The Conservatism of Programme Continuity: Criticism of Lakatosian Methodology in Economics

Bart Nooteboom
University of Groningen, the Netherlands

The thesis of this article is that the practice of mainstream economics has degenerated from rational theoretical tenacity to indefensible theoretical conservatism (dogmatism), and that this is being rationalised on the basis of Lakatos’ Methodology of Scientific Research Programmes (MSRP). In addition to the criterion of increase of corroborated empirical content, that methodology includes continuity with received explanatory principles (“programme continuity”) as a criterion for the development of theory. Use, or perhaps misuse, of this principle is yielding unacceptable theoretical conservatism, shielding established neoclassical theory from criticism. It does not sufficiently take into account the pervasiveness, in economics, of the problem of theoretical “multiplicity” or indeterminacy. An illustration is provided. More cross-programme criticism is required, to generate attention to alternative interpretations of phenomena which compete with the interpretations from established theory, with different implications for policy. By cross-programme criticism economics is to be opened up to insights from psychology and sociology. For cross-programme criticism considerations of plausibility are indispensable. Plausibility fits well in a Quinean theory of meaning and a coherence theory of truth, and is a legitimate methodological category even if (or in fact precisely because) it is a “soft”, linguistic category rather than a “hard”, logical one. We plead not for a “logic” but for a “grammar” of “scientific discovery”.

Introduction

The premise of this article is that methodology should be normative, while in order to be justifiably normative it should also be empirically adequate, in the sense that it has to be tested for its feasibility. There is no point in prescribing actions that are in fact infeasible, or to prohibit actions that are inevitable. In other words: methodology should be pragmatic. But a methodology that drops all normative aspirations no longer is a methodology.

The problem of demarcation between adequate and inadequate science claims attention, and to evade it constitutes a cop-out. If previous attempts at demarcation have failed, the conclusion is not that we should give it up, but that perhaps we should look for a different kind of demarcation. Perhaps it cannot be rigorous, universal and fixed, and can only be provisional and vague. Then we will have to muddle through. Perhaps we need a novel concept of methodological order, but we cannot renounce judgement. If methodologists evade the issue, the question will be directed to others (politicians or bureaucrats, perhaps), and they will make the judgements.

Admittedly, normativity issues from a priori value judgements. This was admitted by Popper, among others: his critical rationalism was intended to maintain an “open society”. The ideological axe that the present author has to grind is his view that the development of theory in economics should be more open to insights from epistemology, cognitive psychology and sociology, and in particular the connections between them, in order to achieve further improvements in the explanation of the conduct of economic agents in present times of rapid technological and economic change.

If we do not accept that “anything goes” (Feyerabend), or that methodology is to be replaced by a merely descriptive anthropology, as seems to be Blamer’s position, or that methodology is to be seen as mere rhetoric (McCloskey), what is there that we might use for a normative methodology? The most likely candidate still appears to be the

The design of the article is as follows. First, the rationality of a certain amount of "theoretical tenacity" is acknowledged, in contrast with unjustifiable "theoretical conservatism". Next, some highlights are given from the Lakatosian reconstructions by Hands and Weintraub, which provide a rationalisation of theoretical conservatism in mainstream economics. It is shown that these are reasonable applications of MSRP: the conservatism that they implement is intended in MSRP and is inherent in it. This conservatism may be in conflict with the progressive intentions of MSRP, but in fact it is undeniably and irreparably embedded in it. Next it is argued that present theoretical conservatism in economics is excessive and indefensible. To illustrate the importance of the issue, some of the evidence of conservatism is provided. The core of the problem lies in the possibility of "multiple interpretations", which is in fact the old problem of induction. An example is provided to illustrate the problem. The conclusion will be that in order to solve the problem of conservatism and multiple interpretations, MSRP is inadequate. A wider scope of cross-programme criticism is required. The basis for such criticism lies in "background knowledge", which yields a vindication of the methodological role of considerations of plausibility. Admittedly, plausibility does not offer the rigour of a logical category. It does not yield a clearcut methodology: it is more a part of a "sprachethik" for economists (McCloskey). It is not part of an algorithm but of a grammar for criticism.

MSRP

There have been several attempts to reconstruct the endeavor of economists by means of MSRP: Weintraub (1985,1988); Maddock (1984); Worrall (1978); Hands (1985,1988); a collection of papers edited by Latsis (1976, in particular contributions by Blaug, Coats, Latsis and de Marchi).

The distinctive feature of MSRP is that it does not look at single theories in isolation, but assesses progress in successive steps of theory development. A theory is rationally given up only in the light of an alternative that has more corroborated empirical content. However, this could be achieved by the "tacking on" of unconnected bits and pieces, yielding a conceptually incoherent, patched-up whole. To prevent this, and as an improvement on Dohrn's criterion of "simplicity", Lakatos proposed that the successor theory should be (reconstructable as) a development from the core of a research programme according to a (positive and negative) heuristic. This is the criterium of "programme continuity".

Hands(1988) and Weintraub(1985,1988) use this latter aspect of MSRP to account for an obvious lack of falsifiability and actual falsification, and for a striking tenacity to basic explanatory principles, in neoclassical theory construction in economics.

The question can be asked whether the reconstructions by Hands and Weintraub are truly Lakatosian: do they conform to the letter and/or spirit of MSRP? On the basis of what Lakatos actually wrote, it will be argued that their reading of Lakatos is reasonable, in the sense that programme continuity clearly was a normative category for Lakatos, even though Hands did read something into MSRP which does not seem to be there in quite that form. It will be argued that either MSRP breaks down or conservatism is implied in it.

The Legitimacy of Theoretical Tenacity

It should be made clear from the outset that the legitimacy of theoretical tenacity is accepted. From a pragmatic perspective it is rational to hold on to a theory or research programme in spite (but not regardless) of empirical anomalies, until they become excessive, in the light of alternative theories or programmes. Kuhn (1962) already gave an economic argument for what he called "normal science": one does not incur the costs of extensive retooling, retraining and reorganisation associated with a new technology at the first sign that present technology is imperfect, but only when the cumulative burden of inadequacies becomes excessive in comparison with a new technology2.

There is also a cognitive argument: if one surrenders a given theory or programme at the first anomaly that it encounters, one will never establish the boundaries of its adequacy, and one will not know what improvements to demand from a replacement, or in what direction to look for one. To paraphrase Lakatos(1970, p.133, note 4):

New theories do not emerge fully equipped from a godhead, like Athena from the brow of Zeus, but from laborious experience with the failures of an old practice3.

Lakatos relied on corroborated excess empirical content of successor theories, within a research programme, to prevent excessive conservatism and to promote progressiveness, to such an extent that a rather cavalier attitude to anomalies is permitted; they can be largely ignored. For reasons to be discussed later, I think that in economics the progressive force of corroborated excess content.
within research programmes, is limited, and therefore tenacity is acceptable to the extent that it is practised not \textit{regardless} of anomalies but \textit{in spite} of anomalies, which should be well recorded and stored, for as long and in so far as this tenacity is needed to explore the boundaries of validity and to obtain hints for renewal. To assess the competitive advantage of a theory and pointers for theory development one should keep books on profit and loss; on successes and failures in the market of applications. This view is more Kuhnian than Lakatosian.

