bitrary assumptions as to either epistemology, paradigm, or fact. Moreover, there is an additional disjunction between recognition of the importance of epistemology and of epistemological limits of knowledge *and* recognition of the limits of epistemology per se.

All economics, as indeed all inquiry, is a series of footnotes to, or derivations of, various philosophical positions, each with their strengths and limitations – a situation of which most economists may be formally cognizant but one that seems to result in little diffidence in the manner in which they practice their discipline and urge the results of their work on others. All this is to say that the philosophical and epistemological entries in *The New Palgrave* tend almost uniformly to be ahead of the actual practice of economics. That is no insubstantial compliment, and it is largely due to the fact that the editors of *The New Palgrave* recruited as authors specialists who know what they are writing about, not practitioners who are seeking rationalizations for their private practices of hobbyhorses – although admittedly their post-Modernist epistemology may be the hobbyhorse of the editors. The pursuit of conclusive epistemological credentials is beset by the same problems as the pursuit of substantive economic statements.

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Nature's Capacities and Their Measurement, NANCY CARTWRIGHT. Oxford: Clarendon Press, 1989, x + 268 pages.

To an economist, Nancy Cartwright has written a startling book. Most economists are familiar with "physics envy." Philip Mirowski has made an industry of psychoanalyzing the profession, revealing that consciously or subconsciously it fashioned neoclassical economics out of much admired but poorly understood nineteenth-century physics (Mirowski, 1990). But, even those who find this thesis fanciful, recognize the economist's pride in practicing the hardest of the soft sciences, and may even support efforts to make economics journals more like those of physics than those of sociology. Most economists, no doubt, imagine that the direction of influence is, and should be, one way. A title "What Econometrics Can Teach Physics" would surely be followed by blank pages. Cartwright is a philosopher of science whose interests are ultimately in physics not economics; yet in her book, Chapter 6, which bears that title, is not blank, but consists of twenty pages of close reasoning. And it is not just the last chapter: economists from John Stuart Mill (writing as much in his role as economist as philosopher) to Lucas seem to stand on the side of the angels. What a psychological boost – and from such a quarter!

It is not really economics per se but econometrics and, in particular, the early econometrics of the Cowles Commission that interests Cartwright. Cartwright focuses on Cowles because of its interest in causal modeling. This will hardly seem up-to-date to many practicing econometricians. Systematic interest in causality has not been a mainstream concern in economics since the early 1950s. Tests of Granger-causality have been widely used since the early 1970s, and they have been debated and criticized; but few economists have attempted constructive analyses of causality. This is hardly surprising: despite their professed love of rigor and the fact that they philosophize constantly, economists largely shun rigorous and systematic philosophizing. Only the philosophically minded minority of economists are likely to benefit from Cartwright's splendid, powerful, and professionally flattering book. More's the pity.

The first of the book's three theses, "science is measurement," relates back to economics; for it is the motto of the Cowles Commission. This thesis, while it largely informs Cartwright's attitude throughout the book, is not really argued for. Her central theses are that capacities can be measured and that science cannot be understood without invoking the notion of capacities. These, however, are general themes. Much of the argument is more particularly about causality, its measurement and its importance in scientific explanation. Causal connections, for Cartwright, are a manifestation of particular kinds of capacities and are the very fabric of science.

The book is also part of a larger, radical project. Cartwright's earlier book, *How the Laws of Physics Lie* (1983), argued that the laws of physics could not be interpreted realistically. Rather, they were instruments, successful precisely because they were false and tractable. To use the laws of physics concretely, one had always to appeal to particular facts or purely phenomenological relations; nothing useful followed from the laws alone. Despite denying reality to laws, Cartwright is not an antirealist. The current book interprets capacities realistically, although the argument for this point is at best indirect. In the current book, laws and capacities coexist. What makes her project radical, however, is the announced aim of her future work: to do away with laws altogether.

Cartwright views nature as rather messy. Truth, for her, is not simple but terribly complex. Unlike the positivist philosophers, who place scientific laws at the very heart of their conception of knowledge, Cartwright finds the content of science not just in its laws but also in its practices. Indeed, laws are rather paltry things: "It is hard to find them in nature and we are always having to make excuses for them: why they have exceptions – big or little; why they only work for models in the head; why it takes an engineer with a special knowledge of real materials and a not too literal mind to apply physics to reality" (p. 8).

At one point, Cartwright observes that laws are odd objects for the allegiance of positivists. Positivists are largely followers of Hume, and Hume denies that real connections are to be found in the world. All we have are regularities. Yet the proponents of the hypothetical-deductive method favor laws, which seem to express hidden connections, while at the same time viewing causality and capacities skeptically. Cartwright opposes the ghost of Hume in the modern philosophy of science. Even more pointedly, she opposes the essentially Humean view held by Bertrand Russell, that functional relations should replace causation altogether in scientific explanation.

