A Case Study in Realism: Why Econometrics Is Committed to Capacities

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Who is a realist about capacities? Everyone, I maintain, who employs the methods of Galilean idealization typical of modern science. But I am not going to defend this broad thesis today. (For a defense, see N. Cartwright 1989.) Instead I am going to concentrate on one tiny corner of modern science — econometrics — where I hope I can show what it is we do in science that commits us to capacities. Notice that I say, "what we do" and not "what we say." That is because I subscribe to the current movement among historians, philosophers, and sociologists that is turning its attention from scientific theory to science as a whole, and especially to concrete scientific practices. This bears on issues of scientific realism. For surely our knowledge of nature is encoded as much in our methods and applications as in our theoretical structures. We learn about what a particular science is committed to not just by listening to what its laws say, but by looking to see what is required in nature to make its methods work.

That is what I am going to do in econometrics. I am going to write down some laws, or rather some abstract forms for laws, and I will claim that the parameters which appear in these laws represent causal capacities. But I do not expect you to be able to read that from the laws themselves. Instead you must look at the methods by which these laws are tested and at the applications to which they are put. I maintain that these two processes, considered jointly, do not make sense together unless these parameters do represent capacities. Seeing first what the gap is between these complementary processes, and second, what is needed to fill it, will, I hope, help make clear part of what my notion of capacity is. In the meantime, think of a capacity as a causal power that a system possesses by virtue of having a certain property or being in a certain state.

I will turn briefly to modern econometrics near the end, but I begin with traditional ideas developed at the Cowles Commission in Chicago immediately after the second world war, which set the program for a good deal of the econometric work in the U.S.A. for the following twenty years. Indeed I begin even a little earlier than that, with the ideas of two of the founders of econometrics, Ragnar Frisch and his student and colleague, Trygve Haavelmo.

First I want to explain a particular concept, the concept of autonomy, as it originated in the work of Haavelmo and Frisch. Then I want to tell you something about the Cowles Commission's doctrines about autonomous equations — namely, that autonomous equations are structural — and what that means. Last, I want to contrast these Cowles' doc-

trines with a modern British view about autonomy. The aim is to make clear why I say that Cowles methods are committed to capacities: that is, they presuppose a world in which specific, quantitative, measurable causal powers can be associated with particular properties or sets of properties.

There are two related features of these powers that I want to stress, features that make clear why the language of capacities is appropriate. The first is that the powers are attributed to separately identifiable properties (or sometimes to separately identifiable sets of interacting properties); and secondly that the attribution is stable across change. The properties carry their capacities with them from one kind of situation to another.

I begin with a famous example of Haavelmo's.

Here is where the problem of *autonomy* of an economic relation comes in. The meaning of this notion, and its importance, can, I think, be rather well illustrated by the following mechanical analogy:

If we should make a series of speed tests with an automobile, driving on a flat, dry road, we might be able to establish a very accurate functional relationship between the pressure on the gas throttle (or the distance of the gas pedal from the bottom of the car) and the corresponding maximum speed of the car. And the knowledge of this relationship might be sufficient to operate the car at a prescribed speed. But if a man did not know anything about automobiles, and he wanted to understand how they work, we should not advise him to spend time and effort in measuring a rela- tionship like that. Why? Because (1) such a relation leaves the whole inner mech- anism of a car in complete mystery, and (2) such a relation might break down at any time, as soon as there is some disorder or change in any working part of the car. . . . We say that such a relation has very little *autonomy*, because its existence depends upon the simultaneous fulfillment of a great many other relations, some of which are of a transitory nature. (1944, pp. 27-28)

The two reasons that Haavelmo gives show that the two features I am interested in were already combined in his thinking about autonomy: First, the autonomous laws will be structural. Unlike the function that connects speed and accelerator-depression dis- tance, autonomous relations will give the "inner mechanism." Second, autonomous laws will be stable. They will not be liable to "break down at any time, as soon as there is some disorder or change."

The two are similarly joined for Frisch. Consider for example Frisch's characteriza- tion of the most autonomous features of a structure as those that "could be maintained unaltered while other features of the structure were changed" (1948, p. 17). Here again we see the emphasis on stability across change. Frisch continues: "The higher this degress of autonomy, the more *fundamental* is the equation, the deeper is the insight which it gives us into the way in which the system functions, in short the nearer it comes to being a *real explanation*" (Frisch 1948, p. 17, underlining original throughout). So for Frisch as well as for Haavelmo, the stability of a relationship is a consequence of the fact that the relationship is fundamental and describes the inner workings of the economy.

