TECHNICAL REPORT
R-524
January 2023

Comments on Nick Huntington-Klein's review "Pearl before economists: *The Book of Why* and empirical economics"

Judea Pearl
University of California, Los Angeles
Computer Science Department
Los Angeles, CA, USA
judea@cs.ucla.edu

I am grateful to the editors of JEM for giving me the opportunity to comment on Nick Huntington-Klein's (henceforth Nick) review "Pearl before economists: The book of why and empirical economics" which clearly lays before economists what *The Book of Why* contributes to the science of causation and to empirical economics. This review may well be the most informed account of *The Book of Why* that the economics literature has seen, perhaps even the most balanced account of modern causal inference in general. I venture to make such bold statements in view of the fact that, with the exceptions of White and Chalak (2009) and Pearl (2015) [`https://ucla.in/2mhxKdO`], one would be hard press to name an econometrics article in which Structural Causal Models (SCM) are given a non-dismissive coverage.

My aim in this note would be to assist applied econometricians in understanding the arsenal of tools provided by SCM and, in particular, to extend the set of contextually useful tools beyond those identified in Nick's reviews.

My first comment refers to the relationships between the Structural Causal Models (SCM) and the Potential Outcome (PO) frameworks, the latter being more familiar to some economists, primarily those following the Rubin, Imbens and Angrist tradition of quasi-experimental design. Nick points out correctly that the essential difference between the two lies in the information with which the analysis begins: "the distinction is that PO focuses first on modeling counterfactuals, while SCM focuses first on modeling the causal relationships between variables" (Huntington-Klein, 2022, p. 326). To be more specific, PO begins with assumptions of counterfactual independencies (sometimes called conditional ignorability statements) which the investigators deems necessary for justifying certain estimation routines, while SCM begins with causal relationships that the investigator judges to be true in the domain. This difference, which may appear trivial to a casual reader, entails in facts a day and night difference in the scientific integrity of the analysis as well as in the veracity of the conclusions drawn.

At issue is the format in which scientific knowledge is stored in the minds of rank-and-file investigators. Is it in the form of causal relationships between meaningful and measurable variables, or in the form of conditional independencies among hypothetical unmeasurable variables named "potential outcomes"? I strongly side with to the former, and for two reasons: (1) conditional ignorability assertions can easily be derived from SCM, not the other way around, and (2) The mental task of ascertaining the plausibility of such assumptions is beyond anyone's capacity, which makes it extremely hard for researchers to articulate or to verify, let alone check consistency or redundancy among such assumptions, or whether they have testable implications.[1]

Conceptually, the differences can be summarized thus: The SCM approach goes where scientific knowledge resides, while the PO approach goes where statistical routines need to be justified. As a result, since conclusions are only as defensible as the assumptions upon which they rest, the SCM analysis produces conclusions that are defensible on scientific grounds while PO ends up with as dubious conclusions as the ignorability assumptions it receives.

To make these claims more concrete, I would like the reader to inspect the four ignorability conditions below, and judge whether they hold in any familiar problem domain of her choice. Just imagine any three variable, X, Y, Z, that stand in some causal relationship known to you, and try to judge whether the following formulas hold in your understanding of those relationships.

$$\begin{array}{ccl} X & \perp\!\!\!\perp & \{Y(0), Y(1)\} \\ X & \perp\!\!\!\perp & \{Y(0), Y(1)\}|Z \\ Z & \perp\!\!\!\perp & \{Y(0), Y(1)\}|X \\ Z & \perp\!\!\!\perp & \{Y(0), Y(1)\} \end{array} \tag{1}$$

These are the kind of judgments that PO researchers must make at the start of every inference task.

To further stress this point, the reader may try to judge whether the four statement above are consistent, whether any of them follows from the other three and whether they have testable implications, if data are available on variables (X, Y, Z). To witness how easily such questions are answered using causal graphs, see Chapter 3 of *Book of Why* and, for more elaborate questions, Appendix A of Pearl (2013) [`https://ucla.in/2L8OCyl`].

