

Explanation and Diagnosis in Economics

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Economists disagree about *whether* they should aim to provide explanations, about *what* they should aim to explain, and about how they should go about explaining. This essay will address all three of these controversies. I shall argue (1) that explanation is a central task in economics, (2) that one should adopt an explicitly causal model of explanation, (3) that economists cannot avoid explaining individual choices and they should attempt to explain the paths that take the economy from one equilibrium to another, and (4) that explanations in economics cite reasons as well as causes. Most of these points apply across different schools of economics, but “economics” in this paper should thus be read as “mainstream economics.”

1 Should explanation be a goal?

Milton Friedman asserts famously that the goal of economics, like that of all of science, “is the development of a “theory” or “hypothesis” that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed“ (1953, p. 7). He does not argue explicitly that economists should not aim to give explanations. Indeed he never even mentions such a view. He dismisses explanation as a goal by *asserting* that the goal of science is exclusively predictive and by scare quotes around many of his uses of the word, “explain.”

Economics is obviously largely oriented toward its possible contributions toward policy, and Friedman’s view that its goals are exclusively predictive has struck a responsive chord. But Friedman forgets to put scare quotes around all of his uses of the word, “explain,” and he never clarifies what the word means with or without its ornaments. He writes, for example,

Yet, in the absence of other evidence, the success of the hypothesis for one purpose—in explaining one class of phenomena—will give us greater confidence than we would otherwise have that it may succeed for another purpose—in explaining another class of phenomena. (1953, p. 28)

Here Friedman speaks of explaining phenomena or classes of phenomena as a “purpose.” He has already insisted that *the* purpose of economics is the development of theories that provide valid and meaningful predictions. Is there an inconsistency here?

One way to make sense of Friedman’s text is to take explanation to be just like prediction except that the phenomenon to be explained has already been observed. Prediction, not explanation, remains the ultimate goal. But explanation in this sense may be a proximate goal, since a theory’s success at explanation is evidence that it will succeed in prediction.

This line of thought provides a reason for those who hold a narrow anti-realist view of science to be concerned about “explanation” in only a minimalist sense. What scientists and lay people call explanations are not just derivations from theories of what is known. In equilibrium the wage and the value of the marginal productivity of labor are mutually determined and equal. But the wage does not explain the marginal productivity, nor does the marginal productivity explain the wage.

Neither does one explain the height of a flagpole by deducing it from the length of its shadow and the angle of elevation of the sun. Explanations of events or states of affairs cite *causes*. Should economists be concerned about explanation in this richer, causal sense?

Consider a contemporary example. There is considerable interest among economists in explaining why there has been a stark increase in inequality within the United States. Both the Summer 1995 and the Spring 1997 issues of the *Journal of Economic Perspectives* featured symposia on this question. Words such as “explains” “accounts for” and “causes” are used repeatedly in these essays. Some of these uses seem compatible with a minimalist view of explanation as prediction of what is already known. For example, Peter Gottschalk writes, “Figure 6 shows a very rapid increase in the experience premium for males during the early 1970s, which partially explains why overall inequality could increase in the early 1970s in spite of the decline in the education premium for males during this early period” (1997, p. 31). But despite the use of the word, “explains” here, Gottschalk does not regard this observation as explanation. He describes his essay as “bring[ing] together the factual material on changes in the distribution of labor market income, which any of these theories must address” (1997, p. 23). Knowing that more experienced male workers were paid relatively more may help explain the overall increase in inequality or it may be symptomatic of factors that independently led to an increase in overall inequality. Merely identifying a correlation that could have been used to predict an overall increase in inequality does not explain why there has been such an overall increase.

Why should we have an institution that aims (according to Friedman) to develop theories that provide valid and meaningful predictions about phenomena not yet observed? Friedman’s answer is evident. We’re interested in *controlling* economic events, not just passively watching them. We want to be able “to predict the consequences of changes in circumstances” (1953, p. 39). Economists and policy makers face specific problems, such as “to determine the effect on retail prices of cigarettes of an increase, expected to be permanent, in the federal cigarette tax” (1953, pp. 36-37). For this practical purpose a model that treats cigarette firms “as if they were producing an identical product and were in perfect competition” (1953, p. 37) will provide the needed predictions. If the problem is instead to predict how cigarette firms would react to price controls in World War II, a different model should be used. Friedman is not interested in prediction for its own sake. He wants prediction in order to guide policies. He wants economic theories to provide predictions that are useful for practical purposes.

