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Kuhn versus Lakatos, or paradigms versus research programmes in the history of economics

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In the 1950's and 1960's economists learned their methodology from Popper. Not that many of them read Popper. Instead, they read Friedman, and perhaps few of them realized that Friedman is simply Popper-with-a-twist applied to economics. To be sure, Friedman was criticized, but the "Essay on the Methodology of Positive Economics" nevertheless survived to become the one article on methodology that virtually every economist has read at some stage in his career. The idea that unrealistic "assumptions" are nothing to worry about, provided that the theory deduced from them culminates in falsifiable predictions, carried conviction to economists long inclined by habit and tradition to take a purely instrumentalist view of their subject.

All that is almost ancient history, however. The new wave is not Popper's "falsifiability" but Kuhn's "paradigms." Again, it is unlikely that many economists read *The Structure of Scientific Revolutions* (1962). Nevertheless, appeal to paradigmatic reasoning quickly became a regular feature of controversies in economics and "paradigm" is now the byword of every historian of economic thought.[1] Recently, however, some commentators have expressed misgivings about Kuhnian methodology applied to economics, throwing doubt in particular on the view that "scientific revolutions" characterize the history of economic thought.[2] With these doubts I heartily concur. I will argue that the term "paradigm" ought to be banished from economic literature, unless surrounded by inverted commas. Suitably qualified, however, the term retains a function in the historical exposition of economic doctrines as a reminder of the

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1. Similarly, sociologists have seized avidly on the Kuhnian apparatus: See, e.g., Ryan 1970, pp. 233–36, Martins 1972, and the collection of essays in Whitley 1974.
2. See Coats 1969, Bronfenbrenner 1971, and Kunin and Weaver 1971.

fallacy of trying to appraise particular theories without invoking the wider metaphysical framework in which they are embedded. This notion that theories come to us, not one at a time, but linked together in a more or less integrated network of ideas, is however better conveyed by Lakatos' "methodology of scientific research programmes." The main aim of my article is indeed to explore Lakatos' ideas in application to the history of economics.[3]

The task is not an easy one. Lakatos is a difficult author to pin down. His tendency to make vital points in footnotes, to proliferate labels for different intellectual positions, and to refer back and forth to his own writings—as if it were impossible to understand any part of them without understanding the whole—stands in the way of ready comprehension. In a series of papers, largely published between 1968 and 1971, Lakatos developed and extended Popper's philosophy of science into a critical tool of historical research, virtually resolving a long-standing puzzle about the relationship between positive history of science and normative methodology for scientists. The puzzle is this. To believe that it is possible to write a history of science "wie es eigentlich gewesen" without in any way revealing our concept of sound scientific practice or how "good" science differs from "bad" is to commit the Inductive Fallacy in the field of intellectual history; by telling the story of past developments one way rather than another we necessarily disclose our view of the nature of scientific explanation. On the other hand, to preach the virtues of *the* scientific method while utterly ignoring the question of whether scientists now or in the past have actually practiced that method seems arbitrary and metaphysical. We are thus caught in a vicious circle, implying the impossibility both of a value-free, descriptive historiography of science and an ahistorical, prescriptive methodology of science.[4] From this vicious circle there is, I believe, no real escape, but what Lakatos has done is to hold out the hope that the circle may be eventually converted into a virtuous one.

Enough said by way of introduction. Let us look briefly at Popper and Kuhn, before putting Lakatos' "methodology of scientific research programmes" to work in a field such as economics.

3. I dedicate this paper to the memory of Imre Lakatos, Professor of Logic and the Philosophy of Science at the London School of Economics, who died suddenly at the age of fifty-one on February 2, 1974. We discussed an early draft of this paper a number of times in the winter of 1973 and, for the last time, the day before his death. He promised me a rebuttal, which now alas I will never read.

4. One of Lakatos' fundamental papers (1971, p. 91) opens with a paraphrase of one of Kant's dictums, which perfectly expresses the dilemma in question: "Philosophy of science without history of science is empty: history of science without philosophy of science is blind."

1. *From Popper to Kuhn to Lakatos*

Popper's principal problem in *The Logic of Scientific Discovery* (1935) was to find a purely logical demarcation rule for distinguishing science from nonscience. He repudiated the Vienna Circle's principle of verifiability and replaced it by the principle of falsifiability as the universal a priori test of a genuinely scientific hypothesis. The shift of emphasis from verification to falsification is not as innocent as appears at first glance, involving as it does a fundamental asymmetry between proof and disproof. From this modest starting point, Popper has gradually evolved over the years a powerful anti-inductionist view of science as an endless dialectical sequence of "conjectures and refutations."[5]

A hasty reading of *The Logic of Scientific Discovery* suggests the view that a single refutation is sufficient to overthrow a scientific theory; in other words, it convicts Popper of what Lakatos has called "naive falsificationism" (Lakatos and Musgrave 1970, pp. 116, 181; Lakatos 1971, pp. 109–14). But a moment's reflection reminds us that many physical and virtually all social phenomena are stochastic in nature, in which case an adverse result implies the improbability of the hypothesis being true, not the certainty that it is false. To discard a theory after a single failure to pass a statistical test would, therefore, amount to intellectual nihilism. Patently, nothing less than a whole series of refutations is likely to discourage the adherents of a probabilistic theory. A careful reading of Popper's work, however, reveals that he was perfectly aware of the so-called "principle of tenacity"—the tendency of scientists to evade falsification of their theories by the introduction of suitable ad hoc auxiliary hypotheses—and he even recognized the functional value of such dogmatic stratagems in certain circumstances.[6] Popper, in other words, is a "sophisticated falsificationist," not a "naive" one.[7]

5. Not to mention his formulation of a political philosophy, generated by the same conception. For a splendid, if somewhat hagiographic, introduction to the wide sweep of Popper's work, see Magee 1973.

6. For example: "In point of fact, no conclusive disproof of a theory can ever be produced; for it is always possible to say that the experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding" (Popper 1965, p. 50; see also pp. 42, 82–83, 108); in the same spirit, see Popper 1962, II, 217–20, Popper 1972, p. 30, and Popper in Schilpp 1974, I, 82.

7. Economists will recognize immediately that Lipsey really was a "naive falsificationist" in the first edition of his *Introduction to Positive Economics* and only adopted "sophisticated falsificationism" in the third edition of the book: see Lipsey 1966, pp. xx, 16–17.

In general, however, Popper deplores the tendency to immunize theories against criticism and instead advocates a bold commitment to falsifiable predictions, coupled with a willingness and indeed eagerness to abandon theories that have failed to survive efforts to refute them. His methodology is thus plainly a normative one, prescribing sound practice in science, possibly but not necessarily in the light of the best science of the past; it is an "aggressive" rather than a "defensive" methodology because it cannot be refuted by showing that most, and indeed even all, scientists have failed to obey its precepts.[8]

In Kuhn's *Structure of Scientific Revolutions*, the emphasis shifts from normative methodology to positive history: the "principle of tenacity," which for Popper presents something of an exception to best-practice science, becomes the central issue in Kuhn's explanation of scientific behavior. "Normal science," or problem-solving activity in the context of an accepted theoretical framework, is said to be the rule, and "revolutionary science," or the overthrow of one "paradigm" by another in consequence of repeated refutations and mounting anomalies, the exception in the history of science. It is tempting to say that for Popper science is always in a state of "permanent revolution," the history of science being the history of continuous "conjectures and refutations"; for Kuhn, the history of science is marked by long periods of steady refinement, interrupted on occasions by *discontinuous* jumps from one ruling "paradigm" to another with no bridge for communicating between them.[9]

To judge a dispute such as this, we must begin by defining terms. In the first edition of his book, Kuhn frequently employed the term "paradigm" in a dictionary sense to stand for certain exemplary instances of scientific achievement in the past. But he also employed the term in quite a different sense to denote both the choice of problems and the set of techniques for analyzing them, in places going so far as to give "paradigm" a still wider meaning as a general metaphysical *Weltanschauung*; the last sense of the term is, in fact, what most readers take away from the book. The second edition of *The Structure of Scientific Revolutions* (1970) admitted to ter-

8. I owe the vital distinction between "aggressive methodologies" and "defensive methodologies" to Latsis (1974). Popper does make references to the history of science, and clearly Einstein is his model of a great scientist. Nevertheless, he is always insistent on the metaphysical and hence irrefutable basis of the falsifiability principle (see, e.g., Schilpp 1974, II, 1036–37).

9. See the revealing criticism of Popper by Kuhn and the equally revealing criticism of Kuhn by Popper (Lakatos and Musgrave 1970, pp. 14–15, 19, 52–55).

minological imprecision in the earlier version[10] and suggested that the term "paradigm" be replaced by "disciplinary matrix"; " 'disciplinary' because it refers to the common possession of the practitioners of a particular discipline; 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification" (Kuhn 1970, p. 182). But whatever language is employed, the focus of his argument remained that of "the entire constellation of beliefs, values, techniques and so on shared by the members of a given community," and he went on to say that if he were to write his book again, he would start with a discussion of the professionalization of science before examining the shared "paradigms" or "disciplinary matrices" of scientists (p. 173).

