Economists' Responses to Anomalies: Full-Cost Pricing versus Preference Reversals

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One feature of scientific disciplines historians and philosophers of science have often studied is how members respond to apparent disconfirmations. This feature has attracted so much attention because philosophers of science hold that a discipline cannot be a respectable empirical science unless it is appropriately sensitive to disconfirming evidence. If a discipline is so constituted that empirical difficulties with its theories are always ignored, then the discipline cannot justifiably claim to be a science, and it cannot justifiably claim that its theories are well-supported.

Philosophers are still arguing about what sort of sensitivity to disconfirming evidence is "appropriate" and what sort is not, and a further reason for studying how apparently empirical disciplines have responded to disconfirming evidence is that the historical record may guide one in formulating reasonable normative criteria. A variety of criteria are to be found in the philosophical literature. On a naive construal of Karl Popper's falsificationism, the only acceptable response to disconfirming evidence is to abandon the theory or to modify it in a content-increasing way. Presumably this need not be done overnight, and if a theory is doing some important job, scientists may legitimately continue to make use of it even after they have recognized that it is unsatisfactory and needs to be

The authors are grateful to Roger Backhouse, John Davis, Uskali Mäki, and Max Steuer for useful comments.
replaced. Historians have had any easy time establishing that scientists do not behave this way. Any sophisticated version of Popper's falsificationism will distinguish between prima facie disconfirming evidence—anomalies—and actually disconfirming evidence—refutations. For Imre Lakatos, the difference between the two depends on whether there exists a content-increasing alternative to the given theory. The notion of refutation in effect coincides with that of a methodologically justified rejection. On this construal, the maxim that refuted theories should be abandoned is unproblematic, since a theory is not refuted until it is abandoned. The issue then shifts to how to define content-increasing changes.

Although Thomas Kuhn writes history of science rather than prescriptive philosophy of science, he clearly believes that scientists have done their jobs in more or less the right way and that one thus can draw normative conclusions from the historical record. He finds that theories always confront apparently disconfirming data and that scientists are largely undisturbed by such empirical problems. They recognize that it is difficult to bridge the gap between theory and data and that it may take a long time to figure out how to apply their "paradigmatic" theory in a way that will eliminate the conflict with the data. But scientists are not always unperturbed about apparent disconfirmations. Given their commitments to certain theories, apparatus, mathematical techniques, and so forth, some empirical problems will be deeply disturbing. With respect to these phenomena, the theory ought to give the right answers. More and more work will (and should) go into studying such anomalies, and this work leads unwittingly to scientific revolutions.

Although Lakatos's work is usually regarded as more normatively demanding than Kuhn's, he may actually demand less of scientists in response to apparent disconfirmations than does Kuhn. If one adopts a stringent notion of increasing content, like one suggested by Lakatos himself, then few anomalies will become refutations, and sophisticated falsificationism might in practice underwrite scientific conservatism. This paradoxical consequence might also result from a weakness in Lakatos's characterization of "theoretical progress." On one plausible interpretation, Lakatos demands only that scientists modify their theories in a content-increasing way so as to cope with some disconfirmation or other. Thus, a "scientific research program" can arguably make splendid progress, even though its practitioners completely ignore almost all anomalies. The problem here is that his methodology leaves us with lit-
tle guidance concerning how to appraise how "central" or "important" a given anomaly may be.

This essay is concerned with how economists respond to anomalies. As the above thumbnail survey makes clear, we cannot borrow from the philosophical literature a well-established rule stating how economists ought to respond to empirical problems. Lacking clear-cut normative guidance, we shall "look and see" how economists have in fact responded to anomalies, and shall leave our appraisal until the end of the paper. It seems obvious that economists do not completely dismiss all disconfirming data and that data conflicting with detailed models are sometimes taken to require serious modification in those models. Beyond that, to diagnose how sensitive economists are to evidence that apparently disconfirms fundamental theories, we will make use of two detailed case studies, the first by Philippe Mongin (1992) on the "full-cost" controversy and the marginalist controversies of the 1940s and 1950s, and the second by Daniel Hausman (1992) on economists' reactions to the discovery in the 1970s of the "preference reversal" phenomenon. These two case studies suggest relevant conclusions about the evolution of economic methodology.

