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John Stuart Mill's Method in Principle and in Practice: A Review of the Evidence

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I. INTRODUCTION: THE STATE OF PLAY

Our concern is John Stuart Mill's methodological pronouncements, his actual practice, and the relationship between them. We argue that verification played a key role in Mill's method, both in principle and in practice. Our starting point is the celebrated declaration regarding verification in the essay *On the Definition of Political Economy; and on the Method of Investigation Proper to It* (1836/1967; hereafter *Essay*): "By the method *à priori* we mean ... reasoning from an assumed hypothesis; which ... is the essence of all science which admits of general reasoning at all. To verify the hypothesis itself *à posteriori*, that is, to examine whether the facts of any actual case are in accordance with it, is no part of the business of science at all, but of the *application* of science" (Mill 1836/1967, p. 325). The apparent position that the basic economic theory is impervious to predictive failure emerges also in a sharp criticism of the *à posteriori* method:

> Having now shown that the method *à priori* in Political Economy, and in all the other branches of moral science, is the only certain or scientific mode of investigation, and that the *à posteriori* method, or that of specific experience, as a means of arriving at truth, is inapplicable to these subjects, we shall be able to show that the latter method is notwithstanding of great value in the moral sciences; namely, not as a means of discovering truth, but of verifying it, and reducing to the lowest point that uncertainty before alluded to as arising from the complexity of every particular case, and from the difficulty (not to say

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> impossibility) of being assured *à priori* that we have taken into account all the material circumstances (ibid., p. 331).

The "assumed hypothesis" of political economy includes a set of behavioral assumptions:

> Political economy does not treat of the whole of man's nature as modified by the social state, nor of the whole conduct of man in society. It is concerned with him solely as a being who desires to possess wealth, and who is capable of judging of the comparative efficacy of means for obtaining that end. It predicts only such of the phenomena of the social state as take place in consequence of the pursuit of wealth. It makes entire abstraction of every human passion or motive; except those which may be regarded as perpetually antagonising principles to the desire of wealth, namely, aversion to labour, and desire of the present enjoyment of costly indulgences (ibid., p. 321).[1]

The wealth-maximization axiom is selected, more precisely, because it is "the main and acknowledged end" in "certain departments of human affairs" (ibid., p. 323). Mill elaborates:

> It is only of these that Political Economy takes notice. The manner in which it necessarily proceeds is that of treating the main and acknowledged end as if it were the sole end; which, of all hypotheses equally simple, is the nearest to the truth. The political economist inquires, what are the actions that would be produced by this desire, if, within the departments in question, it were unimpeded by any other. In this way a nearer approximation is obtained than would otherwise be practical, to the real order of human affairs in those departments. This approximation is then to be corrected by making proper allowance for the effects of any impulses of a different description, which can be shown to interfere with the result in any particular case.[2]

In the *System of Logic*, too, the "immediately determining causes" are said to be "principally those which act through the desire of wealth; and in which the psychological law mainly concerned is the familiar one, that a greater gain is preferred to a smaller" (1843/1973–74, p. 901).

The foregoing passage from the *Essay* alludes to "impulses ... which can be shown to interfere with the result in any particular case"—the "result," that is, emerging as the outcome of ratiocination based on "the main and acknowledged end." Only in application—and in consequence of such "disturbing causes"—does "an element of uncertainty" enter the process:

> When the principles of Political Economy are to be applied to a particular case, then it is necessary to take into account all the individual circumstances of that case; not only examining to which of the sets of circumstances contemplated by the abstract science the circumstances of the case in question correspond, but likewise what other circumstances may exist in that case, which not being common to it with any large and strongly-marked class of cases, have not

[1] On Mill's intentions by "the present enjoyment of costly indulgences," see below note 18.

[2] See also ibid., p. 326: "The conclusions of Political Economy, consequently, like those of geometry, are only true as the common phrase is, *in the abstract*; that is, they are only true under certain suppositions, in which none but general causes–causes common to the *whole class* of cases under consideration–are taken into account."

> fallen under the cognizance of the science. These circumstances have been called *disturbing causes*. And here only it is that an element of uncertainty enters into the process—an uncertainty inherent in the nature of these complex phenomena, and arising from the impossibility of being quite sure that all the circumstances of the particular case are known to us sufficiently in detail, and that our attention is not unduly diverted from any of them (1836/1967, p. 330).

Considering the apparent clarity of these passages, it is scarcely surprising to find it commonly alleged that Mill denied to verification the role of testing, and conceivably of modifying, theory: "in economics, as Mill had explained, we test the *applications* of theories to determine whether enough of the disturbing economic causes have been taken into account to explain what actually happens in the real world after allowing, in addition, for non-economic causes. We never test the validity of theories, because the conclusions are true as one aspect of human behavior, by virtue of the assumptions, which in turn are true by virtue of being based on self-evident facts of human experience" (Blaug 1992, p. 68). Similarly: "If a theory fails to predict accurately, Mill would have said, a search should be made for sufficient supplementary causes to close the gap between the facts and the causal antecedents laid down in the theory because the theory is true in any case *as far as it goes* by the nature of its true assumptions" (ibid., p. 67). Professor Blaug concludes that, for Mill, "whether there is any way of showing a theory to be false is never even contemplated" (1980, p. 81).

In a careful review of Mill's method, Abraham Hirsch similarly understands Mill as denying to verification the task of "test[ing] a theory by determining whether its implications accord with what actually happens" (1992, p. 847). For "if one *knows* that the implications of economic science will not generally accord very well with what happens because we have only reasoned on the basis of these three behavioural laws [above, p. 370], then what need is there to verify a theory of abstract or scientific economics that is based only on three causal laws? What is there to be learned from verifications that we know, because we have set things up this way, will only show the predictive limitations of the theory?" (ibid., p. 862). It is "*not* ... merely that there is *more* uncertainty in applying theory than in deriving it; in Mill's view there is *certainty* in the business of science where the a priori [method] alone is used." Again "when the a priori method is used no verification at all is needed and ... *certain* results are achieved" (ibid., p. 863). As Hirsch reads Mill:

> Economic science is ... incomplete. When we apply it for practical purposes it needs to be supplemented in order to make better predictions than can be made with the science alone, but in application one tests the hypothesis used for making the prediction which postulates how the practical problem can best be dealt with. And while the abstract science is used in helping to formulate this hypothesis, it is primarily one's ability to identify the pertinent other causes in the particular situation that is being tested ... and not the abstract science. That is why verification for Mill in its very conception is part of the application and not of the science process (ibid., p. 848).

In line with this perspective, Hirsch and De Marchi in their joint study of Milton Friedman, observe regarding Millian verification: "in the Millian system the

evidence may be inconsistent with the implications of our theory, thus giving us little in the way of grounds of confidence, yet our belief in the scientific validity of the causal laws underlying the theory remains unaffected" (Hirsch and De Marchi 1990, p. 113).[3] This position is represented as a "radical" contrast with that attributed to Friedman:

1. Friedman does, but Mill does not, view extensive observation of specific experience as a necessary component in the process of deriving "good" (meaning both scientifically sound as well as useful) theory in economics.
2. Friedman does, but Mill does not, view inquiry in economics as a continuous process where observation of specific experience and hypothesizing interact at every stage of the inquiry process.
3. Mill does, whereas Friedman does not, regard "realistic" assumptions or premises—no matter how these terms are defined—as either necessary or sufficient to make theory provisionally acceptable as part of economic science.
4. Friedman does, but Mill does not, believe that the extent to which a theory can predict (and retrodict) should have a bearing on how we judge its premises (ibid., p. 124).

We believe an additional, and contrasting, interpretation of Mill's method is supported by the evidence. For in our view Mill insisted on the possibility of theory modification in the light of inadequacies revealed by empirical evidence, and also held that the central behavioral axiom is not of *universal* relevance but is pertinent only to the *local* circumstances of contemporary Great Britain and America—and, even so, qualified as we shall see—that the axiom itself is empirically based. On our reading, there is more in common between his research strategy and that of Milton Friedman than is sometimes granted, at least when Friedman's position on theory appraisal is appreciated in the manner of Hirsch and De Marchi (1990). As Fels has paraphrased this position in a review: "start with a thorough marshalling of facts, frame a hypothesis to explain them, make predictions from the hypothesis about facts not used in constructing it, compare the predictions with the actual facts, revise the hypothesis in response to the outcome of the tests, and continue in an iterative fashion" (1991, p. 84).

Our study seeks, further, to shed light on just how seriously Mill took the role of verification in practice. For while the positions outlined above suggest that Mill made *no* allowance for theory modification in the light of the evidence, one study (Hirsch 1992) allows for the possibility of theory modification in Mill, but concludes that such modifications are rare: "Mill does not say explicitly that the basic model is impervious to verification, but he leaves the strong impression that on his view the probability that it will be changed through verification is exceedingly small" (p. 865).[4] Hirsch makes repeated qualifications of the same

[3] Coleman (1996) extends this sort of position to the whole body of "English classical economics."

