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Should Economics Be an Evolutionary Science? Veblen's Concern and Philosophical Legacy

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"Why is economics not an evolutionary science?" It is now over a hundred years since Thorstein Veblen formulated this question, possibly the most famous of all questions in the history of economics. In an essay acknowledging Veblen as "the founding father and guiding spirit of American institutionalism," Clarence Ayres concluded that it is the sorts of questions he asked that "reveal the significance of Veblen's legacy" (1963, 62). But what exactly is the feature of Veblen's legacy whose significance is revealed by a largely philosophy-of-science question such as this?

Despite some recent rather compelling assessments that Veblen's philosophical contribution is primarily a "deconstructive" project in epistemology, one similar in some ways to that of modern postmodernists (Hogsbergen 1994; Peukert 2001; Samuels 1990, 1998),[1] I want to argue to the contrary that, in matters philosophical at least, Veblen's primary legacy is (i) a constructive program after all (as traditionalists within institutionalism have mostly maintained) albeit one that is (ii) grounded in ontology (as few institutionalists appear explicitly to have argued). With space here highly restricted I mostly concentrate on defending (i) against recent criticisms, though I do also, and necessarily, touch on (ii).[2]

Method, Theory of Knowledge, and Judgmental Orientation

To proceed quickly to the heart of the issue, it is my assessment that there are in the literature two competing prominent (if mostly implicit) interpretations of Veblen's

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stance or concern in posing his famous question. I want to add a third. To schematize the commonalities and differences in these interpretations it is useful to distinguish three separate orientations that any commentator may take regarding different aspects of a science process in which a given method becomes dominant. These are, first, the method of science under discussion (X); second, the process (Y) by which method X has, or is expected to, become influential; and, finally, the evaluative stance (Z) taken toward this method (X) becoming dominant. With these three aspects distinguished, the traditional, or more common, interpretation of Veblen on these matters can be schematized as model A in table 1:

Table 1. Model A

	X (Method or science under discussion)	Y (Process whereby X is expected to become widely accepted)	Z (Evaluative orientation toward X)
Model A (Ayres and most institutionalists)	Evolutionary	Constructive research program with evolutionary science of economics the goal	Non-neutral

I suspect the assessment that model A captures the interpretation of many institutionalists is not especially controversial. As Ayres expressed matters:

> In his essay entitled "Why is Economics Not an Evolutionary Science?" . . . Veblen raised a question which has been thrown up to institutionalists as a challenge ever since: If the law of supply and demand, the theory of price equilibrium, marginal analysis, and all that, are to be cast aside, what has institutionalism to put in its place? (1963, 54)

Ayres portrayed Veblen as concerned with facilitating understanding. Veblen's starting point is a "rejection of the traditional conception of the economy" (1963, 55), and the "challenge" set by Veblen is to replace it with something that better enables the "economic life process . . . [to] be understood" (Ayres 1963, 55). Ayres, clearly, was of the view that Veblen was suggesting that economics should be other than Veblen found it, specifically, that it should become an evolutionary science, and was attempting to initiate a constructive program with this ambition in mind. Many others concur.[3]

Veblen's Evolutionary Epistemology

Although interpretative contributions conforming to model A usually contain significant insight, I think we must accept that they are also somewhat misleading. For Veblen was very clearly of the impression that economics would fall in line as an evolutionary science independently of any consideration of the latter's likely intrinsic worth as a cognitive device, i.e., irrespective of any potential an economic science may possess to contribute to human understanding. And with this being the case, Veblen regarded a constructive program (whether or not desired or desirable) as unnecessary; economics will become an evolutionary science anyway:

> The later [evolutionary] method of apprehending and assimilating facts and handling them for the purposes of knowledge may be better or worse, more or less worthy or adequate, than the earlier; it may be of greater or less ceremonial or aesthetic effect; we may be moved to regret the incursion of underbred habits of thought into the scholar's domain. But all that is beside the present point. Under the stress of modern technological exigencies, men's everyday habits of thought are falling into the lines that in the sciences constitute the evolutionary method; and knowledge which proceeds on a higher, more archaic plain is becoming alien and meaningless to them. The social and political sciences must follow the drift, for they are already caught in it. (Veblen 1990, 81)

Veblen as a Thoroughgoing Evolutionist

I think, then, that the model A interpretation of Veblen's understanding of how the evolutionary method (X) will come to be accepted has to be rejected. In fact, in viewing the rise of the evolutionary method (X) as inevitable, Veblen argued that the manner of its (expected) rise to dominance (Y) is an "evolutionary process" by which he meant a process of cumulative causation. Commentators who deny that Veblen advanced, or wished to advance, a constructive program have tended to recognize this. And in rejecting model A they have mostly (if implicitly) interpreted Veblen according to model B in Table 2.