\textbf{Heuristic Progressiveness}

Hands (1988) retrieved three different types of "ad hocness" from the works of Popper and Lakatos, and considered their use in economics. According to Hands the first two types are found in both Popper and Lakatos, while the third is peculiar to Lakatos. Ad hocness entails an addition to theoretical structure which is made only to neutralize a falsifier and does not yield any "excess empirical content" = does not have any falsifiers of its own = does not yield any prediction of "novel facts". By Lakatos, non-ad hocness is called "theoretical progressiveness": a step in theory construction is theoretically progressive if it yields excess empirical content in the form of falsifiable statements. Ad hocness arises when none of the excess content got corroborated. By Lakatos non-ad hocness is called "empirical progressiveness". The first two types of ad hocness are closely related, since they both refer to empirical content, and Hands subsumes them in one more general type.

Ad hocness, which is peculiar to Lakatos, arises when an adjustment to theory or introduction of a new theory does not accord with the "heuristic" ("positive" and "negative") of the "research programme": if theories are patched up, disconnected, lacking coherence. Hands calls non-ad hocness "heuristic progressiveness". It is this notion of ad hocness and heuristic progressiveness that distinguishes Lakatosian from Popperian methodology, or so Hands claims.

Summing up: according to Lakatos there is progress when relative to a predecessor a theory entails novel predictions, some of which are sooner or later confirmed, \textbf{and} the theory is constructed according to the heuristic. Looking at the behavior of economists, Hands finds that methodologists tend to focus on ad hocness in the first two senses, while theorists tend to be preoccupied with ad hocness in the third sense:

"Thus, for economic theorists, the sin of ad hocness seems to be infidelity to the metaphysical presuppositions of the neoclassical programme rather than face-saving adjustments in response to recalcitrant data" (p.132)².

I propose that this is correct: it is corroborated time and again in observations of economists.

\textbf{The Neo-Walrasian Programme}

Weintraub (1985,1988) applied MSRP in a reconstruction of what he calls the "neo-Walrasian programme", which appears to be largely (though not quite) co-extensive with neoclassical economics. Propositions of the "hard core, positive and negative heuristic" are neatly specified. This attempt at clarity and explicitness is to be greatly appreciated. It establishes a firm platform for debate and criticism. The core propositions are: There exist economic agents; agents have preferences over outcomes; agents independently optimize subject to constraints; choices are made in interrelated markets; agents have full relevant knowledge; observable economic outcomes are coordinated, so they must be discussed with reference to equilibrium states.

The positive heuristic tells practitioners to construct theories in which economic agents optimize subject to constraints, and that make predictions about changes in equilibrium states. The negative heuristic tells practitioners not to test the hard core assumptions, not to incorporate irrational behavior and not to construct theories in which equilibrium has no meaning. Thus the heuristics simply underline the core statements. The negative heuristic in fact tells us to ignore bounded rationality and to stay away from psychology and sociology. General equilibrium theory (GET) is said to be an "instantiation" of the hard core, and "if the neo-Walrasian programme is roughly coextensive with neoclassical economics, GET shapes a research agenda for applied economic analysis" (1988, p. 213)³.

For its application to economics, Weintraub (1985) extended MSRP to include a second track of appraisal which is more in the nature of Lakatos’ "proofs and refutations", next to the track of tests of corroborated excess empirical content in the Lakatosian "protective belt". Much of the efforts of economists are expended on mathematical analyses, to explore possible implications of the core statements, by reinterpretations of their terms. Weintraub justifies this as a "hardening" of the hard core. These efforts should be evaluated according to their nature as mathematical analyses, as indicated in "proofs and refutations". This notion of "hardening" suggests two important things: that
programmes may have “soft”, i.e. less determinate, beginnings, and that in the process of development meanings of terms in core propositions may shift. This is important for the later discussion.

In Weintraub (1988) a progression of three articles on household decision making is considered as a case within the neo-Walrasian programme, to see whether it qualifies as an episode in Lakatosian theory construction, i.e. whether it satisfies the notions of theoretical, empirical and heuristic progressiveness. Weintraub’s conclusion is that to an acceptable degree of approximation it does, and he infers that the Lakatosian framework is useful for giving an account of the work of economists dealing with the problems of empirical research.

The methodological status of the accounts by Hands and Weintraub is ambiguous: are they to be seen as purely descriptive, or do they imply the claim that such conduct in economics is justified by Lakatosian methodology? Is MSRP used as an explanatory hypothesis: if we assume that economists act as if MSRP represents good conduct, we can explain much of their conduct, or is it used for a rational reconstruction: MSRP represents “good” conduct, therefore if economists act according to MSRP they are doing well? The ambiguity shows, for example, in Weintraub’s paper “The neo-Walrasian Programme is Empirically Progressive”, where the author concludes: “I have suggested, using the case study (of a series of three articles on household decision making), that there is if not justice, at least meaning in the claim that the neo-Walrasian programme is progressive in the Lakatosian sense” (Weintraub, 1988, p.224; italics and text in parentheses added). This leaves room for the disclaimer that we are dealing merely with an attempt to explain, but the paper as a whole strongly suggests justification.

**Criticism: On The Reading of Lakatos**

If the reading of Lakatos by Hands and Weintraub leads to conservatism, then before we proceed we should ask whether perhaps Hands and Weintraub have presented a misconstrual of MSRP. On the basis of what Lakatos himself wrote it will be shown that their reading of Lakatos is on the whole reasonable.

There can be no doubt that ad hocness, programme continuity and the importance of the heuristics were part of Lakatos’ exposition of MSRP. Hands’ notion of ad hoc3, for example, was not construed but picked up directly from Lakatos (1970, p.175, footnote 3). In Lakatos there is talk of “heuristic power”, “autonomy of theoretical science” and the “requirement of continuous growth” (1970, p.1/3). The question is whether, or to what extent, ad hoc3ness, continuity and heuristic power constituted not just a descriptive but a normative category for Lakatos. The textual evidence clearly points to an affirmative answer, as is demonstrated by the following quotes from Lakatos (1970). In the text, cursive printing is copied from the original, but underlining is added, to indicate evidence of a normative rather than only a descriptive view.

1) p.132: “...But the members of such series of theories are usually connected by a remarkable continuity which welds them into research programmes. This continuity – reminiscent of Kuhnian ‘normal science’ – plays a vital role in the history of science: the main problems of the logic of discovery cannot be satisfactorily discussed except in the framework of a methodology of research programmes.

2) p.175: “My account implies a new criterion of demarcation between ‘mature science’, consisting of research programmes, and ‘immature science’ consisting of a mere patched up pattern of trial and error’.

3) footnote 2, p.175: “Earlier...I distinguished, following Popper, two criteria of ad hocness. I called ad hoc1 theories which had no excess content over their predecessors (or competitors) that is, which did not predict any novel facts; I called ad hoc2 theories which predicted novel facts but completely failed: none of their excess content got corroborated.”