These are not fatal concessions for the simple reason that the distinctive feature of Kuhn's methodology is not the concept of paradigms that everyone has seized on, but rather that of "scientific revolutions" as sharp breaks in the development of science, and particularly the notion of a pervasive failure of communications during periods of "revolutionary crises." Let us remind ourselves of the building bricks of Kuhn's argument: the practitioners of "normal science," although widely scattered, form an "invisible college" in the sense that they are in agreement both on the "puzzles" that require solution and on the general form that the solution will take; moreover, only the judgment of colleagues is regarded as relevant in defining problems and solutions, in consequence of which "normal science" is a self-sustaining, cumulative process of puzzle solving within the context of a common analytical framework; the breakdown of "normal science" is heralded by a proliferation of theories and the appearance of methodological controversy; the new framework offers a decisive solution to hitherto neglected "puzzles" and this solution turns out in retrospect to have long been recognized but previously ignored; the old and new generations talk past each other as "puzzles" in the old framework become "counterexamples" in the new; conversion to the new approach takes on the nature of a religious experience, involving a "gestalt switch"; and the new framework conquers in a few decades, to become in turn the "normal science" of the next generation.

The reader who is acquainted with the history of science thinks immediately of the Copernican Revolution, the Newtonian Revolution, or the Einstein-Planck Revolution. The so-called Copernican

10. Masterman (Lakatos and Musgrave 1970, pp. 60–65) has in fact identified twenty-one different definitions of the term "paradigm" in Kuhn's 1962 book.

Revolution, however, took a hundred and fifty years to complete and was argued out every step of the way; even the Newtonian Revolution took more than a generation to win acceptance throughout the scientific circles of Europe, during which time the Cartesians, Leibnizians, and Newtonians engaged in bitter disputes over every aspect of the new theory; likewise, the switch in the twentieth century from classical to relativistic and quantum physics involved neither mutual incomprehension nor quasi-religious conversions, at least if the scientists directly involved in the "crisis of modern physics" are to be believed.[11] It is hardly necessary, however, to argue these points, because in the second edition of his book Kuhn candidly admits that his earlier description of "scientific revolutions" suffered from rhetorical exaggeration: paradigm changes during "scientific revolutions" do not imply absolute discontinuities in scientific debate, that is, a choice between competing but totally incommensurate theories; mutual incomprehension between scientists during a period of intellectual crisis is only a matter of degree; and the only point of calling paradigm changes "revolutions" is to underline the fact that the arguments that are advanced to support a new paradigm always contain ideological elements that go beyond logical or mathematical proof (Kuhn 1970, pp. 199–200).[12] As if this were not enough, he goes on to complain that his theory of "scientific revolutions" was misunderstood as referring solely to major revolutions, such as the Copernican, Newtonian, Darwinian, or Einsteinian; he now insists that the schema was just as much directed at minor changes in particular scientific fields, which might not seem to be revolutionary at all to those outside "a single community [of scientists], consisting perhaps of fewer than twenty-five people directly involved in it" (pp. 180–81).

In short, in this later version of Kuhn, any period of scientific development is marked by a large number of overlapping and interpenetrating "paradigms"; some of these may be incommensurable but certainly not all of them are; "paradigms" do not replace each

11. Toulmin 1972, pp. 103–5. Of all the many critiques that Kuhn's book has received (Lakatos and Musgrave 1970, and references cited by Kunin and Weaver 1971), none is more devastating than that of Toulmin (1972, pp. 98–117), who traces the history of Kuhn's methodology from its first announcement in 1961 to its final version in 1970. For an extraordinarily sympathetic but equally critical reading of Kuhn, see Suppe 1974, pp. 135–51.

12. This is almost obvious because if two "paradigms" were truly incommensurable, they could be held simultaneously, in which case there would be no need for a "scientific revolution": the strong incommensurability thesis is logically self-contradictory (Achinstein 1968, pp. 91–106). What Kuhn must have meant is "incommensurability to some degree," and the new version is simply a belated attempt to specify the degree in question.

other immediately and, in any case, new "paradigms" do not spring up full-blown but instead emerge as victorious in a long process of intellectual competition. It is evident that these concessions considerably dilute the apparently dramatic import of Kuhn's original message, and in this final version the argument is difficult to distinguish from the average historian's account of the history of science. What remains, I suppose, is the emphasis on the role of values in scientific judgments, particularly in respect of the choice between competing approaches to science, together with a vaguely formulated but deeply held suspicion of cognitive factors like epistemological rationality, rather than sociological factors like authority, hierarchy, and reference groups, as determinants of scientific behavior. What Kuhn has really done is to conflate prescription and description, deducing his methodology from history, rather than to criticize history with the aid of a methodology. Kuhn does his best, of course, to defend himself against the charge of relativism and to explain "the sense in which I am a convinced believer in scientific progress" (Kuhn 1970, pp. 205–7), but the defense is not altogether convincing. Actually, a wholly convincing defense would reduce his account of "scientific revolutions" to a nonsense.

Which brings us to Lakatos.[13] As I read him, Lakatos is as much appalled by Kuhn's lapses into relativism as he is by Popper's ahistorical if not antihistorical standpoint.[14] The result is a compromise between the "aggressive methodology" of Popper and the "defensive methodology" of Kuhn, but a compromise which stays within the Popperian camp;[15] Lakatos is "softer" on science than Popper, but a great deal "harder" than Kuhn, and he is more inclined to criticize bad science with the aid of good methodology than temper methodological speculations by an appeal to scientific practice. For Lakatos, as for Popper, methodology has nothing to do with laying down standard procedures for tackling scientific problems; it is con-

13. My sketch of recent developments in the philosophy of science omits discussion of such influential writers as Feyerabend, Hanson, Polanyi, and Toulmin, who have each in his own way challenged the traditional positivist account of the structure of scientific theories. But see Suppe (1974), whose masterful essay of book length covers all the names mentioned above. Lakatos, however, is deliberately omitted in Suppe's account (Suppe 1974, p. 166 n.).

14. See the characteristic reaction of Popper to Kuhn: "to me the idea of turning for enlightenment concerning the aims of science, and its possible progress, to sociology or to psychology (or . . . to the history of science) is surprising and disappointing" (Lakatos and Musgrave 1970, p. 57).

15. Bloor (1971, p. 104) seems wide of the mark in characterizing Lakatos' work as "a massive act of revision, amounting to a betrayal of the essentials of the Popperian approach, and a wholesale absorption of some of the most characteristic Kuhnian positions."

cerned with the "logic of appraisal," that is, the normative problem of providing criteria of scientific progress. Where Lakatos differs from Popper is that this "logic of appraisal" is then employed at one and the same time as a historical theory which purports to retrodict the development of science. As a normative methodology of science, it is empirically irrefutable because it is a definition. But as a historical theory, implying that scientists in the past did in fact behave in accordance with the methodology of falsifiability, it is perfectly refutable. If history fits the normative methodology, we have reasons additional to logical ones for subscribing to fallibilism. If it fails to do so, we are furnished with possible reasons for abandoning our methodology. No doubt, Hume's Guillotine tells us that we cannot logically deduce ought from is or is from ought. We can, however, influence ought by is and vice versa: moral judgments may be altered by the presentation of facts, and facts are theory-laden so that a change of values may alter our perception of the facts. But all these problems lie in the future. The first task is to reexamine the history of science with the aid of an explicit falsificationist methodology to see if indeed there is any conflict to resolve.

Lakatos begins by denying that isolated individual theories are the appropriate units of appraisal; what ought to be appraised are clusters of interconnected theories or "scientific research programmes" (SRP). Duhem and Poincaré had argued long ago that no individual scientific hypothesis is conclusively verifiable or falsifiable, because we always test the particular hypothesis in conjunction with auxiliary statements and therefore can never be sure whether we have confirmed or refuted the hypothesis itself. Since any hypothesis, if supplemented with suitable auxiliary assumptions, can be maintained in the face of contrary evidence, its acceptance is merely conventional. Popper met this "conventionalist" argument by distinguishing between "*ad-hoc*" and "*non-ad-hoc*" auxiliary assumptions: it is perfectly permissible to rescue a falsified theory by means of a change in one of its auxiliary assumptions, if such a change increases the empirical content of the theory by augmenting the number of its observational consequences; it is only changes which fail to do this that Popper dismissed as "*ad-hoc*".[16] Lakatos generalizes this Popperian argument by distinguishing between "progressive and degenerating problem shifts." A particular re-

16. Although Popper's distinction succeeds in refuting "conventionalism," it tends to erode the fundamental asymmetry between verification and falsification which is the linchpin of his philosophy of science: see Grünbaum 1973, pp. 569–629, 848–49. Archibald 1967 illustrates the problem of distinguishing ad hoc auxiliary assumptions in testing the Keynesian theory of income determination.

search strategy or SRP is said to be "*theoretically* progressive" if a successive formulation of the programme contains "excess empirical content" over its predecessor, "that is, . . . predicts some novel, hitherto unexpected fact"; it is "*empirically* progressive if this excess empirical content is corroborated" (Lakatos and Musgrave 1970, p. 118). Contrariwise, if the programme is characterized by the endless addition of ad hoc adjustments that merely accommodate whatever new facts become available, it is labeled "degenerating."

