

Structural vs. Reduced-Form Estimation

Zhou Zhou 15220182202882

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Abstract

In terms of empirical analysis methods, there are two schools in American Economics: reduced-form approach and structural approach. The difference between the two schools lies in their role in the empirical study of economic theory. Reduced-form approach thinks that empirical research should *Let Data Speak for Itself* – they believe that the economic theoretical model is determined by the will of researchers, and the conclusion obtained by imposing the will of researchers on the data will be correct only when the model is correct. Because it's impossible for researchers to know what model is right, their main research tool is simple: using a variety of regression analyses. Structural approach argues that *Data only can never reveals its own data generating process* – if the goal of economic research is data generation process, the data generation structure can only be understood with the help of the researcher model, even if the researcher's model may be wrong.

In this paper, I'll try to briefly explain the main arguments in the two cliques based on authors who wrote papers the professor give to us.

About “Take the Con Out of Econometrics”

Edward Leamer(1983) presented randomized trials—a randomized evaluation of fertilizer, to be specific—as an ideal research design. He argued that randomized experiments differ only in degree from non-experimental evaluations of causal effects, the difference being the extent to which we can be confident that the causal variable of interest is independent of confounding factors.

The chief target of Leamer's essay was regression analysis. After making the tacit assumption that useful experiments are an unattainable ideal, Leamer proposed that the whimsical nature of key assumptions in regression analysis be confronted head-on through a process of sensitivity analysis.

Leamer(1983), Hendry(1980), Sims(1980) and others writing at about the same time were similarly disparaging of empirical practice. Perhaps credible empirical work in economics is a pipe dream that time. Leamer diagnosed his contemporaries' empirical work as suffering from a distressing lack of robustness to changes in key assumptions – assumptions he called “whimsical” because one seemed as good as another.

Structral Econometrics

Credibility Revolution

In these years, it has had a broad and far-reaching impact for Leamer's critique, nearly a quarter century past, econometrics who stand for structural estimation hit back Leamer hard.

Joshua D. Angrist and Jörn-Steffen Pischke's *Credibility Revolution* is also famous like Leamer's article. They put their views into many fields to address the questions of whether the quality and the credibility

of empirical work have increased since Leamer's pessimistic assessment. Reported that Leamer's complaint that "hardly anyone takes anyone else's data analysis seriously" no longer seems justified.

They emphasize empirical microeconomics has experienced a credibility revolution, with a consequent increase in policy relevance and scientific impact. Sensitivity analysis, which Leamer used, played a role in this, but as they see it, the primary engine driving improvement has been a focus on the quality of empirical research designs. The advantages of a good research design are perhaps most easily apparent in research using random assignment, which not coincidentally includes some of the most influential microeconomic studies to appear in recent years. Many studies offer a powerful method for deriving results that are defensible both in the seminar room and in a legislative hearing. Like the results from randomized trials, quasi-experimental findings have filtered quickly into policy discussions and become part of a constructive give-and-take between the real world and the ivory tower, at least when it comes to applied microeconomics. They argue that a clear-eyed focus on research design is at the heart of the credibility revolution in empirical economics.

They sympathize with Leamer's view that much of the applied econometrics of the 1970s and early 1980s lacked credibility.

The deterrent effect of capital punishment, which had been analyzed in a series of influential papers by *Isaac Ehrlich*, seems typical of applied work in the period about which Leamer was writing. Most studies of this time used fairly short time series samples with strong trends common to both dependent and independent variables. In their view, the main problem with Ehrlich's work was the lack of a credible research design. Specifically, he failed to isolate a source of variation in execution rates that is likely to reveal causal effects on homicide rates.

After that, Angrist and Pischke found other examples of poor research design from this time period, which come from the literature on education production. Those literature is concerned with the causal effect of school inputs. The author criticizes that many education production studies from this period ignored the fact that inputs like class size and per-pupil expenditure are inherently linked. Because smaller classes cannot be had without spending more on teachers, it makes little sense to treat total expenditure (including teacher salaries) as a control variable when estimating the causal effect of class size. The main problem with this literature is not data mining, but rather the weak foundation for a causal interpretation of whatever specification authors might have favored.