4) p.175: “Yet one may achieve such ‘progress’ (excess corroborated empirical content) with a patched up, arbitrary series of disconnected theories. Good scientists will not find such makeshift progress satisfactory; they may even reject it as not genuinely scientific. They will call such auxiliary hypotheses merely ‘formal’, ‘arbitrary’, ‘empirical’, ‘semi-empirical’, or even ‘ad hoc’.”

5) footnote 3 on page 175: “...We may call such hypotheses which are not ad hoc1, not ad hoc2, but still unsatisfactory in the sense specified in the text, ad hoc3. These three-unfailingly pejorative-usages of ad hoc may provide a satisfactory entry in the Oxford English Dictionary.”

6) p.175: “Mature science consists of research programmes in which not only novel facts but, in an important sense, also novel auxiliary theories, are anticipated; mature
science – unlike pedestrian trial and error – has ‘heuristic power’. Let us remember that in the positive heuristic of a powerful programme there is, right at the start, a general outline of how to build the protective belts: this heuristic power generates theautonomy of theoretical science). This requirement of continuous growth is my rational reconstruction of the widely acknowledged requirement of ‘unity’ or ‘beauty of science’.

Finally, consider this heavy stuff:

7) p.176: “...It (MSRP) hits patched-up, unimaginative series of pedestrian ‘empirical’ adjustments which are so frequent, for instance, in modern social psychology. Such adjustments may, with the help of so-called ‘statistical techniques’, make some ‘new’ predictions and may even conjure up some irrelevant grains of truth in them. But this theorizing has no unifying idea, no heuristic power, no continuity. They do not add up to a genuine research programme and are, on the whole, worthless.”

8) footnote 1 on p.176: “...nothing but an increase in pseudo-intellectual garbage...It seems to me that most theorizing condemned by Meehl and Lykken (in social science) may be ad hoc3. Thus the methodology of research programmes might help us in devising laws for stemming this intellectual pollution which may destroy our cultural environment even earlier than industrial and traffic pollution destroys our physical environment.”

Item 1) can still be interpreted as a purely descriptive statement concerning programme continuity, but the following items hardly permit such a reading, and their cumulative evidence (indicated by underlining) clearly indicates a normative intention for the category of adhoc3ness/heuristic power/continuity. For the opposite reading that this category was intended only in a deceptive sense, one would have to maintain that the many value judgements are purely ascribed to practising scientists and do not reflect Lakatos’ own normative opinions. Apart from the fact that such a reading of the above texts is hard to defend in itself, it would go against Lakatos’ expressly stated purpose to provide a normative account, which Lakatos (1970, p.177) contrasted with what he saw (erroneously, in my view) as a merely descriptive account by Kuhn (1962).

Points 1), 3), 4) and 5) support the claim by Hands that the distinctive feature of MSRP is the addition of the category of adhoc3ness to the Popperian categories of adhoc2ness and adhoc1ness, and Lakatos himself saw it this way.

However, the present author has not been able to find the term “heuristic progressiveness” in Kuhn’s writings. In the use of this term, Hands (1988, p.127 and footnote 24) no longer referred to Lakatos but to its introduction by Zahar (1983), on the argument that “...his (Zahar’s) statement is more straightforward than Lakatos’, which must be extracted from his discussion...”. This “extraction” has added a connotation that is absent in Lakatos, who reserved the term “progressiveness” for the generation of excess empirical content. But the important point is that this does not eliminate the conservativism of programme continuity in MSRP. Programme continuity may not constitute evidence of progressiveness, as intended by Lakatos, but it is a condition that theories must satisfy before they are admitted into the protective belt for evaluation of progressiveness, i.e. corroborated excess empirical content. This makes matters worse, not better: programme continuity is not a condition on an equal footing with empirical progressiveness, but has logical and temporal priority. The following paragraph analyses the conservatism of MSRP more in detail.

The intention here is not to discredit the notion of theoretical coherence as a normative category, but merely to demonstrate that for Lakatos it indeed was a normative category, and subsequently to show that in combination with the rest of MSRP it makes for conservatism, in economics. The thesis here is not that Weintraub’s reconstruction is entirely in line with MSRP, but that to a large extent the conservative drift of it is supported by MSRP.

The Roots of Conservatism

It was clearly Lakatos’ intention to promote progressiveness, while recognizing the inevitable conventional elements of criticism, and the rationality of theoretical tenacity. He thought he had found this in the notion of corroborated excess empirical content. On the other hand, he added the criterion of continuity, as an improvement on Dullem’s criterion of simplicity, to rule out a disconnected “tacking on” of hypotheses. This idea was worked out in the notion of a research programme, where coherence is achieved by theory construction on the basis of the core, according to the positive and negative heuristic. It will be argued that MSRP is either inherently conservative, or breaks down.

As indicated before, in case of theory development within a programme, clearly
programme continuity or non-adhocness is temporally and logically prior to the other forms of non-adhocness: a theory has to be first developed from the core according to the positive and negative heuristic, before it is admitted into the protective belt for tests of excess empirical content. Thus the conservatism of programme continuity is the dominating because prior criterion in theory development. This we call the problem of heuristic preselection, which is the first root of the conservatism of MSRP.

Lakatos claimed not only that a theory is to be surrendered if there is a successor with corroborated excess empirical content, but also (1970, p.116) that "...a theory (is) acceptable or scientific only if it has corroborated excess empirical content over its predecessor". According to Lakatos "This condition can be analysed in two clauses: that the new theory has excess empirical content ("acceptability") and that some (close) of this excess content is verified ("acceptability2").

But there is a third underlying clause that must first be satisfied: the theory must have a 'predecessor', with which it is to be compared. What is that, where does it come from, how is it identified as a (the?) proper predecessor, and who does the identifying? Is theory T* accepted as a potential successor to theory T if it is not derived from the core of T's programme? Can it claim the right of comparison if it originates from a different programme? What is the immigration policy of MSRP? Lakatos wrote little or nothing about this issue, but what he did write strongly suggests that succession takes place, or is even defined, only within programmes. In the absence of an immigration policy, there appears to be no immigration. Apart from the answer that Lakatos might have given, this issue presents a dilemma for MSRP. If candidates for succession are accepted from outside the programme, nothing is left of programme continuity, but if not, there is conservatism due to cross-programme discrimination: candidates from other, competing programmes are excluded a priori. This we call the problem of succession.

Lakatos (1970, p.116) specified two conditions for excess content: the successor theory should contain all of the unrefuted content of the predecessor, plus predictions of facts that are novel, i.e. "facts improbable in the light of, or even forbidden by (the predecessor theory)". In other words: the intersection of contents should be a proper subset of the content of the challenging successor. Suppose that the challenger does not contain some of the content of the predecessor but does have content that the predecessor does not have. How does one trade off lack and excess of content? This we call the problem of balancing content. Lakatos' formulation suggests that the predecessor theory could maintain its incumbency for as long as the successor theory has not supplied all of the incumbent's content.