[4] Hirsch maintains that though Hollander (1985, p. 127) recognizes "that the basic model might indeed be impervious to verification," he "considers it as only a remote possibility." Hollander's original statement is actually neutral regarding the matter of likelihood: "the basic model might indeed be

order.[5] But once we allow that there is at least the small likelihood of such theory modification in consequence of verification or retrodiction, there is one obvious way of evaluating whether or not—in Mill's mind—verification, by revealing deficiencies, can generate corrections and improvements in theory, and if it can do so, the "likelihood" of that occurring. This is by considering Mill's own practice. Though Mill's formal methodological pronouncements have been studied extensively, there is a general neglect of his actual procedures. Our intention is to redress this imbalance by assessing Mill's methodological practice. We find there to be compelling evidence in that practice to refute the view that for Mill verification serves only to point out the existence of disturbing causes.

In Sections II and III below, the formal pronouncements regarding science and the application of science, upon which so much weight has been placed, are examined. Here emerges Mill's case, in principle, for theory modification in the light of "verification" and the justification of the wealth-maximizing axiom in empirical terms. Thereafter we turn to a sampling of Mill's practice. Section IV considers the central behavioral axiom in the context of pricing and market structure issues. A particular charge against Mill is taken up in Section V, namely that in consequence of his formally stated methodology he was unwilling—despite growing evidence—to admit the irrelevance, indeed the invalidity, of Ricardian growth theory, and proceeded by adopting various "immunizing stratagems" (Blaug 1992, pp. 65–68). In fact, it turns out that though Ricardian growth theory retained for Mill its validity especially with an eye to policy, he nonetheless engaged in major modifications of the land-based model by linking it with the phenomenon of regular business cycles, an intellectual feat of the first order conspicuously reflecting the need for model improvement in the light of new empirical findings. The Ricardian inverse wage-profit relation is addressed in Section VI, where we take up Mill's concerns with its validity in the light of recent British and U.S. data on the profit rate, and his proposed application of the basic model as solution to the apparent anomaly.

We make no claims for an exhaustive treatment of Mill's prac-

impervious to verification ... but [it] is conceivable that the testing procedure yields new information of general relevance–rather than of particular relevance in a specific case–and if that is so it must have an impact on the model" (Hollander 1985, p. 126). This latter conclusion is stated too strongly; it *might* have an impact on the model is all that is intended.

[5] Verification helps "with the discovery of disturbing causes rather than the more general ones, and ... would, except in very rare instances, therefore leave the basic model of an advanced science unaffected" (1992, p. 853); a failed test via retrodiction "would not necessarily show up any shortcomings in the science but only that these causes are special and therefore are not included as part of science" (pp. 847–48); Mill "gives the distinct impression that finding general causes not already known does not happen very often" (p. 855); Mill "did not seem to envisage that there would be much of a chance for verification to be a stimulus for revision of the basic model; rather, he leaves the impression that it is considerably more likely to lead to the discovery of disturbing causes which help only in the *application* of science" (p. 859, italicised in original); "the evidence indicates that while Mill does not rule out the possibility that feedback from attempted verification is the channel from which the basic model of economic science is revised and improved, he seems to feel that the probability is very small" (p. 860); "when, in the course of application, retrodiction fails, Mill saw this as more likely to show that we do not know all of the pertinent causes than that we know the causes (both general and special), but cannot ascertain their effect" (p. 860).

tice.[6] Our concern is illustrative. In each of our cases it is shown that Mill examined the empirical evidence relating to the state of business affairs in contemporary Britain relevant to his theoretical position and, in order better to explain observed phenomena, gave a prominent role to such experiential knowledge either by respecifying the axiomatic framework, or by enriching the causal analysis. Much of our evidence has been set out in Hollander (1985) and Peart (1993, 1995). We have felt it necessary to restate the argument and again to scrutinize some of the relevant texts, in order to reassess the role and significance of experiential evidence in Mill's economics. We hope in particular to use the case studies to shed light on the widely held view whereby in Mill's practice "discrepancies between anticipation and actual facts do not show the original statement to be wrong, only 'insufficient'" (De Marchi, 1988, p. 152).[7] We are not seeking, in Mill, an "ordered program of empirical inquiry" (ibid., p. 152), since in our view this entails too stringent a condition. Nor do we deny that Mill combined the experiential techniques with "logical demonstrations" or the "inverse deductive method" (ibid., p. 159). Our reading of the literature on Mill, however, suggests that he has been too often categorized as wholly or mostly an *à priori* theorist, and only infrequently has it been granted (see Hausman 1981, 1989) that Mill allowed for and relied on model improvement through verification. Consequently this paper seeks to redress the balance.

II. MILL ON METHOD: "VERIFICATION"[8]

The role of verification in Mill turns, in Hirsch's view, on the distinction between theory, which requires no verification, and application, which does. Certainly, for Mill verification is a key part of applied science. At one point in the *Essay*, as we have seen (above pp. 370–371), he accords verification the apparently limited role of isolating the "partial" or "disturbing" causes at play in any particular case to assess their significance relative to the general causes—an

[6] In the interests of brevity, we have omitted two important examples of Mill's practice: his famous treatment of the wage-structure, which belies his strong declaration that analytical economics is constrained to purely competitive structures and takes issue with the theoretical position outlined in Smith (1776/1937, p. 100); and Mill's admission in the famous essay "On the Influence of Consumption on Production" to "some strong appearance of evidence" which had misled those who maintained the "palpable absurdities" regarding consumption (1844/1967, pp. 263–64).

[7] De Marchi finds the evidence in Hollander (1985) to be "less than compelling": "In almost 1,000 pages [Hollander] can point to several examples of model-improvement, but only one convincing instance in which Mill actively subjected a theory to a test against evidence." De Marchi concludes, by contrast, that Mill "did little in the way of active testing against evidence, and it is difficult to perceive in his efforts any *ordered program* of empirical inquiry (attempted verification)" (ibid., p. 152). Elsewhere he finds more common ground: "Hollander denies that simple (point) prediction is what Mill was about. I share this view. But Hollander also finds Mill to be very serious about the empirical basis of his axioms and about checking (and improving) his theory. On this I conclude more circumspectly, that Mill was in principle an empiricist, but that he did not consistently do all that is required by this, sometimes falling back on logical demonstration (see the inconclusive discussion in Hollander 1985, vol. 1, 209–211; cf. 234–235, 236) and sometimes using the inverse deductive method in place of direct factual checking (140, 239; cf. 190)" (ibid., p. 159).

[8] We draw here from both the earlier *Essay* and the *System of Logic*. Our general position is close to that of Hausman (1981, 1989).

exercise that is not itself a part of "science" but of its *application*. But Mill in fact goes a step further. Those disturbing causes:

> which operate through the same law of human nature out of which the general principles of the science arise ... might always be brought within the pale of the abstract science, if it were worthwhile; and when we make the necessary allowances for them in practice, if we are doing anything but guess, we are following out the method of the abstract science into minuter details; inserting among its hypotheses a fresh and still more complex combination of circumstances, and so adding *pro hâc vice* a supplementary chapter or appendix, or at least a supplementary theorem, to the abstract science (1836/1967, p. 331).

Now "adding a supplementary chapter or appendix or a theorem *for this time only (pro hâc vice)*," it has been said, "hardly sounds like the process by which the revision of the basic model is carried out"; rather, the basic model remains unchanged "since the disturbing causes remain in the disturbing ... category" (Hirsch 1992, p. 856). On our reading, however, since Mill chose to express the matter as bringing disturbing causes "*within the pale of the abstract science*," the formulation suggests that the model itself *is* subject to modification if only temporarily. It is the abstract science itself, Mill tells us explicitly, that is affected by the "addition of a supplementary theorem." Yet more important, two paragraphs earlier he had pointed out that some disturbing causes—like friction in mechanics—"may at first have been considered merely as a non-assignable deduction to be made by guess from the result given by the general principles of science; but in time many of them are brought within the pale of the abstract science itself, and their effect is found to admit of as accurate an estimation as those more striking effects which they modify" (1836/1967, p. 330). There is no reference here to "*pro hâc vice*"; and it is plausible to read this passage as alluding to cases of permanent revision of theory resulting from the cumulative effect of repeated "failures" of prediction (or retrodiction). Additional support for this reading is Mill's observation that verification "often discloses to us errors in thought, still more serious than the omission of what can with any propriety be termed a disturbing cause. It often reveals to us that the basis itself of our whole argument is insufficient; that the data, from which we had reasoned, comprise only a part, and not always the most important part, of the circumstances by which the result is really determined" (ibid., p. 332).[9]

Two types of revisions of theory might thus result from the process of verification, though Mill never clarified how one is to distinguish between them. A set of general causes, A, B and C, is used to predict the outcome E. E* is observed, leading the scientist to revise the causal framework by adding D—perhaps, though not necessarily, *pro hâc vice*—to the model. Alternatively, the procedure of verification might reveal that the axioms have been inferred from an incomplete set of circumstances—that the data are "insufficient." In this instance observation of E* leads the scientist to revise A, B, and C to A*, B*, and C* (see Peart 1993, p. 442).