Then, the author briefly reviews some other empirical works published from 1970s to 1980s, they pointed out some viewpoints that are no longer valid today and praised some research achievements that still play an important role today.

Next, they clarified the main points – why there's less con in econometrics today?

1. Better and More Data

Better data often engenders a fresh approach to long-standing research questions. Farther afield, improvements have come from a rapidly expanding reservoir of micro data in many countries. The use of administrative records has also grown.

2. Fewer Distractions

As in the exchange over capital punishment, others writing at about the same time often seemed distracted by concerns related to functional form and generalized least squares. Today's applied economists have the benefit of a less dogmatic understanding of regression analysis.

3. Better Research Design

The best of today's design-based studies make a strong institutional case, backed up with empirical evidence, for the variation thought to generate a useful natural experiment.

4. More Transparent Discussion of Research Design

In recent years, the notion that one's identification strategy—in other words, research design must be described and defended has filtered deeply into empirical practice.

5. Whither Sensitivity Analysis?

Design-based studies typically lead to a focused and much narrower specification analysis, targeted at specific threats to validity. Since the nature of the experiment is clear in these designs, the tack we should take when assessing validity is also clear.

And, with the examples accumulating in recent years, macroeconomics seems primed for a wave of empirical work using better designs. The theory-centric macro fortress appears increasingly hard to defend. They find the empirical results generated by a good research design more compelling than the conclusions derived from a good theory, but we also hope to see industrial organization move towards stronger and more transparent identification strategies in a structural framework.

The rise of the experimentalist paradigm has provoked a reaction, as revolutions do. There are two counter-revolutionary charges and authors' answer:

1. External Validity

The concern that evidence from a given experimental or quasi-experimental research design has little predictive value beyond the context of the original experiment?

– Applied micro fields are not unique in accumulating convincing empirical findings. The evidence on the power of monetary policy to influence the macroeconomy also seems reasonably convincing. Inconclusive or incomplete evidence on mechanisms does not void empirical evidence of predictive value. This point has long been understood in medicine, where clinical evidence of therapeutic effectiveness has for centuries run ahead of the theoretical understanding of disease.

2. Taking the “Econ” out of Econometrics too?

Experimentalists are playing small ball while big questions go unanswered?

– Related to the external validity critique is the claim that the experimentalist paradigm leads researchers to look for good experiments, regardless of whether the questions they address are important. In the empirical universe evidence accumulates across settings and study designs, ultimately producing some kind of consensus. Small ball sometimes wins big games. Many examples and many more speak eloquently for the wide applicability of a design-based approach.

Leamer drew an analogy between applied econometrics and classical experimentation, but his proposal for the use of extreme bounds analysis to bring the two closer is not the main reason why empirical work in economics has improved. Improvement has come mostly from better research designs either by virtue of outright experimentation or through the well-founded and careful implementation of quasi-experimental methods. Empirical work in this spirit has produced a credibility revolution in the fields of labor, public finance, and development economics over the past 20 years. Design-based revolutionaries have notched many successes, putting hard numbers on key parameters of interest to both policymakers and economic theorists. Imagine what could be learned were a similar wave to sweep the fields of macroeconomics and industrial organization.

A Structural Perspective on the Experimentalist School

Michael P. Keane wrote the paper to make some critical points and supplements to the previous article. He begins this article with a joke just like Leamer did but a bad one. He emphasizes that it is the mathematician who is really misguided, by expressing a false degree of certainty. His view, like Leamer's, or the economist in the joke, is that there is no way to escape the role of assumptions in statistical work, so our conclusions will always be contingent. Hence, we should be circumspect about our degree of knowledge.

