

Armando C. Ochangco, *Rationality in Economic Thought: Methodological Ideas on the History of Political Economy* (Cheltenham and Northampton: Edward Elgar, 1999) pp. xv, 332, \$100.00. ISBN 1 85898 955 8.

Few working the history of economic thought (HET)/methodology furrow believe that contemporary economic practice heeds methodological preaching. But does the problem lie with what economists do or with what methodologists and other preachers say they should do? Some, like Mark Blaug, indict economists, or at least perverse disciplinary incentives. Their solution lies in greater epistemic virtue—do the right thing and you'll get the right stuff. But most find fault with the preaching, quarreling specifically with the idea that there exists a working, stable set of methodological rules capable of guiding economists to good scientific outcomes.

The latter group comprises a motley set of critiques, but most fall into one of two camps. The first camp's members reject the whole normative project of divining rules for good scientific practice. Some, like Deirdre McCloskey, will entertain rules of the mild *Sprachethik* variety, but all in this camp are axiomatically "anti-epistemological": they believe that science accomplishes nothing special epistemologically. As such, rules that ostensibly promote good scientific practice must be seen as wholly rhetorical or as devices to promote the interests of current in-groups. Call this camp the post-methodologists.

The second camp rejects traditional preaching though not the preaching enterprise. Their quarrel is not with rules *per se*, but with the application of traditional methods—mostly carried over from the physical sciences—to the special circumstance of economics *qua* social science, what Hayek called scientism. Dan Hausman invokes Mill, Tony Lawson invokes Roy Bhaskar's critical realism, and Uskåli Mäki wants "realisticness," but all are working within the traditional, philosophically informed project of normative methodology.

For the first camp, the importance of history is evident. One may wonder why we should bother with historical or sociological evidence for the view that evidence cannot matter, but it is clear that once you have sworn off prescription, description becomes rather central. History also matters to the second camp, as a source for their normative theories (the best scientific gambits) and as evidence for or against them. Both preachers and preachers against preaching, it seems, need history.

Why should historians care? Because the reverse is also true. Historians need theory in the sense that it inevitably underwrites their historiography. Behind every historiography lurks an epistemology, even if it is anti-epistemology. If your (anti-) epistemology argues that the very idea of knowledge is misbegotten, then you will, for example, regard an internal history of scientific ideas as uninteresting, perhaps even incoherent. The externalist historian who elevates context over text, ultimately does so for (anti-) epistemological reasons. Moreover, for historians of economics (and of science more generally) theory of knowledge also impinges at the level of practice, when it comes time to interpret political economists' understanding of the status of their claims.

Ochangco's book revises a dissertation, which, in original form, seems to have been a fairly conventional (if New School-ish) doctrinal history of canonical

figures' views on the value theory. But after five chapters on Smith and Ricardo, Marx (2), Jevons and Walras, and Marshall, respectively, the urge to interpret proves irresistible, not least because these giants seem to disagree, to the extent they are even talking about the same things. Thus, three chapters of methodological interpretation are appended, and methodological "interludes" are inserted into the original chapters, in an attempt to make sense of the preceding intellectual history.

When one asks, "what were they up to?" it is hard to resist asking "what should they have been up to?" and, further, "what did they claim they were up to?" For better or worse, historians of economics have typically taken these questions to represent different genres: history of political economy (what economists have done), history of methodology (what scholars of various backgrounds say economists should do), and history of what political economists have said about methodological matters, respectively. The last two categories, which clearly overlap, are under-researched, notwithstanding two recent and very different interpretations of the classical political economists' views of philosophy of science and methodology (Poovey 1998; Redman 1997), so Ochango's project is welcome.

For this reader, unfortunately, the problems begin with the title. The book is not a history of the concept of rationality as used in economics, and neither is it a Lakatosian attempt to rationally reconstruct a history of the theory of value. The subtitle, "methodological ideas on the history of political economy," sounds something like historiography. Changing the "on" to "in" gets one closer, but, even here, Ochango says at the outset that he is not offering a history of classical and early neo-classical economists' views on methodology.

I don't wish to be unfair: the difficulty of constructing a decent title is an occupational hazard of the business. But the titular confusion, it soon emerges, is symptomatic of a book that is unsure of what it is meant to be about. Sometimes the book concerns what canonical figures said about the theory of value, sometimes what they said about method, and at other times, it concerns the methodological implications of disagreements among the canonical figures. All of these are worthy themes, but any one, stretched from Smith to Marshall, is already daunting, and all three cannot begin to fit between the covers of an Elgar volume.

What results is a smattering of each. The five historical chapters were already stretched quite thin, and once pared down to accommodate the subsequent methodological interpretation, are too attenuated to maintain any thematic traction. There are certainly worthwhile moments, as with the succinct reading of Jevons's *Theory of Political Economy*. But from what Marx called the classical economists, to Marshall's neoclassicism, there are simply too many tectonic changes in political economy: not merely the displacement of labor and cost theories of value, but the relocation of value from an intrinsic aspect of goods to a species of subjective regard, the concomitant shift in emphasis from production to exchange, the introduction of formal methods into British political economy, and the development of a distinct methodological literature reflecting on the nature and scope of political economy.

