

Causal Pluralism and the Limits of Causal Analysis:*

A Review of

Nancy Cartwright's

***Hunting Causes and Using Them:
Approaches in Philosophy and Economics***

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Abstract

A review essay of Nancy's Cartwright's *Hunting Causes and Using Them* from the perspective of an economist and causal analyst.

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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 towards 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. 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.

To take an example that cuts close to the bone, 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. At the risk of appearing to give a greater importance to my own work in *Hunting Causes* than it deserves, I try to correct in the section III, what I believe to be misunderstandings of my own views.

I.

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 longstanding 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 to 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 come-what-may setting of particular variables.

II

I now turn to a quite specific disagreement with Cartwright. In Chapter 14 (“Hoover on Simon on Causation”) Cartwright draws a distinction between a *production* account and a *strategy* account of causation:

A production account of causation focuses on the relation between x and y : x produces y ; x makes y happen or is responsible for y ; y comes out of x . Hoover’s strategy account by contrast focuses on the relation between us and x , y : we affect y by/in affecting x . So for strategy causation we do not consider what happens to y by virtue of what x does but rather what happens to y by virtue of what we do to ensure that x happens. Roughly, x strategy-causes y if and only if what we do that is sufficient for the value of x to be fixed is “partially sufficient” for y to be fixed. [p. 204]

Cartwright presents the account of strategy-causation that she attributes to me in a positive light; yet it is simply not a position that I hold.

I see the roots of my own views on causation in the work of Herbert Simon (1953). Cartwright argues that Simon’s views are quite different from my mine (in Chapter 13, as well as Chapter 14). And she claims that my approach is fundamentally different from other approaches that she characterizes as production-causation, including those of Woodward (2003), Pearl (2000), and Spirtes, Glymour and Scheines (2001). There are, of course, differences in our approaches, but the distinction that Cartwright is drawing here – whatever its merits in its own right – is not a distinction that separates me from these other authors.

A policymaker is, of course, interested in strategy: what can we do to make something happen? But in common with those authors to whom Cartwright attributes a production-causation account, I believe that it is only by understanding the fundamental connections between x and y that we can know “if what we do that is sufficient for the value of x to be fixed is ‘partially sufficient’ for y to be fixed.” In that respect, my

account is not significantly different from, say, Woodward's manipulationist account that Cartwright classifies as a production-causation account.

The source of at least some of the disagreement between Cartwright and me over how to represent causal relationships in equations and diagrams. Simon presents a schema for representing causal relationships in equations. So, let's start there. My own interpretation of Simon's account of causal order has been extensively developed elsewhere and need not be defended here. (Hoover 1990; 2001, ch. 2-3; 2009a, 2009b). The essence of Simon's approach is that causal order is expressed in a hierarchy of self-contained subsystems of equations.

Consider the simplest example:

$$(1) \quad x = a$$
$$(2) \quad y = bx + c,$$

where x and y are variables and a , b , and c are coefficients expressing the way in which those variables are functionally related.

Simon says that x causes y because x is determined in the subsystem (equation (1) only) independently of the value of y ; while y cannot similarly be determined independently of x . Simon realizes, of course, that different linear combinations of (1) and (2) could produce equations with the same solutions for x and y but different coefficients and, therefore, possibly different apparent causal orders. To block such "observational equivalence," Simon argues that the true causal structure corresponds to a particular set of these coefficients that may be chosen independently by "experimenters," including nature among them (Simon 1953, p. 26). Thus, if we know which set of coefficient has that property, the causal order is determined uniquely – independently of

the way in which the equations are written. This is not a syntactic property, but semantic property that requires that we know something about the world. The reference to “experimenters” is a suggestion of the kind of epistemological strategies that would allow us to come by that knowledge.

I call Simon’s privileged coefficients *parameters*. Their distinctive feature is that a parameters may take any value in its range without restricting what values other parameters may take. This is not generally true of the coefficients of any arbitrary linear combination of equations.

