**Structural Estimation vs. Reduced Form Estimation - Why The Competition?**

In the article "Structural vs. atheoretic appraches to econometrics", Michael Keane makes a strong case for structural estimation and points out many of the caveats and examples of bad research in reduced form estimation. This is important, since the structuralist approach does seem to deserve more attention within academia. He draws on many examples and research where structural estimation has made significant advances in our economic understanding of the causal mechanisms, and criticises reduced form estimation precisely for the lack thereof. Nevertheless, in trying to make a strong case for structural estimation over reduced form, he ends up being too categorical and dismissive of a line of research that is also extremely useful and provides insights into different types of questions.

Keane insists on the idea that "natural experiments" do not offer a simple and "assumption free" way to learn about economic relationships, but that it is indeed the case that even reduced form work relies heavily on a priori-theoretical assumptions. I agree with this view, but I believe that so do reduced form empirical economists! He goes on to quote Angrist and Krueger who say that they "let the data speak", which Keane interprets as them saying that they manipulate data as if they had no prior. I believe this is an unjust interpretation, since no applied economist goes on to estimate any (causal) relationship without an idea why this relationship might exist in the first place, or what might be driving it. Just as Galileo decided to roll his ball down inclined planes because he had a prior, similarly do reduced form economists try to estimate equilibrium causal effects because they have some economic prior in mind. To me it seems like here Keane is attacking reduced form empiricists them on the basis of points that they do not make.

Furthermore, Keane talks a lot about heterogeneous effects of programmes/policies and the inadequacy of even completely valid instrumental variables to estimate anything but a local average treatment effect. These are indeed valid concerns, but they are also almost always discussed in good reduced-form empirical papers. There is also the non-compliance issue (people who are randomly selected into the programme but do-not end up receiving the treatment), as well as the issue of imperfect randomisation or people from the control group going into treatment. We should definitely worry about the latter, but regarding non-compliance, sometimes the identification of the effects of being offered treatment is exactly what we want. Often social programmes cannot force the allocated subjects to receive treatment (e.g. job training), so if we are to estimate the equilibrium effects of job training services offered, the intention to treat is exactly what we want, and with potentially heterogeneous impacts, the effect will be an average impact over the population for which randomisation took place (and we could infer that the individuals for whom gains of the programme are higher will be the compliant group, both in the experiment and reality). Again, the method depends on the type of question we are interested in.

Nevertheless, any welfare programme might change incentives to obtain education, future opportunities, as well as equilibrium wages and incentives to work - and this limits the generalisable lessons we can learn from particular experiments. To do so, we would have to combine the experimental results with an economic model of household behaviour, to be able to evaluate ceteris paribus, as well as equilibrium effects of a scaled up policy. This is precisely what Keane is aiming at with his example of estimating the effects of maternal contact time on child cognitive abilities in a

generalisable way, and I fully agree with him that there are questions and mechanisms that pure reduced-form research just cannot shed light on.

In his comments on Keane's paper, John Rust roughly supports Keane's stance and makes the case for why, even though structural models are always misspecified to some extent and make some functional form assumptions (e.g. Gorman's separability), purely statistical models are no less arbitrary in their linearity and the decisions which variables or interactions should be in and which should be left out. Still, Rust makes the argument less personal and more productive by acknowledging that "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." Where I feel his argument falls short is when he says that "there may not be any possibility of reconciliation or easy communication between the two camps".

I would agree with researchers such as Blundell (2009) or Low and Meghir (2017) who assert that the best way to move forward is to acknowledge that the choice of method should depend the type of question, the type and quality of data available and the mechanism how individuals are allocated or receive the policy - and, most importantly, that, progress in applied economics will be hard to achieve without both sides of empirical work. Recent work combining structural modelling and randomised experiments as an aid in estimation seems to be combining the best of both worlds, while experimental evidence should also be used to validate structural models.

References:
Blundell, Richard (2009), "Comments on: Michael P. Keane ' Structural vs. Atheoretic Approaches to Econometrics'", Journal of Econometrics

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