BOOK REVIEWS

Vernon L. Smith, *Rationality in Economics: Constructivist and Ecological Forms*, New York, Cambridge University Press, 2008, pp. xx+364.

Vernon Smith – winner of the 2002 Nobel Prize in Economics – was born in Wichita, Kansas, and grew up in a farm in the Western planes during the Great Depression. After graduating in electrical engineering at Caltech, he went to Harvard to study economics, a subject that he later taught at Purdue University, the University of Arizona, and more recently George Mason University. Smith won the Prize for his pioneering use of laboratory experiments in economics, a methodological revolution that is now largely taken for granted but would have seemed unthinkable three or four decades ago. (Smith, who has spoken publicly about coping with Asperger Syndrome, suggests that this mild form of autism may have nurtured a tendency to «think outside the box»).

Rationality in Economics (re) is Smith's third book and, as it seems fitting for an 81 year-old laureate, also his most ambitious one. While the first two volumes merely collected his most significant scientific and methodological papers, re is a proper monograph with a large amount of original material and some reprinted chapters that have been edited to fit with the core themes of the book. Smith is giving us the 'Big Picture' here: re explains of what his long and distinguished research project tried to achieve, why, and what legacy is passed over to the next generation of economists and social scientists more generally.

It is worth stressing that re is not merely a jumble of philosophical speculations by a retired scientist. Quite the opposite: like its predecessors, re is full of scientific content and work-in-progress information, reports and commentaries on experiments that Smith has run with his collaborators only a few months ago. The main difference is that this time the experimental results are embedded in a much more general discourse about the goals of economics, the functioning of markets, and very the nature of human sociality.

The general framework – as the title of the book suggests – owes a lot to Friedrich Hayek. This is ironic, for Hayek was a fierce opponent of the application of natural science methodology to the study of social phenomena. The idea of controlled experimentation in economics would have probably struck him as a 'scientistic' opprobrium. But Hayek could not foresee that experiments would provide a vivid demonstration of one of his central theses: the capacity of competitive markets to clear under informational requirements that are much weaker than those imposed by standard Neoclassical theory. The connection between this result and Hayek's ideas, to be fair, was not clear even to Smith until recently. Apart from a 1982 paper, there is hardly any mention of Hayek's work in Smith's previous (and voluminous) scientific production.

In his Nobel lecture, where the seeds of re can be found, Smith first introduced a distinction between two kinds of rationality that is distinctively Hayekian in spirit. While 'constructive' rationality is the product of our conscious attempts at planning our lives and intervening in nature and society, 'ecological' rationality is an unintended result of our actions as they interact with a myriad of factors that human minds cannot take into account during decision-making. The key thesis of re is that *both* forms of rationality are crucial for the development of human societies, and that grave mistakes are made when we focus on one at the expense of the other.

Unfortunately, this is precisely what happens in mainstream economics. Neoclassical theory, with its focus on individual decision-making and its commitment to explaining the properties of market processes from the interaction of rational individual decisions, is a prototypical example of constructivist *hybris*. It ignores that individual decisions and market outcomes are governed by processes that lie off the radar of the individuals involved – including, often, the economists who study them.

The first part of the book is devoted to markets, and to the ways in which institutional solutions to the problem of informational deficiency and dispersion emerged 'spontaneously' over history. I put 'spontaneous' between inverted commas because constructive rationality does play an important role in this process: like any technological device, economic institutions are invented by individuals or groups of people as they are grappling with a given problem. The inventors, however, are usually in a poor position to predict all the implications of their newly invented tools. So ecological rationality is required to determine which institution constitutes a good adaptation to its environment. Using a biological metaphor, Smith argues that constructive rationality produces *variation* (different solutions to a problem) while ecological rationality takes care of *selection*.

Interestingly, Smith sees experiments as one – albeit not the only one – way of selecting by trial and error. An entire chapter (ch. 6) is devoted to reviewing a series of failed attempts at reforming the processes used to auction the electromagnetic spectrum rights of the Federal Communication Commission. Smith blames excessive reliance on game theory for this history of (often wrong-headed) adjustments, whereas the use of experiments to test-bed the functioning of different institutions would have greatly sped the selection process. Chapter 13 expands on this theme by pointing out that scientific progress is driven more by technological success – the construction of 'machines' – than by theory. (I should note with pride that Smith borrows this insight from historians and philosophers of science. It seems that we are not always totally useless as a profession.)

Neoclassical theory is not only wrong, according to Smith, but also encourages wrong-headed theoretical revisions. If perfect information is not required for markets to work, then models of asymmetric information become quite irrelevant (ch. 5). Similarly, behavioural economics of the Kahneman-Tversky kind (ch. 7) is criticized for engaging in the easy and trivial task of highlighting failures of constructive rationality. Of course individuals are not rational in the sense of Bayes-Nash rationality. But again, this does not mean very much, if perfect constructive rationality is unnecessary for the correct functioning of markets.

As these remarks suggest, *ER* is deeply imbued with free-market heterodoxy. Smith's only 'mainstream' theoretical commitment seems to be Adam Smith's insight that wealth is produced by specialization, and trade is required to allocate the fruits of specialization (the «fundamental theorem» of classical economics, p. 156). The interesting scientific challenge is to find out how trade and specialization work. This open-mindedness goes hand in hand with very strong value judgment. Like an entomologist of markets, Smith is clearly in love with his object of study. And yet is perspective is refreshing. His message is that we learn more by studying markets than by studying *theories* of markets, because markets have evolved to solve problems that theorists have misrepresented right from the start. Although this plea to directly investigate empirical reality may seem obvious to many scientists, in a theory-dominated discipline like economics it is not.

It is not obvious even in experimental economics, where many practitioners – unfaithful to Smith's teachings – see their task primarily as theory testing. Three chapters of *RE* are devoted to a detailed discussion of recent experimental work on fairness and reciprocity. In the 1990s Smith took many by surprise when he started to work intensively with Ultimatum, Dictator, and Trust games. Up until then these games were considered primarily as important sources of anomalous results – vivid examples of how experimental subjects fail to comply with the principles of game theory even in the simplest strategic environments. But of course they are anomalies only if one takes game theory seriously as a theory of rationality. Smith does not, and shows how these experimental designs turn out to be important sources for the study of emergent order during repeated small-group interaction.

Smith dismisses the prevailing interpretation of these experiments as tests of one-shot models. If the data told us about one-shot strategic behaviour, then we would have to conclude that experimental subjects are not maximizing their own material gains. This interpretation has been quite popular for a while, and has led to the creation of several theories of other-regarding preferences, where a concern for other's payoffs, or an aversion to inequality, are written in the arguments of each individual's utility function. Smith argues instead that experimental one-shot games are 'windows' open on the social norms that govern our behaviour in one-to-one interactions *outside* the lab. Such norms help us coping with situations in which there is no predetermined property right over the products of social cooperation. They are, by and large, norms of reciprocity ("Do unto others as you would have others do unto you") that have evolved in *repeated* play. Their effects, however, are manifested in a wider range of circumstances – including the lab – than those in which they are ecologically fit. We are tempted to interpret these pro-social tendencies as manifestations of an inner moral character only because we are tricked by our own brain to believe that we (our conscious mind) are in control. Models of fairness and equality simply translate this fallacy of common sense into formal theory.

This interpretation is not entirely novel, for evolutionary psychologists and game theorists have made similar points before. Smith's own contribution consists in introducing various clever experimental designs to investigate the reciprocity hypothesis. The way he uses them is also quite distinctive. Unlike other economists (Ken Binmore, say, or Herbert Gintis) Smith does not have a theoretical axe to grind. He is not interested in vindicating the power of game theory in the face of alleged 'anomalies'. A vague gesture towards folk theorems is the greatest tribute to game theoretic reasoning to be found in this book, which instead is full of examples of how the theory can (and does) lead you astray. This attitude is both a virtue and, potentially, one of the book's weaknesses: a reader looking for answers to the Big Questions will not find any precise model of ecological rationality here, or an explanation of how it combines and complements its constructivist counterpart.

Contemporary theories of the evolution of norms are so full of unproven conjectures, leaps of faith, and untested propositions, that I find Smith's stern empiricism quite admirable. As a consequence, Smith's Hayekian views on rationality appear more as useful correctives to neoclassical economics' excesses than as concrete recipes for new theorizing. Certainly Smith leaves many important questions untouched: granted that impersonal market exchange and norms of personal reciprocity are *both* important for material and cultural growth, *how much* of each can a society afford? Where and when should we rely on one or the other form of ecological rationality? What can we do to foster one of them without so hindering the survival of the other? Because of all these unanswered questions, RE perhaps is best read as an open project for the future. As Smith coherently points out, even the best scientists are usually incapable of foreseeing the trajectory of research in their field. Smith devotes a final chapter to illustrating a few results of neuroeconomics – a booming field to which he has contributed with his collaborators – and speculating what it may deliver in the future. Revealingly, this is also the shortest chapter in the book.

I have only provided a sketch of a rich book that is full of stimulating ideas, empirical data, and projects to keep an entire generation of economists busy. There are many reasons to read it, and several ways of doing it. Historians of economics may be tempted to look for the key to understanding Smith's remarkable and unusual career. Surely they will find in *RE one* way (Smith's own) of making retrospective sense of it. Personally, I still find Smith's first collection (*Papers in Experimental Economics*, Cambridge University Press, 1991) a better source of insight concerning his revolutionary work. While the essays in that collection were still 'raw', *RE* gives a more refined version that may not be entirely faithful to the context in which the experimental revolution took place. Be that as it may, there are many other reasons to read this remarkable product of one of the most remarkable minds of contemporary economic science.

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Stewart Davenport, *Friends of the Unrighteous Mammon: Northern Christians and Market Capitalism, 1815-1860*, Chicago, University of Chicago Press, 2008, pp. 270.