[9] Mill also says that this allowance "even forms an indispensable supplement to" the "method *a priori*" (ibid., p. 327)–indicating that, even in his formal statements, he did not regard the method of abstraction to be entirely separate from that of experience and verification.

It has been objected, however, that in fact Mill in the latter case is referring to particular circumstances rather than general causes (Hirsch 1992, p. 852). The discrepancy between this interpretation and ours turns on how widely applicable such cases are. Our reading suggests they may be general, though not universal. For Hirsch, since Mill defines "disturbing causes" as "circumstances ... [which] not being common to it with any large and strongly-marked class of cases, have not fallen under the cognisance of the science" (see above p. 370–371), it follows that no matter "how great the effect of a cause in a specific instance," that cause remains in the disturbing-causes category if it is not a *general* cause (ibid., p. 854). Yet Mill's passage is explicit that the concern is with "errors in thought, still more serious than the omission of what can with any propriety be termed a disturbing cause," and follows directly upon a statement which *is* concerned with the discovery of disturbing causes:

> We cannot, therefore, too carefully endeavour to verify our theory, by comparing, in the particular cases to which we have access, the results which it would have led us to predict, with the most trustworthy accounts we can obtain of those which have been actually realized. The discrepancy between our anticipations and the actual fact is often the only circumstance which would have drawn our attention to some important disturbing cause which we had overlooked. Nay, it often discloses to us errors in thought, still more serious than the omission of what can with any propriety be termed a disturbing cause (1836/1967, p. 332).

It is in the light of this contrast that one should read Mill's own comment on the passage now under discussion:

> Such oversights are committed by very good reasoners, and even by a still rarer class, that of good observers. It is a kind of error to which those are peculiarly liable whose views are the largest and most philosophical: for exactly in that ratio are their minds more accustomed to dwell upon those laws, qualities, and tendencies, which are common to large classes of cases, and which belong to all place and all time; while it often happens that circumstances almost peculiar to the particular case or era have a far greater share in governing that one case (ibid., pp. 332–33).

Tendencies "common to large classes of cases," in the sense of belonging "to all places and all time," can only mean universally applicable, in which case "circumstances almost peculiar to [a] particular case or era," must relate to a localized geographical and temporal reality—to Britain or the United States, for example, under given conditions. When the term "particular case" is read along with "particular era," it becomes clear that verification might reveal a general—though not universally applicable—cause at play relating to the particular case for which contemporary theory was designed to deal, namely to the advanced economies of Mill's day. (We shall return to this issue in Section III.)[10]

We turn now to an explicit recognition by Mill that verification (in the sense

[10] Mill's reference (ibid., p. 330) to "the more striking effects" modified by disturbing causes, might also suggest a quantitative criterion distinguishing the categories, as do the descriptions of the wealth-maximization axiom (cited above p. 370).

of retrodiction) might reveal the economist's model to be "imperfect even as an abstract system":

> [The political economist's] knowledge must at least enable him to explain and account for what *is*, or he is an insufficient judge of what ought to be. If a political economist, for instance, finds himself puzzled by any recent or present commercial phenomena; if there is any mystery to him in the late or present state of the productive industry of the country, which his knowledge of principle does not enable him to unriddle; he may be sure that something is wanting to render his system of opinions a safe guide in existing circumstances. Either some of the facts which influence the situation of the country and the course of events are not known to him; or, knowing them, he knows not what ought to be their effects. In the latter case his system is imperfect even as an abstract system; it does not enable him to trace correctly all the consequences even of the assumed premises (ibid., p. 335).

This would seem to constitute additional evidence that for Mill economic theory is subject to correction in consequence of verification. Yet Professor Hirsch suggests that Mill is concerned here not with an abstract system in the sense of specialized economic science, but rather with speculative politics more generally (Hirsch 1992, pp. 858–59). Now Hirsch rightly points out that sometimes "Mill is not very clear about who does what in the process of verification" (ibid., p. 857 n. 20). But in our present case Mill refers explicitly to the political economist who "finds himself puzzled" by the empirical evidence and becomes aware that "his system is imperfect even as an abstract system," and whose task, in that case, "is not yet completed." It is true that in the next paragraph it is the "speculative politician" who is said to be obliged to seek the explanation for any failure "conscientiously, not with the desire of finding his system complete, but of making it so"; and who is duty bound to carry out a verification "upon every new combination of facts as it arises." He must allow for "the disturbing influence of unforeseen causes," but he also "must carefully watch the result of every experiment, in order that any residuum of facts which his principles do not lead him to expect, and do not enable him to explain, may become the subject of a fresh analysis, and furnish the occasion for a consequent enlargement or correction of his general views" (1836/1967, pp. 335–36). To the extent that political economy is represented as a branch of the science of speculative politics (as on p. 321), this particular problem is eased. In any event, shortly thereafter, Mill writes more generally about the "danger of falling into partial views" in which context he appears to have reverted to the "theorist": "All that we can do more, is to endeavour to be impartial critics of our own theories, and to free ourselves, as far as we are able, from that reluctance from which few inquirers are altogether exempt, to admit the reality or relevancy of any facts which they have not previously either taken into, or left a place open for in, their systems" (ibid., p. 336). And in the *Logic* it is further clarified that Mill is discussing the *economist's* knowledge of principle, and potential revelations of its inadequacy as a result of verification.[11] We have in mind two equivalent, but more detailed,

[11] We shall have more to say regarding the matter of functional specialization in our Conclusion.

statements to this very effect entailing at key junctures almost identical terminology to the passage from the *Essay*.

The first extract appears in a section entitled, "*Third Stage; verification by specific experience*," which addresses the problem that the "direct observation and experiment" of *à posteriori* method are "illusory when applied to the laws of complex phenomena":

> When in every single instance a multitude, often an unknown multitude, of agencies, are clashing and combining, what security have we that in our computation *à priori* we have taken all these into our reckoning? How many must we not generally be ignorant of? Among those which we know, how probable that some have been overlooked; and, even were all included, how vain the pretence of summing up the effects of many causes, unless we know accurately the numerical law of each,—a condition in most cases not to be fulfilled; and even when it is fulfilled to make the calculation transcends, in any but very simple cases, the utmost power of mathematical science with all its most modern improvements (1843/1973–74, p. 460).

The remedy is provided by the "test" of verification, "the third essential component part of the Deductive Method; without which all the results it can give have little other value than that of conjecture". As in the *Essay*, Mill is crystal clear that in the event of a failure of verification, the theory itself is "imperfect, and not yet to be relied upon" in future application:

> To warrant reliance on the general conclusions arrived at by deduction, these conclusions must be found, on careful comparison, to accord with the results of direct observation wherever it can be had. If, when we have experience to compare with them, this experience confirms them, we may safely trust to them in other cases of which our specific experience is yet to come. But if our deductions have led to the conclusion that from a particular combination of causes a given effect would result, then in all known cases where that combination can be shown to have existed, and where the effect has not followed, we must be able to show (or at least to make a probable surmise) what frustrated it: if we cannot, the theory is imperfect, and not yet to be relied upon (ibid., pp. 460–61).[12]

All this is repeated later in the text, where social science is at issue. The deductive conclusions of social science were to be checked constantly against specific experience, the importance of combining "*à priori* reasoning" with verification increasing as the "composition of causes" became more pronounced: "This remedy consists in the process which, under the name of Verification, we have characterized [pp. 460–63] as the third essential constituent part of the Deductive Method: that of collating the conclusions of the ratiocination either with the concrete phenomena themselves, or when such are obtainable, with their empirical laws. The ground of confidence in any concrete deductive science is

[12] The conditions are stringent: "Nor is the verification complete, unless some of the cases in which the theory is borne out by the observed result, are of at least equal complexity with any other cases in which its application could be called for" (ibid., p. 461).

not the *à priori* reasoning itself, but the accordance between its results and those of observation *à posteriori*" (ibid., pp. 896–97).[13]

A typical problem pertaining to the "more special inquiries which form the subject of the separate branches of the social science"—where "the object is to determine the effect of any one social cause among a great number acting simultaneously"—is illustrated by the analysis of the effect of corn laws on industrial prosperity (ibid., pp. 908–9). Now Mill is evidently concerned here with the narrow realm of economics. He then elaborates on various problems of testing—that there are "no previous empirical generalizations with which to collate the conclusions of theory," and that it is impossible to ascertain that "the circumstances of [an] experiment [are] exactly the same with those contemplated in the theory." Thus a "trial of corn laws in another country or in a former generation, would go a very little way towards verifying a conclusion drawn respecting their effect in this generation and in this country." The only solution "towards verifying the general sufficiency of the theory" is that provided by *retrodiction*, which procedure Mill elaborates in a passage using several of the same terms and phrases as the *Essay*:

> The test of the degree in which the science affords safe grounds for predicting (and consequently for practically dealing with) what has not yet happened, is the degree in which it would have enabled us to predict what has actually occurred. Before our theory of the influence of a particular cause, in a given state of circumstances, can be entirely trusted, we must be able to explain and account for the existing state of all that portion of the social phenomena which that cause has a tendency to influence. If, for instance, we would apply our speculations in political economy to the prediction or guidance of the phenomena of any country, we must be able to explain all the mercantile or industrial facts of a general character, appertaining to the present state of that country: to point out causes sufficient to account for all of them, and prove, or show good ground for supposing, that these causes have really existed. *If we cannot do this, it is a proof either that the facts which ought to be taken into account are not yet completely known to us, or that although we know the facts, we are not masters of a sufficiently perfect theory to enable us to assign their consequences. In either case we are not, in the present state of our knowledge, fully competent to draw conclusions, speculative or practical, for that country* (ibid., pp. 909–10; emphasis added).

