

Book reviews



Guilt, Blame and Politics

By Allan Levite

Stanyan Press, San Francisco,

1998, 259 pp,

ISBN 0-9666943-0-9

It is always interesting to ponder the origins of one's political and philosophical beliefs, to wonder to what extent they were influenced by one's upbringing, the socioeconomic class of one's family or the books read in one's formative periods. Many interesting correlations have been observed and theories proposed though, understandably, most of these are not easily testable. The theory proposed in this book focuses on what the author thinks is an important correlation between a particular condition, which he calls 'political guilt', and the propensity to believe in causes on the Left.

There have, of course, already been many old variations on similar themes explored in Levite's theory. For instance, much reference is made in Australian political debate today to 'chardonnay socialists', a term which, depending on one's political beliefs, is either a good promotional device for chardonnay or gross libel on an otherwise excellent drink.

However, Levite has been careful to craft a theory with predictive power, free of any *ad hominem* connotations, and supported with admirably exhaustive, well-documented and fascinating research.

Notwithstanding his use of Nietzsche's famous quotation on the men of *ressentiment* just before the table of contents, Levite is fair and objective throughout. He does not intend the condition of political guilt, which he uses to great effect throughout this book, to become some new, conjured up psychiatric illness with which to vilify one's political opponents. He emphasises,

from the very first chapter, that what he defines as political guilt is not meant to be a psychological condition, but rather a particular set of beliefs induced by living under conditions psychologists would call cognitive dissonance—'the belief that one's social or economic position is either unduly fortunate or actually unjust' (p. 4).

This condition does not arise because of the failings of people who experience it. Rather, it is generally experienced by those 'whose economic surroundings conspicuously contrast with the circumstances of those at the lower end of the socio-economic ladder'. It should be noted that what is important in Levite's formulation is the *perceived* disparity. Thus, it is not necessarily a condition restricted to the extremely wealthy.

However, as we shall see, many of the people whom Levite finds among the vanguard of left-wing movements such as communism, did in fact come from the ranks of the extremely wealthy or privileged. Perhaps this is because such people are more likely to have the resources and leisure to engage in activism that elevates them to the ranks of the vanguard. Levite would presumably not deny the relevance of this but would argue that, nonetheless, the factor he posits remains an important one for determining greater susceptibility to belief in left-wing, socialistic ideas.

There are five possible situations in which people might experience political guilt: (i) where they can exist without working, perhaps by means of a large inheritance; (ii) where they are relieved of performing difficult or manual labour; (iii) someone from an upper-class, aristocratic or wealthy background; (iv) where they are able to live affluently

whether or not they actually do; and (v) the extent to which they feel separated from the working classes by factors such as lifestyle, education, etc.

Levite also acknowledges the importance of previous social and cultural conditioning in increasing susceptibility to feelings of political guilt. In particular, the so-called Protestant work ethic has played a part in making people, who have long been accustomed to comfort or who earn money with relative ease (no matter how they earn it), feel ashamed if they do not live up to traditional images like the 'sweat of the brow'. Far from being an *ad hominem* characterisation, Levite's theory implies that those among the relatively comfortable classes most capable of empathy, would be more prone to turning to left-wing causes—or is this in turn too generous?

The most famous example, whom Levite quotes in his first chapter, is George Orwell. Orwell by no means had a sheltered, privileged childhood, but the important point was that he was from the middle classes and seemed to

feel the disparity between the working classes and himself strongly enough, that he felt compelled to live among them and record his experiences in some of his most famous works. But then again, Orwell's own writings suggest that a formative period in his life was his experiences at boarding school, where he suffered from the snobbery of his teachers and fellow students. None of this contradicts Levite's thesis, which is simply that the people from the five classes which Levite mentions are disproportionately more likely to be supportive of left-wing causes.

Many of the examples which Levite documents involve explicitly socialist and

communist causes which makes his thesis quite counter-intuitive—we are not talking about 'chardonnay socialists' or, in US parlance, 'limousine liberals' here. But when one looks at the fascinating history behind the formation of these movements, it is not all that surprising.