Stewart Davenport opens *Friends of the Unrighteous Mammon* with a New Testament reference in Matthew (6:24) to «ye cannot serve God and mammon» and ends with a longer quote from essentially the same message in Luke (16:1-13). Davenport sets out to learn how American Protestant preachers/teachers in the North during the antebellum period reconciled this teaching with market capitalism. He focuses on the North for three reasons which to this reviewer are not compelling and are best left to interested readers to evaluate on their own (see pp. 6-7). In reading his work, it is important to keep in mind that Davenport is not an economic historian or a specialist in the history of economic thought. He is an historian.

His investigation, and the book follows this fundamental organization, centers around three groups of Protestant preachers/teachers or as Davenport calls them «Christian economic thinkers» and «Protestant educators». The three are: the clerical economists, the contrarians, and the pastoral moralists. The clerical economists embraced the emerging discipline of economics in the early 19th century as developed by Adam Smith and his immediate followers in England and Scotland and which became known as political economy. The contrarians stood firmly against the underlying principles of the clerical economists, the processes by which they reached their conclusions, and their conclusions as well. The pastoral moralists gave their approval to market capitalism and addressed the issue as to how a Christian could function as a moral economic agent within that system.

The book's section on the clerical economists which is positioned before the other two is much longer than either the section on the contrarians or the section on the pastoral moralists. However, Davenport states at the outset of his discussion of the pastoral moralists that this section «is by far the most complicated».

Davenport identifies five men in the clerical economist group: John McVickar, Henry Vethake, Alonzo Potter, Francis Wayland, and Francis Bowen. They embraced and advocated the central tenets of political economy which they perceived as a Christianized discipline promoting freedom, individualism, and utilitarianism. This group of five presented public lectures on political economy, were effective in getting courses in political economy into the curriculum at American colleges and universities, and wrote their own textbooks on political economy. They left unanswered two questions: how is personal virtue to limit the individual's pursuit of happiness and what is to be done about poverty and inequality.

Two men – Stephen Colwell and Orestes Brownson – formed the core of the contrarian group. Colwell was and remained pro-slavery until the emancipation. His proslavery stance was grounded in two arguments. First, slavery was a profitable and efficient system of labor organization which enriched both North and South. Second, slaves in America were better provisioned than wage-slaves in England. What political economists referred to as self-interest, Colwell called insatiable selfishness. In place of self-interest he urged his followers to embrace Christ's words to love one another. He insisted that socialism was neither atheistic nor irreligious. Indeed he asserted that it was more in touch with Christianity than any other system.

Davenport states that there are two Brownsons, the one who before 1840 switched from Universalism to atheism then to Unitarianism and then to transcendentalism and the one who afterward joined the Catholic Church. Brownson's conversion changed him from a religious contrarian to a social radical with conservative religious convictions that are best described as reactionary. He abandoned the isolated self of radical individualism for the community of Leroux's «life by communion», a philosophy to which he held steadfastly for the remainder of his life. Based on Davenport's research in which Brownson writes about the importance of institutions in «carrying the human race forward» it is not entirely farfetched to identify Brownson as a forerunner of institutional economics.

Both Brownson and Colwell called attention to what Jesus said and did as a way of sorting through the problem of how to deal with one's neighbor. The clerical economists, in sharp contrast, never mentioned Jesus and hardly ever referred to the scriptures.

Prominent among the pastoral moralists were nine men including Orville Dewey, Jason Whitman, Andrew Preston Peabody, Henry Augustus Boardman, and Joseph Emerson. Virtually every one of them was a minister with the well-being of their congregants uppermost in their daily activities. Their common project was to finds ways to conduct oneself as a practicing believer within an ever-evolving market capitalism. Whereas the clerical economists viewed market capitalism as evidence of God's goodness, the pastoral moralists regarded the system as a testing ground for the development of virtue in the followers of Christ.

In conducting his research, Davenport faces two questions the first of which challenges every historian. How is he to transport himself back to the market capitalism of the antebellum period without bringing along his understanding of that system as it exists today? How can he fairly judge these American Protestant preachers/teachers for their ability to reconcile the teachings of their faith with market capitalism when they were informed by a discipline in its infancy with ideas that since have been discredited such as the wages fund and the iron law of wages? The first question only Davenport can address. The second is for his readers to sort out.

A large part of the confusion within economics even today traces to the very origins of the discipline in Smith's *Wealth of Nations* and his earlier work *Theory of Moral Sentiments*. The one underscoring self-interest; the other generosity, sympathy, and benevolence. Davenport's own resolution seems to be (see pp. 210-211) that self-interest is the actuating principle driving the behavior of economic agents and sympathy is the limiting principle. This reviewer addresses the problem differently.

Smith lived in the script stage of human communication well before the electronic stage which began with the telegraph and which altered communications for all human beings including economic agents and transformed their understanding of others and of themselves. With electronic communication the individual as an isolated self enshrined in *homo economicus* evolved into the person embedded in much wider circles of marketplace and workplace contacts in which they function as both individual beings and social beings. The often competing demands of their individuality and their sociality are a powerful source of tension in their lives. Because he was a child of the individualism of the 17-18th century Enlightenment, Smith could not fully reconcile the individuality of the *Wealth of Nations* and the sociality of *Theory of Moral Sentiments.* Had he lived in the electronic stage, Smith probably would have seen more clearly the complementary nature in his own work, and would have shared that more profound vision with his followers. The same no less is true of the antebellum Protestant preachers / teachers.

William Barclay, the well-known Scottish interpreter of the New Testament, offers important insights regarding the parable that Davenport references in the opening chapter of *Friends of the Unrighteous Mammon* and quotes at length in the last chapter. Barclay calls this difficult-to-interpret parable «a bad man's good example». In his commentary, Barclay characterizes the steward, his rich master, and the debtors as a «choice a set of rascals as one could meet anywhere». The steward is a rascal because he is an embezzler. The rich man is a rascal because even though he was shocked at what the steward had done, he appreciated the shrewdness behind the scheme. The debtors were rascals who owed rent to the rich landlord but saw their personal advantage in the steward's 'cooking the books' because it reduced what they owed the landlord in rent.

Barclay sees four lessons in this parable. First, if only the followers of Jesus pursued virtue as eagerly as the man of the world pursues wealth they would become even better Christians. Second, human friendships can be cemented by material possessions in eternal life by giving one's wealth in this life to the poor and in the here-and-now by making life easier for others. Third, the way in which a person discharges his duties regarding small affairs is indicative of his fitness in larger matters. Fourth, we either belong to God totally or not at all.

Davenport states in the last chapter that it is difficult to know how the contrarians and the pastoral moralists would interpret what the parable means. The problem turns on Davenport's own understanding of the parable, borrowed largely from Emil Brunner's *Sowing and Reaping: The Parables of Jesus* to the effect that Christians should be even worldlier (p. 215), the debtors are winners, the rich man a victim, and the steward is the patron saint of self-interest (p. 216). Contrast that with Barclay's claims that steward, landowner, and debtors are rascals; in addition the steward is an embezzler. Finally, Christians are not commanded to become worldly but to become as eager to follow Jesus as the steward is to follow mammon.

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Eckhard Hein, *Money, Distribution Conflict and Capital Accumulation. Contributions to 'Monetary Analysis'*, Basingstoke (uk) and New York (us), Palgrave Macmillan, 2008, pp. xiv+214.

This book gathers a number of contributions to the monetary theory of production that goes back to Keynes's (1933) project, elaborating in particular on the effects of interest rate changes upon economic growth and functional income distribution in the tradition of Kaldor, Kalecki, and Joan Robinson. The author proposes different monetary models of income distribution and economic growth addressing the role of the so-called NAIRU (Non Accelerating Inflation Rate of Unemployment) and the problem of a persistent gap between the long-run equilibrium rate of capacity utilization and the 'normal rate' that firms consider in planning their investment projects for capital accumulation.

After a short introduction, the book is divided into three parts. The first part reviews what Schumpeter (1954) called «real» and «monetary» analysis respectively, deriving a number of implications for the relations between monetary policy, income distribution, and capital accumulation. As «real analyses», Hein considers classical, neoclassical, monetarist, new classical, new-Keynesian, and «New Consensus» theories, which he surveys briefly before presenting more deeply the «monetary analyses» of the Banking School, Marx, and Keynes, as well as those of neo-Ricardians and post-Keynesians. This survey of alternative paradigms in economics provides the basis for the second part of the book, where the author investigates the effects of changes in policy interest rates on functional income distribution and fixed-capital accumulation, using post-Keynesian demand-driven models of distribution and growth. Enlightened by the monetary circuit approach, the analysis in this part of the book shows that the rate of interest is a policy-controlled variable affecting the level and distribution of national income as well as its dynamics over the short and long run. Hein's analysis also succeeds in incorporating the monetary theories of Kaldor, Kalecki, and Robinson into contemporary post-Keynesian monetary models of income distribution and economic growth, extending two principal models in this respect, namely, the Rowthorn-Dutt-Amadeo model, and the Bhaduri and Marglin (1990) model.[1] In the third part of the book, Hein considers the inflationary consequences of conflicts over functional income distribution, and the effects of central bank interventions to curb accelerating inflation. Using the monetary Kaleckian models of growth and distribution put to the fore in the second part, in this part of the book the author focuses on the role of the NAIRU both in «New Consensus» models and in the real world as an «inflation barrier» enforced by monetary policy, without any guarantee that over the long run the unemployment rate will hit (or fluctuate around) such an artificial construct of conventional economics as is the NAIRU. Elaborating on the Dumenil and Levy (1999) approach, the author further shows that in a monetary production economy «the "normal rate" of capacity utilization is endogenous to distribution conflict and monetary policy intervention in the long run» (p. 167).