Even the speculative model might thus be revealed to be inadequate. And in closing, Mill repeats that he is concerned with economic science—not merely with the details of a particular case—and that the presence of unexplained

[13] Mill adds: "Either of these processes, apart from the other, diminishes in value as the subject increases in complication, and this is in so rapid a ratio as soon to become entirely worthless; but the reliance to be placed in the concurrence of the two sorts of evidence, not only does not diminish in any thing like the same proportion, but is not necessarily much diminished at all. Nothing more results than a disturbance in the order of precedency of the two processes, sometimes amounting to its actual inversion: insomuch that instead of deducing our conclusions by reasoning, and verifying them by observation, we in some cases begin by obtaining them provisionally from specific experience, and afterward connect them with the principles of human nature by *à priori* reasoning, which reasoning are thus a real Verification" (ibid., p. 897).

residuals, by generating further investigation, might lead to "an extension and improvement of the theory itself":

> If there be anything which we could not have predicted, this constitutes a residual phenomenon, requiring further study for the purpose of explanation; and we must either search among the circumstances of the particular case until we find one which, on the principles of our existing theory, accounts for the unexplained phenomenon, or we must turn back, and seek the explanation by an extension and improvement of the theory itself (ibid., p. 910).

There seems to be compelling evidence that Mill allowed formally and conspicuously for improvement of economic "science," where the necessity for such improvement is revealed by a verification failure.

III. MILL ON METHOD: THE BEHAVIORAL AXIOM

Abstraction from the multitude of motivations that *actually* prompt action in a social context, and selection of the wealth maximization axiom subject to the two "perpetually antagonizing principles" (above, p. 370), is represented by Mill as a matter of practical necessity in the light of the "complexity of causes":

> Not that any political economist was ever so absurd as to suppose that mankind are really thus constituted, but because this is the mode in which science must necessarily proceed. When an effect depends upon a concurrence of causes, those causes must be studied one at a time, and their laws separately investigated, if we wish, through the causes, to obtain the power of either predicting or controlling the effect; since the law of the effect is compounded of the laws of all the causes which determine it (1836/1967, p. 322; cited 1843/1973–74, p. 902).

Here we focus on Mill's intention by his references to introspection as the basis of the behavioral axiom (1836/1967, p. 329). Much more is involved than appears at first sight, since even within the "complicated and manifold civilisations of the nations of Europe," there are discernible major behavioral differences. Consider the sharp contrast Mill made in the *System of Logic* between entrepreneurial motivation in Britain and the Continent:

> In political economy ... empirical laws of human nature are tacitly assumed by English thinkers, which are calculated only for Great Britain and the United States. Among other things, an intensity of competition is constantly supposed, which, as a general mercantile fact, exists in no country in the world except those two. An English political economist, like his countrymen in general, has seldom learned that it is possible that men, in conducting the business of selling their goods over a counter, should care more about their ease or their vanity than about their pecuniary gain. Yet those who know the habits of the Continent of Europe are aware how apparently small a motive often outweighs the desire of money-getting, even in the operations which have money-getting as their direct object (1843/1973–74, p. 907).

Given this sharp contrast, it is inconceivable that introspection would yield the

identical outcome in a British or American and (say) a French domain.[14] Yet Hirsch and De Marchi reject the conclusion that, for Mill, "evidence drawn from introspection varies from time to time and place to place" (in Hollander 1985, pp. 112–13), on the grounds that it "run[s] together empirical and causal laws, on the one hand, and assumptions and implications on the other" (Hirsch and De Marchi 1990, p. 128 n. 3). This is spelled out in an earlier review by Hirsch, where Hollander's reading is said to go against "both the letter and the spirit of Mill's position":

> Hollander argues (ibid., p. 135) that for Mill "the behavioral axioms of political economy ... are 'approximate generalizations'" which is counter to both the letter and the spirit of Mill's position. In Mill's philosophy, science rests on causal laws which are invariant, exact, and universal and embedded in the premises of theory. One such causal law is the preference of mankind for a greater portion of wealth for a smaller. That is not to deny that Mill did admit that lower level generalizations or empirical laws could differ from place to place and time to time; he reconciled the exact and invariant causal law with the inexact empirical laws—or put somewhat differently, he explained the fact that the implications of the theory of political economy did not coincide with what actually happened in many places and many times—by bringing in "disturbing causes" that accounted for the differences in empirical laws or generalizations. This made Mill far more open than the narrow thinkers, like Harriet Martineau, to whom even the implications of the theory of political economy, even without allowance for disturbing causes, were exact and universal. But it does not follow from this that for Mill "the wealth-maximizing hypothesis ... holds good only in the context of a specified nation and period" (Hollander, p. 136). Such a notion runs counter not only to Mill's views about the science of political economy but to his views of social science and science generally (Hirsch 1986, p. 622).

Yet "approximate generalization" happens to be *Mill's* term for the behavioral axioms:

> wherever it is sufficient to know how the great majority of the human race, or of some nation or class of persons, will think, feel, and act, these propositions are equivalent to universal ones. For the purposes of political and social science this *is* sufficient. As we formerly remarked [p. 603], "an approximate generalization is, in social inquiries, for most practical purposes equivalent to an exact one: that which is only probable when asserted of individual human beings indiscriminately selected, being certain when affirmed of the character and collective conduct of masses" (1843/1973–74, p. 847).

It is also Mill himself who, as we have seen, refers to "the empirical laws of human nature" in describing the appropriateness of the wealth-maximization axiom (and competition) for the analysis of Britain and the United States but its inappropriateness in the analysis of Continental Europe.

So much for the letter of Mill's position. As for the spirit, both the terms

[14] A role for observation and experiment is, in fact, explicitly allowed in the establishment of the behavioral framework (ibid., pp. 454–55).

"approximate generalization" and "empirical laws" are wholly appropriate as descriptions of the behavioral axioms, because Mill is attempting to convey their *conditional* nature in the context of the particular "branches of social inquiry which have been cultivated as separate sciences"—conditional because of the primitive nature of the so-called science of ethology and the weak understanding of "the diversities of individual and national character" (ibid., pp. 905–06). For "if we have not yet accounted for the empirical law—if it rests only on observation—there is no safety in applying it far beyond the limits of time, place and circumstance, in which the observations were made" (ibid., p. 862); but the universal laws—thus far uncovered—"are those of the formation of character" (ibid., p. 863), rather than a locally-relevant behavioral axiom such as wealth maximization.[15]

Hirsch, we have seen, also maintains that the "lower level generalizations or empirical laws" which do differ "from place to place and time to time" are reconciled "with the exact and invariant causal law" *by the device of disturbing causes*. Yet if the habits of Continental Europe are such that "the desire of money-getting, even in the operations which have money-getting as their direct object" is often outweighed by apparently small motives, it makes little sense to suppose that Mill would have adopted, for these locations, pecuniary maximization as the *main and acknowledged* or *principal* objective to be embedded in the premises of theory, and have treated as disturbing causes the *predominating* behavior traits. As Mill expressed the matter: "the generalizations [of political economy] must necessarily be relative to a given form of civilization and a given stage of social achievement" (1865/1969, p. 305).

* * *

The treatment in the *Essay* of the principle of population creates additional difficulties for the drawing of a sharply defined demarcation between theory and application in Mill. For Mill represented this principle as one of "the most

[15] A science of ethology was still a matter for the future, so that there existed a continuous danger of claiming too much for empirical generalizations (the common wisdom of common life). For example:

> when maxims ... collected from Englishmen, come to be applied to Frenchmen, or when those collected from the present day are applied to past or future generations, they are apt to be very much at fault. Unless we have resolved the empirical law into the laws of the causes on which it depends, and ascertained that those causes extend to the case which we have in view, there can be no reliance placed in our inferences (ibid., p. 864).

