The "Full-Cost" Controversy of the 1940s and 1950s: A Methodological Assessment

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1. Introduction and Overview

This essay is part of a larger attempt to investigate the significance of criticism and theory change in economics by means of historically documented case studies. There is of course nothing exceptional about the twin themes of *criticism* and *theory change*. The former is what is left of the refutationist enterprise when the latter becomes assailed with doubts, and concern with the latter is the hardly disputable legacy of the growth of knowledge movement of the 1970s. It is more to the point to make a brief argument for case studies in economic methodology. This general investigation has been prompted by strong dissatisfaction with the way in which current methodological work relates to actual economic theory. On the one hand, there has recently been in the field a growing realization that abstract philosophies of science such as the growth-of-knowledge type, were only, at most, partially relevant to the understanding of the economist's work. On the other hand, the alternative views, especially the rhetorical ones, which have been sparked off by these doubts, themselves remain rather abstract and are only loosely connected with historical evidence. Not even the alleged failure of the older tradition has been documented in a satisfactory way; for instance, we still lack an account of where Lakatosian accounts of

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economics go astray. The point is that the new theorists have usually relied as much as the old ones on the search for examples of their favorite views, an attitude which is open to the charge of casual verificationism. By contrast, the merit of the case study method is that it brings forward material that offers some resistance and differential resistance at that. Each single case is normally amenable to several accounts. None of those should be expected to be entirely satisfactory, but they can be argued for and against, and occasionally one of them wins the point. I will stop at this purposefully somewhat reductive account of the method, leaving open the relevant issue of whether or not falsificationism (or some variant of it) should be restored at the metatheoretical level.[1]

A major reason for selecting the early full-cost debate as one of the case studies is that it fits the pattern of comparison just suggested extremely well. Section 2 offers a broad outline of the discussions initiated by Hall and Hitch's famous article (1939) on "Full-Cost Pricing" (FCP) and their criticism of profit maximization. I follow the exchange of arguments up to about 1955 when the matter was generally regarded as settled to the advantage of the received doctrine. This succession of climax and relief turns out to be *prima facie* compatible with at least the following three accounts of theory change in economics: 1) the falsificationist view of theory change, which could be relevant here *at least normatively and critically*; 2) the rhetorical stand (the debate took place in terms of arguments rather than proofs and refutations and was perhaps resolved satisfactorily in an *ad hominem* sense); 3) the view that the received theory of the firm is inherently antagonistic to informative testing, since its basic concepts are circularly related to each other, making them nonempirical in any interpretation.

Sections 3 and 4 go in detail into the FCP evidence and the accompanying argument against profit maximization. Sections 5, 6, and 7 reorganize the highly diverse attempts of the more traditional economists to accommodate FCP within the purview of "marginalism" and the "theories of imperfect competition," to use contemporary parlance. The three sections analyze intricate material in terms of these basic distinctions: decision-theoretically versus nondecision-theoretically concerned accounts; *ex ante* versus *ex post* models; and static versus

1. This delicate issue was raised by R. Weintraub during a seminar. As with lower-level methodological problems, I shall leave it to the case study itself to provide a preliminary answer.

comparative-static views of the firm. While emphasis is laid on the marginalists' defensive strategies, two related issues are discussed in passing: the connection (or lack thereof) between the FCP crisis and the reformist or deviant hypotheses about the firm's behavior, such as Baumol's or Simon's, that flourished from the mid-1950s onwards; and the partial genesis of Friedman's irrealism thesis that can be found in the FCP as well as related "marginalist" controversies. The main conclusion in section 8 offers a comparison of the *prima facie* plausible accounts as listed above.

2. Sketch of the Full-Cost Debate and Related Discussions of the Time

In 1939, a group of Oxford economists published findings on business behavior that they had gathered with questionnaires. As Harrod pointed out in an accompanying comment, several of the findings were disturbing in light of the received doctrine. Especially worrying for the prevailing "theories of imperfect competition" was Hall and Hitch's article, "Price Theory and Business Behaviour." The authors testified to the fact that firms very often set their prices in a "full-cost" way, for example, by first estimating an average-cost figure at some suitable output level and then adding to it one or more percentage margins. This fact was at variance, or so Hall and Hitch claimed, with the usual profit-maximizing view of the individual firm, which had been a component of the received doctrine at least since Joan Robinson's *Economics of Imperfect Competition* (1933). It is true that Hall and Hitch were ambivalent enough towards profit maximization to *also* suggest a more orthodox account of FCP. That account involved them in the invention of the "kinked demand curve," a minor but curious case of simultaneous discovery in the history of economic thought.[2] In the years following Hall and Hitch's article, evidence accumulated on the high frequency of FCP behavior, both in trade and manufacturing; it was usually found to be connected with oligopolistic markets. The evidence was gathered by economists of various persuasions, some of them as ambivalent towards profit maximization as Hall and Hitch had been,

2. The device of the "kinked demand curve" was independently used in America by Sweezy (1939). A careful reading of *The Theory of Monopolistic Competition*, chap. 5, suggests that the it might have existed already (see Chamberlin's use of *two* demand curves for the firm in the derivation of his "tangency solution").

and the evidence was cross-checked to some degree. By the mid-1950s the empirical significance of FCP had by and large become common ground to most economists with an interest in the firm (note that today's distinction between microtheory and industrial economics had then just begun to come out in the open).[3] Theoretical interpretation was thus the pending issue.

The first significant defense of the received doctrine against the critical implications of FCP is to be found in Machlup 1946. This article is historically pivotal since it did much to sharpen the conflict between Hall and Hitch and the rest of the profession. Even a simple terminological innovation proved effective in structuring the subsequent discussion—the "marginalist" versus "antimarginalist" distinction. Machlup's highly polemical stand as well as this distinction can only be understood against the background of Lester's (1946) earlier attack on the theory of the firm. Lester claimed that the theoretically predicted wage-employment relationship did not fit the empirically observed one. This was of course an entirely different objection from Hall and Hitch's, and it was clear that Lester had a distinctive view of the firm's decision process, one in which output rather than price is the main command variable. However, Machlup attempted to answer two strands of criticism at the same time which resulted in a dramatic twist to the FCP debate. Thereafter, the FCP debate could not be kept entirely apart from a wider set of discussions—the "marginalist controversy"—which developed in American journals, especially the *American Economic Review,* from 1946 to 1950 and was mainly directed toward the arbitration of the Lester-Machlup disagreement.[4]

Machlup's contribution to the interpretation of FCP is threefold. He managed to dispute the quality and relevance of the evidence, and at the same time, to claim that data on price-setting were compatible with several of the available models of imperfect competition; he also

3. In the postwar years the view gradually emerged that industrial economics was a distinctive field of study despite the fact that it had to borrow a large part of its conceptual apparatus from price theory. This view was argued by economists as diverse as Andrews 1952 (in the first issue of the *Journal of Industrial Economics*) and Heflebower 1954. It should be contrasted with the implied view in the "theories of imperfect competition" that industry is just one among many cases of application of general price theory. Machlup's postwar treatises are late examples of that view.

4. The present article will deal only with those parts of the "marginalist controversy" which bear upon FCP. The remaining parts are discussed at length in Machlup 1967, Lee 1984 and Mongin 1986.

sketched a general decision-theoretic argument to the effect that "rules of thumb" (the expression in Hall and Hitch) often reflect an underlying optimizing process. Most of the later neoclassical arguments are already in Machlup's proteistic plea. His general conclusion was that the current theory of the firm hardly needed revising even if the allegedly damaging findings were taken at face value. Still, several of Machlup's arguments involved a redefinition of the main behavioral assumption of the theory—the shift going much further than the purely technical generalization of marginalism proper ("marginal cost equals marginal revenue") into profit maximization. This vacillation did not escape the notice of Oliver (1947) and Gordon (1948) in the *American Economic Review* exchanges, which ended around 1950 but did not indicate a loss of interest in full cost yet.

Lee, who has devoted extensive and useful historical study to FCP (1983, 1984), claims that the American debate came to a halt with Heflebower's detailed summary of the pros and cons at the 1952 *Business Concentration and Price Policy* conference. Contrary to Machlup, Heflebower did not really question the main evidence; rather, he tried to make it more precise. His final assessment is close to Machlup's conclusion that the current theory of imperfect competition can take care of FCP, although he was much more explicit that *some* of the models in Joan Robinson's "box of tools" had to go. He suggested that profit maximization had to be understood in a long-run sense and that oligopolistic markets should become the focus of the theory. From publications of the time, the average American economist seems to have concurred with this moderate sounding, empirically substantiated conclusion. Heflebower's survey (1955) is perhaps the single most impressive neoclassical contribution to FCP both in terms of its acquaintance with the issue and its historical impact. Not the least significant aspect of the latter is that Heflebower gave a definition of the full-cost "principle," classified its variants, and even extracted a body of "doctrine" from the then numerous and diverse writings on the subject. Heflebower's discussion of "full-costers," as we call them now, included Hall and Hitch (1939), Saxton (1942), Andrews (1949), Harrod (1952), as well as Neal (1942), Dean (1951), and Oxenfeldt (1951)—who were said only to give some support to the "doctrine." Questionable as it may strike today's scholars, this list is important in and of itself, as was Machlup's earlier terminological distinction, for it helped to provide a fairly diffuse discussion with the classical structure of a controversy.

A delayed exchange of arguments on FCP had started in England after the publication of Andrews's *Manufacturing Business* (1949), in which Hall and Hitch's price mechanism was revived under the label of "normal-cost pricing." Andrews deserves special mention amidst Heflebower's list of full-costers. More than the others, including Hall and Hitch, he actually tried to put forward a doctrine. His was meant to be a complete system of industrial economics, in which "normal-cost pricing" played a part along with other special features, notably a short-run linear cost curve, a long-run demand schedule, and a general emphasis on competitive pressure from existing business as well as potential entrants. The discussions which took place among British economists in the early 1950s, especially in the *Economic Journal*, perhaps laid more emphasis on the compatibility (or otherwise) of profit maximization with "normal" or "full-cost" pricing than Andrews and his collaborator Brunner (1952) had wished. The exchange of arguments focused more on logical compatibility and less on consistency with data than in the corresponding American debate. The most conservative stand was taken by Austin Robinson, who in effect claimed that all that was needed to accommodate Andrews's model was the very basic model of imperfect competition in Joan Robinson's "box of tools." More qualified views were also expressed, and among them, Wiles's (1950) reinterpretation of profit maximization in a special long-run sense stands prominently. Lee (1983, 1984) claims that the British controversy came to a halt about the same time as the American one (by 1955). It is indeed likely that a majority of English-speaking economists then believed that the standard, profit-maximizing "theories of imperfect competition" (A. Robinson's phrase) were well equipped to cope with FCP.[5]

As a matter of fact, writers—such as Andrews and Brunner—who persisted in claiming that there was something *specific* to industrial models involving FCP behavior were usually regarded as unimportant heterodox contributors by the majority of the economic profession. By and large FCP was "marginalized" in both senses of the word.[6] When

5. There was some awareness of the FCP debate outside the English-speaking world (e.g., Brochier 1951), but to the best of my knowledge the discussions over FCP that took place outside Britain and the U.S. were derivative only.

