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BOOK REVIEWS

Duncan K. Foley, *Unholy Trinity: Labor, Capital, and Land in the New Economy,* The Graz Schumpeter Lectures, London and New York, Routledge, 2003, xi+98.

Duncan Foley's Schumpeter Lectures opens by providing the reader with a promissory note for a spirited defense of Classical political economics against their erstwhile heirs. Here it is:

> My theme in this book is the capacity of the methods of the Classical political economists, Adam Smith, Thomas (*sic*) Malthus, David Ricardo, and their critic, Karl Marx, to reveal the self-organizing character of the capitalist economy regarded as a complex, adaptive, non-equilibrium system (1).

Immediately, the reader's objection is anticipated and answered:

> From one point of view this is an exercise in anachronism, since the language of complex systems theory and its application to economic problems is only about forty years, and is implausible to claim that Smith or Ricardo or Marx thought about the problems of the economy using the conceptual tools of complexity system. ... the language and vision of the Classical political economists incorporates many isnights of contemporary complex systems theory. .... [and] that complexity theory sheds some light on the extraordinary effectiveness of the Classical political economists' method and the depth of their analytical results.
>
> (1)

What Foley finds most attractive to Classical political economy is their vision of a robust self-organizing system:

> From the Classical point of view, competition need not be "perfect" in order to bring about the tendency to self-organization. The self-organizing of complex systems is *robust* in the sense that it does not depend upon any particular detail of the evolution of the system, and will reassert itself even when some of the mechanisms supporting it are frustrated.
>
> (4)

After another twenty-five pages of methodological calls to recover our inner Classical economist, this reader, who was brought up with the wisdom that one does not get rich betting against Adam Smith, was as Huck Finn might say, in all in a sweat to see how the technical details match with the history of economics. Then I read:

The critical reader ... may see a gap ... It would be more methodologically consistent if the examples explicitly employed a simulation methodology showing that

> disaggregated agent-based models can exhibit the self-organizing tendencies of the capitalist economy addressed. In fact, the analysis in these chapters are based on differential equations linking aggregate, or macrospic, variables and study the equilibria of these systems of equations and their stability.
>
> (30)

So, the announced goal of the lectures – to tackle these problems with techniques from complexity theory – are left an exercise to the reader!

> I can only beg the reader's indulgence to fill this methodological gap. But I think a strong case can be made that agent-based disaggregated models of capitalist competition, innovation and population change can be constructed that will exhibit the self-organizing tendencies I point to. I invite interested readers to pursue this positive research program...
>
> (30)

This suggests that we have to think about two books – a book to be written and the book as is was written. I'll say something about each of these books. On the book to be written, I shall focus my efforts on Foley's concern that it would «an exercise in anachronism». The reader will not perhaps be surprised that in this I shall quote a good deal from the book *Spencer & the Economists* by Sandra Peart and myself. This unwritten work sits on my shelf right next to Foley's. Peart-Levy 2005a, b are our promissory notes. On the book which was written, I'll comment a technical econometric issue in the relationship of economics and demographics considered in the fourth chapter.

As preface for the book to be written, Foley traces the linkage of system thinking from economics following the «Malthus to Darwin» pathway. To write this book, I think it uncontroversial that one needs to model self-aware agents. Self-aware agents have theories of their own which may or may not be the same as that of the systematic philosopher who writes about them. Here it is seems to me that the Darwinian move is simply hopeless because neither Darwin's account nor that of followers does very well with self-aware agents. As a matter of history, of course, T. R. Malthus's work had an impact on Darwin. Where's what Darwin says in the 1859 *Origin of Species*:

> A struggle for existence inevitably follows from the high rate at which all organic beings tend to increase. ... It is the doctrine of Malthus applied with manifold force to the whole animal and vegetable kingdoms; for in this case there can be no artificial increase of food, and no prudential restraint from marriage.
>
> (1859, 63)

Darwin has read Malthus quite carefully; rather, more carefully than most 20th century readers. That list, unfortunately, includes Foley. The struggle for existence in Malthus is attenuated by the foresight of agents to delay marriage out of prudential considerations. The non-human agents considered in *Origin* cannot foresee the costs to themselves or to their progeny of their reproductive behavior.

Between *Origin* and the 1871 *Descent of Man*, several important thinkers considered the application of natural selection of humans (Peart, Levy 2003). A. R. Wallace, W. R. Greg and F. Galton agreed that natural selection was inconsistent with the ability of humans to theorize. Wallace noted that human sympathy keeps the unfit from perishing and cheerfully gave up hope of applying natural selection to humans. This was not however the universal reaction. Greg and Galton, lamenting that without the providential order guaranteed by natural selection, the superior foresight of the Scots left the world overly full of Irish. The flourishing science of eugenics grew from this thought. In *Descent of Man*, something he repeated in his letter to Charles Bradlaugh at the time of his trial for the crime of publishing a candid discussion of contraception, Darwin signs onto Greg's and Galton's doctrine. Indeed, he explicitly proposes the new norm of racial perfection induced by natural selection – a theory from outside the system – ought to trump the happiness of the agents inside the system (Peart, Levy 2005). This is not system thinking; this is treating human like cattle who have the temerity to choose their puny goals at the expense of the design of their would-be creator.

Complex accounts from a system point of view begin, as far as I know, with Herbert Spencer's discussion of population theory as of 1850. «Life» for Spencer is equivalent to «co-ordination of actions» (1852, 472). The way station from Malthus's work – and here I depend heavily upon Peart-Levy's unwritten book – seem to be Francis Place and J. S. Mill for whom there is no interesting theoretical guidance other than that which makes sense to self-aware agents. Malthus's worry about «vice» and the preventive check – and thus the moral criticism of contraception – drops out of consideration for these utilitarians.

With Spencer we need not worry about anachronism because his entire career is in conversation with the economists of his time. Some of the details are found in Peart, Levy 2005a, b. He was engaged in public discussion with J. S. Mill over their foundational differences and substantial agreements. F. Y. Edgeworth's 1881 *Mathematical Psychics* comments on Mill's and Spencer's shared egalitarianism as something which Darwinian science had rendered obsolete. Spencer's 1851 *Social Statics* revives Adam Smith doctrine of sympathy, a concern with culminates in his 1893 *Principles of Ethics* in which he proposes that the struggle for existence be turned off by limitation in births to allow sympathy to flourish (Spencer 1893, 1, 329).

