

My instinct is that most academic economists would, as I did, find this book somewhat frustrating. I agree with the premise that economists are having more influence than ever, but I continually felt as I read the book that Mr. Skousen had missed many of the best and most exciting examples of this. Within my field—economic development—a natural thing to discuss is the move toward randomized evaluations and work with NGOs in developing countries to solve on-the-ground problems like high levels of intestinal worms and teacher absence. Economists have started working in-house at tech firms—Google, Yahoo, Microsoft—and, although Mr. Skousen discusses the Google IPO auction, he does not go into any detail about the role economists are playing more generally in these firms.

Having said this, I'll emphasize again that academics are not the intended audience and I think the book fares much better in its intended area—as an entertaining tour through the current state of economist's involvement in the real world. After having read this book, one could have a reasonable sense of at least *something* that is going on in any of a number of fields of economics—finance, development, health, behavioral economics, macro-forecasting, etc. I still feel the book could have used more focus and, in the end, I am not sure that it really found its footing. Is this a book about the value of libertarian principles in economic situations? Is it a book about how economists are tackling applied problems? Is it a book about how to invest your money? The truth is that it seems to be a book about all of these questions and more, and no one question gets a totally complete answer. In the end, though, perhaps this is what differentiates a successful popular book from the academic papers on which it is based. Most people do not want to read the *Journal of Political Economy* on the beach, but they just might want to read this.

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B History of Economic Thought, Methodology, and Heterodox Approaches

Hunting Causes and Using Them: Approaches in Philosophy and Economics. By Nancy Cartwright. Cambridge and New York: Cambridge

University Press, 2007. Pp. x, 270. \$85.00, cloth; \$29.99, paper. ISBN 978-0-521-86081-9, cloth; 978-0-521-67798-1, pbk.

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For the past thirty years, Nancy Cartwright has been one of the most significant philosophers of science. Beginning with a focus on physics, she was at the forefront of the movement to use philosophy to help to understand the practices of physics as seen from the working physicists' point of view rather than simply to pronounce on those practices from an Olympian, but perhaps irrelevant, perspective. Starting with her *Nature's Capacities and Their Measurement* (1989), she has steadily taken in a wider scope of sciences, including social sciences.

In Cartwright's view, economics is not some poor stepchild to physics but a significant part of a complex world in which the sciences are not (as so often thought by philosophers, physical scientists, and economists alike) arranged in a clear hierarchy in which each of the "special" sciences is reducible to the more basic sciences—physics forming the bedrock. Cartwright has also been a major player in the philosophical analysis of causation, a role that suits her turn toward economics, which has been undergoing a causal revival in, for example, the work of Granger in time-series econometrics, Heckman in microeconomic policy analysis, and the program of natural experiments in applied microeconomics. Given this background, a new book by Nancy Cartwright—particularly one that singles out economics in its subtitle—is surely a welcome event.

Hunting Causes and Using Them unfortunately represents a missed opportunity. It is not a systematic treatise but a compilation of occasional papers written with various particular—and mainly philosophical—targets in view. The papers have been too lightly edited to form coherent chapters in a unified volume. They are frequently repetitive and notation shifts from chapter to chapter. It is often difficult to appreciate fully the point of the chapter without the full context of the debates to which they originally contributed. They are heavy sledding for an *economist* not already immersed in those debates.

Despite professing to seeing useful insights in various approaches, Cartwright's method is more critical than constructive. And she sometimes misunderstands the approaches that she criticizes. For example, I do not recognize my own position in her account of my analysis of causal order (chapter 14). She attributes causal judgments to me that straightforward application of the formal definitions of chapter 3 of my *Causality in Macroeconomics* (2001) contradict. This is unfortunate, as she is a deeply insightful philosopher with a rare connection to actual practice and, even here, her discussion is full of genuine insights about causation and the problems of modeling it. A constructive treatise that tempered her criticism with a lucid exposition of its objects would have been exceedingly helpful.

Three themes dominate *Hunting Causes*. The first is that *cause* is a plural concept. The methods and metaphysics of causation, she believes, are context dependent. Different causal accounts seem to be at odds with one another only because the same word means different things in different contexts. Every formal approach to causality uses a conceptual framework that is "thinner" than causal reality. She lists a bewildering variety of approaches to causation: probabilistic and Bayes-net accounts (of, for example, Patrick Suppes, Clive Granger, Wolfgang Spohn, Judea Pearl, Clark Glymour); modularity accounts (Pearl, James Woodward, Stephen LeRoy); invariance accounts (Woodward, David Hendry, Kevin Hoover); natural experiments (Herbert Simon, James Hamilton, Cartwright); causal process accounts (Wesley Salmon, Philip Dowe); efficacy accounts (Hoover); counterfactual accounts (David Lewis, Hendry, Paul Holland, Donald Rubin); manipulationist accounts (Peter Menzies, Huw Price); and others. The lists of advocates of various accounts overlap. Nevertheless, she sometimes treats these accounts as if they were so different that it is not clear why they should be the subject of a single book. And she fails to explain what they have in common. If, as she apparently believes, they do not have a common essence, do they have a Wittgensteinian family resemblance? She fails to explore in any systematic way the complementarities among the different approaches—for example, between invariance accounts, Bayes nets, and natural experiments—

that frequently make their advocates allies rather than opponents.