On one occasion Mill maintains in a discussion of the *relative* strengths of various motives—"the desire of wealth or of personal aggrandizement, the passion of philanthropy, and the love of active virtue"— that "[t]he only one of them which can be considered as anything like universal, is the desire of wealth; and wealth being, in the case of the majority, the most accessible means of gratifying all their other desires, nearly the whole of the energy of character which exists in highly civilized societies concentrates itself on the pursuit of that object" (1836/1977, pp. 129–30). But the passage proceeds to exclude the most influential classes and focus on the middle classes of Great Britain: "Thus it happens that in highly civilized countries, and particularly among ourselves, the energies of the middle classes are almost confirmed to money-getting, and those of the higher classes are nearly extinct" (ibid., p. 130).

striking cases"—he specifies no other by name in this context—where the science of political economy "interpolates into [its] expositions" behavioral patterns *not* dictated by wealth maximization (and its two antagonising concomitants) rather than treat them as "disturbing causes" (1836/1967, p. 323). In such cases:

> the strictness of purely scientific arrangement [is] thereby somewhat departed from for the sake of practical unity. So far as it is known, or may be presumed, that the conduct of mankind in their pursuit of wealth is under the collateral influence of any other of the properties of our nature than the desire of obtaining the greatest quantity of wealth with the least labour and self-denial, the conclusions of Political Economy will so far fail of being applicable to the explanation or prediction of real events, until they are modified by a correct allowance for the degree of influence exercised by the other cause.

Hirsch, in fact, sees here the "one instance where the sharp separation between science and the application of science can be said to have given way," and attributes the breach to Mill's wish to allow for "purposes of instruction" (1992, p. 856 n). Now it is true that in a paragraph that follows the remark regarding the population principle Mill does refer to "the didactic writer ... [who] will naturally combine in his exposition, with the truths of the pure science, as many of the practical modifications as will, in his estimation, be most conducive to the usefulness of his work" (1836/1967, p. 323). But this paragraph—it closes the first section of the *Essay* concerned with definition—does not relate specifically to the treatment of the population principle. Rather, Mill is reminding teachers of their obligation to deal with practical modifications in the sense of disturbing causes within their expositions. What he had to say of the population principle specifically related to *its actual absorption by the science*, albeit that it did not turn on the main psychological law of maximization modified by the two perpetually antagonizing forces. Does classical theory, for Mill, somehow partially exclude or place in a secondary class of importance the population mechanism because it is not based on the three psychological laws and is only allowed in for the sake of practical utility? Needless to say, the single issue that most preoccupied Mill *theoretically*—because of the implications for welfare—is that of population growth in the presence of scarce land. It is practical utility in the sense of policy implication (not merely instruction) that justified its incorporation into the theoretical structure.[16]

[16] Professor De Marchi's (1986, p. 91) characterization may place too little weight on the importance of the population principle: "It needs to be stressed that Mill's science was of very limited scope and potential. It dealt with one particular class of facts, the production and uses of wealth in so far as these are affected by the (psychological) drive to pursue material advantage. Also, it considered usually just one (composite) cause, the desire to maximize wealth with the least effort and subject to a certain degree of myopia. Occasionally it would incorporate in its laws the operation of 'environmental' principles, namely the urge to procreate and the limited powers of the soil (1836/1967, p. 323); but mostly the wealth motive was its sole concern" (ibid., p. 91). Elsewhere the population mechanism is referred to as one of "the underlying laws shaping economic investigations" which were "both known and true" (ibid., p. 98). In a recent MS, De Marchi writes of "diminishing returns, and the law of population" that "[i]n practice Mill takes them as exogenously given" (1998, p. 23).

IV. PRICE DISCRIMINATION AND "MONOPOLISTIC COMPETITION"

We proceed now to our second task, which is to lay out a series of theoretical modifications actually introduced by Mill in the light of empirical evidence. In some cases the evidence even undermines the basic assumption regarding maximizing behavior. Mill declared famously that:

> only through the principle of competition has political economy any pretension to the character of a science ... Assume competition to be their exclusive regulator, and principles of broad generality and scientific precision may be laid down, according to which they will be regulated. The political economist justly deems this his proper business: and as an abstract or hypothetical science, political economy cannot be required to do, and indeed cannot do, anything more (1848/1965, p. 239).

Similarly, the analysis of price formation in "On Exchange" sets out with the declaration that only in so far as prices are determined by competition, "can they be reduced to any assignable law" (ibid., p. 460). It is on these grounds that analysis—turning on the "the axiom ... that there cannot be for the same article, of the same quality, two prices in the same market"—is limited to the wholesale sector, since individual consumers *typically* fail to act in maximizing fashion:

> The values and prices ... to which our conclusions apply, are mercantile values and prices; such prices are as quoted in price-currents; prices in the wholesale markets, in which buying as well as selling is a matter of business; in which the buyers take pains to know, and generally do know, the lowest price at which an article of a given quality can be obtained ... . Our propositions will be true in a much more qualified sense, of retail prices; the prices paid in shops for articles of personal consumption. For such things there often are not merely two, but many prices, in different shops, or even in the same shop; habit and accident having as much to do in the matter as general causes. Purchases for private use, even by people in business, are not always made on business principles: the feelings which come into play in the operation of getting, and in that of spending their income, are often extremely different. Either from indolence, or carelessness, or because people think it fine to pay and ask no questions, three-fourths of those who can afford it give much higher prices than necessary for the things they consume; while the poor often do the same from ignorance and defect of judgment, want of time for searching and making inquiry, and not unfrequently, from coercion, open or disguised.[17]

Now notwithstanding his declaration regarding scientific economics being limited to full-fledged competition, Mill does attempt to provide a theoretical account of observed retail-pricing practice in the presence of non-maximizing consumer behavior. For the very circumstance that pecuniary interest does not apply at the retail level, gives rise to price discrimination; the retailer acts in maximizing fashion by resorting to price discrimination, but the consumer's *failure* to act "on business principles" makes such discrimination possible:

[17] Interestingly, the focus on the array of possible behavioral motivations also pervades the economics of W.S. Jevons. See Peart (1996, 1998) for detailed discussions.

> Not only are there in every large town, and in almost every trade, cheap shops and dear shops, but the same shop often sells the same article at different prices to different customers: and, as a general rule, each retailer adapts his scale of prices to the class of customers whom he expects. The wholesale trade, in the great articles of commerce, is really under the domination of competition. There, the buyers as well as the sellers are traders or manufacturers, and their purchases are not influenced by indolence or vulgar finery [1865: "nor depend on the smaller motives of personal convenience"], but are business transactions (ibid., pp. 242–43).[18]

Moreover, the retail trade is also characterized by Mill along lines of what today would be termed monopolistic competition, namely markets subject to freedom of entry, with competition "instead of lowering prices, merely divid[ing] the gains of the high price among a greater number of dealers" (1848/1965, p. 243). More specifically, "custom" indicated a mark-up over the wholesale prices and what competition there was avoided price cutting:

> even in countries of most active competition, custom also has a considerable share in determining the profits of trade ... . There has been ["is," till 1862] in England a kind of notion, how widely prevailing I know not, that fifty per cent is a proper and suitable rate of profit in retail transactions ... . If this custom were universal, and strictly adhered to, competition indeed would still operate, but the customer would not derive any benefit from it, at least as to price; the way in which it would diminish the advantages of those engaged in the retail trade, would be by a greater sub-division of the business (ibid., pp. 409–10).[19]

From 1852 on, Mill referred to the consequential losses in efficiency resulting when price competition had "a limited dominion over retail prices": "the share of the whole produce of land and labour which is absorbed in the remuneration of mere distributors, continues exorbitant; and there is no function in the economy of society which supports a number of persons so disproportioned to the amount of work to be performed" (ibid., p. 410). Thus the formulation implies recognition of excess capacity.[20]

Mill remarked on the universality of the *method* rather than the *specific content* of economic theory (1834/1967, pp. 25–26; 1865/1969, pp. 305–06). His absorption into the theoretical model of features of reality reflecting "custom"—notwithstanding his strong statements implying the contrary (see our Con-

[18] The reference to "vulgar finery" recalls the term "costly indulgences" in the statement of the basic behavioral axiom (1836/1967, p. 321, cited above, p. 370), suggesting that by the latter Mill intended conspicuous consumption, as well as time preference, which is the usual understanding (see De Marchi's terms "myopia," above, note 16). But the expression, "the desire of obtaining the greatest quantity of wealth with the least labour and self-denial" (ibid., p. 323) also suggests general time preference.

The addition of 1865 seems to distinguish rational types of non-pecuniary interest from irrational types, on which issue see Peart, forthcoming.

[19] The qualification "at least as to price," added in 1862, suggests product or quality differentiation.