6. This article emphasizes the marginalization of full-cost pricing, leaving to others the exploration of the social marginalization of full-costers. The latter is a well-evidenced phenomenon, sometimes interacting with intellectual discussion in an important way. For rea-

interest in that price-setting mechanism was occasionally revived over the last thirty years, it came either from writers outside the mainstream, such as the post-Keynesians, or from empirically minded economists (there was some overlap between these two categories as well as, of course, a few exceptions).[7] The dismissal of these later FCP contributions by the middle-of-the-road economist can be explained in two different ways: either as a reaction to the heterodox denial that FCP is profit maximization in disguise, or as a reaction to the attempt to make FCP, whatever its connection with profit-maximization, part of a heterodox general model. This ambiguity underlies the early rejection of Andrews.

An altogether different picture of the long-run impact of the full-costers' work would emerge if it could satisfactorily be linked to the *dissident* or *reformist* views of the firm that became widespread at the very moment when the controversy was fading out. Simon's (1955) model of "limited rationality" and Cyert and March's (1963) "behavioral" approach to the firm were definite departures from orthodoxy since they were non-optimizing descriptions of individual behavior. Nearer reform than dissidence, there had been a growing realization of the plurality of the firm's objectives all through the 1950s. Already obvious in Papandreou's (1952) influential survey, this trend led to optimizing models of a novel sort, such as Baumol's (1959) sales-maximization hypothesis and Marris's (1964) "managerial" view of the firm. Quite independently, many economists had criticized the usual profit-maximization hypothesis on the grounds that it did not cover the case of uncertainty. The latter was thought to be relevant to the widely recognized fact that firms usually operate under reserve

sons of their own, Hall and Hitch did not take part in the debate over their work. Heflebower's analysis of FCP was no doubt influenced by the fact that no full-coster could respond to him at the 1952 conference, and further, that his conclusions were in some way expected by his audience. Even more clearly, the fate of Andrews's doctrine is related to his professional misgivings (on this, see Lee and Irving-Lessman 1992).

7. The heterodox, post-Keynesian revival of FCP is connected with the work of Eichner (1976). Nordhaus (1971), Godley and Nordhaus (1972) and Coutts, Godley and Nordhaus (1978) testify to a persisting spark of interest in full cost among empirical economists with a critical bias. Note that the empirical researchers just cited used econometrics, a technique which is noticeably absent from the discussions reviewed here. Edwards (1962), Sylos-Labini (1962), and Modigliani (1958), who elaborated on the previous work of Edwards and Sylos, are difficult to fit into a ready-made classification. So is Koutsoyiannis (1975), whose textbook has an exceptionally detailed and sympathetic account of the early controversies; she ends up, however, with the usual conclusion that "average cost pricing reduces to marginalism in a different language" (280).

capacity; however, there was no generally agreed upon model to use until "expected utility of profits," in the von Neumann-Morgenstern sense, became the popular answer to such problems. In brief, the late 1950s and early 1960s were, in the theory of the firm as elsewhere, the "hundred flowers" time. It is natural to hypothesize that the new arguments were in part intended to meet the "anti-marginalist" and especially the FCP challenge. Such a view has been put forward by Lee (1984) who submits that the reformist theories were *needed* to complete the absorption of the price-setting mechanism within an enlarged body of received doctrine.

I hope the above historical sketch is *both* ambiguous and informative enough to suggest that conflicting views of theoretical change and the role of critical discussions in economics can temporarily feel at ease with the FCP debate. Three such views, all connected with the tradition of intellectual, rather than social history, were listed at the end of the last section. As it happens, the material is intricate enough to relate ambiguously to these broad points of view even when they are taken in isolation. Most visibly, the falsificationist apparatus can be made to apply to the facts of the controversy either descriptively or normatively; it would be premature at this stage to claim that the participants ignored the Popperian dictates altogether. Hence the methodological appraisal that this article aims at building in progressive fashion is by no means a trivial task.

3. Hall and Hitch's Contribution against the Background of the 1930s.

Hall and Hitch's 1939 article is a declaration of war against the "current doctrine on price and output policy" (1939, 108), which they identify with Joan Robinson's and Chamberlin's restatements of the theory of the firm. The authors seemed to have indulged the latter more than the former, an attitude general among full-costers, which will be connected with Chamberlin's own attempt at a much later stage to annex FCP into his own system (1952).[8] The distinction between Robinson

8. Chamberlin's general attitude towards FCP was soothing if somewhat patronizing. In 1952 as well as in the late editions of this treatise, he claimed to have anticipated Hall and Hitch and included FCP as "one phase" in his "monopolistic competition" theory. Quite consistently, if not plausibly, Chamberlin described Joan Robinson's "imperfect competition" model as the *only* polemical target of Hall and Hitch, thus involving FCP in the quarrel

and Chamberlin, however, is a subordinate one in Hall and Hitch's paper and probably in the FCP debate as a whole. More importantly the authors in effect combined *two* distinctive lines of criticism against the "current doctrine," one aimed at the profit-maximization hypothesis itself, the other directed towards the formal analysis of demand in the "imperfect" and "monopolistic" competition theories (and by the same token, the implied classification of market positions). This combination of criticisms will have far-reaching consequences for the structuring and resolution of the debate, and some background comments may help to put it in proper perspective.

It is not usually realized that the profit-maximization hypothesis had to await the theories of "imperfect" and "monopolistic" competition in order to be recognized as a core part of the neoclassical approach to the firm. Joan Robinson calls it the "fundamental assumption" (1933, 15) and stresses that it should apply to any market position. Chamberlin is by no means as emphatic as Joan Robinson and he appeared to vacillate on this issue belatedly in his 1952 discussion of FCP. But it is hard to make the case that his *Theory of Monopolistic Competition* is really different from the rival *Economics of Imperfect Competition* in terms of the centrality of profit maximization. All of the Chamberlinian models in 1933 do mention "maximum profit" at some place or another. By contrast, Marshall was vague and hesitant on the logical role of profit maximization. Many readers of his *Principles* had concluded that it was not part of the competitive model. Since monopoly had by and large been a secondary complication in Marshallian economics and he was so influential among English-speaking economists, it should come as no surprise that those economists upgraded profit maximization to the status of "fundamental assumption" roughly at the same time they discovered that imperfect competition was the relevant case. Of course, a history of the theory of the firm dominated by Cournotian, rather than Marshallian economics, would have looked very different. Be that as it may, the Marshallian inheritance was relevant to Hall and Hitch, whose two strands of criticism exactly reflect the dual nature of the imperfect competition revolution. This leads to a related fact that needs emphasis.

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of the "two Cambridges." This maneuver infuriated Robinson. She commented on Hall and Hitch as well as FCP on various occasions. Her comments were always very critical, even at a time when she was distancing herself from her own earlier work (e.g., 1953, 230–31).

The theory at the center of the FCP and marginalist controversies was by no means a long-established one, despite the rhetorical insistence to the contrary by some marginalists. When Hall and Hitch wrote, the following important theoretical issues remained to be settled (see for instance Hicks's 1935 survey): whether the long- or short-term interpretation of Chamberlin's and J. Robinson's basic model was the more suitable; the permissibility (or otherwise) of using individual demand curves when oligopolistic interaction was allowed; and most interestingly, the correct understanding of profit maximization. To elaborate on the last point only, I should first get rid of another secondary distinction, that between "marginalism" and "profit maximization." Of course, profit maximization did not appear as such most of the time, the common formula being Joan Robinson's "marginal revenue (MR) = marginal cost (MC)," generally expanded as "firms attempt to equate MR and MC," "firms expand production to the point where MR = MC," and so on. Most of the participants knew very well that the Robinsonian slogan was merely a convenient way of expressing the first-order condition of the profit-maximization program. They knew that higher-order conditions had to be checked and that corner solutions could occur. Though sometimes disputed in the *American Economic Review* exchanges, these details have on the whole played little historical role. When writing that the understanding of profit maximization was still an open problem in 1939, I therefore refer to a semantic issue: which (if any) of the ingredients of the formalism can be regarded as reflecting the businessman's own decision process? Does he assess demand and cost schedules; does he compute maximums? Clearly, Joan Robinson was as uncertain of the semantics of her models as she was confident of their syntax. She described the "fundamental assumption" as a "technique" and the whole of her theory as a "box of tools" (1933, 15). In the same instrumentalist vein, she repeatedly stressed the "artificial" air of her analysis, but in one passage she appeared to believe that "certain firms actually calculate" their demand curve (56). It is important to realize that the FCP and marginalist controversies were *part* of the process by which these interpretive vacillations on fundamentals came to be addressed. They had not been resolved *beforehand*. We are now in a better position to examine Hall and Hitch's article.

Hall and Hitch had two polemical targets in mind. They reported evidence on price setting gathered by questionnaires from thirty-eight

firms. The answers were discussed at some length with the respondents. A high proportion of firms (30 of 38) were found to adhere ("rigidly," "normally" or "in principle") to FCP. Typically, firms would take an *ex ante* estimate of average direct cost and add to it two percentage margins, one to cover overhead and the other gross profits. The "full-cost policy," which Hall and Hitch also referred to as the full-cost "principle," is in their words a "rule of thumb" which could result in maximum profits as an "accidental by-product only" (1939, 113). This attack against profit maximization was substantiated by the discovery that firms do not have at their disposal the data which would be necessary for equating MR and MC. Businessmen do estimate a cost curve—but it is an average one, and an incomplete one at that. They are vague about elasticities, which led the authors to conclude, "producers cannot know their demand or marginal revenue curves" (114). Now, despite this heterodox bias which infuriated many marginalist readers,[9] Hall and Hitch attempted to relate FCP to the so-called "kinked demand curve" (KDC), which was to be understood virtually everywhere as a component part of a profit-maximizing explanation or price rigidity (see Stigler's [1947] influential interpretation), and which they *themselves* used for the same purpose in this completely orthodox way (117). Hence there is a major logical inconsistency in Hall and Hitch's article.

