The genesis of ‘positive economics’ and the rejection of monopolistic competition theory: a methodological debate

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In ‘The methodology of positive economics’ (1953), Milton Friedman linked the adoption of a falsificationist methodology to the rejection of monopolistic competition as a valid assumption, thus elaborating a point made earlier by George Stigler in ‘Monopolistic competition in retrospect’ (1949B). Both failed to demonstrate that the alternative assumption of perfect competition would perform better under falsificationist rules. These rules themselves are an expression of the ambition to establish an economic science rather than a convincing framework for research. Monopolistic competition theory became the main target of attack, as it highlighted the problematic nature of empirical research interested in perfectly generalisable results.

Introduction

What constitutes progress in economic research? Which cognitive processes lead to generally acceptable results? Academic economists tend to agree that progress in economic theory is achieved by the formulation of new hypotheses that can be mathematically expressed and whose predictions are tested against statistical data. A bad fit of the prediction will lead to the rejection of the underlying hypothesis, while a good fit will allow it to be retained. The procedure of testing a general hypothesis against empirical data mirrors Karl Popper’s ‘deductive method of verification’ which he formulated in his 1934 book, Die Logik der Forschung (The Logic of Scientific Discovery). Popper identified the ‘deductive method of verification’ as the only appropriate method for evaluating scientific sentences and systems. The fact that scientific sentences are falsifiable, i.e., capable of being refuted by empirical testing, enables them to be distinguished from unscientific ones. Hence, the methodology that requires the formulation and testing of predictions in order to reject, or fail to reject, the underlying system of postulated assumptions and relations is referred to as falsificationism.

Milton Friedman’s 1953 article, ‘The methodology of positive economics’, established falsificationism as the dominant methodology in economic research, as far as the self-perception of academic economists was concerned. However, and as will be argued below, Friedman’s article suffers from two major shortcomings. First, the methodological

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postulates of Friedman's contribution were incompatible with his own descriptions of economic practice given in the same article. Second, Friedman's argument that the adoption of falsificationism would necessarily lead to the adoption of perfect competition theory and to the rejection of monopolistic competition theory does not stand up to closer scrutiny. The establishment of falsificationism as the relevant methodology for economics and the resultant rejection of monopolistic competition theory both stemmed from the same impetus to move economics as a research enterprise towards the ideal of an historically invariant form of economic science. It will be shown that in elaborating this double theme Friedman was building on work by George Stigler, his colleague at the University of Chicago, who had originally formulated the link between the two themes in his 1949 article, 'Monopolistic competition in retrospect'. In this article Stigler engaged in a polemic against monopolistic competition theory and, in the conclusion, formulated the outline of an appropriate methodology for economics.

At the same time as Friedman and Stigler were postulating falsificationism as the new methodological norm for economics, Friedman also provided colourful and perceptive descriptions of economic practice, in which progress in economics was attributed to the use of experience, intuition and convention. Although these descriptions were, in principle, incompatible with falsificationism, monopolistic competition theory was singled out, as it had in this context the role of bearer of bad news. By introducing parameters beyond quantities and prices, as well as historical time, it had increased the descriptive realism of economics, but had thereby also highlighted the ambivalent relationship between description and prediction in economic research. In this situation, Friedman and Stigler suggested that a choice between the scientific character and the descriptive realism of economic theory could be made at the price of sacrificing monopolistic competition theory. While the suggestion proved to be attractive, it was logically untenable, since monopolistic competition theory fared better, rather than worse, in terms of predicting firm behaviour, than the competing assumption of perfect competition. The struggle to come to terms with the ultimately insoluble contradiction between the quest for scientific rigour and the need to account for a heterogeneous underlying reality is examined in the final part of this paper as one of the defining characteristics of economics as a distinct discipline.

'The methodology of positive economics'

Frequent reference is made to 'The methodology of positive economics' not only in economic methodology, but also in economics in general. One of its attractions rests in its dual nature: it is not only a methodological treatise, but also an exposition of 'best usual practice'. This double nature proved successful in influencing professional economists' self-perception of the methodological procedures they employ. For researchers it provided the attractive, 'scientific' epistemological framework of falsificationism, as well as a perceptive description of actual economic practice and, most importantly, the assertion that both were fully compatible. Trying to demonstrate how the new methodological propositions were able to distinguish good theory from bad theory, Friedman singled out monopolistic competition theory as a counter-example incompatible with the proposed framework and hence as something to be rejected.

1 "'The methodology of positive economics'...was, for many economists, one of the few methodological works they ever read' (Backhouse, 1995, p. 183).
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Friedman’s first main point is that falsificationism is the relevant methodology for positive economics. Scientific progress is thus to be achieved, as in the natural sciences, by comparing predictions of a theory (which will include the relevant boundary conditions and assumptions) with empirically observed data.

Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which it is intended to ‘explain’...the only relevant test of the validity of a hypothesis is comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted...it is accepted if its predictions are not contradicted; ... factual evidence can never ‘prove’ a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactiy, that the hypothesis has been ‘confirmed’ by experience. (Friedman, 1953, pp. 8-9)

This statement is identical to the methodological postulates of Popper in *The Logic of Scientific Discovery*. Popper had been the first to stipulate the requirement that a scientific theory should have a form that allows it to be falsified, or, in his own words, ‘an empirical-scientific system has to be able to be rejected by experience’ (Popper, 1934, p. 15). He was also the first to emphasise that the falsifiability of the system was not a criterion (or a means) to establish the truth content of a theory. It is exclusively a criterion to establish the scientific character of a theory and a means to reject false theories.