Table 2. Models A and B

	X (Method or science under discussion)	Y (Process whereby X is expected to become widely accepted)	Z (Evaluative orientation toward X)
Model A (Ayres and most institutionalists)	Evolutionary	Constructive research program with evolutionary science of economics the goal	Non-neutral
Model B (Samuels and postmodernists)	Evolutionary	Evolutionary[4]	Evolutionist[5] or neutral

Warren Samuels (1990) provided an excellent contribution that in effect supports model B. On the basis of providing a wealth of textual evidence considered to support his view Samuels concluded:

> Indeed [Veblen] was a true (may I say it that way?) evolutionary economist: He applied his evolutionary thinking to his own thinking, even to evolutionary thinking itself. (1990, 707)

In this:

> Veblen adopts . . . the position . . . that interpretation is interpretation-system specific, that there are no meta-criteria on which to choose between alternative preconceptions et cetera, with any serious degree of conclusivity, except by selecting the premise on which rest the preconception thereby chosen, that there is no independent interpretative standpoint. (Samuels 1990, 703–4)

According to this interpretation, then, Veblen could not suppose that economics should be an evolutionary science (as opposed, say, to a taxonomic or teleological science), for his evolutionist perspective does not allow any such normative comparative inference.

For reasons already noted I think that model B does, in part at least, represent an advance over model A. However, there is a sense on which I think it goes too far. An interpretation that I think better represents Veblen's position, and for which I argue below, is systematized as model C in table 3:

Table 3. Models A, B, and C

	X (Method or science under discussion)	Y (Process whereby X is expected to become widely accepted)	Z (Evaluative orientation toward X)
Model A (Ayres and most institutionalists)	Evolutionary	Constructive research program with evolutionary science of economics the goal	Non-neutral
Model B (Samuels and postmodernists)	Evolutionary	Evolutionary	Evolutionist or neutral
Model C	Evolutionary	Evolutionary	Non- neutral

Why do I suppose model C is the better representation of Veblen? There are various reasons. First let me note that the passages drawn on by Samuels and others to support model B mostly count only against model A. Model B is assumed supported as the only viable alternative. However, once model C is brought into contention we can see that rather more argumentation is required if model B is to be maintained.

To see that model C fares at least as well as model B in the light of the passages referred to by Samuels and others, let me consider some of the latter. It is noted, for example, that Veblen wrote:

> The later [evolutionary] method of apprehending and assimilating facts and handling them for the purposes of knowledge may be better or worse, more or less worthy or adequate, than the earlier; . . . But all that is beside the present point. (Veblen 1990, 81)

We can see, though, that this passage (and I contend the same is true of others like it) establishes not that Veblen denied the possibility of intrinsic merit to any methods of science but only that he considered such matters irrelevant to the process of whether or not any specific method is taken up. The point, according to Veblen, is only that the evolutionary method will come to be dominant in economics irrespective of whether it carries any intrinsic merit. The question of worth is laid aside rather than dismissed as indeterminate; it is simply beside the point.