This book provides a comprehensive and very deep view over many topics and issues in contemporary macroeconomics, enriched by the integration of the role of both money and credit into distribution and growth models in the tradition of Kalecki. Hein has done a valuable service to the economics profession in elaborating an original synthesis of various strands of monetary analysis, with theoretical and policy-oriented arguments that he introduces and elaborates upon in a balanced way throughout the whole book.

Although the book is well structured and the author takes care in always providing an introduction and a conclusion summarizing the contents of any parts of it, the treatment of 'real analysis' should have been more extensive than the four pages Hein has written on it. Indeed, the first part of the book could have been more convincing had the author begun with a critical investigation of 'real analysis' in the history of economic thinking, referring to both past and contemporary critics, from

[1] The Rowthorn-Dutt-Amadeo model has been put forward by ROWTHORN 1981; DUTT 1984, 1987; AMADEO 1986a, 1986b.

which he could have moved on to present, discuss, and elaborate on the modern monetary analysis to which he subscribes, grounding it on its founding fathers and mother (Marx, Keynes, Kalecki, Joan Robinson and others), to conclude this part with a whole chapter on the post-Keynesian monetary views. This would have provided a more convincing ground on which the author would have built then his own contributions more persuasively for the conventionally educated reader.

Another (state-contingent) limit of the book is that the author does not consider in it the (growing) importance of finance in the (mal)functioning of our monetary economies of production and exchange, nationally as well as internationally. Even though the book has been written before the financial meltdown of 2007-2008, the monetary analysis that it elaborates upon would have interested a much wider readership had the author included a «finance sector» – however defined – in his models and discussion of the relevant and abundant literature. This is so much so that Hein has already contributed various papers on the issues raised by «financialization», that is, an economic system characterized by the rising role and importance of «financial motives, financial markets, financial actors and financial institutions in the operations of the domestic and international economies» (Epstein, 2005, 3).[1]

As it stands, the book has nonetheless the merit to throw light on recent advances in the treatment and understanding of monetary policy interventions and their effects upon functional income distribution, fixed capital accumulation, (involuntary) unemployment as well as on the relevance of elaborating on monetary analysis in order to enhance our theoretical and policy tools to deal with a number of contemporary issues in economics and economic systems.

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[1] See for instance Hein 2008a, 2008b, and Hein and van Treeck 2008.

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Samuel Hollander, *The Economics of Karl Marx. Analysis and Application*, New York, Cambridge University Press, 2008.

Professor Samuel Hollander's *The Economics of Karl Marx* (*ekm*) is the latest of his major studies to have appeared over the past quarter century. Hollander's mission has been to promote a new understanding of 'classical economics', essentially identified with his controversial version of Ricardo's economics, characterised by a keen appreciation of 'demand-supply' analysis; interdependence of pricing and distribution and the embrace of a non-Walrasian 'general equilibrium' approach in which the emphasis is on interdependent *processes* rather than simultaneous determination; and adherence to a 'land-based' model of endogenously determined, secularly falling real wages incorporating a 'shared incidence' principle whereby the effects of ongoing diminishing agricultural returns are 'shared' by labourers and capitalists in the form of lower real wages and a declining general rate of profit respectively. According to *ekm*, Marx also subscribed to some features of this «canonical» classical model – as Hollander denominates his own creation – but the message appears to be that Marx was a poor student, falling short of his 'classical' predecessors in key areas. If only Marx had not been imprisoned by his own 'Marxist' dogmas – notably, as it turns out on Hollander's story, the severely compromised 'surplus-value doctrine' – hit up there (or thereabouts) with Ricardo, Malthus and J. S. Mill.

There is no doubt that Hollander is prosecuting a controversial case, but it is not a wholly unexpected one. For example, his peculiar view of Marx as a (kind of) 'general equilibrium' theorist, his claim that *Capital* contains a model of secularly falling real wages under the 'Malthusian' pressure of excessive growth in the labouring population, and his (less controversial) formulation of Marx's 'tendential law' of falling profitability, all of which resurface in *ekm*, have appeared in publications dating back to the early 1980s. But it is only in *ekm*, particularly in its final chapter, that we see the full destructive impact of Hollander's endeavour. This is no sympathetic treatment of Marx's economics by any contorted stretch of the imagination: it is a frontal critique of core Marxian doctrine.

An indication of the content and structure of the book may be given as follows. The first chapter sets out Hollander's agenda in comparatively innocuous terms, with Tony Brewer (1995) actually receiving admonishment – a little disingenuously, perhaps – for having taken «too uncompromising a view of Marxian theory» (*ekm*, 2). Hollander claims that his own criticisms are limited to "what Marx might have been expected in his day and age to uncover and avoid" (*ibid*), thus signalling, for example, that he is not concerned with Bortkiewicz-Seton-Steedman type critiques of the transformation of values into prices of production (and of the falling-rate-of-profit argument) or, for that matter, with post-Marxian critiques and defences of Marx more generally. He also indicates that his coverage of Marx's work is incomplete, notably excluding – surprisingly so in a major study of Marx's economics – any serious discussion «on the nature of 'commodities', the 'substance' of value and the 'form' of value» (EKM, 7).

The book is then divided into five Parts of which the first, on «Principle [*sic*] features of the Marxian 'Canon'» in the three volumes of *Capital*, is the most substantial. It is here, by means of detailed textual exegesis and, on the falling rate of profit argument, mathematical reconstruction that Hollander seeks to establish his main propositions on the nature of Marx's 'mature' economic thought. These propositions include: the interdependence of pricing and distribution and the treatment of the wage rate and rate of surplus value as endogenous variables; a downward secular path for real wages under the pressure of excessive population expansion; an inconclusive argument for a falling rate of profit; a strategy for the transformation of values into prices of production which, at its best, incorporates 'demand-supply' analysis as the vehicle of determination, but encounters complexities with the allowance that all sectors do not participate in the formation of a general rate of profit; a worthy but flawed analysis of reproduction schemes in *Capital* II; and a failure to integrate the analyses of secular trends and cyclical variations.

The three following Parts, dealing successively with Marx's writings in the 1840s, the *Grundrisse* (1857-1858) and the *Economic Manuscripts* of 1861-1863 (incorporating *Theories of Surplus Value*) are a «study of the evolution of Marx's position on the topics discussed in Part I» (EKM, 7). Noteworthy are Hollander's observations that Marx, a committed exponent of a secularly falling real wage theory, strangely missed the opportunity to discuss the 'canonical' treatment of the same subject in 1849 and, in addition, «failed to do justice to the full-fledged 'canonical' classical model» (EKM, 251), or even mention secularly falling real wages, in the *Grundrisse* (or in *Theories of Surplus Value*, one might add). Of more grist to Hollander's mill – a matter of «high importance» to borrow a favourite Hollander trope – is a line from the *Grundrisse* in which Marx writes that «there really does enter here a moment of *value determination* which does not arise from the direct relation of labour to capital», but comes instead from the sphere of circulation (quoted in EKM, 271; italics in original). Although, as Hollander concedes, Marx «then recoils from this extraordinarily 'non-Marxian' conclusion» (*ibidem*), he need not have bothered: the murder was out.

Part Five, «Topics in Application», comprises three chapters with the headings «Economic Organisation and the Equality Issue», «Is there a Marxian 'Entrepreneur? On the Functions of the Industrial Capitalist» and «Principles of Social Reform». In the first chapter it emerges, *inter alia*, that Marx's «keen appreciation» of a range of «'neoclassical' themes» (*sic*) led him to reject «violent transition between economic systems and within systems ... in favour of caution» (EKM, 398, 408). In the second chapter, Hollander sets out to document «a range of 'productive' functions» that Marx attributed to the «individual capitalist», and the «heroic» (that is, failed) efforts that were to reconcile those insights with the «exploitation approach to profit» (EKM, 411). Then, in the third chapter, Hollander advances the arresting claim that «*increasing awareness on Marx's part ... of the potential for welfare reform within capitalist organisation*» points to Marx himself as «the 'first revisionist'» (EKM, 444; Hollander's italics).

And so, after a long and arduous journey, moments of 'high interest' notwithstanding, we arrive at the concluding chapter. Hollander announces at the outset: «Our primary concern is the surplus-value doctrine, for the theoretical core of Marx's enterprise must stand or fall with this kingpin of his system». Sadly for Marx, it is very much a case of falling, leading Hollander to the truly remarkable conjecture that Marx's *own* recognition of his failings could have been the reason «why *Capital* remained unfinished» (EKM, 463). But where had it all gone so badly wrong?

It transpires that Marx's purported adherence to 'classical' general equilibrium theory, with the allowance for an endogenously determined real wage and (therefore) rate of surplus value, was the major stumbling block. Marx's incorporation of variable real wages came «at a high cost indeed, for the contrast between 'necessary' and 'surplus' segments of the workday loses all sharpness», Hollander pronounces sternly (*EKM*, 465). In reaching that verdict, however, he is being almost charitable, for if the real wage (and rate of surplus value, or rate of exploitation) emerge only as equilibrium *solutions* in the Marxian 'general equilibrium' system, *along with* solutions to other relevant variables, it becomes difficult, if not positively incoherent, to describe *any* class of income as a 'surplus'.

But that is not all. Marx's attempt at transforming values into prices of production, much as it is vaunted by Hollander when (and only when) it involves 'supply-demand' analysis (with outputs adjusting so as to yield a general rate of profit and prices of production), is also identified by him as a culprit in undermining 'the surplus-value doctrine'. Because, as Hollander correctly records, Marx suggested that (part of) the surplus-value generated by labourers in agriculture, and in joint-stock companies (and, one may add, in all 'economic' monopolies) may not enter into the formation of a general rate of profit, this allegedly further undermines the «notion of surplus value» (*EKM*, 465, 470-471). So, too, does Marx's «concession» in the *Grundrisse* that (in short) value may be «determined» in circulation (*EKM*, 465-466), but this is as nothing compared with Marx's recognition of an 'entrepreneurial contribution', the implication of which is described apocalyptically as «the disintegration of the Marxian doctrine» (*EKM*, 466). No wonder that *Capital* remained unfinished; indeed, one may wonder why it was ever begun.