I think of causal diagrams in a way closely related to Pearl (2000) or Sprites, Glymour, and Scheines (2001). Causal arrows connect variables. I do not draw arrows from parameters to variables. The reason for this is that it is part of the notion of direct control, invoked by Simon in his discussion of experimenters. The parameter represents how the experimenter sets a variable that is controllable, and the link between the parameter and the variable it influences is a tight and unmediated one. For me a parameter serves a dual function of saying whether a causal connection exists and, if it is one that can exist in multiple (or continuous states), saying which state (or strength of connection) obtains. Given the role that parameters play for Simon in identifying which of a set of observationally equivalent systems of equations corresponds to the true causal order, one way to think about the parameterization of a system of equations is that it determines on which end of the shaft to place each of the arrowheads in a causal graph.

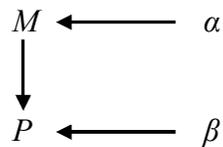
My account can be clarified by considering one of Cartwright’s examples in which she provides an explicit, independent description of the structure. A good one is

the lemonade/biscuit machine (Model (2.1), pp. 209-210), which she claims illustrates how strategy and production accounts diverge. Cartwright tells a personal anecdote:

There was a lovely machine in my residence in Bologna that dispensed lemonade and biscuits. When it dispensed lemonade it made clack-clack noises – by a pump, I was told; for biscuits, a whirring noise by a motor. Most often it made both kinds of noises and gave out both lemonade and biscuits. I never knew if the motor tripped the pump or the reverse or neither, though I was told that the whole thing was made of bicycle parts by local students so I knew that whatever connections there were, were all mechanical. There were levers on the machine to push in order to get the lemonade and biscuits but my Italian was not good enough to read the instructions. So whichever I wanted, I just pulled all the levers and I always got both. [p. 209]

Cartwright offers a hypothesis about the structure of the machine in which the motor causes the pump represented in her Figure 14.2:

Figure 14 (2.1)

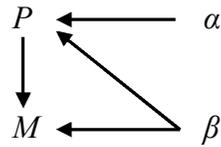


where α and β represent the motion of the two levers and the variables M , the action of the motor and P the rate of the pump. The figure corresponds to the system of equations

$$(2.1) \quad \begin{aligned} M &= \alpha \\ P &= M + \beta, \end{aligned}$$

Cartwright considers an alternative hypothesis in which the pump causes the motor, represented by a graph:

Figure 14 (2.2)



and a set of equations “which keep the same functional relationships but with a different causal order” (p. 210):

$$(2.2) \quad \begin{aligned} P &= \alpha + \beta \\ M &= P - \beta. \end{aligned}$$

The builders of the machine later tell Cartwright that the second hypothesis was correct.

She then goes on to ask:

What are the strategy-causal relations for the machine? α and β are parameters sufficient to fix both M and P and

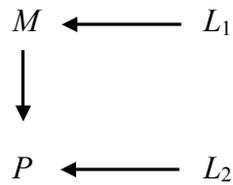
$$Par_M = \{\alpha\} \subset \{\alpha, \beta\} = Par_P$$

so on Hoover’s account M strategy-causes P . And that is reasonable. Anything I could do to set P set [*sic*] M as well, but not the reverse, even though the production relations are just the opposite of the strategy ones. [p. 209]

Here Cartwright appeals to a parameter-nesting condition found in my formal account of causal structure in *Causality in Macroeconomics* (2001, ch. 3). Since Cartwright does not want to believe that the students were lying about the construction of the machine, she believes that strategy and production causation must not be the same.

The problem is that I agree with the students: in the second hypothesis, P causes M . So if Cartwright gives me the same presumption of honesty as she gives the students, something is wrong in her analysis. What is it?