Still, Cartwright is a staunch empiricist. And she wants to sit in judgment of the practices of scientists trying to learn about causal connections. This view of the philosopher as policewoman is controversial, and it puts Cartwright in the camp of Bacon, Popper, and the Vienna Circle. It also puts her at odds with the recent fashion in economics for "rhetorical" methods – the importation into economics by Donald McCloskey inter alia of post-Modernist literary criticism, deconstructionism, and a kind of mushy pragmatism – which, having elevated tastes to the height of intellectual values, then finds judgment to be in poor taste. Philosophical fashion in economics greatly lags developments in philosophy itself. I have no doubt that, in time, Cartwright's empiricist attitude will overwhelm the rhetoricians.

Cartwright is an empiricist, but one who does not seek a foundationalist account of knowledge. She returns over and over to the "bootstrap" method advocated by Clark Glymour (1980). This suits her view of a messy nature; for the bootstrap method suggests that a productive empirical strategy is to take some things (facts, laws, models), which themselves are open to question, as indubitably true for the purpose of the investigation at hand. Later, these things may, in their turn, be questioned, but for now we gain conditional knowledge a little bit at a time.

So much for a general view of Cartwright's purposes and attitudes, but what of the main arguments of her book? Economics for Cartwright is secondary and is used only instrumentally to shed light on physics. As an economist, I am incompetent to judge the analysis of physics, but I can read to see whether the lessons Cartwright draws from economics are well learned and, more importantly, whether economists can learn anything useful from Cartwright.

Econometricians, Cartwright believes, know – in principle, at least – "How to Get Causes from Probabilities" (Ch. 1). Here, she draws on the work of Herbert Simon, parts of which she appears to know only secondhand from Glymour's lectures. This is unfortunate, because it is not clear to me that Glymour, as reported by Cartwright, has interpreted Simon's account of causal structure and the problem of observational equivalence quite correctly. Two questions need to be separated better than they are in this chapter: getting causes from probabilities could mean determining whether A causes B from statistical information, or it could mean measuring the strength of A's effect on B conditional on knowing causal structure.

The first question, does A cause B, seems to interest Cartwright intermittently throughout the book. Here observational equivalence rules with a vengeance. For a given sample, the probability distribution of related variables is completely summarized in the reduced form. Any linear transformation of the reduced form summarizes the identical distribution. Alternative transformations may represent contradictory causal orderings, yet be econometrically identified. Two (or an infinite) set of equations may satisfy the assumption that the error terms are uncorrelated with the independent variables.[1] The errors will differ from one causal ordering to the next, but no particular errors are privileged, the point about errors being that one cannot know independently what the true errors are. Cartwright avoids the problem of observational equivalence through the additional assumption that the data are temporally ordered and that causes must precede effects. While it is true that causes generally precede effects, it is not clear to me that this should form part of our concept of cause: contemporaneous causation seems perfectly natural in many economic and physical contexts. This was Simon's (1953) view as well. As Cartwright notes, the conditions for causal ordering and identifiability in linear systems are the same. The problem of observational equivalence, however, is that data in a single sample do not uniquely determine identification or causal order. But, Simon observes, the equivalence of different causal orderings breaks down out-of-sample if there are interventions in the system. Causal ordering, then, cannot be inferred from probabilities alone. The true causal order is the one that remains invariant in the face of interventions that alter the values of its parameters.

Probabilities without interventions can be used to answer the second question: what is the strength of A's influence on B? Such measurement is conditional on presupposing the causal ordering. This is a bootstrap procedure; for the identifying assumptions may be quite uncertain, but must be assumed to hold if the measurement is to be made.

I do not think that Cartwright has, in fact, confused these two questions. Her emphasis is on measurement, and she is clear that causal knowledge gained econometrically requires strong causal presuppositions. But I also do not think she is as clear as she might be about how strong these presuppositions are. And, for two reasons, I regret that she does not raise the issue of invariance. First, what is the correct causal order is often more important to economists than the strength of causal

1. On this point in general, see Basmann (1988); Cooley and LeRoy (1985) make a similar point when discussing the interpretation of vector autoregressions.

influence: that is, specification dominates estimation as an econometric problem. Second, the invariance of the true causal order ties in nicely with Cartwright's view on capacities. In Chapter 4, Cartwright argues that an account of causation requires a notion of capacities. Capacities are properties that, unlike regularities, are invariant from situation to situation.

The critical importance of causal presuppositions to causal inference is the main theme of Chapters 2 and 3. Cartwright tells the fascinating story of the Lamb dip in the theory of gas lasers. Lamb's analysis was unsatisfactory because, while it was mathematically complete and correct, and experimentally confirmed, it did not provide a causal account. A parallel in economics may be the rational expectations hypothesis. Many economists do not find the stories about how people come to hold rational expectations at all convincing. In one story, people know the true model of the economy and perform the necessary calculations, even though these are mathematically challenging for professional economists. In another story, people do not systematically persist in their mistakes and so grope their way to rational expectations. These stories are so unconvincing that Lucas (1987, p. 13, fn. 4) dismisses them as "silly" and "vacuous," and argues in a Russellian mode that the rational expectations hypothesis is just a consistency criterion for specific economic models. Cartwright rightly insists that a correct causal story is essential to scientific explanation.

