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REPLY TO HOLLANDER AND PEART'S "JOHN STUART MILL'S METHOD"

BY

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I do not think that it would serve much of a purpose to answer Hollander and Peart (1999) point by point, to repeat old arguments, and to once again cite long quotations from Mill. Furthermore, I doubt that the Editor of this *Journal* would give me the space and time to do so.[1] Instead I propose to ask a number of basic questions about Mill's methodology, give my answers to them and show why I find fault with what I take to be Hollander and Peart's answers. I doubt that this will convince my adversaries. But I hope to show our readers what the major issues are in the disagreement and, perhaps, even to get Hollander and Peart to agree that these are the issues that separate us. If I fail in this latter task they will have the opportunity to point this out in their Rejoinder.

Before I get to my questions, however, I need to make a point that applies to all of them. A major problem I have with Samuel Hollander's interpretation of Mill's methodology, which carries over to the most recent version, now done with Sandra Peart, is that their interpretation takes for granted that Mill's view of economic science and theirs is the same. It is my contention that Mill's view is not the same, and out of this difference derives much of our difference about how Mill's methodology needs to be interpreted. Mill had very definite ideas and has set them down for all to see. Hollander and Peart have not ignored this body of Mill's work, but, it seems to me, they have not given to it the kind of close attention that they give to Mill's pronouncements about economics.

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[1] Almost ten years ago I sent Professor Hollander the manuscript of an article on Mill I had written, which was later published (Hirsch 1992), and asked for his comments. In due course I received an acknowledgement telling me that an adequate reaction would require a full-fledged response for which Hollander did not have the time at the moment and promising a response when the time came. To my great surprise, I recently received the article by Hollander and Peart from the Editor of this *Journal*; apparently these are the comments that I had not received a number of years earlier. At this point I am not in the best position to respond; I have had difficulty locating old notes, let alone recalling old thoughts. And a reply, limited, as it is by time and space, is not the ideal format.

But I have been able to incorporate in this Rejoinder observations derived from unpublished research I have done since my last published work on Mill. One such area has to do with the differences between Mill's logic of economics and his logic of other sciences, dealt with under question 2 in what follows, and another has to do with changes in Mill's *a priorism* over time, to show that such changes did not affect Mill's *a priorism* as it applied to economics.

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1. *Why did Mill say in the* Essay On the Definition of Political Economy; and on the Method of Investigation Proper to It *(1836/1967, hereafter* Essay*) p. 325, that "[to] verify the hypothesis itself* a posteriori*, 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?"*

For Mill science includes a body of knowledge that is unquestionably true, that has no falseness in it, and that is a way of deriving knowledge that is unquestionably true.[2] That is why Mill tells us (*Essay*, p. 337) that "[with] regard to *exceptions*; in any tolerably advanced science there is properly no such thing as an exception," and he insists on using the *a priori* method in economic science (*Essay*, p. 327) because it is "the only method by which truth can possibly be attained in any department of the social science." We might be able to increase our knowledge of empirical generalizations by using the *a posteriori*, but such results would not fall within the realm of science. Mill further tells us (*Essay*, pp. 323, 326; *A System of Logic, Ratiocinative and Deductive* 1843/1973–74, book 6, chapter 9, section 3, hereafter *Logic*, 6.9.3.) that by design economic science limits itself to the most important motive, which operates generally in its area and only in very limited ways with the other things.

As a result, since a good bit may be left out, we could hardly expect that the implications of such a logical system will accord with what actually occurs. They may be more or less true, but certainly there is falseness there. Furthermore, so long as we continue to conceive of economics this way, we cannot expect that this situation will change in the future. No matter how intensively we try to improve our basic model of economics, as long as we stick within the parameters laid out by Mill for economic science, its implication would continue to remain partly untrue. Thus, verification does not qualify as an ingredient of science in Mill's methodology of economics. If it did, all the implications of the basic model of economics would have to coincide with what actually happens, as they do with Newton's mechanics, according to Mill, or there would have to be the possibility that some time in the future all of the implications of the basic model would be true, a possibility that is excluded, as has been noted, because the way economics is conceived in the Millian system makes this impossible. This is a perfectly straightforward way of explaining why Mill made the statement about verification and science in the *Essay*. There is no need to use the sophisticated reasoning that Hollander and Peart engage in to try to explain it away.