Other aspects of Marxian doctrine fare little better. The ('tendential') falling rate of profit argument «is not justified… the outcome being highly sensitive to differential rates of productivity increase in the agricultural and industrial sectors» (*EKM*, 478), while the secularly falling real wage analysis, which Hollander had earlier taken great pains to establish, is scuppered by Marx's «failure to justify a supposedly *necessary* expansion of labour supply at a faster rate than demand» (*ibidem*; Hollander's italics). Not only that there was also «too little said» by Marx – nothing, in fact – of «the interconnections» between the trend paths of profit and wage rates and, to make matters worse, if that were possible, Marx's «historical prediction» of falling real wages had turned out to be hopelessly wide of the mark (*EKM*, 479-480).

Marx thus emerges as a pathetic figure, tragically incapable of dealing with the criticisms he «might have been expected in his day and age to uncover and avoid». But how far is this doleful view justified? All depends on the validity of Hollander's interpretation and the inferences he draws from it, and here there is plenty of room for scepticism.

Take, first, the alleged 'general equilibrium' dimension to Marx's work, involving endogenously determined real wages and interdependence between pricing and distribution. If one were to search for a *single* example of such an analysis, in the entire corpus of Marx's extant writings, it would be to no avail. Indeed, Hollander admits that Marx did *not* investigate «effects on general wages… emanating from alterations in demand patterns» (*EKM*, 45) and that he *did* assume a given wage in his demonstration of the origin of surplus value (*EKM*, 48). But this conflicting evidence counts for nothing. Assumptions by Marx of a given and constant real wage were made only «for analytical convenience» (*EKM*, 12) and, in the case of «surplus-value doctrine», merely «to reveal to the unwary the true source of aggregate profits» (*EKM*, 48), so consigning Marx's *actual* practice to oblivion. Furthermore, Marx's system 'must' allow for interdependence between distribution and pricing, «even if he himself neglected to carry out the analysis». But why *must* it so allow? Hollander answers, because «[n]othing in the system precludes it» (*EKM*, 45): it is not enough that Marx 'neglected' to produce a 'general equilibrium' analysis; in order not to have one foisted upon him by Hollander he should have disavowed its future attribution explicitly.

Marx's 'general equilibrium' analysis is of the same provenance as Ricardo's. Both are the fictitious creations of Samuel Hollander, obtained by re-combining analytical pieces from diverse contexts, supplying missing links, and downplaying the significance of all conflicting evidence in favour of an approach that has no extant reality. Doubtless, there are criticisms that might be levelled against Marx's 'surplus-value doctrine', but the claim that it falls foul of *his* 'general equilibrium' analysis surely is not one of them.

It is also less than clear why Marx's 'transformation procedure' should be taken to undermine 'surplus-value doctrine' if, as Hollander purports to do, we consider the 'transformation' on Marx's own terms in feigned ignorance of all later critiques. One point that emerges repeatedly from Marx's discussion, in *Capital* and earlier writings, is that he *always* posits the sum of prices as equal to the sum of values, and the sum of profit as equal to aggregate surplus value, quite irrespective of any process for establishing prices of production (through output variations) or any allowance that some sectors may not participate in the formation in the general rate of profit: both features of the 'transformation' to which Hollander draws attention along with an assumption, mistakenly attributed to Marx, that the 'transformation' assumes a money commodity produced with an 'average' organic composition of capital. However, the problem created by the 'non-participating' sectors is not so much for 'surplus-value doctrine' *per se* as for the determination of the general rate of profit: profit is *always* surplus value, but we do not know how much of its given total mass is 'redistributed' to form a general rate of profit *unless* we know which sectors are 'excluded' (in whole or in part).

With 'realisation problems' Hollander is arguably on stronger ground to the extent that values and surplus value *can* vary independently of the labour expended in production, the real wage and the length of the working day. But one need rake through the *Grundrisse* for evidence of that 'problem'. Even in *Capital*, Marx allows that the magnitude of value, as realised in (and only in) exchange, may vary solely in consequence of changes in commodity supply relative to «social need» (a concept roughly equivalent to Adam Smith «effectual demand») (*Capital 1*, 269, fn. 24). Marx claims, however, that the analysis of the origin of surplus value must be based on the «exchange of equivalents» (entailing the assumption that supplies are equal to «social needs») in order «to prevent our observations from being interfered with by 'disturbing incidental circumstances'» (which would include variations in the real wage, as well as the exercise of exceptional 'entrepreneurial skill'). This, for him, was a matter of methodological principle, not something merely of «analytical convenience» as Hollander would have it, grounded in the belief that «circulation» (demand and supply) can account only for *oscillations* in value and surplus value but cannot account for them when «demand and supply coincide»; that is, when the «real inner laws of capitalist production … are realised in their pure form» (*Capital 3*, 290-291). Of course, it is always possible to dismiss Marx's entire methodological approach as «rather forced attempts» to avoid «non-Marxian» conclusions (*EKM*, 271, 290) but in Hollander's case this serves only to underscore his lack of sympathy with Marx's project, in striking contrast with the lashings of Marshallian 'generosity' to which he has subjected Ricardo, for example.

Other aspects of Hollander's interpretation are also deeply problematical. The iconoclastic portrayal of Marx as «the first revisionist» is based on a passage from *Cap-*

ital in which Marx applauded the 10 hours' Acts of 1850/1852 for bringing about «the physical and moral regeneration of the factory workers» (*Capital 1*, 408). However, Marx later noted that «Since 1866, when I wrote the above passages, a reaction has set in *once again*» (*Capital 1*, 411, fn. 52; italics added): a qualification reported by Hollander minus the italicised words, thus concealing the point that the gains of workers were seen by Marx as subject to *repeated* reversal. A more balanced appraisal of Marx's 'revisionist' stance on factory legislation would also take account of his assessment of the effects of its «general extension ... to all trades for the purpose of protecting the working class in mind and body», namely: «it matures the contradictions and antagonisms of the capitalist form [of production], and thereby ripens both the elements for forming a new society *and the forces tending towards the overthrow of the old one*» (*Capital 1*, 635; italics added). This is a strange type of 'revisionism'.

Also of dubious merit is the attribution to Marx of secularly declining real wages, dubbed by Hollander as «the 'law of immiserisation' under capitalism» (*EKM*, 85). Although Hollander can fairly point to supporting passages from Marx's writings in 1847-1849, and from *Value, Price and Profit* (1865), he cannot produce a single explicit statement of the doctrine from *Capital*. All that Marx commits himself to is the notion of a given real wage in a given society and «epoch», determined by «necessary» and «historical» factors, around which market wages are said to «oscillate» during the various (sub-) phases of the industrial cycle; he does *not* commit to the proposition, required by Hollander, that the given real wage declines over *successive* «epochs» in a developed capitalist economy.

Needless to say, Hollander views the matter of the «secular decline» rather differently, pointing to what he describes as «extensive textual evidence» in its favour (*EKM*, 86). To take a representative example of this species of «evidence», Hollander refers to Marx's «rapid survey of developments in the English cotton industry» (*Capital 1*, 584) in which «[d]eclining commodity wages is a central feature» (*EKM*, 89). Marx does allude to «deductions from wages» in this context, but rather than this being a reference to «a trend» (*EKM*, 89), Marx was referring *specifically* to developments in *a single year* (1863). Similarly, Marx's claim that there had been a «general reduction is wages», described by Hollander as the «net outcome of cyclical period of crisis and depression» was again a reference to wage-reductions in a *particular year* (1847). In reality, the 'secular trend' is completely absent from Marx's account. That it should ever have been cited as evidence" is symptomatic only of the weakness of Hollander's case, and of his creative interpretative technique.

There can be no doubt that Professor Hollander has produced yet another provocative study in which possibly the least controversial statement is the promise to «portray a rather different Marx than that typically presented to students» (*EKM*, 6-7). Some may find that portrait not so much different as, in many respects, completely unrecognisable: the product of an unsympathetic reading of Marx's work, further distorted by a misplaced concern with assimilating it to, and judging it by, a 'canonical' classical economics that never existed. Others may find the portrait a pleasing one, perhaps confirming them in their view of Marx as an overblown, second-rate 'classical' or 'post-Ricardian' economist. But, whoever may choose to read this book, they would be well advised to take nothing at face value.

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Christos P. Baloglou, *Wirtschaft und Technologie im antiken Griechenland*, transl. in German by P. Anastassiou, Marburg, Metropolis, 2008; Greek original edn. 2006.

In fact, this monograph offers a user-friendly reflection on some of the technology and economics underlying ancient Greek society. Contrary to what its title might suggest, it does so, not on the basis of archaeological or material evidence, but primarily through the lense of classical literary carriers of ideas. As such it should appeal, no doubt, to the readership of this journal, not in the least if once upon a time they painstakingly tried to make sense of the classics too, but were never allowed to take a glance at the technological wonders and economic insights they secretively contain.

The central part of this book, which successively highlights significant developments in agricultural, industrial and communications technology, is flanked by a few introductory and concluding chapters that explicitly seek to set them into their proper intellectual context. In chapters 2 and 3 coryphaei of the Greek epic, tragic, and philosophic scene are invoked to describe both mythical and rational attempts to explain the development of civilization. They are also expected to shed light on the harbingers of the economics of labour division, innovation and incentive theory – as documented pre-eminently in Plato's dialogues. At the same time, much weight is given to the high esteem in which authorities like Hesiod, Aischylos, or Xenophon allegedly held technology and its inventors. Chapter 8 seems to suggest that we should limit our prejudices about the supposedly negative view the Ancients took of progress and material prosperity to Hellenistic culture of the last three centuries BC, and its inner happiness movements of Cynicism and Stoicism in particular.