These are relative, not absolute distinctions. Moreover, they are applicable, not at a given point in time, but over a period of time. The forward-looking character of a research strategy, as distinct from a theory, defies instant appraisal.[17] For Lakatos, therefore, an SRP is not "scientific" once and for all; it may cease to be scientific as time passes, slipping from the status of being "progressive" to that of being "degenerating" (astrology is an example), but the reverse may also happen (parapsychology?). We thus have a demarcation rule between science and nonscience which is itself historical, involving the evolution of ideas over time as one of its necessary elements.

The argument is now extended by dividing the components of an SRP into rigid parts and flexible parts. "The history of science," Lakatos observes, "is the history of research programmes rather than of theories," and "all scientific research programmes may be characterized by their 'hard core,' surrounded by a protective belt of auxiliary hypotheses which has to bear the brunt of tests." The "hard core" is irrefutable by "the methodological decision of its protagonists"—shades of Kuhn's "paradigm"!—and it contains, besides purely metaphysical beliefs, a "positive heuristic" consisting of "a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research-programme, how to modify, sophisticate, the 'refutable' protective belt" (Lakatos and Musgrave 1970, pp. 132–35).[18] The "protective belt," how-

17. If the term "scientific research programmes" strikes some readers as vague, it must be remembered that the term "theory" is just as vague. It is in fact difficult to define "theory" precisely, even when the term is employed in a narrow sense: see Achinstein 1968, chap. 4 .

18. Lakatos' "hard core" expresses an idea similar to that conveyed by Schumpeter's notion of "Vision"—"the preanalytic cognitive act that supplies the raw material for the analytic effort" (Schumpeter 1954, pp. 41–43)—or Gouldner's "world hypotheses," which figure heavily in his explanation of why sociologists adopt certain theories and reject others (Gouldner 1971, chap. 2). Marx's theory of "ideology" may be read as a particular theory about the nature of the "hard core"; Marx was quite right in believing that "ideology" plays a role in scientific theorizing but he was quite wrong in thinking that the class character of ideology was decisive for the acceptance or rejection of scientific theories.

ever, contains the flexible parts of an SRP, and it is here that the "hard core" is combined with auxiliary assumptions to form the specific testable theories with which the SRP earns its scientific reputation.

If the concept of SRP is faintly reminiscent of Kuhn's "paradigms," the fact is that Lakatos' picture of scientific activity is much richer than Kuhn's. Furthermore, it begins to provide insight as to why "paradigms" are ever replaced, a mystery which is one of the central weaknesses of Kuhn's work. "Can there be any objective (as opposed to socio-psychological) reason to reject a programme, that is, to eliminate its hard core and its programme for constructing protective belts?" Lakatos asks. His answer, in outline, is that "such an objective reason is provided by a rival research programme which explains the previous success of its rival and supersedes it by a further display of heuristic power" (Lakatos and Musgrave 1970, p. 155; also Lakatos 1971, pp. 104–5). He illustrates the argument by analyzing Newton's gravitational theory—"probably the most successful research programme ever"—and then traces the tendency of physicists after 1905 to join the camp of relativity theory, which subsumed Newton's theory as a special case.[19] The claim is that this move from one SRP to another was "objective," because most scientists acted as if they believed in the normative "methodology of scientific research programmes" (MSRP). Lakatos goes on to advance the startling claim that all history of science can be similarly described; he defines any attempt to do so as "internal history" (Lakatos 1971, pp. 91–92).[20] "External history," in contrast, is not just all the normal pressures of the social and political environment that we usually associate with the word "external," but any failure of scientists to act according to MSRP, as, for example, preferring a degenerating SRP to a progressive SRP on the grounds that the former is more "elegant" than the latter, possibly accompanied by the denial that it is degenerating.[21]

19. However, he is not committed to the belief that every progressive SRP will be more general than the degenerate SRP which it replaces. There may well be a Kuhnian "loss of content" in the process of passing from one SRP to another, although typically the overlap between rival programmes will be larger than either the content-loss or content-gain.

20. This is what Suppe (1974, pp. 53–56) has called the "thesis of development by reduction," namely, that scientific progress comes largely, and even exclusively, by the succession of more comprehensive theories which include earlier theories as special cases. The thesis, even in its weaker version, has been hotly debated by philosophers of science for many years.

21. Lakatos holds that one cannot rationally criticize a scientist who sticks to a degenerating programme if, recognizing it is degenerating, he is determined to resuscitate it. This is somewhat contradictory. Feyerabend (1975, pp. 185–86) seizes on this

The claim that all history of science can be depicted as "internal" may of course be difficult to sustain in the light of historical evidence, but Lakatos recommends that we give priority to "internal history" before resorting to "external history." Alternatively, what we can do is "to relate the internal history *in the text*, and indicate in the footnotes how actual history 'misbehaved' in the light of its rational reconstruction" (Lakatos 1971, p. 107), advice which Lakatos himself followed in his famous Platonic dialogue on the history of Euler's Conjecture on Polyhedrons (Lakatos 1964).

In reply to Lakatos, Kuhn minimized the differences between them: "Though his terminology is different, his analytic apparatus is as close to mine as need be: hard core, work in the protective belt, and degenerating phase are close parallels for my paradigms, normal science, and crisis (Lakatos and Musgrave 1971, p. 256). Kuhn insisted, however, that "what Lakatos conceives as history is not history at all but philosophy fabricating examples. Done in that way, history could not in principle have the slightest effect on the prior philosophical position which exclusively shaped it" (Kuhn 1971, p. 143). This seems to ignore Lakatos' deliberate attempt to keep history as such separate from "philosophy fabricating examples" and provides no resolution of the dilemma which surrounds the historiography of science: either we infer our scientific methodology from the history of science, which commits the fallacy of induction, or we preach our methodology and rewrite history accordingly, which smacks of "false consciousness."[22]

Lakatos, replying to Kuhn, tries to score a logical victory for his own approach to the historiography of science by claiming that it is perfectly capable of postdicting novel historical facts, unexpected in the light of the extant approaches of historians of science. In that sense, the "methodology of historiographical research programmes" may be vindicated by MSRP itself: it will prove "progressive" if and only if it leads to the discovery of novel historical facts (Lakatos 1971, pp. 116–20). The proof of the pudding is therefore in the eating. It remains to be seen whether the history of a science, whether natural or social, is more fruitfully conceived, not as steady progress punctured every few hundred years by a scientific revolution, but as a succession of progressive research programmes constantly super-

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weakness and others in a penetrating but sympathetic critique of Lakatos from the standpoint of epistemological anarchism (ibid., chap. 16, pp. 181–220).

22. The dilemma in question is widely recognized by philosophers of science; as well as historians of science: see, e.g., Lakatos and Musgrave 1970, pp. 46, 50, 198, 233, 236–38; Achinstein's comments on Suppe (Suppe, 1974, pp. 350–61); and Hesse's essay in Teich and Young 1973.

seding one another with theories of ever-increasing empirical content.[23]

2. *Scientific revolutions in economics*

Both Kuhn and Lakatos jeer at modern psychology and sociology as pre-paradigmatic, proto-sciences, and although economics seems to be exempted from the charge, Lakatos seems to think that even economists have never seriously committed themselves to the principle of falsifiability: "The reluctance of economists and other social scientists to accept Popper's methodology may have been partly due to the destructive effect of naive falsificationism on budding research programmes" (Lakatos and Musgrave 1970, p. 179 n). It is perfectly true that a dogmatic application of Popper to economics would leave virtually nothing standing, but it is a historical travesty to assert that economists have been hostile to Popper's methodology, at least in its more sophisticated versions. What is the central message of Friedman's "as-if" methodology if not commitment to the idea of testable predictions? And indeed, the pronouncements of nineteenth-century economists on methodology, summed up in John Neville Keynes' magisterial treatise *The Scope and Method of Political Economy* (1891), are squarely in the same tradition even if the language is that of verification rather than falsification plus or minus a naive Baconian appeal to "realistic" assumptions. The real question is whether the "principle of tenacity" does not figure much more heavily in the history of economics than in the history of, say, physics.[24] Analytical elegance, economy of theoretical means, and generality obtained by ever more "heroic" assumptions have always meant more to economists than relevance and predictability. They have in fact rarely practiced the methodology to which they have explicitly subscribed, and that, it seems to me, is one of the ne-

23. Contrast Kuhn 1957 and Lakatos and Zahar 1975 on the so-called Copernican Revolution. See also Zahar 1973 and Feyerabend 1974 on the Einsteinian Revolution and Urbach 1974 on the IQ debate. Several other case studies applying Lakatos' MSRP to the history of physics, chemistry, and economics, presented at the Nafplion Colloquium on Research Programmes in Physics and Economics, September 1974, will be published in 1975. For the only published application to economics, see Latsis 1972, discussed below.