1. The Full-Cost Pricing Controversy

In 1939 the Oxford economists R. L. Hall and C. J. Hitch published an essay arguing on the basis of questionnaire data that firms set prices by marking up average costs (at some reference level of output) by some fixed percentage. In the wake of this essay, other economists, mostly in Britain and America, found evidence of this "full-cost" policy, particularly in oligopolistic markets. Although Hall and Hitch were ambivalent about whether such pricing behavior could be incorporated into profit-maximizing models of the firm, full-cost pricing came to be seen as a radical challenge to orthodox models of the firm, and for roughly a decade and a half a lively controversy raged. The controversy was resolved in the early to mid 1950s with the vast majority of economists convinced that the essentials of firms' pricing policies were compatible with the standard profit-maximizing theory. Some disagreement remained concerning exactly to what extent companies employ full-cost pricing policies, but this disagreement was of little importance, since there was a consensus that the answer did not bear on the adequacy of the standard theory.
From a normative perspective, this controversy leaves a great deal to be desired. The contending positions were never clearly defined, and those committed to the standard theory often defended its assumptions about the motivation of the firm by confusing them with much weaker and less controversial assumptions, such as that the firm takes into account the advantages and disadvantages of particular decisions. This conflation of the optimizing models of the then-prevailing theories of imperfect competition with a watered-down and commonsensical view of business decisions has been dubbed “benign marginalism” by Mongin (1990–91, 1992); it is perhaps the single most important phenomenon of the controversy. For example, in responding to a passage in which another heterodox economist from the Oxford group, P. W. S. Andrews, had described entrepreneurs as rationally deciding not to undersell their competitors, Austin Robinson concluded: “I find it hard to distinguish this balancing of the advantages and disadvantages of price cutting . . . from the balancing process which the theories of imperfect competition have assumed” (1950, 778). In fact, Andrews had in mind a rational but completely unspecific method of reaching decisions.

Not only was there this confusion, but also there was little serious use of evidence. Data on costs and demand were scanty, and statistical methods were then underdeveloped, but the hypothesis that theories of imperfect competition could encompass full-cost pricing could have been tested. All parties claimed the empirical evidence for themselves, but nobody thought to carry out any serious tests. Urgent empirical questions suggested by the questionnaire data (concerning, for example, the role of demand and the determination of output) were never followed up by either side. And when the controversy died out, the initial theory remained with remarkably little conceptual or empirical progress having been made.

As the full-cost debate was taking shape, a related controversy took place in the American Economic Review of 1946–1948. It started with two strongly antagonistic essays by R. A. Lester and F. Machlup, and evolved into a lively and fairly inconclusive discussion of the strengths and weaknesses of the prevailing theory of the firm. This “marginalist controversy” included an assessment of full-cost pricing by Machlup but also touched on related issues, such as the shape of cost curves, the prevalence of oligopolistic behavior, and the role of quantity and price adjustments versus other adjustments to exogeneous changes.

One important consequence of the full-cost and marginalist controversies was that economists grew more conscious of the gulf between the
terms in their theories—profits, prices, marginal costs, and so forth—and the prices, profits, and costs that make up the reality of business life.¹ Related to this realization was a lasting (and controversial) methodological contribution. Economists increasingly came to hold that their theories were to be judged exclusively by their predictions concerning prices and quantities. This view has a twofold implication. First, economists should not be concerned with the way in which business decisions are made; only the final results of the agents’ deliberations matter, not the procedures. Second, not all of the results matter either; only price and quantity changes really count. Adjustments to changes in cost and demand such as selling efforts, increased efficiency, or bargaining with the working force and competitors, should be seen as negligible or as mediated by quantity and price adjustments. This restrictive doctrine became influential toward the end of the controversies, and it made economists receptive to the related methodological views defended by Milton Friedman (1953) and Machlup (1955) (on this connection see Mongin 1986, 1992).