In discussing the connection between Friedman’s and Popper’s work, care should be taken to distinguish the words ‘positive’, as in ‘positive economics’, and ‘positivist’, as in ‘positivist methodology’. The former is used rhetorically by Friedman to distinguish an economic science based on falsificationist rules from other forms of economics. According to Friedman, normative science is concerned with what ‘ought to be’, while positive science is concerned with what ‘is’ (Friedman, 1953, p. 3). The introduction of this distinction between ‘normative’ and ‘positive’ science fulfils a purely rhetorical purpose. Associating himself firmly with the widely shared ideal of a science that concerns itself with what ‘is’, Friedman provides, from the beginning, strong credibility for the methodology and the specific form of positive science he is advocating in the course of his essay. By opposition, he implies that other paradigms might have normative objectives and should therefore be excluded.

Positivism is the *bête noire* of Popper’s writing. Scientific positivism was a popular approach to scientific methodology centred in post-World War I Vienna. Some of its better known proponents were the physicist and philosopher Ernst Mach, and the philosophers Rudolph Carnap and Ludwig Wittgenstein. Its basic postulate was that scientific hypotheses can be positively verified as true, if they can be shown to be derived from a number of elementary ‘basic sentences’ that are held to be unquestionable tenets of human experience. According to positivism, general scientific hypotheses are thus induced from an ultimately sensory experience. Popper introduced his falsificationist methodology in explicit opposition to the Viennese positivists.

The second major point developed in ‘The methodology of positive economics’ is that monopolistic competition theory has to be rejected, since it does not conform with the newly established falsificationist approach. The argument against monopolistic com-

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1 While Friedman did not refer to Popper in ‘The methodology of positive economics’, he later confirms the closeness of their views in an interview (Frazer and Boland, 1983, p. 129). Moss (1984, p. 314) and Rosenberg (1993, p. 826) also have noted the identity of the basic methodological postulates of the two authors.

2 Translated from the German language edition.

3 The technical term for this referral to a generally accepted principle is apodixis.
petition theory is developed on the basis of the statement that empirical observation has no role to play in the selection of assumptions of testable hypotheses.\(^1\) This point is consistent with scientific falsificationism.\(^2\) However, not content with this fact, Friedman went one step further and argued:

Truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumption... To be important, therefore, a hypothesis must be descriptively false in its assumptions... *(ibid., p. 14)*

In other words, as descriptive realism is no criterion for the selection of an assumption, *therefore* realistic assumptions should be excluded and unrealistic assumptions should be adopted. This absurd extension of Friedman's argument is only comprehensible in the context of its function to support the use of perfect competition as opposed to monopolistic competition as a general assumption in economics:\(^3\)

The theory of monopolistic and imperfect competition is one example of the neglect in economic theory of these propositions. The development of this analysis was explicitly motivated, and its wide acceptance and approval largely explained, by the belief that the assumptions of 'perfect competition' or 'perfect monopoly' said to underlie economic theory are a false image of reality. *(ibid., p. 15)*

Friedman was effectively arguing that monopolistic competition theory should be excluded from economics because of its quest for descriptive realism. The absurdity of the proposition that realistic assumptions were not allowed in the formulation of economic hypotheses did not pass unnoticed and became the centre of an ensuing debate. Paul Samuelson called it the 'F-twist' (Samuelson, 1963, p. 233). The extended debate over the 'F-twist' and its implications in the early 1960s is interesting for the rhetorical brilliance and the personal engagement of its main protagonists. Somewhat surprisingly, though, the debate remained silent on the wider-reaching problems of the adoption of a falsificationist methodology in ‘The methodology of positive economics’.

These problems should have become apparent as Friedman moved in his argument from the formulation of methodological prescriptions to the discussion of actual practice. For the particular practice of doing economics that he describes approvingly, vividly and with great inside knowledge is thoroughly incompatible with the Popperian-style falsificationism laid out in the beginning. Contrary to his original statements on methodology, he now argues that assumptions should be chosen because they seem to 'work' as part of other hypotheses. Indeed, in his view, assumptions are to be accepted as they have 'become part of the tradition and folklore of a science' (Friedman, 1953, p. 22). He assigns great importance to the personal discretion of the researcher as to which parameters are to be included in an evaluation of results and how liberally the *ceteris paribus* conditions are to be interpreted case by case:

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1. *Ad verbum*: ‘[T]he most significant feature of theoretical sentences [assumptions]...is that they are neither descriptively true, nor descriptively false’ (Massey, 1965, p. 1162).
2. ‘The second descriptively unrealistic assumption justified by this argument was utility maximisation, which is referred to as the 'maximization-of-returns hypothesis' (Friedman, 1953, p. 22).
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Each occurrence has some features peculiarly its own, not covered by the explicit rules. The capacity to judge that these are or are not to be disregarded, that they should or should not affect what observable phenomena are to be identified with what entities in the model, is something that cannot be taught; it can be learned but only by experience and exposure in the ‘right’ scientific atmosphere, not by rote. It is at this point that the ‘amateur’ is separated from the ‘professional’ in all sciences and that the thin line is drawn which distinguishes the ‘crackpot’ from the scientist. (Ibid., p. 25)