[20] The "monopolistic competition" model emerges also in the discussion of professional remuneration where "competition operates by diminishing each competitor's chance of fees, not by lowering the fees themselves" (ibid., p. 243). Banking, too, is said to fall into the category, competition acting in part by reducing market shares to keep the return on capital on a par with opportunities elsewhere (1963, pp. 306–08).

clusion)—illustrates this theme. The entire analysis entails, in fact, an attempt to account theoretically for observations of contemporary market structures. It is of some interest that Mill found the ability to differentiate, by location if not by product, already weakened in the "great emporia of trade" (1848/1965, p. 410) and likely to be further undermined by the transport revolution that breaks down the dependency of consumers on local dealers (ibid., p. 243). It is thus a matter of circumstance how custom is to be classified; in some instances its influence is so strong and pervasive that it must be treated as the *general* cause, relegating to competition the role of *disturbance*: "hitherto it is only in the great centres of business that retail transactions have been chiefly, or even much, determined, by competition. Elsewhere it rather acts, when it acts at all, as an occasional disturbing influence; the habitual regulator is custom, modified from time to time by notions existing in the minds of purchasers and sellers, of some kind of equity or justice."

V. RICARDIAN GROWTH THEORY

Mill stood firmly by Ricardian orthodoxy as far as concerns secular trends. Yet the striking features of his account in the *Principles* of British conditions over several decades are constancy of the profit rate despite rapid accumulation and rapid population growth, constancy of the real wage at a level above subsistence (perhaps even an upward trend), and impressive evidence of productivity increase (1848/1965, pp. 742, 159). The *actual* circumstances thus stood in sharp contrast with the *theoretical* in that both capital and population were proceeding at a very rapid pace with the profit rate unchanged over time and the wage rate, at worst, unchanged and perhaps rising. As noted above (p. 373), one view has it that, for Mill, the basic theory is inviolable considering the character of the assumptions and not subject to modification; and that such methodological grounds would account for his refusal to concede the irrelevance of Ricardian theory with the passage of time. The evidence marshalled thus far points directly against this notion of the insulation of theory, suggesting that we must look elsewhere to account for Mill's continued reliance on a diminishing-returns based model.

There is first the matter of Mill's use of theory in policy application. Specifically, as Mill read the evidence, prudential behavior positively directed towards the achievement of higher wages characterized only skilled laborers in the British case (ibid., p. 346), for the high earnings of the *unskilled* town workers had not encouraged a noticeable alteration in their conception of the minimum standard. Accordingly, *future* slackening in the growth rate of capital might lead to a fall in wages to the level of the common farm laborer. This was the ever-present shadow—for there was no guarantee the countervailing forces would continue at play—leading Mill to minimize labor scarcity. The same shadow was cast over the *agricultural* sector. There wages had been kept up above subsistence by the attraction of labor into the towns, a movement that might decelerate considering "the present habits of the people"—again a reference to the unskilled (ibid., p. 351). We must keep in mind also his evaluation that it was "much more difficult to raise than to lower, the scale of living

which the labourer will consider as more indispensable than marrying and having a family," leading him to question "all propositions ascribing a self-repairing quality to the calamities which befall the labouring classes" (ibid., pp. 341–42). That Mill retained the Ricardian model can therefore be accounted for by a belief in its continued relevance from a policy perspective, rather than a methodologically based insulation.

Some might denigrate this sort of position as Malthus-mongering. But we can see nothing unreasonable in a concern to assure behavior modification on the part of the unskilled, considering the uncertain future of technology and industrial supremacy. For under adverse conditions in these respects, the model "predicts" deceleration of the rate of accumulation and reduced real wages, and nothing that had occurred over the preceding decades could be said to have refuted this "prediction," considering the countervailing forces which had been, and still were, at play. *These Mill was most careful to spell out*:

> To fulfil the conditions of the hypothesis, we must suppose an entire cessation of the exportation of capital for foreign investment. No more capital sent abroad for railways or loans; no more emigrants taking capital with them, to the colonies, or to other countries; no fresh advances made, or credits given, by bankers or merchants to their foreign correspondents. We must also assume that there are no fresh loans for unproductive expenditure, by the government, or on mortgage, or otherwise; and none of the waste of capital which now takes place by the failure of undertakings which people are tempted to engage in by the hope of a better income than can be obtained in safe paths at the present habitually low rate of profit. We must suppose the entire savings of the community to be annually invested in really productive employment within the country itself; and no new channels opened by industrial inventions, or by a more extensive substitution of the best known processes for inferior ones (ibid., p. 739).

The charge that Mill "insulated" the basic Ricardian growth model by "empty[ing] the appropriate *ceteris paribus* clauses of whatever specific content they may once have had" (Blaug 1992, p. 65) is evidently too harsh. To the contrary, he carefully investigated the record, filling and extending the *ceteris paribus* pound—the list extends far beyond the assumed state of technology, which is what commentators like to focus on—in line with his own recommendation to examine unexplained residuals (above, Section II).[21]

[21] Needless to say, any test had to be fair, as is made very clear in a letter dated 10 October 1871, defending the Ricardian growth model on the grounds that criticism based on the reported circumstance that "the high wages of the United States cannot be caused by cheap land and sparse population, since land is cheaper and population sparser in Canada where wages are lower" was not definitive (1972, pp. 1840–41). Specifically:

> I should require to know, first, between what parts of Canada and what parts of the United States the comparison as to land and population and wages is made; secondly, whether the wages said to be lower in Canada are wages in gold, and, assuming that they are so, whether, when compared with the prices of articles of consumption, augmented as these prices are by your [US] tariff, they do not enable the Canadian labourer to be fully as well off as his neighbour on your side of the frontier. Finally, if those questions were all resolved in favour

The foregoing is one line followed by Mill, in which the disturbing causes, including technological change, are treated as exogenous phenomena unrelated to the land-scarcity-based tendencies.[22] A second line involves actual modification to the land-scarcity-based model in a very novel way better to account for the complete record. We have in mind the impressive integration of secular trend and cycle.

Of speculative periods Mill observed that "all times are so, more or less" (1848/1965, p. 512), but he elaborated in detail on the quiescent period and its place in the cycle.[23] The quiescent period entails expansion rather than stationariness: "Each person transacts his ordinary amount of business, and no more; or increases it only in correspondence with the increase of his capital or connexion, or with the gradual growth of the demand for his commodity, occasioned by the public prosperity" (ibid., p. 662). This expansion constitutes the necessary condition for the generation of regular cyclical fluctuations, considering the consequences flowing from the downward tendency of the profit rate:

> the gradual process of accumulation ... in the great commercial countries is sufficiently rapid to account for the almost periodic recurrence of ... fits of speculation; since when a few years have elapsed without a crisis, and no new and tempting channels for investment have been opened in the meantime, there is always found to have occurred in those few years so large an increase of capital seeking investment, as to have lowered considerably the rate of interest, whether indicated by the prices of securities or the rate of discount on bills; and this diminution tempts the possessor to incur hazards in hopes of a more considerable return (ibid., p. 651).

Again, that "revulsions are almost periodical" is attributed—in the chapter on the "Tendency of Profits to a Minimum" itself—to "the very tendency of profits which we are considering"; for "the diminished scale of all safe gains, inclines people to give a ready ear to any projects which hold out, though at the risk of loss, the hope of a higher rate of profit; and speculations ensue, which, with the subsequent revulsions, destroy or transfer to foreigners, a considerable amount of capital, produce a temporary rise of interest and profit, make room for fresh accumulations, and the same round is recommenced" (ibid., p. 742).

Capital wastage in these contexts, be it noted, is not treated as a disturbing cause, to the extent that it is induced by the falling profit rate. There is, however,

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> of Mr. Greeley [the critic], the only inference that I should draw is, that the arts of production are less advanced and the labour of the community less efficient in Canada than in the United States; the natural effect of which would be to keep wages lower than the circumstances of the country with respect to land and population would otherwise make them. It should be remembered also that (as you observe) Mr. Greeley's sovereign remedy, Protection, exists in Canada, though not to the same extravagant pitch as in the United States.

[22] We recall here Mill's "composition of causes" of the mechanical variety in his *Logic*, whereby each (separate) cause-effect relationship continues to operate even though disguised by the data: "the separate effects of all the causes continue to be produced, but are compounded with one another, and disappear in one total" (1843/1973–74, pp. 440–41). Mill applied the term "tendency" to any *individual* causal relation.

[23] On the reversal of cyclical movements due to changes in the state of expectations, see Peart (1991).

a further modification along similar lines—Mill's endogenization of both invention and innovatory processes:

> When the capital accumulated is so great and the rate of annual accumulation so rapid, that the country is only kept from attaining the stationary state by the emigration of capital, or by continual improvements in production; any circumstance [such as a profits tax] which virtually lowers the rate of profits cannot be without a decided influence on these phenomena. It may operate in different ways. The curtailment of profit, and the consequent increased difficulty in making a fortune or obtaining a subsistence by the employment of capital, may act as stimulus to inventions, and to the use of them when made. If improvements in production are much accelerated, and if these improvements cheapen, directly or indirectly, any of the things habitually consumed by the labourer, profits may rise, and rise sufficiently to make up for all that is taken from them by the tax (ibid., p. 827).[24]

Mill refers here specifically to the consequences of a tax on profits, rather than of the secular fall in the return on capital itself. Whether this was a deliberate omission is unclear. But the context involves a system operating at a low range of profit such that any further reduction as by a profits tax would generate the results in question; at a higher range the artificial reduction might be less effective. The treatment of technical progress in the secular context purely as a disturbing cause therefore becomes inappropriate. And in fact, Mill actually designates the profit-technology relation as a "tendency"—"the artificial abstraction of a portion of profits would have a real tendency to accelerate improvements in production"—according it formally the same status as pressure on profits of scarce land.