Hollander and Peart reject this explanation and argue instead that verification

[2] Mill, not infrequently, runs together the process of deriving a certain idea with the matter of judging it logically. I point this out at this place because it is clear that for Mill "science" included only material that was true without exception. But in some places there is a hint from Mill that it is also a method by which absolutely true results can be achieved. For our purposes this problem does not matter because for economic science, according to Mill, (1) the implications are not part of science because they are not true without question, and (2) verification of the implications cannot be considered a scientific process because we cannot with the help of this process hope to derive results that are absolutely true.

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was ruled out of science by Mill because to Mill verification "is not itself a device for the derivation of complex causal relations in the face of the problem of 'composition of causes'." But to maintain their position Hollander and Peart would need to explain why in the *Logic* (3.11.3) Mill argues not only that verification *is* part of science but that it is the *critical part*. The difference can be explained by the fact that in Book 3 Mill was not referring to *economic* science. Economic science, as Mill made clear in the *Logic* (6.9.3) and earlier in the *Essay*, is a very special kind of science. And it is for this specific area that verification is not part of science. I should add, in this regard, that in Hollander and Peart's quotes from Mill it should be evident that Mill is talking about application even where Mill talks about "theory" and "abstract system." I get back to this matter in what follows.

2. *Did Mill then not contradict himself in the* Logic *(3.11.3) when he tells us that verification is an essential ingredient of science?*

Hollander and Peart seem to take it for granted that Mill had only one model of science. Mill had somewhat different models for different sciences, and if we ignore this we overlook some significant features of Mill's methodology. Thus, Mill makes a distinction between sciences, such as Newton's mechanics, which are "exact," and others, like tidology and meteorology, which are not exact, as Mill explained (*Logic*, 6.3.1). An exact science takes account not only of the "causes which affect the phenomenon in all cases, and in a considerable degree; … [but also the] others which affect it in some cases only, or if in all, only in a slight degree." Thus it enables us "to deduce the completed law of the phenomenon, by compounding the effects of the greater with those of the minor causes." The "completed laws" enable us to fully explain the facts of specific experience. Such sciences thus enable us to make retrodictions that are true. Mill refers to verification in such sciences merely as "verification," with no adjective attached (*Logic*, 3.11.3); it is very much part of such sciences.

For inexact sciences (*Logic* 6.3.1 and 2) we know only some of the laws, generally the major ones, and this lack makes it impossible to deduce the completed law of the phenomenon. Economics is actually an extreme case of inexactness because, not only is it not an exact science but, as I have noted, we would never expect it to be exact because of the way we have designed it. That is why the implications derived from the premises are unlikely to coincide with what actually happens (that is, they are not entirely true), and as a result Mill relegates the implications out of the realm of science. When we "verify" in economics by comparing our hypotheses for policy with what actually happens when the policy is applied, Mill labels this process (*Logic*, 6.9.5) "indirect verification." Indirect verification does not establish scientific truth. Yet economics is a science in Mill's view: it is the truth of the major causal law, or laws, which makes economics a science.

On the basis of what has been said, I question Hollander and Peart's use of what Mill says in Book 3 of the *Logic*, that does not apply to inexact sciences, and especially not to economics, as if it did apply to economics. In other words, it is hard to avoid the conclusion that Hollander and Peart have run together what

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Mill calls "verification" with what he calls "indirect verification." This confuses matters and makes it difficult to avoid erroneous conclusions.