Yet, granted that the case for a positive assessment by the Greek elite of progress through technology and the improvement of material living conditions can truly be made, certainly there are more easy ways to defend it. Take the first pages of the first chapter (13-14), for instance. Here, Aristotle's *Mechanics* is quoted to demonstrate that authoritative sources were praising 'machinery' (the German text tendentiously translates the Greek 'mèchanè' as «Maschine») as that part of art par excellence which helps man in overcoming his natural needs. Now this is a quite unhappy quote to start from. Though the pseudo-Aristotelian *corpus* does include a highly interesting, yet quite plain treatise on the mathematical physics of movement indeed, its authorship is fiercely debated in classical scholarship, with some even pointing to a philosopher of the very Hellenistic period as its source of origin. Sophocles' praise of men and his creative inventiveness in *Antigone*, mentioned later on by the author, would have made a far less tricky starting point of eulogy, then…

To be sure, the author proves to be a man of wide reading, with an ability to convey his erudition and enthusiasm in an utmost pedagogical way. One will be quite amused, for example, by his stimulating analysis of the mythical labours of Hercules.

Ever thought about the technological prowess no less than physical power it took for him to re-route the Alpheios and Peneios rivers in order to wash out the Augean stables? Our hero's abilities to steer and to drain waters would be adopted on large scale afterwards from Lake Kopais to the Nile Valley. In addition, chapters 4 and 5 will learn you how the Greeks developed the appropriate legal and political tools for those projects to attract investors, operators, and finally boost the profits of their natural resources based economy. The finance structure for the reclamation of Lake Ptechae around 500 BC and the subsequent extension of arable land, for instance, somehow prefigures modern Build-Operate-Transfer (BOT) financing. A rich entrepeneur was granted tax exemption to import all construction materials, a restricted power to disposess people living in areas where a drain canal system had to be digged, a ten years operating licence, and he finally benefitted from special protection in case war would break out.

Similar inventiveness in both mechanical and legal technologies was needed in the mining industry, core business at Laureion, depicted in chapter 5. It provided Athens with the silver glistering foundational layers of its economic, military and cultural dominance. The great Themistocles is said to have demanded his citizens in 482 BC to lend back the 10 drachms the city would annually bestow on each of them from its massive mining proceeds, in order that a powerful fleet against the Persians be built. A rough 150 years later, the historian Xenophon would still insist on further striking new silver deposits (the Greek mentions 'kainotomiai', literally meaning 'new cuttings or delvings', rendered quite biasedly, it needs to be said, as «Innovationen» in German) as a principal source of new revenue for the city, given the seemingly inexhaustible nature if its riches (122). Typically, Athens strength turned out to be its major weakness as the Spartans occupied Deceleia during the Peloponesian wars and made the 20,000 strong slave workforce to flee the mines. Having no choice but to look for other financial resources, the Athenians took to the Parthenon and began melting its gold.

After all, slavery was the maint constituant of the labour factor of production in Antiquity – for the worse, as chapter 7 explains, since it quite possibly acted as a hampering substitute to technological development. For need, the Greeks had learnt, was the main driving force behind invention. Driven to the sea by the geographical constraints of their mountainous home lands, many a technology produced by them concerns survival in a watery environment. Chapter 6, which focusses on achievements in communications (alphabet) and transportation (shipbuilding) technology, documents some of them. Archimedes' screw used to remove the bilge water in ships is but one of the most famous examples. Excellent water engineering skills combined with awe for the Gods eventually led to the creation of automatic doors for a temple with the aid of water pumps. Yet this book sadly fails to deal with the fascinating subject of automatic machinery in Antiquity at all.

At least by the time it went to press, *Wirtschaft und Technologie im alten Griechenland* could boast itself on being part of a recent vogue in quality scholarship on the history of ancient technology. As such, those wishing to get down to the bottom of the matter might consider complementing their reading of Baloglou with two other monographs seeking to unravel the magic texture of economic and technological development in Antiquity now written by Serafina Cuomo and Helmuth Schneider respectively. Yet the pedagogically designed and praiseworthy contribution at hand might prove to be the one that ultimately drives you to embark on a journey to the Elysian Fields of ancient technology altogether.

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Giuseppe Fontana, *Money, Uncertainty and Time*, Abingdon (uk) and New York (usa), Routledge, 2009, pp. xviii+142.

This book elaborates on the PhD dissertation and on a number of ensuing papers that his author published on Keynesian monetary economics, with a focus on both the history of monetary thinking and current monetary analysis. The volume has three parts that fit quite well together, as the reader is taken into a scientific journey from Keynes and his self-declared followers to contemporary post-Keynesian debate on endogenous money.

Fontana introduces the reader to Keynesian economics (broadly defined) with three basic propositions that in his view distinguish this school of thought from other, more mainstream approaches to economics, namely, 1. the possible existence of involuntary unemployment, 2. the principle of effective demand, and 3. policy effectiveness. On these grounds, he then explains the changing nature of the Keynesian dissent over time, distinguishing New Keynesians from post-Keynesians with respect to their links with neoclassical economic thought. He thereby maintains that money, uncertainty, and time are three key elements of analysis that distinguish post-Keynesian economics from other schools of thought.

In the first part of the volume, Fontana considers the historical development of post-Keynesian economics, drawing some lessons from both its successes and failures. In so doing, the author proposes to distinguish, historically, the romantic age from the age of uncertainty: «The former describes the period of optimism and excitement of the 1960s and 1970s, when Post Keynesian economics was seen as a comprehensive theoretical system alternative to the dominant neoclassical paradigm» (p. 15). Fontana considers that a serious shortcoming of post-Keynesian economics during the romantic age was its almost exclusive concern with the theoretical structure of the neoclassical paradigm (p. 21). Indeed, in the early 1980s this romantic age came to an end, as post-Keynesians became increasingly aware of the importance of the methodological characteristics of their own approach to economics. This gave rise to the age of uncertainty in Fontana's words, during which post-Keynesian economists have been searching for an alternative methodology with respect to the neoclassical method, based on formalism and a series of assumptions whose number is usually inversely related to their degree of realism. In this regard, the gradual and progressive exclusion of post-Keynesian economists from leading journals and public policy advice can be explained by the lack of formalization in post-Keynesian analyses, as their authors have been questioning and investigating the proper method for analysis in economics (see, in this connection, the recent issue of *On the Horizon* introduced by Lee and Elsner 2008).

Advocating critical realism as the proper method of explanation for post-Keynesian economics, Fontana adopts the encompassing view, which «defends a positive outlook towards Neoclassical economics» (p. 26). According to the author, «Post Keynesian economists should aim to identify the limits to the domains of relevance of existing theoretical models. Second, they should plan on developing more general models that encompass the existing models in a synthesis with new models in order to extend the domains of relevance» (p. 26). This «should also facilitate and encourage investigations outside Post Keynesian economics, thus having the potential to help bridge the gap between different economic traditions» (pp. 26-27).

In this perspective, Fontana reviews Keynes's methodology and economic analysis in *The General Theory*, in order to explain the particular characteristics of Keynes's work, before contrasting it with the neoclassical theory and method. Indeed, and as the author explains in the second chapter of this volume, «the methodological differences between Keynes and the 'Classics' are relevant, at least in part, in explaining the unrestricted analytical licence of the Keynesian model» (p. 29). As the author recalls in this respect, Keynes (1937, 215) argued notably that «the object of analysis in economics, is non-homogeneous through time and, by extension, space, with the result that any model is historically and geographically determined» (p. 36).

In the second part of the book, Fontana applies Keynes's method and reasoning to two building blocks of post-Keynesian thinking, namely, probability and knowledge (ch. 3), and uncertainty and money (ch. 4). The purpose of the third chapter is to set forth the foundations of a modern theory of uncertainty (as opposed to risk in the neoclassical sense of a probability-distribution event). In that chapter, the author argues that probability relations in Keynes's sense «can be used to develop a general theory of knowledge, which includes the case of certain knowledge, probable (or risky) knowledge, and uncertain knowledge» (p. 47). On this account, Keynes's notion of the weight of argument may play an important role, together with probability relations, in a modern theory of decision-making under uncertainty. Chapter 4 expands on uncertainty and money, showing that, contrary to firmly-held beliefs within post-Keynesian circles, there is «an intimate relationship between Keynes's three major books»,[1] and therefore between three major strands in post-Keynesian economics, which Fontana categorizes as «New Fundamentalist Keynesians», «Monetary Circuit theorists», and «Non-ergodic/Monetary Post Keynesians» respectively (p. 59). In particular, Fontana argues that money is both a final means of payment and a liquid store of wealth: in the former sense, on which the theory of the monetary circuit is based, money's existence would depend on uncertainty in the production–consumption process, whilst in the latter sense money is a «temporary abode of purchasing power» (Friedman 1974, 9) that would explain involuntary unemployment: people hold on to money balances because this lulls their worries with respect to the uncertain course of events in the future.

In the last part of this book, Fontana goes deeper into economic analysis, providing a novel and original reading of Hicks's writings, with the aim of throwing new light on a number of issues discussed in previous chapters. The author shows in particular that the circulation of money as a final means of payment and the holding of it as a liquid store of wealth can be encompassed in a continuation theory in Hicks's 1956 sense, thus in fact replacing single period analysis of the money supply process with a complex flow and stock analysis of money emission, circulation, and holding in agents' portfolios. In an attempt at cross-fertilization, Fontana then goes on building a bridge between the so-called horizontalist view and the structuralist view of money endogeneity, showing their complementary nature and roles within a more general theory of endogenous money, as the author sketches out in the last chapter of this volume, with some cruciform diagrams and referring to single period versus continuation analysis.