This statement, backed up by several examples, comes as something of a surprise in a text which sets out by demanding that observation of predictions formulated on the basis of general hypotheses, not their reinterpretation in the light of particular circumstances, is the only way to comply with the standards of a scientific discipline.¹

This apparent contradiction has contributed to a fascination with the question of what is Friedman’s ‘true’ methodology. Different researchers have answered this question in different ways. Mark Blaug, for instance, has called Friedman’s approach to methodology ‘instrumentalist’. Instrumentalist approaches are, according to Blaug, characterised by the fact that they consider theories ‘only’ instruments for predictions (Blaug, 1992, p. 92). The term thus captures some of Friedman’s nonchalance towards his own statements on falsificationism when he talks about the practice of research. However, the term is not without problems of its own. Popper, for instance, had commented on instrumentalist approaches: ‘For instrumental purposes of practical application a theory may continue to be used even after its refutation... Thus a mere instrument for prediction cannot be falsified’ (Popper, 1969, p. 113). Hence, defining a methodology as instrumentalist implies that theories developed and tested under its rules will not possess the ability to be ‘rejected if its predictions are contradicted’, i.e., falsified. However, the possibility to falsify an incorrect theory was identified by Friedman as the crucial element of a scientific procedure in the first part of the ‘Methodology of positive economies’.

The falsificationist credo of Friedman is thrown into doubt not only by his explicit pronouncements on the practice of economic research. Roger Backhouse, following Abraham Hirsch and Neil de Marchi, has highlighted the incompatibility of falsificationism and the methodology Friedman employs in his own research. In fact, he defines Friedman’s implicit methodology as that of an eclectic empiricist in the tradition of Alfred Marshall and Wesley C. Mitchell. Friedman was, on that account, ‘a working economist, not a “professional” methodologist or philosopher of science’ (Backhouse, 1995, p. 201). While not unattractive, this characterisation can only answer part of the question, as it neither explains the enormous influence of ‘The methodology of positive economics’, nor does it do justice to Friedman’s clear statements on a falsificationist scientific procedure.

Tony Lawson in his article, ‘Realism, closed systems, and Friedman’, does not content himself to set ‘Friedman the practitioner’ against ‘Friedman the philosopher of science’, but analyses how the two different aspects can both be traced to the specific examples of scientific hypotheses provided in ‘The methodology of positive economics’. These examples, of which the two most important ones are the law of gravity and the positioning of leaves in order to maximise the reception of sunlight, are provided in order to illustrate how a correct methodology identifies general underlying causal forces. In Friedman’s eyes, it is able to do so even if single manifestations contradict predictions based exclusively on these general causal forces, as transitory influences independent of these

¹ Note in this connection also the six studies listed by Friedman in 1946 as exemplary for economic research in a letter to Edwin B. Wilson. These studies distinguish themselves by a pragmatic approach combining detailed institutional knowledge with moderate quantification (S. Stigler, 1994, p. 1200).
forces determine the outcome. Lawson shows that the specification of the necessary conditions requested for an appropriate experimental set-up that would deliver the 'right' result would unavoidably lead to a wider discussion of causal forces, including the 'transitory influences', and would eventually imply a more open approach to causality that is identified as 'transcendental realism'. Lawson quotes as a forerunner of such an approach John Stuart Mill (Lawson, 1992, p. 152). And the quoted passage is quite compatible with the eclectic, common-sense approach identified in connection with Alfred Marshall, and 'Friedman, the practitioner'.

Lawson is right when he states that confusion is created by Friedman's explicit commitment to unequivocally testable predictions under fully specified conditions and his desire to establish firmly causal mechanisms that are generally valid independent of the particular outcome of a given prediction (Lawson, 1992, p. 166). Similarly, Blaug and Backhouse identify convincingly different aspects of Friedman's approach to economic analysis. But neither of them explains the underlying motivation for the mix of incompatible methodological approaches that are contained in 'The methodology of positive economics'. In fact, an approach interested only in methodology might not be able to identify any coherent strategy of the text. In order to identify such a unifying purpose of Friedman's essay, its general context and, more precisely, its position in the history of economic thought has to be considered. This context is provided by the ongoing debate between economists who were to varying degrees associated with the 'Chicago School', on the one side, and Edward H. Chamberlin and his supporters, on the other. The topic of this polemic was whether the assumption of monopolistic competition or the assumption of perfect competition should provide the appropriate basis for micro-economic theorising.