What has always bothered him about the “experimentalist” school is the false sense of certainty it conveys. The basic idea is that if we have a “really good instrument” we can come up with “convincing” estimates of “causal effects” that are not “too sensitive to assumptions.” Elsewhere the author has written an extensive critique of this experimentalist perspective arguing it presents a false panacea and that all statistical inference relies on some untestable assumptions.

Then the author give a couple of examples of why natural experiments do not resolve this problem – If the experimental approach claims to be a “revolution”, it should be held to a high standard. As he said earlier, what bothers him is not the natural experiment approach *per se*, but rather the exaggerated claim that it enables us to attain relatively assumption-free statistical inference. *Clearly, Leamer is rejecting the whole notion of “objective” or “assumption free” inference that the experimentalist school claims to provide.*

Angrist and Pischke’s paper argues that labor has developed wide consensus on a broad range of questions while fields like macro and industrial organization remain in disarray – The author think, labor economists don’t agree on much of anything.

Actually, the Angrist and Pischke’s case for broad consensus/progress in labor is essentially rhetorical. They list many experimental papers that have obtained “convincing” and “influential” results but rarely state what the results are presumably because we’d see they are controversial. Keane only find a few specific results mentioned:

- 1) the Frisch elasticity is about one (few but he believe that);
- 2) neighborhood effects don’t matter for earnings (do you think there is consensus on that?);
- 3) smaller class sizes increase achievement (direction not too controversial magnitude certainlyis);
- 4) the death penalty doesn’t affect murder rates (what else is this controversial? Abortion? Gun control? Yankees vs. Red Sox?);
- 5) military service reduces civilian earnings (given the United States has an all-volunteer military, it seems that at least 1.4 million Americans may disagree).

If this is the body of “convincing” evidence the experimentalists have generated, he hardly think it constitutes a “revolution.”

Next, some viewpoints given by the author:

1. Economics Can Learn Something from Marketing

In contrast to labor economics, there is a field where broad consensus has actually been reached on many key issues over the past 20 years. I suspect most economists will be surprised to discover that this field is marketing. Marketing is characterized by three key features:

- 1) the structural paradigm is dominant;
- 2) the data are a lot better than in labor economics, due largely to the availability of consumer panels;
- 3) there is great emphasis on external validation.

2. Good Data Always Helps

There is another key point by Angrist and Pischke with which he agree: the experimentalist school has done a great service to empirical economics by forcing researchers to pay more attention to the sources of variation in data that identify their models. He agree that all econometric work, whether structural or not, should ideally be based on plausibly exogenous variation in the data.

3. The Ability to Do Controlled Experiments Does Not Obviate the Need for Theory

Where he most strongly disagree with Angrist and Pischke is their notion that empirical work can exist independently from, or occur prior to, economic theory. He argue that “we cannot even begin the systematic assembly of facts and empirical regularities without a preexisting theoretical framework that gives the facts meaning and tells us which facts we should establish.” He argue this is true not just in economics, but in all scientific disciplines. Thus, he found it interesting that Angrist and Pischke briefly extend their analysis outside of economics to the field of medicine and argue that an experimentalist approach has been fruitful

there as well: “in medicine . . . clinical evidence of therapeutic effectiveness has often run ahead of doctors’ theoretical understanding of disease.”

4. Different Approaches to Model Validation

The author would contend that specification searches are not a big problem in structural work – it just takes too long to do them. Faster computers won’t change this situation, as much of the time involved is programming time. For this reason, the best structural work has not involved extensive specification testing, but rather careful external validation exercises designed to persuade the audience to take the researcher’s model seriously.

A key point is that structural econometricians do not perform these exercises to persuade the audience that the model is “true.” He knows perfectly well that the models aren’t true. Validation exercises are used purely as a way to persuade the audience that a model may be a useful tool for prediction and policy evaluation.