> The lesson of [the Lamb dip] for physics is that new causal knowledge can be built only from old causal knowledge. There is no way to get it from equations and associations by themselves. But there is nothing special about physics in this respect . . . [It is only because physics] is one area in which the belief has been particularly entrenched that we can make do with equations alone. That is nowhere true. (p. 54)

Not even in new classical macroeconomics. The danger of the new classical school's Walrasian methodology is the implication that, to know anything, one must know everything, which is a recipe for knowing nothing. One lesson for economists to draw from Cartwright is that if we know a little bit, we might learn a little bit more.

In the same vein, Cartwright argues that all effective schemes for causal inference require causal presuppositions. Probabilities reveal only facts of association, unless some causal structure can be imposed. The economists' favorite, Granger-causality, is a case in point. To reach a causal conclusion, one must be able to state the complete list of causal factors to be conditioned on, but this requires causal knowledge ahead of the Granger-test. Granger's own formal way around this is to condition on the whole history of the world up to the current time. This would be an effective strategy if what one wanted to do was to say that

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this A causes this B at this time. But tests of Granger-causality are used differently. They attempt to establish that A causes B generically. Unfortunately, there is no effective means of holding the history of the world constant generically. In the absence of the particular causal history, Granger tests establish only associations, not causes.

Capacities for Cartwright are even more basic than causes. Cartwright finds the econometricians' practice of writing down linear regression equations with constant parameters to be consonant with her view. The causal variables are placed on the right-hand side, and the coefficients measure the capacity of each to affect the dependent variable. This probably reads too much into a practice really governed by considerations of tractability. What is more, it is positively misleading to claim that, when regressors are added to an equation, the econometrician expects the coefficients on the original regressors to remain invariant. This is true only in the special case that the new regressors are not correlated with the original regressors – a point made in every textbook discussion of omitted-variable bias.

Cartwright interprets Lucas's solution to the Lucas critique – namely, to get down to the bedrock of tastes and technology – sensibly as getting down to capacities and how they compose (i.e., what the causal structure is) to yield observed associations. Moreover, showing a better sense of the history of economics than many economists themselves, Cartwright notices that Haavelmo anticipated the Lucas critique by thirty years.

An example that Cartwright does not cite may better illustrate her views on capacities and their composition. An experimental economist may determine the coefficient of, say, relative risk aversion from gambling experiments with college sophomores. One thing that might be learned from this is whether the model appears to fit the facts. A more useful thing might be the actual value of the coefficient. If it is in fact the accurate measure of a capacity, one should be able to use the experimental value accurately and fruitfully in, say, a macroeconomic analysis of consumption.

Having shown that science needs capacities and that they can, with difficulty, be measured, the last question for Cartwright is how our general theoretical accounts can be related to concrete phenomena, for example, how a symbolic and formal physical theory can be used by an engineer to design an actual concrete bridge. The issues here (Ch. 5) are those of abstractions, idealizations, and their concrete manifestations. Cartwright's discussion is fascinating, but more tentative than the rest of the book. Economists, who, as a profession, have so many models, ranging from toy ones meant only to illustrate a principle to ones with various claims on describing the world as it really is, will profit from her discussion. In the end, however, she does not herself believe that she has reached the bottom of this issue.

Cartwright uses econometrics as she believes the early econometricians conceived of it, as a tool for providing a causal account of the world. She does not attempt to assess whether the econometricians are right in taking such a view of their own discipline. Keynes and, to some degree, Mill, she notes, did not believe that economies were complex compositions of *invariant* capacities. This is an issue economists will have to decide for themselves. If the econometricians' view is taken seriously, this engaging book provides an intelligent and fruitful starting place. There is more work to be done, and one can only look with pleasant anticipation to Nancy Cartwright's future contributions.

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Free to Lose: An Introduction to Marxist Economic Philosophy, JOHN ROEMER. Cambridge: Harvard University Press, 1988, x + 203 pages.

John Roemer's book is a variant on a long tradition in economic theory that attempts to evaluate social and political institutions solely in terms of their consequences for welfare. In particular, proponents of this tradition deny that agents can have direct preferences for institutions and procedures that are independent of their consequences for welfare. A conception of justice, that is, a procedure for ordering the claims and interests of different groups, can only be assessed in terms of its consequences for the welfare of the groups in question.

In *Free to Lose*, Roemer is committed to understand Marxist theory from within this theoretical framework. *Free to Lose* uses neoclassical economic models to assess both Marx's concept of exploitation and his endorsement of socialism. From within this framework, Roemer makes