission and the Econometric Society are two venerable organizations that were created to further this goal. The marriage of economic theory and statistical inference inspired whole new classes of problems, models, and methods of inference, including the linear simultaneous equations model and the *identification problem*, both of which distinguish econometrics from other branches of statistics. It is ironic that the method of instrumental variables – arguably the single most important idea underlying the "atheoretic" econometric literature that has become the dominant paradigm today – traces its existence to the structural econometrics pioneered in the 1930s and 1940s at the Cowles Commission. Given this intellectual pedigree, something else must be responsible for its decline in the profession today.

Perhaps the explanation is the professional incentive structure that is now in place. Why should young economists invest in the necessary skills and devote all the time and effort it takes to produce a structural econometric paper when there is little chance it will be published or appreciated? Instead young economists these days can do *freakonomics* and have Steve Levitt to look up to as their role model. Winner of the 2003 John Bates Clark Award, Levitt openly confesses his ignorance of economics "I mean, I just

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1 In the case of the *QJE*, the chances of publication are significantly higher if the paper finds evidence in favor of hyperbolic discounting. See Rubinstein (2003, 2005) for comments on hyperbolic discounting and behavioral economics.

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— I just don't know very much about the field of economics. I'm not good at math, I don't know a lot of econometrics, and I also don't know how to do theory'' (New York Times, August 3, 2003). Levitt exemplifies recent generations of MIT graduates who have been taught that the only tools one really needs in order to be a successful applied economist is a little knowledge of regression and instrumental variables. Of course, it also helps to have a knack for choosing entertaining and controversial topics such as ''The Causes and Consequences of Distinctively Black Names'', Fryer and Levitt (2004).

However, it is no longer necessary to have a model, or an innovation in econometric methodology, and the question doesn't even have to be important as long as one has a clever instrument and an entertaining topic. In fact, Levitt's work demonstrates that it is no longer even necessary to do the regressions correctly in order to achieve fame and superstar status in the dumb and dumber regime we're in. Two of Levitt's most famous papers are false: i.e. his key findings evaporate once elementary programming errors are corrected. This includes his controversial paper with Donohue that claims that the Roe vs. Wade decision was a cause of the significant reduction in crime in the U.S. in the 1990s.[2]

Many of the problems in Levitt's work were known at the time the American Economic Association awarded him the Clark Medal, but *C'est la Vie*. The message this sends to the profession is that it not important to be capable of advancing economic or econometric theory, and it does not even matter whether empirical work is right or wrong: what is important is being able to publish articles that are *entertaining* — a recognition that marks the transformation of economics from science to a type of ''elite infotainment industry''.[3]

I also attribute developments in econometric theory, particularly the rise in popularity in nonparametric and semiparametric methods, as contributing to a false belief that it is possible to do applied work without making any assumptions, and that all parametric assumptions are arbitrary, unjustified, and therefore bad. Don't get me wrong: there are many important new ideas and estimators that have been contributed by this literature or borrowed from statistics that have transformed econometrics. It *is* important to know how to estimate quantities of interest under the weakest possible assumptions. However if we want to do more than just summarize data, such as to try to make inferences about causality, to test theories of behavior, to estimate underlying primitives such as preferences and beliefs, and to make predictions of how hypothetical policy changes will affect behavior and individual welfare — it is impossible to escape the need to make a host of very strong assumptions. On this point Keane and I completely agree.

Another mindset among statisticians and econometricians that militates against structural econometrics is the simplistic view of inference as a process of hypothesis testing and uncovering the ''true parameters''. As we all know, models are by definition simplifications and approximations to reality, and as such, they are all necessarily false as literal descriptions of it. However some false models provide better approximations to reality than others, and hopefully over time our theories and models will become more realistic and provide better approximations to actual behavior and economic outcomes. Thus, approximation theory would be a more appropriate conceptual apparatus for analysis of structural estimation than traditional statistical estimation and hypothesis testing. Even though structural models may be misspecified and depend on more or less arbitrary functional form assumptions, it is still possible that some of these models will do a better job of predicting behavior than purely statistical models that attempt to circumvent the formulation and estimation of a behavioral or economic model and instead estimate some other ''quantity of interest'' under ''weak assumptions''.