The second theme is her distinction between schemes that deductively *clinch* causal inferences and those that inductively *vouch* for them. Her idea is that certain schemes of causal inference work by making such strong background assumptions that inductive arguments are turned into deductive arguments. She is surely right that many arguments take the form of clinchers, conditional on background assumptions. But she is wrong to imply that advocates of these forms of argument are insensitive to the tentativeness and the fallibility of those strong background assumptions. Such sensitivity means that arguments that take the form of clinchers are, in reality, always practically vouchers.

For example, with Bayes-net approaches a statistical model describes data from which probabilities are inferred; and causal order, in turn, is inferred deductively from those probabilities. The inferences are based on strong assumptions. For instance, analysts frequently assume *causal sufficiency* (i.e., there are no omitted variables of a type that would confuse causal inference), the *acyclicity* of causal structure, and the *linearity* of functional relationships. Serious users of Bayes-net approaches are deeply aware of the fragility of the statistics—both the quality of the data and the modeling assumptions (e.g., stationarity and homogeneity). And they are aware that the assumptions about causal structure may fail in practical cases, which is why they have investigated the implications of alternative assumptions—e.g., latent variables (relaxing causal sufficiency), nonlinearity, and cyclical models.

And what is the alternative? Absent the strategy of embedding clinchers within maintained, but criticizable, assumptions, Cartwright provides no account of how evidence vouches for causal claims.

The final theme is the distinction between hunting and using causes highlighted in the title. The distinction gets its bite in Cartwright's belief that the strategies that successfully allow the identification of casual mechanisms frequently serve policy applications ill. Building on a long-standing theme of her work, real world processes are seen as the complex composition of a variety of deeper tendencies. The function of scientific

experiments is to isolate those tendencies through stringent controls so that they can be exhibited in pure form. The application of scientific knowledge in practice is frequently complicated—if not thwarted altogether—because the real world is open and, unlike in the laboratory, the complicating tendencies are uncontrolled. In such cases, it is not necessarily reliable to infer that effects found under stringent controls will play out similarly in the world.

Her insight trades on the old distinction between *internal* and *external* validity. For example, we may discover in a randomized controlled trial that a drug is effective against the malaria parasite; and, yet, for a variety of social and biological reasons, the drug may prove to be practically ineffective in patients. One lesson, perhaps, is that randomized controlled trials need to be supplemented with epidemiological studies. The exact same issues can arise with respect to natural experiments in economics: can the mechanism that they isolate be carried over to other policy contexts?

The theme of hunting versus using causes is elaborated in the final chapter on the use of counterfactuals in economics. Cartwright argues that the relevant counterfactuals isolate a cause from its own causes and set it to some value come what may. Using the same *implementation-neutral* strategies counterfactually to evaluate policies typically results in “imposters”—the wrong counterfactual for the issue at hand. Genuine policy analysis typically, though not always, requires *implementation-specific* counterfactuals. (Not always because some policies need to be robust across different implementations if they are to be useful since, in some cases, targeting is practically restricted.)

Cartwright is clearly correct that good policy requires the right counterfactuals and that, naturally, economists sometimes get it wrong. Yet, as a generic criticism, her case is not persuasive. For example, a straightforward reading of the Lucas critique, which Cartwright cites in other parts of the book with other purposes, is precisely as a plea for understanding counterfactuals in a causally structured, implementation-specific manner. Implementation of policy requires the specification of conditional rules and not a somewhat-may setting of particular variables.

Nancy Cartwright has once again written an intellectually challenging book, full of insights. It is too bad that the presentation is not well adapted to an audience of econometricians and applied economists, for whom the issues that she considers are important and not always clearly thought through.

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Milton Friedman: A Biography. By Lanny Ebenstein. Houndmills, U.K. and New York: Palgrave Macmillan, 2007. Pp. xi, 286. \$27.95. ISBN 978-1-4039-7627-7. *JEL* 2007-1226

Milton Friedman granted Lanny Ebenstein permission to write his biography, but evidently did not carefully examine his qualifications for the task. Friedman did not have a colorful personal background—certainly nothing comparable to that of John Maynard Keynes, who is often cited as an economist whose influence paralleled Friedman's—so there is little need for a biographer to dwell on details of earlier undergraduate and graduate years, marriage, and family (the first fifty pages of the biography). These pages are based on the Friedmans' 1998 memoirs—*Two Lucky People*. The reader who sought these conventional facts might do better to read the original source rather than Ebenstein's transcription.

At a minimum, a biography of Friedman should provide a well-rounded portrait of the man and, above all, an accurate account of the original ideas that he expounded as a professional economist, generated by his prolific intellect.

The one aspect of Friedman's personality that the biography succeeds in conveying is the chapter that describes Friedman's interaction with his students in the course on price theory and the money and banking workshop at the University of Chicago.

However, there are many other aspects of Friedman's personality that a biographer