Though there is much to criticize, at least there was a controversy. Economists discovered an anomaly, and the profession was galvanized to discuss it. The anomaly was certainly not ignored. On the contrary, it attracted attention from leading theorists in leading journals and at important conferences, and the confidence of the profession in some of the central theories of economics was seriously, if temporarily, shaken. Economists clumsily attempted to make sense of the anomalous data, while grappling with the problem of how to operationalize claims about marginal revenue or marginal cost curves or about the differences between competitive, monopolistic, and oligopolistic markets.

2. Preference Reversals

The second reaction to unfavorable evidence with which we are concerned belongs to an altogether different context. In a 1968 study of how individuals evaluate gambles, psychologists Paul Slovic and Sarah

¹. It is instructive to compare Joan Robinson’s (1933) discussion of the basic concepts of the theory of the firm with Machlup’s (1967) late exposition of the same topic. Robinson was aware of the semantic gap between the technical notions of a commodity and of an industry and their counterparts in ordinary language, but she could write rather boldly that “a firm is a concern very similar to the firms of the real world” (1933, 17). By contrast, Machlup insisted that the economic theorist’s firm is primarily a “theoretical construct.” In the intervening time the controversies had exercised their disenchanting influence.
Lichtenstein were led to claim that it should be possible to find pairs of
gambles with the following counterintuitive property: agents who claim
to prefer one will be willing to pay more for the other. Consider, for
example, a gamble that pays off $4 with probability 35/36 and requires
one to pay $1 with probability 1/36. The expected monetary value is
$3.86. A second gamble pays off $16 with probability 11/36 and with
probability 25/36 requires one to pay $1.50. Its expected value is almost
the same—$3.85. Slovic and Lichtenstein predicted that among those
people who claim to prefer the gamble with the $4 payoff, many will of-
fer to pay more for the other gamble! They also claimed that one would
not find such “preference reversals” in the other direction—that is, that
one would not find people who say they prefer the gamble with the $16
prize and who are willing to pay more for the gamble with the $4 prize.

These claims were predictions of novel facts, and they were confirmed
by experiments reported in a 1971 article. The results were strikingly
replicated in an experiment in which the gambles were actually played
on the balcony of the Four Queen’s Casino in Las Vegas. More than two-
thirds of individuals who prefer gambles with low prizes and low risk
(such as the gamble that pays off $4) assign a higher price to gambles
such as the one that pays off $16. Although these results were discov-
ered by psychologists, their relevance to economics is evident. If people’s
preferences between two gambles depend on the mode of elicitation, then
the axioms of ordinal utility theory are pointless or refuted. Economists
noticed Slovic and Lichtenstein’s findings, and in a 1979 article in the
American Economic Review, David Grether and Charles Plott set out to
show that these results would not hold in experiments that more accu-
rately reflected the environment in which economic choices are made.
Instead, they wound up replicating Lichtenstein’s and Slovic’s results.
Grether and Plott recognized that these findings constitute a striking dis-
confirmation of the theoretical core of orthodox economics, but since
there is no alternative with the same extremely wide scope, they did not
recommend abandoning or modifying the theory, or even diminishing
one’s reliance on it.