As with capital wastage, the concept of a disturbing cause is seen not to be given once-and-for-all; all depends on context. It is true that Mill provided no hard and fast rule to determine when a disturbing cause should be incorporated into the analysis. Generality and quantitative effect were both considered; but beyond this the matter is left open-ended. Yet his appreciation for the enormous complexities of causation in social science led him to focus constantly on the adequacy of the causal structure involved.

VI. THE INVERSE WAGE-PROFIT RELATIONSHIP

The Ricardian inverse wage-profit relation illustrates Mill's response to an anomaly created by new quantitative evidence, in contrast to the foregoing instances involving qualitative evidence. We have in this episode a patently honest attempt to "test" Ricardian theory against the evidence, Mill going to considerable lengths to acquire reliable data and seeking to strengthen the underpinnings of the basic explanatory model.

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[24] Elsewhere there is a notion that only under pressure do firms adopt or approach already known optima; see p. 739 regarding "a more extensive substitution of the best known processes for inferior ones."

We admit our inability to understand Mill's intentions by his remark that "the abolition of the Corn Laws has given an additional stimulus to the spirit of improvement" (ibid., pp. 713–14).

On Ricardian principles the return on capital in the U.S. should have been lower than in Britain, whereas the reverse was the case:

> Have you formed any opinion, or can you refer me to any good authority, respecting the ordinary rate of mercantile and manufacturing profit in the United States? I have hitherto been under the impression that it is much higher than in England, because the rate of interest is so. But I have lately been led to doubt the truth of this impression, because it seems inconsistent with known facts respecting wages in America. High profits are compatible with a high reward of the labourer through low prices of necessaries, but they are not compatible with a high cost of labour, and it seems to me that the very high *money* wages of labour in America, the precious metals not being of lower value there than in Europe, indicates a high cost as well as a high remuneration of labour (letter to Cairnes, 1 December 1864; in 1848/1965, p. 1055).

The higher U.S. interest rate, Mill himself went on to suggest, might be accounted for by the fact that investment was, by and large, of European origin and this required an extra inducement (ibid., p. 1056). Yet he remained uneasy and appealed for more precise statistical data that might throw light on the issue (ibid., pp. 1088–89). "I am much obliged to you," he wrote to Cairnes soon after, "for the trouble you have taken to get information respecting the rate of profit in the United States, but ... [t]he scientific question remains as great a puzzle to me as ever" (ibid., p. 1092); for the new data provided by Cairnes' U.S. informants did not overcome the scientific puzzle—the apparent refutation of Ricardian distribution theory: "From their statements it is clear that the ordinary notion of the extravagantly high rate of profit in the United States is an exaggeration, and there seems some doubt whether the rate is at all higher than in England. But that does not resolve the puzzle, as even equality of profits, in the face of the higher cost of labour, indicated by higher money wages, is as paradoxical as superiority. This is the scientific difficulty I mentioned, and I cannot yet see my way through it" (ibid., p. 1093).

Cairnes' proposed solution—the incomparability of the monetary units of the two centres in which wages were expressed—did not satisfy him: "As far as it goes, I fully admit it; but my difficulty was, and still is, in believing that there can be *so great* a difference between the cost of obtaining the precious metals in America and in England, as to make the enormous difference which seems to exist in money wages, consistent with a difference the contrary way in the cost of labour" (ibid., pp. 1093–94). Shortly thereafter, he cryptically suggested that he had hit on a satisfactory solution to the apparent anomaly: "I am inclined to think that the real solution of the difficulty, and the only one it admits of, has been given by myself in a subsequent place" (ibid., p. 1095). The proposed solution, not spelled out in detail, reverts to the cost of obtaining the precious metals—a line Mill had himself originally abandoned—as elaborated in the chapter "Of Money, Considered as an Imported Commodity," which takes into account the broad implications of the general theory of international values including transportation costs (ibid., pp. 619–20).

VII. CONCLUDING REMARKS: SOURCES OF INTERPRETIVE DIFFERENCES

It remains to sort out the implications of the foregoing demonstrations and to assess how our characterization of Mill differs from the Mill commonly portrayed in the literature. That difference stems from the relative weights placed on the importance of *à priorism* and verification in Mill's method. For Hirsch (1992, pp. 860–1), "extensive feedback from the verification component of practical application to revision of the basic model of economic science" is "not consistent with Mill's *à priori* approach to economic science"; and his "disagreement" with Hollander, as he puts it, "arises primarily because I see Mill's a priorism as being far more important in his approach to science than Hollander does" (ibid., p. 861). He has Hollander attributing to Mill "a kind of anti-*à priori* view" (ibid., p. 865), insisting that the *a priori* method for Mill is:

> *the* method of economics ... and its major characteristic is that the results it yields refer only to what happens *generally* ... This being the case it obviously makes little sense to test theory by observing how well its implications accord with what actually happens in specific instances. Even if retrodictive tests fail, the theory could still be true in general. That is why, of course, detailed investigation of specific experience is not considered by Mill to be part of the *a priori* and could not be considered part of the business of science (ibid., p. 861).

We do not intend in any way to play down the *a prioristic* dimension to Mill's scientific methodology; indeed, we maintain that Millian verification, since it is not part of *a priori* theorizing as such, need not necessarily be carried out by specialist theorists. Rather, verification plays on theory indirectly by suggesting to the theorist the need to improve his models in order to assure their practical relevance, especially for policy recommendation.[25] Furthermore, we fully agree with Hirsch and De Marchi that a failure of a retrodictive test—after allowance of course for "disturbing causes"—will not *necessarily* affect the basic theory, only that it *might* do so. Our point is that Mill, realizing this possibility, recommended continuous verification of theory and engaged in the constant reappraisal of theory in the light of evidence. Even the limited number of case studies considered above demonstrates that he engaged in major modifications of theory, better to allow for, and make sense of, new or newly recognized empirical evidence manifested in the "actual commerce of human affairs" (1848/1965, p. 239) or the "state of facts" (ibid., pp. 380, 383).[26]

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[25] See references above to the use of "our speculations," or theoretical constructs, in "guidance" (p. 375) and "control".

[26] Although Cairnes frequently reverts to the self-evident nature of the basic axioms of theory, he yet also is on record as maintaining that "the only test by which a theory is justified... [is that of] explaining facts, and if it be a new theory... explaining facts not explicable, or not so simply explicable, by received theories"; and Cairnes himself in his empirical work "placed great weight on the systematic use of evidence to illustrate theory and to assess the predictions of theory" (Bordo 1975, pp. 351–54). Bordo raises the question "[w]hether Cairnes would have been willing to discard a theory whose predictions were not consistent with the evidence." At times he resorted to the search for disturbing causes; but he did use empirical evidence to reject Newmarch's theory (ibid., p. 354).

Our conclusion is that Mill focused on observed phenomena that required accounting for, analytically, frequently proceeding in line with his formal methodological account whereby, in the face of extreme causal multiplicity, the researcher might invert the operation of deduction and induction, and commence with an observation requiring theoretical explanation; and that his attempt to account for anomalous facts yielded the imperfect competition analysis and the abandonment of Say's Identity in general and discernment of a linkage between secular trend and cycle in particular—all, in fact, major contributions to theory. On our reading, while Mill's notion of verification is a far cry from a statistical test to which indeed he would have objected strongly (see Peart 1995), his engagement with the real world is most impressive in its qualitative detail.[27]

How do we account for widely divergent readings of Mill? First, there is Mill's strongly worded statements—pre-eminently that verification "is no part of the business of science" (above, p. 369). Professor Hirsch has focused in this context on who, professionally speaking, is to undertake the process of verification. This emerges in his reaction to a statement in the *Essay* regarding the function of the economic theorist:

> Although, therefore, a philosopher be convinced that no general truths can be attained in the affairs of nations by the *à posteriori* road, it does not the less behove him, according to the measure of his opportunities, to sift and scrutinize the details of every specific experiment. Without this, he may be an excellent professor of abstract science; for a person may be of great use who points out correctly what effects will follow from certain combinations of possible circumstances, in whatever tract of the extensive region of hypothetical cases those combinations may be found. He stands in the same relation to the legislator, as the mere geographer to the practical navigator; telling him the latitude and longitude of all sorts of places, but not how to find whereabouts he himself is sailing. If, however, he does no more than this, he must rest contented to take no share in practical politics; to have no opinion, or to hold it with extreme modesty, on the applications which should be made of his doctrines to existing circumstances (Mill 1836/1967, p. 333).

Hirsch (1992, pp. 856–57) finds here proof that Mill could not have believed "that verification was the major route, or even *one* major route, through which revisions in the basic model were made"; for had this been his position, "it would then have been vital for the professor of the abstract science to be concerned with verification; otherwise the professor would cut himself off from the basic source for the improvement of the basic model." Rather, for Mill, "the economic scientist, *as economic scientist*, need rely only on introspection and casual observation" (italics in original).