Suppose, furthermore, that judgements on relevance, degree of refutation and 'probability in the light of theory' differ from the perspectives of the contending theories? This we call the interpretative problem. As admitted by Lakatos, there is no clear demarcation, regardless of context, between theoretical and observational statements, since observations rely on underlying 'observational theories'. But then we may run into the interpretative problem: different theories may yield different empirical judgements, and hence different judgements on non-refuted content, and hence different judgements on excess content. Note that the burden of proof lies on the successor theory: the incumbent theory maintains its position until the entrant has demonstrated excess content. Until the entrant has succeeded, it remains "unacceptable" and "unscientific". There now is a strong and a weak form of the interpretative problem. In its weak form the problem arises only between programmes: within a programme, due to continuity, predecessor and successor share the same observational theories and judgements of relevance. In its strong form the problem arises also within a programme. But note that even in its weak form, the problem of cross-programme conservatism is increased. The problem can be restricted to its weak form only under certain conditions of meaning invariance to which we return later.

Furthermore, there is a problem in the development of a successor theory, which we call the development problem. Lakatos (1970, p.133, note 4) noted that: "The hard core of a programme does not actually emerge fully armed like Athene from the head of Zeus. It develops slowly by a long, preliminary process of trial and error. In this paper this process is not discussed". The development problem now is that to the extent that it takes time for a successor theory (or programme) to develop before its empirical content can be decided, it is entertained in spite of not having shown excess empirical content over established theory, by which according to Lakatos' words it is "not acceptable or scientific". Who is willing to carry such a stigma, and for how long? This problem also has a weak and a strong form: with a lag between inception and delineation of content only in programmes, or also in theories within programmes.
A well known problem of MSRP is that the criterion for diagnosing degeneration of a programme, by the absence of new theories within the programme that yield excess empirical content, has little bite because corroboration of excess empirical content need only occur “intermittently”, while no frequency or term for this is specified: lack of corroboration may be accepted for “long” periods of time. This is the problem of postponed corroboration.

Meaning Variance

Do the core statements of the “neo Walrasian programme”, as proposed by Weintraub, satisfy Lakatos’ intentions? That is hard to judge, since no clear criteria for such statements have been provided. A specific issue is what room is allowed, in theory construction, for reinterpretations of terms in core statements, before a break of continuity must be acknowledged. Weintraub is justified in claiming some room for this: the core could hardly form the basis for theory construction if meanings were rigidly fixed. As words move from one context to another there is bound to be some shift of meaning (unless we are prepared to regress to some essentialist theory of meaning).

In the series of articles of the case study in Weintraub (1988), attention shifts from independent choice of individuals via choice of collectives (households) to interdependent choice of individuals (within households). In a discussion on Weintraub (1988; de Marchi, 1988, pp.47-51), a discussant (Birner) noted that this involves reinterpretations of terms in the hard core propositions that are so basic as to throw severe doubts on the claim that the different articles belong to the same programme. If core propositions allow for such width of interpretation that one can move from independent choice to its negation = interdependent choice, how “hard” can the core still be said to be? Another dilemma presents itself here. On the one hand, if meanings are allowed to vary, what significance is there left of the hard core? On the other hand, if meanings are fixed, how can there be development? As indicated before, Lakatos acknowledged that the hard core of a programme “does not actually emerge fully armed from the head of Zeus”, and Weintraub spoke of the “hardening of the core”.

The criticism is germane and difficult to answer in general terms. Such an answer would require a theory on continuity and discontinuity of meaning. In view of difficulties of synonomy (cf. Quine’s well known explorations in this area) no attempt will be made to provide such a general answer here.

In this particular case Birner appears to be right, but only at first sight: some continuity of meaning can be identified. In the transition from independent decision making to decision making in the setting of game theory, decision making indeed becomes interdependent in a sense, but methodological individualism is maintained, and it is probably that which constitutes the intended meaning of the core statement (its intended meaning is the injunction to keep out sociology, and the applicants of game theory in economics usually obey). The core of “independent decision making”, as no doubt intended by Weintraub, is that preferences are given: autonomous; not subject to influence by others or by the decision making process.

But suppose Birner is right, and a clear break of meaning occurred in the core statement on independent decision making. The other core propositions and heuristics were strictly maintained. How does one balance several continuities and one discontinuity (supposing there is one)? Is Wittgenstein’s concept of family resemblance acceptable as a principle of programme continuity, by which two stages that do not directly share any interpretation but are linked by a succession of stages where each link has at least one interpretation in common with the preceding link? Why would this not be permissible? We can only conclude that there can be no more rigorous criteria of continuity than criteria of sameness of meaning, which are notoriously indeterminate. This is the problem of meaning variance.

As a result of meaning variance one may claim continuity even when it seems dubious. This can be used conservatively, but perhaps also progressively, in a Trojan horse strategy: a imported break with the programme can perhaps be sneaked in under the pretense of a mere shift of meaning. If meanings shift in within-programme development, this will in principle lead to the strong form of the interpretative problem: judgements on excess content may differ from the perspectives of different theories within a programme. The implication of the problem further is that MSRP does not offer the rigour of logic but a more diffuse order which is not more rigorous than the order of meaning: some theories may be more continuous with the core than others; even theories from other programmes may show some continuity. I welcome this, since it yields a perspective for cross-programme criticism, but it destroys the rigour of MSRP, and probably of methodology in general. I see no reason to regret this.
Evidence of Conservatism

In the building of our thesis that with Lakatosian excuses theoretical tenacity has degenerated into conservatism, we should perhaps produce evidence for the thesis that in mainstream (neoclassical) economics conservatism now prevails. We will indicate a few salient problems.

Most important, from a Popperian, if not from a Lakatosian perspective, is the persistence of anomalies, which to a greater or lesser extent are empirical, which have been established and have remained unsolved for a considerable period of time, and which are generally ignored by economists while they can be explained from psychology or sociology.

One of these anomalies is the phenomenon of preference reversals, as analysed from a methodological point of view by Hausman (1990). The phenomenon is that people systematically reverse their preferences for gambles according to whether they choose between alternatives or assign values to them. Hausman shows that although the phenomenon is not ignored totally by economists, and a few attempts have been made to solve the problem from within the perspective of established theory, overall concern with it is extremely limited, in spite of the pervasiveness of the phenomenon (established by psychologists) and failures to explain it away in a fashion that is consistent with neoclassical axioms.

A second anomaly is Arrow’s paradox of majority voting, which yields a aggregation problem in the assumption of transitive preferences. The logic of the situation is that when there are reversals in the order of preferences between different agents, then aggregate preferences established by majority voting can be intransitive. More seriously, because of the implications for microeconomics, the logical equivalent of this can occur on the micro level of the individual agent if utility has different dimensions, with preference orderings in each, while the dimensions are “aggregated” according to the logical equivalent of majority voting (one chooses the alternative which is preferred in most dimensions). For an application of this to the selection of shops by consumers, see Nooteboom (1984). A problem in the empirical identification of intransitive preferences is that in markets it manifests itself in erratic or perhaps random choice behavior which may also be consistent with indifference rather than intransitivity, and this is what adherents to established theory can and do claim. In experiments, however, intransitive preferences are clearly identified as such (choices between marriage partners where orderings are reversed across properties such as wealth, looks and kindness).

