The early full-cost debate 
and the problem of empirically testing 
profit maximization

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1. Introduction

Professor Langlois' article (1989c) is a significant step in the process of rigorously discussing the empirical relationship between full cost pricing (FCP) and profit maximization. By combining skillful reformulation and novel evidence, it gives some support to the widely held view that the former mechanism is nothing but an instance of the latter hypothesis in disguise. My assessment of the author's method and findings will be constrained by the fact that I am not an industrial economist. I stumbled upon FCP some time ago in the course of working around Friedman's "irrealism of assumptions" thesis (Mongin, 1986) and am still approaching it as a nice case study in economic methodology and the history of ideas rather than for its own sake. Fortunately, Langlois' article brings much grist to the methodological mill. It connects with what struck me as the central feature of the early full cost debate: that it was not a debate in the respectable sense, since none of the empirical arguments made by each camp ever reached the level of actual testing. From the vantage point of positivist or related—say Popperian or Lakatosian—doctrines in the philosophy of science, the discussions of the 1940s and 1950s proved abortive. They belong to the context of collective heuristics, not at all of knowledge and scientific progress. This pessimistic diagnosis is good news to Langlois, who appears by contrast to have undertaken a highly welcome quantitative check of the neoclassical claim on FCP.

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Methodological pessimism with respect to the early full-cost debate is far from being shared everywhere. Each camp in the controversy seems to have capitalized on the polemical points scored against the other, building up confidence in either of the following theses: FCP reduces to marginalism in a different language; FCP is counterevidence to marginalism and part of a forthcoming alternative theory. As Lee (1983, 1984) has shown, the former thesis has been the prevailing wisdom in economics since roughly the mid 1950s, when leading marginalists such as Austin Robinson and Heflebower expounded their views of the price-setting mechanism. The latter thesis was rarely adhered to at the time of the controversy, itself an interesting fact to note, and evolved most clearly among P. W. S. Andrews’ followers. Today it has some grip on post Keynesian circles, as can be seen from Eichner and Kregel (1975) and Lee (1985).

Both theses strike me as equally misconceived—this is where I shall have to disagree with Lee-the-interpreter after having much benefited from studying Lee-the-historian. To substantiate the unpopular claim that neither camp should have capitalized on the results of the early full cost debate would involve a detailed historical argument, which I have pursued elsewhere (Mongin, 1989). The more limited task to which I am setting myself in this article is to explain why the debate never reached the level of a careful quantitative test of the sort undertaken by Langlois. That there is an intriguing problem here can be seen from the following. The writers of the 1940s and 1950s were of course acquainted with the simple formula \((p - mc) / p = 1/e_d\), which antedates Langlois’ more sophisticated formalism, but they did not even make a crude effort to subject it to the test.

To solve the enigma, I shall have to investigate the structure of the neoclassical (or “marginalists” as they were then called) answer to the FCP literature. It will be seen that part of the solution lies with the *rhetoric* of economics—the highly diversified counterattacking moves of the marginalist camp were, or so I shall argue, effective *ad hominem* replies. Still, my final interpretation of the FCP debate makes its rhetoric a subordinate feature. Rather, it emphasizes the role of *empirical* misconceptions at the decision-theoretic level. The crucial explanatory concept in this non-relativistic account is *benign marginalism*, which is best defined as a compound of the crude model of profit maximization and lax semantics, allowing for reinterpretation of the optimizing process in terms of trial-and-error, bounded rationality, etc. In my opinion, it is benign
marginalism that barred detailed comparisons of FCP and marginalist models and therefore did much to sterilize the debate. By explaining this point in some detail, I intend obliquely to take sides on the recently much discussed issue of rhetoric in economics, as well as to cast some light on the novelty, hopes, and limitations of Langlois' contribution.

II. A historical brief on the early FCP debate

The 1940s and 1950s witnessed fairly extensive research on the price-setting behavior of individual firms (see Lee, 1983, appendix VI). It was pursued partly with a view of giving a realistic basis to price theory and improving upon the then prevailing models of imperfect competition, partly in the hope of investigating macroeconomic and policy-oriented issues (such as price rigidity in the face of low output in the 1930s). The leading study in the field was, of course, the Oxford survey summarized by Hall and Hitch in their famous article (1939). It was found that a high proportion of the 38 firms that had discussed the questionnaire with the interviewers set their prices by reference to some ex ante estimate of average cost, usually of average direct cost only. Typically, firms would take the latter as a base and add two percentage margins, one to cover overhead and the other to take care of gross profits.