After Grether and Plott’s report, the preference reversal phenomenon
attracted a good deal of attention, and the American Economic Review
published a number of articles devoted to it. Very few of these essays
took seriously the psychological theorizing that had motivated Slovic and
Lichtenstein’s prediction: subjects would, Slovic and Lichtenstein main-
tained, focus much more on the size of the prizes in pricing gambles than
in choosing among them, hence the discrepancy. Different practical tasks evoke different forms of cognitive processing. Nor were economists impressed by the fact that the psychologists had predicted the phenomenon before it was observed. Indeed, some of these essays showed only a faint grasp of what the phenomenon is (Cox and Epstein 1989). The bulk of the work was devoted to efforts (which were sometimes extremely ad hoc) to make the phenomenon go away or to show that it did not, after all, refute ordinal utility theory. Given that the choices involve gambles, it is not altogether surprising that economists, such as E. Karni and Z. Safra (1987), tried to interpret the preference reversal phenomenon as another failure of the empirically weak link in expected utility theory: the von Neumann–Morgenstern independence axiom. Others put the blame on the “reduction of compound lotteries” axiom (Segal 1988). There have also been suggestions that preference reversals could be accommodated by versions of utility theory that permit intransitivities (e.g., Loomes, Starmer, and Sugden 1989).

Another intellectual factor had a more important role in dulling the impact of preference reversals: economists who studied this anomaly internalized an implicit constraint on the scope of acceptable economic explanations. The reason Grether and Plott were not more deeply disturbed at having replicated the findings of reversals is that they take it for granted that a competitor to orthodox theory must, like orthodox theory itself, have an extremely wide scope: “The fact that preference theory and related theories of optimization are subject to exception does not mean that they should be discarded. No alternative theory currently available appears to be capable of covering the same extremely broad range of phenomena” (1979, 634). For this reason, Grether and Plott could not be impressed by Slovic and Lichtenstein’s explanatory hypothesis: a theory which applies only to pricing gambles is simply not a competitor.

3. Some Similarities in the Reactions to Anomalies

In the case of preference reversals, unlike the case of full-cost pricing, there has been no active controversy. Yet, unlike the consensus view of full-cost pricing, preference reversals appear to pose a serious empirical problem for the fundamental theory.

Before addressing this puzzle, we will point out the many important similarities in the reactions of economists to both problems. First, in neither case was the evidence seriously denied. Admittedly, Machlup in
1946 cast doubts on the data collected by the antimarginalists, particularly on the grounds that they were unrepresentative and biased by the questionnaire technique. But this strategy of denying the evidence was not consistently pursued, even by Machlup in his 1946 article. R. F. Heflebower’s 1953 report, which brought the American part of the controversy to an end, contains a careful summary of all existing evidence on full-cost pricing (see Heflebower 1955). He concluded that the existence of this pricing method was well-established on certain types of oligopolistic markets, but only on those; it was widespread but not as universal as the Oxford economists had claimed. Similarly, if Grether and Plott’s initial reaction was to suspect that Slovic and Lichtenstein’s data were spurious, they changed their mind after replicating the data. Later experimental work by economists on preference reversal led them to qualify claims about the frequencies of reversals (Reilly 1982), but never to deny that reversals of the kind predicted occur on a significant scale. Even if economists are by and large more interested in theorizing than in searching for new data, they do not disregard evidence.

Second, in each case there were attempts to reconcile the evidence with existing theory. These attempts had similar weaknesses: only part of the evidence was taken into account, the attempted reconciliations depended on unsupported auxiliary hypotheses, and models were juxtaposed in an unsystematic way. Different defenders of orthodoxy maintained that different constructions in Joan Robinson’s “box of tools”—such as the standard monopoly, long-term competition, and various oligopolistic constriuals—were relevant to full-cost pricing, but nobody showed that full-cost pricing could formally be derived from standard theory with the help of any of these. In addition, the required auxiliary assumptions were not subjected to independent tests; most of the time they could not be tested because they had not even been clearly stated in the reconciliating “model.” There were fewer but much more precisely articulated reconciliating attempts in the preference reversal case. The most typical one was the already-mentioned strategy of reducing the reversals to another case of expected utility violation. Auxiliary hypotheses were clearly stated, but they remained unsupported and apparently ad hoc. Part of the evidence was left out, notably the crucial fact that “unpredicted” reversals (preferring the high-prize gamble but pricing the other higher) rarely occur.

