

# THE IMPACT OF SMITH'S PHILOSOPHY OF SCIENCE ON HIS ECONOMICS

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An economic "methodology" is a view of the aims of the discipline and of the criteria by which work within the discipline should be judged. If one accepts the widely held view that the aim of economics is to explain human behavior, then "methodology" is, more specifically, a view of what constitutes a good explanation. Such views, when explicitly defended, fall under the domain of philosophy of science. However, even the work of an economist who has no explicit methodology, still reflects some implicit methodology, i.e., aims at meeting some general standards of what constitutes a good explanation. In the pages that follow we seek to learn whether economists who write on methodology are wasting their time. The main modus operandi will be to examine carefully a past episode in economics in which we might expect methodology to have played an important role in advancing the science. The primary point of controversy is not whether methodological soundness is important, since most would agree that it is, but rather whether systematic attention to methodology can isolate the methodologically sound approaches in a way that is helpful to the actual practitioners of the science.

Philosopher of science Stephen Toulmin has suggested in conversation that at some point in the growth of a science the criteria by which theories are evaluated come to be determined autonomously within a discipline. If this is true we would expect that any impact of 'philosophical' reasoning on the criteria of theory evaluation would be greatest at a science's inception. If methodology has ever had any importance in the development of economics, then, we would expect to observe it at the inception of the discipline in the works of Adam Smith.

One of the great difficulties in analysing Smith's self-conscious views on methodology of economics is that no manuscript survives explicitly stating them. In particular, where

such comments would be most appropriate, in the beginning of the *Wealth*, he instead jumps right in with a statement of the substantive question that he hopes to answer. So it is necessary to infer his self-conscious views from what he says elsewhere in other contexts. Apart from short asides there are really only three sources for Smith's views on methodology and philosophy of science: (1) the *Essays on the History of Astronomy and Physics*, (2) a couple of pages on the didactic mode of presentation in *Lecture on Rhetoric and Belles Lettres* and (3) a couple of pages in the education section of the *Wealth* on the subdivisions of philosophy. One problem in determining the influence of a thinker's methodological statements on his substantive work is the difficulty of determining the direction of causation when they were written at the same time or the methodology was written after the substantive work. In such cases the charge may be made that the methodology, even when consistent with the substantive work, is not useful in explaining it since it may well be serving as ex post rationalization. The three works mentioned above will be discussed in order of decreasing immunity from this charge, i.e., the earliest first.

The exact date of the writing of the *History of Astronomy* is not known but 1750 is a plausible conjecture.<sup>1</sup> It is remarkably modern sounding in its view of theories, not as realistic representations of the way things 'really' are but rather as intellectual creations that account more or less adequately for the phenomena considered important at a particular time. In this vein, Skinner (9, p. 312) claims parallels between Smith's essay and the work of Thomas Kuhn.<sup>2</sup> Concerning the intellectual insight of the *Essay* Schumpeter (6, p. 182) has said: "Nobody, I venture to say, can have an adequate idea of Smith's intellectual stature who does not know these essays. I also venture to say that, were it not for the undeniable fact, nobody would credit

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the author of the *Wealth of Nations* with the power to write them.” For my purposes, there are four passages that elucidate most clearly Smith’s views on the aims and method of science. The first is one locus of Smith’s claim that the end of science is to ease the discomfort of the imagination (3, p. 50). In the second passage (12, p. 66) Smith displays an awareness of the importance of systems in science and makes an apt comparison of systems to machines. The third passage (12, p. 77) contains Smith’s, perhaps paradoxical, claim that successful theories may conflict with the facts. The possible paradox is that it is hard to see how any system that was significantly discrepant with the facts could ease the imagination.