Taking the Dogma out of Econometrics

Another paper written by *Aviv Nevo and Michael D. Whinston* commented on Angrist and Pischke’s article, they address their criticism of structural analysis and its use in industrial organization, and also offer some thoughts on why empirical analysis in industrial organization differs in such striking ways from that in fields such as labor, which have recently emphasized the methods favored by Angrist and Pischke.

Credible Identification and Structural Analysis: Complements, Not Substitutes

They firmly believe in the importance of credible inference, or “credible identification” and applaud the ingenious approaches to generating or identifying exogenous variation that often appear in the work using actual or quasi-experiments. In the discussions of identification and its credibility play a central role in the presentations and discussions, regardless of whether the paper’s approach is “structural” or not.

However, empirical analysis must deal not only with credible inference, but also with what might be called “generalization,” “extrapolation,” or “external validity”. This is where structural analysis comes in. Structural analysis is not a substitute for credible inference. Quite to the contrary, in general, structural analysis and credible identification are complements.

Structural analysis gives us a way to relate observations of responses to changes in the past to predict the responses to different changes in the future. It does so in two basic steps: First, it matches observed past behavior with a theoretical model to recover fundamental parameters such as preferences and technology. Then, the theoretical model is used to predict the responses to possible environmental changes, including those that have never happened before, under the assumption that the parameters are unchanged.

Given the many possible circumstances of a *merger*, it seems inevitable that many possible proposed mergers will not have been seen and studied before. In that case, to use past mergers to predict future outcomes, one needs a model. This model can be a statistical model or it can be an economic model. A statistical model, Angrist and Pischke’s preferred approach, would seek to predict the outcome of a merger using either a group of not-too-dissimilar mergers, or more generally fitting some prediction function based on a set of observable merger attributes.

An alternative approach to predicting a merger’s effect instead consists of using economic theory to simulate what the effect of the merger is likely to be. The basic idea is simple. Historical data are used to recover the structure of an economic model that consists of demand, supply, and competition. Identification of the fundamental parameters of this structure follows instrumental variable procedures similar to those in classical demand and supply estimation. Using the model, one can then simulate the effect of the merger under a variety of assumptions.

Besides of the discussion on Merger, they also suspected that researchers in industrial organization and those in fields where treatment effect methods are dominant would both do well to ask themselves where adoption of each others’ approaches could prove useful, while respecting the fact that differences in the markets, data and questions considered in different fields will call for differing approaches.

Their view is that the future of econometrics and applied microeconomic work is in combining careful design, credible inference, robust estimation methods, and thoughtful modeling. Therefore, any serious empirical researcher should build a toolkit consisting of different methods, to be used according to the specifics of the question being studied and the available data. That this should not be an either-or proposition seems quite obvious.

Reduced-Form Econometrics

The Limits of Inference with Theory

John Rust reviews *Kenneth I. Wolpin's* (2013) monograph *The Limits of Inference without Theory*. While I readily agree with Wolpin's basic premise that empirical work that eschews the role of economic theory faces unnecessary self-imposed limits relative to empirical work that embraces and tries to test and improve economic theory, it is important to be aware that the use of economic theory is not a panacea.

He point out that there are also serious limits to inference with theory:

- 1) there may be no truly “structural”(policy invariant) parameters, a key assumption underpinning the structural econometric approach that Wolpin and the Cowles Foundation have championed;
- 2) there is a curse of dimensionality that makes it very difficult for us to elucidate the detailed implications of economic theories, which is necessary to empirically implement and test these theories;
- 3) there is an identification problem that makes it impossible to decide between competing theories without imposing ad hoc auxiliary assumptions (such as parametric functional form assumptions);
- 4) there is a problem of multiplicity and indeterminacy of equilibria that limits the predictive empirical content of many economic theories, he conclude that though these are very challenging problems, agree with Wolpin and the Cowles Foundation that economists have far more to gain by trying to incorporate economic theory into empirical work and test and improve our theories than by rejecting theory and presuming that all interesting economic issues can be answered by well-designed controlled, randomized experiments and assuming that difficult questions of causality and evaluation of alternative hypothetical policies can be resolved by simply allowing the “data to speak for itself.”