24. "It may be said without qualification," Keynes wrote in *Scope and Method,* "that political economy, whether having recourse to the deductive method or not, must begin with observation and end with observation . . . the economist has recourse to observation in order to illustrate, test, and confirm his deductive inferences" (Keynes 1955, pp. 227, 232). But it is characteristic that most of chapters 6 and 7, from which these sentences are drawn, is about the difficulties of verifying deductive inferences by empirical observations; we are never told when we may reject an economic theory in the light of the evidence or indeed whether any economic theory was ever so rejected.

glected keys to the history of economics. The philosophy of science of economists, ever since the days of Senior and Mill, is aptly described as "innocuous falsificationism".[25]

Let us begin by reviewing the attempts to apply Kuhn's methodology to economics. What are the ruling "paradigms" in the history of economic thought? According to Gordon, "Smith's postulate of the maximizing individual in a relatively free market . . . is our basic paradigm"; "economics has never had a major revolution; its basic maximizing model has never been replaced . . . it is, I think, remarkable when compared to the physical sciences that an economist's fundamental way of viewing the world has remained unchanged since the eighteenth century" (Gordon 1965, pp. 123, 124). Likewise, Coats asserts that economics has been "dominated throughout its history by a single paradigm—the theory of economic equilibrium via the market mechanism," but, unlike Gordon, Coats singles out the so-called Keynesian Revolution as a paradigm change, a Kuhnian "scientific revolution," and subsequently he has claimed almost as much for the so-called Marginal Revolution of the 1870's (Coats 1969, pp. 292, 293; Black, Coats, and Goodwin 1973, p. 38; but see p. 337). Benjamin Ward, a firm believer in Kuhn's methodology, also dubs the Keynesian Revolution a Kuhnian one, and furthermore he claims that the recent postwar period has witnessed a "formalist revolution" involving the growing prestige of mathematical economics and econometrics, which leaves him wondering why such a radical change should have made so little substantive difference to the nature of economics (Ward 1972, pp. 34–48). Lastly, Bronfenbrenner, after defining a "paradigm" as "a mode or framework of thought and language," goes on to cite Keynesian macroeconomics, the emergence of radical political economy, the recent revival of the quantity theory of money, and the substitution of the Hicksian IS-LM cross for the Marshallian demand-and-supply cross as cases in point, a procedure which falls into the trap set by Kuhn himself (Bronfenbrenner 1971, pp. 137–38). Bronfenbrenner identifies three revolutions in the history of economic thought: "a laissez-faire revolution," dating from Hume's *Political Discourses* in 1752; the Marginal Revolution of the 1870's as a "second possible revolution"; and the Keynesian Revolution of 1936.

If we had not previously recognized the inherent ambiguities in Kuhn's concepts, this brief review would suffice to make the point. Be that as it may, it appears that if economics provides any examples at all of Kuhnian "scientific revolutions," the favorite example

25. I owe this happy phrase to an unpublished paper by A. Coddington.

seems to be the Keynesian Revolution, which at any rate has all the superficial appearance of a paradigm change. It is perfectly obvious, however, that the age-old paradigm of "economic equilibrium via the market mechanism," which Keynes is supposed to have supplanted, is actually a network of interconnected subparadigms; in short, it is best regarded as a Lakatosian SRP. It is made up, first of all, of the principle of constrained maximization, "Smith's postulate of the maximizing individual in a relatively free market," or what Friedman calls for short the "maximization-of-returns hypothesis." The principle of maximizing behavior subject to constraints is then joined to the notion of general equilibrium in self-regulating competitive markets to produce the method of comparative statics, which is the economist's principal device for generating qualitative predictions of the signs rather than the magnitudes of his critical variables. The "hard core" or metaphysical part of this programme consists of weak versions of what is otherwise known as the "assumptions" of competitive theory, namely, rational economic calculations, constant tastes, independence of decision making, perfect knowledge, perfect certainty, perfect mobility of factors, etcetera. If they are not stated weakly, they become refutable by casual inspection and cannot, therefore, be held as true a priori. The "positive heuristic" of the programme consists of such practical advice as (1) divide markets into buyers and sellers, or producers and consumers; (2) specify the market structure; (3) create "ideal type" definitions of the behavioral assumptions so as to get sharp results; (4) set out the relevant ceteris paribus conditions; (5) translate the situation into an extreme problem and examine first- and second-order conditions; etcetera. It is evident that the marginalists after 1870 adopted the "hard core" of classical political economy, but they altered its "positive heuristic" and provided it with a different "protective belt."

Keynes went still further in tampering with the "hard core" that had been handed down since the time of Adam Smith. First of all, Keynes departed from the principle of "methodological individualism," that is, of reducing all economic phenomena to manifestations of individual behavior. Some of his basic constructs, like the propensity to consume, were simply plucked out of the air. To be sure, he felt impelled by tradition to speak of a "fundamental psychological law," but the fact is that the consumption function in Keynes is not derived from individual maximizing behavior; it is instead a bold inference based on the known, or at that time suspected, relationship between aggregate consumer expenditure and national income. On the other hand, the marginal efficiency of capital and the liquidity-preference theory of the demand for money are clearly if not rigorously derived from the maximizing activity of atomistic economic agents. Similarly, and despite what Leijonhufvud would have us believe, Keynes leaned heavily on the concepts of general equilibrium, perfect competition, and comparative statics, making an exception only for the labor market, which he seems to have regarded as being inherently imperfect and hence always in a state, not so much of disequilibrium as of equilibrium of a special kind.[26]

The really novel aspects of Keynes, however, are, first of all, the tendency to work with aggregates and indeed to reduce the entire economy to three interrelated markets for goods, bonds, and labor; secondly, to concentrate on the short period and to confine analysis of the long period, which had been the principal analytical focus of his predecessors, to asides about the likelihood of secular stagnation; and thirdly, to throw the entire weight of adjustments to changing economic conditions on output rather than prices. Equilibrium for the economy as a whole now involved "underemployment equilibrium," and the introduction of this conjunction, an apparent contradiction in terms, involved a profound change in the "hard core" of nineteenth-century economics, which undoubtedly included the faith that competitive forces drive an economy towards a steady state of full employment. Furthermore, the classical and neoclassical "hard core" had always contained the idea of rational economic calculation, involving the existence of certainty equivalents for each uncertain future outcome of current decisions. Keynes introduced pervasive uncertainty and the possibility of destabilizing expectations, not just in the "protective belt" but in the "hard core" of his programme. The Keynesian "hard core," therefore, really is a new "hard core" in economics. The Keynesian "protective belt" likewise bristled with new auxiliary hypotheses: the consumption function, the multiplier, the concept of autonomous expenditures, and speculative demand for money, contributing to stickiness in long-term interest rates. It is arguable, however, whether there was anything new in the marginal efficiency of capital and the saving-investment equality. Keynesian theory also had a strong "positive heuristic" of its own, pointing the way to national income accounting and statistical estimation of both the consumption function and

26. The best single piece of evidence for this statement is Keynes' reaction to Hicks's famous paper, "Mr. Keynes and the Classics." "I found it very interesting," he wrote to Hicks, "and really have next to nothing to say by way of criticism." Since Hicks's IS-LM diagram ignores the labor market, the reaction is hardly surprising. On Leijonhufvud's reading of Keynes, see Blaug 1975 and the references cited there.

the period multiplier. There is hardly any doubt, therefore, that Keynesian economics marked the appearance of a new SRP in the history of economics.

Furthermore, the Keynesian research programme not only contained "novel facts" but it also made novel predictions about familiar facts: it was a "progressive research programme" in the sense of Lakatos. Its principal novel prediction was the chronic tendency of competitive market economies to generate unemployment. Now, the fact that there was unemployment in the 1930's was not itself in dispute. Orthodox economists had no difficulty in explaining the persistence of unemployment. The government budget in both the United States and Britain was in surplus during most years in the 1930's. It did not need Keynes to tell economists that this was deflationary. It was also well known that monetary policy between 1929 and 1932 was more often tight than easy; at any rate, neither the United States nor the United Kingdom pursued a consistent expansionary monetary policy. Furthermore, the breakdown of the international gold standard aggravated the crisis. There was, in other words, no lack of explanations for the failure of the slump to turn into a boom, but the point is that these explanations were all "*ad hoc*," leaving intact the full-employment-equilibrium implications of standard theory. The tendency of economists to join the rank of the Keynesians in increasing numbers after 1936 was therefore perfectly rational; it was a switch from a "degenerating" to a "progressive" research programme, which had little to do with contentious issues of public policy.