Levite's thesis may not ultimately be convincing, but then how can any one theory be in such a complex area as the motivations behind political belief? Indeed, it must be noted that he is by no means proposing his theory as a monocausal explanation. Nonetheless, the book is still worth acquiring for the research that went into it, in particular its compilation of fascinating case studies, vignettes and anecdotes. The implications of his thesis are also enormous in light of the unprecedented rewards accruing to entrepreneurs in the new economy. Will there be a new upsurge of political guilt, and hence radical left movements, among the heirs to current cyberfortunes?

A Hayekian perspective would help stave this off. The 'sweat of the brow' view of meritorious labour, which Levite regards as increasing one's susceptibility to political guilt, owes much to a naïve labour theory of value, as opposed to the subjectivist understanding of the origins of economic value propounded by Hayek.

Reviewed by Jason Soon



The Problematics of Moral and Legal Theory

by Richard Posner

Belknap Press, 1999, 336 p.p., US\$31,
ISBN 0674707710

Richard A. Posner is best known as a prime mover in the Chicago-school law and economics movement. In addition, he is a Chief Judge of the US Court of Appeals for the Seventh Circuit, a senior lecturer at the University of Chicago Law School and a prolific author of academic journal articles and

textbooks on topics ranging from law and economics to human sexuality. In 1999, Judge Posner was appointed to mediate the litigation between Microsoft and the Justice Department over Microsoft's alleged abuse of market power.

We know from his earlier works that Judge Posner considers himself an 'intellectual pragmatist' in the sense of an individual who believes in lively debate, open mindedness and flexibility in the social sciences.

This form of pragmatism should not be confused with 'philosophical pragmatism' which considers that knowledge cannot be grounded on any absolute foundations, nor with 'political pragmatism' which favours civil liberties, tolerance and flexible experimentation. In his latest book, Posner takes his 'intellectual pragmatism' and his fondness for the law and economics movement and focuses his sights on moral theory, particularly in the legal context.

Moral theory, in the context of this textbook, is perhaps best described as a form of abstract premises and theorising on the self and society. It is often associated with the works of, amongst others, Kant and Dworkin.

Moral theorists seek to establish individual rights, moral duties, the proper scope of state action and the substance of constitutional law from a wide set of moral premises. Non-economic critiques of the Chicago School of law and economics often emanate from the proponents of moral theory, so it was only a matter of time before one of the Chicago protagonists launched a non-economic retort.

Posner's text, *The Problematics of Moral and Legal Theory*, covers so many varied topics dealing with legal theory—extending from theories on abortion, to the length of law school degrees, to the use of judicial precedent—that it is not

possible to provide an overview of the entire text and its main conclusions.

Speaking then in terms of themes and key messages, it may suffice to say that Posner addresses the thorny issue of whether 'philosophy' or 'science' should prevail in situations involving judicial discretion. Posner's conclusion, in the very broadest of terms, is that moral philosophy has 'nothing' to offer judges or legal scholars as far as adjudication and the formulation of legal doctrine are concerned. Posner's clear preference is to

adopt what he refers to as 'pragmatism', in the sense of methods of inquiry guided by 'social science and common sense'.

The argument for this claim is twofold. Firstly, moral theory does nothing to actually shape society's moral beliefs (except in those instances where we *already* agree with the moral theory due to our pre-existing political or religious beliefs), partly because Posner believes that only

passionate, emotional arguments such as those presented by a Martin Luther King or a Ghandi have the power to change people's convictions and beliefs, and partly because there is no such thing as a transcendent moral truth from which legal axioms can be derived.

Secondly, Posner considers that society would derive greater benefits from devoting resources to social scientific theories and to the collection of data that tells us how the legal system actually operates and with what costs and other consequences.

In many respects, Posner's philosophical perspective and outlook seems to rest more on his interpretation of history—where he has provided interpretations on diverse issues ranging from racial segregation to free love to biological differences between the sexes to the costs of the judicial system—rather than an application of economic or any other theory.

Whilst Posner does occasionally succumb to the frequent flaws of legal

philosophers—in so far as he draws unrealistic distinctions, relies on extreme examples and makes sometimes untenable comparisons—this may be necessitated to some degree by the academic terrain and the nature of the opposition he has sought to engage. Certainly, Posner was never going to convince legal philosophers of certain moral precepts by reference to comparative advantage, utility curves, economies of scale or other economic principles.