He think the jury is still out as to whether statistical models or economic models will prove to be more useful and insightful. But if we can at least agree that there is a benefit to using some type of model, perhaps we are making progress. To the remaining skeptics and haters of structural modeling, the main message of Wolpin's book is clear: be not fearful of the unknown, but go boldly into that brave new world—or at least, try not to stand in the way of progress.

But Economics Is Not an Experimental Science

“The fact is, economics is not an experimental science and cannot be.” Christopher A. Sims argued that “natural” experiments and “quasi” experiments are not in fact experiments. They are rhetorical devices that are often invoked to avoid having to confront real econometric difficulties. Natural, quasi-, and computational experiments, as well as regression discontinuity design, can all, when well applied, be useful, but none are panaceas.

Recent enthusiasm for single-equation linear instrumental variables approaches in applied microeconomics has led many in these fields to avoid under-taking research that would require them to think formally and carefully about the central issues of non-experimental inference. As the core of econometrics. Providing empirically grounded policy advice necessarily involves confronting these difficult central issues. If applied economists narrow the focus of their research and critical reading to various forms of pseudo-experimental

analysis, the profession loses a good part of its ability to provide advice about the effects and uncertainties surrounding policy issues.

Natural experiments, difference-in-difference, and regression discontinuity design are good ideas. They have not taken the con out of econometrics – in fact, as with any popular econometric technique, they in some cases have become the vector by which “con” is introduced into applied studies. Furthermore, overenthusiasm about these methods, when it leads to claims that single-equation linear models with robust standard errors are all we ever really need, can lead to our training applied economists who do not understand fully how to model a dataset. This is especially regrettable because increased computing power – and the new methods of inference that are arising to take advantage of this power – make such narrow, overly simplified approaches to data analysis increasingly obsolete.

My Thoughts and Criticisms

When conducting a **reduced-form analysis**, an economist uses economic theory, intuition, and other considerations (e.g., prior research results) to specify an equation describing hypothesized relationships between a dependent variable of interest and some explanatory variables. The equation is then estimated using econometric methods to see if the relationships are borne out by the data. While relatively simple to implement, non-structural analyses have a disadvantage in that they do not allow economists to rigorously quantify the effects of possible changes in public policies on individual or aggregate behavior.

This is a relatively traditional method of econometrics, which is enduring but tests economists’ economic intuition. As the old saying goes: articles are naturally made without artificial work. They are acquired by highly skilled people by chance. Only with profound literary skills and creative inspiration can they write good articles.

The development of econometrics can not be separated from the solid foundation laid by these traditional studies. It is just some inspirational talents who clear the convincing and predictive estimation results from the complex problems related to people’s trade-off decisions that seem to have no theoretical basis and practical connection, which has played a great role in the early research and the development of econometrics.

In the case of a **structural analysis**, an economist formulates an economic model to explain some observed behavior, chooses specific functional forms for the model’s components and explanatory variables to include, and typically estimates the model as a whole. Structural econometric analysis is usually more difficult to implement than the non-structural approach and requires more computing power and programming skill. However, it enables the simulation of the effects of alternative public policies and other hypothetical scenarios.

Of course, genius can not always be full of inspiration. Not everyone needs that kind of inspiration. Structural econometric estimation method combines the cutting-edge achievements of statistics, computer science and many other disciplines - no single discipline in the world can become an island; This provides a steady and powerful paradigm for countless latecomers. How to conduct econometric research? The method of structural estimation has become a new trend, every econometrist seems to grasp the relevant methods as a powerful empirical means in research.

I strongly agree with *Aviv Nevo and Michael D. Whinston*: “*This should not be an either-or proposition seems quite obvious.*” In economic research, inspiration, ingenious data collection and scientific analysis methods are the qualities that economists must possess.

Reference

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