On the whole, this book provides new material to think about old issues in monetary economics, such as the nature, role and workings of money and credit, the relationships between central and commercial banks, and the importance of uncertainty and effective demand in generating involuntary unemployment in capitalist systems of production. It may represent a promising starting point of a long awaited discussion about conceptual thinking and the proper role of logic and causality in economics.

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Andrew Caplin and Andrew Schotter (eds), *The Foundations of Positive and Normative Economics: A Handbook*, Oxford, Oxford University Press, 2008, pp. xxii+382.

This volume is the first book in the new «Handbooks in Economic Methodology» series published by Oxford University Press. The reader familiar with recent handbooks in economic methodology may be surprised by both the topics discussed and by those doing the discussing. There is very little of the argumentation that one usually finds in economic methodology – applications of contemporary philosophy of science; closely argued analysis involving concepts such as 'scientific explanation', 'empirical verification', or 'causality'; debates about the realist versus instrumentalist view of scientific theories, and so forth. The book has fifteen chapters and only one by someone who has made a serious contribution to the literature on economic methodology (Daniel Hausman); there are no references to anything published in *The Journal of Economic Methodology* or any journal in the history of economic thought, and there are only two references to work published in *Economics and Philosophy*. Not your typical handbook on economic methodology indeed.

Despite the absence of a connection to the existing methodological literature, and despite the fact that I believe almost all of the contributors could have benefited from the extensive methodological literature on rational choice and decision theory, I do believe the book is a very important contribution to the methodological literature. There is currently a major debate over whether criticisms of rational choice theory coming from recent behavioral economics and related fields will, or should, change the way economists model individual behavior, and this volume goes right to the heart of that debate. Unlike most handbooks, it does not cover a wide range of different methodological topics. In fact, the entire book discusses only one topic: Faruk Gul and Wolfgang Pesendorfer's methodological criticism of what they call «neuroeconomics». Chapter one is a clear, if polemical, statement of their position – «The Case for Mindless Economics» – and the remaining fourteen chapters are various commentaries on their paper, primarily by economists working in the relevant areas of applied economics. Most, but not all, of the authors are critical of Gul and Pesendorfer's position, although not always for the same reasons. Given the attention that Gul and Pesendorfer have received, and the timeliness of the questions raised, the volume provides an excellent entry point into this important methodological debate.

Gul and Pesendorfer defend traditional rational choice theory – what they call «standard economics» – against a host of recent criticisms coming from fields like experimental economics, behavioral economics, and neuroeconomics. These criticisms are well-known: agents in laboratory environments and certain real-world situations frequently violate the axioms of expected (and risk-free) utility theory by exhibiting endowment effects, framing, preference reversals, social preferences, irreversibilities, status quo bias, and such. Gul and Pesendorfer call all of this literature «neuroeconomics» even though neuroeconomics is actually only a small part of the relevant literature and many of those doing neuroeconomic research see it as a way of defending, rather than criticizing, standard microeconomic theory.

Gul and Pesendorfer defend both the positive (rational choice) and normative (welfare) parts of standard economic theory. On the positive side, they argue that none of the psychological evidence that has been used to criticize rational choice theory is effective – it is basically irrelevant because it has nothing to do with the subject matter of economics. On the normative side, they focus on the recent effort by Daniel Kahneman and others to revive hedonistic welfare economics – to use experienced (not decision) utility as the standard for welfare judgments – and the paternalistic policy recommendations associated with such neo-hedonism.

There are number of different aspects to Gul and Pesendorfer's defense of positive rational choice theory, but the main theme is that new discoveries about the brain states or the feelings of economic agents are irrelevant because economics says nothing about the causes of human behavior: «Neuroscience evidence cannot refute economic models because the latter make no assumptions and draw no conclusions about the psychology of the brain» (p. 4). Standard economics (everyone agrees) uses utility maximization under constraint to predict and explain economic behavior, but according to Gul and Pesendorfer this does not commit economists to any position about the underlying causal process 'behind' economic choice. They argue that revealed preference (*i.e.* 'choice') data are the only evidence available to economists and that standard economics merely uses formal models to rationalize such data and to predict various out of sample empirical relationships – and as such economists have no evidence for, nor any need to posit or make any assumptions about, the «psychological processes underlying decision making» (p. 9). Standard economic analysis employs constrained utility maximization, but utility maximization is not offered as (nor is anything else offered as) the underlying cause of economic behavior. Choice is the only concern and «'utility maximization' and 'choice' are synonymous» (p. 7). No new evidence about the underlying psychological or physiological processes behind human choice has anything to do with economics since economics is not concerned with such underlying processes. Gul and Pesendorfer can thus conclude «that the neuroeconomic critique fails to refute any particular (standard) economic model and offers no challenge to standard economic methodology» (p. 7).

The style of Gul and Pesendorfer's argument changes a bit when they turn from positive economic science to normative welfare economics. In the normative case the argument is less about the failures of the neuroeconomic critique and more about the difficulties associated with one particular version of behavioral welfare economics. The target is the proposal by Kahneman and others endorsing the use of hedonistic experienced utility as the basis for welfare analysis and public policy. The neo-hedonist argument has two main parts: people *do not do* what really makes them happy in terms of experienced utility, but social welfare *should* be based on such happiness. This view often leads to paternalistic policies because if people do not do that which gives them the most experienced utility / happiness, and yet they should do so, the door is opened for government to intervene in paternalistic ways to make people do what they should do (*i.e.* maximize their own experienced utility). Gul and Pesendorfer offer a number of different criticisms of such a neo-hedonist approach to welfare economics. First of all, following in the footsteps of their arguments against the critique of positive economics, economics does not, and cannot, say anything about happiness, brain states, or experienced utility. Since all economists know about what is good for people is what they choose, then what people choose – as opposed to the feelings they might have when choosing or consuming – is the sole basis for welfare economics. Second, unlike standard welfare economics, the argument implies that the goal of welfare analysis is therapeutic – «the neuroeconomic view of welfare analysis builds on an inappropriate analogy between the economist and therapist» (p. 26) – and economics is not, and should not be, therapy. And finally, the argument that welfare should be based on happiness is simply a philosophical position that has no basis in legitimate social science (and standard welfare economics does not require such philosophical commitment).

Given the number of chapters and the wide variation in the responses of various authors, it is impossible to summarize the arguments made either for, or against, Gul and Pesendorfer's position in this short review. I suggest that anyone interested in this topic – and given the importance and timeliness of the debate that should be a large audience – should simply read the book. There is a lot of variation in the responses, and no doubt most readers will be repeatedly whiplashed between admiration and revulsion as they work though the different arguments, but all in all it is a very worthwhile experience. As I noted above, I think it would have been a more worthwhile experience for everyone involved, including most of the authors, if they had spent some time with the existing methodological literature, but it is still a valuable experience which I heartily recommend.

As an alternative to summarizing all of the various chapters I will just mention two papers I think made particularly important points, and then close with some general comments about Gul and Pesendorfer's overall approach. The two papers I would like to note are chapter two (Colin Camerer's «The Case for Mindful Economics») and chapter six (Daniel Hausman's «Mindless and Mindful Economics: A Methodological Evaluation»). Given Camerer's extensive contribution to the «neuroeconomics» literature, one would expect him to be critical of Gul and Pesendorfer's position – and he is – but the criticism is fairly reserved. One of his main points is that «neuroeconomics» just involves looking inside the black-box for consumer / utility theory in the same way that economists have already done for the theory of the firm: «Neuroeconomics proposes to do the same by treating an individual economic agent like a firm… The neuroeconomic theory of the individual replaces the (perennially useful) fiction of a utility-maximizing individual that has a single goal, with a more detailed account of how components of the individual-brain regions, cognitive control, and neural circuits – interact and communicate to determine individual behavior» (p. 46). Camerer also criticizes Gul and Pesendorfer's rhetorical style – «their summaries are either overgeneralized or wrong in almost every sentence» (p. 63) – but he does so in an appendix which seems like a reasonable way to address the issue.

Although Camerer provides a reasonable defense of the new practices that Gul and Pesendorfer oppose, he never takes on the important issue of their particular (I would also say peculiar) interpretation of standard economics – in fact almost no one addresses this issue with any seriousness other than Hausman (perhaps confirming that one should send a methodologist to do a methodologist's job). Hausman offers a detailed analysis of Gul and Pesendorfer's «relevance thesis»: that only «data concerning choices and the consequences of choices … are relevant to the acceptance or rejection of economic models» (p. 127). He examines the three (somewhat intertwined) arguments they offer for the relevance thesis – the Samuelson argument, the revealed preference argument, and the Friedman argument – and explains the difficulties with each. He also provides a very useful service unpacking the variety of different views that travel under the label of «revealed preference» and clarifying the various problems with each. Finally, he argues that even though what Gul and Pesendorfer say about welfare economics and neo-hedonism is way «off the mark», there is in fact «a good deal to criticize» in «the account of well-being defended by some psychologists and behavioral economists» (p. 143). He makes it clear that experienced utility may be a poor resource for making welfare judgments and underwriting social policy, even though Gul and Pesendorfer fail to identify the problem.