A further passage regarded by Samuels as fundamental to his argument runs as follows:

> In the modern culture, industry, industrial processes and industrial products . . . have become the *chief* force in *shaping* men's daily life, and therefore the *chief* factor in *shaping* men's habits of thought. Hence men have learned to think in the terms in which the technological processes act. This is *particularly* true of those men who by virtue of a *peculiarly strong susceptibility* in this direction become addicted to that habit of matter-of-fact inquiry that constitutes scientific research. (Veblen 1990, 17, emphasis added)

Now in arguing for model C, I do not wish to deny that industrial and community habits and so on have significant influence, even in science; all of us are in some ways shaped by our social conditions. But to accept this is not to deny all space for critical evaluative thought. My claim is only that the noted influences are not all determining and that Veblen recognized this. I have italicized words and phrases from the quoted passage just because, it seems to me, they allow a critical distance to remain. Habits of thought are shaped but not determined. And if it is "particularly true" of some that they have learned to think in such ways, the point of emphasizing this is presumably just because it is not particularly true of everyone else. Once more, though, the focus is really on the column 2 entry. And the evidence is that Veblen thought the process of change to be broadly evolutionary, but not completely; not everyone is totally susceptible.

A further passage central to Samuel's assessment runs as follows:

> A discussion of the scientific point of view which avowedly proceeds from this point of view itself has necessarily the appearance of an argument in a circle;

> and such in *great part* is the character of what here follows. It is in *large part* an attempt to explain the scientific point of view in terms of itself, *but not all together.* (Veblen 1990, 32; italicized emphasis added)

Samuels was correct that this reveals Veblen's recognition of the need to be self-referential, or to situate oneself within one's own analysis. But the italicized words serve to qualify the extent to which Veblen saw himself arguing "in a circle." His next sentence, explaining his qualifications, reads:

> This inquiry does not presume to deal with the origin or the *legitimation* of the postulates of science, but only with the growth of the habitual use of these postulates, and the manner of using them. (Veblen, 1990, 32; emphasis added)

Here I take Veblen to be saying that he was dealing only with the evolutionary process whereby the evolutionary postulates come quite widely to be habitually used; the question of their legitimization, though, is not considered. Veblen was not going as far as to suggest that the question of the legitimacy of these postulates is beyond being addressed; he is indicating only that this is not his objective.

In Support of Model C

So far, then, I have suggested that even textual evidence of the sort noted by Samuels does not clinch the model B interpretation of Veblen. It does count against aspects of model A, but a defense of model B requires more. For proponents of model B to establish the evolutionist justificatory stance it is essential they also demonstrate that, for Veblen, these evolutionary forces are all determining, while the evolutionary method itself is beyond evaluation. Samuels did seem happy to attribute to Veblen a "cultural determinism of habits of mind and habits of behaviour" (1990, 711), but I do not think the passages he took from Veblen can sustain such an attribution. Indeed, the various (noted) qualifications in the passages reproduced by Samuels do not really make sense on model B and already suggest the superior adequacy of model C. But it is possible to produce further textual and other evidence that can be sustained only by model C.

We can note, first of all, that throughout Veblen's methodological writings the "evolutionary method" is more or less defined as that appropriate for dealing with a "quantitative sequence" or "causal relation" or, more expansively, a "mechanical sequence in events" or "a genetic process of cumulative causation"; in all such formulations any teleological tendency is regarded as absent. In short, evolutionary method is defined more or less in ontological terms; it is a method appropriate for dealing with a reality of a certain type or nature. This, then, is my claim concerning Veblen's ontological orientation noted at the outset. In the famous "evolutionary essay" under discussion, specifically, Veblen wrote that the main "difference between the evolutionary and the pre-evolutionary sciences" is "a difference of spiritual attitude" turning on "the basis of valuation of the facts for the scientific purpose, or in the interest from which the facts are appreciated." In brief:

> The modern scientist is unwilling to depart from the test of causal relation or quantitative sequence. When he asks the question, Why? he insists on an answer in terms of cause and effect.......This is his last recourse. And this last recourse has in our time been made available for the handling of schemes of development and theories of a comprehensive process by the notion of a cumulative causation. (Veblen, 1990, 60–1)

In this passage the idea that evolutionary scientists are "unwilling to depart from the test of causal relation" is significant (a matter to which I return below).