It is literally a case misrepresentation – i.e., a case in which Cartwright’s representation does not correspond to the representation on which I base my analysis. Cartwright interprets α and β as levers. To me this is a category mistake. Levers are elements of the machine. The state of the levers can be represented by variables that take the values of, say, *on* and *off* or *up* and *down*. The machine in which M causes P might therefore be represented as

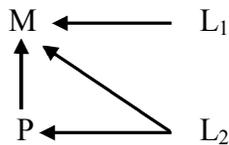


Now this may appear to be a trivial amendment, since it looks like I have simply renamed α and β . But that is not quite right, because there is a substantive difference between the L s and α and β . A parameter might, on Cartwright’s interpretation, simply govern the setting (or partial setting of a variable) – e.g., whether the lever is up or down. But it may also govern the existence and strength of connection between the variables. (It is common in linear path diagrams to indicate a causal strength for each arrow.) Implicitly, Cartwright has normalized all causal strengths to unity, so that she fails to make explicit at least one parameter, namely the one that governs the relationship between M and P . So, in place of 2.1 we could more clearly write the first hypothesized structure of the machine as

$$\begin{aligned}
 & L_1 = \alpha \\
 & L_2 = \beta \\
 (2.1') \quad & M = aL_1 \\
 & P = bM + cL_2,
 \end{aligned}$$

where, when the levers are connected, a and c represent the rate *ceteris paribus* at which the motor and the pump operate when L_1 or L_2 is in the *on* position, and where b represents the rate at which the pump responds to the motor. Cartwright's original parameters, α and β , indicate the state of the levers (*on* or *off*). (Of course, this is an easy case; nothing prevents us from having more complicated interactions among the levers and the motor-pump linkage.) The parameters are α , β , a , b , and c . In Cartwright's models a , b , and c have been in effect normalized to unity, so that they are invisible in Model (2.1).

Normalization is not necessarily a problem, but I think that it leads to confusion, when Cartwright considers the second hypothesis in which she imagines that the pump causes the motor and the motor slows down when the pump is on, because the pump lever is connected to a damping mechanism, which she represents as an arrow running from β to M . I would diagram the structure of the model that Cartwright describes as



which might be represented by a model such as

$$\begin{aligned}
 L_1 &= \alpha \\
 L_2 &= \beta \\
 M &= dP + eL_2 \\
 P &= fL_1 + gL_2,
 \end{aligned}
 \tag{2.2'}$$

where α , β , d , e , f , and g are the parameters, with α , β as before and d representing the rate at which the motor responds to the pump and e , f , and g the rates *ceteris paribus* at which the motor or pump responds to the levers being on. In particular, $e < 0$, since it is supposed to damp the motor. (Note, however, that this stretches the linearity assumption a bit, since damping is a negative influence that operates only when other positive influences are operating. Nonetheless, that could easily be fixed; so I ignore it for simplicity.)

Under this description, which I think exactly captures Cartwright's physical description of the machine, contrary to Cartwright's claim about what I should conclude, the definitions in Chapter 3 of my book imply P causes M (Hoover 2001, ch. 3). What is the source of Cartwright's confusion?

She is, I think, misled by failing to follow my usage with respect to parameters. She thinks that the second, quite different machine, can be represented with exactly the same functional forms as the first machine, because she fails to make explicit the parameters implicit on the variables of the system and elides the variables that represent the state of the levers by including only the parameters that govern their setting (α and β) and not the parameters that govern the strength of their influence (e and g) in the causal diagram. Had she been explicit about the full parameterization, she would see that, in general, there is no reason for the parameters of the second model to be algebraic transformations of those of the first model (generally d , e , f , and g , need not be algebraic transformations of a , b , c).

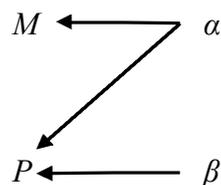
Cartwright wants us to read Model (2.2) as taking β to represent the damping of one of the levers on the motor. But because Model (2.2) merely rewrites Model (2.1), the

value of β is necessarily exactly the value needed to offset the putative effect of P on M . That is not damping, since M runs exactly the same speed for the same α and β in Model (2.2) as it does under Model (2.1). Model (2.2) cannot be the generic representation of the second machine as she describes it, but a quite particular representation that ensures that it will be observationally equivalent Model (2.1) in terms of the values of the variables it generates.