Spencer's 1852 article on population, which begins with a comment on Richard Whately's statement on instinct, opens his work on evolution for self-aware agents. Unlike Darwin's 1859 survivalist model, an account Spencer sketches as applicable for those obtuse enough not to recognize costs (Spencer 1852, 499-500) – his theory of evolution supposes that giving life is costly:

> Hence the maintenance of the individual and the propagation of the race, being respectively aggregative and separative, *necessarily* vary inversely. Every generative product is a deduction from parental life; and, as already pointed out, to diminish life is to diminish the ability of preserve life. The portion thrown off is organised matter; vital force has been expended in the organisation of it, and in the assimilation of its component elements; which vital force, had no such portion been made and thrown off, *would have been available for the preservation of the parent.*
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> (1852, 478-479)

Self-aware agents can see these costs just as well as the outside theorist. Sympathy seems to be used by Spencer to describe how the agents in his account view the social world around us and how it influences our behavior. Spencer uses this late in his life to argue for the voluntary limitation of births as a way to expand the scope of our theoretical concerns:

> Whence it follows that the sympathies can become more and more acute, only as fast as the amount of human misery to be sympathized with becomes less and less; and while this diminution of human misery to be sympathized with, itself must be due in part to the increase of sympathy which prompts actions to mitigate it, it must be due in the main to the decrease of the pressure of population upon the means of subsistence. While the struggle for existence among men has to be carried on with an intensity like that which now exists, the quantity of suffering to be borne by the majority must remain great. This struggle for existence must continue to be thus intense so long as the rate of multiplication continues greatly in excess of the rate of morality. Only in proportion as the production of new individuals ceases to go on so greatly in excess of the disappearance of individuals by death, can there be a diminution of the pressure upon the means of subsisten-

ce, and a diminution of the strain and the accompanying pains that arise more or less to all, and in a greater degree to the inferior

(1893, 1, 329)

It seems to me that for Foley's unwritten book, a good deal of attention ought to be paid to Spencer and the economists. To clear Spencer of the eugenics label, which may have something to do with the lack of attention paid to him in recent years, one cannot do better than read Edgeworth's *Mathematical Psychics* to see how Galton's most technical gifted disciple read the arguments. It is important to pay rather close attention to what one is supposed to do with a person when the total utility of a life integrates to zero or negative (Peart, Levy 2005). The author of our written book under review is a little casual about the consequence of the Pareto principle and population (28). Edgeworth is deadly serious; Galton's «celibate monastery» for the dysgenic was not a joke.

Now a word on the book as written. In chapter three Foley (p. 83) presents a chart of the relationship between income per capita and fertility. Output per capita, although sanctified by hundreds of economists in thousands of applications, is an enormously dangerous concept in this context. Think about it this way. Q = output; P = population; L = labor force. Output per capita can be expressed as the product of two separate concepts – labor productivity and the ratio of labor force to the population. Thus

$$Q/P = (Q/L)\,(L/P)$$

The econometric problem is that both right-hand side concepts are influenced by technology but in opposite directions. Technology, as measured by aggregated US privately-financed R&D expenditures, improves labor productivity and helps keep people who aren't working alive (Levy 1992). When one uses a per capita variable mixed with a demographic variable the results are enormously hard to interpret.

The only thing I have to say to Foley's (chapter two) discussion of innovation is that I would have thought that investment in R&D might have warranted a mention as would the stock of R&D in productivity. What Foley writes about global warming and economics (chapter three), which emphasizes the importance of endogenous technology seems sensible to me. Perhaps, thinking of anti-nuclear ideology as stock of political capital might be a way to make endogenous political decisions which influence the tradeoff between 'real' externalities and perceived externalities.

It customary at the end of a warm, favorable review to commend the book to the attention of the reader. At the end of this warm, favorable review I cannot do better than to commend the unwritten book to the attention of the author! We wait for it eagerly.

David M. Levy
Public Choice, George Mason University, Fairfax (va)
DavidMLevy@aol.com

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Liah Greenfeld, *The Spirit of Capitalism, Nationalism and Economic Growth*, Harvard, Harvard University Press. 2001.

During the last 15-20 years there has been a renewed interest in capitalism. The discussion has revolved around ideas of new phases of the contemporary economy. Capitalism has since the 1980s become a common term, and scholars have suggested that capitalism should be understood in relation to, for example, knowledge, aesthetics and the Internet. This focus on contemporary trends may, however, occasionally lead us astray.

With the book, *The Spirit of Capitalism, Nationalism and Economic Growth*, Liah Greenfeld has chosen to analyze the origin of capitalism, by following the well-known comparative historical research strategy. Her book not only address issues of interest to historians, but also questions that social scientists studying contemporary society struggle to come to grips with.

By using the same title as Max Weber's famous book, originally published in two parts in 1904-1905, Liah Greenfeld, one hundred years later, indicate that she has large ambitions with her book. The subtitle of the book, *Nationalism and Economic Growth*, not only separates her title from Weber's, it also adds a layer to her analysis. Her main argument is summarized in the following quotation: «the spirit of capitalism was nationalism», and the reason is that nationalism was, «the ethical motive force behind the modern economy of growth» (p. 58).

Weber argued that there could be many forms of capitalism, but Greenfeld is only interested in the most famous of Weber's question, that is, how can one explain the origin of the Western (or rational as Weber said) capitalism? Her answer is that the spirit of capitalism is a consequence of nationalism. More specifically she studies how the «societal attitudes in regard to the acquisitive drive and its dramatic valorization» shifted, and she claims that this was due to, «a new secular form of collective consciousness (thus a system of ethical standards): nationalism» (p. 21). She thinks, in other words, that Weber did not find the correct motivational principles for capitalistic actions in the religious sphere, though she shares Weber's premises that capitalism is not a «natural» process; it is socially produced, and that it is a scientific question to address its origin. Her argument, however, stresses the global dimension much more than Weber did.

Before discussing the book more in detail, it is good to remember that Weber defined (rational) capitalistic action, «as one which rests on the expectation of profit by the utilization of opportunities for exchange, that is on (formally) peaceful chances of profit» (1968 [1904-1905], 17). An important aspect of Weber's argument is calculation, and another is the market. Only in markets, which represent a peaceful avenue to profit, can calculations be made.

Greenfeld has written an extremely well organized book, which is almost 550 pages long. She begins with one theoretical chapter, and then follows empirical chapters on capitalism in Great Britain, The Netherlands, France, Germany, Japan and the United States. The book is easy to read and one sometimes feels almost trapped in the interesting stories about individuals and events. This outline has some advantages, but the book could have been more concentrated, without distorting the main argument. This study builds on Greenfeld's earlier work on nationalism. This means that she stresses the egalitarian character of nationalism, and sees it as the force that unites people, and made them see and orient themselves to this base of collective identity.

The first empirical chapter presents another story than the Weberian of the emergence of capitalism in Britain, which is indicative of her analytic approach. She claims that people in certain states, such as England, France, Germany and Japan begun to formulate ideas of nationalism in relation to the perceived threat from other nations. This occurred at a time when countries competed for global dominance. As a result, nations developed political, economical, military and not the least ethical strategies to increase the national strength through enhancement of national awareness and identity. This created a breeding ground for economic virtues, such as frugality, which were often combined with a calling, and acceptance of profit accumulation. Many countries saw a strong economy as a way of strengthening the nation, and capitalism became a means in this process.

Let us look more closely at how Greenfeld sees this process in Britain. She starts by stressing that Adam Smith actually wrote a book on The Wealth of Nations. Then she traces the birth of British nationalism, and connects it to how people saw the economy as a means to boost the nation. At the time Smith wrote his book, few people thought of social wholes in terms of nations; dynasties, private individuals and other parts were more known entities. The force of nationalism did not sway until the 19^{th} century. Smith and other peoples' work that Greenfeld quotes were part of the movement of defining boundaries between nations, and effectively creating «nation-states». This meant that the nation, as it were, should be composed of equal men (obviously disregarding women). But the consciousness of nationalism were not diffused evenly in society, nor were all people economically active. In essence, economic nationalism started among economically active people (p. 33). In contrast to neoclassical theory, which assumes egoistically rational actors, Greenfeld claims that the driving force cannot be reduced to interest; it was rather a result of growing nationalistic sentiments.