Second, Veblen wrote not of the "mechanical sequence in events" being absent in earlier times (prior to the evolutionary method being taken up in some sciences) but of its being less visible. For example, he wrote:

> There is little of impersonal or mechanical sequence visible to primitive men in their everyday life; and what there is of this kind in the processes of brute nature about them is in large part inexplicable and passes for inscrutable. (Veblen 1990, 62)

This observation is often repeated. And as, with time, the "impersonal sequence" becomes more clearly visible, Veblen referred explicitly to a corresponding movement of "habits of thought in the realistic direction" (1900, 63). This realistic direction is just that associated with the evolutionary habit of mind:

> But in the hands of the later classical writers the [economic] science . . . was . . . out of touch with that realistic or evolutionary habit of mind which got under way about the middle of the century in the natural sciences. (Veblen 1990, 69)

On this interpretation, then, Veblen was defending the evolutionary method as in effect more realistic than alternatives. And these passages and others like them, I suggest, can only be made sense of from the perspective of model C. Veblen was being evaluative.

Knowledge and Reality

But actually the relative support for model C over model B revealed in passages such as these is stronger still. Far from arguing the cultural determinist case that we are trapped within our interpretive frameworks or that, as Peukert 2001 puts it, "There is no truth beyond alternative and often opposing frames (scientific or otherwise)," Veblen's account is of the inexorable impacting of reality on our habits of thought and behavior. Veblen's is not a contribution concerned merely to reveal scientists' ontological or metaphysical preconceptions. It is an account of how the nature of material reality, most recently of the industrial process, effectively coerces habits of thought so that they very often fall in line:

> As time goes on . . . the circumstances which condition men's systematisation of facts change in such a way as to throw the impersonal character of the sequence of events more and more into the foreground. The penalties for failure to apprehend facts in dispassionate terms fall surer and swifter. The sweep of events is forced home more consistently on men's minds. The guiding hand of a spiritual agency or a propensity in events becomes less readily traceable as men's knowledge of things grows ampler and more searching. In modern times, and particularly in the industrial countries, this coercive guidance of men's habits of thought in the realistic direction has been especially pronounced; and the effect shows itself in a somewhat reluctant but cumulative departure from the archaic point of view. The departure is most visible and has gone farthest in those homely branches of knowledge that have to do immediately with modern mechanical processes, such as engineering designs and technological contrivances generally. (Veblen 1990, 63)

This is not an account of individuals being locked into their cultural or epistemological frameworks (Samuels 1990); it is not a deconstruction of the idea of objectivity in knowledge (Peukert 2001). It is closer to an account of external reality making its nature felt eventually, whatever the current epistemological or methodological frameworks may be. Matter-of-factness is viewed not as "derivative" (Samuels 1990, 707) but as somewhat imposing.[6]

Critical Ontology

Did Veblen actually attempt to defend one set of ontological or "metaphysical" preconceptions over another? I think he did. I am effectively suggesting that Veblen's is primarily an ontological project, concerned centrally with processes of cumulative causation. This is counterpoised to the deductivist[7] mainstream project of his time, of which the output of early mathematical economists provides an example.

Notice, first, that Veblen recognized that many "modern scientists" (especially those concerned to apply mathematical formalism) reject the metaphysical idea of cumulative causation, with its cognate categories of continuity, efficiency, and activity (Veblen 1990, 33). Veblen observed that this group of modern scientists attempts to avoid the metaphysical notion of causation by focussing only on the observable "concomitance of variation." But, according to Veblen, these very scientists cannot help but "impute" causal sequence to the facts, even as they profess not to do so:

> The claim [not to impute causality], indeed, carries its own refutation. In making such a claim, both in rejecting the imputation of metaphysical postulates and in defending their position against their critics, the arguments put forward by the scientists run in causal terms. For the polemical purposes, where their antagonists are to be scientifically confuted, the defenders of the non-commit-

> tal postulate of concomitance find that postulate inadequate. They are not content, in this precarious conjuncture, simply to attest a relation of idle quantitative concomitance (mathematical function) between the allegations of their critics, on the one hand, and their own controversial exposition of these matters on the other hand. They argue that they do not "make use of" such a postulate as "efficiency," whereas they claim to "make use of" the concept of function. But "make use of" is not a notion of functional variation but of causal efficiency in a somewhat gross and highly anthropomorphic form. The relation between their own thinking and the "principles 'which they' apply" or the experiments and calculations which they "institute" in their "search" for facts, is not held to be of this noncommittal kind. (1990, 34)

It should be clear that the matter of primary relevance, here, is not whether Veblen's ontological argument is correct or even compelling but that he endeavored to make it at all. Rather than accept any ontological commitments uncritically, Veblen was very clearly seeking to demonstrate that one set, that which facilitates the evolutionary method, is explanatorily superior to another.[8]

Interpretation

My contention, then, is that critics of model A have tended to suppose that its rejection leads necessarily to model B, whereas model C is the better supported.