Although he does not say so in so many words, Friedman thus wants economic theory to determine the *causes* of phenomena. We want to be able “to predict the *consequences* of changes in circumstances” “to determine the *effect* on retail prices” of a tax increase. Discovering a correlation between two things that are not related as cause and effect, though not useless, is less helpful. Knowing that falling barometer readings are correlated with storms tells us when to take shelter. But the usefulness of such a correlation between effects of a common cause is limited. It does not tell us how to do anything about storms. Identifying causes is of much more practical importance. When we know the causes, we are not only warned about what will happen. We may also be able to act to bring things about or to prevent them. If we cannot manipulate the causes (as in the case of storms), then we cannot manipulate the effects, but even then it is better to know the causes than merely to know correlations; for at least we know what levers to pull if only we had the strength to pull them.

Friedman’s no-nonsense practical “instrumentalist” orientation, which he takes to lead away from a concern with explanation, actually leads toward an interest in explanation. For one kind of explanation is the discovery of causes, and knowledge of causes is exactly what is needed for practical purposes. One one looks at the debate concerning the explanation of the growth of income inequalities, one finds that the participants are explicitly concerned about *causes*. For example, J. David Richardson concludes that “it may be difficult to separate trade from

technology as a cause of growing income inequality,” (1995, p. 51). Similarly, George Johnson argues that the experience and education premia are explained by a shift in demand for high-skilled labor and that shift in demand in turn has two main causes (1997, p. 46) — the increased openness of the U.S. economy and skill-biased technological changes.

As this example (to which I will return in section 3 below) illustrates, economists sometimes debate long and hard about the *causes* of a phenomenon. Such a debate is, obviously, a debate about the *explanation* of the phenomenon, because to cite the causes of some aspect of an event or state of affairs is to explain it. Someone who defends Friedman’s view that the goals of economics are exclusively predictive might, of course, regard debates like those devoted to determining the explanation of the large increases in inequality in the U.S. and the U.K. as confused wastes of time and energy nurtured by methodological mistake. But they might also take a more compromising (and much more plausible line). If the ultimate goal of economics is *useful* prediction, and useful prediction requires knowledge of causes — that is, *diagnosis* —, then explanation is a central task. It is a central task, because the *practical* objectives of economists require that they explain phenomena, whether or not they should (as I believe) also aim disinterestedly at pure knowledge or the resolution of curiosity.

The question of whether economists should be concerned to explain phenomena or only to predict it is to some extent independent of questions about the ultimate goals of economics or of science in general. Whether those goals are the disinterested pursuit of pure knowledge or the practical pursuit of useful predictions, economists need to seek out the causes of things. Diagnoses assist in cures. Only an anti-realist who also plays down the practical goals of science can repudiate the search for causes.

2 How economists explain

In contemporary work in philosophy of science, one finds three main accounts of scientific explanation.¹ First, there is the old-fashioned deductive-nomological model (Hempel 1965). To explain is to deduce a statement of what is to be explained from a set of true statements including essentially at least one law of nature. If one were to take this as necessary and sufficient for scientific explanation, then one would have something like the “explanation as prediction of what is already known” view discussed above. Unlike that view, however, the deductive-nomological model insists that the statements in the explanans all be true. But nobody believes that the conditions of the deductive-nomological model are sufficient for scientific explanation. One can deduce that a man who takes birth-control pills regularly will not get pregnant from the law that taking birth-control pills prevents pregnancy, but one has surely not explained why the man does not get pregnant (Salmon 1971, p. 34). The conditions of the deductive-nomological model are not necessary either, because they rule out probabilistic explanation. It certainly seems that one can explain events that are not determined. So the deductive-nomological model is (to put it mildly) problematic.

A second account of explanation is pragmatic.² Explanations are responses to why questions. To understand what explanations are requires attention to the nature of why questions. What do such questions presuppose? What sort of information are people looking for when they ask them? When people ask, “Why have income inequalities in the United States increased?” they presuppose that they are correct about the “topic” — that inequalities have indeed increased. If they are wrong about that, one responds by criticizing the why question rather than answering it.