This is exactly the problem that Simon warned us about. Systems of linear equations with undirected equalities do not wear their causal orders on their faces. I pin down the causal order by saying which things are parameters as I have defined them. Pearl and Spirtes *et al.* pin it down by saying which variables are connected by causal arrows. I conjecture – and I have yet to see a counterexample – that any causal graph under their rules can be represented by a system of equations under my rules in which the parameterization determines exactly the same causal orderings among the variables. In this respect, Cartwright’s attempt to drive a wedge between Pearl and Spirtes *et al.* and me is misguided.

Consider another of Cartwright’s causal graphs:

Figure 14(1.1)



(We need not interpret this graph as involving motors and pumps; it can just as well involve money and prices.) The graph violates my standard conventions. That would

pose no problem – Cartwright is not obligated to adopt my representational conventions – except that she writes as if she were respecting my conventions. Consider a model corresponding to Figure 14(1.1):

$$(1.1) \quad \begin{aligned} M &= \alpha \\ P &= \alpha + \beta. \end{aligned}$$

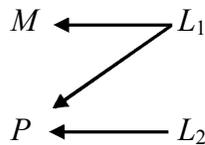
A diagram, such as Figure 14(1.1) is not, according to my conventions, a proper representation of Model (1.1) for the simple reason that, if α and β are truly parameters and not structural variables, then by the definitions of causal order in my Chapter 3, the only diagram compatible with Model (1.1) is $M \rightarrow P$. And this does not change, when the model is transformed algebraically. Thus, Model (1.1) is, to my mind, written more perspicaciously as Cartwright’s Model (2.1), which I take to be exactly the same model, with exactly the same causal diagram ($M \rightarrow P$):

$$(2.1) \quad \begin{aligned} M &= \alpha \\ P &= M + \beta. \end{aligned}$$

Its advantage follows only because we have an implicit convention that interprets the equal signs causally as running right to left, and that allows us to read the causal diagram directly off the equations. (This is why, in my book, I use \Leftarrow as a causally directed equal sign. Elsewhere Cartwright uses something $c^=$ with the same intent.) Applying my definitions to Model 1.1 or to 2.1, generates exactly the same causal relations. Cartwright’s notation misleads her, as she makes trivial algebraic transformations and treats them as if they represent new causal orders. This seems to me to be the problem that Simon exposed perfectly clearly. If we always wrote models in a canonical form with causal equal signs (\Leftarrow) and the convention that every causal equal

sign has one causally structural variable on the pointed side and at least one on the non-pointed side, it would be hard to be misled by such transformations.

Rather than starting with the algebra, start Figure 14(1.1). Then, Cartwright appears to say that it something like the lemonade/biscuit machine but with the linkage between the motor and the pump omitted – there really is no structural connection between M and P . Yet she believes that I am committed to identify M as causing P . Again, this is clarified if we are careful about diagrammatic conventions. Cartwright continues to view α and β as levers or switches; so let me propose the following as a diagram that suits my conventions but gets to her major point:



One model that corresponds to this diagram is:

$$L_1 = \alpha$$

$$L_2 = \beta$$

System (C'')

$$M = aL_1$$

$$P = bL_1 + cL_2.$$

Using the definitions of my *Causality* (pp. 61-63), M does not cause P . Let one subsystem be:

$$L_1 = \alpha$$

Subsystem (C')

$$M = aL_1.$$

Cartwright is tempted to say that my definitions would imply that M causes P , since the parameters of this subsystem are $P^1 = \{\alpha, a\} \subset \{\alpha, \beta, a, b, c\} = P^2$, the parameters of the