Third, in both cases defenders of accepted theory attempted to blunt the criticisms by denying that any alternative theory accounted for the
anomalies. Hall and Hitch’s survey was taken by many, especially in England, as suggesting a new industrial doctrine: hence, the curious expression, full-cost principle. This portentous reconstruction of the Oxford economists’ work partly reflected their own meaning. For instance, in Andrew’s Manufacturing Business (1949), full-cost pricing was elevated to the level of a behavioral postulate competing with “marginal revenue equals marginal cost.” This theoretical twist of the controversy in England facilitated its resolution there: one reason full-cost pricing was declared to be an unimpressive anomaly was that the theoretical alternative to which it apparently led—to take full-cost pricing as the fundamental decision-theoretic assumption of the theory of the firm—was grossly unsatisfactory. The conflation of the full-cost anomaly with the expression of a new doctrine that marred the British debate also shows itself in the American debate, even though the latter was more thorough and brought out a few novel facts in the course of the discussion. In the case of preference reversals, many economists have remained complacent on the grounds that no alternative—or at least none that economists are willing to entertain—accounts for the phenomenon either. In the 1980s, this methodological argument was sufficiently well-known to be made briefly and crushingly. The defense of orthodox theorizing on the grounds that no “satisfactory” alternative exists is regarded by many economists as one of the indisputable contributions in Friedman’s (1953) controversial essay. There were numerous occurrences of this strategy from the late 1950s onward (e.g., Koopmans 1957).

4. Explaining the Dissimilarities

The common features of the economists’ reactions to the two anomalies are perhaps not so striking as the dissimilarities. One would have thought that the preference reversal phenomenon ought to have mattered to economists more than full-cost pricing. The theory it casts in doubt is more central. The phenomenon itself is better established, and its bearing on theory is less ambiguous. Yet preference reversals have been of interest to relatively few economists, while work on full-cost pricing gave rise to a major debate. If the full-cost pricing and marginalist controversies show the profession awkwardly coming to terms with an ill-defined phenomenon of dubious relevance, the discussion of preference reversals finds the profession reluctantly establishing that a certain phenomenon does indeed disconfirm the central theory of con-
temporary economics, and then treating this conclusion as of no great importance.

What explains this apparently striking difference? The first thing to emphasize is that the work on full-cost pricing was done by economists and concerned behavior that was believed to be clearly within the purview of economics. The work on preference reversals, in contrast, was initially done by psychologists, and the importance of preference reversals in market contexts is questionable. One reason why there was a controversy over full-cost pricing (but not over preference reversals) is that full-cost pricing seemed to challenge conclusions traditionally regarded as *economic*, while preference reversals did not. Indeed, Grether and Plott’s starting point was the conviction that preference reversals were artifacts of psychologists’ experiments and that they would not occur in a context where actual buying and selling took place. Since Grether and Plott’s replication of the preference reversal phenomenon conflicts with this conjecture, it would seem that we have not yet clarified the difference in reactions to anomalies from one controversy to the other.

Here is perhaps a better formulation: in the face of suitable replications, economists might concede that preference reversal could be seen as a phenomenon that actually occurs in economic contexts; but at the same time, they might deny that such problems should shake their confidence in standard theorizing. They might say, “Obviously the fundamental principles of economics are a great oversimplification. If one takes them literally, they’re false. The serious question is whether they do their job of explaining and predicting market phenomena well.” By contrast, the full-cost pricing data directly bore on those market-related predictions which economists have always regarded as central. By claiming that full-cost pricing policies are really explained by the fact that “producers cannot know their demand or marginal revenue curve,” Hall and Hitch had suggested that pricing is insensitive to changes in demand. This was a direct challenge to the optimizing model set up by Joan Robinson: that marginal revenue equals marginal cost.