This inconsistency is better understood if we realize that use of the KDC actually belongs to the second polemical line, that which aims at the modeling of demand and the classification of market situations in the current doctrine. Hall and Hitch stressed that "most businesses take into account in their pricing the probable reactions of competitors," whether actual or potential (125). Neglect of oligopolistic interaction was then a recurrent, and fair, criticism levelled against Joan Robinson and Chamberlin. Robinson had really assumed away any sort of interaction by offering an extraordinarily comprehensive definition of the individual demand curve (1933, 21). Chamberlin had reserved true strategic interaction for the "small group" (1933, V:6), while developing the nonstrategic interactive analysis which is typical of monopolistic competition in the "large group" case. Chamberlin's discussion of the "small group" was notoriously unsatisfactory, but his

9. The widespread view of the time was Joan Robinson's: producers do not know their demand curve initially, but they come to know it *by trial and error* if the market conditions remain unchanged for long enough (see 1933, vi).

very distinction between small and large, which is hazy, made it possible for him to claim that he could accommodate at least some degree of oligopoly within the basic model of monopolistic competition. Hall and Hitch appear to criticize just this when they point out that "the typical case is that of monopolistic competition with an admixture, *which is usually large,* of oligopoly" (1935, 122, my emphasis). Their KDC is an attempt, however crude, to face what they call the typical case. Note carefully that this line of analysis, which is both polemical and constructive, did not have to rely on the simultaneous rejection of profit maximization. Quite the contrary; in the relative theoretical vacuum where oligopoly analysis developed the marginalist apparatus was very helpful.

The use of the KDC in Hall and Hitch raises further significant difficulties in the theory sketched in their article. There is a customary, if perhaps not very convincing, attempt to relate price rigidity and the KDC. Even taking this relationship for granted, Hall and Hitch would have had to complete a logical triangle by also connecting, on the one hand, FCP with the KDC, and, on the other, FCP with price rigidity. As far as linking FCP with the KDC, they simply took it for granted, thus laying themselves open to later criticism both from full-costers and from marginalists. Kahn testifies to the discontent of both camps at the same time: "as Mr. Andrews complains, 'the reason why the "kink" should occur at the full-cost . . . level of price was not adduced. If anything the determination of prices appeared even less rational than before' " (1952, 122). As far as linking FCP with price rigidity, Hall and Hitch made useful, but unsystematic, suggestions. They seem to have regarded full-cost prices as flexible relative to changes in costs when those changes *equally* affect all the firms in the branch, as in a wage or tax rise. What happens in the case of non-uniform cost changes is unclear. Their treatment of demand changes is even more cavalier. The rather indefinite suggestion that full-cost prices do not respond to "moderate or temporary shifts in demand" (1939, 125) leaves open the possibility that those prices roughly follow the ups and downs of the business cycle.

The preceding details were provided because the ensuing debate on FCP will largely focus on the twin issues of price inflexibility and the role of demand in the price-setting mechanism. Hall and Hitch's lack of clarity makes it the more remarkable that the FCP "principle" or "doctrine" soon came to be regarded as a tentative explanation of ac-

tual sluggishness of prices after the Great Crash. Prominent among the economists of the time who had evidenced and attempted to explain the phenomenon was a New Deal official, Gardiner Means. He had produced data to the effect that, first, post-1933 output had fallen most where prices had been the most sluggish, and second, low output and sluggish prices were likely to be found in sectors with a high degree of "concentration" (1935a and 1935b). According to Means's own theoretical hypothesis, there were two sorts of prices, "market prices" and "administered prices"; only the former reacted in the way which traditional economics would have led one to expect; as a matter of fact, the growing trend towards "concentration" had inevitably resulted in the prevalence of the latter. Means's views make an interesting comparison with the later FCP "doctrine." Of course, the emphasis here is on aggregate price and quantity series as well as on economic policy generally speaking. But Means's doctrine began as the realization that firms set prices rather than react to them, and hence that the internal workings of the firm are *prima facie* relevant to the economist. It connects with some informal microeconomics, and Galbraith's later theory of the "large corporation," and the already mentioned "managerial" views of the 1960s. Means's own triangle of concepts—administered prices, concentration, price rigidity—bears some familiarity to Hall and Hitch's—full-cost pricing, kinked demand curve, price stability. Like the KDC in Hall and Hitch's case, "concentration" was the place where traditional analysis could reenter the stage. Lerner's (1934) "degree of monopoly," which turns out to be relevant to the FCP debate as well, is just the Robinsonian, profit-maximizing counterpart of Means's concept, and one that is not at all incompatible with his own suggestions. There were extensive studies of the link between concentration and price rigidity using this as well as other tentative measures.[10] Such background might have influenced the reception of Hall and Hitch's paper, especially among official circles, and suggested that a rival explanation of post-1933 price behavior was to be read in it. It is interesting to note that the imperfect competition theories had little to contribute per se here; whatever they had to say was channeled through Means's or Hall and Hitch's hypotheses.

10. Lerner's definition was also used by Kalecki in the context of an explanation of the stability of distributive shares in the national income. Kalecki's discussion occasionally touched upon the issue of FCP (see 1943). The two themes coexist in the later post-Keynesian literature.

Though touched on in the comparison just made, the following aspect of the intellectual context deserves special emphasis: "Price Theory and Business Behaviour" was but one among many attempts made at the time to confront theoretical hypotheses with data. The 1930s were clearly the time when modern self-understanding of economics as an empirical science took shape. The point, of course, is not that economists suddenly discovered the role of factual evidence, but rather that they began to *diversify* their understanding of it; besides the traditional play with stylized facts and the use of examples, which still prevail in J. Robinson's and Chamberlin's treatises, more systematic forms of empiricism emerged, some of which come arguably close to the philosopher's notion of "test." The work of the Oxford Economists' Research Group as a whole was typical of the new attitude, which would become clear from an examination of one companion study to Hall and Hitch's—the still famous survey of the firm's reactions to interest rates.[11] Here and there, an attempt was made beforehand to identify and state the target hypotheses; then evidence was adduced. As it turns out, that evidence usually had to be sought out rather than found wherever it was available. Questionnaires (which were normally followed by discussions of the answers between the subject and the interviewers) were a crucial aspect of the Oxford approach and were widely resorted to elsewhere, but they became a subordinate or dispensable feature in other studies which nonetheless displayed the new empiricism to a high degree (such as Neal's [1942] statistically based investigation of pricing).[12]

11. The survey on interest rates and its accompanying discussions were published along with "Price Theory and Business Behaviour" in the early issues of *Oxford Economic Papers;* both papers are taken up in Wilson and Andrews (1951). The discussion of interest rates was technically more sophisticated than that of pricing (it included some use of econometrics) and it involved the Oxford economists in even more detailed interpretive issues. Lee (1983) gives an account of the questionnaire procedures that were used by the group.

12. The inquiries of the 1930s and 1940s into the firm's cost curve provide another relevant example of the new way of thinking. Viner's 1931 article gave the Marshallian doctrine of the U-shaped short-run cost curve its final polish and bluntly claimed it to be beyond fundamental criticism. His piece is very much in the style of English-speaking apriorism. Ten years or so later a completely different methodological attitude prevailed. The shape of the curves was then widely regarded as an empirical problem. The common answer was that the short-run cost curve was horizontal on its terminal range (see the conference *Cost Behavior and Price Policy,* 1943). Importantly, all of the full-costers except for Wiles, as well as several of their respondents, took that factual conclusion for granted throughout the controversy.

4. Later Contributions to FCP

This section reviews the contributions made to FCP after Hall and Hitch, up to about 1955.[13] A convenient way of reorganizing the material is to ask whether or not, and possibly how, full-costers attempted to fill the various gaps left in "Price Theory and Business Behaviour." Since I will claim that they did not really improve on the theoretical perspective, most of the section will center on the added empirical evidence, which I will discuss in a stepwise fashion.

It is a striking fact that the basic discrepancy in Hall and Hitch—their simultaneous adherence to and rejection of profit maximization—was usually reproduced in the later writings. Harrod's implied model in his *Economic Essays* (1952) seems to be consistent with his earlier statement that the economist "ought rather to assume that producers (or a large proportion of producers) proceed upon the full-cost principle" than to continue to assume profit maximization (1939, 3). But Saxton (1942), Andrews (1949), Dean (1951), Oxenfeldt (1951), and Wiles (1956), although supporting the view that in many circumstances firms set prices in the way described by the FCP "principle," did not put the profit-maximization hypothesis in sharp contrast with it. To that extent, full-costers can be said to have themselves begun to accommodate the principle within marginalism. This sort of accommodation could have taken place as a result of lack of clarity and eclecticism, as in Hall and Hitch, but it was sometimes pursued deliberately.[14] Turning now to the other loose end in Hall and Hitch's modeling, the full-costers did not decisively improve on the theoretical understanding of the relationship between FCP and price rigidity. No precise doctrine became available beyond the presumption that FCP *favored* price

13. I have limited my investigation to Heflebower's list of books supplemented with Wiles (1956), and to those studies of FCP which appeared in the major American and British journals.

14. The most remarkable example of a marginalist full-coster is Saxton, who deserves to be quoted at length: "In general it cannot be doubted that entrepreneurs do make attempts to maximize their profits by every method which is open to them, though superficially their actual conduct in any given situation may appear unlikely to achieve that aim. It may be that there are many entrepreneurs who do not calculate explicitly, and thereafter equate marginal cost and marginal revenue. But when their actual conduct is analyzed in detail it may be found that they have sought and achieved the same result implicitly" (1942, 5). This passage could have been excerpted from Machlup or any late marginalist writer, with its rhetorical use of the distinction between "implicit" and "explicit," and the exploited ambiguity of the statement that business people "attempt" to maximize profits.

rigidity. The piecemeal arguments put forward here and there to predict when prices would or would not react to changes in the economic environment are hardly viable. The lack of a proper investigation was mainly due to the fact that the definitional boundaries between FCP and price rigidity had become blurred; that is, full-costers were just reiterating Hall and Hitch's initial equivocation in some form or another. To illustrate, consider the following from Wiles, "The full cost [firm] . . . is very ignorant of demand. . . . In this ignorance as to demand the entrepreneur will often hold to any price that experience shows to be profitable" (1956), 47–48). Supposing that ignorance of demand should result in sticking to a price, why should that price have been a full-cost price in the first instance? The objection resembles that leveled by Kahn against the use of the KDC as an alleged justification of FCP.