In my advancing model C, the significant challenge that remains is to explain Veblen's coyness about revealing his support for the evolutionary approach. For, although his acceptance of the superior cognitive worth of the evolutionary method is apparent, I must admit it is not overly emphasized. Even the just-noted ontological critiques (or determinate negations—see Lawson 1997), demonstrating relative support for the preconceptions of Veblen's preferred method, are wholly relegated to a footnote. Moreover, if Veblen did display support for the evolutionary method over rivals in the main body of text, I accept it is stated mostly in terms of the merely pragmatic criteria of being "more up-to-date," or some such thing (1990, 57). Why, then, this apparent reluctance on Veblen's part to emphasize his (on close examination clear) relative support for the evolutionary method, and for economics becoming an evolutionary science?

I believe this coyness is explained precisely by Veblen's wariness of being (mis)interpreted as saying that the evolutionary method will catch on just because it is in some sense the most realistic. This is not his message. Veblen himself was presenting an evolutionary story; he is providing an evolutionary account of the rise to dominance of a specific (as it happens: the evolutionary) method. He was effectively attempting to be the first (or an early) evolutionary epistemologist. I have already noted that Veblen tied evolutionary method to an ontology of cumulative causation. Veblen further held that it is a mark of the modern evolutionary scientist that he or she "is unwilling to depart from the test of causal relation or quantitative sequence" (1990, 60), that he or she maintains a "refusal to go back of the colourless sequence of phenomena and seek higher ground for their syntheses" (1990, 61). Hence it is essential that Veblen's account, too, if it is to qualify as evolutionary according to Veblen's own understanding of this term, does not depart from "the test of causal relation or quantitative sequence." In consequence, any belief that he also has in the greater adequacy of the method, or in its more realistic ontology, must clearly not figure (and be recognized as not figuring) in his evolutionary story.

In short, it was precisely in order to emphasize his view that the evolutionary model is (he believed) rising to dominance on evolutionary grounds that Veblen was coy about giving his own evaluation of the evolutionary method too often.

But to interpret a method as (becoming) dominant is not of necessity to view everyone as submitting to it (as is illustrated by the institutionalist resistance to the dominant deductive approach of modern times). And, to the point, there is nothing per se in the natural selection evolutionary model that prevents natural selection forces working to select a model that can be judged best or worthy according to criteria that bear not at all on the mechanism by which it is selected. Certainly, Veblen did not deny the possibility of the evolutionary method being evaluated or of its having intrinsic worth in given contexts. And we have seen, indeed, there are various arguments and statements made by Veblen that reveal he does consider the evolutionary method as realistic and its ontological presuppositions the more sustainable.

Back to the Constructive Program of the Proponents of Model A

So where does all this take us? My argument is that Veblen, in effect, had two concerns at least in formulating his famous question. The first was his desire that economics become more "realistic," that economics ought to be an evolutionary science. The second was his interest in explaining why it has not happened yet, and in announcing that, whatever the obstacle, it will inevitably do so soon.

Now it is highly significant that, in terms of implications for practice, there is a clear sense in which the presuppositions behind his second concern dominate those implicated in the first. After all, if something is thought bound to happen the question of "what should be done" to make it happen becomes effectively a non-issue. This is why a constructive program is never explicitly formulated or actively developed by Veblen.

However, Veblen has proven to be wrong (so far) in his assessment of the inevitability of economics becoming an evolutionary science. He advanced an evolutionary epistemology, and in the precise predictive form it is specified at least, it has proven to be erroneous. There is no inevitability to what happens in the academy. As I say, the very fact that so many (old) institutionalists currently survive amid the modern dominant group, the latter with their deductivist habits of thought, is testament to this. In consequence, the former of Veblen's concerns, and the practical implications of accepting it, remains no longer superfluous to the situation.

