

ECONOMICS AND REALITY. BY TONY LAWSON. LONDON AND NEW YORK: ROUTLEDGE. 1997

Tony Lawson, an economics lecturer at Cambridge University, defends a thesis sure to arouse the interest of Austrians. Mainstream economics lies crushed in the fatal grip of positivism. The futile search for constants in human behavior condemns econometrics to sterility; and economic theory as a whole is little better. It disregards the basic truth that human beings act.

Our author's thesis sounds as if it had been lifted from the pages of *Human Action*; but, so far as one can tell, Mises has exerted no influence whatever on *Economics and Reality*. Not Mises but Roy Bhaskar, a Marxist philosopher of science, is our author's guide in all matters methodological. Though he notes occasional disagreements with Bhaskar, the latter's critical realism "has proved most useful for my [Lawson's] own [project]" (p. xvi).

The book nowhere cites Mises, and Lawson's acquaintance with the Austrian School stems principally from Menger and Hayek, whose work on methodology he finds lacking by critical realist standards. But does this not make the coincidence between Mises and Lawson all the more remarkable? If our author has arrived at Misesian insights by a trek through Bhaskar's dense prose, is that not all the better? Will not his range of reference, so different from that of most Austrians, enable us to gain new insights into our own views?

Alas, this hope is not entirely fulfilled. Though Lawson's work contains many insights, these often prove to be entangled in fallacious arguments; and much of what he says about philosophy seems mistaken. For Lawson, the root of all evil is not, as another source would have it, the love of money: it is rather the deductive-nomological model of explanation.

"The main culprit," Lawson writes, "is a mode of explanation that can be referred to as deductivist, or, more particularly, it is the conception of 'laws' (or 'significant results' or 'theoretical formulations') upon which deductivist explanation ultimately depends" (p. 16).

In this view, known affectionately to philosophers of science as the D-N model, to explain something is to deduce it from initial and boundary conditions, together with a law of the form "whenever X, then Y." An example will clarify the D-N procedure. Suppose we wish to explain why Senator Edward Kennedy has introduced yet another of his interminable socialistic "reforms." We note that the Senator is a certifiable leftist; and, by hypothesis, he has perceived a social problem. These facts constitute our initial conditions.

All we need to complete our explanation is a universal law, which may readily be supplied: "Whenever a certifiable leftist perceived a social problem, he advances a socialistic program to cope with it." The Senator's behavior has been deduced from a law and a set of initial conditions: hence, it is explained.

Lawson locates in the D-N model, when applied to economics, a fundamental mistake. Unlike the physical scientist, the economist can rarely, if ever, resort to controlled experiment. The economist confronts the same situations as the historian, as characterized by J.A. Froude. He faces an immense number of facts which, like the letters of the alphabet, can be arranged in an infinity of patterns. He cannot isolate the effect of one variable from an otherwise constant situation in order to test hypotheses of constant conjunction.

Lawson's point has merit, but it does not rule out laws of the form "whenever X then Y," so far as human action is concerned. Why cannot there be a priori laws of human action, in the fashion elaborated by Mises and Rothbard?

Our author does not tell us; he mentions the a priori only in passing.

If in the manner suggested it turns out that philosophy can so establish synthetic a priori knowledge (of the sort we are hoping to achieve) it can only be in the relative or conditional sense. . . . [Philosophy] represents . . . an investigation that necessarily takes contingent historical premises and specific social conditions, and aims to produce hypothetical and conditional conclusions. (p. 60)

Bhaskar's philosophy, no doubt, will entertain no more robust version of the a priori than Lawson offers. I cannot think, though, that our author's adoption of Bhaskar's view justifies his a priori neglect of the Misesian position.

No doubt Lawson will respond that he here follows simple common sense. Human action of necessity involves choice. If so, how can it obey invariable laws? "An interesting question to pursue, then, is what is implied by the reality of people making choices? Now if choice is real any agent could always have done otherwise . . . if under conditions X an agent in fact chose to do Y, it is the case that this same agent could really instead have not done Y" (p. 30).

Let us suppose that Lawson is entirely right: whenever I choose to do act A, I might have refrained from so doing. How does it follow from this that there are no a priori laws of action? No doubt such laws, if they exist, will not necessitate the outcomes of particular choices; but why must a law of human action assume this suspect form? I should have thought, once more, that the work of Mises and Rothbard offers much evidence to the contrary.

But, it may be replied, this mistakes Lawson's essential claim. His target is not the Misesian a priori, but the constant conjunctions beloved of logical positivists. As these philosophers and the economists who follow in their wake see matters, laws merely state that one sort of fact always follows another. No claim to discern an inner necessity is being made. But, if laws are taken in this way, Lawson's appeal to freedom fails of its purpose. His argument, I take it, is this: A supposed law of action says that whenever X takes place, then Y will occur. But if Y is an act of free choice, it is always possible that Y does not occur. To deny this flies in the face of free choice. How then can one maintain that a law always holds? The occurrence of Y is not inevitable.