This is particularly apparent when we consider the source for unstable or non- autonomous relationships. Typical examples in Frisch and Haavelmo involve transfor- mations of one set of equations into another. The first equations are autonomous: they are fundamental, or structural, and they are stable. Lack of autonomy arises because each of these equations must operate subject to the constraints imposed by each of the others. In the end the actual behavior of the system will be described by a single equation (the so-called "reduced form" equation, which in Frisch's language of 1948 is a *coflux* equa- tion) which is the consequence of the joint operation of the various structural equations.

192

This equation will clearly not be autonomous with respect to the structural equations. While a change in one structural equation will leave the others unaltered, in general one expects just the reverse with respect to the reduced form.

The derivative nature of the reduced form gave rise not only to problems of autonomy, but to epistemological problems as well, problems well-known for economics. I describe the problem briefly because the shift from the realistic methods of the Cowles Commission to more instrumentalist views, one of which I shall describe in a moment, is usually attributed to a loss of confidence in our abilities to solve these epistemological problems. The Cowles Commission had an ambitious idea: they expected first that they could figure out the correct *form* for the laws that governed economic phenomena (or correct enough to be able to make headway); and secondly, they thought it reasonable to suppose that laws of the prescribed form could be identified from the data. Identifiability is a very strong notion. Using the language of Clark Glymour, identification is a kind of boot-strapping from data to hypothesis. It means that the equation itself can be inferred from the data, once the form of the equation is given. The Cowles Commission hoped that structure would be identifiable. But Frisch thought that would be a happy accident. The reason is that we cannot conduct controlled experiments in economics:

> This is the nature of passive observations, where the investigator is restricted to observing what happens *when all* equations in a large determinate system are actually fulfilled simultaneously. The very fact that these equations are fulfilled prevents the observer from being able to discover them, unless they happen to be coflux equations." (Frisch 1948, p. 15)

To see exactly how autonomy fails in Haavelmo or Frisch's picture, consider the following transformation described in Frisch. He considers two structural equations of the form

$$\Sigma_{i\phi} \, a_{ki\phi} \, x_i \, (t - \phi) = 0,$$

where **k** represents different equations. So they look like this, letting **i** vary over 1 and 2, and ϕ run over two lag periods.

1. $a_{110}x_1(t) + a_{111}x_1(t - \phi_1) + a_{112}x_1(t - \phi_2) + a_{120}x_2(t)$
    $+ a_{121}x_2(t - \phi_1) + a_{122}x_2(t - \phi_2) = 0$

2. $a_{210}x_1(t) + a_{211}x_1(t - \phi_1) + \ldots + a_{222}x_2(t - \phi_2) = 0$

Frisch imagines that the particular values of the parameters are arranged so that the two equations are linearly independent. Hence they can be non-trivially transformed into a new equation of the same form. In his examples, a new equation results in which a number of the parameters take on the value 0. This equation is called a "coflux" equation, because it is not itself fundamental.

$$x_1(t) = ax_1(t - \phi_1) + bx_2(t) + cx_2(t - \phi_2)$$

where

$$a = a(a_{110}, a_{111}, a_{112}, a_{112}, a_{121}, a_{122}),$$

$$b = b(a_{110}, a_{111}, a_{112}, a_{112}, a_{121}, a_{122})$$

$$c = c(a_{110}, a_{111}, a_{112}, a_{112}, a_{121}, a_{122}).$$

Notice that different parameters in the new equation are functions of the same structural parameters. Imagine then that one wants to implement a policy that would shift the effect which, x_2's occurring at $t - \phi_2$ has on x_1's occurrence at t. With the coflux equation in hand, the obvious strategy is to try to bring about a change in the value of **c**. But if **c** were to change, knowledge of the original coflux equation would no longer be sufficient for predicting $x_1(t)$. That is because the parameters in the coflux equation are not independent of each other. If **c** changes, so may **b** and **a**. What happens to $x_1(t)$ depends, through **a** and **b**, on the way **c** is changed. Since the coflux equation does not give us information about how the parameters relate, it is of no use for predicting the effects on $x_1(t)$ of making "structural" changes in the value of **c**. It is impossible to predict the effects from the new equation alone.