We have no doubt that this difference in the “bearing” of full-cost pricing and of preference reversals goes a long way toward explaining why preference reversals have been of so little concern. But this explanation is shallow as long as it is not put in historical perspective. What is within the purview of economic theory is not simply given. It depends on methodological decisions. This explanation presupposes that economists are only concerned about implications concerning changes
in prices and quantities. When mentioning this doctrine in section 1, we made the point that its canonical formulation, in particular in Machlup (1955), was not independent of the full-cost pricing and marginalist controversies. It did not preexist the initial reactions to Hall and Hitch’s or Lester’s findings, and it was very much influenced by the way the discussion evolved. On the other hand, this doctrine—that only the predictions of theories concerning (changes in) prices and quantities matter—could be taken for granted a generation later when preference reversals came to be discussed.

A second and related explanation for the differences in the economists’ reactions is that full-cost pricing appeared to be of immediate relevance to economic policy, while the relevance of preference reversals is dubious. We think that there is something to this explanation, but that it also is superficial. Like many empirical studies of pricing pursued in the 1930s, Hall and Hitch’s inquiry was motivated by the main economic riddle of the time: Why had industrial prices remained sticky after the Great Crash, leaving output and employment to adjust to the new conditions? The Oxford group was unavoidably concerned with possible remedies to price stickiness, typically in terms of regulating competition on oligopolistic markets. It is thus readily understandable why full-cost pricing attracted so much attention.

But does the preference reversal phenomenon have no policy implications? The dependence of preference on mode of elicitation could matter to the firms’ advertising policy and even to the way in which they determine the quality of products. This might have consequences on regulation of competition and consumer protection. By and large, the greater and more systematic the defects in individual decision making, the more tenuous the case for laissez-faire. At a more theoretical level, much of welfare economics is based on a “willingness-to-pay” measure of individual advantages and disadvantages, and would simply crumble if that measure were not also indicative of a preference ranking. One wonders what would be left of the notion of a Pareto-improving transfer if more than one preference concept were required. Even if, unlike full-cost pricing, preference reversals do not bear immediately on contemporary policy discussions, it would be surprising if economists could not look beyond the moment’s debate and see the policy relevance of a major challenge to their fundamental theory. A passage in Grether and Plott’s article (1979, 624) shows that at least some of them did. Even more than the first, the second explanation raises more questions than it answers.
A third—and, in our view, more important—explanation is that there have been significant methodological changes within the economics profession. First, the profession has become more sensitive to the need for testing and to the requisites of good tests. Although there have been some episodes since the full-cost pricing controversy in which concerns about testing have played a surprisingly small part—witness the so-called Cambridge controversy (Hausman 1981)—one would expect contemporary economists to discuss full-cost pricing evidence more carefully than did their predecessors. Second, the profession is on average more hostile to survey data than it was fifty years ago. A study such as Hall and Hitch’s would have a hard time getting a hearing today, even if it relied on a larger sample and a more sophisticated design. The hostility has tapered off over the last decade, but it seems fair to say that the average economist is still deeply suspicious of surveys.2 These two methodological changes help explain why a major debate concerning full-cost pricing might not take place nowadays, but they obviously do not explain why the preference reversal phenomenon has been largely ignored.

The methodological change we would like to emphasize connects with the following finding: the exact interpretation of fundamental theory was not yet settled during the period of the full-cost pricing controversy. As explained, even major theorists such as Austin Robinson and Ronald Coase did not clearly distinguish between profit maximizing and “rationality” in some less clearly delineated sense. At a rather late stage of the discussion, E. H. Chamberlin could write approvingly of the full-cost “principle” on the grounds that it showed the profit-maximization assumption to be dispensable in the analysis of industrial competition. Chamberlin’s statement makes it clear in retrospect that there had been a gulf yawning between his own Theory of Monopolistic Competition (1933) and Joan Robinson's Economics of Imperfect Competition (1933). Still, the two approaches were often conflated in the 1940s under the label “theories of imperfect competition.”