This argument rests crucially on an equivocation. What does "inevitable" mean? If it means that Y must follow X, that given X, then Y cannot fail to happen, then the supposed law does indeed deny free choice as our author conceives it. If Y had to happen, then it is false that Y might not have happened. So much is evident. But it is equally evident, I fear, that this consideration leaves the positivist unscathed. On Lawson's own showing, he claims that Y always follows X. He does not claim that X

and Y are of necessity conjoined; where then is the problem that free choice poses for his view?

And there is yet another instance in which Lawson's critique aims at the wrong target. He wishes to combat Humean causal laws. The laws of science appeal to essences: they tell us what it is in the nature of things to do. (On this view of causal laws, there cannot be scientific laws that dictate the outcomes of free choices, since one cannot freely act against one's nature.)

Devotees of Man, Economy, and State will applaud Lawson's contention. But what does this have to do with the D-N model? Why does a supporter of this position have to appeal to laws of constant conjunction, rather than to essences or natures? No doubt Carl Hempel and Karl Popper, the foremost advocates of the D-N model, did have the suspect view of causal law. But this is a mere conjunction; Lawson demonstrates no essential connection between the D-N model and Humean view of causal laws.

In like manner, why cannot a proponent of the D-N model make use of structures in his explanation? We touch here on a key point in Lawson's presentation. As he sees matters, positivists are confined to the surface. They search for regularities in what is given to the senses, but they ignore the structures that give rise to these appearances. What underlies the regularities of sense? Here is a question, Lawson avers, that positivists cannot answer. Since they do not admit natures and powers of objects into their ontology, they cannot cope with structure.

Once more, why is this a problem for the D-N model view? Why cannot a supporter of this view appeal to structures, in just the sense that our author does?

One might here object in this way. Suppose that Lawson has wrongly connected the D-N model with the view of causal necessity he opposes. What is that to us? Why should we be so anxious to defend the D-N model?

The answer lies not (I hope) in an unconscious sympathy on my part for positivism. Rather, if the D-N model is rejected, what is the alternative account of explanation our author wishes us to use? He may wax eloquent about nature and structure all he wishes; but a question remains for his analysis. What exactly is supposed to be the connection between the essence or structure and the surface events it is alleged to explain? If the connection is not that the surface events follow deductively from the structures, what is it? Lawson owes us an alternative account of explanation. If not D-N, what then?

But has not Lawson given us an adequate response? True, he gives us no theoretical account of how structures explain events non-deductively. Nevertheless, he answers the question by illustration: *solvitur ambulando*. Does not his elaborate historical account of the slow growth of British productivity during the last hundred years show us exactly what he means by explanation (p. 255ff)? And may we not go further? Does he not give a detailed account of how his explanation operates? Must not the charge that he has failed to offer an alternative to the D-N model be withdrawn?

As it seems to me, the accusation against him stands good. He devises an intricate technical vocabulary to describe the historical account he provides. (Readers who wish to find out what a "contrastive demi-reg" is may consult the text).

But he does not, to repeat, show us how all this apparatus explains slow British productivity, if not by enabling us to deduce it from laws. A proponent of D-N

would claim that Lawson's historical narrative can readily be accommodated by his model. If not, why not?

Further, even some of Lawson's criticism of the Humean view of causation misfires. Why cannot a Humean appeal to underlying levels and structures? These structures will, by hypothesis, not involve causal necessities stricter than constant conjunction, but why is this to the point? To say that causation is constant conjunction is not to say that we cannot stray from what appears to the senses.

Austrians have a ready answer to those who demand of us why we ought to proceed in praxeological fashion. The axiom of action, and what follows deductively from it, are true. Are the results of Austrian analysis illuminating as well as true? Those in search of an answer have available the treatises of Mises and Rothbard for study.

What has Lawson to show to skeptics who ask why we should pursue economics according to the critical realism of Bhaskar? Our author rejects much of mainstream economics; for him a truly scientific economics is largely a project for the future. He cannot then appeal to a developed body of economic theory to show why the methodology he supports ought to be adopted.

Instead, he resorts to an argument from analogy. Bhaskar, he tells us, has endeavored to answer a transcendental question: how is physical science possible? Bhaskar's response brings in the whole apparatus of structures and essences about which I have been able only to hint at in this review.

To see what is at stake here, we must grasp the underlying situation, as Bhaskar and his disciple Lawson conceive it. The question, "how is physical science possible?" depends for its interest on the premise that physical science is a highly successful enterprise. The question, "how is astrology possible?" is of much less interest.

Here arises a fatal problem, so far as economics is concerned. In Lawson's view, mainstream economics is not a successful discipline. He does not, in analogy to Bhaskar, ask how is existing economics possible? To do so in his view would not generate results of interest. Instead, Lawson conjectures that the conditions under which physical science is possible, according to Bhaskar, will also prove capable of generating a science of economics. Once more, no such discipline yet exists. Our author's entire edifice, on his own showing, rests on the merest conjecture.

Of this Lawson is well aware. In contrast with physical science, "the actuality of a social science cannot be taken as given or uncontentious. Rather, we are questioning its possibility, and the best we can do is take the (correctly) most adequate account of (natural) science as conditioning" (p. 56). In brief, critical realism explains physical science; maybe it will enable us to construct a viable economics as well. This hardly seems a sound basis for economics. To paraphrase Edith Cavell, anti-positivism is not enough.

DAVID GORDON
Ludwig von Mises Institute