1. This is (unsurprisingly) a simplification. I am passing over many views of explanation.

2. See Achinstein 1971; van Fraassen 1980, and Miller 1987. This paragraph follows van Fraassen’s version (1980, ch. 5).

Second, one needs to recognize the “contrast class” that is implicit in the question. People want to know why income inequality has grown so much, not why it has not grown more. In some contexts they may want to know why it has grown in the U.S. rather than in Norway; in others they may want to know why it grew in the U.S. in the 1970’s and 1980’s rather than in the 1940s and 1950s. An answer to a why question must discriminate the topic from the alternatives in the implicit contrast class. A purported explanation for the increased inequality that implies that inequality should have increased just as much in Norway or in the 1940s and 1950s fails to answer the question. The answer must also be “relevant.” There may be a good correlation in the U.S. between inequality and percentage of recreational trucks in use, but the percentage of recreational trucks in use fails (in van Fraassen’s terms) to satisfy the contextually determined relevance relation.

In simpler terminology, which van Fraassen avoids because of philosophical scruples concerning the notion of causation, “because the percentage of recreational trucks in use increased” does not answer the question “Why did inequality in the U.S. increase?” because the percentage of recreational trucks in use is not a *cause* (or not a significant cause) of the increased inequality. The third account of explanation focuses on causation. Why questions in most contexts are requests for the causes of what is to be explained. “[T]o explain an event is to provide some information about its causal history” (Lewis 1986, p. 217). According to James Woodward, to explain requires that one displays a pattern of counterfactual dependence. Such views of explanation are compatible with a pragmatic view, but more specific. Instead of saying that an answer to a why question must be relevant, where relevance is determined by context, one says what the relevance is: when one asks why an event occurs, one expects the answer to provide information about its causes. The view of explanation as citing causes or as providing information about causes complements the deductive-nomological model as much as it competes with it. When one deduces price and quantity from supply and demand curves, one simultaneously cites factors that influence price and quantity.

A causal theory of explanation leaves many questions unanswered: why should citing causes explain, while citing effects fails to explain? Why are why-questions requests for information about causes and not for information about effects? Although I cannot go deeply into these questions here (for more on them, see Hausman 1998, ch. 8), I shall argue in the next section that the link between explanation and diagnosis—between curiosity and the desire to cure—is crucial to their answer. To understand why explanation is so important to economics, one needs to understand its causal structure.³

3 The New Symmetry Thesis

Why does citing causes explain? When does citing causes fail to explain? Just what is the structure of explanation in economics, and how can one make sense of that structure? The key to answering these questions is, I think, a new “symmetry thesis.” In developing his deductive-nomological model of explanation, Hempel argued for a symmetry between explanation and prediction. If one is able to explain P, then one could also have predicted P if one had known the explanans before knowing P. A great deal of ink was spilled over this symmetry thesis in the 1950’s and 1960’s, and an appreciation of its failures helped philosophers to appreciate the inadequacies of the deductive-nomological model.

3. Many economists have expressed discomfort with causal language because they mistakenly take it to be opposed to the insight that the values of economic variables are often simultaneously determined or because they mistakenly take causal language to commit one to the view that outcomes have a *single* cause. For a further defense of the claim that explanations in economics are causal, see Hausman 1983 and 1990.

Although I would not attempt to resurrect or defend Hempel's symmetry thesis, I think that something analogous can be defended: In explaining P, one provides the knowledge concerning P needed to bring about P or to prevent P. The knowledge concerning P needed to bring about P or to prevent P may not suffice actually to accomplish either of these things because action requires more than such knowledge. For example, Newton's theory gives us the knowledge concerning tides needed to change them — just move the moon or change its mass — but our causal capacities are of course not yet up to the task of doing these things. There is, I suggest, a symmetry between what one needs to know for the purposes of explanation and what one needs to know for the purposes of control. Furthermore, if one supposes that the search for explanation is prompted by interests in control, this symmetry helps one to account for some of the peculiarities of explanation.