My final remark about the volume concerns the general style of the Gul and Pesendorfer argument. Even though I think their paper is deeply flawed – not only as a methodological argument, but also with respect to the various empirical claims they make about standard theory and «neuroeconomics» – I think it is rhetorically quite masterful. Just as methodologists notoriously had a difficult time trying to explain the popularity of Milton Friedman's famous 1953 essay on economic methodology in terms of the purely 'methodological' case it made, so too for Gul and Pesendorfer. In the Friedman case, economists at the time were hungry for a professionally-persuasive explanation of why it was okay to continue doing what they had been doing and ignore all the criticisms of the 'unrealism' of the discipline's assumptions. The situation seems to be similar for the recent criticisms based on behavioral economics and related research. What many economists would like to have today is a professionally-persuasive explanation of why standard economics is just fine and the profession does not need to change its practice in response to all of the recent psychological criticism. Gul and Pesendorfer are very effective in this respect. First, note the use of the term «neuroeconomics» for all of the critical literature. The term «neuroeconomics» conjures up alien images of fMRI machines and people strapped to laboratory tables. Actually of course, much of the negative empirical evidence that has challenged rational choice theory comes from experimental economics, a field that is now well-established, not alien at all, and often supports standard economic theory. By using the term «neuroeconomics» Gul and Pesendorfer are effective in delivering the message that the threat is alien, coming from the outside by those that simply do not understand standard economics. So too with their emphasis on normative economics. Most economists have no desire to return to hedonism and most also question both the philosophical arguments for, and our high-tech ability to measure, happiness; most economists are also very wary of any form of governmental paternalism. So instead of presenting the opposition as challenging the empirical track record or scientific credentials of rational choice theory, Gul and Pesendorfer present «neuroeconomics» as primarily a normative doctrine narrowly focused on reviving hedonism and providing a justification for increased government intervention. If Gul and Pesendorfer had been more accurate and characterized the recent critical literature as primarily focusing on the (positive, empirical, scientific) question of whether rational choice theory is the best available theory for predicting and explaining individual economic behavior, the argument would not have been nearly as effective. But they didn't. They made it an attack from outside involving alien machines, feelings, therapy, and the visible hand of an intervening government;

and as a result, it is a methodological debate that has received, and will continue to receive, a lot of attention by members of the economics profession.

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Sraffa or An Alternative Economics, ed. by Guglielmo Chiodi and Leonardo Ditta, Houndmills, Basingstoke and New York, Palgrave Macmillan, 2008, pp. x+300.

As the two editors Guglielmo Chiodi and Leonardo Ditta point out in their «Introduction», this book offers a selection of papers presented at the International Conference *Sraffa or an Alternative Economics* held in Rome in December 2003, to commemorate the twenty years elapsed from Piero Sraffa's death. The selected papers, extensively revised by their authors before the present publication, offer readers with a hint of the new lines of research pursued within the Sraffian school of thought. The editors' declared hope is that a consistent and fully-fledged reasearch program, alternative to mainstream economics, may be built on such bases.

The book is organized into four Parts. In Part i, Reactions, Bellino critically examines the main reviews of *Production of Commodities by Means of Commodities*; Part ii, Economics, is the most wide-ranging and consists of eight papers by, respectively, Afriat («Sraffa's Prices»), Bellofiore («Sraffa after Marx: An Open Issue»), Cesaratto («The Classical Surplus Approach and the Theory of the Welfare State and Public Pensions»), Nisticò («Sraffa 1926 and Sraffa 1960: An Attempt to Bridge the Gap»), Schefold («Saving, Investment and Capital in a System of General Intertemporal Equilibrium») followed by a comment by Parrinello, Chiodi («Beyond Capitalism: Sraffa's Economic Theory») and Ditta («Notes on Early Development Economics' Story and its Relation to Sraffa's Contribution»); Part iii, Philosophy, includes two papers by, respectively, Edwards («Sraffa: Notes on Moralizing, Money and Economic Prudence») and McGuinness («What Wittgenstein Owed to Sraffa»); finally, Part Four, Mathematics, is made of four papers by, respectively, Maroscia («Some Mathematical Remarks on Sraffa's Chapter I), Lippi (Some Observations on Sraffa and Mathematical Proofs with an Appendix on Sraffa's Convergence Algorithm), followed by a comment by Salvadori, Kurz and Salvadori («On the Collaboration between Sraffa and Besicovitch: The 'Proof of Gradient'») and Velupillai («Sraffa's Economics in Non-Classical Mathematical Modes»).

The selected contributions provide an ample coverage of the various analytical threads that make up the contemporary debate among those economists who, in various degree, take their lead from the work of Piero Sraffa and his interpretation of classical economics. From this point of view, the book is particularly suitable for those readers who have grown inside different theoretical traditions and intend to acquire at least a bird's eye view both to the main results achieved and the open issues still under discussion within the Sraffian school. The papers here published are so diversified for their content and style to be able to arouse the interest both of the mathematically minded readers and of readers more inclined towards the history of economic thought and methodology. Moreover, it has to be underlined that some papers (those by Bellofiore, McGuinness and Kurz and Salvadori) make extensive use of Sraffa's and Wittgenstein's unpublished manuscripts. In the absence of an integral critical edition of this unpublished material, such papers are highly welcome since help readers to extend their knowledge of Sraffa's thought beyond the scope of his published works. Readers may thus be able to confirm or revise their opinions in the light of the fresh material there available. In this perspective, it is a kind of a lost occasion that more space and emphasis has not been devoted to such important source of inspiration for Sraffian scholarship.

Notwithstanding the heterogeneity of the contributions, it is however possible to single out a *fil rouge* that runs through the book. As is well-known, in the years following the publication of *Production of Commodities*, scholars' attention has been mainly focused on the elements of criticism and dissatisfaction towards mainstream economic theory which Sraffa's 1960 book contained *in nuce*. Considered in retrospective, one of the most disappointing results of the famous controversy on the theory of capital among the two Cambridge has been the spread of the opinion according to which 1. Sraffian economists have basically devoted their intellectual energies to develop the *pars destruens* implicit in *Production of Commodities* but have largely neglected to do the same job on the *pars construens*, 2. Sraffa's critique of mainstream economics concerns uniquely abstract matters of logical consistency and 3. Sraffa's critique applies solely to the simplified versions of mainstream economics while leaving unscathed its most refined versions, such as the intertemporal general equilibrium models (to the above considerations, one may add that for many outstanding neoclassical economists *Production of Commodities* amounts to little more than an interesting particular case, that of constant returns to scale and some variant of the non-substitution theorem). Probably for these and similar reasons the average economist's interest in Sraffian economics has progressively declined. Given the above situation, the crucial intellectual challenge facing Sraffian economists is to show that it is possible to widen the scope of their analysis beyond the traditional (and by now largely explored) borders. From this point of view, the contributions by Nisticò and Cesaratto deserve particular mention since they are aimed at analysing from a Sraffian perspective the themes of imperfect competition and welfare state. What is to be underlined is the fact that not all the theoretical tools forged by Sraffa in his 1960 book can actually be employed to explore such new path. As an example, Nisticò's analysis of imperfect competition marks a conscious departure from the established analytical tradition of long-period method by dropping the standard assumptions of uniform rates of wages and profits and of given sectoral outputs. It is hardly an exaggeration to say that such kind of creative work, combined with a thorough and open-minded analysis of Sraffa's manuscripts, is the most promising way to give new life to Sraffian scholarship.

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N. Emrah Aydinonat, *The Invisible Hand in Economics. How Economists Explain Unintended Social Consequences*, Abingdon and New York, Routledge, 2008, pp. xvi+254.

As the subtitle «how economists explain…» indicates, cf. the subtitle of Mark Blaug's well-known book on economic methodology, this monograph does not intend to write a history on the invisible hand, but attempts to explicate what kind of explanation the invisible hand concept provides. For this purpose, however, Aydinonat discusses famous historical examples of invisible-hand explanations such as Adam Smith's invisible hand, Carl Menger's account of the origin of money and Thomas Schelling's checkerboard model of residential segregation. This book shows that it is not impossible that philosophers may have something sensible to say to historians. The latter is particularly the case when Aydinonat discusses the invisible hand in the works of Adam Smith.

The book is about «'unintended consequences of human action' and the mechanisms that bring about these consequences» (2). The invisible hand is defined in this framework as a specific subset of the set of possible types of unintended consequences, namely the social unintended consequences that were brought about by individuals who were intending to bring about consequences at the individual level. Aydinonat discusses in subsequent chapters the strengths and weaknesses of invisible-hand explanations concerning the emergence of social consequences from a philosophical perspective. Most of interest for historians is his discussion of the invisible hand in the works of Adam Smith.

Among historians it is commonly known that, although Smith's invisible hand is an influential metaphor in economics, he used the phrase only three times and in different contexts. The first time in *The Wealth of Nations* (1789, IV.2.9):

> By preferring the support of domestic to that of foreign industry, [the merchant] intends only his own security; and by directing that industry in such a manner as its produce may be of the greatest value, he intends only his own gain, and he is in this, as in many other cases, led by an invisible hand to promote an end which was no part of his intention. Nor is it always the worse for the society that it was no part of it. By pursuing his own interest he frequently promotes that of the society more effectually than when he really intends to promote it.
>
> (quoted in Aydinonat, 70)

The second time in *The Theory of Moral Sentiments* (1790, IV.I.10):

> The rich only select from the heap what is most precious and agreeable. They consume little more than the poor, and in spite of their natural selfishness and rapacity, thought they mean only their own conveniency, though the sole end which they propose from the labours of all the thousands whom they employ, be the gratification of their own vain and insatiable desires, they divide with the poor the produce of all their improvements. They are led by an invisible hand to make nearly the same distribution of the necessaries of life, which would have been made, had earth been divided into equal portions among all its inhabitants, and thus without intending it, without knowing it, advance the interest of the society, and afford means to the multiplication of the species.
>
> (quoted in Aydinanot, 70)

And the third time in an essay on *The principles which lead and direct philosophical enquiries* (1795, 49):

> Fire burns, and water refreshes; heavy bodies descent. And lighter substances fly upwards, by the necessity of their own nature; nor was the invisible had of Jupiter ever apprehended to be employed in those matters. But the thunder and lightening, storms and sunshine, those more irregular events, were ascribed to his favour, or his anger.
>
> (quoted in Aydinanot, 69)

The fact that Smith used the metaphor in three different contexts allows for various interpretations of what he meant by it, and among historians there is no consensus about these interpretations. Aydinonat's interesting contribution to this discussion is his interpretation of the invisible hand in the third, 1795, context which provides him the background in which the other two uses are examined. His target is to disprove Emma Rothschild's (2001) view that the concept of the invisible hand is not consistent with Smith's system of thought, while he nevertheless agrees with her suggestion that Smith's use of the invisible hand is somewhat ironic.

