

# Rejoinder

Guido Imbens

I am very grateful for the comments on the paper and the careful reading that went into them. Since instrumental variables concepts and methods have become popular in a range of substantive areas beyond economics, there have been a number of significant contributions from other areas, and it is useful to have the different perspectives on these methods that these comments reflect. I will attempt to address some of the issues raised in the comments, but many of these comments will undoubtedly stimulate new studies, as the general area of research on causal inference in observational studies continues to flourish.

## KITAGAWA: “INSTRUMENTAL VARIABLES BEFORE AND LATER”

I am grateful for the kind words by Kitagawa. He has been doing very interesting work on testing for validity of instrumental variables in recent years (e.g., Kitagawa, 2010, 2013) that will undoubtedly be influential in the literature. I am also glad that Kitagawa likes my summary of the differences between econometric and statistical approaches to causality as “choice versus chance.”

Kitagawa’s comments on the impact of the local average treatment effect literature on economic practice agree with my views. As emphasized in the paper, the LATE concept was never intended to change the question of interest, but to clarify what we could learn from the data. Nevertheless, in some cases the LATE may well be representative of a subpopulation that is of substantial interest on its own. Consider the draft lottery example (Angrist, 1990; Hearst, Newman and Hully, 1986) where the compliers are the men who served, or would have served, in the military, because of their draft lottery number. Arguably, this is the group on the margin for whom the effect of military service is most interesting. Similarly, in the Angrist and Krueger (1991) study of the returns to education using compulsory schooling laws as an instrument, the compliers are

the individuals for whom schooling decisions are affected by compulsory schooling laws, again arguably an interesting subpopulation for educational policies that are often targeted at those receiving lower levels of education. Nonetheless, in general the subpopulation of compliers is not chosen for its interest, but because we can hope to learn something about them. It is about the primacy of internal validity over external validity (Shadish, Cook and Campbell, 2002).

Kitagawa discusses instrumental variables in the context of another example that, like the supply-and-demand example I discuss in the paper, is a classic one, that of the estimation of returns on inputs in a production function. Specifically, he focuses on the causal effect of labor inputs on output. The starting point for an economist is exactly as Kitagawa describes: firms do not choose input levels randomly, but choose them optimally, for example, to maximize profits. This leads quickly to settings where we cannot simply regress output on inputs if we are interested in the causal effect of input on output. Moreover, the context in combination with economic theory on firm behavior suggests where a researcher might look for instruments that satisfy the exclusion restriction, namely cost variables that affect the choice of input levels but that affect output only through their effect on input levels.

In his comments, Kitagawa also distinguishes between various objectives for the researcher. If the goal of the researcher is what he calls “scientific reporting,” Kitagawa agrees with my recommendation to report both estimates of the local average treatment effect and bounds on the overall average treatment effect. If, on the other hand, the goal is directly to make a decision, say, on whether to extend the treatment to the entire population or not, he advocates a decision theoretic approach, either Bayesian along the lines of Chamberlain (2011), or the type of Manski “data-alone” frequentist approach. I agree with that, and I think the distinction between scientific reporting and decision making is a useful one to bear in mind.

## RICHARDSON AND ROBINS: “ACE BOUNDS; SEMS WITH EQUILIBRIUM CONDITIONS”

Richardson and Robins make two sets of comments, one about bounds on the average causal effect (ACE), and one about simultaneous equations models (SEMs).

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In the discussion on bounds, they formulate four sets of assumptions, captured by different graphical models that allow for construction of the same set of bounds. They relate these assumptions to their novel Single World Intervention Graphs (SWIGs). I find the SWIGs an intriguing approach, and one that might help make the graphical approach more relevant for researchers interested in causal effects. One concern I have with the discussion of the four sets of assumptions is that it is not clear when there is a substantively important difference between the assumptions. For example, I find it difficult to think of substantive applications where the independencies hold one pair at a time [Assumption (iii)], but not joint independence [Assumption (i)].