First, as already suggested, to explain requires that one cite a cause, not merely a factor that stands in some sort of necessary or nomological relation to what is to be explained. Citing an effect or another effect of a common cause does not explain P. Why not? Why when we ask why do we seek information about causes? I suggest that the answer is practical. Only knowledge of causes is relevant to control. If one wants knowledge about how to bring about or to prevent P, knowing effects of P or other effects of causes of P will not suffice. For example, Richard Freeman (following Alan Deardorff and Dalia Hakura (1994, p. 78)) points out that since wages and the volume of trade are simultaneously determined, "the effect of trade on wages cannot be meaningfully explored without additional specification of what outside force caused trade to change" (1995, p. 27). The fact that there is a necessary connection between wages and volume of trade does not itself permit one to cite either to explain the other. One needs to know whether the volume of trade is a cause of wages, an effect of wages, or whether the two are related merely as effects of a common cause. Without knowing what caused trade to change, what assurance could we have that interventions with respect to trade would have the desired impact on wages? Similarly, Robert Topel writes that "technical change favoring high-skill workers has been an ongoing force toward rising inequality in developed economies..." (1997, p. 61). In identifying a causal force, Topel picks out a factor that could in principle be used to influence the course of effects.

Second, to explain requires *more* than merely citing a cause. The cause one cites must discriminate between the topic and the contrast class. Consider the joke: Why was George Washington buried on a hillside? Answer: because he was dead. His being dead is, of course, a cause of his being buried on a hillside, but it does not make his being buried on a hillside any more likely than any of the other ways he might be buried. By the same token, it does not provide the knowledge one needs to bring about or to prevent his being buried at one location rather than another.

The participants in the debate on income inequalities are well aware of this point. For example, Nicole Fortin and Thomas Lemieux argue that demand changes cannot explain why inequality has risen so much more rapidly in the U.S. than in any other country except the U.K., because there were comparable demand changes in countries in which inequalities did not grow appreciably (1997, p. 76). Since comparable demand changes have often left income inequality unaffected, there is no reason to believe that interventions in demand would reverse or accelerate that growth. Thus one needs also to consider the role of institutional factors. Citing unchanged institutional structures leads to similar problems because "An existing institutional structure cannot explain by itself the growth in wage inequality in a particular country. However, institutional changes [such as a diminishing minimum wage or deunionization] can become a source of rising inequality" (1997, p. 76). An unchanged institutional structure cannot explain a changed economic reality and does not provide the knowledge need to change economic reality or to prevent its being changed.

Third, explanations that identify causes that discriminate between the topic and the contrast class may nevertheless be unsatisfactory, because they are too shallow. For example, even if the shift in demand for skilled and experienced labor is a significant and discriminating cause of the increased inequality, one might object that “the jury is still out on what exactly is the driving force behind these demand changes” (Fortin and Lemieux 1997, p. 76). Such superficiality is as important from the perspective of control as from the perspective of explanation. An intervention with respect to a causal intermediary may not be well defined without knowledge of the underlying causes. Without knowing “the driving force behind these demand changes,” what sense can one make of a possible intervention to influence these demand changes? Similarly, Fortin and Lemieux note that even if institutional changes are genuine and discriminating causes, they “may simply be endogenous responses to more fundamental changes in supply and demand conditions” (1997, p. 77). In that case attempting to control the growth of income inequality by reversing these institutional changes may be as fruitless as attempting to prevent World War I by preventing the assassination of Archduke Ferdinand. The relationship between the assassination and the war is not robust or invariant to interventions that prevent the assassination. In both cases there might be alternative intermediaries waiting in the wings to carry the underlying causes to their ultimate consequences. Adrian Wood writes, “That these reductions in trade barriers have occurred seems beyond dispute. More open to argument is whether they were the main cause of the growth of manufactured exports from developing countries, and whether they were truly exogenous — [causally] independent, in particular, of other demand and supply shifts in developed-country labor markets” (1997, pp. 61-2). These remarks could be read as a question about whether the reduction in trade barriers is genuinely a cause or whether, even if it is a cause, it is the sort of cause that is, in principle, usable for the purposes of manipulating its effect.

Explanation and diagnosis go hand in hand, even though explanation is largely driven by pure curiosity while diagnosis is largely a practical search for the sites where intervention might be possible. The two go so far together because (I conjecture) explanation is to a considerable extent a practical matter, too. Though many scientists have had no interest in building a better mousetrap, their curiosity and our curiosity is human curiosity; and human beings are unavoidably agents. What we want to find out (I suggest) is the sort of information that could be useful to agents.