In the context of this debate and in the wake of Stigler's article on 'Monopolistic competition in retrospect', Friedman's article on 'The methodology of positive economics' was designed to provide an authoritative statement. Only in this perspective does the juxtaposition of its two incompatible central statements, i.e., that falsificationism is a desirable methodology for economics, and that economic practice is a matter of judgement, experience, intuition and tradition, become intelligible. In a first step, falsificationism is established as the relevant methodology for economics in order to refute monopolistic competition theory and to render meaningless its claim of working with assumptions that are observably more realistic. In a second step, the assumption of perfect competition is justified, since it 'works' as part of a set of conventionalised rules reflecting an institutionalised consensus. The fact that predictions based on assumptions of perfect competition may or may not be confirmed by empirical data is countered by the argument, illustrated in the examples discussed by Lawson, that the role of theory is to identify the broad underlying forces and not to concentrate on single instances which may be determined by transitory phenomena. The result of these arguments is that perfect competition (and to a lesser degree the maximisation of returns) is the only

1 Despite the confident falsificationism proclaimed at the beginning of his essay, Friedman was willing to go to great lengths in order to defend a hypothesis once it had been established. He writes, for instance, that 'the "evidence" [of a test of predictions] may be internally contradictory, so there may be no hypothesis consistent with it' (Friedman, 1953, p. 9n). This implies a hypothesis that is in principle unfalsifiable. When a hypothesis fails to explain the facts, Friedman suggests that it is the facts and their contradictions that are at fault. This 'all-the-worse-for-the-facts' attitude follows the logic of scientific positivism and leads directly to Popper's criticism that such a procedure is uncritical,...it makes statements where it expects new insights; thus its statements become dogmas' (Popper, 1934, p. 25).
admissible assumption for microeconomic theorising regardless of its predictive performance.¹

In presenting this line of argument, Friedman was broadening points that had been made earlier by Stigler, also with explicit reference to the debate on monopolistic competition. It is interesting to see how ‘The methodology of positive economics’ had an impact way beyond the debate on monopolistic competition, by combining the claim to adhere to a demanding methodology with the defence of an actual practice that was based on methodologically less demanding principles. The persuasive power of Friedman’s arguments, ultimately tenable or not, had been enhanced by the fact that they managed to formulate an emerging consensus. The choice of falsificationism as the appropriate and quasi-official methodology for economics, for instance, was not Friedman’s responsibility alone, but the result of wider forces. Samuelson, for example, for all his sarcasm about the ‘F-twist’, never questioned the basic appropriateness of falsificationism for economics, which was close to his own methodological preferences. It was Friedman, however, who linked falsificationism irrevocably to the assumptions of perfect competition and maximisation-of-returns and to the refutation of monopolistic competition theory. It is shown below how this line of reasoning originated with Stigler and his article ‘Monopolistic competition in retrospect’. Friedman himself confirmed his debt in this respect in his contribution to the obituary edition of the Journal of Political Economy honouring Stigler:

George and I carried on an intensive correspondence while he was exiled to Brown and Columbia. On perusing the surviving records recently, I was struck by his contribution to my methodology article, in the course of exchanges between us about an article that he was writing on imperfect competition. (Friedman, 1993, p. 770)

Stigler and the polemic against monopolistic competition theory

Stigler’s position was developed in his influential article ‘Monopolistic competition in retrospect’. This text is interesting not only for historians of economic thought, but also for those researchers interested in rhetoric, as Stigler’s consistently sustains the attempt to deny monopolistic competition theory any legitimacy as an economic paradigm, while keeping the tone of an even-handed evaluation. To understand better both the subject and the style of the article, it is helpful to consider its context, which was the ongoing debate between supporters of monopolistic competition theory, first and foremost among them Chamberlin, and supporters of an economics based on the assumption of perfect competition, who were associated to varying degrees with the Chicago School. The last stage of this debate had begun with the publication of Stigler’s The Theory of Price in 1946, which, under the heading ‘imperfect competition’, was restricted mainly to the discussion of cost and demand conditions under monopoly. Monopolistic competition theory proper, focusing on product differentiation and advertising, was given scant space and reduced to an uninteresting, not very important phenomenon due to consumer ignorance. Even this was already a concession in comparison with an earlier version of 1942 called The Theory of Competitive Price that had not mentioned the subject at all.²

¹ The existence of perfect competition is regarded to be a ‘basic sentence’ in a positivist, as opposed to positive, sense, i.e., if empirical testing will reject a prediction formulated on the basis of this assumption, the contradiction will lie with reality, not with its representation in the model.

² The third edition published in 1966 again omits the chapter on ‘imperfect competition’ in favour of a lengthier treatment of oligopoly.
Chamberlin's review had set the tone for subsequent contributions: 'Our criticisms have passed from matters of pure organization to a questioning of how far the author has really digested his new subject' (Chamberlin, 1947, p. 415).

The persuasive power of 'Monopolistic competition in retrospect' itself relied on a mixture of logical argument, appeal to an emerging consensus about the purpose of economic theory and an impressive series of rhetorical devices. Its substantive arguments are not very tightly knit and constitute in the end an expression of preference rather than a logical conclusion. The article, nevertheless, deserves close analysis because of its position at the centre of the debate about monopolistic competition theory, as well as for presenting *in nuce* the original formulation of a falsificationist methodology for economics. It rang the death knell for monopolistic competition theory, and developed the fundamental argumentative basis leading up to the 'The methodology of positive economics'. The key result of this article was its direct and indirect influence on the methodological self-perception of the economics profession.

The title, 'Monopolistic competition in retrospect', itself demonstrates the rhetorical character of the argument against monopolistic competition theory. The formulation 'in retrospect' implies that the subject under consideration constitutes an historically closed episode on which a final verdict can be formulated, rather than a paradigm of research that has to be evaluated on equal grounds with alternatives. The title reveals also a second interesting rhetorical figure: the phenomenon studied is substituted for the conceptual framework, i.e., 'monopolistic competition' for 'monopolistic competition theory'. This implies that the empirical phenomenon of monopolistic competition itself can be considered a closed chapter. This would not only make the approach itself obsolete, but it would also free any alternative approaches from the need to come to grips with the problems and questions posed by the empirical phenomenon of monopolistic competition.