I have the same problem when I find Hollander and Peart attributing to Mill views about economic science on the basis of what he says about sociology and ethology. As Mill explains in the *Logic* (6.9.2), because of the complexity of social phenomena, the mode of inquiry appropriate for economics "must therefore, on pain of frivolity be limited to those classes of social facts which, though influenced like the rest by all sociological agents, are under the *immediate* influence, principally at least, of a few only." For the rest of the social sciences we invert the process of inquiry, starting with empirical generalizations, which are derived from experience, and try to fit them to causal laws from which such generalizations can be deduced. In such a science "perpetual verification" must take place where the "conclusions of theory cannot be trusted unless confirmed by observation; nor those of observation unless they can be affiliated to theory by deducing them from the laws of human nature …" (6.5.6). Hollander and Peart quote this as evidence of Mill's views about economics. But here Mill is very explicit that this process does not apply to economics. He tells us: "… in those more special inquiries which form the subject of the separate branches of the social science, this twofold logical process and reciprocal verification is not possible …" (*Logic*, 6.9.6). Economics, of course, is the major one of the special inquiries, if not the only one, to which Mill refers. The material from Mill that Hollander and Peart quote in fact supports an interpretation opposite from theirs. While for *sociology* Mill makes the feedback from the verification of implications an inherent ingredient of the method, for *economics* he tells us that it cannot be done. That it can be done is one of the pillars on which Hollander and Peart's interpretation rests.

3. *Why did Mill structure the* Essay *the way he did and what is the consequence of this?*

Mill wanted to show that the *a posteriori* of the "practical man" was not an appropriate means for deriving scientific knowledge in economics. In addition, he wanted to show how to derive results in economics that met the requirements of science. He accomplished both of these objectives in the middle part of the work. But this work showed that economic science was not as potentially useful as we would like it to be, and so Mill went on in the last part of the *Essay* to show how we can derive a more practical economics by going beyond science and adding to it non-science elements, such as the empirical generalizations and knowledge of the details of the specific practical case being considered, which the "practical man" could help us with. The division of the *Essay* into one part on abstract economics or economic science and another on the application of science[3] derives from Mill's objectives in writing the *Essay* on the one hand, and from his ideas about science and practical application on the other. Mill is very clear about this. After talking about economic science in the middle part of the

[3] The first part of the *Essay* deals with the definition of political economy and does not concern us here.

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Essay, concluding with the statement, "[o]f this character is the science of Political Economy in the writings of its best teachers" (p. 329), Mill introduces the last section with the comment: "When the principles of Political Economy are to be applied to a particular case, then it is necessary to take into account all of the individual circumstances of that case …" (p. 330). Then he goes on to talk about the application of economic science; he considers how disturbing causes are to be dealt with in application and tells us where we have been and where we are going: "Having now shown that the method *a priori* … is the only certain or scientific mode of investigation … We will be able to show that the [method *a posteriori*] … is not withstanding of great value in the moral sciences … not as a means of discovering truth, but of verifying it …" (p. 331). Here verification is no part of science for Mill. In other words, he is now in the area of application. Mill does not shuttle back and forth between the two parts and the two objectives. His structure is quite transparent if one is aware of Mill's views about science.

Hollander and Peart disagree with what I have said about the structure of the *Essay* and point out that under my classification the population principle should have been discussed in the last section, which deals with the application of science, and not in the middle one, which deals with the basic model of economic science. But Mill himself was aware that he was violating his own system of organization in doing this. Not only that, but he was also violating the rationale of his division between science and the application of science itself. He tells us that in dealing with the principle of population, "the strictness of purely scientific arrangement being thereby somewhat departed from for the sake of practical utility …" (p. 323). In other words, had Mill not departed from the strictness of purely scientific arrangement, he would not have included the population principle with abstract or scientific economics. And had he stuck to scientific arrangement he would have considered the population principle in the last part. Of course, had Mill deviated from what he himself considered the logical arrangement many times then it might be appropriate to question whether for the purposes of interpreting the *Essay* the logical arrangement means very much. But this is the only place where Mill tells us that he is deviating from the logical arrangement, and it is the only place, as far as I am aware, where he does in fact deviate.