4 What should economists explain?

I conclude that whether pursuing pure or merely useful knowledge, economists need to search for causes and thus to explain phenomena. But which phenomena? From a “pure” perspective, research priorities depend on curiosity, theoretical tractability and theoretical centrality. A pragmatic perspective that emphasizes the practical goals of science maintains that economists should explain those phenomena that are of the most practical importance. The devastating increase in inequality in the U.S. and the U.K. is obviously of great importance, but it is not obviously of more importance than highly esoteric matters that have wide-ranging indirect consequences.

Let us bring this question of what to explain down to earth by considering two detailed and controversial questions. First, should economists aim to explain the choices of individuals, or does it suffice to explain the aggregate consequences of those choices (and of many other factors)? Second, should economists settle, as they have, for explaining features of equilibria, or should they also explain how economies get from the neighborhood of one equilibrium to the neighborhood of another?

4.1 Should Economists Explain Individual Choices?

Economists are for the most part not interested in explaining individual choices. They are, in fact, concerned to distance themselves from psychological theories. In Lionel Robbins' words, "the implications of choice in a world of scarcity" — with which economics is concerned — "are in fact as little dependent on the truth of fashionable psychology as the multiplication table" (1935, p. 92). According to Debra Satz and John Ferejohn "rational-choice theory need not rest on either psychological or individualist foundations" (1994, p. 86). On an "externalist" perspective, rational-choice theory (including microeconomics) can "explain action merely by appeal to coherence restrictions on choice" (1994, p. 86). For example, the response of firms to a change in an input price is fixed by the pressures of a competitive market and has nothing to do with the psychology of decision-makers within the firms (1994, pp. 78-79). In a similar vein, John Hicks writes, "But economics is not, in the end, much interested in the behaviour of single individuals. Its concern is with the behaviour of groups. A study of individual demand is only a means to the study of market demand" (1946, p. 34).

Hicks is right to observe that economists "are not much interested in the behaviour of single individuals." But they may have to explain that behavior in order to explain the behavior of groups. To say that the study of individual demand "is only a means to the study of market demand" could mean that the study of individual demand is only a pedagogical step, a ladder that can be discarded once one ascends to the group level; or it could mean that the theory of individual demand is an unavoidable part of the theory of market demand. The latter seems to be Hicks' view—with one caveat—that the "falsifications" in the theory of individual demand, such as the assumption that commodities are infinitely divisible, "can be trusted to disappear when the individual demands are aggregated" (1946, p.11n). Since individual choices are crucial links in the causal chain leading from shocks (such as the imposition of a new tax) to some new equilibrium, explanations of individual choices, however intrinsically uninteresting they may be to economists, cannot be avoided.

The most systematic defense of the view that economics can completely dispense with any explanation of individual choice is that provided by Satz and Ferejohn.⁴ Their central argument for "externalism" [that claims about psychology play no role in rational choice explanations] rests on the possibility that in certain environments different sets of preferences and beliefs result in the same outcome. Since it does not matter which preferences and beliefs one imputes to individuals, one might as well impute to them a particularly simple psychology, regardless of the correctness of that imputation. In this way one can explain the aggregate outcome without providing any acceptable explanation of the individual choice. What matters is a particular structure in which the actions are embedded. The preferences and beliefs are imputed from or determined by the structure. The structure, not the psychology, is what is doing the explaining (1994, pp. 80-81).

Satz and Ferejohn are right to maintain that the structure within which rational choices are embedded, rather than the preferences and beliefs, is usually what is doing the explaining. Since many of the outcomes are robust over a reasonably wide variety of specifications of beliefs and preferences, citing beliefs and preferences contributes little to explanations. But Satz and Ferejohn are wrong defend "externalism" and to deny that citing beliefs and preferences has any role. Shallow and uninteresting causes are nevertheless causes. Furthermore, outcomes are often dependent on specific beliefs and preferences, and the explanation of individual choices is not