At the same time as the content of economic theory has been made definite, the work of neoclassical economists has become more tightly constrained by theory and less eclectic. In the late 1930s many economists were more open-minded. For example, the extent to which they regarded the shape of average cost curves as an empirical matter is evidence that

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2. At least when economists do the surveys. As Robert Goldfarb has pointed out, economists have few qualms about employing government statistics, which are often the results of surveys.
they were prepared to give up part of the traditional framework. A leading economist such as Stigler could accept the full-costers’ conclusion that (contrary to the Robinsonian picture) the average cost curve is flat on the relevant operating interval. Some major economists—Roy Harrod (1952) is one striking example—were even prepared to consider the possibility that the standard theory of the firm might be badly mistaken. In the 1980s, in contrast, economists were much more firmly convinced that their theory, though not necessarily fully correct, was nevertheless the best possible first approximation.

5. Some Philosophical Conjectures

The discussion above highlights the differences between the two episodes, but at the same time it points to subtle similarities in the way economists respond to anomalies. We would like now to elaborate on this dialectical point. The reader will have noticed that in their responses to the two anomalies economists made some of the same arguments, even if the novelty, degree of explicitness, and relative weight of these arguments varied from one case to the other. Analyzing these common arguments, we are led to highlight the following two features, which are already invoked by Hausman (1992) as part of his general assessment of economic theory.

First, economists often defend their particular theories by arguing that their discipline is separate from other modes of social inquiry. Most of the economists’ research on the preference reversal phenomenon was driven by the question, Is it possible that the psychologists’ findings survive in an economic setting? More discretely, but already there, this commitment to a separation of economics underlies the controversies over the firm. Witness the following pronouncement by Joan Robinson at the outset of her treatise:

The single assumption which it is necessary to make . . . is that the individual firm will always arrange its affairs in such a way as to make the largest profits that can be made. . . . It is this assumption that makes the analysis of value possible. If individuals act in an erratic way, only statistical methods will serve to discover the laws of economics, and if individuals act in a predictable way, but from a large number of complicated motives, the economist must resign his task to the psychologist. (1933, 6)
It might be asked, would it be such a disaster if economists resigned their investigation of motives to statisticians and psychologists? Robinson’s paradigmatic answer is that in no circumstances should economists resign because if they did, economics would not exist as a distinct science.

The controversies suggest that separation can be seen in two ways: either as a primarily methodological concept, related to the “formal” definition of economics by Robbins and others, or as a substantial constraint on what causal factors one attends to, in a way consistent with John Stuart Mill’s philosophy of the social sciences. Hausman’s (1992) account emphasizes the latter view. Either way, separation provides a key to the puzzling question of how economists can live with anomalous phenomena within the usual territory of economics (in “situations where economic theory is generally applied” to use Grether and Plott’s words).

Second, potential economic theories face a maximal scope constraint, which also helps keep economics separate from other fields of inquiry. Remember the way Grether and Plott discredited the psychologists’ accounts of preference reversals. A similar scope requirement is implicit in Joan Robinson’s consideration of a “single” fundamental assumption throughout her theory. Machlup’s final word against Lester is that “The ‘mental ruts’ of the marginalists are equipped to take care of all the economic considerations which Professor Lester has mentioned as factors in business decisions. This makes marginal analysis less simple but more revealing than a theory which tries to explain the volume of employment in the firm solely with reference to its sales possibilities” (1947, 154). Lester’s unconventional hypothesis (that employment is determined by sales prospects) is disqualified on the grounds that it does not apply to the same broad range of situations as profit maximization. The Robinsons rejected Harrod’s and Andrews’s suggestions that full-cost pricing be taken as a primitive assumption for a similar reason: it would cover only a limited range of industrial situations. They added the related but distinct point that even in its range, the full-cost “principle” would not explain everything. This last distinction helps clarify the scope requirement as follows: not only should the alternative hypothesis apply to at least as many phenomena as does the existing theory but it should also explain them with at least the same precision. Notice the analogy between the full-cost “principle” and Slovic and Lichtenstein’s behavioral hypothesis: the latter lacks not only a broad scope but also precision. For example, it does not say how a “preference reverser”
would compose a portfolio of $-bets and P-bets if he or she were given the choice.