Now that we have some idea of the concept of autonomy inherited by the Cowles Commission from Frisch and Haavelmo, let us turn to a more modern view, that of David Hendry, who is an influential theoretician in Britain. There is also a considerable movement in the United States opposed to various modern descendants of Cowles, and especially prominent are those who defend the use of vector-auto-regression techniques. But their philosophical position seems to me to be a kind of crude pragmatism, and makes a less instructive contrast for us than the Hendry school. The contrast between the ideas of Hendry and his colleagues on the one hand, and the Cowles Commission on the other is very instructive for my concerns about capacities. That is because Hendry's work, while it has much in common with Cowles, is not committed to capacities; whereas, I maintain, the Cowles Commission was.

I begin with worries shared by Frisch and Hendry. Frisch asks of our reduced form equation above:

> What does the equation mean? It means that *so long as x_1 and x_2 continue to move with the same time shapes as they had in the past* I can compute the value of x_1 at any point of time t from the knowledge of x_2 at this same point and x_1 and x_2 at certain earlier moments as indicated in the formula. In other words the equation is simply a description of the "routine of change" which x_1 and x_2 follow. The equation determined in this empirical way does *not* state that if a situation occurs where $x_1(t - \phi_1)$, $x_2(t)$, and $x_2(t - \phi_2)$ have some arbitrary values I can again compute $x_1(t)$ by [this equation]. (1948, p. 16)

In order to secure equations — autonomous equations — which can do that job, Frisch says "we are led to constructing a sort of super-structure." Hendry uses the term "super" as well to mark autonomous equations, in the expression "super-exogeneity." Exogeneity is a complex notion. It means intuitively "determined outside the system." But what kind of determination is this? In a well-known paper from 1983, Robert Engle, David Hendry, and Jean-Francois Richard distinguish three conceptually distinct but related notions. The first is straightforwardly causal: exogenous variables in an equation represent causes; endogenous, effects. The second concerns questions of estimation which are at the core of the econometrician's interest, but not of special relevance here. The third is super-exogeneity. A variable is super-exogenous for Engle, Hendry, and Richard just in case it is exogenous in the first two senses — it is a cause of the endogenous variables and the right criteria for estimation are satisfied — and it is stable across change. That means that the laws describing superexogenous variables can be relied on for policy manipulations. These laws do not represent what Frisch called a "mere routine of change" which holds good so long as nothing else critical is altered. Rather they allow us to predict what effects the cause will produce even when other factors are manipulated. As Engle, Hendry, and Richard urge, the concepts they use to characterize super-exogeneity are meant to "guarantee the appropriateness of 'policy simulations' or other control exercises" (1983, p. 284).

194

With respect to capacities two features of the Engle, Hendry and Richard view are important. The first is a feature they do want; the second, one they deny. What they want, as we have been seeing, is a certain kind of stability of causes, and that is one essential ingredient in accepting capacities. They want to distinguish causal relationships which, though they are perfectly genuine, may hold only so long as no other essential ingredient of the system changes in a basic way, from causal relationships that hold across time. Recall Haavelmo's example of the automobile. Pressing on the accelerator is a genuine cause of the increased speed of the car — if that is not a causal process, what is? Yet the causal relationship is clearly not stable if the parts of the engine are rearranged. It is not a natural capacity of this small lever to be able to make a car go faster; it is rather a fortuitous consequence of the arrangement of circumstances.

What is missing in the Engle, Hendry, and Richard scheme? Why do I say that their scheme has no capacities, whereas the Cowles Commission did? What is missing is the commitment to structure. I will turn in a moment to some specific aspects of the structure that matter for capacities, but first I would like to make a more general point about realism. Engle, Hendry and Richard are concerned with problems of policy and manipulation. Yet they do not pursue this concern in the conventional way. The Cowles Commission believed in the simple Baconian idea: we can control nature, but to do so we must understand the fundamental principles by which it works. Engle, Hendry and Richard also believe in control, but they want to bypass fundamentals and go direct for control. That is what a model is for Hendry and his school. Models do not aim to represent how nature works, but rather to describe immediately a control procedure. From their equations we are supposed to be able to tell what would happen to y if we were to manipulate x. And that's it. This is not meant to be an easy task; yet they take it to be a more possible task to seize control than to win it through understanding.

I call this way of thinking the "new instrumentalism." Science is an instrument for control, not a vehicle for understanding. When I say new, I mean new relative to the post-war thought with which most of us were brought up. From Ernst Nagel and others we learned to contrast realism and instrumentalism. But it was always in the context of *theory*: we talk about the realist versus the instrumentalist interpretation of theories. But the real opponent of an instrumentalism of the kind we see in Engle, Hendry and Richard is not realism. Admittedly they have the kind of epistemological concerns that motivate many anti-realists. The Cowles Commission hoped that the inner structures themselves could be identified from the data; these more modern opponents of Cowles methods despair of that.