The building of nationalism must be related to the perception of negative identities, manifested in the idea of enemies of the nation, and England's enemy was Holland. It was with Holland England compared itself, and it was with Holland that England competed. Thus, nationalism is embedded in a kind of market logic, which implies that countries tried to outperform one another. Comparison does not call for sophisticated accounting principles or anything like that, though this would make it easier. Greenfeld's argument, as I see it, relies much more on the impression of competitive relations and especially of inferiority, and possibly also of resentments. Hence, neither Weber nor Greenfeld see the emergence of capitalism as a rational process; it is enough with subjective conditions to generate the effect, though no discussion of the accuracy of the relation and the relative strength is made in the book.

The second substantial chapter deals with Holland, which saw a rise of economic wealth, the first effective joint-stock company formed in 1602, and well developed financial infrastructure. There is certainly support for the argument that the birth of the modern economy took place in Holland. But the economic miracle of Holland did not last, instead its economy declined. Greenfeld rejects those who try to explain the decline in terms of a «modern economy» (p. 89). The reason, we are told, that the economy did not have a healthy development is that Holland at that time was not a nation. This means, to recall the argument, that no construction of a national consciousness has taken place, and that Holland, therefore did not see other nations as rivals. Thus, the difference between England and Holland in terms of economic efficiency, wealth and the like may have been small. However, the important dimension is phenomenological – that people in these two countries saw the world differently (p. 101). Thus, but applying her ideas on the negative case of Holland, *i.e.*, a country with a developed and rational economy, and showing why it did not 'take off', she clearly strengthens her argument.

She then takes on France, Germany, Japan and the US. For the presentation of how capitalism emerges in these countries, the reader is taken on a historically dense journey. Greenfeld shows, in a similar way as she did with England and Holland, how a combination of struggle among individuals, status groups and classes—about the rights of these groups – and the theories that should be employed for understanding the economy, effectively generated ideas of the intimate relation between the economy and the nation. Thus, much space is used by Greenfeld to show how the science of economics came into being in the different countries, and how ideas travelled between continents, within societies and between individuals. Especially intriguing is to see cases when theories are used as means for individual groups, and the rise of economics as a social science. In other words, economic theories are not only of society, they are also very much in and for society. This is reflected by the emergence of the German *Staatswirtschaft*, or economy of the state. During the journey the reader get insights about conflicts of interests. An important struggle, of course, is the one for the freedom of people, most notably the farmers, who could become independent economic actors, and at the same time become citizens. Furthermore, a large debate takes place between those who only think that agricultural work is useful, and who look down upon other forms activities, such as trading and commerce, and those who were active in these new forms of economic activities.

It is here not possible to give the reader but a glimpse of the variety of levels the analysis is conducted. The overall picture that she presents in the book, however, is clear: gradually nationalism is formed, and capitalism is a factor in this equation. This means that political, economical, military and social interests are blended, and in most of these cases the role of the state becomes pivotal.

With a book of this scope every reader can easily identify shortcomings. However, some of these are part of the nature of an undertaking like Greenfeld's; by necessity she has to make generalizations, and make assumptions that cannot be discussed in detail or explicated. Still, she rarely makes methodological comments or discusses the selection of the empirical material. But as the book stands, it is unable to ward off a number of critical points, which undoubtedly diminishes the power of her argument. A methodological appendix or a more extended discussion of these important issues may have straightened out a few the question marks the book generates.

It would also have been nice to see Greenfeld engage more with competing theories of capitalism. To discuss institutional economics, and the role of property rights, which Douglass North sees as the reason for the rise of the Western capitalistic society, would have improved the book. To integrate more of the literature on values, and mechanism for change of value would also have proved useful.

All in all, I find this book very stimulating, and from these comments follows that it is a very good sociological study in the historical comparative tradition. It is easy to recommend a book like this one, which is enjoyable to read and full of interesting historical points. The way she shows that ideal interest are at least as profound as material conditions, and how they are constructed, sharply contrasts her view with the outmoded Marxian view.

One may also reflect on issues that the book rises. Are trends, for example, by Muslims to create an Islamic Nation likely to recast some tables of value to facilitate capitalistic actions for the cause of Islam? How should one conceptualize the economy boom in China – as a form of nationalism? These, I think, are examples of the kind of questions this book raises, and they also point to the fact that the book is not only relevant to those who are interested in historical developments, but perhaps more to those of us who primarily are interested in contemporary questions.

Patrik Aspers
The London School of Economics and Political Science and Stockholm University

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S. M. Ghazanfar, *Medieval Islamic Economic Thought: Filling the "Great Gap" in European Economics*, London, Routledge, 2003, pp. 304.

This book is an assemblage of fourteen papers, all but one having been previously published in a variety of journals and books, some in the respected history of political economy journals. Most were either authored or coauthored by S. M. Ghazanfar. The aim of the book is to show that knowledge from the Islamic scholars of the middle ages fills what historians of economic thought, in particular Joseph Schumpeter, have heretofore regarded as a gap between the «philosophical contributions of ancient Greece and their rediscovery in the middle ages» [in the works of the Latin scholastics] (xi). This gap has led to a «blind spot» in economics, it is alleged. As Professor Ghazanfar sees it, economics, following Schumpeter, is «an incessant struggle with the creation of our own and our predecessors' minds» so that «the process of the filiation of ideas is carried forward unabated». (p. 19) The Islamic writers covered are Abu Yousuf, Ahmed Ibn Hanbal, Abu Munhmmad Ibn Hazm, and Abu Hamid Al-Ghazali. Because of its limited coverage, it is not an effort to identify a 'school of thought' or a 'profession'. It is more a report of various Arabic writers from different times and places during the middle ages who were of interest to Ghazanfar's filling-the-great-gap thesis.

The authors' arguments and quotations are compelling. They rewind the reader that works of the ancient Greeks were translated into Arabic and read by Islamic scholars; while they were largely destroyed and went unread by the earliest Latin scholars. When later Latin scholars learned arabic, they learned those early ideas as well as to the ideas of the Islamic scholars themselves. This sequence of events suggests that, rather than a gap, there was a continuity of ideas starting with the Greeks, passing through the Arab-Muslin scholars, and ultimately finding a resting place in the works of the Latin scholastics.

Not only does the story sound reasonable, Ghazanfar *et alii* cite passage after passage in an effort to show a similarity between the two sets of pre-Enlightenment scholars. Nevertheless, the claim that the Arabic writers anticipated the scholastics is not defended by a close examination and comparison of the original texts. Rather, it is defended by showing that the ideas of scholastic ideas reported by others writers on the history of ideas were present in the works of the arabic writers, who wrote before them. In other words, although Ghazanfar *et alii* examined the arabic texts, they show no evidence of having examined the scholastic texts first hand. Given the preliminary nature of the thesis, however, this should not be regarded as a shortcoming.

This reviewer, who reads neither Latin nor Arabic, cannot write informatively about whether Ghazanfar *et al* correctly interpreted the relevant literature. It may be worthwhile, however, to comment on the significance of this work and others that are concerned with what is aptly named the prehistory of economics. Broadly speaking, the difference between pre-Enlightenment and post-Enlightenment observers of market interaction is the 'constructivism' of the latter. The post-enlightenment classical economists believed that through reason and thought, they could help to create a better society. This attitude competed directly with the religious view that everyone should try to please God by acting according to the prescriptions given in the great books or by the revered clerics. Thus the Enlightenment helped to clear the way the political eonomists of the classical era to write about wealth and the market economy without worrying inordinately about religion. It seems reasonable to say that no important writers of this period regarded their work as an interpretation of the holy texts. In developing principles like the invisible hand theorem, comparative advantage, and the quantity theory of money; they did not worry about being branded heretics or subversives.