Hall and Hitch insisted that this mechanism, to which they referred as "full cost," was a "rule of thumb" and could result in maximum profits by accident only (1939, p. 113). Despite this definitely heterodox bias, they attempted to relate FCP to the so-called "kinked demand curve" (KDC), which is usually understood as a rationalization of price stickiness along profit-maximizing lines. Their use of a demand schedule, however shaped, seemed more generally to be at variance with their sweeping claim that "producers cannot know their demand or marginal revenue curves" (1939, p. 114).

The unclear role of the KDC in Hall and Hitch relates not only to an unresolved tension between conventional and unconventional interpretations of the brute FCP mechanism, but also to the more subtle problem of connecting the latter with predictions of price rigidities. On the whole, the 1939 article is path-breaking, but highly incomplete in its implicit modeling. Its questionnaire technique was somewhat casual and questionable, so that even the factual evidence on how firms set their prices needed to be clarified.

Various empirical inquiries followed in the wake of Hall and Hitch,
as well as fewer theoretical developments. Part of this work, most notably Andrews' intricate *Manufacturing Business* (1949) and Harrod's definitely non-optimizing model in his *Economic Essays* (1952), carried with it the heterodox suggestion that profit maximization should be replaced with the so-called "full cost principle," which was understood to be a *theoretical* construct logically distinct from, and empirically competing with, the former. However, the more widespread view was that FCP referred to a piece of behavior, i.e., an empirical datum, which raised a problem for the profit-maximizing theory of the firm. The general strategy of the marginalist respondents (most notably Austin Robinson, Machlup, and Heflebower) consisted in simultaneously debasing the claim that the "full cost principle" was the starting point of an alternative theory, and defending traditional theory in the face of FCP evidence by adding some qualifications to the former.

Lee (1984) has shown that the discussion in the United States came to a halt somewhat abruptly in 1953, when the conference *Business Concentration and Price Policy* took place and Heflebower put into practice the dual strategy just mentioned in a carefully designed report that strongly impressed the attendants. Heflebower had favored the conclusion that FCP was compatible with profit maximization. Thereafter, the issue was regarded as settled to the benefit of marginalism by the majority of the profession in America.

The British story of FCP is perhaps not so easy to tell, due to the persisting heterodoxy of some (like Andrews) in the Oxford group, but it is best seen as culminating in the early 1950s, with A. Robinson's devastating review (1950) of Andrews' *Manufacturing Business* and the ensuing controversy in the *Economic Journal*.

It should be noted that the FCP debate came to be intermingled with a separate strand of antimarginalist arguments, when Machlup (1946) attempted to answer in the same breath both Hall and Hitch and Lester (the latter had claimed in 1946 that output and employment rigidity refuted profit maximization). Strictly speaking, the label "marginalist controversy" should be reserved for the controversy that ensued from Lester and Machlup, as well as for the related discussions in the *American Economic Review* from 1946 to around 1952. But definitions are of course unimportant, provided that the specificity of the FCP debate is well understood.

In spite of his role in bringing about the "demise" of FCP, Heflebower is a good concise source on the early developments. From my own
inquiry, I have to concur with his unpleasant conclusion that the work on the "full-costers" (as they came to be called) "short cuts a deep understanding of the market" (1955, p. 375). His own restrictive assessment was that FCP is not acknowledged beyond doubt outside the following competitive context: (i) oligopoly with a leading firm whose cost figures are borrowed by followers, (ii) oligopoly with agreement to use conventional cost figures, and, less clearly, (iii) oligopoly with implicit agreement such as was involved in cases brought to the courts. As far as the type of business is involved, Heflebower noted that FCP behavior occurred both in manufacturing industry and trade, with many examples in the retail trade and multi-product manufacturing concerns.