A final point should be made about the structure of the *Essay* which, in my view, shows how consistent Mill turns out to be. Mill deals with the formulation of economic "theory" in both the middle section, which deals with economic science, and in the last part, which deals with application. In the middle section Mill tells that we formulate economic theory by hypothesizing about causes and deriving effects from these causes by using the economic motive as causal link. In this scheme we derive partial truth if the causes are in fact true. There is no explicit mention of verification. In the last part of the *Essay* verification is the central feature of the process through which new theory is formulated. The reason for the difference is that in the middle portion Mill is dealing with "theory" as conceived in the field of economic science. In the last section the "theory" Mill is concerned with refers to hypotheses designed to deal with particular problems at particular times. In the latter case it makes sense to refer

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to hypotheses in application as "theory" because they are the reasoned-out hypothetical part in the applied area. After all, in arguing about what to do about a particular problem at a particular time, to make a credible case for a proposed resolution, one cites evidence but also uses "theory" that relates the evidence to the desired outcome. This is the case Mill makes in the last part of the *Essay*. There is far more involved in the "theory" in application than in the "theory" of the basic model of the science; it may include elements from sociology and ethology, minor causes that are not included as part of the basic model, empirical constants contributed by the practical man, and the combination of any or all of them. And it is on the basis of *this* "theory" that one has the confidence to proceed with the business of policy execution.

4. *How much of a role, according to Mill, does verification play in the revision of abstract economics or the basic model of economic science?*

Hollander and Peart repeat at length much of what Hollander has said before and I do not think it would serve much of a purpose if I were to follow their example. I will, therefore, confine myself to making a few very general points. To begin with we have to determine whether, in Mill's view, the basic model as such is verifiable, and if not, how it might be used to fashion a device through which we might be able to perform such verification.

In the middle part of the *Essay* that deals with science, Mill says nothing about verification and tells us that economic science has the character of geometry; geometry, of course, cannot be and need not be verified. In the *Logic* (6.9.6), which refers to economics, Mill tells us that the hypothesis we use for testing can legitimately be tested only for the specific case for which the model was formulated. This means that for a hypothesis to be tested it must contain things other than the basic model of economic science. And, indeed, in the last part of the *Essay* Mill tells us what these things are: input from the ethologist and sociologist, and the knowledge of specifics of the "practical man," whose procedures are not scientific. Because of these additions verification of the basic model of economic science—if indeed the basic model could be verified—turns out to be a more problematic procedure than at first appears.

First, the "theory," hypothesis, or set of related ideas that we are able to test includes input from the sociologist and ethologist, and the economist is not competent to judge this component of the theory when the model that is being used for application falls short. In fact, the model may also include, as I have noted, the non-scientific generalizations of the "practical man" and it would take another "practical man" to adequately judge whether this component of the hypothesis is the element at fault. Merely finding deviations between the expected and the actual is not a simple matter for the economist alone. Second, Mill uses language that suggests that it is not the basic model of economic science that is being tested; he speaks, for example, of the basis of *argument,* of *the system of opinions,* and *errors in thought*. This suggests that Mill is talking in very general terms about reasoning in the policy process, so that when at the end of this discussion he talks about imperfections in the *abstract system*, it does not appear that he is talking about the basic model of economic science at all.

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Third, in talking about verification Mill brings in "the practical man," the "practical statesman," the "speculative politician," "the philosopher." How the economist operates in this environment is not clear. Fourth, Mill's focus is to try to gain more "confidence" for one's ability to predict, not to improve the basic model of economic science. Fifth, suppose the economist tries to improve his model after he comes to the conclusion that its deviations when applied result from defects in the abstract economic elements of the hypothesis. He might add factors from a "disturbing cause" category to the specific hypothesis in question but this is likely to be for this case only.

It is conceivable, as Mill suggests in the *Essay*, that the economist comes to the conclusion that the disturbing causes, particularly if they relate to the economic motive, are important enough to be incorporated into the general model of the science. But to do this intelligently the economist would have to wait for a great deal of additional evidence, since unless this deviation is found in many other instances it would be inappropriate to make even a relatively small change. Sixth, might the economist conclude that the basic model of economic science had falseness in it? Not in Mill's system because, having accepted the basic model in the first place, he had certified that its axioms are true.