In other words, in the circumstances an impetus to develop an evolutionary science can, after all, be said to be a component of Veblen's legacy. Even if model A is not a correct interpretation of his contribution, the activist program it highlights is nevertheless something to which we might reasonably have expected Veblen to turn in the circumstances. Thus Ayres and others can, after all, be said to be developing a Veblenian program.

In fact, we can, with some legitimacy, infer more than this. Veblen at one point indicated his view that if economics is to succeed in becoming an evolutionary science "the way is plain so far as regards the general direction in which the move will be made" (1990, 70–72). Of course, he thought that the outcome was inevitable. But with the failure of his predictions to be realized we might reasonably treat his projection of how an evolutionary economics will likely turn out as indicative of his assessment of how it should turn out. Thus we might, after all, interpret Veblen's contributions on such matters as providing not only support but also a suggestive basis for a Veblenian constructivist program. And with Veblen's tying of the evolutionary method to the analysis of unfolding genetic processes of cumulative causation, my claim (developed further elsewhere—Lawson, forthcoming) is that such a constructivist program will be explicitly ontological in orientation.

Conclusion

Veblen had two concerns (at least) in posing his famous question. He both (i) believed that economics should be an evolutionary science, that an evolutionary economics would mark an improvement over the existing state of affairs, and (ii) was interested in announcing the inevitability of economics becoming an evolutionary science and in explaining its non-occurrence so far (and indeed its likely form). Because Veblen believed it inevitable that economics would become an evolutionary science, and because he was convinced it would do so via an evolutionary process, i.e., independently of normative concerns, he was keen to play down the fact that he also supported the idea of economics as an evolutionary science. That is, he was keen consistently to provide an evolutionary argument, perhaps to be the first evolutionary social theorist within economics.

However, because Veblen's specific version of evolutionary epistemology turned out to be wrong, because his prediction that economics would soon become an evolutionary science has been confounded by events, his normative stance and their practical implications now come into play.

Veblen's normative stance, I have briefly alluded, is one that closely reflects his holding to a different ontology to that presupposed by the current mainstream. It is an ontology of (a genetic process of) cumulative causation. And the way to make the most of Veblen's insights is to recognize this, to render the ontological insights of the tradition more explicit, sustained, and systematic.

Veblen assessed that economics not only was, but also ought to be, falling in line as an evolutionary science. I am in effect suggesting that an appropriate modern day restatement of this assessment is that the tradition of modern (old) institutionalism may be, and seemingly also ought to be, falling in line not just as a constructive project but as a specific (evolutionary) project within ontology, i.e., within realist social theorizing.

Notes

1. Helge Peukert (2001), for example, suggested that:

   > Veblen deviates fundamentally from the common assumption that he endeavored to develop a constructive research program in the Lakatosian sense. . . . He did not, and did not want to, unfold a positive, new, and evolutionary approach which could practically be applied to the analysis of economic processes. He did not pretend to uncover any developmental logic of economic history or institutions. (544)

   Rather, according to Peukert, Veblen "had only one scientific aim," and this was "a radical and deconstructive critique of what he called prevailing habits of thought" (544). Alternatively put, "Veblen's basic intention [was] to disclose scientific preconceptions," a goal which reflects the influence upon him of his early reading of Kant. The upshot is that Veblen is interpreted as "having formulated a postmodern epistemology at the turn of the twentieth century" (551) or as coming "close to so-called postmodern thinkers like Richard Rorty" (550).

   Warren Samuels, too, might be interpreted as suggesting a postmodernist conception of Veblen (Hoksbergen 1994, 694). Nor is this an ascription with which Samuels seems unhappy to accept for himself (Samuels 1998). Further, Roland Hoksbergen (1994) assessed that this postmodern element is a growing force within institutionalism.
2. A more developed defence of (ii), the thesis that Veblen's orientation is effectively of an ontological sort, is provided in Lawson (forthcoming).
3. Numerous examples were referenced by Peukert (2001), who concluded:

   > Positive or negative interpretations, with either a neoclassical, a new, or an old institutionalist bias, *all* have one point in common: they suppose that Veblen developed, or tried to develop, a positive heuristic and a constructive alternative research program. (Peukert 2001, 543)
4. Notice that Veblen (mostly) reserved the term *evolutionary* for a form of method or science. But this was not always so. He occasionally included passages such as "the evolutionary process of cumulative causation" (1990, 55). It is this usage that is intended in column 2 of table 1. Here the term *evolutionary* indicates the sort of (evolutionary) causal process by way of which the method X will (or is expected to) come to predominate in economics; in short it indicates an evolutionary epistemology.
5. By evolutionist evaluative orientation, I mean one in which any outcome of a causal process is regarded just as that, i.e., as a fortuitous or "impersonal" outcome; its being caused or "selected" does not make it thereby normal, good, or laudatory, etc.
6. If there is a danger with Veblen's emphasis at this point, it is not that ontology is neglected but that it is regarded as playing too dominant a role in almost determining our habits of thought and knowledge; although scientific development is mediated or provoked by the prior change in the habits of the wider community, the latter are themselves induced by changes in the nature of technological exigencies. It might be suggested, indeed, that Veblen came close to committing the ontic fallacy, to implying that questions of knowledge or modes of thought

can always be rephrased as (or reduced to) questions about being. But Veblen avoided the fallacy by making his account of how the methods or habits of science evolve one that renders cultural and other forces powerful, but never all determining.

Let me emphasize at this point that to accept the assessment I am advancing does not, of course, necessitate agreement with Veblen on all matters. Veblen was suggesting that the spread of the machine process necessarily encourages a widespread acceptance of "matter-of-fact" habits of thought presupposing a non-teleological causalist ontology. It was this movement that was predicted eventually to lead to the widespread acceptance of the evolutionary method even in the economics academy. We know this assessment to be in large part wrong. My purpose is only to identify aspects of Veblen's argument.

7. Veblen did not use the label *deductivist* to refer to the mainstream project of his day; more usually he described it as *taxonomic.* But as an explanatory endeavor it is clearly the same sort of enterprise as the modern deductivist program. As Veblen observed, if a segment of an industrial field is to be investigated, a predictive scheme (referred to as normalized or teleological because predetermined) is adopted. And with

> this normalised scheme as a guide, the permutations of a given segment of the apparatus are worked out according to the values assigned the several items and features comprised in the calculation; and a ceremonially consistent formula is constructed to cover that much of the industrial field. This is the deductive method. The formula is then tested by comparison with observed permutations, by the polariscopic use of the "normal case"; and the results arrived at are thus authenticated by induction. Features of the process that do not lend themselves to interpretation in the terms of the formula are abnormal cases and are due to disturbing causes. In all this the agencies or forces causally at work in the economic life process are neatly avoided. The outcome of the method, at its best, is a body of logically consistent propositions concerning the normal relations of things—a system of economic taxonomy. (1990, 67)

For an illuminating wider discussion of this issue and indeed of many that I touch on in this short paper see the excellent contribution of Anne Mayhew (1998).

8. Notice, too, that at one point Veblen attempted a version of an argument familiar in critical realism. Elsewhere I have argued that when empirical realists (those who reduce reality to events and their correlations) hail the controlled experiment as the exemplar of science on the grounds that it is where event regularities are produced, they lack the means of explaining why event regularities are mostly restricted to these experimental conditions. Or rather they can explain the latter phenomenon only by admitting that which they wish to deny: in order to make sense of the situation it must be recognized that event regularities are so restricted because it is only in controlled experiments that underlying causal mechanisms can be insulated (from countervailing mechanisms) and empirically identified (for a detailed discussion, see Lawson 1997). Veblen was surely getting at the same insight when (in the footnote under examination) he made the following observation:

> Least of all is the masterly experimentalist himself in a position to deny that his intelligence counts for something more efficient than idle concomitance in such a case. The connection between his premises, hypotheses, and experiments, on the one hand, and his theoretical results, on the other hand, is not felt to be of the nature of mathematical function. Consistently adhered to, the principle of "function" or concomitant variation precludes recourse to experiment, hypotheses or inquiry—indeed it precludes "recourse" to anything whatever. Its notation does not comprise anything so anthropomorphic." (1990, 35)

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