This assertion is likely to arouse consternation because we all have been taken in, to a greater or lesser extent, by the mythology which has come to surround the Keynesian Revolution. According to the Walt Disney version of interwar economics, the neoclassical contemporaries of Keynes are supposed to have believed that wage cutting, balanced budgets, and an easy-money policy would soon cure the Great Depression. It comes as a great surprise to learn from Stein (1969) and Davis (1970) that no American economist between 1929 and 1936 advocated a policy of wage cutting; the leaders of the American profession strongly supported a programme of public works and specifically attacked the shibboleth of a balanced budget. A long list of names, including Slichter, Taussig, Schultz, Yntema, Simons, Gayer, Knight, Viner, Douglas and J. M. Clark, concentrated mainly at the universities of Chicago and Columbia but with allies in other universities, research foundations, and government and banking circles, declared themselves in print well before 1936 in favor of policies that we would today call Keynesian. Similarly, in England, as Hutchison (1968) has shown, names such as Pigou, Layton, Stamp, Harrod, Gaitskell, Meade, E. A. G. and J. Robinson came out publicly in favor of compensatory public spending. If there were any anti-Keynesians on questions of policy, it was Cannan, Robbins, and possibly Hawtrey, but definitely not Pigou, the bogeyman of the *General Theory*.[27] This, by the way, explains the reactions of most American and British reviewers of the *General Theory*: they questioned the new theoretical concepts, but dismissed the policy conclusions of the book as "old hat."

A fair way of summarizing the evidence is to say that most economists, at least in the English-speaking countries, were united in respect of practical measures for dealing with the depression, but utterly disunited in respect of the theory that lay behind these policy conclusions. What orthodoxy there was in theoretical matters extended only so far as microeconomics. Pre-Keynesian macroeconomics in the spirit of the quantity theory of money presented an incoherent mélange of ideas culled from Fisher, Wicksell, Robertson, Keynes of the *Treatise*, and Continental writers on the trade cycle. In a sense then the Keynesian theory succeeded because it produced the policy conclusions most economists wanted to advocate anyway, but it produced these as logical inferences from a tightly knit theory and not as endless epicycles on a full-employment model of the economy.[28]

It would seem that certain puzzles about the Keynesian Revolution dissolve when it is viewed through Lakatosian spectacles. The attempt to give a Kuhnian account of the Keynesian Revolution, on the other hand, creates the image of a whole generation of economists dumbfounded by the persistence of the Great Depression, unwilling to entertain the obvious remedies of expansionary fiscal and monetary policy, unable to find even a language with

27. I ignore the Stockholm School, which developed, independently of any clearly discernible influence from Keynes, most of the concepts and insights of Keynesian macroeconomics before the publication of either the *General Theory* (1936) or *The Means of Prosperity* (1933): see Uhr 1973. For Ohlin's recollections of the impact of Keynes of the Stockholm theorists, see Ohlin 1974, pp. 892–94.

28. Keynes himself put it in a nutshell. Writing to Kahn in 1937 with reference to D. H. Robertson and Pigou, he observed: "when it comes to practice, there is really extremely little between us. Why do they insist on maintaining theories from which their own practical conclusions cannot possibly follow? It is a sort of Society for the Preservation of Ancient Monuments" (Keynes 1973, p. 259). A hint of the same argument is found in the *General Theory*: a footnote in the first chapter refers to Robbins as the one contemporary economist to maintain "a consistent scheme of thought, his practical recommendations belonging to the same system as his theory."

which to communicate with the Keynesians, and, finally, in despair, abandoning their old beliefs in an instant conversion to the new paradigm. These fabrications are unnecessary if instead we see the Keynesian Revolution as the replacement of a "degenerating" research programme by a "progressive" one with "excess empirical content." Moreover, in this perspective, we gain a new insight into the postwar history of Keynesian economics, a history of steady "degeneration" as the Keynesian prediction of chronic unemployment begins to lose its plausibility. In the 1950's, the contradiction between cross-section and time-series evidence of the savings-income ratio, the former yielding a declining and the latter a constant average propensity to save, spawned a series of revisions in the Keynesian research programme, from Duesenberry's relative income hypothesis to Friedman's permanent income hypothesis to Modigliani's life-cycle theory of saving. Simultaneously, Harrod and Domar converted static Keynesian analysis into a primitive theory of growth, a development which discarded principal elements in the Keynesian "protective belt" and more or less the whole of the "hard core" of the original Keynesian programme. Friedman's monetarist counterrevolution went a good deal further, and for a few years in the late 1960's it almost looked as if Keynes had been decisively repudiated. The efforts of Patinkin, Clower, and Leijonhufvud to give a disequilibrium interpretation of Keynesian economics, and thus to integrate Keynesian theory into a more general neoclassical framework with still greater "excess empirical content," would seem to constitute a "progressive" research programme, superseding both static pre-Keynesian microeconomics and static Keynesian macroeconomics. Keynes' General Theory is now a special case, and this is scientific progress in economics, perfectly analogous to the absorption of Newton as a special case in the general theory of relativity.

It is possible to give a similar "internalist" account of the so-called Marginal Revolution as further demonstration of the applicability of MSRP to economics. The difficulties in the standard notion that marginalism was a new "paradigm" in economics were thoroughly thrashed out at the Bellagio Conference (see Black, Coats and Goodwin 1973) and it is only necessary to add that the innovations of Menger, Jevons, and Walras are more suitably described, not as a new SRP, but as a "progressive problem shift" in the older research programme of classical political economy. As frequently happens in such cases, there was "loss of content" as well as gain. What was lost, such as theories of population growth and capital accumulation, had become by the 1860's an incoherent body of ideas, virtually empty of empirical implications. The reaction against the Classical School was more a reaction against Ricardo than against Adam Smith. The Ricardian system was itself a "progressive problem shift" in the Smithian research programme, motivated by the experiences of the Napoleonic Wars and designed to predict the "novel fact" of the rising price of corn, leading in turn to rising rents per acre and a declining rate of profit. The "hard core" of Ricardo is indistinguishable from that of Adam Smith, but the "positive heuristic" contains elements which would have certainly surprised Adam Smith, and this explains the difficulties that many commentators have experienced in identifying disciples of Ricardo who were not also disciples of Adam Smith.[29]

I once argued that the distinctive feature of the Ricardian system was, not the labor theory of value, not Say's law, not even the inverse relation between wages and profits, but "the proposition that the yield of wheat per acre of land governs the general rate of return on invested capital as well as the secular changes in the distributive shares" (Blaug 1958, p. 3). The notion that Ricardo is at one and the same time the heir of Adam Smith and his principal critic can be conveyed succinctly in the language of MSRP. All the leading British classical economists up to Jevons and even up to Sidgwick subscribed to the basic Ricardian link between the productivity of agriculture and the rate of capital accumulation, and it is in this sense that we can speak of a dominant Ricardian influence on British economic thought throughout the half-century from Waterloo to the Paris Commune. There are unmistakable signs after 1848 of "degeneration" in the Ricardian research programme, marked by the proliferation of "*ad hoc*" assumptions to protect the theory against the evidence that repeal of the Corn Laws in 1846 had failed to bring about the effects predicted by Ricardo (Blaug 1968, pp. 227–28).[30] On the other hand, the Ricardian research programme

29. See, e.g., O'Brien (1970), who shows that even John Ramsay McCulloch, Ricardo's leading disciple, never succeeded in resolving the conflict in his mind between Smith and Ricardo.

30. In an illuminating paper on Ricardo's and John Stuart Mill's treatment of the relationship between theory and facts, de Marchi (1970) argues that Mill did not, as I have alleged, evade refutations of Ricardo's predictions by retreating into an unspecified ceteris paribus clause; he was simply careless with facts and declined to reject an attractive theory merely because it predicted poorly. The issue between us is one of subtle distinctions and, as I am going to argue later on, these distinctions still plague modern economics. Suffice it to say that a defensive attitude to the Ricardian System is increasingly felt in successive editions of the *Principles* and even more in the writings of Cairnes and Fawcett (Blaug 1958, pp. 213–20).

was by no means dead by 1850 or even 1860. Cairnes' work on the Australian gold discoveries and Jevons' study *The Coal Question* (1865) showed that there was still unrealized potential in the Ricardian system. Nevertheless, Mill's "recantation" of the wages fund theory in 1859 expressed a widely felt malaise, typical of those who find themselves working within a steadily degenerating SRP.

The trouble with this line of argument is that Ricardo did not exert a preponderant influence on Continental economic thought. There is absolutely no evidence of any widespread sense of increasing discomfort in France or Germany around 1870 with classical economic doctrine, conceived broadly on the lines of Adam Smith rather than of Ricardo. What was missing in the British tradition, it was felt, was the utility theory of value, which had roots on the Continent going back to Condillac, Galiani, and even Aristotle. What we see in Menger and even more in Walras, therefore, is the attempt to concentrate attention on the problem of price determination at the expense of what Baumol has called the "magnificent dynamics" in Smith, Ricardo, and Mill, in the course of which due emphasis was given to the neglected demand side. This could be seen, and indeed was seen, as an improvement rather than an outright rejection of Adam Smith. There was no room in this schema for the specifically Ricardian elements, except in afterthoughts about long-run tendencies. In the Continental perspective, that is, the whole of the Ricardian episode in British classical political economy was regarded as something of a detour from the research programme laid down by Adam Smith. In other words, whatever we say about Jevons and the British scene, there was no Marginal Revolution on the Continent: there was a "problem shift," possibly even a "progressive problem shift," if predictions about "the price of an egg" may be regarded as more testable than predictions about the effects of giving free rein to the workings of "the invisible hand."