Of course, it is conceivable that the conclusion that the basic model of economic science falls short could result from a verification process after a lot of tests of policy hypotheses had been performed over a long period of time and covering a very large number of different types of applications, though the likelihood of this happening seems very small. But even if we did manage to overcome all the obstacles and come to a consensus that the model is lacking, there still remains the question of how much help could be expected from verification in deriving an amended model that is a generally more useful instrument. To do this we would need to reason backwards from the specific facts of different types of examples in order to derive possible causes that would yield implications that are closer to the data when the disturbing causes are added to them. Mill did consider such a process, which I referred to as perpetual verification (in Mill's *Logic* 6.5.6), and prescribed it for ethology and sociology. But Mill prohibits this process for economics, as I have noted. This creates an anomaly.

While Mill did not say this directly, we can infer from what he did say that there is at least a slight possibility that, after extensive study of a wide variety of data, it might be reasonable to conclude that our basic model is lacking. But then how should we go about changing and improving it? Hollander and Peart tell us that Mill recommended perpetual verification for economics, but this was not consistent with what Mill actually said as I noted above. Then what are we to do? The best answer, I think, is to use the same means that Mill suggested in the *Essay* for economic science, namely, to consider hypothetical possibilities and reason out by using the economic motive as axiom and then adding to each of the models the other things that are needed to bring the result closer to what actually happens. But however that may be, I find little evidence for the argument that "Mill allowed for the alteration of the basic model in consequence of the procedure of verification—testing against specific experience: its 'improvement,' 'correction,' 'completion,' 'extension' " (Hollander 1985, p. 130),

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giving the impression that Mill took this to be an everyday occurrence that goes on routinely and continuously over time. As I see it, what Mill says about testing hypotheses designed to deal with policy questions, Hollander and Peart take to apply to the testing of the basic model of economic science.

5. *Did Mill believe that the basic axiom of economics was universally true?*

I have an even more serious problem with Hollander and Peart's argument that Mill held that "the central behavioral axiom [of economic science] is not of *universal* relevance but is pertinent only to the *local* circumstances of contemporary Great Britain and America … ." It seems to me that if we accept this interpretation we would have to conclude that Mill did not consider economics to be a science even in its abstract or science form. I say this because for Mill, as has already been noted, science involves absolute truth, and in economics, as in other "inexact" sciences in Mill's view, the absolute truth is found in the causal law (or laws) embodied in the premises of the theory. This, of course, is the central behavioral axiom in economics, as everyone knows. What pretensions can economics have to be called a science if its central causal law is so limited? This paradox is solved when we recognize, as almost all interpreters have, that Mill believed the central behavioral axiom to be universally true. Mill tells us so himself. In the *Essay* (p. 329) he argues that "the conclusions that are correctly deduced from the assumption constitute *abstract* truth … and when completed by adding or subtracting the effect of the non-calculated circumstances, they are true in the concrete … ." It looks to me as if Hollander and Peart have conflated assumptions and implications. If the economic motive is weaker in French than in British businessmen, it no more shows that the fundamental assumption of economic science is relative, anymore than the lifting of a hot-air balloon when it is released shows that the gravitation assumption in Newton's system is not universal. French businessmen acting differently is an implication of an explanation that includes the basic behavioral assumption plus other causal laws or disturbing causes that are not the same in both cases. In the Millian system the fact that the implications differ merely shows that there are different causal laws to be considered in both cases. To believe otherwise would force us to conjecture that Mill as economic methodologist did not know what Mill the logician stood for. I do not find this a plausible line of explanation.

In a way, this point is basic to the disagreement between Hollander and Peart and myself. After all, if the causal law or laws embodied in the model of economic science is only true in a restrictive sense, like the hypotheses designed for practical purposes, then the distinction between science and the application of science loses its meaning. By the same token, the distinction we make in logical status between the basic model of economic science and hypotheses used to predict for policy purposes evaporates because they are all merely empirical generalizations. That would be an easy way to end the argument! But I do not think that it would be a wise way because if our purpose is to understand Mill's logic of economics, this formulation would get us away from our objective rather than towards it.