Perhaps we shall be more satisfied with the full-costers' contribution after examining the more empirical parts of it. On this score too the Oxford study had been far from definitive. First of all, granting that FCP behavior was widespread, but not universal, a fact which emerged from the initial questionnaire, the question arose of which economic characteristics would correlate well with such behavior. One might have expected Hall and Hitch's article to be followed by definite attempts at correlating the frequency of FCP behavior with the competitive type. This was not done. There were not even rough statistical attempts such as those that had been pursued by Neal (1942) in connection with the administered-price-concentration hypothesis. True, most of the full-costers noted that FCP occurred in oligopolistic contexts, but they did so in a piecemeal and often anecdotal way. Hence, Heflebower was quite correct to claim that their work "short-cuts a deep understanding of the particular market" (1955, 375). His own careful survey is relatively successful in reorganizing the scattered evidence. He concluded with the restrictive assessment that FCP is only acknowledged beyond doubt in the following specific context: 1) oligopoly with a leading firm whose cost figures are borrowed by competitors; 2) oligopoly with explicit agreement to use conventional cost figures; and much less clearly; 3) oligopoly with implicit agreement such as was involved in cases taken to court.[15]

15. Besides the type of market, various factors were found to influence the frequency of FCP behavior. It is favored by a firm with numerous and repetitive price decisions, or when

The second issue to be addressed is that of price and cost computations; it led to numerous developments. It turned out that Hall and Hitch's formula was only one among several. Firms may also set their price by reference to a more encompassing average-cost concept, one which comes close to the total average cost of standard theory. Interestingly enough, it may also happen that the relevant cost concept is even narrower than Hall and Hitch's direct (i.e. variable) average cost. There are firms relying on unit labor cost only; others (mainly in the processing business and in retail trade) take into account unit cost relative to a single commodity. On the other hand, sometimes there are more than two added margins, each normally expressed as a percentage so that financial costs may be treated separately from either overhead (i.e. depreciation) or profit. The details are well covered by Heflebower (1955). They are less interesting than the principles to which they roughly conform: first of all, decision makers drastically simplify their information. Whatever cost concept they use, it is always incomplete, and percentage margins enter the computation to substitute for missing pieces of information. As a result, all costs are taken to move at the same pace as the basic cost reference, an assumption which can lead to seemingly puzzling results, given that average *fixed* cost has to be a rectangular hyperbola whatever the shape of the average *variable* cost function. Most conspicuously, and in contradistinction to the standard theories, demand does not explicitly enter the price mechanism, though there is a reasonable suspicion that it should influence the margin rates and/or the difference between actual transaction prices and price quotes on a FCP basis. Second, and perhaps more arguably, profit has no exact accounting counterpart. However refined the various computations, profit appears to be lumped in with

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there are many goods among which the common costs are to be allotted. In the 1950s *size* was shown to play some role as well. Relevant though ambiguous evidence in this respect was provided by Earley's 1956 study of 110 "fairly large companies," of the multiproduct and multiprocess type, which he selected from among those manufacturing concerns classified as "excellently managed" by the American Institute of Management. One finding was that accounting had undergone significant changes in recent years, drifting from gross rule of thumb towards some form of "marginal accounting." Connected with this trend was an alleged fall into disuse of full-cost computations. Earley's work is careful enough, but the representativeness of his sample cannot be rated very high because of his avowed bias towards normativity. Lanzilotti (1958) countered Earley's view by pointing to the use of nonmarginalist procedures (including FCP ones) by large companies. Thus the initially suggested empirical linkage did not resist scrutiny.

some indirect costs at least. This feature of pricing formulas relates to the more general fact that the FCP mechanism relies on data provided by the accountant.

This fact helps explain the *prima facie* surprising way in which the full-costers' discussions of price formulas dealt with reference volumes, a most delicate issue that Hall and Hitch had hardly addressed at all. The more empirically minded among them insisted that the formulas relied on costs calculated at some assumed "normal" or "standard" volume. This accounting terminology had to be translated into economists' language if proper use were to be made of FCP evidence. "Normal" or "standard" output could refer to last year's figures, some average of the previous years, an expectation of future sales or a wholly conventional variable. Each of these possibilities seems to have been proposed at some stage by the full-costers. A fact which they often emphasized is that the firm's own costs are *not* always the relevant ones. Computations may refer to the leading firm, if there is one, or to some conventionally agreed-upon average situation, which may be publicized throughout the industry. This does reinforce, of course, the connection between FCP and special oligopolistic settings. Interesting as these findings are, they are not amenable to any general conclusion beyond the fact that, for one, there is some significant historical and conventional component in the way in which firms base their cost figures on output, and, for another, functional reasoning is usually foreign to the businessman, who instead tends to rely on predetermined numerical values. The vagueness of the conclusion can be partially excused by the sheer difficulty of the subject matter; at least the full-costers can be credited for having pointed out the severe discrepancy between the practitioner's understanding of cost and the economist's, a problem which had not at all been addressed by theories of imperfect competition. But there was another, possibly more important reason why the full-costers eventually left the issue of the reference volume in an indefinite stage; most of them—Andrews (1949) is most typical here and Wiles (1956) is the exception—shared in the widespread belief of the time in constant average variable costs (in the short run). It was cavalier to dispense with further research on "normal" or "standard" volume only on the basis of this added fact, but this is, roughly speaking, how things went.

The problem of the reference volume is logically distinct from the third question on our agenda: what is the resulting actual output once

prices have been set in accordance with one of the FCP formulas? Hall and Hitch appear to have assumed that actual output is determined on the demand curve at the price level preset by the firm. This assumption is clearly made by Andrews (1949), Brunner (1952) and Harrod (1952), but was by no means made by all of Hall and Hitch's followers, some of whom could claim to have anticipated the non-market-clearing literature of the 1970s (queues are discussed in Wiles 1956). But *ex post* determination of output on the demand curve is by and large the prevailing way of closing the FCP model. This non-profit-maximizing equilibrium model is an interesting theoretical combination which could have led to many developments. Unfortunately, it is no more than a theoretical model; no attempt was ever made to test the market-clearing assumption. More generally, the full-costers collected little or no data at all on output, even of the sort their questionnaires would have made possible to gather. The resulting picture is an unsatisfactory one in which the workings of the price mechanism appear to have been overelaborated at the expense of the quantity side of the market.

Hall and Hitch had been unclear on the role of demand in the full-cost formulas; this is the fourth and last empirical issue on the agenda. It became clearer, although not so much by the full-costers' individual endeavors as by the partial confrontation with evidence which resulted from the FCP debate itself. At the *Business Concentration and Price Policy* conference, Heflebower (1955) particularly scrutinized the version of the FCP "doctrine" in which demand considerations are left out altogether from the pricing decision. This version implies that margins are unaffected by demand and that there are never any discounts on prices as defined in the formulas. The available evidence, Heflebower claimed, went against such a rigid version of FCP. The formulas turned out to be less significant than they had first seemed to be because they helped compute *quoted* prices which were used for accounting and planning, but which could well be different from actual transaction prices. Heflebower noted that transaction prices fell in the troughs of the business cycle, whereas quoted prices stayed relatively rigid. The question of margins is a more subtle one to address, since available data normally describe *ex post* margins, which contrary to transaction prices and *ex ante* margins, are not a command variable of the firm. That is, testing the hypothesis that demand does not influence the price decision by means of data relative to *ex post* realized margins

is surely possible, but it involves making a definite assumption about how output is determined and another about the shape of the cost curve. This problem does not seem to have been properly recognized at the time of the debate.[16] For instance, Heflebower pointed out that *ex post* margins are definitely variable both through time and cross-sectionally, even within the same firm, and jumped to the conclusion that demand should therefore be seen to influence the price-setting mechanism. Earley's (1956) discussion went one step further; he claimed to have found not only that price-cost ratios vary *ex post* but also that firms did not appear to aim at stabilizing them *ex ante*. However questionable the representativeness of Earley's sample, it is remarkable that the two most interesting pieces of evidence that we have found on margins were produced by empirical researchers of marginalist persuasion at a later stage of the debate. The full-costers had been as vague on this issue as they had been elsewhere. Saxton had gone so far as to define his doctrine almost limitlessly: profit margins "may vary from zero upwards to meet changing circumstance" (1942, 127). No doubt, the full-costers could have answered Heflebower by claiming that his evidence was not novel to them and that they had already accommodated it in their own flexible interpretation of the price mechanism. This insight, however, had been left unelaborated, and it coexisted unhappily with the recognition of widespread price rigidities in the full-costers' writings.

5. Setting the Stage for the Marginalist Response to FCP

Two interpretations of FCP help set the stage for the marginalist response. Hall and Hitch believed that the standard doctrine did not capture the way in which businessmen make decisions ("they are thinking in altogether different terms" [1939, 113]), but that it was not entirely useless, since maximum profits may accidentally result from the application of the businessman's rule of thumb. At the other end of the spectrum so to speak, Saxton's final assessment was that the businessman's deliberations are described by the assumption that he tries to maximize his profits (1942, 5). This contrast of accounts suggests the following question: 1) Is FCP compatible with profit maximization

16. An exception to this statement is Cheek 1949. The problem is recognized and carefully addressed in the late empirical work on FCP (Coutts, Godley, and Nordhaus 1978).

when both mechanisms are understood as rules of conduct? Of course, Hall and Hitch would respond in the negative and Saxton in the affirmative. A similar question can be raised with respect to other rules of thumb such as those discussed by Machlup (1946), Gordon (1948), and Wiles (1956, ch. 11): 2) Is FCP compatible with profit maximization when both mechanisms are interpreted in terms of their effects? Hall and Hitch do not have a clear-cut answer here, while Saxton would again answer in the affirmative. It would be begging the question at this stage of the inquiry to restrict the "effects," i.e., the outcomes of the idealized decision process to "prices" and "quantities." The interpretive issues raised here are not those currently addressed by economists. It is not far-fetched to assume that if faced with the FCP debate anew, researchers of the 1990s would narrow their investigation to question 2 above, understood with the usual restriction to observable changes in price and quantity. How this narrowing of interest took place in the theory of the firm—an important methodological issue—can be illuminated by the account of the marginalist answers to FCP.

Most of the actual answers fall within the scope of questions 1 and 2, or under the roughly similar distinction of *ex ante* and *ex post* profit-maximizing accounts of FCP.[17] A significant feature of the controversy, it suggests in a limited sense at least that the participants understood each other well; most were aware that they were discussing profit maximization rather than any other facet of the theory of the firm. Such a concern with fundamentals can be explained by the background of the debate as sketched above. In demonstrating the organizing power of the profit-maximization hypothesis, the theories of imperfect competition had cleared the ground for the fierce FCP and marginalist controversies; and the issue of the shape of the variable cost curve was not a factor, since it was by then regarded as settled by many participants. Second, I connected questions 1 and 2 to the full-costers' *own* accounts. This is an indication of the general strategy to which the marginalists adhered. They tried to make the best of the full-costers' voluntary or involuntary concessions to the standard

17. An *ex ante* account of FCP relies on cost curves, and more generally on market data, as they are perceived by the businessman, whereas an *ex post* account typically takes realized data for granted and assesses the consequences. It does not coincide with the distinction between profit maximization as a rule of conduct and as a way of predicting observable effects. In actual practice, we shall use the two distinctions as the same filing system. The *ex ante* account of FCP is very much in the vein of "subjective" microeconomics that the Austrians had made popular and that Machlup was still advocating in 1946.

approach, a maneuver which seems promising in view of the numerous vacillations pointed out above. Third and most importantly, there were other discursive strategies available to the marginalists, such as dismissal of the questionnaire data, or rejection of rules of thumb on the grounds that they are irrational. The fact that these maneuvers were *not* used widely deserves special attention, and I elaborate on it in the remainder of this section, while leaving the core of the interpretive debate to sections 6 and 7.