Unfortunately the simplistic view of econometrics as a method for discovering the ''truth'' seems to pervade, and practitioners of structural econometrics are repeatedly confronted with the question, ''but what if your model is misspecified?''. Instead, the question should be framed as: ''does your model fit the data better *and* provide more accurate out of sample forecasts than any other competing model?''. However, the statistical mentality that ''all structural models are rejected, therefore none of them are any good'' does a disservice and contributes to a radicalization of some members of the profession. For example, in macroeconomics, the leader of the ''Minnesota School'', Edward Prescott, completely rejects econometrics as a useful scientific tool. Instead he promotes *calibration* as the preferred method for uncovering the unknown parameters of structural models and for evaluating and comparing their ability to fit the data. I view calibration as a big step backward in terms of scientific methodology, but I can appreciate the frustrations that lead Prescott and other members of the Minnesota School to reject econometrics. I interpret their behavior as a reaction against the nerdly behavior of some econometricians who seem to care only about arcane details of statistical methodology but hardly at all about economics. I share these frustrations, and while I have an extremely high regard for Edward Prescott as an economist and empirical modeler, I still think his campaign against econometrics is excessive and has been counterproductive to the overall progress of empirical economics.

As economists, we have to accept some of the fundamental limits to our science. One of these limits is that most of the economic models that we are interested in estimating are *nonparametrically unidentified.* That is, there is little we can say about the underlying *primitive objects* of these models if we are unwilling to make any parametric assumptions.[4] Even though the identification problem is one of the most important limits to science that we must confront when doing structural estimation and inference (the *curse of dimensionality* being another one), we should not conclude that the existence of such limits makes the entire enterprise a fruitless undertaking. However Heckman and Navarro (2005) express the view that if a class of models is nonparametrically unidentified, it must be completely without empirical content, and therefore arbitrary and meaningless: ''His paper (i.e. my 1994 *Handbook of Econometrics* chapter) has fostered the widespread belief that dynamic discrete choice models are identified only by using arbitrary functional form and exclusion restrictions. The entire dynamic discrete choice project thus appears to be without empirical content and the evidence from it at the whim of investigator choice about function forms of estimating equations and application of *ad hoc* exclusion restrictions''. (p. 2).

2 One paper, published in the *AER*, does generalized least squares incorrectly, improperly inverting the weights on each observation, as noted by McCrary (2002). The paper with Donohue, published in the *QJE* claims that abortion caused the reduction in crime even after including state dummy variables and controls for other factors such as improving economic conditions, greater police enforcement, and an aging population. However, Foote and Goetz (2008) show that this study did not actually include these dummies as claimed, and the result evaporates when the dummies are actually included in the regression.

3 Poterba's (2005) paean to Levitt reveals how low standards of academic excellence have fallen: ''Steve's response [5] to McCrary is a model of the advance of academic dialog and the recognition that replication plays an important part in empirical research. Steve not only acknowledges the programming error that resulted in the difference between the original results and the results that flowed from correct implementation of the weighting procedure, but he praises McCrary for 'his careful work that has laid bare these errors'. (p. 1244)''. (p. 187). See Rubinstein (2006) for a less charitable view of Levitt's work.

4 See, for example, Rust (1994) and Magnac and Thesmar (2002) for nonparametric nonidentification of dynamic discrete choice models, and Ledyard (1986) for a discussion of identification problems in multiagent Bayesian games.

Although I disagree with this view, I have the very highest regard for James Heckman (winner of the Nobel Prize, he was also awarded the John Bates Clark medal back in the old regime where the value of work that bridged economic theory and econometrics was still unquestioned in the profession). I think Heckman's view probably encapsulates concerns that many antistructuralists have with structural econometrics more generally (although it is clear to me that Heckman is not one of them). In view of Heckman's importance to econometrics and because it is also a key point in Keane's paper, I think it is appropriate to take the space to provide a more detailed response. To start, I assume that Heckman does not disagree with Mike's statement that "One is forced to accept that *all* empirical work in economics, whether 'experimentalist' or 'structural' relies critically on *a priori* theoretical assumptions". (p. 3) since Keane quotes Heckman's own (1997) critique of the strong implicit behavioral assumptions underlying estimators of causal effects in the experimental, treatment effects, and instrumental variables literatures.