A third anomaly is exhibited in conflicts between self-interest and moral duty, and in phenomena of “weakness of the will”. Examples of the former are bravery in war, sacrifices for friends: actions are performed which cannot plausibly be ascribed to self interest. An example of the problem of weakness of the will is the inability of addicts to quit while they want to. A complication is that formally a reconstruction in terms of self-interest and utility maximisation is always possible. Church visit has been accounted for as the maximisation of the present value of eternal life. It appears that for any behavior one can construe a utility function such that its maximisation “saves the phenomena”. Some consider this to constitute a compliment for established theory, while in methodological fact it constitutes the weakness of unfalsifiability. At the time of Copernicus and Galileo the phenomena could be saved from the perspective of established theory by means of epicycles upon epicycles, and we still consider that as one of the major methodological lessons of history. As argued by Ezioni (1988) the question is whether much behavior cannot be explained more straightforwardly and more simply on the assumption of a basic tension between self-interest and moral or social duty or decency.

In mainstream theory entrepreneurship is paradoxical. Before equilibrium is reached entrepreneurship is profitable in the arbitrage of discrepancies between supply and demand. But if we consider equilibrium, as according to Weintraub’s neo-Walrasian programme we should always do, entrepreneurship is an enigma: why he an entrepreneur if marginal revenue equals marginal cost? In fact, entrepreneurs will seek to disturb equilibrium by product differentiation or innovation. Thus it is hard to reconcile equilibrium with entrepreneurship, while the latter fortunately is a pervasive phenomenon.

Rationality

Probably the most significant anomalies lie in phenomena of lack of information and bounded rationality. The significance lies in the fact that information is acknowledged, in mainstream economics, to be crucial. The superiority of the market over central planning lies in the fact that the demands on the gathering and processing of information that would have to be satisfied by central planning, while maintaining sufficient speed and scope of arbitrage between supply and demand, are impossible to satisfy, whereas in a market
economy price contains all the information needed for a decentralised decision making in which private vices become public virtues. However, imperfections in the mechanisms of real markets impose more exacting demands on the information and understanding of economic agents.

Mainstream theory claims to have accounted for the problem of limited information by including the costs for its search in the maximisation of utility. But this play runs into Arrow’s paradox of information: If one does not have information one thereby does not know its marginal utility, so that maximisation cannot take place. A way out of this might be to assume either that marginal utility can reliably be inferred from own experience with similar information in the past, or can be obtained from others who already possess the information. The first choice retreats from the challenge that the information is new, and the second leads to the surrender of methodological individualism and from there to institutionalism and sociology.

From a different angle, the problem is that of fundamental uncertainty in the sense of Knight: Maximisation is foreclosed if the set of options and/or the sets of outcomes of options are not closed; if one does not know what options there are or what outcomes options might yield. Such uncertainty is an obvious fact, particularly in present conditions of rapid technological change (Hodgson 1989, Nooteboom 1992a).

According to Popperian methodology anomalies are sufficient by themselves to lead to the rejection of a theory. According to Lakatosian methodology one holds on until a better alternative presents itself. A problem here is, of course, that nothing new ever simply presents itself: it must be developed at the cost of much effort and ingenuity. In any case, there are alternative perspectives, in epistemology, psychology and sociology, from which the anomalies can be explained and understood. The argument that is often used by economists against considering such alternatives is that they are too casual and analytically too loose: there is lack of rigorous, conceptual grip and coherence.

But there is a reasonable view of theory construction which states that one usually begins with an unordered inventory of partially interpreted phenomena, to proceed to more structured “middle range” theories, to ultimately achieve some degree of formal, coherent, sparse axiomatic reconstruction. One can argue that this is how neoclassical economics developed. Lakatos admitted that his criteria applied to “mature” theories. But then it is unreasonable, and a sign of dogmatism, to demand that emerging theory be as formal as established theory, because it requires maturity at birth: Athena from the brow of Zeus after all.

Confronted with the choice between on the one hand conceptual, formal grip together with lack of empirical grip and a number of important unexplained anomalies, in mainstream economics, and on the other hand the combination of rich but fragmented empirical explanation and lack of axiomatisation, in an admittedly incoherent field of psychology, sociology and motley bands of unorthodox economists, the profession in great majority tacitly chooses the former. That is predictable and understandable, but not thereby justifiable. The description of what is happening reads almost as a definition of dogmatism.

As argued by Hodgson (1988), there lies at the core of mainstream theory a fundamental problem of epistemology: what is knowledge and how is it acquired? Implicitly, mainstream theory is pre-critical in the philosophical sense, in that it appears to espouse an empiricist epistemology: knowledge arises passively and objectively, by some association of sense impressions. As a result, different epistemological subjects (consumers, firms) have objective and thereby equal knowledge. The formation of knowledge therefore is not an issue. Preference formation is not an issue either, though this appears to have an ideological rather than an epistemological basis: liberalism is based on the autonomy and supremacy of the individual, and preference formation should not be considered lest it lead to authoritarian manipulation.

A post-critical epistemology is more consistent with observations in psychology and sociology. Perception and evaluation are conditioned by mental categories that are developed and acquired intersubjectively, i.e. socially. Such categories both enable cognition and provide obstacles for it. This is what Hodgson (1988) called the dilemma of the rationalist: to be rational we require categories, which limit our rationality. To change one’s knowledge, i.e. to learn, one must shed or transform categories that may have become second nature. Although the intersubjectivity of knowledge and language ensures similarity of categories, and hence a basis for communication, categories are not intersubjectively identical, and vary with both inherited endowments, experience and socio-cultural conditions. There is a world of thought here which mainstream economic theory cannot absorb and which it therefore chooses to ignore, since it is not continuous with the core of its programme. For further explorations of the implications for economic theory, see Nooteboom (1992a, 1992b).
The Inadequacy of MSRP

The descriptive or explanatory merits of MSRP are considerable; it is much better able to explain the behavior of economists than Popperian methodology. It can without undue distortion be used to explain the theoretical conservatism of economists, on the basis of nonad hocness, "heuristic power" or "programme continuity". Its descriptive adequacy is further improved by Weintraub's addition of the method of conjectures and refutations for an extended explanation of the widespread preoccupation of economists with mathematical analysis.

Normatively, MSRP is unacceptable because it is used, and can legitimately be used for a rationalisation of conservative programme continuity, while in economics insufficient countervailing power can be offered to prevent degeneration into theoretical conservatism. Thus MSRP can legitimately be used to rationalise conservatism, even if that is against the intentions of Lakatosian methodology. The potential excesses of conservatism are rooted in the problems of the heuristic preselection of theories, the succession of theories, the balancing of empirical content, the interpretation of content, theory development, postponed corroboration and meaning variance, as discussed previously. These problems realise their conservative potential particularly in economics.

A familiar problem in economics is that the "distance" between the core statements and empirical testing of "excess empirical content" is "too large", and the protective belt is far too wide and cushy. Economic theories (in neoclassical economics anyhow) are so general as to require many subsidiary assumptions, to make the theoretical structure more determinate, to construct an empirical model, to bridge the gap between theoretical variables and available statistics, and to construe relevant facts, before a confrontation between implications and facts can be made. Perhaps one can argue that in this respect the Lakatosian reconstructions do not satisfy Lakatosian standards.