Disputing the FCP data was a natural attitude to take for those economists who were skeptical of the value of questionnaires and pointed out that, when entrepreneurs were asked about pricing policy, they tended to rationalize it either in a normative way or by invoking rough-and-ready generalizations reflecting their own interpretations of the case. That there is a normative side to FCP is obvious: when asked about "gross" or "net" margins, respondents would typically say that they included a "fair" or "normal" return on incurred expenses. More specifically, there is an interesting connection between FCP and the scholastic concept of "just price," as Wiles (1956, 195) notes. Lack of reference to profit maximization either as a simultaneous or a competing objective may thus be explained by the moralistic stance that fair commercial practice does not entitle one to charge as high a price "as the market can take." This kind of argument was used in the controversy, with a view to casting doubts on the evidence (e.g., Machlup 1946, 540–41). It could not achieve very much, because pointing out the moral character of FCP is not a counterargument per se; it can be alleged that entrepreneurs do comply with an ethical code. The more interesting suggestion was occasionally made that questionnaires implicitly called for rationalizations on the entrepreneurs' part in the sense that they searched for *ad hoc* explanatory regularities. The abstract response here is that the observer must make some presumption of rationality if he is to make sense of the interview at all: supposing that the businessman in effect expressed his view of how prices are generally set, he may be trusted to mean that he himself conformed to the alleged general mechanism, or else he would have mentioned the anomaly. This line of reasoning might have responded to the problem raised by illusory generalizations but was not really pursued to the best of my knowledge.

The view at the time on questionnaires would have to be reconstructed from scattered remarks by Saxton, Earley, and other empirical

researchers, and deserves a separate study. A common view can be roughly summarized as follows: questionnaires could be trusted to an extent, provided that they are phrased in terms of the effects of decisions rather than the decision-making process itself, and provided that they are complemented by other sources of observation. Following this criterion, FCP can be said to have been well evidenced around 1950-55. By then, not only had several sets of questionnaire data been published, some of them (e.g., Saxton's) were reasonably in agreement with the indirect questioning technique, and some cross-checking had been done with studies of the books and past policies of particular firms (e.g., Edwards 1952). Machlup stood apart from the mainstream view; his long-lasting stand towards questionnaires was definitely hostile, and along with Friedman he did much in his methodological work to elaborate the dismissive attitude that has become part of the *Weltanschauung* of today's economics. In 1946 and 1947 he downplayed the relevance of both Hall and Hitch's and Lester's data by emphasizing the inconclusiveness resulting from "semantic pitfalls" and artificially eliciting "rationalization" type responses (539–42). Though he did not say as much, his arguments implied that the shortcomings of the antimarginalists' inquiries were largely inherent in the questionnaire technique itself.

Another stand which was taken by some of the more orthodox economists was to accept evidence on FCP at its face value and interpret the latter as the kind of irrationality one is commonly faced with in real life. Dean (1951, 450–51), as a typical example, described FCP behavior as both very widespread in American industry and "inadequate in most situations." Earley, as another example in point, besides documenting the change in cost accounting techniques, endorsed the rejection of FCP by a group of up-to-date accountants: "Rigid adherence to a full-cost plus approach may result in loss of business to competitors and failure to obtain most profitable utilization of a company's facilities" (Committee on Research of National Association of Cost Accountants, quoted in 1955, 238). Earley's factual discussion does not agree with Dean's, but they come close to each other at the normative level. Neither Dean nor Earley would attempt to conflate the FCP principle with marginalist economics; rather, they highlighted the clash between irrational and rational methods. The primary objection that they leveled against FCP was its alleged neglect of demand and competitive pressure. They also made the usual complaints against the

use of historical rather than replacement-cost valuations and the peculiar way in which fixed costs may be taken into account within the pricing decision. In the marginalist's eyes, the occurrence of a fixed cost element within the formula, however it was computed, pointed to a deep-seated confusion: since fixed costs are incurred once and for all, they should not be allowed to influence the pricing decision at all. Besides violating the principle that "bygones are bygones," the computation of prices from average cost figures inclusive of fixed costs led, or so it was claimed (e.g., Kahn 1952), to the "absurd" result of countercyclical pricing behavior.[18]

Dismissing FCP on the grounds that it is irrational is hardly a sensible attitude to take for those who regard economics as an empirical discipline, and the writers of the "imperfect competition revolution," Chamberlin, Joan and Austin Robinson, Kahn, and Machlup, clearly believed that they were engaged in such a pursuit, not to mention early specialists of industrial economics, such as Heflebower, who were even more empirically oriented. Hence, the "irrationality" or "absurdity" of FCP at most signaled to the marginalists that the questionnaire data were dubious. But as just noted, dismissal on this ground was not possible. Thus, the marginalists ended up squaring with the theoretical and interpretive issues of full-cost pricing.

6. The Marginalist Response to FCP: Absorption of Rules of Thumb

In this section, I examine the decision-theoretic, or *ex ante*, reduction of the FCP anomaly. The marginalists relied on three basic lines of reasoning, one of which amounted to redefining profit maximization in the long-run sense, whereas the other two reconciled FCP with the standard approach after finding some give either on the side of the profit concept or on the side of maximization.

Even independently of the full-cost or marginalist controversies, it became a standard point by 1950 that prewar marginalism had over-

18. Assuming variable costs to be constant and percentage margins to be relatively inflexible, average fixed costs should prop up prices at the peaks and depress them at the troughs of the business cycle! Although Oxenfeldt (1951) claimed to have found passing evidence of such countercyclical behavior among firms, full-costers were not prepared to make it part of their "principle." The straw-man nonetheless was influential among some quarters, and Andrews (1949) and Brunner (1952) were at pains to distance "normal costs" from "full cost" in part to avoid this charge of "absurdity."

emphasized the short-term theory of price determination. This is a paradox, for both Chamberlin and Joan Robinson had made it clear that they were after a theory of long-term equilibrium. The essence of the "tangency solution" is to displace Marshall's competitive model. While the former implies production under declining cost, it shares the long-term character of the latter model: entry in, and exit from, the branch do not occur anymore and the number of firms is determined. Austin Robinson's interpretation of the theory of imperfect competition similarly complies with the Marshallian emphasis.[19] Still, Blaug is quite correct to stress that the longstanding effect of the imperfect-competition revolution was to change the time horizon of standard analysis: "price theory ever since has been a theory of the firm in the short-run" (1978, 412). Very roughly speaking, and even before the disparaging critique of Chamberlin by Chicago economists, many authors seemed to have learned only the following simple-minded lesson from the developments of the 1930s: standard monopoly analysis provides a model of short-run price and output determination for the single firm. This picture of marginalism underlies much of the postwar attack against the doctrine, as well as several responses from the orthodox side (e.g., the Lester-Machlup exchange in 1946 and 1947). To a degree marginalists had to rediscover the role of long-run analysis in order to accommodate FCP.

Hall and Hitch (1939, 118) opened the way by referring to "goodwill," (i.e., preservation of long-term relationships with customers) as a basic reason for sticking to the FCP price. This point was repeatedly made afterwards by Andrews and his followers, as well as by the marginalists. It involved the usual conflation of FCP with a price-rigidity doctrine; that is, it left unexplained the initial choice of a cost plus rather than any other value. The same objection applied to another long-term rationalization which was first sketched by Machlup (1946) and also became popular. It relied on the special connection between FCP and collusive oligopoly: "If a business man believes that the best policy for him in the long run is to stick to the cartel, this does not mean that he disregards the marginal principle. On the contrary, the feared consequences of breaking away from the cartel, its probable

19. The model that he had in mind (1950) implied simultaneous realization of long-term and short-term maximum profits. Farrell (1951) criticized the logic of the analysis, a rebuttal that should, in my view, have reflected on the theories of imperfect competition more generally.

effects upon long-run demand and revenue, dictate his combined adherence" (1946, 543). The game-theoretic reasoning which is implicit in this passage again leaves unexplained the choice of the initial cartel price. As far as FCP proper is concerned, the one and only long-term tentative rationalization may well be Machlup's insight of "average cost as a clue to demand elasticity" (1946, 543): in the absence of knowledge of prices set by competitors, whether actual or potential, a hint is given by the firm's own cost. The underlying argument is that setting a price higher than the firm's average total cost (inclusive of customary profit) would expose it to the risk of losing business (if a competitor has a lower cost curve); on the other hand, setting a price lower than average total cost would mean accepting negative profits and the firm might as well leave the branch right away. Machlup's insight implies three intermingled assumptions: a fixed technology, cautious behavior (which can easily be described in terms of maximin behavior, a connection pursued in Wiles's 1984 retrospective), and concern only with long-term profits. Other long-term arguments will be covered in the next section since they belong to, or lean towards, *ex post* reasoning. A majority of participants in the controversy readily concluded that FCP was no problem to marginalism in the long run. (This was exactly Gordon's summing up of the case [1948, 278].)

By contrast, most of the participants, marginalist or not, felt that the short-run marginalist theory of the firm needed to give some ground, although they disagreed on the changes to be made. Going back to fundamentals, the FCP principle could be seen either as a denial of the primacy of the profit motive, or as a nonmaximizing approach. Hence two reform programs were attempted, if not implemented rigorously. The notion that profit was not, or not always, the right variable to maximize on the entrepreneur's part did play some role, although a limited one, in the marginalist controversy. The rather vague expression of "non-pecuniary considerations" was usually taken to mean benevolent behavior. Some might have assumed a connection between FCP and disinterested management, a point which is reminiscent of the discussion above of "fair price." Few arguments attracted critical attention.[20] A more elaborate departure from standard doctrine is Fellner's (1948) article, which contended that "expected profit"—in a special sense—

20. Machlup's (1946, 526) brief discussion of "non-pecuniary considerations" was rebutted by several commentators as weakening the testability of the standard theory and as being inconsistent with the general stance of his 1946 article.

was the relevant objective of the firm. The implied criterion of choice is some form of maximin; it differs from the expected monetary value of profits and the expected utility of profits in the senses that had just been formalized by von Neumann and Morgenstern (1944). It would be a serious mistake to believe that the "unification of the theory of the firm and consumer theory" around the von Neumann-Morgenstern concept of utility—a theme forcefully expounded by Papandreou in 1952 and popular henceforth among neoclassical quarters—played even the slightest role in the FCP debate. I have been surprised that the participants did not have much to offer in terms of redefining the profit objective.