In the 19th century, classical economics was challenged by the claim that the market economy is unfair or that it actually is harmful to distinct classes of people. European and American writers responded by developing neoclassical economic theory. This theory put consumer's well being, as opposed to national wealth, at the top of the value scale, thereby marginalizing the idea that the market economy should serve kings or sultans.

Writers in this new profession elevated *reason* to a new level by introducing the so-called 'science of choice' based on the ideas of consumer's sovereignty and opportunity cost. The Austrian branch made the special contribution of recognizing the role of prices and interest rates in economizing on and communicating knowledge about preferences and the means that people perceive to satisfy them. With this development, the divorce between economics and both religion and public administration was complete.

The «great gap» described by Schumpter and emphasized in the book under review was a episode in the pre-history of economics. It will be of interest to historians who aim to trace writings that report observations and speculations about market interaction before the Age of Reason.

J. Patrick Gunning
Feng-Chia University, Taiwan

Dan Friedman and Alessandra Cassar (eds), *Lab – An intensive course in experimental economics*, London, Routledge, 2004, pp. xiv+233.

This book aims to capture the essence of the Summer School on Experimental Economics for economics graduate students, held in Trento (Italy) in June 2001 and coordinated by Axel Leijonhufvud.

Since Daniel Kahneman and Vernon L. Smith were awarded Nobel prize in 2002, laboratory experiments have been definitely considered a legitimate tool that can be used to gather data in many fields of Economics and with different purposes and approaches.

The book proposes the state of the art of the discipline and its methodological status, with all the ambiguities and unexplored potentialities that have characterised it from its very beginning. Therefore it presents the rules and criteria developed within this discipline to design and conduct experiments (and to have articles published) and offers many practical instructions, related to the conduct of experiments. As it is more agile than the traditional handbooks available on the market, it is interesting also for those scholars who just wish to know something more about this discipline.

The technical notes on how to handle the data gathered and how to analyse them are very useful, as they also highlight, directly or indirectly, important methodological aspects of the discipline. Usually, for example, experimentalists use non parametric statistical procedures to test their hypotheses. Nevertheless, this kind of test are rarely presented in the basic statistical course, focused on the traditional parametric tools.

Consider a game where a number of subjects interact. What is an independent observation in this case? The most conservative position is that the entire session should be seen as a single trial. On the other side, each individual choice can be used as an independent observation. The book does not give an answer to this dilemma – no answer, in fact, exists in literature – but it sheds the proper light on this issue. Other interesting methodological issues of this kind, usually neglected in other books on the subject, are discussed (such as, for instance, the relevance of the outliers in shaping the statistical index).

After this practical and methodological introduction, the book describes a few of the most relevant laboratory results, showing the wide range of possible applications. Experiments have been used for: (1) discriminating among alternative theories, (2) testing the predictive power of a single theory, (3) finding empirical regularities to help extend a theory into a new area. Many sessions have also been conducted for practical policy purposes, *i.e.*, guiding the choice of public or private organizations.

The applications described regard: markets, auctions, oligopoly, games, choice anomalies, policy analysis and institutional engineering. Two short papers by Reinald Selten on learning and imitation are also included.

Market experiments are among the most well-known. The Nobel Prize Vernon Smith is the main reference in this area, as he developed Edward Chamberlain's pioneering preliminary idea (1948). Chamberlin simulated a market using Harvard PhD students attending his class. Thus he created a market characterised by a high number of traders and imperfect information, privately assigning single unit values to buyers and costs to sellers. Students, then, wandered around the room searching for counterparties to conclude an exchange in bilateral negotiations. The result was a considerable price dispersion and significant inefficiency.

In Smith's experiments, carried out from 1956 on, buyers and sellers transacted through publics bids and asks. He created a market institution that is now known as

Double Auction. He also introduced monetary payments to motivate participants (today, payments are generally necessary to have an experimental paper accepted by the experimental community, though important results have been reached in sessions with no monetary reward).

Smith's findings were different from Chamberlin's: the prices of all transactions converged rapidly towards the competitive equilibrium values. Since then, thousands of experiments have confirmed these conclusions and their robustness under varying conditions. Smith (1982) read this series of works as a corroboration of Hayek's hypothesis, more than as a demonstration of the neoclassical theory of the market. In fact, the participants in those sessions need to know very little and to have a low-level rationality to achieve competitive equilibrium. Market institutions force individuals to reveal their private knowledge. Therefore perfect information is not necessary and few competitors are sufficient to get the most efficient outcome.

The analysis of experimental markets has increasingly developed, giving rise to a parallel theoretical literature. Many kinds of auctions have been tested. The book proposes a good and brief guide to this wide area or research, to its results and new developments (such as the analyses of real world auctions carried out over the internet).

Experimental auctions have been also used to compare alternative possible policies. In such cases the analysis has a specific, rather than general, aim: trying to answer well-defined and localized questions. One of the most well-known experiments of this kind was carried out by Grether and Plott (1982) for the Federal Trade Commission of the United States, to assess the claim that four domestic producers of a gasoline additive were colluding, to maintain high prices, using various practices. Grether and Plott simulated different versions of the market, including or not the practices under exams, so as to compare the equilibrium prices in the different cases. Their conclusion supported the idea that the behaviour held by the firms lead to higher prices. After the experiment, the firms lost the case against the government, but later they won on appeal.

Another related important and flourishing experimental area is that of institutional engineering. Experiments have been used, for example, to define the mechanism necessary to assign new medical doctors to US internship, or to allocate airline landing rights in airports; in the 1980s this kind of experiments played also an important role in creating exchange institutions in the market of electric power and natural gas in the United States.

Another main area of research is that here referred to as «Choice anomalies»: it was inaugurated by Allais (1953) and developed by the Nobel Prize Kahneman with Tversky (1979). This stream of studies is at the borderline between economics and psychology (Kahneman is a psychologist himself) and give rise to the so called *behavioural economics.* This new approach aims at changing some of the neoclassical hypothesis, but remaining in the same formal environment. The Prospect Theory, proposed by Kahneman e Tversky as an alternative to the expected utility, for example, models situations of risk rather than uncertainty (with reference to the distinction made, among the others, by Knight 1921), thus maintaining the idea of perfect information, but with agents not able to maximize.

The main anomalies in the individual decision making, briefly but accurately described in the book, are the following: (1) the *loss aversion:* individual well-being is more influenced by a decrease in consumption than by an equal increase in it; for individuals the value of a little loss is almost twice as high as the value of an equivalent gain; (2) the *endowment effect*: once a person owns a good, it becomes more valuable; (3) individuals have many limitations and make errors in their statistical and probabilistic reasoning (for example, the *gambler's fallacy*: if a fair coin does not come up tails for a while, people generally think that tail is more likely than head); (4) *confirmatory bias*, when individuals develop a strong idea, they will perceive new information according to such hypothesis, sometimes even misreading the additional evidence; (5) *anchoring bias*, new situations are evaluated in relation to the starting point and not to their absolute value; (6) *framing effects*, the choice among equivalents alternatives is influenced by the frame, *i.e.*, the way in which the comparison is presented (for example, the same hypothetical cancer treatment receives a different evaluation if it is described using its mortality rate or survival rate).

Besides, several experiments have shown that people are not as egoist as in the traditional economics view.