Finally, Smith offers (12 p. 108) enthusiastic praise for the Newtonian system while still indicating that even it can not, consistent with his own views, be taken as a final account of the way things ‘really’ are. To say that Newton was not the final answer is an obvious commonplace now; to say it in 1750 on the basis of a plausible view of theories, was extraordinarily acute. Smith’s *Lectures on Rhetoric and Belles Lettres* are the report of a student attending Smith’s lectures in 1762–1763. We also have the report of a student who attended Smith’s 1763 lectures on jurisprudence which contained a good deal of what would later become the *Wealth*. So we know that at the time of Smith’s brief comments on science in the *Lectures on Rhetoric* he had already done considerable substantive work in economics. In the section of his lectures dealing with composition Smith discusses didactic writing, which is the form appropriate to science. He identifies two forms of didactical writing. One, as illustrated by Newton’s physics, uses a very few principles to explain a broad range of phenomena. The other, as illustrated by Virgil’s work on agriculture, advances a separate principle to explain each separate phenomenon. In discussing the value of the Newtonian method Smith (10, pp. 139–140) writes that:

It gives us a pleasure to see the phenomena which we reckoned the most unaccountable, all deduced from some principle (commonly, a well-known one) and all united in one chain, far superior to what we feel

from the unconnected method, where everything is accounted for by itself, without any reference to the others. We (need) not be surprised, then, that the Cartesian philosophy (for Descartes was in reality the first who attempted this method), though it does not perhaps contain a word of truth,—and to us who live in a more enlightened age and have more inquired into these matters, it appears very dubious,—should nevertheless have been so universally received by all the learned in Europe at that time. The great superiority of the method over that of Aristotle, the only one then known, and the little inquiry which was then made into those matters, made them greedily receive a work which we justly esteem one of the most entertaining romances that have ever been wrote.

From the above passage Myers (4, pp. 281–296) has concluded that “Smith seems to be far more impressed with Newton’s results than with his method.” He takes Smith as being sarcastic when speaking of the learned ‘greedily receiving’ Descartes’ physics. But far from ridiculing the learned, I interpret the thrust of the above passage to indicate approval of them for accepting what was at the time the best science around. Smith’s point here, especially when read in the light of the *History of Astronomy* is that theories are rightly evaluated only within the context of the stage of scientific development in which they are proposed.

However when read in the light of the following passage from *The Theory of Moral Sentiments*, (11, pp. 313–314) Myers’ position becomes more plausible: “The vortices of DesCartes were regarded by a very ingenious nation, for near a century together, as a most satisfactory account of the revolutions of the heavenly bodies. Yet it has been demonstrated, to the conviction of all mankind, that these pretended causes of those wonderful effects, not only do not actually exist, but are utterly impossible, and, if they did exist, could produce no such effects as are ascribed to them. But it is otherwise with systems of moral philosophy; and an author who pretends to account for the origin of our moral sentiments, cannot deceive us so grossly, nor depart so very far from all resemblance to the

truth.” Here Smith appears more the naive realist of older times than the sophisticated Kuhnian relativist of modern ones. I would argue that phrases like ‘grossly deceive’ and ‘resemblance to the truth’ are not consistent with the tone and substance of the *History of Astronomy*.

The final comments of Smith relevant to methodology of science are to be found in the section “Of the Expence of the Institutions for the Education of Youth,” in the *Wealth of Nations*. Four aspects of these comments should be noted. First Smith dismisses metaphysics as a fruitless and sophistical enterprise.<sup>3</sup> This would be important for us if it could be shown that he included, as some have done, epistemology in metaphysics. For epistemology can have significant overlap with issues of methodology and philosophy of science. But what Smith (12, Vol. 2, p. 770) had in mind by ‘metaphysics’ was apparently “the doctrine of spirits,” synonymous with ‘pneumatics.’ Smith is not very specific about what he would include under this head but I doubt that much of what we are concerned with would fit there. Later on Smith defines ‘pneumatology’ as “comprehending the doctrine concerning the nature of the human soul and of the deity.” (12, Vol. 2, p. 772). If pneumatology = pneumatics = metaphysics, then my conclusion is surely sound.