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6. *What does it mean to say that Mill was an* a priorist*?*

Since Hollander and Peart and I agree that Mill was an *a priorist*, this point need not be debated. But what does this mean? It is in this area that differences between Hollander and Peart and myself appear. Mill's *a priorism* is most clearly revealed in an example he gives in the *Essay* (p. 325). The question at issue is how absolute kings behave. The *a priorist* would not, argues Mill, observe how absolute kings have actually behaved. Instead, he would study how human beings tend to behave and from such study claim that we are justified in inferring that a despotic king would make bad use of power, and "this conclusion would lose nothing of its certainty even if absolute kings had never existed or if history furnished us with no information of the manner in which they had conducted themselves." Note that no verification is required to achieve *certain* conclusions once assumptions believed to be true are used and no error in logic is made; as Mill suggests by way of justification, this is the way of geometry.[4]

That is essentially all that Mill tells us in the *Essay* about the nature of economic science. Had Mill not had a very strong interest in practical application he might have left the matter there and there would be no question about the nature of his *a priorism*. But Mill did have practical interests, as I have previously noted, so he went on in the *Essay* to talk about the role of disturbing causes, thereby going a little beyond the strict *a priori*. A fundamentalist *a priorist* would find no reason to do this, nor would he find reason to make a distinction between sciences that are "exact" and those that are not exact. In his *Logic* Mill did make this distinction, as I have shown, developing in Book 3 a chapter on method (3.11.3) that differs in an essential way from what he had said about method in the *Essay*.[5] This puts Mill a little further still from the simple *a priorist*. But it is in Book 6 when talking about sociology, ethology, and history, that Mill almost seems to abandon the *a priori*. He there proposes an inverse method, different from the one he had formulated in the *Essay* and different, too, from the one he formulated in Book 3.

In this approach one starts with broad empirical generalizations and then works backwards looking to find premises whose implications are consistent with the empirical generalizations. Such a method can be said to go beyond empiricism because one goes beyond the empirical generalizations and explains them. But still this method of "perpetual verification" can be said to be inconsistent with the *a priori*, first, because it suggests starting with empirical generalizations—that is what the unscientific "practical man" does. More important, in such a procedure implications are in no sense inferior or secondary to the causal laws used in the axioms since one depends equally on both. It is impossible to have a science in this scheme whose scientific status depends entirely on the truth status of the axioms and not at all on that of the

[4] There is the question whether this is a random retrogression on Mill's part to an earlier way of thinking or, as seems likely to me, Mill is talking here about what happens "in general" or "in the abstract," as he did in the middle part of the *Essay*. But I cannot pursue this matter here because it has little bearing on issues involved in my disagreement with Hollander and Peart.

[5] At least it seems to differ since in Book 3 Mill makes verification an essential ingredient of science. It is apparent only, however, because in Book 3 Mill is not talking about economics.

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implications. While it is true that the explaining is done by the more general axiom, were there no implications considered to be reliable there would be nothing to explain. In fact, with this method the feedback from the test of implications to the determination of the correct premises is the essence of the method. In this respect, Mill had all but given up the *a priori* by the time he got to this point,[6] and one can understand Hollander and Peart's arguing the way they do because they take this to be *the method* of Mill.

It is here that I disagree with them. I do so because Mill says explicitly (*Logic*, *6*.9.5), as we have seen, that "in those more special inquiries [of which economics is the most important member] which form the separate branches of the social science, this twofold logical process and reciprocal verification is not possible." Mill may have moved more or less from the *a priori* in different places, but for economics he moved only to the extent of allowing for disturbing causes. We can argue about why this is so but that it is so, is clear.[7] Of course, it may be legitimate to gather together what a writer has said about science and about method in different places and at different times to strengthen one's case about an interpretation. But this is legitimate only so long as it is clear that the context of all of these remarks is the same. In this instance it is not, because there are subtle differences between Mill's views about different sciences, of economics from other sciences, and particularly the exact ones from economics.