Clearly, economists after 1870, or rather 1890, reassessed the nature of the facts that economics ought to be concerned with. It is conceivable that this "gestalt switch" can only be explained in terms of "external history." If so, and particularly if we lack any independent corroboration for this historical explanation, we have a refutation of MSRP as a metahistorical research programme. I have been arguing, however, that an "internalist" account makes it unnecessary to resort to "external factors." It would be premature, however, to arrive at that conclusion on the basis of my crude sketch of historical developments. Only a series of detailed case studies of the spread of marginalism on the Continent after 1870 could settle that question.[31] What I want to insist here is simply that MSRP gives us a powerful handle for attacking these problems.

3. *The theory of the firm as a case in point*

It is tempting to bring the story foward and to ask whether MSRP is capable of shedding light on the apparent "degeneration" of the Marshallian research programme in the first two decades of the twentieth century, culminating in the debate on "empty economic boxes" and the emergence of the theory of monopolistic or imperfect competition; or the less controversial "degeneration" of the Austrian theory of capital after Wicksell's failure to resolve certain outstanding anomalies in the concept of an "average period of production"; or the startling failure of the Walrasian programme to make much progress until Hicks's *Value and Capital* (1939) and Samuelson's *Foundations* (1948) provided it with a new "positive heuristic"; and so forth and so forth. But I will resist these temptations[32] and turn instead to an examination of Latsis's indictment of the traditional theory of the firm, the first attempt in the literature to provide a case study of MSRP in economics.

Latsis argues convincingly that theories of perfect and imperfect competition may be considered together as forming part of the same neoclassical research programme in business behavior with one identifiable "hard core," one "protective belt," and one "positive heuristic." The "hard core" is made up of "(1) profit-maximisation, (2) perfect knowledge, (3) independence of decisions, and (4) perfect markets".[33] The "protective belt" includes several auxiliary assumptions: "(1) product homogeneity, (2) large numbers, and (3) free entry and exit." The "positive heuristic" consists of "the analysis of equilibrium conditions as well as comparative statics" (Latsis 1972, pp. 209, 212). This research programme is labeled "situational determinism" because "under the conditions characterising perfect competition the decision-maker's discretion in choosing among alternative courses of action is reduced simply to whether

31. Black, Coats, and Goodwin (1973) provide a few of such case studies which seem to me to strengthen the internalist thesis.

32. I will also resist the temptation to apply MSRP to Marxian economics, which began badly to "degenerate" in the first decade of this century when the German Marxists failed to respond creatively to Bernstein's revisionism, and which has continued to "degenerate" ever since, the unmistakable signs of which are endless regurgitation of the same materials, the continual substitution of appeals to authority for analysis, and a persistently negative attitude to empirical research.

33. This formulation strikes me as being too strong to constitute the irrefutable metaphysic of the neoclassical research programme, which only shows that two Lakatosians need not agree on how to apply MSRP to a particular case in question.

or not to remain in business" (Latsis 1972, p. 209).[34] This seems to ignore the fact that, apart from remaining in business, the competitive firm also has to decide what output to produce. But the nub of the argument is that the firm either produces the profit-maximizing level of output or no output at all: "I shall call situations where the obvious course of action (for a wide range of conceptions of rational behaviour) is determined uniquely by objective conditions (cost, demand, technology, numbers, etc.), *'single exit'* or 'straightjacket' situations" (Latsis 1972, p. 211).

In other words, once an independent decisionmaker with a well-ordered utility map in a perfect competitive market is given perfect information about the situation he faces, there is nothing left for him to do, according to neoclassical theory, but to produce a unique level of output, or else to go out of business. There is no "decision process," no "information search," no rules for dealing with ignorance and uncertainty in the theory: the problem of choice among alternative lines of action is so reduced that the assumption of profit maximization automatically singles out one best course of action. The motivational assumptions of "orthodox theory," Latsis concludes, could be "weakened from profit maximisation to bankruptcy avoidance," without affecting its predictions (Latsis 1972, p. 223).

But what are these predictions? The "positive heuristic" of the research programme is directed at such questions as "(1) Why do commodities exchange at given prices?; (2) What are the effects of changes in parameters (say demand) on the variables of our model once adjustment has taken place?" (Latsis 1972, pp. 212–13). But Latsis spends little time considering the specific predictions of neoclassical theory under given circumstances. For example, a standard prediction of the traditional theory of the firm is that a change in the corporate income tax, being a change in a proportionate tax on business income, does not affect the level of output of a competitive firm in the short run because it does not alter the level of output at which profits are maximized; for that reason the theory predicts that the tax will not be shifted. There is a considerable literature which tends to refute that prediction (Ward 1972, p. 18), and this is relevant, although not necessarily clinching, evidence against traditional theory and, by the way, in favor of the sales-maximization hypothesis. Latsis largely ignores these and other refutations. At various points he does refer to evidence indicating that highly competi-

34. The phrase "situational determinism" is derived from Popper's *Open Society*, where *the* method of economic theory is described as "analysis of the situation, the situational logic" (cited in Latsis 1972, p. 224).

tive industries sometimes fail to behave in the way predicted by the theory (Latsis 1972, pp. 219–20), but for the most part he takes it for granted that traditional theory has a poor predictive record.[35]

He has little difficulty in showing that the habitual appeal to conditions of perfect competition as an "ideal type" fails to specify the limits of applicability of the traditional theory of profit maximization, so that even the behavior of oligopolists has come to be analyzed with the same tools. But such "immanent criticism" tells us nothing about "the degree of corroboration" of a theory. For that we need a report on the past performance of the theory in terms of the severity of the tests it has faced and the extent to which it has passed or failed these tests.[36] Latsis provides no such report. In part, this is because his central argument is that all the programme's successive versions have failed to generate empirical results. But the fact of the matter is that they were thought to do so. For example, the Chamberlin tangency solution was supposed to predict excess capacity in the case of many sellers with differentiated products. Similarly, theories of joint profit maximization under conditions of oligopoly were supposed to predict price rigidities. We cannot avoid asking, therefore, whether these predictions are borne out by the evidence.

Thus, it is difficult to escape the conclusion that Latsis's charac-

35. In the same way, Friedman simply takes it for granted that traditional theory has a splendid predictive record: "An even more important body of evidence for the maximization-of-returns hypothesis is experience from countless applications of the hypothesis to specific problems and the repeated failure of its implications to be contradicted. This evidence is extremely hard to document; it is scattered in numerous memorandums, articles and monographs concerned primarily with specific concrete problems rather than with submitting the hypothesis to test. Yet the continued use and acceptance of the hypothesis over a long period, and the failure of any coherent, self-consistent alternative to be developed and widely accepted, is strong indirect testimony to its worth" (Friedman 1953, p. 23). This is without doubt the most controversial passage of an otherwise persuasive essay because it is unaccompanied by even a single instance of these "countless applications." No doubt, when the price of strawberries rises during a dry summer, when an oil crisis is accompanied by a sharp rise in the price of oil, when share prices tumble after a deflationary budget, we may take comfort in the fact that the implications of the maximization-of-return hypothesis have once again failed to be refuted. However, given the multiplicity of hypotheses that could account for the same phenomena, we can never be sure that the repeated failure to produce refutations is not a sign of the reluctance of economists to develop and test unorthodox hypotheses. It would be far more convincing to be told what economic events are excluded by the maximization-of-returns hypothesis, or better still, what events, if they occurred, would impel us to abandon the hypothesis.

36. In Popper's words: "By the degree of corroboration of a theory I mean a concise report evaluating the state (at a certain time t) of the critical discussion of a theory, with respect to the way it solves its problems; its degree of testability; the severity of the tests it has undergone; and the way it has stood up to these tests. Corroboration (or degree of corroboration) is thus an evaluating *report of past performance*" (Popper 1972, p. 18).

terization of the neoclassical theory of the firm as "degenerating" (Latsis 1972, p. 234) is actually based on an examination of the theory's assumptions rather than its testable implications. This conclusion is strengthened by considering his discussion of "economic behaviouralism" in the writings of Simon, Cyert and March, Williamson, and Baumol as a rival research programme in business behavior. He usefully distinguishes "behaviouralism" from "organisationalism," the former emphasizing learning and "slack" in a fluid and only partially known environment, the latter emphasizing the survival needs of organizations; "behaviouralism" is applicable to a single decisionmaker but "organisationalism" denies that there are such animals and insists that the objectives of decisionmakers should not be postulated a priori but ascertained a posteriori by observation of decision making in the real world. Traditional theory turns the decisionmaker into a cypher, whereas both behavioral and organizational theories focus attention on the nature and characteristics of the decision-making agent or agents; they do so by repudiating all "hard core" concepts of optimization, rejecting even the notion of general analytical solutions applicable to all business firms facing the same market situation.