To be more precise, Aydinonat disputes three of Rothschild's propositions. The first proposition is that the word «invisible» implies «blindness». The second is that the invisible hand presupposes the existence of a philosopher who sees more than any ordinary individual can. The third is the suggestion that Smith's proposal that merchants should not seek their individual interests by political means is conflicting with the idea that they would promote the public good by pursuing their self-interests. According to Rothschild, all three propositions are un-Smithian, on which Aydinonat agrees.

To develop his arguments against Rothschild, he uses the third 1795 quotation of the invisible hand to show that, according to Smith, it is the task of philosophy to explicate the apparently invisible chains of nature and society. The next step is then to show that the individual is only blind with respect to the interests of others and blind for the social consequences of these interests but nevertheless the best judge of their own interests. So pursuing self-interest by political means, that is, intentions targeted at the social level, is an entirely different matter from pursuing self-interests at the individual level, and thus there is no conflict between not seeking individual interests by political means and unintended promotion of public good by pursuing self-interest. But if no individual knows better than the other what is good for society, how can the philosopher, Smith in particular, know better? Smith, however, sees philosophers as products of division of labour. They are not naturally better acquainted than other for inquiring the invisible chains of nature and society, but because of specialisation they can do better than the ordinary individual. Moreover, 'unintended' does not necessarily mean 'unanticipated'. It is possible to have anticipated but unintended consequences. The absence of foresight and awareness of the social consequence is not a necessary condition for invisible-hand type of explanation. It is possible that a philosopher foresees the unintended consequences but fail to act accordingly to change these consequences.

The other 'paradigmatic example' of an invisible-hand explanation of interest to historians is Menger's explanation of the origin of money. This chapter, however, is much more a philosophical assessment of his account of whether money evolved spontaneously, by design or from social relations other than market exchange. This philosophical assessment is done by a rational reconstruction of Menger's theory using the framework of unintended social consequences as developed in this book. It, nevertheless, shows that "contrary to common belief", Menger takes institutional factors into account in his explanation of the origin of money and he does not undervalue historical research: both historical and theoretical research are considered to be necessary for a complete understanding of the emergence of money, though he does not take historical facts into account.

The third example, Schelling's checkerboard model, is one of the key examples appearing in current philosophical discussions on the representativeness of models. For that reason, there is also a nice surveying chapter on «Models and representation». But these chapters are of a much more indirect concern to historians of economics.

This monograph is a very nice proof of scholarship in the field of economic methodology. In the first part it systematically develops an account of invisible hand explanations in a persuasive and well argued way and equipped with this framework it studies in the second part various theories including current game theory. The philosopher of science, Imre Lakatos (1976, 2), in his book on the history and philosophy of mathematics to clarify his approach paraphrased Kant: «the history of mathematics, lacking the guidance of philosophy, has become *blind*, while the philosophy of mathematics, turning its back on the most intriguing phenomena in the history of mathematics, has become *empty*». This monograph is written by a philosopher who did not turned his back to the intriguing concept of the invisible hand and how it is used in economics. The historian can see how a well-defined philosophical concept can give a new perspective on historical debates.

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Tim Congdon, *Keynes, the Keynesians and Monetarism*, Cheltenham (UK), Edward Elgar, 2007, pp. 000.

Keynes, the Keynesians and Monetarism is an intriguing miscellaneous of essays by one of Britain's leading monetarist economist in the 1980s and in the 1990s. The book indeed brings together the main academic papers written by the author revising and up-to-dating the previous collection titled, *Reflections on Monetarism*, with the new papers published in the first years of 2000. The book by this 'advocate' of monetarism is very often appealing and provocative, covering topics that are fundamental to macroeconomic thinking and policy-making.

This contribution has, without doubt, the merit to put under the scrutiny *how* the long debates between Keynesians and Monetarists *seem to have ended,* because of the many continuing tensions and uncertainties that has characterized this two school of economic thought. This point is particularly important since I agree with the author that the Keynesian-Monetarist querrel is something like a «not extinct volcano», both Keynesianism and Monetarism are indeed still evolving. This is even particularly true today if one consider the current period of *financial turmoil* and *crisis* that certainly will promote new 'eruptions' of this volcano.

An other relevant and original topic by Tim Congdon's book is the distinction between *British* monetarism from *American* monetarism. This is argumented in *chapter 7* where the author argued that monetarists in the UK had inevitably to devote more critical attention to income policies than their counterparts in the USA. Futhermore, as the reader will see in *chapter 7*, comparing the two types of monetarisms, Tim Congdon argued that the first disagrements between them laid in the emphasis placed on broad and narrow money and in the implementation of monetary control policies.

Nevertheless, while the analysis that contrasts keynesianism with the two different kind of monetarisms, quoted above, is exaustive and contributed to enlighting ideas on the argument, all the sections of the book dedicated to Keynes and to Keynesians are, in my opinion, incomplete, not always clearly written and not well argumented.

For example, the author banished and relegated to a footnote (no. 33, «Introduction») the relevant contributions by the *Post-Keynesians* economists despite the importance that this school of thought still attribute to money and financial markets to the understanding of macroeconomic outcomes. Both the UK and the USA *post-Keynesians* are indeed very seldom quoted in the book. This is a serious defect particularly in last section (part SIX) dedicated to *Money, asset prices and economic activity*.

The *quasi-irrelevance*, that the author attributed to the *liquidity-trap argument*, is another great sin of the analysis dedicated to the aspects and to the debates of the monetary trasmission mechanism on the real economy. Furthermore the author do not emphasized that the restatement of the quantity theory of money by Milton Friedman is a *special case* of Keynes's own analysis since in Friedman's approach there is no speculative demand for money and no fundamental uncertainty.

Concerning with the so-called *new Keynesians*, the author do not make distinction *between* and *within* them. All *new Keynesians* are indeed put under the provocative label of «output gap monetarists», since in his opinion they are very far from the *Old Keynesians*. The former indeed concentrate their analysis on the importance of monetary policy to favour *inflation target regime*. But as wellknown, *new Keynesians* are very different each others. On one hand, some of them have showed and microfound the relevance of wages and prices rigidities (see for example the works by Mankiw and Romer) to make sense of *money non-neutrality* and *involuntary unemployment*. On the other hand, others new Keynesians (see the contributions by Stiglitz, Greenwald and Weiss) outlined the relevance of imperfect and asymmetric information in labor and capital markets opening the way to credit rationing and coordination failures. The latter also contend that more *flexibility* in wages and prices can always play a *stabilizing* role.

The book by Tim Congdon is certainly appealing for macroeconomists and researchers, but the main and more interesting topics are concentrated, in my opinion, on the evolution and on the differences between different kind of monetarisms (*i.e. British monetarism vs American monetarism*) rather than on Keynes and on the so-called *Keynesians*.

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Mark I. Choate, *Emigrant Nation. The Making of Italy Abroad*, Cambridge (ma), Harvard University Press, 2008, pp. 320.

Mark Choate's book investigates Italian emigration between the end of xix century and World War i from many points of view: cultural, political, social and economic. From the unification of Italy to the end of the first world war more than 15 millions left Italy for Europe, America and Oceania, so reshaping tue situations of both Italy and host countries. Emigration very soon became a sort of testing-ground for post-unification Italy, especially after the defeat of Adwa and the following sudden end to Italian expansion policy in Africa. Emigration not only reduced Malthusian pressure in the homeland, but it also fostered Italian economic development trough remittances during the whole *Belle époque*. Assuring protection for Italian emigrants became a major political priority, in which national pride was widely involved. In the same way emigration was a spur for outlining the concept of «italianità», a central problem for a land which had been divided in different sovereign States for centuries, a problem which is to this day largely unresolved. These facts explain not only the conflicting Italian mood about emigration, swinging from pride to humiliation but even the strong opposition of Italian nationalists to mass emigration, seen as the proof of Italy inferiority compared to other European powers.

Choate stresses the exceptional nature of Italian emigration, especially if compared to other countries' experiences, but also the similarity with Japanese emigration during the Meiji period. Once hopes for colonial expansion vanished, mass emigration gave Italy the chance to build a long term network of useful, profitable relations: «Exerting influence through the "informal empire" of Italians settled peaceably abroad was more humble than the British use of naval power, military force, and racial domination, but also proved more enduring and more beneficial to all involved. Italy as an "emigrant nation" used creative and innovative means to enter the global economy before the Great War. The Italian "ethnographic empire" was based on free travel, open culture, and memory as the state worked to build social communities abroad and link them culturally and economically to the fatherland. Rather than using force, the Italian state provided emigrants with every incentive for loyalty. Free settlements beyond imperial territories laid the basis for a transnational, global network of economic, cultural, and population exchange, with Italy at its center» (p. 232).

The book does not show innovative, mould-breaking analysis but it offers anyway a good overall view of Italian mass emigration for readers who don't speak Italian language. The reviewer must confess his incomprehension of why the author calls Banco di Napoli, one of the most important Italian banks, a «non profit institution». The seven finale figures have no indication of their sources.

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