My second comment concerns whether economists versed in structural equation models (SEM) would find added value in SCM, beyond what they can already do by standard SEM tools. Nick expresses doubts in this added value, and states, for example: "it seems unlikely that economists would find it worthwhile to go out of their way to apply SCM." (Section 3.4) and "the diagram obscures the actual research design for a reader, and most of the work with functional form assumptions

[1] I have repeatedly challenged PO researcher to submit to such test, with no success, but the reader can easily witness how insurmountable such tasks are in the PO notation, by watching how simple problems are solved side by side in both the SCM and PO approaches, as in my book *Causality* (2009).

in SCM must be done in the structural equations, which may not add much beyond what economists already do in these cases." (Section 3.5) I will present some examples where the tools developed in the SCM framework go way beyond those available in standard SEM analysis. 1. Consider the textbook economic problem of Price-Demand equilibrium, shown in Figure 1,

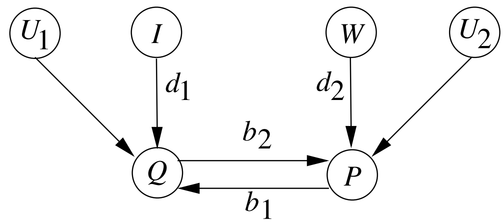

Figure 1: Causal diagram illustrating the relationship between price (P), demand (Q), income (Z), and wages (W).

The economics literature has given rise to elaborate techniques of identifying parameters in such models, including methods of incorporating complex interactions and functional form restrictions of various sorts. Yet what one cannot find in that literature is a method of going from the parameters to questions about interventions and counterfactuals. For example, assuming linearity, the parameters (d_1, d_2, d_3, d_4) of the model above can be estimated from data on the observed variables (Q, P, I, W). Now suppose we ask the following questions:

1. What is the expected value of the demand Q if the price is controlled at $P = p_0$?
2. What is the expected value of the demand Q if the price is reported to be $P = p_0$?
3. Given that the current price is $P = p_0$, what would be the expected value of the demand Q if we were to control the price at $P = p_1$?

Readers of The Book of Why would immediately recognize questions 1, 2, and 3 as belonging to Rungs 2, 1 and 3 (respectively) of the Ladder of Causation. expressed formally as:

1. $E(Q|do(P = p_0))$
2. $E(Q|P = p_0)$
3. $E(Q_{p_1}|P = p_0)$

Standard econometric literature however does not guide readers towards computing expressions 1 and 3 above from the estimated parameters (d_1, d_2, d_3, d_4). Indeed, as I remark in a footnote (Pearl 2009, p. 216), "I have presented this example to well over a hundred econometrics students and faculty across the United States. Respondents had no problem answering question 2, one person was able to solve question 1, and none managed to answer question 3." This is not surprising in light of the fact that the economic literature fails to provide its practitioners a formal a definition of counterfactuals in terms of structural equations.[2] In contrast, SCM's provide a simple definition of counterfactuals at the onset, called "The First Law of Causal Inference" (see (Pearl, 2014), [`https://ucla.in/2QXpkYD`] or (Pearl, 2015), [`https://ucla.in/2mhxKdO`]). *The Book of Why* demonstrates (using the firing squad example, Chapter 1) how it is operationalized to compute any conceivable counterfactual, and (Pearl 2009, p. 216) demonstrates how it answers the three questions we posed to the Price-Demand model of Figure 1. Writing in 2023, my guess is that the vast majority of economists still do not know how to answer question 3 above.

One may argue, of course, that, once the structural parameters are estimated, economists do not need to go any further to compute direct answers to interventional and counterfactual questions. But this is not the case. Heckman and Pinto (2013) for example, go to great length computing such answers, even inventing a new operator called "fixed" (essentially the same as "*do*") to facilitate the computation. In fact, Heckman and Pinto reveal to us what laborious and torturous computations one must go through when economists revert to their classical analytical methods and graphical tools (e.g., *d*-separation) are avoided (Pearl, 2013).

The need for graphical tools become even more transparent in problems where the model parameters cannot be identified. Marschak (1953) noted that many policy questions do not require the estimation of each and every parameter in the system–a combination of parameters is all that is necessary and, moreover, it is often possible to identify the desired combination without identifying the individual components. This identification task invokes a playful game on graphs (Pearl 2000, pp. 153–4p. 216; 2009) and turns a nightmare in the algebraic representation.

I will now present another set of problems that can benefit from SCM analysis and which economists, both experimental and theoretical, would be missing when avoiding graphical tools. I am referring to the problem of finding good instrumental variable (IV) in a system of equations. It often happens that the conditions of instrumentality, i.e., exogeneity and exclusion, are not satisfied by any variable in the system, and a natural question arises whether there exists a variable Z that could be turned into a good IV by conditioning on other variables, say W. Fig. 2 illustrates such cases. In Model (1), Z is not a proper IV due to the direct path $Z \to W \to Y$ which violates the exclusion restriction. Conditioning on W, on the other hand, would block that path and would turn Z into a valid IV. But what if additional arrows are present, as in model (2), or if an arrow $X \to W$ is added to model (1). Graphical considerations reveal immediately whether such conditioning is feasible (Brito and Pearl, 2002; Cinelli, Forney, and Pearl, 2022).