The alternative strategy was to weaken the requirement of optimization while sticking to profit as the objective of the firm. This was a very popular line among the marginalists and the full-costers themselves. It appears in some form in works by Saxton, Dean, Oxenfeldt, A. and J. Robinson, Pearce, Heflebower, Coase, Earley, and Haynes—that is, virtually everywhere. The crucial point is that it was normally pursued unknowingly. From the viewpoint of contemporary decision theory, especially after Herbert Simon's work on bounded rationality, these writers departed from accepted doctrine in a crucial respect. In effect, they replaced optimization with weaker rationality assumptions such as "satisficing," trial and error, and optimization in a simplified framework; or more accurately, they were replacing optimization with looser formulations that are compatible with, and even suggestive of, "satisficing," trial and error, and simplified optimization. As a result of a perennial conflation of rationality with optimization, they were unconscious of their heterodoxy and simply believed that they were rephrasing, rather than diluting, the accepted doctrine.

Two examples follow of this significant phenomenon of the FCP debate (as well as of the marginalist controversy). At the 1952 conference, Heflebower had in effect dismissed the crudest doctrines of FCP and suggested that a sophisticated, variable-margin version was compatible with marginalism. Coase, one of Heflebower's commentators, aptly summarized the paper: "many of the arguments used by supporters of the full-cost principle are no way inconsistent with orthodox economic theory" (1955, 393). Coase added the comment that FCP could be regarded as a "descriptive" restatement of a marginalist procedure on the grounds that variable, demand-influenced margins reflected "what the businessman thinks he can get" (1955, 394). The

faulty logic in this argument lies in inferring adequacy to the standard, optimizing theory of the firm from the bare fact that the businessman reacts to his demand expectations in some appropriate way. The businessman may after all be a "satisficer" of a sort with respect to "what he thinks he can get." Not surprisingly, Simon (1959) came to interpret FCP behavior in this way. On the face of it, his interpretation is no worse than Coase's.

Roughly speaking, A. Robinson's (1950) lengthy review of Andrews had the same authoritative influence on the understanding of FCP among British economists as Heflebower's 1952 discussion had among American economists. In typically marginalist rhetoric, A. Robinson set out to absorb some of Andrews's views after refuting his crudest pronouncements. Referring to a passage of *Manufacturing Business* where entrepreneurs are seen as rationally deciding against undersellling their competitors and expanding output, A. Robinson concluded, "I find it hard to distinguish this balancing of the advantages and disadvantages of price cutting and of expansion from the balancing process which the theories of imperfect competition have assumed" (1950, 778). The error here, is to jump from evidence of incremental reasoning to the presumption of marginalist reasoning in the technical sense. Not all incremental reasoning is optimizing: the businessman might be content to compare net incremental values with some threshold value, a case of "satisficing," or limit his computations to a subset of feasible factors, a case of simplified optimization. Andrews's discussion of the businessman had not excluded such nonstandard interpretations. Indeed, there is occasional evidence of an anti-optimizing stand both in *Manufacturing Business* and his later work.

Many quotations in the same vein exist. Some illustrate the related confusion of marginalist reasoning with a rational trial-and-error process such as decreasing margins *ex post* in the face of declining demand (an altogether different procedure from taking *ex ante* account of demand elasticities as assumed in the marginalist theory of the firm). Like the reasoning above, this conflation antedates the FCP and marginalist controversies (it appears to be deep-seated in the received semantics of the profit-maximization hypothesis among prewar economists). The view that agents make decisions after appropriately balancing advantages and disadvantages, conjoined with the (false) belief that this is all that marginalism says, I call *benign marginalism.* Coase's and A. Robinson's examples serve as evidence that this

doctrine was instrumental in bringing about the absorption of FCP. Machlup (1946) put it to use as well to accommodate other heterodox assumptions on the firm's behavior.

Given the foregoing, a question posed at the beginning of this article can already receive a reasonably clear-cut answer: there is *no* significant historical connection to be found between the "reformist" theories of the firm and the end of the FCP debate (a partial rebuttal of Lee's [1984] account). For one example, the sales-maximization hypothesis can logically be related to full-cost behavior (see Baumol 1959, 66–67). However, that hypothesis came after the FCP debate had been resolved. In the period under review, I have found no more anticipation of this hypothesis than of any other significant relaxation of the profit motive within the optimizing framework. A paradox of the orthodox response is that it shows definite anticipation of the "dissident" theories of the firm, as opposed to the "reformist" ones, though of course in a completely unconscious way.[21]

7. The Marginalist Response to FCP: Absorption of Price and Quantity Predictions

To many contemporary economists, a large part of the marginalist controversy would simply be meaningless. While they would readily acknowledge the fact that actual agents reach a decision by applying a rule of thumb, they would not be prepared to discuss rules of thumb except in terms of their effects on economic variables, probably only on prices and quantities. I have already mentioned that this doctrine—the sufficiency thesis, for short—was not generally agreed upon by 1940 or even by 1950. The genesis of the sufficiency thesis has much to do with two strands in the history of the theory of the firm: the debate on monopolistic competition, and the FCP and marginalist controversies themselves. Once such a crucial methodological doctrine is granted, further arguments become available to the effect that FCP and

21. For the sake of completeness I should mention two absorption strategies of an *ex ante* type that were adopted on occasion. The following hint was made during the marginalist controversy: firms do not optimize their decisions in each and every circumstance, but settle upon rules, which, given any particular circumstance, would more or less mechanically imply an approximation of the optimum. Also, various writers (e.g., A. Robinson 1950, 776) echoed the time-honored doctrine of economic natural selection: profit maximization need not be assumed because it results from the pressure of the market. However, this last hint could not lead very far in the essentially noncompetitive context of FCP.

marginal analysis are compatible. The absorption arguments turn out to be more definite than the decision-theoretic ones of the last section, but—a fact consistent with the historical point just made—they were not widely resorted to at the time.

There are four distinct facets to the sufficiency thesis itself.[22] First of all, it only makes sense against the background of some distinction between decision *outcomes* (among which are the effects to be studied) and decision processes. Such a distinction formed the basis of the contrast of two major interpretative questions in section 5.[23] Part of the sufficiency thesis is to say that emphasis should be placed on decision outcomes, however they are reached. This now seems so deeply ingrained in the structure of standard economics that one would be tempted to regard it as anonymous. Such is not the case, however. The section above emphasized that marginalist economists of the period 1930 to 1960 often understood profit maximization as a rule of conduct. They analyzed their "fundamental assumption" in a procedural way, however sketchily. The reformulation of profit maximization as a purely substantive hypothesis is ambiguously recorded in Machlup's early work (1939, 1946, 1952) and can be said to result from the joint methodological efforts of the late Machlup (1955, 1967) and Friedman (1953).

Second, among the decision outcomes, prices and quantities are expected to play a special, possibly exclusive, role. This side of the "sufficiency thesis" should be seen as a partial rejection of the theories of monopolistic competition, (i.e., of the Chamberlinian version as opposed to the Robinsonian one). Chamberlin's added variables, product quality and selling expenses, had been dealt with casually in his book and by his followers. It is well known; Stigler (1949) and Friedman (as early as 1941) contributed much to the debasement of the multivariate version. Indeed, the alleged sufficiency of price and quantity variables is best understood as the special contribution of Chicago economists. Although he was never as severe as Friedman or Stigler, and occasion-

22. The following analysis was strongly influenced by conversations with N. de Marchi.

23. Admittedly, this is not an easy distinction to draw in the empirical sphere: deliberative processes of real life may be antecedent to and separable from the resulting decisions (as in an auction bid on a tender), but they may also, and possibly more commonly, sequentially interfere with decisions (as in a step-by-step negotiation or a trial-and-error pricing process of the sort sometimes considered by full-cost pricers). The distinction, or so I claim, is not an empirical demarcation, but relates to distinctive ways of placing theoretical emphasis: some rationality models are "substantive," others are "procedural," to use Simon's (1976) most convenient taxonomy.

ally resorted to the Chamberlinian variables himself (in 1946 and 1952), Machlup did not as a matter of principle dispute the fact that prices and quantities were all the information that was needed by the economist (a significant example is 1946, 521, where he only mentions "output, prices, and employment"; see also 1955, 13).

Third, the sufficiency thesis is normally (but not always in the period under review) concerned with price and quantity changes, that is, comparative statics. It was Machlup who was most emphatic about this side of the picture. In his 1946 article, Machlup claimed: "Instead of giving a complete explanation of the 'determination' of output, prices, and employment by the firm, marginal analysis really intends to explain the effects which certain *changes* in conditions may have upon the actions of the firm" (521). This statement was made in the context of his attack against Lester and the full-cost pricers, but there is also a nonpolemical connection with an important strand in the history of economics, which Archibald (1961) called the Robbins-Samuelson program of qualitative comparative statics. In 1955, Machlup reiterated the notion that economics was a study of change in a noncontextual fashion, describing the typical explanation in the field as the linkage of observable statements on changes by means of various nonobservable assumptions. An important, though little noticed, consequence of that view was that the competitive type of the firm had to be seen as nonobservable in character. He took this drastic stance again in 1967: "the firm is only a theoretical link, a mental construct helping to explain how one gets from the cause to the effect" (9). The *terminus ad quem* of the sufficiency doctrine is Machlup's distinction between two concepts of the firm: on the one hand, the theoretical and so-called "heuristic" artifact, which he claimed is all that the marginalist theory normally needs; on the other hand, the descriptive, "realistic" concept, which is congenial to common sense and organization theory.

Fourth, the sufficiency thesis should carefully be distinguished from Friedman's irrealism of assumptions thesis, despite widespread conflation of the two in current methodological thinking. Recall that Friedman's celebrated 1953 article is primarily concerned with abstract methodology, at least in its explicit wording. Its most interesting claims relate to general philosophy of science and have quite correctly been interpreted as such; they revolve around such abstract notions as "realism," "as-if reasoning" and the "assumptions" versus "consequences" distinction. This is not to deny, of course, that Friedman had

economic applications in mind. He explicitly offered two such applications which are within the scope of this essay, a defence of the profit-maximization hypothesis against the antimarginalist attack launched in the *American Economic Review* (1953, 15–16), and a famous dismissal of Chamberlin's version of monopolistic competition (34–39). But the logical point remains that there is a gap between the "irrealism of assumptions" thesis and the view that the fundamental assumptions of economics should only be tested on a given set of empirical consequences (for instance, observable changes in price and quantity, as implied by a popular interpretation of Friedmanite methodology).[24] Now, if one moves from logic to the history of ideas, the fact emerges that conflation of irrealism with sufficiency of (possibly aggregate) predictions of price and quantity changes may have been the single most influential component of contemporary economic methodology. In the controversy under review here, it plays a limited role, as the following comments show. "Realism" had been an issue under discussion in the theory of the firm ever since Chamberlin and J. Robinson had published their treatises—a connection that Blaug (1978) is justified in stressing. Even the "assumptions" versus "consequences" distinction occasionally occurred in the pre-1953 writings on the theory of the firm. This undoubtedly means that in 1953 Friedman was using a language that was in part common with the researchers on the firm and echoing methodological concerns of their own. But there is *no* evidence that Friedmanite "irrealism" was instrumental in settling the FCP debate. The most significant fact to stress in this connection is that when Friedmanite terminology occurs in the discussions over full cost, it is used *across* the boundaries of the conflicting camps; a curious example is Andrews's methodological pronouncements (1952, 73) which come very close to Friedman's preview of his thesis (also published in 1952).[25]

24. Neither part of the doctrine implies the other: for one, the abstract distinction between "assumptions" and "consequences" may be filled in *any way;* for another, authoritative restriction of testable content, as involved in the sufficiency thesis or any variant of it, does not imply that the part of the theory claimed to be untestable should be regarded as "irrealistic" (it does not even imply that that untestable part should be described as a set of "assumptions" rather than of "consequences").

