

## Problem Set 1 - Paulo Henrique Ramos

As Keane points out, through several examples, both structural and reduced-form estimation should be grounded in economic theory to make sense, and their validity equally depends on the validity of the underlying theoretical assumptions used. But they are methodologically very different and the questions and tasks each method can handle also differ. Thus, both methods can be equally valid, depending on the task at hand, and the choice of which method to use should be connected to the question or task the researcher has. In other words, I defend the use of both methods generally, with the choice in each specific situation depending on how well the method can handle the task.

Regarding reduced-form strategies, as Keane shows, these estimates do not tell us anything about the underlying mechanisms at work. Even if the causal effect from one variable to the other is accepted, understanding the mechanisms that create this causality is necessary to guide policy (p. 5), since a policy change could change the underlying mechanism that was generating the causality found, and reverse it, thereby causing unexpected effects of a policy intervention. Also, as Keane points out in section 4, without an underlying theory, it is not possible to understand the results of reduced-form estimation. This theory is exactly what makes explicit the underlying mechanism. Thus, for policy intervention and understanding the economic mechanisms at work, for instance, structural work should be the choice.

But there are cases where the reduced-form strategy is enough to obtain the desired results – if the underlying theory is made explicit, since, without it, as put by Keane, there is no way to interpret the results. We might have a theoretical model in mind, but do not want to estimate the whole model, since we are only interested in a certain causal relation that the model predicts. For instance, Syverson (2004) explicitly lays out a theoretical model relating substitutability to productivity in the concrete industry, derive comparative statics and runs reduced-form regressions to check the causal relations. The main objective was not to do counterfactuals or policy interventions, but just to document a (demand-side) source of productivity dispersion. A reduced-form strategy, grounded in economic theory, allowed him to obtain meaningful results, interpretable in light of his underlying model.

I would also like to temper Keane's critique related to heterogeneous effects (p. 5). He points out that estimates in the draft lottery study overstate the negative effects of military service. But his example shows what the "reduced-form theory" predicts in this case: the coefficient is the average effect on those affected by the instrument (in this case, the type 2). You still need assumptions to get this, and those should be grounded in economic theory, but sometimes that effect might be exactly what you need.

I also would like to temper Rust's point that approximation theory is better to judge structural estimation (p. 22). Even though we should evaluate how well our model approximates the "true" model, we still need to obtain the parameters of the approximated model and, for that, econometrics and statistic theory are still helpful to assess whether we might be correctly identifying those parameters. I share Keane's view in section 3: seriously analyzing how variation in the data generates the parameter values in a structural estimation is a welcome critique. To mention an example where this analysis might raise concerns on the model: in a structural simple logit demand model, substitution patterns are identified solely by market shares, and this might be against the economic theory behind the model (e.g., if these patterns should be related to product characteristics, and these are not related to shares).

Also, although I share Rust's view that there is no superior approach in general (p. 24), I would like to temper his point that the choice depends on the task at hand, and skills and preferences of the researcher. I believe the decision should hinge much more on the suitability of the method to answer to the question at hand. Sure, there might be some cases where both approaches are suitable, but, as exemplified by Keane in section 5 (problems where there are no possible instruments), there are cases where skills and preferences should not determine the method, because only one is suitable.

In sum, I believe both approaches are valid, and the choice depends on the task the researcher faces; and, in all cases, economic theory will play an essential role in indicating the best method.

## **References**

- Chad Syverson. 2004. "Market Structure and Productivity: A Concrete Example." *Journal of Political Economy*, no. 6: 1181.