The problem of cost computations had led to numerous developments, and in 1955 Heflebower could make a list of various price formulas along FCP lines, some of them more and others less sophisticated than Hall and Hitch's own variant. The general problem of those formulas is that they rely on a predetermined output volume that is left implicit and that none of the full-costers were able to explain. Actually, the issue was hardly addressed at all. This fact becomes less surprising when it is realized that most of those writers believed that marginal, therefore average direct costs were constant on the output range relevant to price setting. Still, the problem of interpreting FCP should in principle have been kept independent of any assumption on cost, and the silence of the literature about implied reference volumes is one of its most severe shortcomings. To a degree, this failure reflected on the discussions of price rigidity, a problem that had given much impetus to the research on price setting. The general feeling was that stable prices were congenial to FCP behavior, but more precise general statements were wanting both at the empirical and theoretical levels.

To summarize, there had been some progress made after Hall and Hitch, but the adduced evidence remained definitely incomplete, and partly as a cause, partly as a consequence of this failure, the implied models of the full-costers were nebulous. In retrospect, the task of answering the FCP "challenge" to profit maximization does not strike one as all too difficult. The marginalists had to choose between the following moves: dismissal, reform, absorption-cum-status-quo. Since the third one definitely prevailed, I shall very briefly dispose of the first two strategies. *Dismissal* would have primarily meant disputing the data, a stand that was occasionally taken but was difficult to sustain, given the rich evidence gathered at least on the price-setting mechanism itself.
Reform was another line of defense to adopt, and the historian of the early FCP debate (as well as of the related "marginalist controversy") is quite naturally led to ask: was there any connection between those crises and the well-documented emergence in the late 1950s and early 1960s of a whole spectrum of alternative hypotheses to profit maximization? A curious result of my inquiry, which is at variance with a suggestion made by Lee (1984, p. 1122), is that this much expected connection is by and large not to be found in the early literature on FCP. The theory of the firm that emerged as the victor in the early debate is but prewar or immediately postwar marginalism—roughly speaking, Joan Robinson's analysis of the single firm with monopoly as its central case. The early FCP debate somewhat disappointingly resulted in absorption-cum-status-quo.

The marginalists' absorption strategies were highly diversified, with several arguments originating in the FCP literature itself. The discussion below will be reorganized along an ex ante/ex post distinction. Marginalists could have regarded as irrelevant the fact that firms make decisions in the full cost way, or in accordance with any rule of thumb whatsoever. These authors would thus have only been concerned with reconciling the observed effects of firms' decisions on, say, price and quantities, with corresponding predictions of a suitably chosen profit-maximizing model. This typically is an ex post absorption strategy, for it is best achieved by assuming that profits are at a maximum, given the objective cost and demand conditions of the firm, and by drawing the desired consequences.

Marginalists, however, could also have been concerned with salvaging profit maximization as a description of the essentials of the decision process itself. Such a move is not as hopeless as it first seems to be, since it capitalizes on a semantic point repeatedly made in Machlup (1946): profit maximization has many logically equivalent variants, some of which may come close to the FCP evidence. This typically is aiming at ex ante reconciliation, for one does not now have to insist that actual profits are at a maximum. Rather, the strategy is to assume some view, possibly incorrect, of the cost and demand conditions on the firm's part, and to rationalize as much of its decision-making process as possible.

III. The marginalist response to FCP

I shall first illustrate the ex post strategy. A beautiful example is Wiles' 1950 article, which was explicitly directed toward a solution of the FCP
debate. Wiles singled out two facts in the FCP literature: the firm sets its price and makes it sticky, so that its marginal revenue is a horizontal segment (ending at full capacity), and it normally operates, or rather claims normally to operate, in conditions of constant marginal costs. The *explicandum* is then the firm's decision of producing $X$ as in Figure 1.

Note that Figure 1 correctly depicts an average total cost curve, which lies above the horizontal stretch of the marginal cost curve. It also assumes that price or marginal revenue is above average total cost at the operating level $X$, i.e., that the particular firm does not make losses. Hence a problem for standard theory: the FCP firm operates at a level $X$ where $MR > AC > MC$; supposing $X$ is not full capacity, it cannot be profit maximizing. Figure 2 offers the sketchy solution to this problem. The marginal cost and marginal revenue schedules have been changed, so that the former is increasing and the latter decreasing on the relevant range; they happily intersect in the vicinity of $X$. The cost
concept in Figure 2 is a long-run one in a special sense: it involves the delayed costs of using fixed equipment and managerial staff. It is claimed that overtime, overloading of plant, etc., are not duly taken into account in the short-run schedule of Figure 1.