25. The *American Economic Review* controversies are more complicated, as the textual evidence given in Mongin (1986) shows. Not only are there occurrences of Friedman's themes and glimpses of his theses in those controversies, but they occasionally exhibit the pattern that is notably absent from the FCP debate: the dismissal of antimarginalists on the

It is time to proceed to the consequences of the sufficiency thesis, just sketched out, on the controversy itself. The marginalist theory of *long-term* competition predicts that prices will be equal to average cost inclusive of "normal profit"; this is commonly understood to follow from the free-entry–free-exit assumption, which is not made in short-term analysis, along with the sometimes tacit assumption that business opportunities are perfectly known and potential entrants are many. One could resort to such a traditional piece of reasoning in order to reconcile marginalism and full-cost doctrines in terms of their long-term price consequences. It is indeed part of the textbook assessment of FCP in the rare case that this mechanism is discussed at all (Koutsoyiannis 1975, ch. 11). To be used at all, the argument needs the sufficiency doctrine, if only because competitive models involve a price-*taking* agent whereas FCP explicitly relies on a price-*making* one: the former, a "theoretical" interpretation bearing no relationship to the latter. The absorption argument here is simple enough, but there is no clear evidence of it during the marginalist controversy. One of the full-cost pricers, it may be recalled, was inclined to think of pricing in long-run competitive terms: Andrews's construal resembles the textbook model of long-run competition, even more so when it is restated by his collaborator Brunner (see 1952 and Andrews and Brunner 1977, ch. 2). But it remains an open question whether or not they both lead to the same price and output predictions. Presumably, the Andrews price should lie above the marginalist price and the Andrews output below the marginalist output: *Manufacturing Business* allows for underutilization of capacities and use of a short-term cost schedule rather than the envelope long-term schedule of marginalism (1949, ch. 5). In other writings of the time the competitive absorption argument is often difficult to disentangle from other long-term rationalizations of the oligopolistic type as discussed above. Machlup's 1946 account is typical of this vacillation.[26]

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basis of the "irrealism of assumptions" doctrine, or some primitive form of it. The comments made here should be compared with Hirsch and de Marchi's (1990) pioneering study of the intellectual context surrounding Friedman's methodology.

26. When he does not dispute the data or attempt to absorb rules of thumb in an *ex ante* optimizing framework, Machlup's favorite account of FCP is of the oligopolistic type. However, he *also* had the competitive argument in mind, though he did not put it to use except very vaguely (565).

Granting the sufficiency thesis once again, the marginalist "box of tools" has a more effective absorption device to contribute than the competitive construals above. Following the well-known formula of monopoly analysis, price can be expressed as a function of marginal cost and price-elasticity of demand e: $P = MC(e/e - 1)$. Hence the following simple prediction for margin over marginalist cost (as a percentage), $(P - MC)/P = 1/e$. Given the usual assumption of constant marginal costs, e becomes commensurable with FCP data and it is enough to obtain (ordinary) elasticity data to implement a test. Thus, a quantitative solution of the FCP debate, in the *ex post* sense, should be forthcoming at this juncture. Depending on the data available, the test could be either of the static type, or of the comparative static type (the pure style of Machlupian economics), and clearly it could only be a partial *ex post* solution to the extent that the neoclassical absorption argument would be tested against the background of an admittedly disputable hypothesis on costs as well as of a special variant of the FCP mechanism (one involving average direct cost as the base cost figure in the formula). However relative the conclusions, obviously they would have been highly welcome in the FCP debate. Still, to the best of my knowledge, the literature of the 1940s and 1950s did not even make an attempt to implement the test.

This becomes very intriguing when it becomes obvious that the formula above was widely known and that it was occasionally used as an absorption argument, though not in the way that would be expected. That the formula should have been familiar to anyone involved in the controversy is clear from its repeated use in J. Robinson's *Economics of Imperfect Competition*.[27] Indeed, it was used among British participants; the first occurrence may be in Hall and Hitch's article itself. A. Robinson mentioned it, although cryptically, in his early review of "Price Theory and Business Behaviour" (1939, 541–42). None of these examples is very explicit. They testify to the following argumentative procedure, on the face of it, a surprising one: the formula in and of itself, and not its successful testing, is the alleged reason why FCP should collapse into the standard doctrine. That is, the participants ap-

27. She mentions it under the heading of monopoly equilibrium, soon after making the crucial pronouncement that "the individual producer may be referred to as a monopolist" (1933, 52); she then employs it as the basic tool of her comparative statics discussion. As mentioned above, the formula was used in the context of Lerner's (1934) "degree of monopoly" throughout the 1930s. In brief, it could not have escaped anybody's attention when the FCP debate burst.

peared to have been satisfied with the conclusion that their theory could also describe the price-setting mechanism in terms of a margin over average cost. To that extent, the "elasticity argument" as used in the 1940s and 1950s does not go any further than Machlup's typical remark that profit maximization is amenable to seemingly different, though logically equivalent reformulations, some of which *may* be relevant to FCP. Without even being a tough-minded falsificationist, the empirically oriented reader could have expected more—a quantitative check of the formula in order to discriminate between the marginalist redescription and the implied claim in the full-cost literature that margins over average cost are *not* functionally related to the price elasticity of demand.

I hasten to add that the actual testing would not have been a trivial task. There was a reasonably well-developed body of data on margins by the mid-1950s, mostly owing to Heflebower's and Earley's efforts (see section 4). There were, however, few available data on the elasticity of demand and probably none of the sort that would match the data on margins. For instance, Earley's figures relate to large companies from the manufacturing sector and could be compared only with data pertaining to demand by wholesalers. The problem of matching elasticity and margin data is still a vivid one as can be seen from a recent (and exceptional) Symposium on Full-Cost Pricing (1990–91). Accordingly, it would be unfair to take the marginalists to task for not having succeeding in testing the formula. Rather, the point is that they never felt that a test was needed. It is not unfair to say that they missed the logical structure of the discussion.

The most probable explanation of the peculiar use of the elasticity argument among the marginalists is that it would have been viewed as redundant with the usual consequence of benign marginalism, namely, the notion that full-cost pricers and marginalists are basically in agreement on the role of demand: margins, it was claimed almost everywhere, "take account of" the role of demand. Of course, such a conflation of arguments is not correct: the formula is precise and even quantified, whereas the view common of benign marginalism and the flexible version of FCP is vague; most importantly, it allows for interpretations in terms of demand shifts as well as elasticities. Later literature makes it possible to find supporting independent evidence for this explanation. In her 1975 textbook, Koutsoyiannis mentions the neoclassical price-elasticity relationship as an important step in the

marginalist absorption of FCP. She adds the following empirical observation as supposedly corroborative of marginalism: multiproduct firms set lower margins on commodities that have close substitutes than on commodities that do not (1975, 280). This is no doubt an interesting fact to report, but the purely qualitative evidence is so broad that it can be easily reconciled as well with non-optimizing accounts of pricing—for instance, one based on satisficing. Therefore to believe that the given evidence corroborates the marginalist formula is to miss the distinction between the elasticity formula and the non-optimizing accounts: once again, it means indulging in benign marginalism.[28]

8. Methodological Appraisal

A methodological assessment of the FCP debate can now be offered in terms of the basic accounts of theory change that were listed at the beginning of this article. The discussion of the falsificationist view should be pivotal here. For it is, in a sense, the easiest one to compare to the narrative; and if it fails, documenting its failure is already going some way towards alternative accounts. One pleasantly straightforward application of this doctrine to the sequence of events reviewed would be as follows. Suppose that a connection could be found between the FCP debate and the emergence of "dissident" or "reformist" theories of the firm in the 1960s; then, the falsificationist interpreter could hope to have stumbled upon the desirable pattern: conjecture (the theory of imperfect competition)—refutation (FCP evidence)—new conjecture (e.g., Baumol's sales-maximization hypothesis). The alleged connection, however, did not survive scrutiny. The single most

28. This discussion gained clarity after F. Lee commented on an earlier draft. Lee's own account (1984) is a markedly different one. In my view, it overemphasizes the role of the formula in bringing about the "demise" of FCP in the American debate and leaves unexplained the reasons why British participants could draw comfort only from the existence of the monopoly formula. Among the remaining absorption arguments used in the controversy, Wiles's (1950) conciliatory solution deserves special mention. It focuses on two empirical facts—prices are sticky and firms claim to operate under conditions of constant marginal costs. After showing how this evidence could be sharpened into a refutation of profit maximization, Wiles proposes an idiosyncrasic interpretation of the long-run/short-run distinction and ends up with the claim that marginalism is salvaged in his long-run sense. In our classification this is *ex post* reasoning at its best, in the restrictive style of the sufficiency thesis.

impressive fact about the FCP debate is that it was resolved to the satisfaction of the Lakatosian "elite"—Machlup, A. and J. Robinson, and Heflebower can be counted as such—by means of the traditional, prewar theories of imperfect competition. Now, falsificationism could also have served as a *descriptive* account of theory change if the case study had substantiated the following interpretation: the full-cost "principle" was a failed attempt at refutation; that attempt was severe; hence standard theory emerged by 1955 as corroborated. This alternative picture did not resist scrutiny either. I summarize the reasons why it turned out to be inadequate.

