

Comments on Aradillas-Lopez and Tamer

Patrick Bajari
University of Minnesota and NBER
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In this short note, I will make some comments on the Aradillas-Lopez and Tamer manuscript. Most of my effort in this comment will be directed towards open research questions that exist in the literature, rather than critiquing the particulars of the current paper. I as a researcher, I am always more interested in the question of where can we possibly go from here rather than dwelling on the limitations of a particular paper. I will start off by describing the contribution of this paper to the growing literature on the econometric analysis of games. Next, I will make some suggestions for some possible, fairly immediate extensions of the current research. Finally, I will discuss some general outstanding issues that exist in the literature on estimating games.

Most of my comments will be made from the perspective of an applied economist. I view myself as a potential end user of the methods that Aradillas-Lopez and Tamer are creating. I hope that my comments may suggest some directions for extensions that will be useful to econometric theorists, who serve as upstream developers of these exciting new tools.

Contribution of the paper

This paper is part of a growing literature at the intersection of econometrics and game theory. An important class of models in this literature are generalizations of standard discrete choice models, such as the conditional logit, which allow for strategic interactions. In applied microeconomics, and especially empirical industrial organization, we are frequently confronted with problems where the discrete choices of agents are determined simultaneously. For example, starting with Bresnahan and Reiss (1990,1991), entry in spatially separated markets has typically been modeled as a simultaneous system of discrete choice models. Bresnahan and Reiss noted that entry is naturally modeled as a discrete choice. Economic theory suggests that firms should enter if profits are greater than zero, and not enter if profits are less than zero. Therefore, it is natural to include demand shifters, such as the number of consumers in the market, their

income and cost shifters, such as the wage rate across markets in the discrete choice model. However, in many applied problems, the market structure is concentrated. Therefore, the entry decisions of the agents cannot be modeled in isolation. For example, a reasonable model of the decision by Wells Fargo to open a branch in mid sized city should not be viewed in isolation of Bank of America's decision. To account for this interaction, Bresnahan and Reiss would have us include Bank of America's entry decision as a right hand side variable in Well's Fargo's utility, and vice versa. Consistent with economic theory, we assume that the observed choices are a Nash equilibrium to payoffs defined in this manner.

Econometrically, these entry games boil down to a simultaneous system of discrete choice models. Researchers quickly realized that estimating these models was a difficult problem. For a given set of parameters, covariates and random preference shocks, these models may have multiple Nash equilibrium. Even worse, for certain specifications of the game, there may be no Nash equilibrium in pure strategies. Therefore, these discrete games may generate one, zero or many predicted outcomes (in pure strategies). Obviously, a straightforward application of maximum likelihood or GMM is not possible in these settings.

A novel approach to estimating these models was proposed by Tamer (2002). He notice that while discrete entry games do not predict unique equilibrium, the assumption of Nash equilibrium could be used to bound the probability of observing various patterns of entry. Using these bounds, he proposed an estimator which would allow us to estimate the set of parameters that are consistent with the observed frequencies of entry decisions.

This paper extends earlier work by Tamer (2002) on the analysis of entry games and by Aradillas-Lopez (2007) on games of private information. However, these earlier papers make the assumption that the observed behavior is a Nash equilibrium, or Bayes-Nash in the case of private information games. Assuming that agents are playing a Nash equilibrium in applied work is controversial. First, it assumes that agents are able to engage in rational, self-interested behavior. While most economists agree that this is a useful starting point for our analysis, we also realize that it is an imperfect approximation to the complexities of human behavior. Second, even assuming that agents are rational, the assumption of Nash equilibrium may be controversial. Nash equilibrium assumes

that agents act “as if” they are able to make a best response to their common prior about the equilibrium actions. However, it is unclear how agents coordinate on which equilibrium to play. It is well known that discrete games can have many Nash equilibria, particularly as the number of strategies and players grow large. It is hard to believe that agents are able to formally solve the game from first principles, as in economic theory and determine which equilibrium is “correct”. After all, the leading theorists have not been able to determine a process for selecting a unique equilibrium to a game (and it is possible that they never will). Therefore, it is a strong assumption to assume that agents from first principles have been able to resolve a problem which has escaped solution, despite considerable effort, by our some of our best theorists.

Aradillas-Lopez and Tamer argue that it may be worthwhile to impose a less restrictive notion of rationality proposed by Bernheim (1984) and Pearce (1984) which has been referred to as rationalizability in the literature. Level-k rationalizability can be understood as behavior that can be rationalized by some beliefs that survive at least k-1 steps of iterated deletion of dominated strategies. For example, Level-1 rationalizability imposes the restriction that an agent’s actions must be a best response to some set of beliefs. This relaxes the Nash equilibrium assumption that agents act as if they have coordinated on a common set of beliefs about the equilibrium actions and make a set of best responses. Rationalizability is arguably the weakest solution concept that can be imposed on a game. Studying the implications of the weakest modeling assumptions from economic theory seems consistent with the spirit of the bounds literature, where the econometrician takes a very conservative approach to imposing assumptions on the data generating process. Overall, this is an excellent research question and the authors deserve a lot of credit for opening this avenue of research.