Then the argument reduces to a disagreement about the *relative validity* of different types of econometric approaches and identifying assumptions. An assumption that seems to be implicit in the nonparametric and semiparametric literature is that strong but *nonparametric* identifying restrictions are *good* but that *parametric* identifying restrictions (i.e. assuming a utility function has a particular functional form, or a random variable is a member of a particular parametric family) are *bad.* Examples of nonparametric identifying assumptions in the dynamic discrete choice literature are *additive separability, conditional independence,* and the hypothesis of *rational expectations.* The first two assumptions are used to simplify the way in which *unobserved state variables* affect decisions. These are *very strong assumptions.* In particular, the conditional independence assumption implies that unobserved state variables follow an *IID* process and are conditionally independent of observed state variables in the dynamic discrete choice model. The rational expectations assumption states that a person's subjective beliefs about the *observed state variables* (i.e. the state variables which can be observed both by the agent and the econometrician) coincide with the *objective probability measure* and thus can be consistently estimated from data on the realizations of agents' observed states and decisions. Hardly an innocuous (or even plausible) assumption.

It is far from clear to me that these types of assumptions, even though nonparametric in nature, are any less strong than parametric assumptions, such as assuming a utility function is a member of the constant relative risk aversion family, or that wages are lognormally distributed. However, as strong as these nonparametric identifying assumptions are, *they are not enough to nonparametrically identify the utility function, the distribution of unobserved state variables, and the subjective discount factor in a dynamic discrete choice model.* We need to impose additional *parametric* functional forms for preferences and the distribution of unobserved states, and only then is there a sufficient *a priori* structure to consistently estimate the discount factor and the unknown parameters of the utility function and the distribution of unobserved state variables.

Certainly there are many leading econometricians who are just not comfortable with economic models and the need to make these sorts of parametric assumptions about agents' utility functions, beliefs, and so forth. Instead, many econometricians are much more comfortable with simpler *statistical models* that abstract or bypass consideration of a deeper, more complex economic model and rely instead on a host of *statistical assumptions.* Under certain circumstances the imposition of certain qualitative or nonparametric identifying assumptions makes it possible for econometricians to identify causal effects, or "treatment effects" of changes in policy, and so forth. However, note that this is possible only by dramatically circumscribing the types of questions that can be asked and answered: the statistical model typically cannot tell us anything about individual welfare, efficiency, or the behavioral response to hypothetical policy changes that have not yet occurred. Many of the statistical assumptions that are commonly imposed seem implausibly strong to me, such as the assumption that the outcome of interest is well described by a linear regression model (an assumption underlying much of the instrumental variables literature) or the so-called *unconfoundedness assumption* in the treatment effects literature. This latter assumption, which is equivalent to a conditional independence assumption that there are no "unobservables" that affect both the decision to be "treated" and the treatment outcome, strike me as much more restrictive and questionable than the parametric assumptions about unobservables that were employed in the selectivity bias literature which did allow for the impact of such unobservables.

There is a sort of middle ground between structural and reduced form models: these are the quasi behavioral or quasi structural models which are not explicitly derived from any underlying economic theory, but at least have some sort of behavioral interpretation. Heckman's seminal (1979) sample selection model is a prominent example of this middle ground. His initial work was done in a fully parametric framework, under the assumption that error terms in the selection equation and in the linear outcome equation are jointly normally distributed. However the research on semiparametric estimation methods in the 1980s succeeded in relaxing the normality assumption, and showing that one could consistently estimate the parameters of interest without making strong parametric assumptions about these error terms. However there has been considerably less investigation of questions such as: (1) under what circumstances will the sample selection model arise as the solution (or approximation to a solution) to some underlying profit or utility maximization problem or describe behavior predicted from some other type of behavioral economics model?, (2) why is it reasonable to believe that the outcome equation is linear, that error terms enter additively separably, and that other observed covariates affect the model in a linear-in-parameters specification?

Some economists (such as myself) feel uncomfortable working with an econometric model that is only loosely and informally connected with an underlying theory — unless we are just using these models to summarize data without any particular theory in mind. However, once we have a particular theory/model in mind, why not try to go out and directly estimate and test it? The parameters of these quasi structural models, such as the coefficient of the option value in a semi-reduced form option model (e.g. Coile and Gruber, 2007), strikes me as very difficult to interpret. Similarly, my feeling after reading many supposedly empirically oriented applied econometric papers that rely on highly abstract, qualitative identifying assumptions/arguments such as the unconfoundedness assumption is well summarized by Alice's reaction to hearing the poem *Jabberwocky:* "Somehow it seems to fill my head with ideas — only I don't exactly know what they are". (Carroll (1895) *Through the Looking Glass* p. 23).