I forcibly reorganized the evidence gathered on FCP with a view to making it commensurable with the philosopher's notion of a test. The exercise cannot be said to have led to very convincing results: as it turned out, the evidence was incomplete and never used decisively. By its being incomplete, I mean that the full-costers were not particular about checking auxiliary hypotheses, except for costs, on which most of them took a definite stand and had independent evidence on which they could rely; they were imprecise about the type of competition and the time horizon that FCP firms faced. Besides, they were not really concerned with collecting data on the quantity side of the firm's policies or on the clearing of the market. A few gaps in the empirical argument were filled in by the marginalists themselves, but the picture which emerged towards the end of the controversy was still a very incomplete one. It did emphasize the connection of FCP with oligopoly and long-term behavior, but remained nearly silent on the determination of "normal" volumes and *ex post* realized output, as well as on the detailed working-out of auxiliary hypotheses. By the evidence never being used decisively, I mean that presumably the profit-maximization hypothesis needs some flexibility of interpretation if there is to be an empirical test of its validity at all, but not too much flexibility if the test is to be informative. The issue is, therefore, which of the many variants of the hypothesis are to count as acceptable candidates: one way of looking at it is to ask whether literal readings only or some nonliteral readings also qualify for the test. A literal reading of profit maximization is, roughly speaking, a logical equivalent of the initial hypothesis, (e.g., "$MC = MR$" and "$P = MC(e/e - 1)$" echo that "the firm maximizes its profits" in a literal way). Accordingly, a nonliteral reading of the hypothesis is one which cancels out something significant in

its logical implications (e.g., "the firm balances incremental costs and revenues in some appropriate way" is a logical weakening of "the firm maximizes its profits" and could only count as a nonliteral interpretation of the latter). Semantic clarity appears to be a prerequisite of empirical work in economics; counterexamples may hit one variant of the target hypothesis and not the other, so that empirical discussions may not result in any useful information in the absence of at least a rough definition of what a permissible variant is. Now, the full-costers were never really aware of the pitfalls surrounding the interpretation of profit maximization; while they were sometimes literalist, sometimes nonliteralist, they never explained their semantic criteria. As a result, they left a clear field to such extreme nonliteralism as exemplified by some of Machlup's long-term construals, his discussion of nonpecuniary motives and, above all, to benign marginalism of the Austin Robinson or Coase sort.

In sum, shortcomings in either the evidence itself or its use make it impossible to claim that profit maximization was refuted, let alone ever faced with severe attempts to refute it. Therefore it is not possible either to claim, except by way of equivocation, that the hypothesis was corroborated by the FCP debate. Should one now relinquish any hope of fitting falsificationism to the narrative? There is a final possibility to consider. The marginalists were, after all, not responsible for the full-costers' incompetence and could have been good scientists, in Popper's or Lakatos's demanding sense, even while being confronted with a feeble attack. So, the touchstone of descriptive falsificationism here should be the marginalists' reactions. They usually did not dispute evidential data; nor dismiss them on the grounds that marginalism was a normative theory. Both attitudes are *prima facie* evidence of a healthy empirical stand. Also noteworthy in this connection is the fact that the familiar methodological shortcut, that is, concentrating empirical content on the comparative statics of prices and quantities, did emerge at some point in the controversy but was by and large nonessential to its resolution (not to mention Friedman's irrealism doctrine, which played no active role in the FCP debate). Still, even after a somewhat vigorous reorganization of the arguments, the neoclassical response to FCP remains unsatisfactory from the point of view of empiricism (even more broadly than falsificationism). Its two basic weaknesses are implicit redefinition of profit maximizing and failure to cope with auxiliary hypotheses. The former relates to the above discussed issue of

benign marginalism. The latter is strikingly illustrated by the fact that the marginalist interpretation of FCP ranged over the whole spectrum of competitive types. There were several oligopolistic accounts, one which followed the lines of the monopoly or monopolistic competitive model (the elasticity argument), and even a purely competitive account. Even a single article (Machlup 1946) could range over the whole spectrum! The time horizon of the entrepreneur received similar treatment although to a lesser degree. There was widespread preference among the marginalists for the long-run account, but the reader was occasionally presented with short-run accounts of FCP (the elasticity argument is often interpreted in this way in later work). If conventional economists were to take advantage of the debate to improve on their own knowledge, they had to reduce this plethora of interpretations.

It should be clear at this juncture that if falsificationism should have a say, it can only be as a *normative and critical* account. Those methodologists who have blamed economists for lack of progress and low standards of scientific discussion could draw support from the present study as well as from the related discussion of the *American Economic Review* controversies in Mongin (1986). Enlarging upon these two case studies: economists enjoy the curious monopoly of having empirical controversies which, despite some scientific respectability, result in absorption without corroboration. This is an important distinction to make, though, unsurprisingly, one novel to the standard philosophy of science. Typical noncorroborative attempts at confirmation do not check all the initial conditions, or do not provide for independent testing of the more obvious auxiliary hypotheses, or do not distinguish the hypothesis subjected to the test from readily available rivals. Absorption of tentative counterinstances, as opposed to their being truly superseded, only requires that the gaps left in the non-severe test be notionally filled in the proper way. Absorption basically consists of pointing out either a candidate for the unspecified initial condition or a redefinition of auxiliary hypotheses that would take the edge off the counterexample. It is a game with logical possibilities but it does not carry significant empirical information with it.

Admittedly, there are two intellectual consequences of the FCP debate that, if consistently pursued, could have led to interesting results. On the one hand, the discussions had the general effect of shaking the

economist's confidence in the exhaustiveness of the market classification offered by the theories of imperfect competition. The fact that FCP was found to be connected with some forms of oligopoly is less important in this respect than the broad recognition of the pervasiveness of that competitive type in manufacturing business and trade. The FCP (though not the marginalist) controversy contributed to devaluing J. Robinson's question-begging analysis of the individual demand curve. There is an interesting connection between the work of full-costers and the more recent research on oligopoly—one which goes through Sylos-Labini, Edwards, and Modigliani. On the other hand, as one of the commentators in the *American Economic Review* debate, Oliver (1947, 38), was correct to note, the antimarginalist case should have upset the prevailing view of a businessman in a continuous state of "alert," ready to adjust to exogenous change. More technically, this means that either Chamberlin's added variables were relevant after all, since they could in many cases be made to bear the brunt of short-term adjustments in the face of rigid prices, or that the traditional economist's concentration on short-term consequences was off the mark. One picture emerging from the FCP debate was that of the firm as a long-term optimizer which makes infrequent decisions. Such a picture could be accommodated by marginalism provided that it placed the emphasis differently and sacrificed the more popular parts of its apparatus. This heuristic line was not pursued seriously after the controversy came to a halt. It is important to realize that the two directions of research just sketched, oligopoly and long-term optimization, relate to the auxiliary hypotheses of the theory and not its core assumption. To subject the latter to a true test would have required breaking away from, and not simply pursuing, the few promising hints dropped during the debate.

Is it a disappointing result to report another failure by economists to comply with a methodology which, in this particular instance, they did not really preach? At least we could claim to have understood better (though indirectly or negatively) the profession's actual mode of internal criticism. A simple but significant fact of the controversy is that the participants thought of their interchanges in terms of arguments before anything else, that is, before entering any distinction what-so-ever between "theory" and "fact." The marginalists blamed their opponents for this style of exposition: "Mr. Andrews confronts existing theories not with a solid body of opposing and irreconcilable facts but with a

ready-made alternative set of theories" (A. Robinson 1950, 771). However, they were no different themselves. We cannot make too much of Heflebower's and Earley's half-baked attempts to elaborate upon the FCP literature; neither resulted in a "solid body of facts." Rather, the marginalists conceived of the debate as an *ad hominem* one. The game was to read the full-costers' claims as if they were hostile to marginalism (which they were not always) and to find gaps in them. A point was scored any time their arguments, with all their crying lacunae, had been shown not to contradict a variant of the theory, preferably a thus-far unnoticed one. The procedure served to prove the alleged opponent's weakness, not the strength of the theory under attack. The last word was to exclaim "There is more to marginalism than its textbook versions" and leave things at that.

The rhetorical view of economics is the easiest of all to reconcile with the narrative above. The objection to it is the same here as elsewhere: it labors over the obvious. That the sciences in general have an argumentative component is a fact. So what? Does this statement help us understand what is special about theory change in economics? It is true that the rhetoric of economics is especially exuberant: the FCP and marginalist controversies may serve as evidence of this, which, however, is not very illuminating either. For the description in the last paragraph may well be both correct and incomplete. It does not preclude the possibility that full-costers and marginalists were groping after something other than personal success in the contest. The rhetorical view leaves open the question of how economists define their norms of intersubjective agreement. Also following another common line of criticism, argumentation analysis has little discriminating power when it comes to appraisal. There seems to be some indulgent or conservative bias embodied in the rhetorician's interest, in a way that some have found as infuriating as the falsificationist's armchair pronouncements.

In order to escape from a dilemma between "old" and "new" methodologies, which strikes me as an oversimplification, I have let another discussion of the FCP debate enter the stage. Sometimes it has been argued in philosophical circles that the social sciences are inherently not amenable to the usual discipline of testing, relying as they do on the time-honored rationality principle at every stage in the process of theory building and of confronting theories with data. That is, *explanans* hypotheses in the social sciences would consist of variants of the

rationality principle, which itself is needed to assess *explananda*, auxiliary hypotheses as well as initial conditions; this, it is said, is circular reasoning. This broad thesis has a *prima facie* plausible application to the FCP debate with profit maximization in the role of the rationality principle. Recall that profit maximization cannot be tested independently of our specifying the competitive type of the firm, its cost structure, and time horizon. The theories following from the imperfect competition revolution are confronted with an inescapable Duhem problem. Are the auxiliary conditions themselves amenable to an independent test? On one seemingly plausible reading, they are. To take the most obvious example, the competitive type can be identified with the number of firms in the branch—a proposal which was occasionally made by Stigler (e.g., 1968). But this line of reasoning will quickly be beset with difficulties: none of the various empirical characterizations offered for the competitive type has ever gained general acquiescence, and justly so, because what the theory has to say about competition does not justify selecting the number of firms, rather than the value of cross-elasticities, the size distribution within the branch, or the entry-exit conditions. Lack of a clear-cut theoretical taxonomy of competitive types makes it impossible to choose nonarbitrary empirical characteristics; hence the temptation to abandon altogether the search for independent characterizations of auxiliary hypotheses. This attitude is well exemplified by the proposal (also considered by Stigler) of identifying the degree of competition with a marginal cost over average cost ratio (i.e., of reverting to Lerner's 1934 definition). Such tentative characterization is an avowedly nonindependent one, since it amounts to inserting the profit-maximization hypothesis itself amongst the auxiliary assumptions. It is a proposal of despair. If it is adhered to as the least evil, one should conclude that the Duhem problem is ingrained within the basic structure of the theory of the firm. This would not mean that the latter has no empirical content, but rather that whatever empirical content it has is unknown to us to a large degree and should remain so indefinitely. There is no hope of learning much from experience when only grossly nonindependent characterizations of auxiliary hypotheses are available. If this highly pessimistic line of methodological interpretation were correct, a conjecture which needs cross-checking and may induce further case studies, there would be little to expect from the economist's conversion to falsificationist methodology.

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