What are the effects of all these anomalies on the economic outcome? This is an important question, dealt with in the book. In some cases people learn how to overcome the biases. In other situations, different institutions (such as markets) evolve to help people overcome such limitations (the main example is the recalled *double auction* mechanism, which functions independently of the information and computational capability of the individuals, who are generally unaware of the market functioning).

Nevertheless, in other cases institutions can lead to an opposite result, amplifying biases (and determining, for example, market bubbles). Learning processes, as well, have proved to lead to new anomalies, such as path dependent form of behaviours, locked-in the past.

These results are the centre of the attention of another stream of experimental analysis, whose main interest is understanding how people behave in the reality, how they build knowledge and learn to face new (individual, social or organizational) problems.

I have elsewhere (Novarese, 2003) defined this approach as *cognitive experimental economics*. It is related to the new area or research called *cognitive economics*, intersected but not coincident with the behavioural stream. Its experiments are less bounded to the theory and more interested in drawing out hypotheses on the real human behaviour in situations of uncertainty.

This book does not take into account this area directly, probably because it is considered too heterodox with reference to methodologies and areas of interest. Still, the absence of any reference to this approach is surprising, considering that Massimo Egidi (University of Trento) was one of the lecturers in the school that inspired the book, and he is also one of the main representatives of this field of analysis.

In his introductory chapter, Axel Leijonhufvud highlights the relevance of such approach. He points out that mainstream economists deduce behaviour starting from the optimization idea. Since a part of the discipline is trying to go beyond maximization, such deduction will have less relevance and therefore economists will have to study «how people actually behave, how decisions are reached, implemented and monitored in organizations» and a bridge will lead economics «toward the cognitive science and organization theory» (p. 10). Laboratory studies will be a necessary tool to reach such aim. This is completely true. With few exception, yet, so far experimental economics has barely undertaken this path, despite important scholars such as, for example, Simon and Cyert and March, have clearly indicated it (see Novarese 2004).

Marco Novarese
Università del Piemonte Orientale

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J. P. Potier, D. A. Walker (eds), *La correspondance entre Aline Walras et William Jaffé*, Paris, Economica, 2004, 10, pp. 134.

Jean-Pierre Potier and Donald Walker, both renowned Walras scholars, have recently edited a curious booklet containing 67 letters between Léon Walras' devoted daughter, Aline, and William Jaffé, the translator and editor of the English language edition of *Eléments d'économie pure* (1954) and the celebrated editor of the historically important *Correspondance of Léon Walras and related papers* (1965).

The protagonist of this collection is clearly Aline Walras. She not only displays a deep love of her father and grand-father (Auguste Walras) and their profound intellectual legacies, she also reveals an interesting understanding of major political events. For example, in 1935 she considered the Italian war in Ethiopia to be the beginning of new world war. She also expressed deeply conservative and liberal views, through her opposed to: fascism; bolshevism; and the political ideas of Léon Blum. She was dissatisfied with the unsupportive position of the University of Lausanne towards the publication of her father papers and tried to find other ways of realising this end. Over a period of twenty years, her persistence culminated in the emergence of a small, but enthusiastic, group of scholars who attempted to celebrate the intellectual achievements of Walras in various ways. In this regard, Etienne Antonelli tried, unsuccessfully, to put together an edition of Walras correspondence (a task that Jaffé subsequently revived and completed with distinction) and Gaston Leduc re-published, in 1936, the *Etudes d'économie sociale* (Paris, Pichon and Durand-Auzias) and the *Etudes d'économie appliqueé* (*idem*), and, in 1938, the *Abrégé des éléments d'économie politique pure* (*idem*).

Jaffé's interest in translating and editing Walras' *Eléments* commenced with his first significant paper on the scientific work of Walras. This volume of correspondence affords historians of thought many insights into the academic and personal problems that Jaffé had to overcome in order for this, the first of his two great Walras project, to come to a successful conclusion, which was finally achieved 12 years after Aline Walras' death in 1942. Perhaps the most interesting period of the correspondence concerns 1934-1935, when Aline was deeply disappointed by the failure of Europe to mark the centenary of her father's birth, even though this anniversary was celebrated in the United States through a series of important papers and meetings. The collection also reveals Aline's rather naïve view that, as daughter of the great Walras, her candidate, Georges Lutfalla, should succeed Pasquale Boninsegni (Pareto's successor) to the chair in political economy at the University of Lausanne.

Generally speaking, the letters contained in this booklet are a rich source of information on the international scientific and political climate in the years up to Aline Walras's death. In view of this, perhaps it would have been appropriate for the editors to include additional, and more detailed, editorial notes. Conversely, the editors' introduction and their list of *dramatis personae* are very useful.

Fiorenzo Mornati

Leonidas Montes, *Adam Smith in Context: A Critical Reassessment of some Central Components of his Thought*. Houndmills and New York, Palgrave, 2004, pp. xii+186.

In recent decades the emphasis in studies of Adam Smith has shifted from an almost exclusive focus on the *Wealth of Nations* to a much broader recognition of the importance of his other works, notably the *Theory of Moral Sentiments*. The focus in this book is mainly on Smith's moral philosophy. After a brief introduction, there is a survey and discussion of the «Adam Smith problem» (or '*das*' Adam Smith problem, since it was a German invention) followed by chapters on Smith's treatment of the virtues, on the concept of propriety, and on Smith and Newton from a methodological standpoint. The chapters, we are told, can be read independently, and it is indeed rather difficult to trace a common thread through them. This is a familiar structure for a PhD thesis, which is how the book started off.

The survey chapter covers the literature on the «Adam Smith problem» well, with some background on German reactions to Smith in the nineteenth century and a thorough discussion of developments since 1976 (when the editors of the Glasgow edition of the *Theory of Moral Sentiments* thought they had settled the matter). If I have a criticism it is that commentators have often been rather unclear about exactly what the 'problem' is, that is, what question it is that is under discussion, and Montes does little to clarify the issue. At one end of the scale, the 'problem' might be identified with a claim that the *Theory of Moral Sentiments* and the *Wealth of Nations* are incompatible because in the former Smith treated individuals as altruistic while in the latter he treated them as selfish. It is not clear to me that anyone has actually stated quite such a crude reading except to reject it as patently false. At the other extreme, if the Adam Smith problem is redefined as «the very complex task of establishing Smith's intentions as a moral and social theorist», as Teichgraeber (1981) has proposed, there is not likely ever to be a definitive resolution. In between, there are a variety of issues which might be seen as relevant to Smith's views of human behaviour in the two books. How do people actually behave? How should they? How do they judge others' behaviour? How do they judge their own actions? How is it best for society that they should behave? Any or all of these might be seen as relevant to the relation between Smith's main books, and might thus be thought to be related to the 'problem'. Montes is sympathetic to the view that there may yet be more to be said about these issues.

The two chapters which follow deal primarily with Smith's moral philosophy.

Late in his life Smith added a new part VI to the *Theory of Moral Sentiments* dealing with «the character of virtue». Of Smith's four virtues Montes particularly stresses self-command, the ability to control the passions and stay within the bounds set by the impartial spectator and thus by conscience. The other three virtues of prudence, justice, and benevolence have a consequentialist focus, but self command underpins them by stressing that we judge a person not just by the results of their actions but also by the reasons for them and the difficulties overcome or the sacrifices they have made. The 'virtues' have a long tradition in moral philosophy, stated in different forms and with different rationales from ancient times onwards, but in the early modern period they became particularly associated with what is now called 'civic humanism'. Montes argues that Smith's virtues can be regarded as a last glimpse of that dying tradition.