However, the solution to this disagreement is simple: if I am uncomfortable with a particular model, I just don't use it. If others are comfortable with statistical approaches, by all means they should use them. What's wrong with letting 1000 flowers bloom? Conversely, many economists are uncomfortable about working with models that are too explicit, or which they view as too oversimplistic to take seriously in the first place. I have a great deal of sympathy for the latter point of view, and there are many structural models that I reject out of hand because I feel they are too simple to be taken seriously. I think the main defense for starting with such models, besides a legitimate disagreement about what constitutes "overly simplistic", is that we have to start somewhere.

I view all of the above as legitimate grounds for disagreement between structuralists and antistructuralists. However it is not legitimate (or intelligent) to pretend that we don't need to impose very strong assumptions to obtain interesting conclusions about causal effects and to obtain a deeper understanding of economic phenomena. Where I think there is legitimate grounds for disagreement is over the specific types of models and assumptions different people feel comfortable with. Some econometricians feel very uncomfortable about building economic models and appearing to take theory too seriously, but they are content to restrict their attention to a much less ambitious set of questions, using statistical models and imposing statistical assumptions that make them more comfortable. I might describe these types of econometricians as *statistical modelers.* On the other hand, structural econometricians are generally more ambitious in the types of questions and issues that they try to address, and they are willing to impose more assumptions and to formulate economic models to try to get answers. Structural econometricians do take economic theory seriously and go to considerable effort to formulate, estimate, and test the unknown parameters of these models. I would describe these types of econometricians as as *economic modelers.*

I have gone on too long, but as you can see, this is a topic I care *deeply* about. I really do not understand the widespread antipathy towards structural econometrics. I do not see any basis for the belief that the reduced form approaches adopted by statistical modelers is more justified or legitimate (or is less subjective) than the structural econometric approach adopted by economic modelers. Both types of modelers have to impose strong assumptions, and it seems all that we can say is that these models and the underlying identifying assumptions are just *different.* Just as "men are from Mars and women are from Venus" there may not be any possibility of reconciliation or easy communication between the two camps, except for some exceptional people such as Jim Heckman who have been able to move easily between these worlds and make fundamental contributions to both. To me, many of the choices of how to do empirical work come down to personal preferences and beliefs about the approach one thinks is likely to be the best way to gain new understanding of the data and an economic question under consideration. However, I do not see any clear way of demonstrating that approach A or approach B is a superior approach in general. It depends on the case at hand, and also on the skills and preferences of the person doing the empirical work. Of course, there are always extremists (or should I say *fundamentalists*?) who seem to believe there is only one right way, that all other ways of doing things are wrong, and that the infidel practitioners must be exterminated.

It really isn't productive to criticize the *status quo* in economics these days, nor is it productive to try to promote the virtues of structural estimation. Criticism only encourages the practitioners to rally around the flag. I think it is equally a waste of time to try to engage in salesmanship. I recall early in my career attending numerous conferences on Bayesian econometrics, where small groups of Bayesian devotees would preach the virtues of this approach to inference and criticize the limitations of classical methods of inference. However they were largely preaching to the choir, but few others were attending their sermons. What has really helped to spread the use Bayesian inference is the development of tools for doing Markov chain Monte Carlo, which has made it computationally feasible to do Bayesian inference for a wide array of econometric models. A better way to sell a tool to an educated consumer is to demonstrate that it is not too expensive or hard to use, and that it can do important things that would be difficult or impossible to do without it.

Even though I am not happy to see the advent of the new 'lite' research paradigms such as freakonomics, I still operate under the presumption that economists are intelligent and well informed consumers who will make their own choices about the methods they should use and topics they should pursue in their research. I try to inform my own students about the risks involved in doing structural estimation, and the pros and cons relative to reduced form approaches that are currently in vogue. The luxury of tenure insulates me from the most severe consequences for speaking out and doing the type of research that I think is best. As long as I have the freedom to say what I believe and do the work I think is best, I really have little to complain about. However I do lament young economists who would like to do structural econometrics, but who fear the consequences for their careers. There should be ample room for different methodological approaches to empirical work: in the words of Rodney King, "Why can't we all just get along?"

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