The discussion on market equilibrium and bicausal models is very interesting and stimulating. I am happy to see Richardson and Robins endorse my interpretation of structural equations in terms of potential outcomes. Although, as the authors point out, this interpretation of structural equations is not universal, in my view, partly based on conversations with other economists, it is the leading one in economics. The discussions of normalization issues that the authors refer to generally arise in the context of estimation in settings where there are multiple instruments. In that case, the difference between estimation methods such as limited information maximum likelihood (LIML, going back to Anderson and Rubin, 1948), and two stage least squares (TSLS) matter. In the recent literature on weak instruments, these differences have been shown to potentially matter a great deal. Staiger and Stock (1997) is a key paper, and Stock and Andrews (2005) provide an overview.

Although economists routinely use the supply-and-demand example in textbooks and teaching, most discussions no longer explicitly discuss where the equilibrium that is assumed arises from, making the work in this area more difficult to access for researchers from other areas than it need to be. The model used by Richardson (1996) where the data come from a discrete approximation to a finer recursive model appears to capture well the mechanisms researchers implicitly have in mind. See Bergstrom (1966) for a related discussion in the older economics literature discussing the relationship between nonrecursive (bicausal) models in discrete time and recursive continuous time models.

**SHPITSER: “CAUSAL GRAPHS: ADDRESSING THE CONFOUNDING PROBLEM WITHOUT INSTRUMENTS OR IGNORABILITY”**

Shpitser is concerned that I did not discuss the growing literature on causal graphical models. This is a

very interesting and rapidly expanding literature that has important antecedents (Wright, 1921) that were influential in the economics literature, and where Richardson and Shpitser have made major contributions. However, I saw the focus of my paper on an econometrics perspective on instrumental variables, and there graphical models do not currently play a major role. It is an interesting question why economists have not felt that graphical models have much to offer them. Pearl (2013) has also raised this question, and concludes somewhat dismissively that: “economists are still scared of graphs.” He sees this as an “educational deficiency,” and writes that “This educational impairment is the main factor that prevents economists from appreciating much of the recent progress in causal inference” (Pearl, 2013, page 8).

My view on the lack of use in the econometrics literature on the graphical models is more sanguine. I see substantial evidence that as a group economists are willing to adopt new methods from other disciplines that are viewed as useful in practice. There are many examples of this even within the area of causal inference. The rapid adoption of the Rubin potential outcome approach starting in the early 1990s with Heckman (1990) and Manski (1990) is one, as is the by now widespread use of matching and propensity score methods, and the current boom in studies using methods associated with regression discontinuity designs that were originally developed in the psychology literature (see Cook, 2008, for a historical overview). In contrast, the causal graphs have not caught on in economics. In my view a major reason is that there have been few compelling applications of causal graphs to social science questions where the causal-graph approach has generated novel analyses or prevented researchers from making mistakes that other frameworks might have encouraged them to make. A second reason may be that some assumptions are not easy to incorporate in the graphical approach. Monotonicity, which Swanson and Hernán are particularly concerned with in their comments, and which plays a key role in instrumental variables analyses, is difficult to capture in a causal graph. See the discussion in Imbens and Rubin (1995).

Let me flesh out the first part of this argument. There are thousands of empirical studies in economics where researchers use instrumental variables methods. Implicitly, they may have a causal graph like Figure 1 in the main paper, or Figure 1(c) in the Shpitser comment, in mind. Often there is considerable discussion in