There are some surprises in this book for readers who have followed Posner's work over the years. It is interesting, although not necessarily inconsistent with his law and economics writings, that Posner applauds the efforts of sociologists and, in particular, their postmodern scepticism. In addition, and this is somewhat remarkable given Posner's generally conservative leanings in social and legal matters, he advocates a method of judicial interpretation that supports policy judgments grounded on facts and consequences rather than on concepts and generalities.

The 'pragmatic adjudication' approach to legal interpretation, of which Posner is a major advocate, requires the judge to 'come up with the decisions that will be best with regard to present and future needs' (p. 242). Sometimes this will involve the judge following *binding* precedents, sometimes it will not: 'Lay people think that the law is something written down in a book. Lawyers learn, in their very first year of law school, that the law is an inference from often ambiguous and even conflicting cases' (p. 292).

Many law and economics adherents will find the discussion of moral theory a futile aside to the real policy issues. Posner similarly considers moral theory to be a convenient means of disguising political preferences and predetermined moral outcomes (a charge that is often leveled at law and economics advocates!).

We may debate the merits of moral theory, but it is undeniably important that its detractors appropriately critique the foundations of moral theory. It is no longer possible to rely solely on an economic critique to demonstrate that moral theory

lacks, for example, an awareness of real life constraints and imperatives.

Posner is to be applauded for once again crossing a theoretical divide between two disciplines, this time, economics and moral theory. While it is unlikely that his critique will convince many adherents of moral theory, it is an important first step in that direction. Putting aside his obvious disdain for certain issues such as abortion and homosexuality—which colour his views on many moral theories—Posner manages to engage in a wide ranging discussion of moral theory in a consistent, non-economic framework.

What is most remarkable about this text is not so much that Posner has exposed the political bias of moral legal theory—which has long been suspected—but that he has performed that audacious feat while employing the language and customs of legal philosophers with only limited recourse to economic concepts or terminology. The resulting arguments are impressive.

Reviewed by Jeffrey Lawrence

Genome: The Autobiography of a Species in 23 Chapters

By Matt Ridley

Harpercollins, 2000, 344 pp, \$US 26.00, ISBN 0 521 56461 1

In Disney's recent film, *Mission to Mars*, explorers from Earth stand on the brink of First Contact. They have encountered, on the surface of Mars, a beacon of sorts, a strange signal broadcast over the radio, repeating over and over. The signal is in fact a code for a code. It translates into the spatial coordinates for a double helix, an extract from 'the Martian genome'. This is the recipe, written in DNA molecules, for building and maintaining a Martian. Guessing correctly at the intent of all this, the

explorers carefully modify the signal to encode 'the human genome' and re-broadcast it back. It is diplomate for we-showed-you-ours-now-you-show-us-yours.

Fair enough, I suppose, except for one small problem. As Matt Ridley remarks in his wonderful new book, *Genome*, '[t]here is no such thing as "the human genome". Neither in space nor in time can such a definite object be defined.' This is not to nit-pick Disney films, but rather to stress a point that goes to the paradoxical heart of the Human Genome Project itself. For as Ridley insists, the project is 'founded upon a fallacy.'

"The project's declared aim is to publish the average or 'consensus' sequence of 200 different people. But this would miss the point... Variation is an inherent and integral part of the human—or indeed any—genome.'

(p.145)

Our personal traits, both physical and psychological, and our genes co-vary, and it would be silly to ignore this information. It is almost as if you decided to map out 'the human literature' as some kind of average of 200 arbitrarily selected works. Once you had mixed in Shakespeare along with Dr. Seuss, some significant details would begin to blur.

Fortunately for us, Ridley's book is not only rich in delicious details—from the 'greatest wrong theory in history' to the

reason why grown men drink milk—but also skilfully avoids much of the geneticists' impenetrable jargon. The structure of the book is modelled after Primo Levi's *Periodic Table*, sparking off each chromosome in turn to describe some aspect of human nature, in much the same way as Levi drew connections between various episodes of his life and the chemical elements.

For example, the chapter on Chromosomes X and Y ('Conflict') focuses on sexually antagonistic genes, genes that are 'at an advantage in one sex and at a disadvantage in the other.' Follow the logic and you arrive at fruit-fly sperm that is 'so effective at manipulating female behavior' that it is 'effectively toxic: it could kill females.' If killer sperm is not enough, you can proceed to take your pick from several fascinating genetic theories of homosexuality.