Such an interpretation, however, neglects Mill's insistence that the "philosopher" is "behove[n] ... according to the measure of his opportunities, to sift and scrutinize the details of every specific experiment." Why should he put the matter this way if verification is not necessary to theory revision? The

[27] Mill was preoccupied with the causal structure–assuring its adequacy by making revisions–in a way foreign to modern hypothesis testers who seek for a narrow H_O/H_A significance test and obtain a yes/no answer, but who desist from considering the entire set of causal relationships (see Peart 1995).

issue relates to the immediately preceding proposition in the *Essay* that verification "often reveals to us that the basis itself of our whole argument is insufficient; that the data, from which we had reasoned, comprise only a part, and not always the most important part, of the circumstances by which the result is really determined" (cited above, p. 375). "Such oversights," Mill believed, "are committed by very good reasoners," and are "a kind of error to which those are peculiarly liable whose views are the largest and most philosophical" (above, p. 376). It seems plausible that when Mill referred to a neglect by the philosopher of specific experience as an oversight and a kind of error, he meant to be taken seriously. Most important, what would be the purpose of verification should it yield flaws in the body of theory (as Mill insists that it might) that may be ignored by theorists? A fundamental objective of theory, for Mill, is to serve in application, and the test of useful theory for that end is satisfactory retrodiction; yet on the reading of Mill we oppose there is no sort of verification failure that is to be allowed to affect the body of theory.

We do recognize, however, that there is no necessity for the theorist himself to engage in verification; he need only be attentive to the outcome of verification perhaps undertaken by others. Mill says just this when he expresses the yet to be achieved ideal:

> But while the philosopher and the practical man bandy half-truths with one another, we may seek far without finding one who, placed on a higher eminence of thought, comprehends as a whole what they see only in separate parts; who can make the anticipations of the philosopher guide the observations of the practical man, and the specific experience of the practical man warn the philosopher where something is to be added to his theory (1836/1967, pp. 334–35).

But however the tasks are allocated,[28] theoretical demonstration amounted only to conditional demonstration—"a demonstration nisi—a proof at all times liable to be set aside by the addition of a single new fact to the hypothesis" (ibid., p. 334).

Our reading is reinforced by recalling that while verification may reveal the need for improvement in the axiomatic foundation or even in the logical process itself, it is not itself a device for the derivation of complex causal relations in the face of the problem of "composition of causes." That remains the function of ratiocination.[29]

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[28] Mill even found a gender basis for specialization in contemporary England: "To discover general principles, belongs to the speculative faculty: to discern and discriminate the particular cases in which they are and are not applicable, constitutes practical talent: and for this, women as they now are have a peculiar aptitude" (1869/1984, p. 305).

[29] Making allowance for the indirect nature of any effect that verification might have on theory reduces the contrast that Hirsch (1992) perceives between the second and third sections of Mill's *Essay*. In fact, Hirsch himself is bothered that "the divisions are somewhat obscured by the fact that Mill sometimes uses the term 'political economy' to refer only to abstract or scientific economics but at others to both abstract and applied economics together" (p. 845), a practice generating a "clumsiness" of outcome (p. 846). This practice would be neither clumsy nor obscure were it Mill's actual position, as we believe it to be, that there is no fine distinction to be made between abstraction and application, in the sense that the two are part of an overall procedure entailing abstraction leading to verification which may reveal weaknesses in, and generate modifications to, the abstract structure.

As we have seen, Mill left himself open to alternative readings by some of his pithy formulations. Another striking instance occurs in his account of "the creation of a distinct branch of [social] science" turning on "one class of circumstances only," such that "even when other circumstances interfere, the ascertainment of the effect due to the one class ... is a sufficiently intimate and difficult business to make it expedient to perform it once for all and then allow for the effect of modifying circumstances" (1843/1973–74, p. 901). This and similar expressions would doubtless encourage the interpretation that we are challenging. But in the light of the evidence provided above, Mill must not have intended "once for all" literally, but only as a conditional, empirically-based, first approximation. Mill himself, in this very context, justifies a specialist economics on the grounds that the relevant phenomena "*mainly* depend, at least in the first resort, on one class of circumstances only". In brief, the behavioral axioms themselves are provisional; and the specialist procedure only worked, even provisionally, when "the diversities of character between different nations or different times enter as influencing causes only in a secondary degree" (ibid., p. 906).[30] Accordingly, as he put the matter in a general account of hypothetical science, it is only "if we find, and in proportion as we find, the assumptions to be true" that the process "may be performed once for all, and the results held ready to be employed as the occasions turn up for use" (ibid., p. 259).

Mill's forceful statements championing "the method *a priori* in Political Economy" provide a further illustration. We suggest that these reflect his hostility towards the "inductivists" and his condemnation of irresponsible and biased appeals to experience for substantiating causal relations.[31] As with all the classical economists, abstraction and deduction were held to be indispensable.[32] But this does not mean that he denied the essential role of experience in verifying and possibly generating improvements to the theoretical formulation. His formal exclusion of verification from the realm of deductive theory reflected not a belief that it can have no effect on theory, but the indirect nature of any effect it may have.

There is, too, the uncompromising assertion that the scientific basis for economics turned on the axiom of competition. Yet we have seen that Mill

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Mill does indeed move back and forth between subject matter in the *Essay*. For example, in the second, not the third, part will be found both the allowance for the absorption of the population principle into an analytical structure, albeit supposedly based on the "three behavioural laws," and also treatment of disturbing causes which interfere in "particular cases," albeit the subject matter of application. Conversely, in the third part on application will be found the bulk of the texts recognizing the possibility of deficiencies to the theoretical structure revealed by verification, especially by retrodiction.

[30] That Mill himself made no concerted effort to incorporate knowledge creation, we suggest reflects the circumstance that the causal phenomena relevant for the generation of knowledge touched too closely upon the problem of social progress in the evolutionary sense (see Hollander 1985, pp. 191f).

[31] For the strength of his hostility in the *Essay* on method–it might account for its structure–see Mill (1836/1967, p. 334). Other examples spanning Mill's entire career will be found in Mill (1823/1986, p. 40 and 1867/1984, pp. 236–37).

[32] Mill, however, gave himself a difficult task, for he was also, if perhaps not equally, concerned with over-confident theorists, all too ready to apply the unqualified results of the model, those, that is, who practised the (so-called) "Ricardian Vice."

himself proceeds on an unorthodox behavioral basis, entailing non-maximizing consumer behavior—only some instances of retail pricing can be described in terms of high search costs—and pricing decisions by firms entailing "customary" mark-ups, to achieve an analysis of price discrimination and also one of monopolistic competition which recognized the efficiency losses of excess capacity with reduced market shares for each competitive firm. Even custom is thus absorbed into the model to a degree, providing perhaps an instance of Mill's observation in the *Essay* that "disturbing causes ... which operate through the same law of human nature out of which the general principles of the science arise ... might always be brought within the pale of the abstract science, if it were worthwhile" (above, p. 375).

There are doubtless other explanations for the view that Mill excessively championed *a priorism*. Consider Mill's response to the apparent anomaly created for the Ricardian inverse wage-profit relation by the comparative U.S.-UK data (above, Section VI). It is impossible to tell what steps he would have taken had he still remained unable to account for the empirical record. But until such time, he treated the Ricardian distribution model as robust, for there is in his approach a basic confidence in its validity, reflecting a belief that 1817 opened up a new era in theoretical economics, which would not in all likelihood be reversed. Here a suggestion by Pedro Schwartz comes into play—that Mill's philosophical perspective played down the likelihood of fundamental revisions (Schwartz 1972, pp. 236–37); it is theory "improvement" on the basis of the accumulation of factual knowledge rather than "displacement" that is expected to flow from the testing process.[33] This orientation might lie behind the unfortunate pronouncements that the theory of competitive value neared perfection (1848/1965, p. 456). Mill allowed himself to be carried away, for we know that he applauded ongoing improvements such as those by W.T. Thornton and, of course, he himself made major contributions to the theory of equilibration. Such strong statements, however, do not negate Mill's clear allowances in principle and practice for the improvement of theory in the light of empirical evidence.

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[33] Cf. Mill's early comment regarding the physical sciences as "continually growing, but never changing: in every age they receive indeed mighty improvements, but for them the age of transition is past" (1831/1986, p. 240). Jevons objected strongly: "In the writings of some recent philosophers, especially of Auguste Comte, and in some degree John Stuart Mill, there is an enormous and hurtful tendency to represent our knowledge as assuming an approximately complete character. At least these and many other writers fail to impress upon their readers a truth which cannot be too constantly borne in mind, namely, that the utmost successes which our scientific method can accomplish will not enable us to comprehend more than an infinitesimal fraction of what there doubtless is to comprehend" (1877/1907, pp. 752–53).

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