He then turns to the concept of propriety, which Smith introduced at the very start of the *Theory of Moral Sentiments* – part I of the book is titled «The Propriety of Actions». We sympathise with others, but only up to a point. Correspondingly, when we act we must moderate our feelings and actions if others are to sympathise fully with them. This is propriety. Montes argues that the concept takes on additional meaning with the introduction of the impartial spectator, internalised as conscience and backed up by self-command, when it becomes a guide to action and not simply a precondition of sympathy.

Does all this have any bearing on the history of economics? Montes wants to use it to reject the claim that Smith is, in some ill-defined sense, the father (or grandfather, or ancestor) of modern mainstream economics, and in particular of general equilibrium (GE) theory. His main argument seems to be that Smith saw humans as essentially social and moral animals, in opposition to the 'cold greediness of the *homo oeconomicus* as a socially detached acquisitive individual' (p. 88; other equally intemperate definitions of the view of man which mainstream economists are said to hold are scattered through the book). Montes returns repeatedly to his criticism of what he says is the modern mainstream view of Smith's place in the history of economics, but his critique is weakened by his unwillingness to admit that there might be any justification for the mainstream view. His account of Smith's moral philosophy is careful, scholarly and contextual, but it is counterposed to a straw man, with no context or history.

It may be worth adding that Montes is attacking a rather dated straw man. At least in the economics departments I know, someone who is described as a 'theorist' nowadays is normally a game theorist (and I have not heard Smith claimed as the ancestor of game theory). Formal general equilibrium theory still exists, but mainly in specific applications such as simplified macro GE models. If there is a branch of modern economics that claims inspiration from Smith, it is endogenous growth theory. To Montes, however, the mainstream is identified with Walrasian GE theory.

To take one example of the style of criticism involved, Montes cites Stigler's (1982, 136) description of the *Wealth of Nations* as 'a stupendous palace erected on the granite of self-interest' and rejects it as 'not only biased and misleading, but simply mistaken' (p. 88). What is astonishing is that this brutal judgement is not based on any substantial discussion of the *Wealth of Nations* at all. As far as I can understand it, Montes simply assumes away the Adam Smith problem by taking it for granted (without discussion or comment) that if the *Theory of Moral Sentiments* deals with people who have moral concerns linked to sympathy and hence to social interaction, then the *Wealth of Nations* cannot be based on self-interest. But this has to be demonstrated, not assumed.

In any case, I see no inconsistency between Montes's account of Smith's moral philosophy and Stigler's claim that the *Wealth of Nations* is based on the «granite of self-interest». The *Wealth of Nations* deals with economic issues, and hence with the way people actually behave in making mundane, everyday economic decisions. Smith recognised that self-interested («prudent») behaviour was entirely proper in such circumstances. 'Cold greediness' is another matter, but no mainstream economist that I know has ever advocated it.

The target of Montes's criticism, the claim that Smith is in some sense the ancestor of Walrasian GE, is not well defined and Montes does little to clarify it. GE theorists (though not Walras himself) have sometimes claimed descent from Smith in a rather casual way, but there are also substantial interpretations of Smith, notably by Hollander, which identify what are claimed to be proto-Walrasian elements in the *Wealth of Nations*. Montes does not examine these views closely – for example, Hollander is mentioned once, in the chapter on Smith and Newton, but his substantive arguments are not discussed at all.

It is worth remembering that once a book is in the public domain, readers are free to take from it what they will. I can well see how the *Wealth of Nations* might inspire a general equilibrium theorist. Take, for example, the discussion (in *WN*, ch. 9) of the acquisition of new territory or new branches of trade. Capital is diverted to profit from the new opportunities, leading to a reduction of supply and increased prices in established activities as the system adjusts. There is very clearly an insight here into the way an interrelated multi-market system adjusts, though not, of course, a formal equilibrium model. Equally, Marx was in part inspired by Smith to construct a wholly different system. Montes himself admits that the *Wealth of Nations* «inspired, but did not necessarily lead to, the neoclassical project» (p. 162). The metaphor of parenthood is quite appropriate – human beings can have more than one child, and they can turn out in ways that their parents might not approve of. It is not clear to me why Montes should choose to return repeatedly to the attack on such an innocuous target.

The third and final substantive chapter, on Smith and Newton, has little connection to the rest of the book except that it attacks the same target, the claim that Smith is the father of modern economics. According to Montes, «economists conventionally take it for granted that Smith applied Newton's method to political economy. Because Newton's method is thought to be similar to that of modern mainstream economics, [this] is taken to further bolster the claim that modern mainstream economics continues the Smithian tradition» (p. 130). I find this rather far-fetched. Many modern economists would like to be seen as scientific, but I do not believe that any connection between Newton and Smith bulks large in their self-image.

Newton described his method as consisting of «analysis» or «resolution» followed by «synthesis» or «composition», that is, first the discovery of underlying principles, then the application of those principles to explain observed phenomena. Montes claims that this is different from the widespread axiomatic-deductive interpretation of his method which puts the stress on deduction (preferably mathematical) where Newton put the stress on discovering the principles (which then become axioms). As it happens, Smith gave his own account of what he calls the Newtonian method: 'in the manner of Sir Isaac Newton we may lay down certain principles known or proved in the beginning, from whence we account for the severall Phenomena' (1983, 145-146). This suggests that Smith saw Newton's method in axiomatic-deductive terms – too much so, perhaps, for Montes to be really comfortable with.

Montes provides an analysis of chapter 7 of the *Wealth of Nations*, which is often taken to foreshadow general equilibrium thinking. I and most others would say that the system of natural prices surely describes what can be called an equilibrium, or at the least a sketch of a necessary condition for (long-run) equilibrium. Montes rejects this reading on the curious grounds that although there is a tendency towards this equilibrium, repeated disturbances ensure that it is never reached. I don't think this would worry most mainstream economists, who understand perfectly well that a model of equilibrium is exactly that – a model, designed to help us grasp a more complex reality.

Overall, this book has much of interest to say about Smith's moral philosophy and his view of human behaviour and character, set in its historical context. However, the claim that mainstream economists have in some way misread or misrepresented Smith is less convincing.

Anthony Brewer
University of Bristol

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Giulio Palermo, *Il Mito del Mercato Globale. Critica delle Teorie Neoliberiste*, Roma, manifestolibri, 2004, pp. 215.

According to Antonio Negri's prefatory remarks, this is a fundamentally political book, useful for every reader (8) but on the very next page Palermo writes that it is above all, «un libro di economia o, più precisamente, di economia critica». (9) The back cover informs us that the argument is directed above all at the «lettore commune» and Palermo assures the reader that the book should be accessible to the non-economist (13); yet he also says (14) that he wishes to challenge professional economists and the argument deals with the existence, uniqueness and stability of general equilibrium, with the Sonnenschein-Mantel-Debreu result, with the two theorems of welfare economics, etc.. It is thus not at all clear for whom this book is intended. The «lettore commune» will find it really hard going, while the well-trained economic theorist may well find that it swings between being boring and being annoying; for on the one hand, as Palermo acknowledges (133), most of the theoretical and practical issues raised are not really original and have indeed been long debated while, on the other hand, there are passages of purple prose in which economists appear to be denounced for giving implicit support for the existing distributions of income and wealth, or for relating demand only to preferences and not to constraints!

