

The Myth Of Adam Smith

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Salim Rashid, *The Myth Of Adam Smith*, Edward Elgar, 1998,
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The aim of this book is to destroy the reputation of Adam Smith. More precisely, it's aim is to destroy Smith's reputation as *the* economist of the 18th century (rather than just another good economist of the 18th century). The author pursues this aim by way of a simple argument. He first posits a test for greatness: a great thinker will say things that are important, true and original. He then argues Smith said nothing that was important, true and original.

It is not difficult to substantiate the assertion that Smith was not original. This is well known to historians of thought. The productiveness of the division of labour; the operation of "demand" and "supply" in a loose sense; the canons of taxation; none of these were novelties in 1776. Rashid's contribution is to underline this lack of novelty by a scholarly exploration of the many and curious antecedents of Smith's thought.

If Smith was not original; how much was true? It is not a demanding task for the modern critic to find Smith's doctrines badly wanting. Smith presents some rather vulnerable targets in this respect: the wages fund; the hierarchy of capitals; real bills doctrine. As it happens, Rashid tends not do battle with Smith on such large, abstract questions of theory. Rashid's quarry are instead Smith's more specific errors: Smith's complacency about the possibility of famine in an unregulated grain market; his implicit mischaracterisation of the contemporary industrial development (pin factories not steam engines); his deprecation of that most useful legal device, the joint-stock company. Rashid also sifts the 1052 pages of Smith's five book volume to find more incidental (if equally wrong) errors: such as Smith's "extraordinary" claim that price of coal is set by the cost of the most productive mine, rather than the marginal mine. In a similar spirit he collates 90 errors in Adam Smith's references or statements that have been previously noted by Smith's various editors. "It is not a small list", writes Rashid (56).

Rashid like any good author is confident of his thesis. This reviewer, however, is not. Consider the issue of error in Smith. I would venture that Smith had enough insight in several of the topics, where Rashid finds him in error, to give him a remarkable prevision of two centuries about to follow. If Smith underestimated Manchester, he foresaw Henry Ford's Detroit. If Smith would be surprised to observe the success of joint stock companies of the 20th century, he would not have been surprised by recurrent tensions between management and shareholders. If Smith was too complacent about the prospect of famine in an unregulated market, then he was right to judge the state as the most culpable author of famine. The 20th century has born out Smith's judgement terribly: the largest famine in history, with 20-30 million deaths, was China's Great Leap Famine of 1959-1961, and was largely a product of state policy.

Further, the establishment of error in Smith is sometimes arguable, since his expression of theoretical argument in an informal manner necessitates a degree of interpretation. (This is true of all classical authors: should we interpret Ricardo in the way that leaves him right, or wrong?) An example is Smith's "extraordinary" assertion

that the most productive mine "regulates" the price in its neighbourhood. Smith's inadequate attempt at justifying this claim invokes price undercutting: is therefore a Bertrand oligopoly (rather than perfect competition) the best interpretive tool of Smith's contention? And if so, would would Smith's conclusion be wrong?

Finally, the proof of error in any author does not by itself necessarily discredit that author very greatly. Error is noise in the signal. As long as there is a signal there will be error; this is a way of the world. As long as something important is said, something wrong will be said too. So while the correction of errors is an activity which advances inquiry, the most important activity of the critic is the measurement of the amount of signal. Or, to be more accurate, the amount of *innovative* signal. This brings us too Smith's originality.

Originality is rightly admired. But there is originality in ideas and originality in system. Originality in system does not require originality in the elements of the system. Consider Ricardo. By Rashid's criterion it would seem that Ricardo was feebly derivative: diminishing marginal productivity; the "Ricardian" theory of rent; comparative advantage; Say's Law; the quantity theory of money; the falling rate of profit; not one these are Ricardo's brain child. But Ricardo contrived a system, which neither Turgot, nor Malthus, nor Torrens, nor Say, nor Hume managed. I would go further: I would say originality in system is more important than originality of ideas. J.S. Mill once commented that the only thinkers who are remembered are those who built systems (even if it is only a fragment of that system that is remembered). Is this not the way it should be? And if it should be then it is right that Smith be remembered, for Smith did build a system, however blurry it may be at the edges and soggy down the bottom.

I believe it is useful to compare Smith with the 18th century physician Herman Boerhaave. Despite the huge esteem he enjoyed in his lifetime Boerhaave is not responsible for any significant medical discovery whatever. But consider what one of his students wrote in 1710:

"He was the most extraordinary man of his age, perhaps in the whole world; a clear understanding, a sound judgement, with strength of memory that could exceed none, and indefatigably laborious: it is true he had not the brightness of invention, that some authors have; but with his talents he has done more service to the world of physick, than all his predecessors in the whole world put together; by digesting a huge heap of jargon and indigested stuff into an intelligible and rational system".

Something similar could be said with justice of Smith.

Rather than trying Smith's greatness by identifying those elements in it which are important, true and original, I would instead put the following question: if the *Wealth of Nations* is not *the* book of the 18th century, then what is? Cantillon's *Essay on Commerce in General*?; any of the productions of Quesnay?; Steuart's *Principles of Political Economy*? If any one would select any of these, then let the selected work be compared in originality, truth and significance with that of Smith. Does anyone believe that any would do better than Smith?

Let me change the test: if you had to choose just one book to give to a novice who wished to absorb the best of 18th century economics, which would you give them? *The Principles of Political Economy*?

I said my piece. But I have not yet performed the most useful function of any book review: to indicate to the potential reader whether it should be read or not. The answer is clear: this book must surely be read by all Smith scholars. Rashid has cast a sharp, noteful eye over a wide range of Smith's intellectual activities and attributes. He has marshalled a great amount of critical commentary by Smith's contemporaries and

near contemporaries. He has done so with learning and intellectual discipline. He has achieved controversy without ever contraversialising.

Henceforth, in any discussion of Smith, the Smith scholar's round of activity must include "looking up Rashid".

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