[2] Lacking a *do*-operator, Heckman and Vytlacil (2005) have attempted a definition of causal effects using the notion of "external variation." In Pearl (2009, p. 375), I discuss the limitations of this definition.

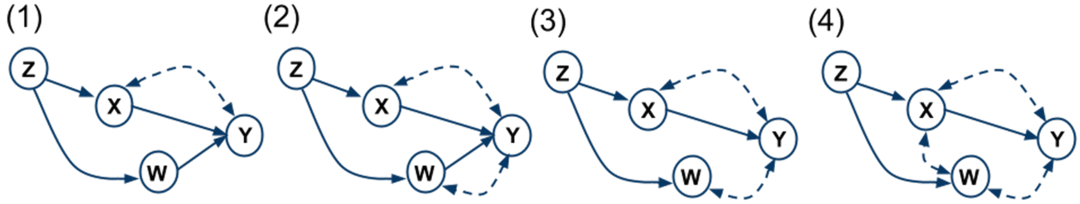

Figure 2: Models (i.e., systems of equations) in which it is desired to find an instrumental variable Z for the causal effect of X on Y.

Conclusions

I hope I have convinced readers that there is hardly any area in econometric research that could not benefit from the insights and tools of SCM, the back bone of modern causal inference (CI, in short). The question of why CI is less popular in econometrics than other fields (eg. epidemiology) has simple answers, resting on leadership more than science. Economists shun graphs because: (1) They are unfamiliar with graphical vocabulary, (2) They are constantly being warned by overly opinionated leaders that graphs are ad hoc, or not needed and, (3) They haven't been shown explicitly what the cost is of following overly opinionated leaders.

In my 2022 Year-End Review [`https://ucla.in/3GCJn9e`], I have described the lingering differences between CI and other frameworks in the following terms:

> Graphs are new mathematical objects, unfamiliar to most researchers in the statistical sciences, and were of course rejected as "non-scientific ad-hockery" by top leaders in the field (Rubin, 2009). My attempts to introduce causal diagrams to statistics (Pearl 1995, 2000, 2009) have taught me that inertial forces play at least as strong a role in science as they do in politics. That is the reason that non-causal mediation analysis is still practiced in certain circles of social science (Hayes, 2017), "ignorability" assumptions still dominate large islands of research (Imbens and Rubin, 2015), and graphs are still tabooed in the econometric literature (Angrist and Pischke, 2014). While most researchers today acknowledge the merits of graph as a transparent language for articulating scientific information, few appreciate the computational role of graphs as "reasoning engines," namely, bringing to light the logical ramifications of the information used in their construction. Some economists even go to great pains to suppress this computational miracle (Heckman and Pinto, 2015; Pearl, 2013).
>
> My disagreements with Heckman goes back to 2007 when he rejected the *do*-operator for metaphysical reasons (see [`https://ucla.in/2NnfGPQ#page=44`]) and then to 2013, when he celebrated the *do*-operator after renaming it "fixing" but remained in denial of *d*-separation (see [`https://ucla.in/2L8OCyl`]). In this denial he retreated 3 decades in time while castrating graphs from their inferential power. Heckman's latest interview

in *Observational Studies* (Heckman, 2022) continues his on-going crusade to prove that econometrics has nothing to learn from neighboring fields. His fundamental mistake lies in assuming that the rules of *do*-calculus lie "outside of formal statistics"; they are in fact logically derivable from formal statistics regardless of any modeling assumptions, but (much like theorems in geometry) once established, save us the labor of going back to the basic axioms.

My differences with Angrist, Imbens and Rubin go even deeper (see [`https://ucla.in/36EoNzO`]), for they involve not merely the avoidance of graphs but also the First Law of Causal Inference [`https://ucla.in/2QXpkYD`] hence issues of transparency and credibility. These differences are further accentuated in Imbens's Nobel lecture (Imbens, 2022) which treats CI as a computer science creation, irrelevant to "credible" econometric research. In [`https://ucla.in/2L8OCyl`], as well as in my book *Causality*, I present dozens of simple problems that economists need, but are unable to solve, lacking the tools of CI.

It is amazing to watch leading researchers, in 2023, still resisting the benefits of CI while committing their respective fields to the tyranny of outdatedness. I hope this Review by Nick Huntington-Klein will help econometric researchers see the usefulness of CI tools in their field, and thus become full pledged beneficiaries of the new science of cause and effects.

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