MACS 40200 - Problem Set 1

Yewon Kim

January 10, 2017

I am studying quantitative marketing as a Ph.D. student, and my discipline cares very much about consumer heterogeneity. Given rich individual-level panel data that we use, once I thought it was almost right to take a structural approach that could offer rich heterogeneity argument and policy evaluation. As a marketing researcher, modeling consumer utility function, building up choice outcomes from the model, fitting the predicted outcomes to the data to estimate parameters, and using those parameters to evaluate current managerial policies seemed natural. However, as I studied recent literature that took computationally heavy structural approach (for example, Berry Levinsohn, and Pakes (1995) that introduced the famous BLP model in Industrial Organizations), I have to admit I became somewhat skeptical about the validity of the model. Would these layers of assumptions and equations be able to recover the actual parameters that affect fundamental utility of consumers? If each layer contains errors, would the final outcome be too much distorted from the true parameters (if they exist) due to the cumulative error?

Keane(2010) illustrates about how structural approach is different from - or even better than - atheoretic approach. Issues he addresses include the following: 1) even atheoretic approach is based on certain assumptions that are implicit (and thus can be more problematic), 2) estimates from atheoretic approach are not interpretable as a true 'causal' driver, and 3) there are cases in which parameters of interests cannot be identified even with ideal instruments. I am particularly convinced by his argument about the interpretibility of the estimates from Angrist(1990); he points out that even the lottery-based draft cannot be a truly random thus ideal instrument, given that selection bias might have occurred in many ways by those who changed their human capital investment based on draft eligibility status (either staying at school or giving up). This surely contaminates the estimate and thus the estimate fails to represent the sole effect of military service on future earnings as any other naive estimates do. However, I do not agree with his argument that the estimates that experimentalist approach recovers do not have meaningful interpretation. Back to Angrist(1999) example, I agree that the model clearly missed out that there might have been a group of people who chose to stay at school to avoid drafts, which increases the earning gap between veterans and civilians ex post. However, low human capital investment by people who were eligible before they joined army can be seen as a part of the effect of military service experience if we define the effect general enough. Similarly, I agree that the estimate cannot pinpoint through which specific mechanism that wartime participation affected veterans' earnings, but to me it does not mean that it fails to recover any meaningful effect at all. If we forget about the selection bias for a moment, the study showed that, no matter what the actual causes were, military service lowered the veterans' income after they were discharged in various possible channels. I believe that it is a choice of a researcher how narrowly or broadly he or she defines the 'cause.'

I also agree that ill-defined natural experiments can be dangerous. Clearly, the examples he demonstrates on page 12 (small firms vs. large firms, men vs. women, people under 65 vs. peo-

ple over 65) are simply too wrong to be true. Indeed, defining natural experiments as 'natural' is mostly done by researchers, which is in nature equivalent to making assumptions. However, I agree more with Rust(2010) that existence of assumptions in atheoretical approach does not imply that the assumptions made by structural approach are not very strong (p.24). As Rust says, structural econometricians rely on economic theory more heavily and pay strong assumptions to get much deeper and economically meaningful results. And it implies that the danger of making wrong assumptions, like in the case of absurdly wrong natural experiments, is also present as a serious problem for structural econometricians.

I believe that both approaches are surely complementary to each other. As Keane(2010) mentions, these two approaches shed lights on the issues that the other approach could not have seen. As a young marketing researcher, I would probably spend more time on structural estimation than on reduced form estimation given the inherent interests of the discipline. However, I would not exclude any works that are done with experimentalist approach; I would be able to get a broader sense about how the effect of interests is directed, and then choose a tool that best serves my purpose.