4. Despite the quotation above, Robbins concedes that economics does rest on psychology, albeit on utterly uncontroversial psychology. For a more extensive critique of Satz and Ferejohn, see Hausman 1995, from which the remarks in this paragraph and the next derive.

irrelevant. What would be the point of market research and focus groups if beliefs and preferences never mattered? Consider the best case for Satz and Ferejohn: the reactions of firms in competitive markets to price changes or tax impositions. Except in the most intensely competitive industries, even the market-clearing price and quantity will not be robust to different explanations of the choices of individual managers. And even in intensely competitive industries, lots of other facts about particular firms, such as product quality, short-run profits, or levels of advertising will depend on how individuals choose. No matter how much economists would like to escape depending on any psychological generalizations, the causal story they tell of market outcomes always includes a chapter concerning how individuals choose. That chapter is often uninteresting, and it often contributes little to our understanding of the phenomenon or to our ability to influence the phenomenon. But the chapter is nonetheless indispensable.

4.2 Should economists explain dynamics?

Economists rarely attempt to explain the path of adjustment to changes in the economic environment. Instead they explain only how the new economic equilibrium will differ from the old. Models that attempt to explain increased inequalities by lowered trade barriers compare equilibria with higher and lower trade barriers. They do not attempt to show how the effects of lowered trade barriers ripple through the economy. They do not consider the consequences of the many disequilibrium interactions that will take place along the way toward a new equilibrium (if indeed such is ever reached).

Most economists do not welcome this state of affairs. It arises because good theories of the dynamics of adjustment are unavailable. But I would suggest that economists have become complacent about this inadequacy — they have become willing prisoners of the limitations of their theories. One advantage of not worrying about adjustment paths is that it encourages inattention to the details of individual choice, which may be crucial to the adjustment processes without (perhaps) having any very considerable consequences for which new equilibrium is reached.

Economists indeed often overlook the extent to which their explanations are limited. Consider some other remarks of Hicks’:

We have now...to deduce the laws of market conduct—to find out what can be said about the way the consumer will react when prices change. Discussion of equilibrium conditions is always a means to an end; we seek information about the conditions governing quantities bought at given prices in order than we might use them to discover how the quantities bought will be changed when prices change. (1946, p. 26)

This makes it sound as if Hicks is going to offer a theory of dynamics — an account of how consumers will react, of the consequences of their reactions, the consequences of those consequences and so forth. But in fact he offers only a “comparative-statics” account of how the new equilibrium will differ from the old. Little is said about how severely this limits economics.

Why are the limits so severe? In comparative statics, economists take preferences, resource endowments and technological possibilities to be given. When there is, as in the case of the growth of income inequalities, an exogenous technological change or an exogenous lowering of trade barriers, one holds fixed preferences, endowments and other technological possibilities in considering the consequences for income distribution. But this is a reasonable first approximation only in the short run. Indeed Wood argues that it is not a reasonable approximation even in the short run. He maintains that conventional calculations of the

consequences of lowering trade barriers mistakenly overlook the search for new labor-saving technologies that cheap imports stimulate (1995, p. 67). It is no secret that preferences adapt and that new technologies are sought. So the standard framework of comparative statics is appropriate only for the short run. But it is precisely in the short run that the consequences of adjustment processes are likely to be most pronounced. Furthermore, equilibria are generally not unique. Without an account of the path leading from one equilibrium to another, there is no general way to say which of the new equilibria the economy will settle in. These complaints about comparative statics do not, of course, remedy the inadequacies of theories of adjustment dynamics. Demonstrating the need for such theories does nothing to satisfy that need, and it may be that economists cannot overcome these explanatory limitations. But they could be less complacent, and more research effort might be devoted to adjustment mechanisms and disequilibrium processes.

5 Causal explanation and rational criticism

I have argued so far that economists need to give explanations, including explanations of individual choices and of dynamic processes, that these explanations are causal, and that consequently practical concerns with control go hand in hand with theoretical explanatory concerns. At this point a crucial complication enters the story. Causal explanations in economics typically include a link involving individual choice. If lowered trade barriers cause increased wage inequality within the U.S., then part of the story (though admittedly an uninteresting part) involves consumer choices of cheaper imported goods over more expensive domestically produced goods, decisions of executives to move factories to other countries where unskilled labor is cheaper, and so forth. Explanations of those choices are, I maintain, causal: the manager of a firm or a division would like to increase its profits or market share. Given the lowered trade barriers, he or she believes that goods of equal quality can be brought to market here cheaper by moving factories abroad. The belief and the preference jointly cause the decision to move factories abroad.