7. *What do Hollander and Peart's examples show?*

Hollander and Peart's examples are designed to bolster the case for their version of Mill's methodology, but I fail to see how the examples actually accomplish this. For example, when what looked like falsifying evidence showed up for the Ricardian growth model, Mill tried to account for the difference, and his refusal to reject a piece of the model of economic science does not show that he merely brushed the facts away when they were inconvenient. In fact, had Mill rejected the model on the basis of the evidence offered this would indeed have been a significant victory for Hollander and Peart and defeat for my interpretation.[8] My point is that this is what we should expect from reading Mill's logic of economics. A basic segment of the Ricardian growth model could not be rejected in the Millian logic of economics on the basis of the evidence offered.

In the retail trade example Hollander and Peart show quite properly that, as Mill makes clear, competition lies at the very heart of his basic model of economic science. And as Hollander and Peart show, too, Mill found that in retail trade competition was so weak that in some instances it had a smaller influence on price than did custom. Yet Mill continued to argue that competition was at the heart of the model of economic science. But that is exactly what we would expect, in my view. Had Mill "improved" his model to reflect this aspect

[6] I say "all but given up" because as shown in his argument with William Whewell (*Logic*, 3.12.4 and 3.12.6), Mill never completely gave up the *a priori*.

[7] The most plausible answer, it seems to me, is that, as Mill put it in the *Essay*, "such is undoubtedly its character as it has been understood and taught by its most distinguished teachers."

[8] It is interesting that in none of Hollander and Peart's examples does Mill ever reject *any* piece of the economic model.

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of reality, he would have weakened it as a tool of analysis or an *organon*. One could say, as do Hollander and Peart, that since Mill tries to identify the cause or causes of the deviations of what happened from the basic model, that Mill absorbed "into the theoretical model … features of reality reflecting 'custom'." But note that this use of the term "theoretical model" involves the use of an hypothesis to explain a specific set of facts; it does not refer to the basic theoretical model of economic science.[9]

8. *How could Hollander and Peart present such an array of evidence against me if my position about Mill's methodology is essentially correct?*

The attempt to answer this question gives me the opportunity to summarize succinctly what I have said above about why I have doubts about Hollander and Peart's position. First, Hollander and Peart fail to make the distinction between an empirical generalization and a causal law, a distinction that I think is very important in Mill's *Logic* (3.16). That, for example, is the reason they can argue that the basic axiom of economic science is a very limited generalization. Second, they believe that whatever Mill says about science generally (*Logic*, 3.11), or about ethology and sociology (*Logic*, 6.5.6 and 6.9.1), was meant to apply to economics as well. This stance makes somewhat plausible their argument that economic science was and should be constantly and routinely revised and improved through verification. I would almost agree with them if they were referring to Newtonian astronomy or ethology and sociology. I cannot agree in the case of economics. Third, Hollander and Peart take it for granted that every time Mill talks about "theory" he is referring to the basic model of economic science, even when he is involved with the policy process. For example, when Mill is talking about "the whole basis of the argument" in a policy discussion, Hollander and Peart take this to apply to the basic model. I disagree. I believe that Mill, like the rest of us in this kind of situation, often had in mind the causal part of the formulation in favor of, or against, an argument for a policy proposal at a particular time and place. He was not referring to a formulation that shows the implications that follow logically from assumptions, including the causal law or laws of the basic model of economic science.

I invite the reader to compare Mill's position on these questions with the position Hollander and Peart attribute to him. And I invite Hollander and Peart to comment in their Rejoinder on whether the position I attribute to them about Mill's economic methodology is correct, and if not, what basis they have for their disagreement. Though their article is long and backed by many previous writings, these questions, so far as I am aware, have never been addressed.

[9] Hollander and Peart observe: "Mill remarked on the universality of the *method* rather than the *specific content* of economic theory" but they fail to observe that at the heart of the method is the *organon* or basic model of economic science and that the *organon* and the theoretical explanation one derives in the specific case are not the same thing.

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