In linguistic theory this identification of conceptual with empirical reality, of 'reference with phenomenalism', is considered to be the constituent element of an ideological argument in which aspects of reality that contradict the original statement are defined away. In this case, the use of this argumentative figure preempts discussion of the principal impetus for research in monopolistic competition theory, i.e., of the 'need to turn toward monopoly' that Piero Sraffa had identified (Sraffa, 1926, p. 542). This 'need to turn toward monopoly' was the result of the decades-long struggle of the economics profession with the concept of the Marshallian industry and its final rejection owing to the logical incompatibility of *empirically* observed increasing returns at the level of the individual firm and the concept of perfect competition.

A similar rhetorical figure is employed by Stigler when he interchanges the merits and demerits of the contributors with those of their contributions. In all but one case, summary judgements substitute for a discussion of the theory. Meticulous attention to rank and position establishes firm hierarchies of scientific authority, i.e., the respective academic ranks of Professor Chamberlin, Dr Triffin and Mrs Robinson serve to determine the amount of space accorded to the discussion of their work. The author

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1 In the same vein, Stigler reports the following personal meeting with Chamberlin in his memoirs: '[W]hen I was a professor at Columbia University, I attended a meeting of the American Economic Association in Washington, D.C., and on the flight back to New York to my surprise I found myself sitting next to Edward Chamberlin. He opened the conversation, “You and Professor Knight are the two most mistaken economists I know on the subject of monopolistic competition.” Thank heaven it was a short trip' (Stigler, 1988, p. 58).

2 Compare to de Man, 1986, pp. 10–12.
establishes himself in a position of authority by use of the oratorical *pluralis majestatis* and casts himself in the role of a severe, but benevolent, omniscient judge. Mrs Robinson is 'in no sense a revolutionary, although at times her language was rebellious' (Stigler, 1949B, p. 12); Professor Chamberlin's vision of economic life was 'legitimate' (*ibid.*, pp. 13–14); Dr. Triffin was an 'able disciple', and Marshall's (no title) logic was 'tolerable' (*ibid.*, p. 20).

The attitude of even-handedness, seemingly distributing praise and criticism in equal measure, leads to semantically empty phrases such as 'these are points, not of unimportance, but of complete irrelevance...' (*ibid.*, p. 14), which serve no other purpose than to confirm on a rhetorical level the role of the speaker. It is complemented by a technique that denigrates arguments or their proponents under the cloak of jest: for instance, the 'indisputable general contribution of monopolistic competition' is said to be that 'we are now more careful to pay attention to the logical niceties of definitions of industries and commodities' (*ibid.*, p. 24). This is damnation by faint praise. On the historic role of monopolistic competition theory Stigler states: '[N]ew ideas are swallowed up by the existing corpus, which is thereafter a little different. And sometimes a little better' (*ibid.*, p. 24). One notes the 'sometimes'. The rhetorical function of these gibes¹ is to appeal to the sensibility of the reader. The argument is thus judged on the basis of existing prejudices rather than by critical examination. In advance, the author is thus preempting disagreement or criticism which would seem the result of hurt feelings or a lack of sense of humour, rather than an expression of genuine conceptual disagreement.

As far as the discussion of substantive arguments is concerned, Stigler limits himself to setting up Chamberlin's concept of 'group equilibrium' as a straw man. This concept had at the time already been repudiated by Chamberlin himself for its logical inconsistencies.² Its main weakness was the necessary driving assumption of subjective demand curves, which meant that producers would forgo real profit opportunities in equilibrium. Curiously, Stigler refers to the relevant passage in Chamberlin, 1938, without mentioning this inconsistency itself (*ibid.*, p. 17). By concentrating on the unwieldy and unrepresentative construction of the 'group equilibrium', he fails to discuss, with the exception of Joan Robinson's *Economics of Imperfect Competition*, the whole body of British literature on monopolistic competition theory including the work by Shove, Harrod, Kaldor, Kahn and Joan Robinson herself.³ Stigler does not even mention key concepts such as product differentiation, indivisibilities, or barriers to entry, which are at the heart of monopolistic competition theory.

The critique of single concepts of monopolistic competition theory prepares the ground for the discussion of monopolistic competition theory in the wider context of the methodology of economic research. The conceptual basis for 'The methodology of positive economics' is created in these passages. Proceeding from Triffin's *Monopolistic Competition and General Equilibrium Theory* (1941), the most comprehensive representation of monopolistic competition theory, Stigler dismisses Triffin's pragmatic solution for the continuing use of industry data, alongside the assumption of monopolistic competition as 'ad hoc empiricism'.