I was also surprised to learn in Chromosome 9 ('Disease') that there is much more to blood groups than compatibility for transfusions. The various blood groups—A, B, AB, and O—confer resistance, to a greater or lesser extent, to diseases like cholera and malaria. For cholera, 'so powerful is this resistance in AB people that they are virtually immune'. Moreover, the level of resistance, together with the way blood groups are passed on to children, determines the relative frequency of the blood groups. Two A's beget an A, two B's a B, and an A and a B will have an AB child. Since A's are more resistant to cholera than B's, you would think that B's would die out, but they don't because AB's are the most resistant of all—and a quarter of the progeny of two AB's are B's. 'It is a world of strangely fluctuating fortunes. The very combination that is most beneficial in your generation guarantees you some susceptible children.'

This type of reasoning has broad implications. Indeed, as Ridley notes, 'it seems to be one of the commonest reasons that we are all so genetically diverse.' Which brings us back to the original question. 'When the Human Genome Project publishes the sequence of the typical human being, what will it publish

for the ABO gene on chromosome 9?' Any kind of averaging would clearly be wrong, 'because it is a crucial part of its function that it should not be the same in everybody.'

Ridley is a master storyteller who still marvels at his own tale—the fast unravelling mystery of our genes—and, soon enough, I found myself marvelling right along with him. His compelling narrative pulls us along through many twists and turns on an amazing scientific journey spanning almost two centuries—and counting.

Reviewed by Guy Calvert

The Second Wave of Law and Economics

by Megan Richardson and Gillian Hadfield (eds)

The Federation Press, 1999, pp141, \$45,

ISBN: 1-86287-316-X

The modern foundations of 'Law and Economics' can be found in articles such as Ronald Coase's *The Problem of Social Cost*, but it was not until the publication in 1973 of Richard Posner's *Economic Analysis of the Law* that the discipline became recognised as a discrete area of study with a distinctive methodology and framework. Since then, it is well known that in a short space of time, economic analysis of the law has yielded a cornucopia of absorbing and challenging scholarship.

Despite its relatively recent appearance on the legal landscape, there is already introspection within the field of Law and Economics and criticisms from beyond, leading to a questioning of its ideological underpinnings, reductionist hypotheses and confronting analyses. In particular, a lack of judicial acceptance has forced proponents of the discipline to revisit and rework that first

tsunami which emanated from the Chicago school. It is at this juncture that Megan Richardson and Gillian Hadfield find a context for their book *The Second Wave of Law and Economics*. Their purpose in collating a series of articles from eminent academics and jurists is to remedy a misconception that Law and Economics is still, and must forever be, linked to the idiosyncrasies and perceived limitations of the Chicago School.

The taxonomy employed by the authors is simple, yet effective. The book is organised neatly into three parts. The first section traces the background of the first wave of Law and Economics, moving on to discuss the elements of the second wave whilst pointing to differences between the two, and presenting arguments as to why the latter is more likely to find success.

The first wave is categorised by Michael Trebilcock and Gillian Hadfield as bounded in two ways. Firstly, its spheres of influence were always limited to the traditional analyses of markets in areas such as trade practices and securities regulation. Secondly, when examining areas beyond these traditional fields, the approach of lawyer economists in applying the efficiency and wealth maximisation criteria to issues such as abortion, euthanasia or human rights, failed to captivate or convert wide audiences outside the economics fraternity.

The background story to the first wave is supplemented by Trebilcock's candour in describing the qualifying factors surrounding the Pareto and Kaldor-Hicks criterion for welfare analysis and the limitations of the rational actor hypothesis. This tone is continued by Hadfield, who talks of the second wave being 'humbler', 'pragmatic', 'sceptical' and 'politically progressive'.

Despite this conciliatory approach, the authors maintain the belief that economics, with all its analytical tools and efficiency criteria, still has something powerful to offer. When decisions are made in the name of the public interest, economic analysis provides both sophisticated models and a rigorous framework to illuminate the attendant costs and benefits. What differentiates the second wave from the first, therefore, is an understanding that

economics is merely one lens through which we can view the world, and that it may be important to ensure that legal rules or policies satisfy other normative criteria as well.