As far as declining marginal revenue is concerned, it depends on a "goodwill" explanation: the closer the firm comes to full capacity, the less it is able to serve rush orders. Despite its idiosyncratic understanding of the long run, Wiles' account is a typical one. It does not attempt to square with facts of a decision-theoretic sort, only with output and price data. "True" cost and revenue curves (as against those alleged by the firm) are used in the marginalist model to show that the FCP output/price combination is a profit-maximizing one. This is ex post reasoning at its best. An obvious weakness of the example is that it is concerned with stylized facts rather than detailed evidence (for that matter, with price rigidity rather than FCP) and its alleged solution relies on grossly qualitative arguments of a persuasive kind.

Marginalists could do better: this is where the ancestor of Langlois' formalism enters the stage. Following a well-known textbook formula of monopoly analysis, price can be expressed as a function of marginal cost and price elasticity of demand $e_d$:

$$ p = mc \left[ e_d/e_d - 1 \right] $$

Hence the following simple prediction on the markup rate (expressed here as margin over marginal cost as a ratio of price):

$$ (p - mc)/p = 1/e_d $$

Granting the usual assumption of constant marginal costs, $e_d$ becomes commensurable with FCP data and it is enough to get hold of (ordinary) elasticity data to carry out a test. Thus, a quantitative solution of the FCP debate, at least in the ex post sense, is forthcoming at this juncture. 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, they would obviously have been highly welcome in the FCP debate. Still, to the best of my knowledge, the early literature does not report any attempt to implement the crude test of this paragraph. This is a most intriguing
problem, since the formulas above were, of course, widely known. They belong to the core part of the doctrine of the time—Joan Robinson’s analysis of imperfect competition—and were used in the (admittedly different) context of discussions about Lerner’s and Kalecki’s measurement of the “degree of monopoly.” Actually, the formulas were even alluded to in the FCP debate, as if they by themselves supported marginalist absorption of FCP, a fact that is puzzling both to the empirically-minded economist and to the methodologist.

The ex ante absorption strategy is best seen as a general argument aimed at substantiating and making the best of the following two claims. Claim 1: One may describe decision routines—more generally than FCP—in terms of the agent’s optimizing some objective variable, despite the fact that he may not be aware of doing the required computations. Claim 2: By varying the competitive conditions and time horizon, one can avail oneself with reformulations of the profit maximization hypothesis, some of which do conform with FCP data about the firm’s decision making. Machlup’s (1946) famous example of the automobile driver was an illustration of claim 1, which I shall not discuss in detail here, because it did not play the major role. Its limitations as an absorption device are obvious.

Even granting the somewhat farfetched claim that every rule of thumb is amenable to an optimizing account, it would remain to be shown that profit in the usual sense is the objective variable relevant to the FCP rule of thumb—a logical step with which reformist theories of the firm, such as Baumol’s sales maximization hypothesis, were concerned later, but which was usually just taken for granted at the time. Even more important, the decision-theoretic arguments that supported claim 1 only relied on examples and analogies and could therefore not sustain any general statement, except in a persuasive way. They often were an accompaniment of the seemingly more powerful claim 2.

One example of the latter is Machlup’s conviction that FCP is but ex ante long-term profit maximization in an oligopolistic context. The point is argued in two distinctive ways. One simply amounts to the claim that collusive oligopoly is a profit-maximizing equilibrium of the branch (1946, p. 543). In the same breath, Machlup examines the completely opposite case of an oligopolist having no information whatsoever on his competitors’ reactions, and goes on to argue that applying an FCP rule should be long-term optimal gain. This is so, because in the absence of knowledge of prices set by competitors (whether actual or potential), a
clue is given by the firm's own cost (cf. "average cost as a clue to demand elasticity").

There were other ex ante rationalization schemes besides that of Machlup. While discussing Andrews, A. Robinson came to suggest that the standard construal of long-term perfect competition could accommodate FCP (1950, p. 777). This was an obvious enough point to make, since the latter model equates price with full average cost (i.e., average total cost inclusive of "normal" profit). It was easier to make in an ex post context, where one could dispose of the decision-theoretic objection that FCP is not price-taking behavior. Still, A. Robinson's argument is—possibly inconsistently—part of a plea for ex ante reconciliation.