But the belief and the preference do not only cause the action. They also constitute *reasons* for the action. Causes are, of course, not always reasons. A tornado that causes the destruction of a factory is not a reason why the factory should be destroyed. Nor are reasons always causes: it is difficult to see how the weak reasons Iago gives for his hatred of Othello could have caused his terrible deception. But, as Donald Davidson (1963) argues, effective reasons — that is reasons that are actually responsible for the actions they are reasons for — are always causes. Some of the causes of choices that economists cite — preference orderings and expectations — are reasons, too, while others, such as some constraints, may be causes only.

The fact that explanations in economics involve both reasons and causes of the action to be explained has far reaching consequences, because reasons, unlike causes, can be *appraised*. There are good reasons for action and reasons that are not so good. If the causes of an action are also good reasons to do it, then one has *justified* as well as explained the action. If the causes are not good reasons, then one has raised doubts about whether the action was justified. The evaluation here is *rational* or *prudential*, not *moral*. To do something that there is good reason not to do is to act foolishly, not necessarily immorally. But it is hard to keep what is rational and what is moral completely separate, and the fact that explanations of choices in positive economics cite reasons is one conduit by which moral evaluations become entangled in positive economics (see Hausman and McPherson 1996, ch. 4). The fact that explanations of choices involve reasons also explains the peculiar fact that mainstream economics is built around a theory of rationality. This is not an optional oddity. Only rational beings can be moved by reasons. Agents must be rational (though not necessarily perfectly so) if their beliefs and preferences are to explain their choices. It is, then, no surprise that empirical difficulties with the

basic model of rationality have done so little to shake that model. The entire explanatory structure of contemporary economics rests on the rationality of economic agents.

This observation about explanation has important implications from the perspective of control. Since a factor such as a technological innovation or a lowered tariff barrier is a reason as well as a cause of a phenomenon such as increased income inequality, its operation is necessarily mediated by the beliefs and preferences of economic agents. A lowered tariff barrier that an agent *expects* to be temporary may have a very different consequence than a lowered tariff that an agent *expects* to be permanent. A technological innovation that implies work relationships in conflict with local norms may be adopted slowly or rejected altogether, while another innovation that is more compatible with “preferences” may be adopted rapidly. Arguments and values matter, though how much they matter cannot, of course, be decided from the philosopher’s armchair.

6 Conclusions

Even economists whose concerns are exclusively practical should be interested in diagnosing the causes of various phenomena, because an understanding of the causes helps one to control their effects. If one decides that Adrian Wood is right and that lowered trade barriers are the most important cause of increased inequalities, it does not of course follow that protectionism is the best way to diminish those inequalities, and Wood himself argues against this solution (1995, p. 78). But an understanding of the mechanism by which lowered trade barriers allegedly had this consequence helps one to identify other kinds of interventions to alleviate the problem. Regardless of one’s view of the ultimate goals of economics, explanation has a central place on the agenda.

Economists typically explain only the differences among equilibria. This is an unfortunate consequence of the weaknesses of their theories of adjustment processes. Those theories need lots more work, because adjustment processes matter a great deal, at least in the short-run; and it is only in the short-run that typical comparative statics explanations have any reasonable application.

The diagnoses economists give include an essential (if often uninteresting) role for individual choices, and the diagnoses are not acceptable if their explanations of individual choice are not acceptable. Although important links in the causal chain, citing beliefs and preferences often contributes little to explanations, since these factors are often superficial and the dependence of the outcomes on them is fragile. Explanations of individual choices cite the reasons as well as the causes of choices. This fact explains why there is an ineliminable normative element within economics and why economic theories are constructed around a theory of rationality.

There is a great deal more to be said about explanations in economics. Explanations in economics appear to be inexact or only approximate. They appear to involve idealizations and abstraction. They confine themselves to a strikingly short list of kinds of causal factors. All of these issues, which are not broached here (see Hausman 1992), must be addressed by any comprehensive account of explanation in economics.

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