a particular application whether the two key assumptions that there is no direct effect of  $Z_i$  on  $Y_i^{\text{obs}}$  (no arrow from  $Z_i$  to  $Y_i$ , and no confounding of the effect of  $Z_i$  on  $Y_i^{\text{obs}}$  (no unobserved common cause of  $Z_i$  and  $Y_i$ ) are plausible. In observational studies in social science, both these assumptions tend to be controversial. In this relatively simple setting, I do not see the causal graphs as adding much to either the understanding of the problem, or to the analyses. Similarly, there are thousands of empirical studies in economics where researchers use matching type methods based on the assumption of no unmeasured confounders, and where implicitly they may have a causal graph like Figure 1(b) in mind. Again, the assumptions underlying such a graph are typically controversial and researchers often put in substantial effort in arguing for the absence of unobserved confounders. In this case, again I fail to see what using a causal-graph approach would add in practice. Now consider a more complicated setting such as the “hypothetical longitudinal study represented by the causal graph shown in Figure 2,” in the comment by Shpitser, or Figure 1 in Pearl (1995). Here, identification questions are substantially more complex, and there is a strong case that the graph-based analyses have more to contribute. However, I am concerned about the relevance of such examples in social science settings. I would like to see more substantive, rather than hypothetical, applications where a graph such as that in Figure 2 could be argued to capture the causal structure. There are a large number of assumptions coded into such graphs, and given the difficulty in practice to argue for the absences of one or two arrows in instrumental-variables or no-unobserved-confounders applications in social sciences, I worry that in practice it is difficult to convince readers that such a causal graph fully captures all important dependencies. In other words, in social sciences applications a graph with many excluded links may not be an attractive way of modeling dependence structures. As Andrew Gelman writes on his blog in a discussion of graphical models and potential outcomes, “Nothing is zero, everything matters to some extent” (Gelman, 2009). Of course, instrumental variables methods do also critically rely on the absence of particular dependencies, but my point is that the larger graphical models such as those in Figure 2 of the Shpitser comment or Figure 1 in Pearl (1995) with many variables and many excluded links require researchers to evaluate critically many more of those assumptions. The causal graph methods appear to be more suited to answering the question whether given a complex set

of conditional independencies particular causal effects are identified, whereas in my experience in many social science applications researchers proceed by assessing a few conditional independencies given which it is known particular effects are identified.

#### **SWANSON AND HERNÁN: “THINK GLOBALLY, ACT GLOBALLY: AN EPIDEMIOLOGIST’S PERSPECTIVE ON INSTRUMENTAL VARIABLE ESTIMATION”**

First of all, I want to commend Swanson and Hernán for their work on improving the reporting the results of instrumental variables analyses (Swanson and Hernán, 2013). Although many of their recommendations such as the reporting of estimates of the proportion of compliers are routinely followed in the economics literature (these estimates are there often referred to as the first stage coefficients in the two-stage-least-squares terminology), these practices had not made it to the epidemiology literature, and their work will likely improve practice there. I am also glad to see that they do not attempt to defend the homogeneity assumptions that would allow for point identification of the ATE: it appears that there is growing consensus that such assumptions are not realistic. There are other areas where there is less agreement. Swanson and Hernán take issue with the focus in the paper on the local average treatment effect (LATE). Whereas Kitagawa felt LATEs were “valuable pieces of information about causal effects” (Kitagawa, page 359), Swanson and Hernán take the view that “the LATE is not generally relevant to epidemiological questions” and propose to “refocus on the global ATE in the population of interest” (Swanson and Hernán, page 371).

In my response to Swanson and Hernán, I want to make three points. First, I want to correct the record concerning my position on presenting estimates based on IV assumptions. Swanson and Hernán summarize my position in terms of “two options ... (1) present bounds for the ATE, ... , or (2) present point estimates” (pages 372–373) and then add that “of course ... we can always do both.” Swanson and Hernán appear to have missed that presenting both the bounds and the point estimate for the LATE (which is the same as the point estimate for the ATE under homogeneity) was what I in fact proposed (see also the comments by Kitagawa). One concern with the sole focus on the ATE that Swanson and Hernán appear to favor, either directly, or in combination with tighter bounds on outcomes, is that one may discard relevant information. Let me ex-