Another rather popular line was to allude to the basic model of monopoly, and the role of demand in it, in a way suggestive of ex ante reconciliation. In brief, marginalists (sometimes even one and the same writer) went over the whole range of competitive conditions in an effort to absorb FCP, and did not seem to be worried by such an eclectic procedure. On the other hand, they came to some sort of consensus view that FCP was to receive a long-term explanation and that standard theory could provide one, despite its admittedly exaggerated concern with short-run price and output determination (cf. Gordon's summing up of the debate in 1948).

The examples above conveniently lead to a general assessment of the marginalist response. They have the common structure of compatibility empirical arguments. By this I mean arguments to the effect (only) that part of the FCP evidence fits in with some suitably chosen profit-maximizing model. Had the marginalists used the whole FCP evidence, however scanty it was, they would presumably have relied on the special connection between that price mechanism and oligopoly with explicit or implicit collusion. Had they not been interested in just finding a favorable case and closing the discussion at that, they would again have narrowed down the range of relevant competitive conditions. True, they felt uneasy about oligopoly and were prone to deal with its problems within the formalism of other models. But the reader should note that all of the marginalist points restated in this section partake of the same logical structure, which may be restated as follows: existential reasoning along with only partial concern with the data. (The methodological critique implied in this paragraph may be compared with Rosenberg's (1989) discussion of "generic" predictions in economics.)

What could such a mode of thinking hope to achieve? Taken in
isolation, virtually nothing. But the marginalists responded and their arguments are best understood as ad hominem ones. This is obvious from Machlup (he claimed to dispel misunderstandings fostered by casual or misdirected empirical research) and A. Robinson (who had carefully selected the startling claims of Andrews in order to refute him), but it is also visible elsewhere, even in those pieces which were intended as empirical ones. The marginalists' weak existential claims could be excused as polemically relating to the antecedent claim (allegedly found in the full cost literature) that no marginalist model could ever account for FCP evidence. Also, the full-costers' mode of reasoning had itself been permeated by existential reasoning: recall, for instance, that price rigidity had not been delineated rigorously and the only safe general claim was that full cost prices did not instantaneously adjust to demand. Again, the marginalists could excuse themselves from searching after nomological statements: they were just beating their opponents with their own weapons.

IV. Benign marginalism and the problem of testing profit maximization

The account of the last section emphasized the rhetoric of the debate. Despite the fashionable trend nowadays in economic methodology, I do not think one should stop at that. Not every economic debate is purely—or even mainly—a rhetorical one. The rhetorical turn of the FCP debate, its barrenness from the point of view of positivist or Popperian philosophies of science, calls for an explanation. Mine is grounded on the fact that marginalists misrepresented the profit maximization hypothesis, along with the conjecture that this misrepresentation prevented them from discussing the FCP findings in a truly empirical way. I shall now argue both points in succession.

As it happens, marginalists had a much broader interpretation of the optimizing model than has become common since H. Simon's work on bounded rationality became known. (Simon's first "limited rationality" model is as early as 1955; applications to the firm are discussed in some detail in his 1962 article.) That is, Simon alerted us to the fact that, independently of which objective variable is chosen, optimization is never a trifling assumption to make. It has definite implications of a cognitive nature. As a result, one should not hastily believe that some
optimizing process underlies every single piece of conscious rational deliberation whatsoever.

Simple though it may strike us now, this point was very nearly completely overlooked in the early debate. For example, referring to a passage in Manufacturing Business where Andrews had described entrepreneurs as rationally deciding against underselling their competitors, 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, p. 778).

In fact, Andrews had had a non-specific rational procedure in mind. A. Robinson's non sequitur is to jump from evidence of incremental reasoning to the presumption of profit maximization in the technical sense. Not every incremental reasoning is an optimizing one: the businessman may as well content himself with comparing net incremental values with some threshold value, a case of satisficing; or limit his computations to a subset of his feasible set, a case of simplified optimization; or take one of the relevant variables (revenue) as given at some value while minimizing the other, a case of suboptimization.

All of these cases have been documented at some point in the decision-theoretic literature. It has been shown that the use of "break-even" charts for profits is not in itself evidence for profit maximization. Still, the confusion of incremental reasoning with the latter, highly specific model underlies the whole of the marginalist literature, or very nearly so. Other examples would involve the confusion of rational trial-and-error processes (= decreasing margins ex post in view of disappointing realized output and profit) with usual monopolistic reasoning (= taking ex ante account of elasticity). The list of illegitimate extensions is large indeed. I shall call benign marginalism the view that underlies them, i.e., the view that agents make decisions after balancing advantages and disadvantages in some rational way, conjoined with the (false) belief that this is all that marginalist decision making says.

