

Second Thoughts On *The Philosophical Origins of Austrian Economics.*

David Gordon

Professor Barry Smith's characteristically erudite remarks about my pamphlet provide me with a welcome opportunity to offer some additions and corrections. I have no major disagreement with Smith's comments, but he has at one place ascribed to me a much more ambitious thesis than I intended.

He thinks I wish to divide "nineteenth-century philosophical thinking in the German-speaking world" (p. 125)¹ into two camps: German, which I see as "Hegelian, anti-science, and organicist" and Austrian, which in contrast is "Aristotelian, pro-science and individualist" (p. 125-26). Against this view, Smith maintains that Hegel, Marx, and the German Historical School display marked affinities with the Austrians: both groups, in particular, count as Aristotelian.

I meant to advance a much more limited conjecture than this: Hegel's stress upon organic unity may have influenced the aversion toward a universal science of economics found among Schmoller, Sombart and other members of the German Historical School. I also had a little to say about Hegel's politics, but I did not intend a full characterization of Hegel's philosophy, much less nineteenth-century German and Austrian philosophy as a whole.

Smith's emphasis on the Aristotelian elements in Hegel seems to me entirely well taken and supported by longstanding scholarly opinion. As an example, one outstanding British authority on Hegel, G. R. G. Mure, in his *Introduction to Hegel* (Clarendon Press: Oxford, 1940) devotes his first few chapters entirely to Aristotle before so much as mentioning Hegel. But I venture to suggest that

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¹All references to *The Philosophical Origins of Austrian Economics* (Auburn, Ala.: Ludwig von Mises Institute, 1993) and to Barry Smith's review, which appears in this volume, are by page number in parenthesis in the text.

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the similarities between Aristotle and Hegel leave my suggestion untouched. For Hegel, "the Truth is the Whole" in a way that inhibits the elaboration of separate sciences. Like Aristotle, Hegel favored teleological explanation; but if, as Hegel thought, everything is organically related to everything else, how can one develop a distinct discipline of economics with universal laws?

Or so at least it seemed to me in 1988, when I gave the lecture on which the pamphlet is based. I did not then know that an important study had challenged the view of Hegel's doctrine of internal relations which I presented. R. P. Horstmann, in *Ontologie und Relationen* (Koenigstein: Atheneum, 1984) argues strongly that Hegel did not support a doctrine of internal relations in the style of the British Idealists. Further, Robert B. Pippin, in *Hegel's Idealism* (Cambridge: Cambridge University Press, 1989), sees Hegel as a "conceptual holist" rather than the advocate of a metaphysical thesis.

But it is exactly here that Hegel's philosophy poses a problem for a science of economics. If one believes that our categories generate contradictions that can only be resolved by resort to a "higher" standpoint, and that this overcoming or "sublation" is continually repeated, will it not be difficult to construct independent scientific disciplines? Even, then, if my statements about internal relation in Hegel need to be changed, my suggestion is still in the running.

To turn to a few details, Smith with complete justice notes that my picture of the German Historical School ignores the views of the earlier Historical School (his term, "simplifications" is much too kind). My remarks on the group should be taken as limited to the later Historical School, as I note at page 43 of the pamphlet. When I gave the lecture, I did not know the material on the earlier group to which Smith refers.

Smith notes that Brentano was "decisively influenced by the thinking of the German metaphysician, F. A. Trendelenburg" (p. 126). Certainly, this makes it difficult to assert a complete polarity between German and Austrian philosophy; but, once more, this is not my thesis. I do not think that Trendelenburg's influence can be used to show a similarity between Hegel and Brentano, since Trendelenburg, far from being a Hegelian, sharply criticized Hegel's *Logic*. But Smith does not use Trendelenburg for this purpose.

I think it doubtful that the "presence of intelligible change implies . . . that there is no problem of induction for either group of Aristotelians" (p. 127). It is of course right that if one grasps a law-governed change, one is not restricted to induction by simple enumeration. But does this solve the problem of induction? Does it logically

follow from the existence of an intelligible change at a particular time that the law will continue to hold in the future? Or are these doubts merely an undue Humean skepticism? (I am not sure whether Smith intends only to give the view of the Aristotelians or also to endorse it.)

Smith's review has a fundamental failing I have so far ignored: he is entirely too easy on me. Before I turn from Smith to my own corrections, however, may I say that I hope the rumor is true that Smith has forthcoming a book on the philosophy of the Austrian School. He is one of the world's foremost authorities on nineteenth and twentieth century Austrian philosophy.

And now for my "second thoughts." At page 7, it would be better to say that Sombart knew Mises rather than that the two economists were friends.² At pages 10–11 I describe the doctrine of internal relations in a grossly mistaken way. A supporter of internal relations thinks that any property of an entity is essential to it. But it does not follow from this that any change in a property will affect every other *property* of an entity. Someone might hold that internal relations connect only properties and substances, not properties by themselves. (A more exigent version of the doctrine *would* hold that every property is internally related to every other property of the substance it modifies. A still more demanding version would hold every property is internally related to every other property of *any* substance). And the first sentence on p. 10 should read: "the person who has met the President is an *essentially* different person from the one who has not."

At p. 27, when I claim that for Aristotle "[e]mpirical science exists as a placeholder for true science, which must work through deduction," this wrongly suggests that a deductive science for Aristotle is non-empirical. "Empirical" must be understood in the sense of "mere empirical hypotheses" of the preceding paragraph. For Aristotle, the evident principles of a deductive science come from observation of the world.

Much more serious is the confused discussion of self-evident axioms on pp. 27–28. The regress argument of the *Nicomachean Ethics* is used to establish the existence of a highest end. I should have explicitly stated that the regress argument that I discuss is a generalization of the argument of the *Ethics*, not given there in the form in which I present it. An objection to my discussion which I overlooked is this: I claim that a science can have several basic axioms: justification need not proceed from a *single* self-evident

³I am grateful to Ralph Raico for this point.

axiom. But if there are several axioms, can't they be combined into a single axiom through conjunction? I ought to have specified that the argument is restricted to axioms that are not logical parts of other axioms. Further, it is not clear that the discussion is needed: has anyone claimed that a science is derived from a single axiom? Perhaps Mises hints at it; but even he allows subsidiary postulates.³

The discussion of the verification principle at p. 36 is seriously mistaken, and I am greatly indebted to Matthew Hoffman for pointing this out to me. First, I ought to have made clearer that I make two assumptions *not* part of the verification principle, on which my argument depends: if a statement is verifiable, its negation is verifiable; and any logical consequence of a verifiable proposition is verifiable. The argument then proceeds as follows: "From p , we derive (p or q). But suppose that p is false—then we have:

$$p \text{ or } q$$

$$\underline{\text{not-}p}$$

$$\therefore q$$

By hypothesis, p is verifiable; then (p or q) and ($\text{not-}p$) are verifiable, by our assumptions. Then q is verifiable, since it is a logical consequence of verifiable propositions." This should be substituted for the erroneous argument at p. 36.

³I am grateful to Murray Rothbard for this objection.