A central claim of the book is that the growing dominance of 'market values' and of the assumption that the market is always the best form of organization is widely – but wrongly – supposed to have 'scientific' support within explanatory and normative economic theory. Palermo deals with, and is careful to distinguish between, 'neoclassical' general equilibrium theory and 'Austrian' theory. (Negri unhelpfully refers only to «economia politica classica», «teoria classica» and «analisi classica»! It would be difficult to prove on the basis of his two-page «Prefazione» that he was well-equipped to introduce this book.) For Palermo, despite their differences both neoclassical and Austrian 'subjectivist' economics serve to support a liberal world view (17-20). Whatever may be the case with respect to Austrian economics, it is far from self-evident that GE theory and Paretian welfare economics will, of themselves, persuade anyone (who really understands them) of any wide-ranging political and economic policy standpoint. The book could have been a lot more interesting if Palermo had deleted his purple prose and used the space to show, with evidence, *who* derives their general policy outlook from GE theory and how they see the latter as giving 'scientific' support to the former. Again and again throughout this book the reader wonders just who is being referred to.

Few economists will need to read Palermo to learn that Paretian efficiency is consistent with extreme inequality, or that a Pareto improvement could make someone better off but still leave them badly off (29-30). Many of them, however, will be surprised to learn that for the «economista borghese» the importance of an individual in society is given by his ability to spend (28, *italicized* in original!) and that the «teoria economica borghese» gives no importance to the distribution of income (34). They may also find it odd that Palermo discusses the two theorems of welfare economics but *not* compensation criteria or cost-benefit analysis (both open to criticism, of course). According to our author, when we speak of the functioning of the market we identify demand with preferences and not with constraints. (47; see also 49) One can only ask – Who are *we* here? Not GE theorists, clearly.

In the first part of chapter 3, «Il mercato e i suoi miti», Palermo refers frequently to «power» and claims that most economic theory ignores it/denies it. Unfortunately, no adequate explanation is given of what *exactly* Palermo means by unequal power in this context – presumably something other than unequal property ownership? – so that it is difficult to assess his claims. Later in the chapter, in the context of information dissemination, he states that firms plan and that the larger they are the more planning they do (76), which lends him to ask why capitalist planning is supposed to be efficient, while socialist planning is taken to be impossible (77).

In chapter 4, «Mercati teorici e mercati reali», Palermo has much to say about the existence, uniqueness and stability of GE; non-economists may find this very difficult and economists may find it very familiar – apart from the remarks on Arrow's Impossibility theorem (86) –. How many theorists are satisfied with the state of uniqueness theory or stability theory? Rather more interesting perhaps are Palermo's reminders that 'general' equilibrium theory is only 'less partial' equilibrium theory. If firms are profit maximizers and can influence technology and/or preferences so as to increase profits, how can technology or preferences appear amongst the data (88-94)? Unfortunately, Palermo presses on to ask how all resources can be taken as data, *without* telling the (non-economist) reader about growth models, intertemporal choice models, resource-extraction theory, etc., etc. (94-95) Although Palermo cites McKenzie's *New Palgrave* entry on GE he does not point out that McKenzie there raises these very questions about the 'data' of GE theory; or that McKenzie is explicit re assumptions concerning the survival of the consumer, about which Palermo waxes lyrical. (99) – at (102) there is a reference to Guerrin (1985) but this appears to have been omitted from the bibliography –. Economists will learn little from the discussions of increasing returns, externalities and public goods.

The first half of chapter 6 deals briefly with both neo-institutionalism (Williamson) and neo-Keynesianism (Stiglitz) but the second half is given over to 'radical economics' *à la* Bowles and Gintis, with a passing reference (23) to 'analytical Marxism'

– and it will not escape the reader either that Negri describes these arguments as «molto importanti» or that Palermo himself, in the very last paragraph of the book, highlights his criticism of the «sinistra di mercato» (182) We are left in little doubt that radical economics is dangerous (for the political left). More specifically, the work of Bowles and Gintis is portrayed as an extension/internal critique of GE but as providing no basis for any real alternative to the mainstream. Even if they consider power, it is interpersonal power rather than social power relations that they analyse. (125-128; the pagination is missing for 126-128) The result that profit is positive if and only if surplus labour is positive is mentioned (130), without any warning to the non-economist reader that this is *not* equivalent to showing that exploitation is the cause of profit.

Chapter 6 considers the spreading acceptance of the «valori del mercato» and argues that the political left inevitably has problems once it accepts such values. Chapter 7 asks: what is to be done? It is predictably depressing, giving the reader no more that phrases about one head – one vote, democratic planning, using voting to inform the planners about subjective preferences (167), conscious control of society and planning on the basis of a democratic criterion (172). (There is a puzzling discussion (173) of the fact that price ratios are not equal to use-value ratios, with *no* reference to marginal use-values). Such phrases simply do not add up to a serious proposal. To take but one issue, the book contains many references to the world as a whole, the huge numbers of very poor people, etc. But Palermo's phrases re democratic planning say nothing of whether this is to be done nation by nation, or for the whole world. In the former case, how would trade flows be planned? In the latter, why should the citizens of a small, cold [or hot] country agree to be out-voted by those of a large, hot [or cold] one? Such questions may sound light-hearted but it is *only* when the 'one-world' nature of economic life is taken seriously by advocates of 'democratic planning' and 'conscious control of society' that they themselves can expect to have their proposals taken seriously.

The book has five pages of end-notes, a six page bibliography and a 21 page glossary of economic terms; annoyingly, there is no index. It is not clear who is intended to benefit from reading this book, or who will in fact so benefit. Palermo himself tells us on his last page that «In raising these criticisms I have done no more than make explicit what economists … know perfectly well (or ought to know)». (182)

Ian Steedman
Manchester Metropolitan University

Bryan Fields, *The Catholic Ethic and Global Capitalism,* Aldershot (Hampshire), Ashgate Publishing, 2003, pp. xvi+278.

The thesis of Max Weber that the Protestant ethic was linked to the rise of capitalism was widely accepted for much of the twentieth century. Economies largely managed by those ascribing to Protestant values, in particular the United States, were seen as much more successful than those where a Catholic ethic prevailed, as for example, in Latin America. In Ireland, the country that Bryan Field takes as his case study, the Protestants of the North attributed their higher living standards and more buoyant business conditions compared to the Catholic south to religious factors, at least until the early 1970s when the economy of the North was partially destroyed through sectarian conflict. Nevertheless, even as late as 1999 there were writers who suggested that Catholic countries tended to be poorer than their Protestant counterparts, and this was attributed to a weaker Catholic work ethic.

Capitalist development since the 1990s seems to be much less religious or sectarian specific, as the global economic order changes with the emergence of new, so called, 'tiger' economies. The American Catholic theologian, Michael Novak, anticipated this increasing diversity of forms of capitalism when he wrote in *The Spirit of Democratic Capitalism* (1982) that at the heart of capitalism there was a spiritual void that into which the faithful could introduce their own belief system. Fields does not cite Novak, but the latter's ideas fit in well with the Irish experience, which has been of a specifically Irish form of capitalist development, with its own unique features drawing on the country's changing, yet enduring, Catholic values.

Field cites a number of authoritative surveys showing that the Irish remain devout Catholics, with ninety four percent believing in God and eighty five percent in Heaven. In the 2002 census eighty eight percent of the population of the Irish Republic described themselves as Catholic, and the proportion attending Sunday Mass is the highest in Europe. There is no evidence that the new prosperity resulting from the tiger economy has resulted in the population becoming more secular, materialistic, consumer driven and neo-pagan. This conflicts with the common view, at least in Europe, that rising living standards are associated with a decline in religion, worship being a solace for the poor.