Benign marginalism was most instrumental in bringing about the "demise" of FCP. It underlies and in part explains the orthodox interpretation of Heflebower's work as closing the full cost debate in favor of the standard theory. In effect, what Heflebower had shown was that margins were more flexible than full-costers had claimed and more rigid than prewar marginalism would have predicted. To jump, as for instance Coase (1955) did in his comment, from such an ambiguous fact
to the conclusion that marginal analysis was salvaged and perhaps even reinforced, required a rather strong hidden premise. I think that it is benign marginalism which made possible Coase's inference, which I restate as follows: businessmen rationally react to demand information, hence profit maximization is upheld. Now, to go one step further, I venture to claim that even the early rhetorical turn of the FCP debate owes much to benign marginalism. This has already been argued with reference to A. Robinson, but I could have chosen to discuss Machlup's typical conflation of rational rules-of-thumb and optimization, or the very lax definition of profit maximization that is also to be found in the work of some writers classified as full-costers (e.g., Saxton, 1942).

Note that the account suggested here of the end of the FCP debate does not rely on the use of the "elasticity argument" in its technical form. It is therefore markedly different from Lee's account, which appears to place exaggerated emphasis on that piece of formalism (1984, pp. 1118-1119). Similarly, Langlois seems to me to be wrong in attributing to Machlup (1946) the all too definite concept that markup stands for a proxy of the inverse of price elasticity. This concept is not to be found in Machlup's article, which is not to say, of course, that he was unacquainted with the usual monopoly formula (rather, he did not really distinguish it from the altogether different claim that markups should react to demand changes in the qualitatively appropriate way). If there is a historical point to be made with respect to the use of the "elasticity argument," it should therefore be... the lack thereof. Benign marginalism, in my opinion, accounts for much of the neglect of such a handy test, both in the early controversy and later on. This suggestion should be compounded with the following, more tangible reasons: there were indeed some scanty data about margins (e.g., Helebrower's) but elasticity data of the sort consistent with the former were probably not so easily available; from Langlois' work, I understand that this matching data problem may still be a vivid one. And the assumption of constant marginal costs would, of course, have made the test a far from decisive one.

The historical discussion of the "elasticity argument" conveniently leads to appraising Langlois' article. She takes the ingenious step of replacing the standard assumption with that of per-unit-time profit maximization, time being now regarded as a decision variable by the monopolist on a par with output and price. Adding technical regularity conditions, this reformulation will serve two purposes. (1) It implies a test formula involving markup over average rather than marginal costs.
(ii) It always makes the optimal price/output combination an interior solution, thus disposing of the awkward case of corners (where the elasticity formula would not follow from profit maximization). At the theoretical level, Langlois claims that her endogenous treatment of time is more satisfactory than the usual implicit assumption of a fixed indivisible period. The procedure suggests the following comments.

First, the model is tested by using ordinary elasticity data, whereas the theoretical elasticity concept is a novel one, being variable with time (see Langlois, 1989c, pp. 135–136). One may wonder whether or not there is a discrepancy here. Presumably, elasticity figures are computed on the assumption that the elasticity concept is invariant with respect to time. But such an assumption would surely be undesirable in Langlois’ model.

Second, comparison of FCP with a formula involving the final consumers’ elasticity of demand has the obvious effect of restricting the assessment of FCP to the retail trade. While the price mechanism was also alleged to prevail at the retail level, and was investigated as such by Heifebower, the early full-costers were mainly concerned with the manufacturing industry. Andrews emphasized the fact that “the typical customer of a manufacturing business is another business, not a consumer of the conventional kind” (1949, p. 149). Typically, the demanders facing a manufacturing concern are few and place large, possibly intermittent orders, possibly on the basis of a “goodwill” relationship with the firm. FCP, and the apparent neglect of demand implied in it, may have a special connection with such special features—roughly speaking, the lack of markets in the usual sense. This is not to say that testing the connection between FCP and marginalism at the level of retail trade is of no value, but only that the work should be pursued at the more relevant level of the producer. I have, of course, no idea of how to construct suitable elasticity data for non-final consumers. The problem of matching elasticity and margins data would arise vividly here; note that it is circumvented by Langlois in a way that is not entirely satisfactory, since she uses direct elasticity data on the final consumers’ side, but has to construct the dealers’ markup from those of the producers (Langlois 1989c, p. 150).