More broadly, I am very pleased to see Aradillas-Lopez, Tamer and other young econometricians work on econometric models of strategic interactions. Empirical industrial organization economists typically conceptualize the data generating process in most markets as being a game. We think of the data generating process in this way because there is a wealth of evidence from previous descriptive work, antitrust cases and talking with industry participants that agents act strategically. Firms certainly realize that their profits and other outcomes are interdependent and worry a great deal about the

actions of their competitors. They may or may not behave in perfect accordance with the hyper rational models of economic theory. However, there is something to be said for working with the models that are available, if for no other reason than to take game theory to the data very directly to see if it works.

It is my hope that the entry by these talented young scholars will bring greater econometric precision and formality to the analysis of strategic interaction by empirical IO economists and applied microeconomists more generally. As the current paper demonstrates, there are a wealth of leading questions and models from economic theory that have yet to be explored econometrically. I am very excited to see where this literature will lead over the next decade.

Possible Extensions

The current manuscript is well done and is largely self-contained. However, there is clearly work to be done on extending the contributions contained in this paper. In this section, I will describe what I believe are low hanging fruit in terms of extensions. I conjecture that some of these extensions have occurred to Aradillas-Lopez and Tamer. However, I believe that it is useful to include them in my discussion in the hope that it might stimulate further research.

First, this paper cries out for an application. The authors are econometricians, and theories of specialization suggest that their comparative advantage is in proving econometric theorems about the difference between rationalizability and Nash equilibrium at a formal level. However, as a practitioner, I would like to see these methods taken to data in a substantive application. Could we ever clearly reject the assumption of rationalizability from Nash behavior for any set of game (not just entry problems)? How does the behavior systematically differ from Nash? Is the difference between Nash and rationalizability of importance for an applied policy or welfare analysis question? In passing, I note that Goldfarb and Yang (2007) have independently made some first steps in this direction and have found evidence suggesting that large markets exhibit “more” rationality in a sense not unlike having a high level-k of rationalizability. But clearly, more work is needed. This question seems like low hanging fruit for an entrepreneurial young applied micro-economist.

Second, I would prefer to see an analysis of rationalizability for games other than entry games. As an applied researcher, I am somewhat skeptical that entry can be plausibly modeled as a static decision. Entry typically requires firms to incur fixed costs and many investments are plausibly modeled as irreversible. Furthermore, the firms may have a learning curve. As a result of these factors, entering firms may not always expect to make profits in the first period. A more reasonable model of their entry decision should involve forward looking behavior in which firms attempt to forecast their expected discounted profits over their lifetime in a particular market. Finally, entry is almost never simultaneous. There is no date zero at which markets open and firms must decide once and for all whether to enter. More commonly, entry data sets have a panel structure and we observed the entry (and exit) of firms over a period of years. The one case where entry may reasonably be modeled as a static, simultaneous problem is in auctions, as in Athey, Levin and Seira (2004) and Krasnokutskaya and Seim (2007).

This suggests two possible extensions. First, it would be useful to think about games which more reasonably might be taken to data. Second, if we do wish to study entry, perhaps we could extend our analysis of rationalizability to a dynamic setting.

Third, much of the work in the current paper focuses on games with two players and two strategies. Data sets in substantive applications often have a larger number of players and strategies. For example, in Krasnokutskaya and Seim (2007), the authors identify the set of potential entrants as the plan holders to the contracts. There are often many more than two potential plan holders. The methods proposed in this paper may require the researcher to analytically characterize the subset of the payoff space in which different actions are rationalizable. This becomes increasingly difficult as the number of players or the cardinality of the choice set increases. For games with 4 players and two strategies, the payoff matrix corresponds to a vector with $4 \cdot 2^4 = 64$ elements.

Finding the subspaces, analogous to figure 1 in a 64 dimensional subspace is a daunting task that is likely to be infeasible. In order to operationalize this procedure in higher dimensions, an alternative approach that exploits numerical methods is likely to be required.

Finally, this paper, and the bounds estimation literature more generally needs to develop methods for dealing with unobserved heterogeneity in these models. In our

applications, if we see a positive relationship between the actions of two agents, this could be because we have failed to include all of the relevant information about demand, costs or other payoff relevant variables in our analysis. Our ability to control for these factors is almost always imperfect outside of laboratory experiments. The recent paper by Pakes, Porter, Ho and Ishii (2005) takes important steps in this direction, but clearly more work needs to be done.