It would be premature, Latsis argues, to attempt an appraisal of "behaviouralism" as a budding research programme. The approach may have potential for problems to which the traditional theory is unsuited but "neoclassical theory gives some simple answers to questions which we cannot even start asking in terms of behaviouralism (namely, in the domain of market structure and behaviour)" (Latsis 1972, p. 233). Likewise, behavioralism has not "successfully predicted any unexpected novel fact" and "as a research programme, it is much less rich and much less coherent than its neoclassical opponent" (Latsis 1972, p. 234). But lest this imply the superiority of traditional theory, Latsis hastens to add that these are uncommensurable research programmes: "the two approaches are, in my view, importantly different and mutually exclusive over an extensive area" (Latsis 1972, p. 233).[37] In other words, the neoclassical research programme is condemned as "degenerating" al-

37. Loasby (1971) reaches the same conclusions, using Kuhn's methodology; like Latsis, he views profit maximization as irrefutable because it is not a hypothesis but a "paradigm." In reply to Latsis, Machlup (1974) has seized eagerly on the admission of incommensurability between behavioralism and marginalism, claiming that "a research programme designed to result in theories that explain and predict the actions of particular firms can never compete with the simplicity and generality of the marginalist theory, which, being based on the constructs of a fictitious profit-maximiser, cannot have the ambition to explain the behaviour of actual firms in the real world."

though it has no rival in its own domain, and furthermore, the condemnation is based on the logic of single-exit determinism and not on its record of repeated refutations. In the final analysis, therefore, Latsis denies the normative "hard core" of MSRP: neoclassical theory is primarily rejected because it is theoretically sterile and only secondarily because it fails to be empirically corroborated. There is nothing wrong with such a criticism, but it is less than might have been expected from an application of MSRP to economics.

There is a further point. One of the promising features of Lakatos' methodology is the insistence that we literally cannot appraise single theories: we test theories, but we appraise research programmes. The neoclassical research programme is much more than a theory of the firm; it is also a theory of the determination of wage rates and interest rates, and it includes, and some would say it starts with, a theory of consumer behavior. If the neoclassical research programme in the economics of industry is to be written off as "degenerating," the rot should show up in the theory of factor pricing and in the theory of demand. One can sympathize with an author who declines to review the whole of microeconomics in order to assess its "degree of corroboration," but that is no excuse for not mentioning the entire research programme. It is certainly impossible to understand the tenacious defense of marginalism in the field of business behavior without recognition of the fact that what is at stake is the whole of price theory.[38] Here, as elsewhere, Latsis seems to me to do less than justice to Lakatos' methodology.

4. *Do economists practice what they preach?*

Having said that much, it only remains for me to do what I criticize Latsis for not doing, namely, to appraise the whole of neoclassical economics with the aid of Lakatos' methodology. But I am not equal to that task. What I will do is to voice some misgivings about the applicability of any philosophy of science grounded in the history of the physical science to a social science like economics. I

38. As Krupp has so aptly observed: "The degree of confirmation of an entire theory is highly intertwined with value judgements which reflect, among other things, the selection of its constituent hypothesis. It is not coincidental, therefore, that the advocates of the theories of competitive price will simultaneously defend diminishing returns to scale, a low measure of economic concentration, the demand-pull explanation of inflation, a high consumption function, the effectiveness of monetary policies on full employment, the insignificance of externalities, and the general pervasiveness of substitution rather than complementarity as a basic relation of the economic system" (Krupp 1966, p. 51).

express these misgivings tentatively. If they are widely shared, so much the worse for the prospect of writing an entirely "internalist" history of economic thought.

I begin by quoting Machlup, who in his long career has returned repeatedly to problems of the methodology of economics:

> When the economist's prediction is *conditional*, that is, based upon specified conditions, but where it is not possible to check the fulfilment of all the conditions stipulated, the underlying theory cannot be disconfirmed whatever the outcome observed. Nor is it possible to disconfirm a theory where the prediction is made with a stated *probability* value of less than 100 per cent; for if an event is predicted with, say, 70 per cent probability, any kind of outcome is consistent with the prediction. Only if the same "case" were to occur hundreds of times could we verify the stated probability by the frequency of "hits" and "misses." This does not mean complete frustration of all attempts to verify our economic theory. But it does mean that the tests of most of our theories will be more nearly of the character of *illustrations* than of verifications of the kind possible in relation with repeatable controlled experiments or with recurring fully-identified situations. And this implies that our tests cannot be convincing enough to compel acceptance, even when a majority of reasonable men in the field should be prepared to accept them as conclusive, and to approve the theory so tested as "not disconfirmed" [Machlup 1955, p. 19].[39]

This passage may be read as a criticism of "naive falsificationism," but it may also be read as a plea for still more "sophisticated falsificationism." It is precisely because tests of economic theories are "more nearly of the character of illustrations than of verifications" (I would prefer to say "falsifications") that we need as many "illustrations" as possible. But that implies that we concentrate our intellectual resources on the task of producing well-specified falsifiable predictions; in other words, we give less priority to such standard criteria of appraisal as simplicity, elegance, and generality, and more priority to such criteria as predictability and empirical fruitfulness. It is my impression, however, that most modern economists would order their priorities precisely the other way round.

Ward's recent book asks *What's Wrong with Economics?* and his answer in brief is that economics is basically a normative policy sci-

39. In the same spirit, see Grunberg and Boulding in Krupp 1966.

ence traveling in the false disguise of a positive one. Insofar as it is a positive science, however, he agrees that "the desire systematically to confront the theory with fact has not been a notable feature of the discipline," although that, he contends, "is not the central difficulty with modern economics" (Ward 1972, p. 173). What I want to argue, by way of contrast, is that the central weakness of modern economics is in fact the reluctance to produce theories which yield unambiguously refutable implications.

When, in the long process of refining and extending the neoclassical research programme over the last hundred years, have we ever worried about "excess empirical content," much less "corroborated excess empirical content"? Consider, for example, the preoccupation since 1945 of some of the best brains in modern economics with problems of growth theory, when even practitioners of the art admit that modern growth theory is all about "shadows of real problems, dressed up in such a way that by pure logic we can find solutions for them" (Hicks 1965, p. 183). But that example is too easy. Take rather that part of the neoclassical research programme which comes closest in matching the rigor and elegance of quantum physics, the modern theory of consumer behavior, based on axiomatic utility theory, to which a long line of economists from Fisher, Pareto, Slutsky, and Johnson to Hicks, Allen, Samuelson, and Houthakker have devoted their most intense efforts. There is little sign that these prodigious labors have had a substantive impact on household budget studies or on the literature dealing with statistical demand curves. Or to switch fields, consider the endless arguments in textbooks on labor economics about the assumptions that underlie the misnamed "marginal productivity theory of wages" at the expense of space devoted to considering what the theory actually predicts and how well it has fared. If this is not misplaced emphasis, what is? We all recognize that misplaced emphasis at least implicitly, which is why Lipsey's textbook was so well received when it first appeared: to this day, its relative emphasis on empirical testing stands out among the current textbooks on elementary economics.

But surely economists engage massively in empirical research? Certainly they do, but much empirical work in economics is like "playing tennis with the net down": instead of attempting to refute testable predictions, economists spend much of their time showing that the real world bears out their predictions, thus replacing falsification, which is difficult, with confirmation, which is easy. A single example must suffice. Ever since Solow's celebrated article of 1957, estimation of aggregate Cobb-Douglas production functions for purposes of measuring the sources of economic growth and drawing inferences about the nature of technical progress has become a widespread practice in economic research. Ostensibly, such work tests the prediction that production functions in the aggregate obey the condition of constant returns to scale and that individual markets, despite trade unions and despite monopolies, impute prices to factors in accordance with the theory of perfect competition. More than a decade passed before Fisher (1971) showed conclusively that it is perfectly possible to obtain a good fit of an aggregate Cobb-Douglas production function even if the underlying pricing mechanism is anything but competitive. But long before that, several econometricians had argued convincingly that the concept of aggregate production functions, as distinct from microproduction functions, lacks a firm theoretical foundation.[40] If the advice was ignored, it was because most economists are delighted with puzzle-solving activity of an empirical kind even if it is virtually tantamount to "measurement without theory." Marshall used to say that "explanation is prediction written backwards." Many economists forget that prediction is not necessarily explanation written forwards.[41] It is only too easy to engage in empirical works that fail utterly to discriminate between competing explanations and which consist largely of mindless "instrumentalism."

Those who explicitly revolt against orthodoxy are often infected by the same disease. So-called Cambridge controversies in the theory of capital, which actually are controversies about the theory of functional income distribution, have raged on for twenty years without so much as a reference to anything but stylized facts, such as the constancy of the capital-output ratio and the constancy of labor's relative share, which turn out on examination not to be facts at all. The fundamental issue at stake between Cambridge U.K. and Cambridge U.S., we are told by no less an authority on the debate than Joan Robinson, is not so much the famous problem of how to measure capital as it is the question of whether saving determines investment instead of investment determining saving.[42] That issue depends in turn on the question of whether the world is better described by full employment or by underemployment equilibrium. Inasmuch as the entire debate is carried out in the context of steady-

40. For a fuller discussion, see Blaug 1974.

41. What I am denying is the well-known "thesis of the structural symmetry of explanation and prediction": see Hempel 1965, pp. 367–76 and Grünbaum 1973, chap. 9.