This issue is hard to decide: how wide and loose does MSRP allow the protective belt to be? Lakatos did allow considerable scope for defensive actions in the protective belt, by means of an "appeal court" where there is "no reason why we should not regard a falsifying hypothesis - and the basic statement it supports as being just as problematic as a falsified hypothesis" and where it is in particular the interpretative theory in the light of which the truth value of the falsifying hypothesis has been established that is the object of questioning (Lakatos 1970, pp.122, 12f). But what if that interpretative theory comes from outside the research programme? Will there be no discrimination against foreigners?

The situation is problematic due to the well known Duhem-Quine problem. When falsification arises (i.e. a falsificatory hypothesis is accepted), it is not clear what part of the overall testing system is to be blamed. As a result there is scope for "selective falsification": the blaming and replacement of an inessential side assumption, without touching the basic theory. As indicated previously, the problem is that in economics, or neoclassical economics at least, there seems to be no end to such opportunities. It looks as if there is no phenomenon in society (and even outside of it) which could not be explained in terms of profit maximization under full information, if only one is clever enough in specifying the appropriate, to side assumptions. If you want to oppose neoclassical theory, then it becomes like fighting the mafia: how many lower ranking soldiers do you have to arrest before you even come close to the godfather?

A related normative problem lies in the old truism that one can falsify but not prove a theory (modus tollens; the problem of induction). There is, in other words, a problem of "theoretical multiplicity" or "indeterminacy" (Quine); if a theory explains and/or correctly predicts a phenomenon, there may still be any number of alternative theories which explain or predict it equally well. Remember that it was this problem that led Popper to propose the falsification principle as the overriding principle of methodology. In Lakatosian methodology the problem appears to fall from sight.

The present objection to the criterion of programme continuity can now be restated as follows: it legitimizes selective falsification and diverts attention from the issue of theoretical multiplicity. Now we will demonstrate by means of a few examples why this is a serious issue. The examples also illustrate the "interpretative problem", and the application of considerations of plausibility in the choice between multiple, empirically equivalent theories.

An Example of Theoretical Multiplicity

The empirical equivalence of neoclassical profit maximization and evolutionary theory is a well known case of theoretical multiplicity. In the "single exit" situation (Latsis, 1976) that firms can survive only at maximum profits, random decisions plus environmental selection (Latsis: "situational determinism") yield the same outcome as neoclassical profit maximization. But the simulation studies of Nelson & Winter (1982) show that also
in “multiple exit” situations a variety of decision rules plus a selection environment with a bias towards profits or growth has outcomes which are empirically equivalent to those of neoclassical theory. What in the interpretive context of neoclassical theory is seen as evidence of profit maximisation is seen as evidence of selection in the interpretive context of evolutionary theory. Given empirical equivalence, plausibility will favor the evolutionary interpretation, since it makes fewer unrealistic demands on the rationality of agents (availability of information and capacity to absorb, understand and process it). These demands are unrealistic not only according to common sense or introspection, but according to an assessment of market conditions, theoretical and experimental psychology and epistemology.

But here we consider another case which, though hardly new, appears to be less well known. It is a well-known fact that labor productivity correlates consistently with the wage rate relative to the price of output. The standard approach, in neoclassical production theory, is to interpret the coefficient in a loglinear regression of productivity on (relative) wage rate as the elasticity of substitution between labor and capital. The policy implication is that if employment is a policy target, the wage rate should be restrained to prevent further substitution by capital. There are, however, at least two alternative interpretations, which are consistent with fixed input-output coefficients (yielding no direct effect of wage rate on employment). The first is that labor is not homogeneous, and that there are wage differentials to compensate for differences in productivity, or, in a time series study: wages are adjusted to productivity growth. The second is that prices are set as a mark-up on costs. The first alternative is obvious. The empirical equivalence of neoclassical production theory and mark-up pricing is demonstrated below.

In formulae, the situation is as follows: According to mark-up pricing, price is set as a mark-up on costs (actual or “normal”), so that:

$$\pi q = (1+\mu)(\alpha a + \beta k)$$ (1)

where: \(\pi = \text{price};\)
\(q = \text{volume of output};\)
\(\mu = \text{percentage mark-up};\)
\(\alpha = \text{wage rate};\)
\(a = \text{labor volume};\)
\(\beta = \text{price of capital};\)
\(k = \text{volume of capital};\)
all coefficients (Greek symbols) > 0.

Rearrangement of terms yields:

$$\frac{q}{a} = (1+\mu) \left[ 1 + \frac{\beta}{\alpha a} \right] \frac{\alpha}{\pi}$$ (2)

which reads: labor productivity is proportional to relative wage rate, with the coefficient of proportionality a function of the capital/labor ratio.

Compare this with the marginal equation for labor under neoclassical profit maximization subject to a constant-elasticity-of-substitution (CES) function with constant returns to scale:

$$\log \frac{q}{a} = \tau_0 + \tau_1 \cdot \log \frac{\alpha}{\pi}$$ (3)

which reads: labor productivity has an elasticity of \(\tau_1\) with respect to the relative wage rate.

In neoclassical production theory, coefficient \(\tau_1\) is interpreted as the elasticity of substitution. If \(\tau_0\) is found to be unity, this indicates the Cobb-Douglas production function.

In (3) there is no effect of the capital-labor ratio \(k/a\), but such an effect appears in a generalization with a variable-elasticity-of-substitution (VES) production function. For a Liu and Hildenbrand function, for example, one finds the following marginal equation (for details see Nootboom, 1980, Appendix A, p.A4; Revankar, 1971):

$$\log \frac{q}{a} = \tau_0 + \tau_1 \cdot \log \frac{\alpha}{\pi} + \tau_2 \cdot \log \frac{k}{a}; \text{coefficient}>0$$ (4)

which reads: labor productivity is loglinear with respect to relative wage rate and the capital/labor ratio.

In neoclassical production theory, coefficients \(\tau_1\) and \(\tau_2\) are associated with the elasticity of substitution (which varies with the capital-labor ratio \(k/a\)).

With a Kadiyala VES function (see Kadiyala, 1972) we would have:

$$\frac{q}{a} = \phi_0 \left[ \phi_1 + \phi_2 \left[ \frac{a}{k} \right] \right]^{\phi_3} \cdot \left[ \frac{\alpha}{\pi} \right]^{\phi_4}; \text{coefficients}>0$$ (5)

which is a generalization of (2), and hence of (1).
but of course with an entirely different interpretation of the coefficients. Coefficient $\phi_1$ is associated with the (variable) elasticity of substitution.

If (1) with its mark-up interpretation were true, there would be a tendency, in the interpretative context of (3), (4) and (5), to find a unit value for parameters $\tau_1$ and $\phi_1$, with the conclusion, in the context of neoclassical theory, that the elasticity of substitution is unity (in case of (3)), or varies around unity (in case of (4) or (5)). This might explain the ubiquity of the Cobb-Douglas function in neoclassical empirical work. In fact, of course, given the assumption that (1) is true, there might not be any opportunities for substitution at all.

Conversely, of course, if the neoclassical interpretation were true an unorthodox economist might erroneously interpret substitution as evidence for mark-up pricing.