The second aspect of Smith’s *Wealth of Nations* comments worth noting is his view of logic. ‘Logic’ is defined by Smith more broadly than is common among modern philosophers. For him (12, Vol. 2, p. 770) it is “the science of the general principles of good and bad reasoning.” This would include, I believe, many of the sorts of questions that we currently class under methodology and philosophy of science. Though taught first in the schools, logic according to Smith arises only after science has produced competing theories. Outside of ‘philosophy’, which in Smith’s time was synonymous with ‘science’, common sense suffices in distinguishing good reasoning from bad. A final aspect of Smith’s views worth mentioning is the claim (12, Vol. 2, pp. 780–781) that without public institutions for education, “a private teacher could never find his account in teaching either an

exploded and antiquated system of a science acknowledged to be useful, or a science universally believed to be a mere useless and pedantic heap of sophistry and nonsense.” If the emphasis in this statement is to be on ‘useless’ then it does not seem consistent with his approval elsewhere of astronomy and other such sciences. Of course, ‘useful’ might be taken in a broad sense so that astronomy is seen as useful for easing the imagination. But then there would be no science, however pedantic, sophistical and nonsensical that would not be useful to someone for some purposes (if nothing else, then they would at least be useful for maintaining the standard of living of their espousers).

This is about all we have of Smith’s views on science. Before examining his substantive work to see what impact these views had upon it, we should consider how applicable Smith would have thought his philosophy of science to be to his economics. There are two reasons for thinking that he might have thought the applicability to be small. The first is that in 1773 he wrote to Hume of the *History of Astronomy* that he was beginning “. . . to suspect myself that there is more refinement than solidity in some parts of it.”<sup>4</sup> There are two reasons why we should not put too much weight behind the mild repudiation of his views implicit in this line. The first is that we do not know which parts of his work he was doubtful of. When speaking of over-refinement he may well have been speaking of the detailed accounts of some of the systems of astronomy rather than the more general observations that are mainly of interest to us. Secondly, Smith did after all decide to preserve this essay while burning some sixteen volumes of other materials (5, p. 434). For a man so singlemindedly determined that these other papers be burned to have saved the *History of Astronomy* essay indicates that on balance he thought highly of it. So much for the first ground for attributing to Smith the belief that there was little applicability of his views on science to his economics. The other ground for attributing to Smith this belief is that he might have thought economics differed from the physical and astronomical sciences in some crucial respect. Most clearly he *did* think that they differed in the motive for

their pursuit: economics being pursued for its narrowly conceived usefulness in telling us how to act, the physical sciences being pursued by men of leisure out of a sense of wonder. But just as clearly, though Smith thought the motives for pursuing the sciences differed, he thought the method of that pursuit to be the same. The best evidence of this is part of a passage that I have already quoted from the *Lectures on Rhetoric* (10, p. 140): “. . . the Newtonian method is undoubtedly the most philosophical, and in every science, whether of morals or Natural Philosophy, etc., is vastly more ingenious, and for that reason more engaging, than the other.” I will try now to test the hypothesis that Smith’s methodology had an impact on his economics. The prime difficulty in making such a test is that lack of correspondence between the methodology and the science may indicate either a lack of impact or an unsuccessful striving to follow the methodology. Only in the former case and not the latter can lack of correspondence be interpreted as refuting my hypothesis. Thus until some way is designed to distinguish the case of failure to do what was intended from the case of success in doing something other than what was intended, any test of my hypothesis is apt to be inconclusive.

Still, ignoring this limitation, the best test would be of the sort that I applied have in the context of the marginal utility revolution. The rationale of this test is that if you locate methodological disputes and find that there were certain common features in the methodologies of those who produced the better economics that were not shared by those who produced the worse economics, then you would have strong *prima facie* evidence for the importance of explicit methodology in economics. To apply this method to Smith’s period, we must find important rivals of Smith who differed from him in method. I will consider two possible prospects: the school of Political Arithmetic and Sir James Steuart. Jacob Hollander in his essay on “the Dawn of Science,” argued that political arithmetic constituted one of two main subdivisions of economic writings, the other being political economy (1, p. 3). Hollander went on to claim that much of political arithmetic consisted of “a Baconian endeavor to obtain