Third, the treatment of oligopoly in Langlois’ paper is as cavalier as one might expect from anybody working within the purview of a monopoly model. I shall not press this point too far, since too severe a concern with oligopoly would prevent one from testing the “elasticity
argument, ’’ and even possibly from carrying out any quantitative check whatsoever of the relationship between FCP and profit maximization. A crucial problem in this connection is that of a suitable measurement of price elasticity of demand to the firm. Langlois’ rather mechanical extension of her model to oligopoly (1989c, p. 137) implicitly relies, when it comes to testing, on the assumption that the elasticity of demand is the same at the firm level as it is at the industry level. Admittedly, a similar assumption is to be found in some oligopoly models (e.g., Sylos-Labini, 1962) but it is more easily justified in the context of sketchy theoretical construals than of empirical testing.

My fourth comment stems from an attempt to relate the author’s discussion of costs and corner solutions to the early literature. Langlois is annoyed at corner solutions, since the nice marginalist relationship among price, marginal cost, and elasticity does not hold there, which impedes the testing of the original “elasticity argument.” But corner solutions are not a nuisance to the empirically-minded economist—on the contrary. Langlois mentions the useful fact that marginal costs in the automobile industry lie below average costs (p. 8). Now, if we assume an FCP firm, the short-run inverse demand curve should presumably be a horizontal segment ending at capacity level. Assuming further that the automobile firm is not making losses, Wiles’ argument applies: \( MR > AC > MC \) on the feasible interval, whence the implacable conclusion that the (short-term) profit-maximizing output is a corner solution, i.e., the automobile firm should produce at full capacity. Here is a test that would seem easy enough to implement: check whether or not profit-making automobile companies produce short of capacity. It is worth noting that this test is the one that should have emerged from the rather muddled discussion over Eiteman and Guthrie (1952) in the American Economic Review.

Now, why is it that Langlois has tried to circumvent the logical possibility of corner solutions rather than use it in the manner of the suggested test? One reason may be that she would not take the step implied in Wiles’ reasoning, of assuming a horizontal short-run \( MR \) schedule. But if this is so, Langlois should have little hope of reconciling marginalism with FCP, despite the attractive title of her article. For it can be claimed that one of the (modest) findings of the early FCP literature was that prices were sticky for some time after being set, a statement that is arguably translated into a horizontal short-run demand schedule if one has to resort to traditional terminology. Following this
interpretation, therefore, the paper under discussion is but a test of a sophisticated variant of the usual short-run monopoly model and should have no pretense to settling the debate recapitulated here.

There is, however, another reason for which Langlois may have not considered the test of the last paragraph: she would have known the answer already. Glossing over the semantic difficulty in assessing operating rates and full capacity, there is much evidence both in Andrews (1949) and in the more recent literature (e.g., Koutsoyiannis, 1975) that manufacturing firms normally produce short of capacity. *Hence short-run profit maximization, in the usual sense and as an unqualified hypothesis, is refuted.* I am wondering whether or not the author would agree with such a prima facie inescapable conclusion. Supposing that she would, she may explain how her formal generalization of marginalism relates to the refutation of the standard short-term construal. This is a pressing point, since one may wonder whether the former, with its questionable addition of one free variable, does not necessarily obscure the latter important fact.

My first comments expressed some reservations about the severity of Langlois' test, whereas the last one is intended to prevent misinterpretation even if the test were both severe and successful. Empirically supporting per-unit-time profit maximization is not immediately relevant to the FCP debate, which was concerned with the traditional profit maximization hypothesis. If it is a fact that the latter fails in its usual short-run interpretation, so much should be said *ab initio*: a point would then—belatedly—be scored against the marginalists. The old controversy would not be settled for that, since marginalists had eventually come to the view that only *long-run* profit maximization was relevant to FCP. There remains room for empirical investigations along traditional lines, i.e., checking the primitive "elasticity argument" against some suitably constructed long-run demand elasticity data (ideally at the manufacturing firm level). The curious story of this simple piece of economics has by no means come to an end with Langlois' ingenious reformulation.

REFERENCES

The references for all articles in this symposium are gathered on pp. 278–281.