Econometricians tend to take the neoclassical interpretation as given, and to search for the production function that fits best, with increasingly sophisticated specifications of substitution, going from the Cobb-Douglas to the constant-elasticity-of-substitution function (CES), and next to one form or other of a VES function. In a Lakatosian reconstruction this would no doubt emerge as an episode which is theoretically and empirically progressive, since each step would add empirical content, which is corroborated first by the finding of elasticities of substitution unequal to unity and next by statistically significant effects of the capital-labor ratio (corroborating variable elasticity of substitution). The episode would also satisfy programme continuity, since it implements the core assumptions of neoclassical doctrine and achievements in its protective belt.

Such behavior of econometricians is easily recognized, and accords with the description given by Morgan (1988). The majority of econometricians apparently do not see it as their job to put alternative interpretations on the agenda, if those are based on theories outside the established paradigm. This is in agreement with the non-adherence in the third sense = programme continuity which Hands found to be the main driving force of economic theorists (in neoclassical economics). But the point is that there appears to be no limit: one can always specify yet another production function to fit the data and save the theory. No doubt excess empirical content will be produced in the process. Meanwhile alternative interpretations, with different and perhaps conflicting policy implications, are not considered, let alone subjected to debate. It is foul play to state, or even to suggest, that there is only one interpretation to phenomena, with attendant policy implications, while in fact there are others, due to theoretical multiplicity.

What would the criterion of plausibility do in this case? In many instances lack of substitutability among resources, in view of the extant technology and/or the time scope considered, will plead for the mark-up interpretation. In view of the likely lack of information on price elasticities of demand, the rules of thumb implied in mark-up pricing appear to be more realistic than profit optimization.

No doubt acceptance of the mark up interpretation would cause a break of continuity with respect to the core of neoclassical economics. What does/should one prefer? Like the majority of economists, Lakatos would probably choose to adhere to the neoclassical ways. I would not. Given empirical equivalence I would choose the theory with the most plausible assumptions.

Given a chance, this theory may develop more coherence, either within its own assumptions or with psychology or sociology. If that takes us away from economics to something else, a new discipline perhaps, what is wrong with that? Is it really useful and warranted to persist so tenaciously in the analysis of behavior from the single minded perspective of rational, individualistic maximization of utility under given preferences, when it is so blatantly and admittedly in conflict with behavior as we know it? Are social and moral/ethical dimensions; problems of perception, understanding and communication; shifts of technology and preferences; entrepreneurial disturbance of equilibrium, and ensuing uncertainties (in Knight's sense) so secondary and independent that they can somehow be "added" or "mixed in" subsequently, after the economic analysis is done, in the art of "translation" from economic analysis to policy? Or would it be more fruitful to recognize the inherently social and dynamic nature of entrepreneurship, perception, knowledge, preference and meaning? This leads to issues of plausibility and cross-programme criticism.

**Plausibility and Cross-Programme Criticism**

Elements of MSRP may survive in future methodology. The normative validity of theoretical tenacity and of evaluation in the light of alternatives remains. But fewer escapes should be allowed into theoretical conservatism. Attention must be demanded for possible alternative explanations, even if they originate from other programmes.

Also, if there is lack of empirical grip on the content of theories, we cannot afford to ignore
criticism of the plausibility of assumptions, as Friedman advised. Remember that Friedman justified his instrumentalism on the argument that success or failure of predictions provides an adequate selection device. This argument fails due to the lack of empirical grip. In fact, as is shown for example in a survey of economic practice by Lind (1990) economists do not behave in this instrumentalist fashion. Many economists could not care less about empirical predictions, let alone their success.

Also, while considerations of plausibility may not be feasible or meaningful in the natural sciences (from whose methodology Friedman uncritically adopted his methodological views), in economics the situation is very different. When we make hypotheses about behavior, these can be tested directly by (going from weak to strong) introspection, direct observation of economic behavior, clever interviewing and behavioral experiments. Thus there is no fundamental limitation to indirect testing of implications, as there is in natural science. Psychology and sociology are available as auxiliary sciences of behavior.

See Nooteboom (1986) for a more extensive discussion of the role of plausibility in economics. Plausibility is interpreted as coherence with established, i.e. intersubjectively acceptable experience and knowledge. The underlying theory of truth is a theory of coherence within knowledge (cf. Quine’s “seamless web” of knowledge) rather than reference to some mysteriously knowable external reality. Admittedly, the notion of “coherence” requires further development.

The totality of knowledge within which coherence is sought includes “knowing that” and “knowing how”, and has several dimensions or levels: experience with and conventionally acceptable propositions about (perceived) reality, theories about reality, theories and methodological norms concerning theories, and ethical views concerning such norms. “Established” or “intersubjectively acceptable” knowledge is never absolute but always contingent and context dependent. Cf. Neurath’s parable, quoted so often by Quine, of the mariner who repairs his boat plank by plank while staying afloat in it. What at one moment, in one context, is taken as the basis for criticism, may next itself be the focus of criticism.

The Scope of Coherence

Admittedly, plausibility does not provide a sharp criterion, but it does supply an entry to critical discourse. In the seas of global knowledge there may be many places to anchor. One story may cohere better with this part of knowledge, and another story with that, and one’s preference may depend on circumstance.

The notion of coherence may be seen to lead us back to programme continuity, which was introduced to eliminate patchwork, fragmentation, lack of coherence. Programme continuity is a form of localised, bounded coherence, restricted to a plot of knowledge staked out by some school of thought. Programme continuity may be required to achieve local progress, but is in danger of preventing progress writ large. One’s mind should remain open to a recognition of limits and new opportunities. Will... a school of thought there may be room for a division of labor: there may be dogmatic defenders of the purity of the faith, as long as they do not shut up the doubters and innovators.

My objection to programme continuity is that it raises a short term tactic to a strategic principle. In respect of coherence it does not go far enough. If it is simplicity in the sense of coherence we are after, why stop at continuity with a single set of core assumptions of a programme that inevitably is only one of many, in the totality of experience. In other words: implausibility of theoretical assumptions, in the sense of inconsistency with established knowledge, may be seen as ad hocness in a fourth sense, which is a generalisation of ad hocness in the third sense. Plausibility is continuity taken to its logical conclusion.

Concerning cross-programme criticism, consider the following quote from Lakatos (1970, p. 122): “The honesty of sophisticated falsificationism demanded that one should try to look at things from different points of view, to put forward new theories which anticipate facts, and to reject theories which have been superseded by more powerful ones”. We plead for such open mindedness; not only within but also between programmes.

This is necessary in view of the successor problem: to admit candidates for succession from outside the programme. In a confrontation between a mature programme and immature criticism such open-mindedness requires, in view of the development problem, that one does not demand that emerging competing views constitute a finished programme with full axiomatisation and programme continuity. In view of the interpretative problem, it requires in particular that in front of an “appeal court” the interpreting theory underlying a falsification of established theory is not required to satisfy heuristic progressiveness within the programme; i.e. to be derived from the core of that programme. In view of the theory ladenness of
observation that would restrict empirical evidence to that which is preselected to fit the theory.