pond on comments in the main paper in this regard. Consider the following two versions of an artificial example with a dichotomous instrument, treatment and outcome. Let  $p_{zxy}$  be the population fraction of units with  $Z_i = z$ ,  $X_i = x$ , and  $Y_i = y$ , for  $z, x, y \in \{0, 1\}$ . In the first example, suppose  $p_{000}^1 = 1/4$ ,  $p_{001}^1 = 1/12$ ,  $p_{010}^1 = 0$ ,  $p_{011}^1 = 0$ ,  $p_{100}^1 = 1/24$ ,  $p_{101}^1 = 7/24$ ,  $p_{110}^1 = 7/24$ ,  $p_{111}^1 = 1/24$ , and suppose these fractions are estimated precisely. In this case, the fractions of compliers, nevertakers and always takers are  $1/2$ ,  $1/2$  and  $0$ , the bounds on the ATE are  $[-3/16, 5/16]$ , and the point estimate of the LATE is  $-1/4$ . In the second example,  $p_{000}^2 = 1/6$ ,  $p_{001}^2 = 1/6$ ,  $p_{010}^2 = 0$ ,  $p_{011}^2 = 0$ ,  $p_{100}^2 = 1/8$ ,  $p_{101}^2 = 5/24$ ,  $p_{110}^2 = 1/8$ ,  $p_{111}^2 = 5/24$ . In this case, the fractions of compliers, nevertakers and always takers are again  $1/2$ ,  $1/2$  and  $0$ , the bounds on the ATE are the same,  $[-3/16, 5/16]$ , and the point estimate of the LATE is now positive  $1/4$ . Under the instrumental variables assumptions, the bounds for the ATE are identical in the two examples, but the LATEs are very different. In the first case, there is evidence of a substantial negative effect for a subpopulation, whereas in the second example one knows there is a subpopulation for which the effect is substantial and positive. That would appear to potentially lead to very different substantive conclusions. Simply reporting bounds would miss these results.

In the second part of my response to Swanson and Hernán, I will discuss more explicitly the concerns about external validity that are implicit in the discussions of the relative merits of the overall average effect (ATE) and the LATE. Swanson and Hernán are interested in the ATE in the population of interest, and then without explicitly saying so, assume that the study population is representative for this population of interest. Matters are rarely so clear cut in practice. The study sample need not be a random sample from the population of interest because of nonresponse, or the policy maker may be interested in the average effect if the treatment were to be extended to a larger population at a future date, or were to be offered on a voluntary basis to the general population. What the population of future volunteers looks like may well depend on the efficacy of the treatment according to the statistical analysis. There are many examples where even in randomized experiments the causal effects found for the study population did not generalize to the population subsequently subject to the treatment. Once one recognizes that even the study population may differ from the population of interest much of the concern with the LATE

that Swanson and Hernán raise loses its force. My position here is again essentially similar to the Shadish, Campbell and Cook (2002) view on the primacy of internal validity over external validity.

In the third part of my response, I will make some comments on the monotonicity assumption. Swanson and Hernán present a generic example where the monotonicity condition is likely to be violated, and argue that the instrument in this example is one of the “most commonly proposed instruments in epidemiology.” In fact, the example demonstrates how much there is to be gained from a closer study of the earlier econometric literature, as it was discussed in the original paper on the LATE (Example 2, page 472, Imbens and Angrist, 1994); see also Section 5.3 in the current paper. The generic example is as follows. The assignment of individuals to the treatment is partly based on preferences of an administrator (physician in the epidemiological version of the experiment). The assignment of administrators to individuals is as good as random. Different administrators may have different preferences on average, but it need not be the case that the resulting instrument is monotone because the set of individuals who would be assigned to the treatment by one administrator need not be a proper subset of the set of individuals who would be assigned to the treatment by a second administrator. That setting also arises in applications of instrumental variables in legal settings where the administrator may be a randomly assigned judge: see Aizer and Doyle (2013) with an application in the criminal justice system, and Dobbie and Song (2013) with an application to bankruptcy proceedings. It is important to distinguish such settings from those where the instrument corresponds to an increase in the incentive to participate, in which case the monotonicity assumption is plausible. It is precisely by articulating explicitly these assumptions and describing the role they play that we may be able to avoid misleading decision-making efforts.

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