The second part of the book provides three separate illustrations of how Law and Economics can be applied in the real world. Richardson discusses the effect on tortfeasors from liability for economic loss being imposed upon them. She then joins with Sgro in using the tools of economics to analyse pre-and post-contractual behaviour in a regime where specific performance or damages may be the default remedy. Finally, Williams and Tehan provide a review of efficiency outcomes from native title decisions, particularly in light of the resultant inalienable property rights which cases such as *Mabo* granted indigenous peoples.

The final section of the book presents judicial perspectives of the future and utility of Law and Economics in the courts. Justice Michael Kirby of the High Court of Australia and Sir Ivor Richardson, President of the Court of Appeal in New Zealand, provide a number of examples of cases and areas where legal argument would be effectively supplemented by economic analysis.

An important issue, which is alluded to in the introduction of the book, is the phenomenal growth and acceptance of the first wave of Law and Economics in the United States and its failure to take hold in Australia and other common law countries.

Hadfield and Richardson maintain that this is largely due to the fact that first wave writers did not comprehend that 'the richness, the complexity, the texture, and insistent importance of legal problems will not submit to economic analysis'. They argue, however, that economics in the form of the second wave will become accepted by an Australian audience when it 'harnesses, rather than fights, the multiple movements in law'. I agree that if the second wave writers can achieve this objective, it will certainly provide compelling reasons for the judiciary to give consideration to their work, but one cannot

talk of its inevitable acceptance without a deeper exploration of this issue.

As Justice Kirby points out, the use of the Brandeis brief in the United States has permitted interveners to place before the courts information about the potential social and economic impact of judicial decisions for the past ninety years.

By comparison, the hesitation of the judiciary in Australia to make use of this right of intervention (with the associated increased time and costs), may be an important reason for only a partial acceptance of economic analysis in the courts. In other words, the first wave may well have been effective if the curial process in Australia more closely reflected that of the United States. Alternatively, systemic change to court procedures may be a necessary precondition before any type of economics is utilised by Australian courts.

Even when the traditional procedures of presenting argument can be used to deploy economic analysis in the courts, there are further barriers with which to contend. Sir Ivor Richardson correctly asks whether litigants will be prepared to bear the financial burden of having advocates prepare and present lengthy cost-benefit analyses, particularly when the issues addressed go far beyond their immediate interests in the case. Do advocates or judges have the skills to develop, understand and properly analyse such economic arguments? Was Sir Anthony Mason's rebuke of Law and Economics (irrespective of its accuracy) indicative of Australian judges remaining inherently sceptical about the instrumental and normative implications of economic analyses, seeing such argument as better dealt with by the political process?

If any or all of these questions are answered in the affirmative, what use will the second wave be? While the 'new' Law and Economics may be more congenial in approach, and worthy of adoption from an academic viewpoint, there is no guarantee that lawyers beyond Justice Kirby and Sir Ivor Richardson will take up the call.

The scholarship presented in this book is by no means groundbreaking, but it was clearly not the intention of the editors to

make it so. The limitations and benefits of economic analysis are canvassed and illustrated succinctly and the various articles provide a useful insight into the developments of Law and Economics over the past three decades, and its future role in the judicial decision-making process. However, whether a change in style for lawyer economists will be sufficient to ensure the impact of the second wave is more than a ripple along the Australian coastline remains to be seen.

Reviewed by Michael Rush



The Vices of Economists—The Virtues of the Bourgeoisie

by Deirdre McCloskey

Amsterdam University Press,

Amsterdam, 1997, 135 pp, ISBN 90 5356 244 3.

University of Michigan Press, 2000,
ISBN 905362338, paperback
forthcoming

There is an abundant literature critical of the methodological and epistemological underpinnings of modern economics. What is perhaps less widely appreciated is that some of the best of this literature comes from within the classical liberal tradition. Economics and classical liberalism are often so closely identified that those outside the liberal tradition have difficulty separating them. This results in impoverished criticism of both.

A major concern of those in the classical liberal tradition has been the retreat of modern economics, particularly in the academic world, into an excessive commitment to formalism. In so doing, the discipline is becoming increasingly distant from substantive scientific or policy concerns.