a body of economic principles by systematic induction from the facts of record, . . .” To further support his claim of methodological disagreement he cites Smith’s statement from the *Wealth*, (12, pp. 534–535) that “I have no great faith in political arithmetick (sic), and I mean not to warrant the exactness of either of these computations.” But I think Hollander may be reading too much into this passage. What Smith objects to is not the usefulness of collecting statistics *per se*, but rather the reliability of the methods used to collect them in his time. In fact when making a similar comment in a letter to George Chalmers, Smith “cited as an example the difficulties which had encumbered Alexander Webster’s attempt to offer an accurate account of the population of Scotland.”<sup>5</sup> That Smith does not have any sweeping objection to political arithmetic is most powerfully indicated by the fact that he occasionally (12, Vol. 2, p. 948) refers to statistics in the text “in order to illustrate and confirm what is said in the (text).” The other most notable use of statistics occurs when in the Digression on Silver he supports his argument with tables on the price of wheat (12, Vol. 1, pp. 204, 267–275). Finally, while some of the earlier practitioners of political arithmetic may have been devoted Baconians, Arthur Young, the main contemporary rival to Smith from this group, though he thought his facts could stand on their own, also wanted to show how they naturally arose from first principles (17, p. ix). Thus if I am correct there is not so much a disagreement here on the role that statistics should play in economic theories as there is one over the advancement of the state of the *art* of statistic gathering.

The second prospect for an important rival to Smith who differed from him in methodology is Sir James Steuart. Many have viewed Steuart as the main contemporary English competitor to Smith.<sup>6</sup> Unfortunately, Steuart wrote even less on methodology and philosophy of science than Smith. The implicit methodology underlying what he did write (14, pp. 7 & 28) seems consistent with Smith’s emphasis of finding a few explanatory principles capable of explaining the phenomena. There is also, however, a greater emphasis on simple induction than we find in Smith (14,

p. 6). It may be that this emphasis led him to “a marked tendency to trace out the ramifications of each part of his analysis in turn; a tendency which often leaves a perfectly clear line of thinking lost in a mass of detail and applications.”<sup>7</sup> It can be argued, however, that such a defect is more one of style than of substance. In fact Samuel Hollander (2, p. 42) has attributed to Steuart a comprehension of what I will later take to be Smith’s primary substantive contribution: “Formally much emphasis is placed on the characteristic interdependencies of an exchange economy, and in discussion of industry and trade Steuart explicitly stated that it is ‘the combination of every price interest which forms the public good.’” So, finally, Schumpeter’s evaluation (6, p. 176) of the *Principles* is probably a just one: “It was intentionally and laboriously systematic: what he wanted was to consolidate the factual and analytic knowledge of his time into a ‘regular science,’ that is to say, he clearly aimed at the same goal as A. Smith.”

Barring the possibility of comparing Smith’s substantive work with that of methodologically divergent rivals, we are only left with the alternative of looking at the *Wealth* in order to see if we can detect in it the impact of Smith’s views of science. We have seen in both the *History of Astronomy* and the *Lectures on Rhetoric* that he conceives of good science as the explanation of diverse phenomena on the basis of a few general principles elaborated in the form of a system or what we would today call a model. The question then is whether by his own standards the *Wealth* succeeds in being good science. Another way to state the question is to ask whether the explicit methodology that is expressed in the *History of Astronomy* and the *Lectures on Rhetoric* is the same methodology that implicitly underlies the economics in the *Wealth*.