Broader Issues

More broadly, a skeptical observer might ask, who cares about the econometric analysis of games? Isn't this just an entirely academic exercise? We know that Nash equilibrium, and probably rationalizability as well, assumes too much rationality on the part of agents. Given that these assumptions strain credibility, who cares about the results?

I think we care about these models for three reasons. First, in empirical industrial organization and regulation, we commonly need to conduct applied welfare analysis. For example, an economist at the Federal Trade Commission would like know how much a proposed merger will increase or decrease consumer surplus. A regulator at the Federal Communications Commission would like to know whether allowing for the portability of cell phone numbers will increase competition and consumer welfare enough to justify the increased cost for the industry. In most markets, it is not plausible to assume that all actors are perfectly rational. On the other hand, as an economist, I do not know of a practical option for measuring welfare other than to carefully calculate consumer and producer surplus.

In these applied policy problems, it is tempting to only use qualitative implications from applied theory in order to inform policy. Frequently, the economists survey the applied IO theory literature and ask what are the qualitative predictions of various theoretical models and only use these qualitative predictions.

I believe that structural estimation of these models adds value beyond consulting the theory literature. Forcing the model to confront the data is a learning experience. It is often surprisingly difficult to make a particular structural econometric model, derived from an a priori reasonable theory, fit the data with reasonable parameter values. Making

the data confront the model in this fashion has always helped me to learn about the usefulness of competing theories.

Moreover, if we believe that firms are very irrational, in principal, we should be able to help them considerably improve profits. While there is a market for academic economists to consult firms on pricing and other strategic decisions, to be fair, it is a reasonably thin market. In markets with sufficient feedback (i.e. stupid decisions result in the loss of profits and potential exit), I believe that rationality is a reasonable starting point. One of the lessons of the literature of learning and evolution in games is that if feedback from the economic environment pushes firms in the right direction, the only thing that behavior can converge to (assuming it converges) is a Nash equilibrium.

A second reason to care about these models is that as matter of positive economics, it is important to force game theory to confront the data. In applied microeconomics, it is common to test the implications of game theory by first deriving a comparative static of the game (e.g. margins should fall if more players enter) and then test this through the use of standard statistical tools such as regression (e.g. regress margins on the number of firms). While I believe that this sort of descriptive work is useful, it is often a fairly weak test of the theory.

A far more demanding task of the theory to find reasonable parameter values that are capable of matching the data. Even if we prove that our model is just identified, or slightly underidentified, that does not mean that we can rationalize the data with parameter values that are within the realm of plausibility. For example, we might find that we can only rationalize entry with profit margins of greater than 60 percent and residual demand elasticities near one. In a highly competitive industry, this would be evidence against even an underidentified model. In substantive applied problems, researchers are frequently surprised to find how well they can fit some aspects of the data, and how poorly they can fit others. I believe that this exercise informs us about how we should extend and modify our theories. However, we can only learn this by being bold and taking our models to the data in the most demanding way possible by structurally estimating them.

A third reason that these models are important is because we need them to do counterfactuals. For example, in the merger analysis or number portability questions

discussed above, at the time the regulators made the decisions, there was available experiment where a treatment and control group had received the regulation at random. In order to make these counterfactual predictions, in structural applied work, we use the model to make these extrapolations. Such exercises are by necessity a fairly bold extrapolation. However, the fact that we have bothered to write down a model and carefully estimate its parameters adds intellectual clarity and rigor to this exercise. Antitrust agencies and market regulators, if they are acting in order to maximize welfare, must either implicitly or explicitly makes assumptions that allow them to forecast the impact of out of sample policy experiments.

Taking as given that structural models of games are useful to estimate, I believe that it is important for us to use these models on important and substantive applications. As academics, it is frequently too tempting to examine problems of little economic importance, but where the data is clean and the identifying assumptions are reasonably uncontroversial.

As an applied researcher, I find that the major limitation of the bounds approach is that it is not possible to simulate the models given the parameter estimates. For a fixed payoff matrix, many games will give us multiple Nash equilibrium and the bounds literature, by design, sidesteps the question of which equilibrium is played in the data. However, without taking stance on equilibrium selection, I cannot simulate behavior using the model.

As a result, it is not clear how to use bounds estimator to conduct applied welfare analysis or conduct policy counterfactuals. Moreover, I cannot use the parameter estimates to conduct out of sample testing and validation as in Todd and Wolpin (2002). This limits my ability as a researcher to test my theories to assess their strong and weak points.

Clearly, these issues are extremely difficult and it may take a series of papers by multiple researchers to find a satisfactory solution. Also, I am by no means the first to raise this concern. However, I hope that upstream developers, such as Aradillas-Lopez and Tamer, along with other young econometric may provide some guidance on these practical issues.

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