Ochango, to his credit, is alert to such matters, but his hands are tied by the

scope of the existing survey, and the result is a patchwork of interesting but ultimately unconnected ideas. At one point he admits:

Though various normative interpretations, appraisals and proposals are indicated and scattered in the text and footnotes, I did not pursue at any length to find out whether there were “good fits” among these; nor whether the methodological grounds intended to provide the more general principles of justification for the solutions to these problems were, themselves, adequate or valid (p. 246).

The author chooses to read this outcome not as a failure of integration or as the product of an overambitious project, but as an opportunity for making historiographic inferences. He sensibly notes that writers as disparate as Smith and Walras and Marshall are inevitably attempting different things, and use varying methods applied to different domains. Well, yes, but it does not follow that attempts to find a thematic continuity must produce either perplexity or Whiggishness, a claim that is asserted (pp. 41, 78, 113, 153, 217, 220) more than argued.

I’m afraid the methodological overlay does not help in this regard. For a book in the *Advances in Economic Methodology Series*, the methodological chapters are eccentric. There are almost no references to the economic methodology literature, and, from theory of science, there are but a few desultory waves at Kuhn and Lakatos and the like. Technical terms are introduced and abandoned fairly haphazardly. These chapters are really more an extended reflection on particular difficulties of doing successful history of economics, which are real enough.

The book’s central incongruity—talking methodology in vaguely post-methodological fashion, while practicing the kind of doctrinal intellectual history that post-methodologists axiomatically foreswear—may offer a worthwhile lesson. It is this: adopting a particular historiographic stance does not, by itself, confer immunity from the traditional sins of history of economics. Whiggishness, for example, is not unique to traditional HET, with its efforts to map intellectual lines of descent, nor is it due solely to credulousness with respect to traditional theories of science.

Mary Poovey’s stimulating book helps show this. Where the present volume is history in search of a theme, Poovey’s book is a theme relentlessly seeking history. She conceives of the nascent sciences (I use this term and its cognates here anachronistically) of society and wealth as attempts to legitimize, with the imprimatur of expertise, intellectual enterprises that already have been recognized as hopelessly compromised by Hume’s problem of induction. Thus, for example, even social “science” such as Adam Smith’s conjectural history is portrayed as an early attempt to legitimize the emerging political economy as properly scientific. Who but a turn-of-the-millennium post-methodologist, well versed in contemporary epistemological debate, could propose a theme as presentist as this? Whiggishness and other historical pitfalls are equal opportunity: they do not defer to the historian’s view of knowledge—anti-epistemological or other.

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REFERENCES

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- Redman, Deborah. 1997. *The Rise of Political Economy as a Science: Methodology and the Classical Economists*. Cambridge, MA: MIT Press.

Kevin H. O'Rourke and Jeffrey G. Williamson, *Globalization and History: The Evolution of a Nineteenth-Century Atlantic Economy* (Cambridge, MA, and London: The MIT Press, 1999) pp. xii, 343. ISBN 0 262 15049 2.

Most of the fourteen chapters present data and results from two or three of the authors' previously published papers along with summaries of the relevant literature. The resulting flood of information is controlled and purposeful, resulting in an impressive demonstration that careful research in economic history has much to offer.

They argue that the defining characteristic of the New World was great natural wealth and a relative lack of labor. As transportation costs fell it became feasible to shift land-intensive production processes to the New World, reducing the pressure on the land in the old. Expansion of production in the New World attracted capital flows to develop railroads and other public infrastructure, much of it closely related to trade. The high wages and growth attracted young workers from throughout Europe while the Irish famine provided a focal point.

Trade and the great migrations of labor would both tended to reduce land values in Europe while increasing them in America. Predictably, landowners in Europe opposed trade. On the continent the opposition succeeded to such a degree that the relative prices of agricultural goods rose while they fell rather dramatically in free trading England. The authors argue the success of free traders in England was due to strong industrial interests that would benefit from the shift away from agriculture.

As unskilled wages fell relative to average income in the Americas countries that were open to foreigners began to change policies. The slowdown in immigration was associated with declining capital flows that were severely interrupted by two world wars and the depression. The authors argue that the interruption of globalization was at least partly an endogenous response to rising income inequality and not an exogenous shock associated with wars.

This brief survey allows one short paragraph per hundred pages and does not begin to do justice to the painstaking presentation of evidence. Each step in the chain is subjected to extensive scrutiny while the idiosyncrasies of individual countries and periods are given full attention.

At its best, the long historical perspective over a variety of countries allows convincing evidence to be presented with simple charts and tables. For example, figure 9.2 shows a scatter diagram of countries for the period 1870 to 1913 with an index of the change in equality on the vertical axis and the real wage on the horizontal. There is a clear negative relationship indicating that the resource-rich, high-wage countries experienced reduced equality as immigration depressed