¹ In literary criticism this figure of speech is called charientismus.
² Stigler here uses a single, unrepresentative example for the theory of monopolistic competition theory. According to Aristotle's rules of rhetoric, this is one of the classic inadmissible tropes of argument (Lanham, 1991, p. 169). Freedman identifies the same figure of speech in two other instances as a technique employed by Stigler to denigrate theories that might threaten his microeconomic convictions (Freedman, 1995; 1996).
³ In many aspects the omitted works were analytically superior to the Economics of Imperfect Competition. See Keppler, 1994, pp. 63–77, for a detailed discussion.
In explicit opposition to Triffin’s argument, the original link between the need for a scientific methodology for economics and the rejection of monopolistic competition theory is formulated. In words almost identical to those used later by Friedman, to whom reference is made in a footnote, he specifies falsificationism as the relevant methodological framework for economic theory:

The purpose of the study of economics is to permit us to make predictions about the behaviour of economic phenomena under specified conditions. The sole test of the usefulness of an economic theory is the concordance between its predictions and the observable course of events. (ibid., p. 23)

George Stigler, while not falling into the trap of the ‘F-twist’, also prepares the ground for the rejection of an assumption because of its descriptive realism:

Often a theory is criticized or rejected because its assumptions are ‘unrealistic’. Granting for a moment that this charge has meaning, it burdens theory with an additional function that of description. This is a most unreasonable burden to place upon a theory... (ibid., p. 23)

Stigler avoids drawing an explicit conclusion, but the implication is that the assumption under discussion, i.e., monopolistic competition, will not perform well in a test of its predictive power, since it is ‘most unreasonably burdened’ with its own realism. While Stigler hinted at the ‘F-twist’, Friedman would later formulate it as a categorical exclusion of realistic assumptions.

The two main elements of Friedman’s enormously influential methodological treatise on ‘positive economics’ were thus prepared and assembled by Stigler in the context of the debate about monopolistic competition theory. In a book review, also written in 1949, Stigler had also alluded to the role of deductive methods of verification. ‘A Survey of Contemporary Economics’, the review of the eponymous collection of articles edited by Howard Ellis, had equally introduced a distinction between ‘casual observation’, in which ‘a single contrary investigation causes consternation’, and the ‘tedious and difficult work of testing theories by a comparison of their predictions with evidence’ (Stigler, 1949A, p. 104). The article thus foreshadowed Friedman’s use of examples from the natural sciences by arguing for discretion in the interpretation of results and a selective use of falsificationism.

Stigler’s discretionary use of falsificationism, as well as Triffin’s pragmatic solution, which might indeed lead to ‘ad hoc empiricism’, are both identical to the procedure presented by Friedman in the second half of ‘The methodology of positive economics’ in a famous example of the analysis of American cigarette firms. In this example, he suggests that hypotheses of firm behaviour, based once on the assumption of perfect competition, and once on the assumption of monopolistic competition, could be confirmed in both cases depending on the specific question of interest and the qualifications of the final result (Friedman, 1953, pp. 36–7). While not unappealing as a particular kind of ad hoc procedure, both assumptions, of course, would have to be discarded under a strict falsificationist procedure, which raises a wider question. While the criticism of the ability of monopolistic competition theory to adhere to strict falsificationism and of its need to resort to ad hoc empiricism seems persuasive, the implication by default that an alternative theoretical framework in economics, i.e., perfect competition theory, could perform

1 After Stigler has stated the necessity ‘to set forth certain methodological principles’, the footnote reads: ‘The present interpretation of these principles is due to Professor Milton Friedman’ (Stigler, 1949B, p. 23). This confirms again the closeness of the working relationship between the two economists in this field.
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better, is not. The question is, therefore, whether falsificationism can provide, in principle, a useful methodology for economics, independently of the specific merits of the two paradigms.

Falsificationism in economics

The answer to the question of whether falsificationism can provide a relevant methodology for economics, depends essentially on the answer to the related question of the extent to which economic theory captures the phenomena in which it is interested with its own particular instruments and abstractions. That is, to what extent can economically relevant phenomena in the real world be formulated in the language of economic theory?

The link between a model with simplified relations and a complex reality is established in all science with the help of *ceteris paribus* clauses. In principle, there is nothing to prevent the economist also from isolating testable predictions deduced from arbitrary assumptions and making consecutive statements about reality, provided that all parameters that are not being tested can be controlled for. Consequently, the Popperian ideal of formulation of hypotheses and their subsequent rejection or non-rejection through the testing of predictions ought to be relevant for economics.

In economics, however, all parameters of a hypothesis, i.e., those elements of a model that are held constant during the analysis, are necessarily derived from the econometric analysis of past economic data, as the language of the theory does not allow for an independent specification of parameters such as, for instance, taste. This gives rise to the famous *ceteris paribus* clauses that accompany the predictions of future values of economic variables. In his discussion of falsificationism in economics, Piero Barrotta points out that these *ceteris paribus* clauses employed in economics effectively make predictions unfalsifiable, and hence strict Popperian falsificationism cannot provide relevant rules to gain new insights in economics.¹ If a prediction fails, it is not possible to decide whether the hypothesis, i.e., the postulated causal relations, is wrong, or whether the parameters have shifted. It is crucial in this connection that the influence on the structural parameters is exogenous, i.e., that it cannot be expressed in the language of the theory and integrated into the hypothesis under consideration. There is thus a fundamental difference between a *ceteris paribus* clause in economics and the specification of the complete initial conditions of an experiment in physics, where all possible factors influencing the result, such as temperature or air pressure, can be fully specified in the language of the theory.²