Although there is some notable writing on the sociology of religion, Field argues that sociologists tend to underestimate the influence of religion and culture on development and overemphasise economic factors. Leading writers in the past such as Freud, Durkheim and Weber who equated Catholicism with backwardness were themselves disbelievers, and therefore not really objective analysts. Field however follows Durkheim's methodology in his study by building an incremental and evolutionary picture of the linkages between economic and religious values, but in the Irish case this leads to very different conclusions to those viewed by Durkheim as being generally applicable.

The profile of Ireland provided is rather basic but should be informative for those with little knowledge of the country. The treatment of income distribution and employment is rather cursory, and the account of the reasons for Ireland's success as a tiger economy drawing on international reports, such as that of the International Monetary Fund in 2000, adds little to what is already known. Field is not an economist however, and in this field he has less to contribute.

The strength of the work is analysis of the interviews used to explore the relationship between religious values and Ireland's economic miracle. These were conducted with four groups of respondents. The first group were eight respondents from what Field designates as the national domain: government ministers, a representative from the employers' confederation, trade unionists and senior church leaders. The second group were seven senior Irish executives of multinational companies, the third group of seven being employees of these companies. The final group consisted of four unemployed men, three homeless people and an advocate of the homeless, as well as four Traveller women or gypsies. Obviously such as sample is not necessarily representative or large enough to permit statistical inferences to be drawn, but the work is qualitative rather than quantitative and the interviews are in depth. There is a detailed discussion of methodological issues, and Field's approach seems sound. There are extensive quotations from the respondents and an attempt to summarise these through thematic tables.

Catholic social teaching heavily influenced the views of the respondents, with an emphasis on social partnerships and a balance between equity and competitiveness. Socialist ideology appeared to have no influence on any of the respondents, and this has never been a significant factor in Irish economic life. Some suggested that unlike Protestants, Irish Catholics were freed from the burden of finding answers to ultimate questions as this was left to the priests, leaving the labour force free to focus on its work. The respondents regarded a charitable disposition as especially virtuous and felt a sense of social obligation. The executives were hard working, but did not perceive this industrious ethic as being related to their own spiritual salvation.

Almost all respondents viewed work as a means of securing a comfortable life, and most respondents were content with the material goods they had and were realistic about what they wanted with respect to possessions. There was a general desire to maintain a balance between work and leisure, and a high value was put on the latter. Nevertheless there was a strong work ethic, and it was apparent that some respondents had a real vocational conscience. Competitiveness was seen as natural, but this was balanced by a concern for equity. There was an instrumental approach to work that implied that up to a point there was a Protestant type of work ethic, but once enough wealth for personal satisfaction was achieved, the stress was on maintaining wealth rather than seeking yet more material goods. In summary, religion was seen as facilitating an adaptive yet detached approach to economic opportunity, as respondents took advantage of the more favourable conditions in contemporary Ireland, but did not become obsessive about work and money.

Fields examines the evolution of so called 'combative occupations' in Ireland to discover whether those involved had any of the characteristics identified by Marx that were pre-requisites for class conflict. The evidence appears to suggest that was not the case, largely because religion was such a powerful influence on behaviour, a factor seriously underestimated by Marx. There is perhaps more relevance in the Insider and Outsider theory advanced by Elias within the Irish and British colonial relationship. As the Irish felt exploited historically they were not likely to be attracted to a British and Protestant work ethic, but their isolation as outsiders served to foster an alternative Catholic ethic and merge it with nationalist identity and ultimately loyalty to the Irish state. In this context it was Catholic social teaching that was welcomed, not secularist socialist ideals, the latter being also associated with the colonial power.

Although some may challenge Field's assertion that Ireland's rapid emergence as a tiger in the Global capitalist economy was accidental, there is much in the study that is convincing. The Catholic ethic is extremely adaptable, and many Irish people have succeeded in securing both material and spiritual well-being: a 'win-win' situation without any real trade-offs. This could be regarded as complacent and excessively self-satisfied, but the evidence is clear. In some respects the optimistic conclusions of Field's study are like a breath of fresh air, in comparison to the dull and depressing analysis of Marxists and many of the fathers of modern sociological thought.

Rodney Wilson
University of Durham
School of Government and International Affairs

NOTES FOR CONTRIBUTORS

1. Articles should be submitted in single hard copy, together with a floppy disk and a separate cover, to: The Editor, *History of Economic Ideas*, Department of Economics, University of Pisa, via Curtatone e Montanara 15, I 50126 Pisa, Italy, e-mail: hei@ec.unipi.it

2. Authors are required to indicate their affiliation and their e- mail address.

3. Articles should be written in standard English and should not normally exceed 12,000 words, including footnotes and references. An abstract of less than 100 words is also required.

4. Authors should carefully check the conformity of their manuscripts to our editorial guidelines. In particular, they should obey the following requirements:

- Non-English words should appear in italics.
- Due attention should be paid to the use of single or double quotation marks. The first are to be used when highlighting a word or expression (e.g., the so-called 'civilized' society), the second only when reporting another author's word or expression (e.g. Smith's «impartial spectator»).
- Full quotations should be between « ». If their length exceeds 40 words they should be displayed as separate from the main text. Please refer to one of our issues to see how this should be done.

5. References should be listed at the end of the paper as follows:

Weintraub E. R. 1991, *Stabilizing Dynamics: Constructing Economic Knowledge*, Cambridge and New York, Cambridge UP.

Shionoya Y. (ed.) 2001, *The German Historical School*, London and New York, Routledge.

Schumpeter J. A. 1950, «The March into Socialism», *American Economic Review*, 40, 2, 446-456.

Samuels W. J. 1997, «The case for methodological pluralism», in A. Salanti and E. Screpanti (eds) 1997, *Pluralism in Economics*, Cheltenham and Brookfield, Edward Elgar, 67-79. [or: in Salanti and Screpanti (eds) 1997, 67-79 in case the volume is separately listed in the references]

Costa G. 2001, «Solow on exogenous and endogenous growth, and the Swan proposition», *Studi e Ricerche*, Dipartimento Scienze Economiche, Università di Pisa, n. 83.

6. References in text and footnotes should normally be written as: Weintraub 1991, 15-18. The bracketed form Weintraub (1991, 15-18) should be used when the reference is part of a sentence. Please, avoid the use of p. or pp.

7. When quoting from or making reference to either reprinted or translated material, the main reference should always be made to the edition the quotations are taken from, while the year of the original edition should be put between square brackets. For example:

- in text and footnotes: Keynes 1973 [1936], 126.
- in the references: Keynes J. M. 1973 [1936], *The General Theory of Employment, Interest and Money*, London, Macmillan for the Royal Economic Society.

Schumpeter J. A. 1961 [1912], *The Theory of Economic Development*, Cambridge (MA), Harvard UP; original edition: *Theorie der wirt-schaftlichen Entwicklung*, Leipzig, Duncker & Humblot.

8. Papers failing to satisfy either the linguistic or the editorial requirements will be rejected without consideration. All other papers will be double-refereed.

9. Headers of book reviews should obey the following format:

John B. Davis, *The Theory of the Individual in Economics*, London and New York, Routledge, 2003, pp. viii+216.