The separation and the maximal scope arguments play an overt role in the preference reversals case. This study shows that they were also present in the earlier controversies. Having identified these common factors, we would like to offer a speculative answer to our major puzzle: Why were economists so much more dogmatic in one case than the other? In the preference reversal case, the two methodological points just identified are mutually supporting: psychological explanations happen to be partial in the sense excluded by the scope constraint, so that this requirement strengthens the effect of the disciplinary or causal separation strategy. By contrast, in the full-cost pricing case, only the maximal scope requirement is fully binding. The other argument, that economics should be separate, is effective, but not to the same degree; in effect, it served only to exclude the more procedural interpretations of profit maximization without going to the heart of the full-cost debate. The major problem in the full-cost and related cases was not the fact that firms did not make calculations in the way marginalism had suggested, but the apparently anomalous reactions of firms to changes in economic conditions. Hence, the need for a reinterpretation of at least some of the negative evidence. In sum, economists were consistent with their long-term commitments by being completely unwilling to consider the alternative theory in one instance, and merely reluctant to do so in the other.

In comparing reactions to the two anomalies, we have tried to be primarily descriptive. But such self-constraint has its limits, and we would like to close with some comments on whether we think economists' reactions have been justified. In our view, though economists cannot take much pride in either episode, those who want to dismiss economics as ideological pseudo-science should be taken aback. In particular, the view that economists pay no attention to evidence is refuted by our narrative. Moreover, we can see some limited form of progress— particularly in the appreciation of what counts as a model and what counts as a test— as well as in the appreciation of the need for testing. The comparison between the two episodes is, however, for the most part disquieting, if only because it confirms the pervasive role of the separation and scope constraints in stopping controversies before they really take off.

As noted in the introduction, contemporary philosophy of science provides no precise guidelines concerning how scientists should respond to anomalies. So one cannot draw on any established philosophical rules to
say whether economists have responded as they should have. But useful hints may be drawn from the works of Popper, Lakatos, and Kuhn. For instance, we noted Kuhn's point that anomalies influence the articulation of a paradigm, even if for a long time they coexist with unchanged formulations of accepted laws and facts. One finds little such influence of full-cost pricing or preference reversals. There are few echoes of either in textbooks or treatises in industrial economics or choice theory. The fact that conventional economics tends to ignore past anomalies points, we think, to a failure of economics as a science rather than a failure of philosophy of science to advise economists.³

In this context we can do little more than to assert these normative judgments. We must also hasten to add that very recently one finds some conflicting trends in the methodological development of economics. The hostility toward questionnaires is perhaps fading, as survey results are once again appearing in major journals. At the same time that economists are, on the whole, more focused on implications concerning price and quantity data than they were in the 1930s, there is also a great deal more experimenting and theorizing that is specifically concerned with the fine grain of decision making, and sometimes even with the procedural side of decisions.⁴ The growing importance of this body of work should redirect attention toward other things than market prices and quantities.

References


³. Full-cost pricing is still discussed, and sometimes even actively, outside conventional, "neoclassical" economics. Managerial economics is one example. "Post-Keynesian" economics is another; on this particular connection, see F. S. Lee 1984, and the winter 1990–91 issue of the *Journal of Post-Keynesian Economics*. Post-Keynesians have followed Andrews in basing an alternative doctrine of price decisions on full-cost pricing. However, it could be argued that Post-Keynesians do not study the anomalies faced by the full-cost doctrine any more than orthodox economists do the anomalies faced by the standard theory of the firm! To make this criticism in any detail would have distracted us from our main point. But we would like to suggest that there is a sense in which economics at large, not only conventional economics, ignores the past anomalies.

⁴. On the experimental side, see in particular the collection recently edited by J. K. Kagel and A. E. Roth (1995). Among many other things, this volume contains a thorough discussion of evidence relative to the preference reversal phenomenon.