There seems to be an implicit acceptance of this problematic in the methodological passages of ‘The methodology of positive economics’, in which Friedman evokes the Heisenberg principle and Gödel’s theorem. The first refers to the fundamental difficulty in particle physics of separating the phenomenon being measured from the process of

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¹ ‘Economists and ordinary people know very well that the level of consumption is determined by psychological and sociological factors...and it is beyond the scope of economics (or, at least, neoclassical economics) to explore this countless number of factors. Actually, parameters in economics conceal precisely this point. As a consequence of that we can always at the same time claim the truth of all possible statements that can be expressed in the language of economic theory and the falsity of the consequent (namely the given prediction). As it is an elementary law of deductive logic that if the consequent is false at least one premise must be false, we are forced to conclude that the link between economic theory and the given prediction is not deductive’ (Barrotta, 1993A, p. 6).

² A similar point, namely that *ceteris paribus* propositions are frequently hopelessly ambiguous, had been made as early as 1938 by Terence W. Hutchison in his book *The Significance and Basic Postulates of Economic Theory*. Hutchison also deserves mentioning for his introduction of Popper’s concepts to English-speaking economists.
measurement, while the second states that, if a theory employs arithmetic, then it cannot
be both consistent and complete with respect to the class of true sentences that can be
written in the notation of the theory (Massey, 1965, p. 1158). Both theorems specify
fundamental limits on the possibilities of human knowledge. Yet the indeterminacy of the
results of economic research begins on a semantic level where 'economic' arguments
are separated from 'non-economic' ones in order to define a discrete discipline. The
difficulties in isolating meaningful abstractions in economics arise long before physicists
or meta-mathematicians need to question the power of their instruments owing to the
conceptual structure of the human apparatus of perception. By assigning the limits of the
project to establish a firm methodology of 'positive economics' based on falsificationism
to a sphere of the general limitations of human knowledge, Friedman glosses over the
issue with a nobody-is-perfect attitude.

On the other hand, while falsificationism cannot be the rigorous methodological
framework it was presented as by the Chicago School, it nevertheless has something to say
to economists, albeit in the form of a virtual ideal of economic procedure, rather than as a
ready-made prescription. Friedman's point that 'doing economics' is learnt by experience
rather than by rote captures this aspect well. In instances in which structural parameters,
i.e., those elements of a model on whose formation economic theory has nothing to say,
remain relatively constant, hypothesis testing through prediction does provide useful
information on the postulated links between economic variables. In other words, so long
as structural parameters can with some confidence be assumed to remain constant,
economic hypotheses can be rejected, or not rejected, and are hence falsifiable. The
application of falsificationist procedures in economics is, however, always limited by the
behaviour of non-economic structural parameters. Once information outside economic
theory suggests that structural parameters might have changed, the model has to be re-
jigged and predictions have to be reformulated on the basis of new parameters. Hence,
any falsificationist procedure in economics is applicable only so long as structural
parameters can be assumed not to change.

Although economics as a research discipline fails to fulfil its ambition of attaining an
'objectivity' equal to that of the natural sciences even at the price of descriptive realism,
this ambition is also its ultimate raison d'être as a separate discipline distinguishable from
history, sociology, psychology or moral philosophy. The methodological debate started by
Friedman and Stigler can thus be read as an expression of their desire to prove the
distinctness of economics as a research discipline. Such a reading can accept falsifi-
cationism as an interesting guiding point for research efforts, while recognising that it is
ultimately impossible to adhere to its demands. Needless to say, the essential openness of
the process of hypothesis formulation, prediction and testing in economics poses
questions for research as interesting as those that would be posed if falsificationism were
fully applicable. To explore these questions, however, would require a greater willingness
to consider more open methodological procedures than that shown by Friedman and
Stigler.

Monopolistic competition theory and the irony of economics

This leaves the question of the role of monopolistic competition theory in the present
context. Although no final judgement on its merits in methodological terms could be
delivered in comparison with the competing assumption of perfect competition, it is no
coincidence that it was chosen as the main subject of attack by Friedman and Stigler.
The genesis of 'positive economics'

Before analysing the reasons for this, it is worth considering first a related question: how could these two articles, with their idiosyncratic mixture of conceptual argument, rhetorical exhortation and pure assertion, have such a resounding impact on the economics profession? Their argument, which stated that only the assumption of perfect competition economics was admissible in rigorous research, was accepted because the audience wanted to believe it. The ground for this willingness had been prepared by several factors.

First and foremost, monopolistic competition was not a very exciting field for theoretical economics at the time. Friedman's correct assessment that it did not provide for an equilibrium beyond that of the single firm had limited its appeal (Friedman, 1953, p. 38). Even the static equilibrium of the single firm was more a possibility than a necessary consequence of economic forces, as evidenced in the discussion between Chamberlin and Kaldor on excess profits and Euler's Law. The precarious nature of the equilibrium conditions of monopolistic competition theory were highly unattractive in terms of the changing preferences of the academic community. It made the theory of monopolistic competition useless for the two most exciting fields of academic economics: the construction of large-scale macroeconomic models and the work on general equilibrium theory emanating from the work of Debreu, Arrow and McKenzie in the early 1950s.