But two points must be taken into account. The first concerns the idea of "inner mechanisms." Despite Haavelmo's metaphor of the inner workings of the engine, hidden under the hood, the variables in a Cowles structure were not in any way hidden. They did not represent theoretical entities or theoretical properties in our usual sense of the term, for they were supposed to stand for macroscopic features of economic life which were not just observable but measurable. It was not the fundamental entities that might be hidden on a Cowles account, but rather the fundamental relationships. That is because, as we learned from Frisch, each relationship operates under the constraints of all the others, and only the overall result can be observed passively. So the difference between Cowles' views on the one hand and those like Engle, Hendry and Richard on the other is not like the difference between realists and instrumentalists that we see drawn over theoretical entities.

More important, I think, is the realization that in one sense central for our contemporary philosophical debates, programs like those of Engle, Hendry and Richard are realist, for they are objectivist. They believe after all in autonomy, and they believe that autonomous laws reflect objective processes in nature. (Hendry calls them "data generating processes" in Hendry 1979.)

The point is simply made by contrasting Hendry with some of the Americans I mentioned earlier. Consider for example the famous paper by Edward Leamer, "Let's Take the Con out of Econometrics" (1983). The con that Leamer is worried about is just the attempt by econometricians to establish autonomous and objective laws of the kind I have been discussing. Leamer takes a familiar line, now highly fashionable. All inference makes presupposition. We are never in a position to know which presuppositions are correct. Hence we are never in a position to know which conclusions are right. So, concludes Leamer, econometricians shouldn't make inferences and shouldn't draw conclusions. "The job of the researcher is then to report economically and informatively the mapping from assumptions into inferences" (1983, p. 38). Here we see a real example of the California dictum: "Different strokes for different folks."

I do not want to dwell on this difference, for this kind of dispute is already familiar to us all. I describe it rather by way of contrast with a distinction that is less well rehearsed among us. Views like those of Hendry's are instrumentalist. But they are not, I want to say, anti-realist or anti-objectivist. They are rather anti-theoretical. The Hendry program admits the possibility of knowledge, and it does not in any way try to divorce knowledge from justification. Claims to autonomy must be reasoned about, argued for, and tested. Laws which go wrong must be corrected, and the corrections are not to be made, as Leamer suggests, by whim, but rather by reason. But are the reasons trustworthy? Well, we know how to carry on with that debate.

The more novel contrast I want to point to is the contrast with the work at the Cowles Commission. For the Cowles Commission, claims about autonomous laws are to be defended by the knowledge that is contained in a theoretical structure, a structure which is supposed to represent the fundamental principles which govern the phenomena. But the knowledge which Hendry's program relies on comes in little bits and is highly specialized to subject matter. It is not represented in a unified scheme and in fact, it is sometimes not represented at all. It often reposes in techniques and methods and practices, and it is only from these that it can be read. In fact this too is an increasingly familiar story. I have talked about it before; it is the central theme of Ian Hacking's *Representing and Intervening*; and with people like Peter Galison, Norton Wise, Tim Lenoir, and Steven Shaffer it is at the core of the movement in history of science, that looks to practice rather than theory to find out what we know in science. But the Hendry program helps focus the issues. True, compared to Leamer, Hendry and his coworkers are realists. But compared to the Cowles Commission views like those of Hendry are instrumentalist, and not because they refuse to interpret theories realistically. Rather they eschew theory altogether. It is not scientific theory which is to yield power and control, but rather science itself which is to serve as an instrument, science with its entire tool-kit of tiny refined pieces of knowledge and techniques specialized to specific problems and specific tasks.

This has been a long aside. Having made my plea for a new way of organizing the debate between realism and instrumentalism, let me return finally to my concern with capacities. I said that Hendry's program missed out on capacities because, unlike the Cowles Commission, its models are not structural. Frisch believed in structure, so let us turn back to his equations to see what I find of significance there. I said in the introduction that the parameters in these equations — $a_{110}, a_{111}, \ldots a_{210}, \ldots$ — represent capacities. What are these parameters? Think about what is probably the most well known example of such a law — a demand equation.

$$q = ap + u$$

Here **q** is quantity, **p** is price, and **u** some kind of random shock factor. The parameter **a** is the price elasticity of demand. It tells us how much effect price has on the quantity demanded; or, in my language, it measures the strength of price's capacity to produce (or inhibit) demand.