If there is one general principle that underlies Smith’s system of economics it is that within an orderly market system self-interested individuals will make decisions that result in an economically efficient (national income maximizing) allocation of resources. Smith’s account of how the price system operates in order to provide information on profit making opportunities can be taken as

the basic model of his economics.<sup>8</sup> One who wanted to argue that Smith’s *Wealth* is more similar in method to Virgil’s *Georgics* than to Newton’s *Principles* could easily do so by pointing out several instances in which Smith could have applied this model but did not. One could point out with Bentham (13, p. 167) Smith’s failure to apply the model in his condemnation of usury or with Stigler (15, p. 264) Smith’s failure to apply the self-interest motive in his discussion of taxes. Or one could note that Smith (12, p. 25) begins his explanation of the division of labor not in terms of self-interest but rather in terms of the somewhat ad hoc “propensity to truck, barter, and exchange one thing for another.” Or, again, with Samuel Hollander (2, p. 317) one could point out Smith’s failure to apply his model to case of underdeveloped or underendowed nations. How much weight to place on these various examples must finally be a matter of judgement. Perhaps, as Bentham implies at the conclusion of the “Defence of Usury” (13, p. 167), Smith’s exceptions stand out so painfully because the *rule* had been argued for with such clarity and force. If so, then we may conclude that Smith in the *Wealth of Nations* did indeed live up to his own standard of good science.

Some believe that Smith, in his other main work, *The Theory of Moral Sentiments*, also strove to follow the example of Newtonian science. James Wodrow, who had attended Smith’s lectures, wrote that the *Theory* was “. . . a very ingenious attempt to account for the principal phenomena in the moral world from . . . one general principle, like that of gravity in the natural world, . . .” (quoted in introduction to Smith, 11, p. 3). Smith in the text, however, seems less confident that his work approaches the status of Newtonian science. In the course of mildly criticizing the introduction of God as a final cause of human actions Smith writes that “. . . the system of human nature seems to be more simple and agreeable when all its different operations are in this manner deduced from a single principle.” (11, p. 87). The implication is that in the absence of a divine final cause, no single principle has succeeded at explaining human nature. If Smith aimed at achieving the standard of Newtonian science, he failed because

his *Theory* proposes almost as many ad hoc sentiments as there are actions to be explained. But Smith's aim is unclear, and may have been more to provide edification than to provide explanation. Evidence for interpreting edification as the aim can be found in Smith's criticizing other works in moral philosophy for being "... incapable of exciting in the heart any of those emotions which it is the principal use of books on morality to excite." (11, p. 340).

At the outset it was suggested that the progress of economic science depends on the soundness of the implicit methodology underlying the work of economists. By examining the impact of Adam Smith's philosophy of science on his economics, we have attempted to test the proposition that self-conscious methodological reasoning should be of value to the economist. First discussing the meager sources of his views on science, I finally argue that it is plausible that those views had a significant impact on his economics.

### Notes

1. Cf. Herbert F. Thomson, "Adam Smith's Philosophy of Science," *Quarterly Journal of Economics*, (May 1965), p.213. There is clear internal evidence that the *Essay* was written before 1758: Cf. Adam Smith, "History of Astronomy," in Lindgren, ed., *The Early Writings of Adam Smith*, p. 106.
2. See also: Andrew S. Skinner, "Science and the Role of Imagination," Chap. 2 in *A System of Social Science*, Oxford: Clarendon Press, 1979, pp. 14–41.
3. But cf. his 1786 letter to Thomas Cadell (in W. R. Scott, *Adam Smith, as Student and Professor*, p. 299) in which he says of a work on moral philosophy: "It is as free from metaphysics as is possible for any work on that subject to be. Its fault in my opinion is that it is too free from them (sic)."
4. As reprinted in John Rae, *Life of Adam Smith*, pp. 262–263.
5. Editor's footnote in Smith, *Wealth*, (Oxford edition), Vol. 1, p. 535.
6. Cf. e.g., Schumpeter, *History of Economic Analysis*, p. 176 and S. R. Sen, *The Economics of Sir James Steuart*, passim.
7. Andrew S. Skinner, "Analytical Introduction," in Steuart, *Principles of Political Economy*, (Chicago edition), p. lix.

8. Cf. "Of the Natural and Market Price of Commodities," in Smith, *Wealth*, Vol. 1, pp. 72–81.
- \* I am grateful for comments from George Stigler.

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16. Thomson, Herbert F., "Adam Smith's Philosophy of Science," *Quarterly Journal of Economics*, (May 1965): 212–233.
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