However, neither Friedman nor Stigler elaborated on the missing equilibrium conditions, but chose instead to attack monopolistic competition theory exactly in that area where it continued to be of considerable value, i.e., as a tool to formulate and predict firm behaviour under realistic market conditions. Ironically, methodology was probably the area where Friedman and Stigler, in their own eclectic, pragmatic ways, uninterested in general equilibrium, were closest to monopolistic competition theory itself. Consequently, their methodological arguments are unable to offer much in terms of advancing the profession's knowledge about a sustainable methodology of economics. But helped by the considerable rhetorical skills of the two Chicago economists, their argument that monopolistic competition theory was methodologically inadmissible was accepted because it seemed to provide a justification for abandoning a theory that was no longer attractive for other reasons.

But why did monopolistic competition theory seem worth all the effort? If it was already on the wane, why did Friedman and Stigler expend so much effort on discrediting it? Why was it necessary to attack an approach that, after all, had the charm of consistently predicting an important empirical fact—namely, that the output of an individual firm would rise in response to an increase in demand? The threat posed by monopolistic competition theory did not relate to its predictive performance. It can only be understood against the backdrop of the ambition to develop a practice and reputation for exactness in economics akin to the natural sciences, in order to leave its roots in psychology and history behind once and for all. Monopolistic competition theory questioned the feasibility of this project.

Drawing attention to parameters beyond price and quantity, examining the individual unrepeatable firm, acknowledging the changing tastes and information requirements of consumers, monopolistic competition theory concentrated on the singular, historically contingent case as opposed to the generalising, perfectly competitive model. There is also the strong link between monopolistic competition and economic dynamics in historical time through the concept of increasing returns, brilliantly highlighted by Allyn Young in his article 'Increasing returns and economic progress' (1928).

A dictum of Stigler highlights his disregard for historical contingency: 'A war may
ravage a continent or destroy a generation without posing new theoretical questions' (Sowell, 1987, p. 499). Apart from the familiar rhetorical figure employed by Stigler to insert potentially questionable statements behind a provocation of established sensibilities, the phrase highlights his preference for an historically invariant economic science, i.e., a body of economic theory independent of individual or even collective historical experience.1 Friedman's use of the law of gravity as an analogue to the 'maximization-of-returns hypothesis' makes the same point.2 Both authors are fascinated by the double quality of physical laws of unassailability and concreteness. It is not the logical unassailability of a proof of the existence of general equilibrium that fascinates them, but rather the presumed empirical unassailability of the existence of infinitely repeatable human actions, explicable exclusively by economic utility maximisation in markets that are perfect in all but their most superficial appearance.

Monopolistic competition theory in contrast highlighted 'the inability of economic theory to fulfill its desire of totality' (Khan, 1992, p. 787) and thus the inherent irony of economics as a research enterprise. It does so by introducing 'non-economic' arguments of inertia, fashion, taste, geography, brand names, information, transaction costs or just 'friction' in general, and it does so also by introducing historical time.3 It maintains an essential openness of argument by refusing to establish conditions of static equilibrium. New entry or exit might depend on changes in design, the qualities of an individual entrepreneur or on the slow transformation of dexterity and experience into increasing returns. Monopolistic competition theory pointed towards a 'break of the intra-textual closure of figures and representations' (ibid., p. 788), i.e., its inability to formulate all relevant facts in its own language. This was understood by Friedman and Stigler and yet they were unwilling to accept it. As pragmatists trying to be 'reasonably realistic', they were carrying their own share of the pathos of irony, yet the myth of a closed, self-referential and self-sufficient economic science had to be preserved even at the price of logical consistency. Methodology was crucial and yet its clear exposition revealed the brittleness of the totalising project of insisting on an economic science based on the assumption of perfect competition.

Monopolistic competition theory embodied at the time, as it does today, the inherent tension between the scientific consistency and the empirical realism of economics. The decision to reject monopolistic competition, for this very reason, resulted in the postulation of a procedural framework that was a myth rather than a model. It burdened economics with methodological prescriptions that were eagerly embraced for the scientific rigour they promised, but that proved to be unfulfillable at a practical level. To establish new and acceptable results in economics is still as much an art as it is a science; it is still learned by experience, not by rote. The ambition to reconcile strict causality with the richness of human behaviour is the crux of economics. But it also offers a challenge

1 Compare in this connection the comments by Nathan Rosenberg on Stigler's approach to the history of economic thought that concentrated on the extraction of a 'core theoretical system' independent of the individual author (Rosenberg, 1993, p. 835).

2 Employing the formula \( s = \sqrt{2gt^2} \) as a proxy for the law of gravity, Friedman writes: 'The formula is accepted because it works, not because we live in an approximate vacuum' (1953, p. 18). He overlooks that predictions of the hypothesis of the law of gravity were precisely tested through the observation of bodies that do move in a perfect vacuum, i.e., the planets. Unwittingly, his ill-chosen example highlights the problematic nature of the transfer of methodological rules from a natural science to a social science.

3 One of the greatest ironies in this context is provided by Stigler himself. He was later to win the Nobel Prize for Economics for having introduced the 'cost of acquiring new information' as a major new theme in microeconomics. Thus, in the later stages of his career he would provide a firm conceptual basis for monopolistic competition theory.
that goes a long way towards explaining its ongoing fascination as a discipline of research. And the fascination with this double nature of economics is equally strong for those who acknowledge it, as it is for those who deny it.

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