42. For references and details, see Blaug 1974.

state-growth theory, and as everyone agrees that steady-state growth is never even approximated in real economics, there is no reason whatever for refusing to operate with both models, depending on the problem at hand. Neither model has any predictive power, and Cambridge controversies, therefore, are incapable of being resolved by empirical research. This has not, however, prevented either side from battling over the issues with redoubled fury. Protagonists in both camps have described the controversy as a war of "paradigms," but in fact the two "paradigms" intersect and indeed overlap almost entirely.

Even the radical political economists in the United States have spent most of their efforts on "telling a new story": the same old facts are given a different interpretation around the "paradigm" of power conflict in contrast to the "paradigm" of utility maximization in mainstream economics (see Worland 1972). What little empirical work has appeared in the *Review of Radical Political Economy* on race and sex discrimination, the financial returns to education, and patterns of social mobility in the United States has lacked discriminating, well-articulated hypotheses that could distinguish between orthodox and radical predictions (see Bronfenbrenner 1972). But the movement does at least have the excuse of explicitly announcing its preference for social and political relevance over simplicity, generality, and falsifiability as characteristics of "good" theory.[43]

Neoclassical economists do not have the same excuse. They preach the importance of submitting theories to empirical tests, but their practice suggests that what they have in mind is merely "innocuous falsificationism." Of all the great modern economists who have advocated a falsificationist methodology—Harrod, Koopmans, Friedman, Samuelson, Baumol, and Boulding—Friedman is almost the only one whose analysis and research exemplify his own precepts. His work on Marshallian demand curves, on the expected-utility hypothesis, on flexible exchange rates, and particularly on the permanent-income hypothesis is marked by a constant search for refutable predictions. *The Theory of the Consumption Function* (1957) is surely one of the most masterly treatments of the relationship between theory and data in the whole of the economic literature. But

43. Franklin and Resnik 1974, pp. 73–74, provides a typical methodological pronouncement: "From a radical perspective, in which analysis is closely linked to advocacy of fundamental changes in the social order, an abstract model or category is not simply an aesthetic [sic] device. It is purposely designed to assist in the changes advocated, or in describing the nature of the barriers that must be broken down if the advocated changes are to occur."

even Friedman produced his "theoretical framework for monetary analysis" long after making dramatic claims of direct empirical evidence in favor of the quantity theory of money (see Friedman 1970). As a monetarist, even Friedman has failed to live up to his own methodology.[44]

I have left to the last the issue of welfare economics, where of course no questions of testable implications can arise. Here the Lakatos methodology is helpless because there is nothing in the physical sciences that corresponds to theories which deduce the nature of a social optimum from certain fundamental value judgments. Economists have talked a great deal of nonsense about "value-free" welfare economics on the curious argument that the standard value judgments that underlie the concept of a Pareto optimum—every individual is the best judge of his own welfare; social welfare is defined only in terms of the welfare of individuals; and the welfare of individuals may not be compared—command wide assent and this consensus somehow renders them "objective." They have also swallowed whole the untenable thesis that "normative" as distinct from "methodological" value judgments are not subject to rational discourse and have thus denied themselves a fruitful area of analysis.[45] But these issues apart, the intimate relationship between normative and positive economics has been a potent source of "ad hoccery" in economics, the effort to retain theories at all costs by the addition of assumptions that lack testable implications.

No doubt, welfare economics and positive economics are separa-

44. The case of Friedman also illustrates the fact that agreement on falsificationism among modern economists disguises a significant spectrum of attitudes in respect of the type of test that is deemed appropriate in different circumstances. As Briefs (1961) argues, in an unduly neglected book, economists have always disagreed about the role of statistical significance tests versus that of historical analysis as alternative methods of refuting economic hypotheses; even supporters of statistical testing differ about the admissability of single-equation regressions in contrast to simultaneous equation estimates, depending in turn on whether the individual writer favors partial or general equilibrium analysis. Friedman's writings exemplify all three methods.

45. For the beginnings of such an analysis, see Sen 1970, pp. 58–64. The positive suggestions for reconstructing economics in Ward 1972 are along similar lines. It is worth noting that the failure to distinguish "methodological" and "normative" value judgments has been productive of much misunderstanding surrounding the value-fact dichotomy in social inquiry. Methodological judgments involve criteria for judging the validity of a theory, such as levels of statistical significance, selection of data and assessment of their reliability, adherence to the canons of formal logic, etcetera, which are indispensable in scientific work. Normative judgments, on the other hand, refer to ethical judgments about the desirability of certain kinds of behavior and certain social outcomes. It is the latter which are said to be capable of being eliminated in positive science. See Nagel 1961, pp. 485–502, for almost the last word on this endlessly debated topic.

ble in principle. However, practical policy recommendations typically violate the logical separability of the two. Decisionmakers demand as much advice on their objectives as on the means to achieve these objectives, and the supply of advice naturally responds accordingly. Besides, as Samuelson said in the *Foundations*: "At least from the time of the physiocrats and Adam Smith, there has never been absent from the main body of economic literature the feeling that in some sense perfect competition represented an optimal situation." The modern Invisible Hand Theorem provides a rigorous demonstration of that feeling: every long-run perfectly competitive equilibrium yields an optimal allocation of resources, and every optimum allocation of resources is a long-run perfectly competitive equilibrium. Of course, this leaves out the "justice" of the associated distribution of personal income; furthermore, "optimal allocation" is strictly defined with reference to the three basic value judgments of Paretian welfare economics. Nevertheless, every economist feels in his bones that the Invisible Hand Theorem is almost as relevant to socialism as to capitalism, coming close indeed to a universal justification for the role of market mechanisms in any economy. It is hardly surprising, therefore, that economists fight tooth and nail when faced with an empirical refutation of a positive theory involving the assumption of perfect competition. For what is threatened is not just that particular theory but the entire conception of "efficiency" which gives raison d'être to the subject of economics. No wonder then that the "principle of tenacity"—the fear of an intellectual vacuum—looms so large in the history of economics.

The upshot of this long harangue is to suggest that MSRP may not fit the history of economics: economists may cling to "degenerating" research programmes in the presence of rival "progressive" research programmes, while denying that the "degenerating" programme is in need of resuscitation because they are suspicious of hard data, inclined to assign low priority to the discovery of novel facts, accustomed by long habit to deny the feedback of evidence on theory, or simply because they are deeply attached to the welfare implications of their theories. If this should prove to be the case after a detailed examination of twentieth-century economics with the aid of MSRP, it may tell us something more fundamental about the difference between natural and social science than the old saws about the unchanging universe of physics and the continually changing universe of economics.

5. Conclusions

Lakatos' metahistorical research programme has a "hard core" of its own: scientists are rational and accept or reject ideas for good intellectual reasons, the only problem being to determine what they are. The programme also has a "protective belt" which contains such propositions as these: scientists attach importance to the ability of theories to survive tests but they do not discard theories after a single failure; scientists appraise programmes, not theories; scientists appraise programmes historically as they evolve over time and continually revise their appraisals; lastly, scientists appraise programmes in competition with rivals and will retain a programme at any cost if no alternatives are available. The "positive heuristic" of the metahistorical research programme is equally obvious: collect theories into research programmes; spell out the "hard core," "the protective belt," and "the positive heuristic" of the respective programmes; examine the efforts that have been made to test theories, and trace the manner in which falsifications are dealt with in the programme; set out the anomalies that are recognized by practitioners of a programme and, if possible, the anomalies that have come to be forgotten; trace the standards by which the adherents of a research programme judge their predecessors and by which they hope to be judged by their followers, that is, analyze their methodological pronouncements; and, finally, highlight the novel facts which are discovered in the course of a programme. The object of the exercise is to show that most scientists join research programmes that have "excess empirical content" and desert research programmes that lack this characteristic. This is "internal history," and every other reason for joining one camp rather than another is "external." It was Lakatos' claim that the "rational reconstruction" of the history of science conceived in these terms would in fact need few footnotes referring to "external history."

Can the history of economics be written in this fashion? It is perfectly true that most externalist accounts of scientific progress are very persuasive—they are selected to be so. When certain theories become the ruling scientific ideas of their times for "good" internalist reasons, there are frequently also ideological reasons that make the theory palatable to vested interests and appealing to the man in the street. These can be invoked subsequently to argue that the theory was in fact accepted for external reasons (consider Malthus's theory of population, or Darwin's theory of natural selection). But such externalist explanations, while not wrong, are nevertheless redundant if we have regard to professional rather than popular opinion. To be convincing, the externalist thesis in the history of ideas must produce instances of (1) internally consistent, well corroborated, fruitful, and powerful scientific ideas which were rejected at specific dates in the history of a science because of specific external factors, or (2) incoherent, poorly corroborated, weak scientific ideas which were in fact accepted for specific external reasons. I can think of no unambiguous examples of either (1) or (2) in the history of economics and therefore conclude that a Lakatosian "rational reconstruction" would suffice to explain virtually all past successes and failures of economic research programmes.

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