On the other hand, in view of the problem of meaning variance, if discontinuity with respect to theory is allowed, where does one draw the line of programme continuity? It seems that no matter how you turn it, MSRP cannot be made to work without either self-destructing or winding up in conservatism.

A further question is whether it is at all possible to achieve a cognitive and a communicative basis for cross programme criticism. From a post-critical epistemological perspective a research programme may be just another, though “high-level”, category for perception and interpretation which, like any such category, enables us to think while at the same time providing an obstacle to thinking differently. In other words, we may wind up with Kuhn’s problem of incommensurability. That problem, however, seems exaggerated. Full incommensurability, precluding any meaningful debate on conflicting interpretations of shared facts, is conceivable in a science fiction of intelligences with different histories of evolution, but is not endemic in a common world with a common evolution and some sharing of culture.

There is some common ground for communication at least on the level of ordinary language and daily life. If a quarrel arises concerning interpretation, there is always some level where agreement can be achieved, but this may be far removed from the core of one’s pet programme. On the level of common knowledge we can have some meaningful critical discussion in terms of plausibility. This may not carry any weight in natural science, but in social science it does.

The Vagueness of Normative Order

When plausibility is proposed as a normative category, the response invariably is on the one hand that of course we all employ it, but on the other hand that it is not acceptable as a normative criterion because it is too vague or indefinite “to do any real work”. “Real work” is logic, and beyond that lies the arbitrary; the chaotic. In other words, we are all routinely doing something which is disreputable. But I propose that we need an order that allows for shifts of meaning and interpretation. Language provides such a flexible order, which allows for creative shifts of meaning, as in poetry, while yet it constitutes a genuinely normative intersubjective order.

This leaves us with a methodology which is not in the nature of a recipe or a set of laws of logic. In the image offered by Blaise Pascal: it belongs not to the “esprit de géométrie” but to the “esprit de finesse”. Normative order in the “esprit de finesse” is, also found elsewhere. In legal judgement and in judgements of sense in language, Methodology is in the nature of a grammar. By this metaphor I wish to indicate that the order of methodology is linguistic rather than logical: it provides a basis for argument rather than criteria for instant judgement; it does not yield definite necessary and sufficient conditions for sciency but exemplars that guide judgement; not a universal rule but a set of criteria whose scope and weight depend on the context, not an immutable order but one which is shifted by experience. We were mistaken to ever expect otherwise.

We noted before that because of the problem of meaning variance, MSRP cannot be more rigorous, in its concept of programme continuity, than the notoriously indefinite criterion of synonymy. If this by itself is not an obstacle for taking MSRP seriously, the indefiniteness of plausibility should not be an obstacle either.

Note that I am not substituting rhetoric for methodology, as McCloskey and Klammer appeared to be doing, but that I am saying that methodology is to provide a normative order which resembles linguistic order rather than rigorous rules or laws of logic. But perhaps the distance from McCloskey’s more recent views is not so great. In earlier publications there was a strong rejection of normative methodology, but later (see McCloskey, 1989) McCloskey accepted the normativity of “Sprachethik”, while focusing his criticism on the maintenance of “middle ground methodologies”. The latter appear to be close to the core assumptions of a Lakatosian programme, and their protection by programme continuity, which I criticize. My proposal of debate in terms of plausibility, to bring subsidiary assumptions into focus, and to seek wider coherence, may be close to the “Sprach-ethik” intended by McCloskey.

In my view coherence theory and Habermas’ discussion theory of truth (“Theory of communicative action”, see Habermas, 1982, 1984) come together: coherence in the sense of continuity with different areas and levels of knowledge is not given but is the outcome of (and at the same time constitutive of) “Diskurs”. This is not to say that consensus equals truth or that consensus will or always can be reached. I am not as positive about either the possibility or even the desirability of consensus as Habermas appears to be, but this forms no excuse to stop trying tenaciously.
Note:

1. As pointed out by Hutchison (1988).

2. Incidentally, this clearly normative statement contradicts the popular thesis (espoused also by Lakatos) that Kuhn's treatment was purely descriptive, as part of a "mere" sociopsychology of science.

3. This view can be further supported by the "genetic epistemology" of Jean Piaget (see for example Piaget 1950, 1970), which indicates how new knowledge or intelligence develops from established knowledge by processes of "assimilation and accommodation". The merit of Piaget's work, as I see it, is that to some extent (though not completely) it fills the gap that Kuhn (1970) left between "normal science" and "revolution". In Kuhn's view "puzzles" were part of normal science, while the accumulation of "anomalies" leads to revolution. How does this process work, and when do puzzles turn into anomalies? Piagetian assimilation and accommodation yield part of the answer.

4. An extensive study of the practice of economists and its discrepancy with the preoccupations of methodologists is given by Lind (1990), which unfortunately is available only in Swedish, but contains an English summary.

5. In the later 1988 version, this statement was shifted significantly into "Independent agents optimize subject to constraints". Thus allowance is made for (mutually?) dependent agents, but no statement is given on their choice behavior.

6. Note that by implication neoclassical economics is seen as coextensive with economics; other streams in economics do not seem to be recognized.


8. In the process, Weintaub makes a lot out of the familiar problems one has to deal with in empirical economics: facts do not lie ready made on the shelf, but have to be laboriously constructed from fragmented statistics, in a design which is geared to the test at hand, with measures to account for missing values, changes in the basis of statistics, missing variables, inhomogeneities, price indices, stochastic properties, etc. I agree with Weintaub that this invalidates the simple minded view of falsification that some methodologists may have.

9. See what is coming to be known as the "imperialism" of neoclassical economics in sociology and psychology (Etzioni, 1988; cf. also Winston & Teichgraeber, 1988). See also the analogy of leaves maximizing the reception of sunlight, given the positions of their neighbors and laws of physics, as an explanation of the position of leaves on a tree, that Friedman (1970) used in the defense of his instrumentalism.

10. Apart from the possibility of a negative Keynesian effect of a lower wage rate on employment.

11. Causality may also run in the other direction: higher wages set as a reward for productivity may cause higher productivity.

12. The interpretation in terms of mark-up pricing is not new, and was noted in Klein (1970), for example. It is striking how this alternative interpretation is acknowledged and subsequently appears to be ignored. For an empirical study of productivity and pricing that tries to interpret results both in the neoclassical framework and in terms of mark-up pricing, see Nootboom (1970, 1980).

13. Cf. Nootboom (1980, 1985), where the size of the mark-up is related to average firm size, demand growth, type of product/market, stage in the life cycle of the product; and Nootboom, Kleinweg & Lhuirk (1988), where attention is paid to the "normal cost" versus the actual cost hypothesis, and the implications for demand effects on the... 

14. First proposed by Solow, Minhas, Arrow and Cheney (1961), and hence also known as the SMAC function.

15. Cf. de Saussure (1979), with his notions of language ("langage") as an intersubjective order ("langue") which forms the basis for individual, idiosyncratic usage ("parole"), but is also continually shifted by it.

References


Hausman, D.M. 1990, On Dogmatism in Economics: The Case of Preference Reversals, paper presented at a meeting at the University of Groningen, the Netherlands.