full model (System C''). But while that would get the conclusion that she wants – namely, that strategy-causality is different from production-causality – it does so only by ignoring an essential element of my definitions. Parameter subset relationships are not everything. In fact, there exist another subsystem (C^*), defined by

$$L_1 = \alpha$$

Subsystem (C^*)

$$L_2 = \beta$$

$$M = aL_1$$

that intervenes between C' and C'' – that is, C' determines L_1 and M ; and C^* determines L_1 , L_2 , and M , while C'' determines P in addition to the other three variables, so that the subsystems are arranged in hierarchy. The existence of the intervening subsystem rules out (for Simon as well as for me) M being a direct cause of P . At best, it could be an indirect cause (e.g., M causes X and X causes P), but in fact there is no intermediating variable (i.e., no X) in C'' , and so there is no causal connection between M and P at all. This is a very formal way of making the point that, despite the fact that the same parameters influence M and P , the actual value taken by M does not really matter – independently of the parameter α itself to the value of P . My definitions, then, point to exactly the same conclusions about the causal relation between M and P as those that Cartwright attributes to the production-causal account. The distinction between a strategy-causal and a production-causal account, insofar as it rests on my own position collapses.

How is it that Cartwright misreads my account to the point that she concludes that I would identify exactly the opposite causal order than the ones I believe that my definitions imply? One possibility, which I take seriously, is that the representation

scheme in Chapter 3 of *Causality* uses unfamiliar mathematics and is explained less clearly than it could have been. Two recent papers try to present a clearer account in more familiar notation and actually widen the class of causal information that can be encoded into the bargain (Hoover 2009a, b).

Whatever the source, one consequence of reading me as unconcerned with what Cartwright calls “internal production relations” is that Cartwright claims that I define causal relations in a framework of *reduced forms* in the econometricians’ sense of that term. This is baffling, as the entire point of my account is to provide a structural understanding of causation. One source of the confusion may be that, when talking about inference from data, I am often using what are clearly reduced forms (in the econometrician’s sense). But when characterizing and discussing causal relations – that is through most of the first seven chapters of my book – I am always talking about structural and not reduced form equations. I see the characterization of causal order – conceptually and ontologically – as distinct from the epistemological problem of how to infer it from data. My methods of conducting causal inference (see Chapter 8 of *Causality*) involve structural-break testing of reduced forms. Similarly, the Bayes-net methods of Spirtes *et al.* involve the construction of correlation matrices, which are essentially a byproduct of reduced-form estimation. But in both cases, the reduced forms are treated as a source of information, whereas object of the investigation is to find out what sort of internal production relations must lie behind them. I believe that this involves a problem of digging, and that one will gain coarser knowledge (for instance, , through an unknown mechanism, pulling lever 1 delivers lemonade) which may with luck be refined later (for instance, by providing an accurate mechanical description of the

mechanism that connects the lever to the lemonade). Cartwright's discussion of my account does not, however, have anything to do with these inferential issues; rather it considers how to characterize what causal relations are. At this level, there is not a reduced form in sight in my work.

The account that Cartwright attributes to me is actually quite similar to that of Stephen LeRoy (1995). LeRoy believes that internal causal relations are often hopelessly ambiguous. Some clear causal relations run from *exogenous* to *endogenous* variables. Exogenous variables in LeRoy's schema are similar to variables such as L_1 and L_2 in the models of the lemonade/biscuit machine – variables that cause, but are not caused by, other variables in the system. For LeRoy, the only unambiguous internal causal relations occur when a variable, say, x is determined by a subset of the exogenous variables that determine another variable, y . This is a nesting criterion similar to the parameter nesting criterion in my representation schema. Unlike Simon's or my schemas, however, LeRoy does not have a hierarchical account of systems of equations nor an identity criterion for those systems based in a notion of a unique parameterization. These are precisely the features of my account that Cartwright fails to respect, which suggests that LeRoy provides a better example of her notion of strategy-causation.

III

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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