196

One central reason for calling it a capacity is the one I have been stressing. It is treated as autonomous. It is assumed that this parameter will not change even if other parameters in other equations do. As a result of manipulations elsewhere in the structure, the total demand may well shift or even the relationship between demand and some of its other causes. But that should not affect the relationship between price and its contribution to demand.

How do we know that this parameter is supposed to be autonomous? Not by looking at the equation itself, but rather by considering how the equation is tested, and then how it is used. As I mentioned, for the Cowles Commission this kind of parameter is to be boot-strapped from the data. It is boot-strapped from the behavior which is observed under one fixed arrangement of the rest of the structure; yet is is used for predictions about what will happen when other pieces of the structure are altered. It is this kind of peculiar stability across change that leads me to use the language of capacities, this stability that Frisch saw as a happy accident. I say that **a** here represents not just the size of **p**'s effect on **q**, but rather its *capacity* because this is an ability that **p** carries around with it, from situation to situation.

We still have no contrast with Hendry though. I turn to that last. The second essential ingredient in the concept of capacity I am exploring is connected with the fact that for Frisch and Haavelmo and the Cowles Commission, an effect is attributed to the joint action of separate individual causes, and the parameters measure the influence of each cause separately i.e. the strength of its capacity. That is not so for Hendry. Look at the difference between what counts as a model for him and what counts as a model for the Cowles Commission. We have seen in Frisch an example of the kind of model developed at the Cowles Commission. It consists of a set of structural equations. According to Hendry,

A crude schematic structure for econometrics is as follows: To a first approximation . . . data generation processes in economics can be written as . . .

$$\mathbf{y}_t/\mathbf{z}_t \sim NI\,(\underline{\Pi}\,\mathbf{z}_t,\,\Omega)\qquad (t = 1, ..., T)$$

where $\mathbf{y}_t$ is a vector of endogenous variables, $\mathbf{z}_t$ is a vector of all relevant past and present information so that $E(\mathbf{y}_t/\mathbf{z}_t) = \underline{\Pi}\mathbf{z}_t$ and $(\underline{\Pi}, \Omega) = \underline{\mathbf{p}}$ is taken as approximately constant by working in a sufficiently large (but assumed finite) dimensional parameter space . . .

An "economic theory" corresponds to asserting that . . .

$$\underline{\mathbf{p}} = \underline{\mathbf{f}}(\underline{\otimes})\qquad\qquad \underline{\otimes}\,\varepsilon\,\Theta$$

where $\underline{\otimes}$ contains fewer parameters than $\underline{\mathbf{p}}$. (Hendry 1979, pp. 17-18)

So for Hendry giving an economic theory consists in specifying the parameters of a normal distribution as some function of a smaller set of (hopefully measurable) parameters. That is, a theory is just a gigantic probability distribution — the conditional distribution of the endogenous variables given the exogenous. What is important for my point here is that the autonomous parameters characterize the process as a whole. They are in no way associated with a partition of the total influence into separate causes. The process under study may remain stable while changes are made in the basic behavior of the central variables; but there is no suggestion that that stability is due to the operation of separate causes whose influence remains fixed while other factors change. In this respect the Hendry program is holistic, and I think that holism precludes capacities, at least in one natural sense of the idea.

Perhaps I can make this clearer with some cryptic remarks about Galilean idealization, hopefully cryptic only because brief. I think an attempt like Hendry's to divorce autonomy

from structure will not work. But that is not for the reasons of the traditional scientific realist who wants to defend the place of fundamental theory. I do not want to defend the view that we need to find some fundamental set of equations for nature in order to secure autonomy. I do not think that instrumentalist programs like Hendry's will fail because it is anti-theoretical, but rather because it is anti-Galilean, and Galilean methods are all we have. In the Galilean method you study features individually, in isolation. You strip away all the impediments, as best you can, in order to see how the feature behaves on its own. But why do you think that this isolated situation is special among all others? The answer must be that you think that in this situation you have learned *about the feature itself*; you have learned what its natural capacities are. That is why you can predict from what it does in this particular situation to what it will contribute when you set it back in far more encumbered situations. What I want to stress is that this is the method we have — this method that pre-supposes that individually identifiable features carry with them enduring capacities. We have no method for studying nature holistically; so we had better hope that nature has provided the enduring capacities which our methods are competent to find.

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