Deirdre McCloskey's book is an incisive and polemical contribution to this critique, based partly on some of her earlier work. McCloskey sets out to

demonstrate that as a scientific field, 'economics can be shown to be wrong from arguments with which everyone in the field would agree' (p. 15). However, as McCloskey readily concedes, these arguments might be accepted individually, but there is a great deal of reluctance to accept their wider implications for the discipline. McCloskey identifies three 'vices' which she argues have come to characterise contemporary academic economics. These are the attachment to statistical significance exemplified by Lawrence Klein, the attachment to blackboard proofs exemplified by Paul Samuelson, and the interest in social engineering exemplified by Jan Tinbergen.

McCloskey devotes most of her discussion to the Kleinian vice of statistical significance. Her complaint is that modern economics addresses substantive questions by treating them essentially as sampling issues. The problem is that addressing the sampling question does not settle the substantive scientific or economic question. Economists understand the difference between statistical and economic or scientific significance in principle, but do not acknowledge it in academic practice. McCloskey poses the question, 'suppose you had enormous sample sizes, so that the sampling variability of your estimates approached zero. All coefficients would be significant... Now ask yourself: what are the economically important variables? ... The answer is, you still don't know' (pp. 31-2). McCloskey argues that most substantive questions in economics concern what magnitudes are economically interesting, but that significance testing cannot answer these questions. They are, ultimately, questions for economic judgment.

Economics is not alone in this regard. As McCloskey points out, the same mistake is often made in the disciplines of psychology and sociology (p. 40). But the problem is particularly acute in economics. McCloskey refers to her earlier work in arguing that most of the work published in leading economics journals makes inappropriate use of claims of statistical significance to argue for

substantive policy conclusions. The reason the Kleinian vice has become so rampant is that many substantive economic questions do lend themselves to some form of quantification (contrary to the claims of academic formalists such as Debreu). Together with the dramatic decline in the cost of computing power since the 1970s, this has greatly facilitated empirical work and sophisticated statistical testing. But McCloskey argues that such testing rarely proceeds beyond the question of statistical significance to address substantive scientific or economic issues. The problem is compounded by the requirements of academic publishing. McCloskey sees all three of the vices she identifies as 'tools for generating publishable papers. . . The published papers then generate job offers for the authors. Statistical significance makes for careers in science. That the careers do not generate actual scientific findings, or make economics move forward as a science, does not seem to worry the average economist' (p. 51).

The second vice is what McCloskey terms the Samuelsonian adherence to blackboard proofs. This vice involves elevating what are essentially mathematical problems to the status of substantive scientific questions. Thus, while 'in the 1960s most dissertations for the PhD in economics were empirical... by the 1990s, most PhD dissertations in economics ring the changes on someone else's mathematical model. They consist of theoretical essays in search of a point' (p. 65). McCloskey points to the 'low prestige of axiom and proof in real sciences' and anecdotal evidence that mathematics has assumed an even greater prominence in economics than in disciplines such as physics. She argues that economists 'come to view the mathematics as an end in itself, not a tool' (p. 73). McCloskey gives the example of a Chicago PhD student who, having written a dissertation weakening one of the axioms of Arrow's Impossibility Theorem, was unable to give an oral defence of the scientific uses of this work (pp. 76-77). McCloskey rejects Debreu's claim that economics is a non-experimental field, making economists wholly dependent on theoretical

argument. McCloskey wants to see a return to empirical economics, minus the Kleinian vice of over-reliance on significance testing at the expense of economic judgement.

McCloskey gives a stylised depiction of the development of many research programmes in economics, in which an unbounded sequence of assumptions leads to an unbounded sequence of conclusions: 'take any recent "finding" from the blackboard. I predict that if the "finding" is thought to be important enough, then within a short time there will appear a paper by Economist Number Two showing that by making an alternative assumption A the "finding" is reversed. And shortly afterwards a paper will appear. . . in which a set of assumptions A will reinstate the old conclusion. And so forth' (p. 89). McCloskey again blames the dominance of such formal approaches on academic convenience: 'mathematics is easier to teach than skill at actually using economic thinking for science' (p.82).

For McCloskey, the greatest of the three vices is the interest in social engineering, exemplified by Jan Tinbergen, although McCloskey's analysis in this regard is somewhat brief. Following the Austrian School, among others, McCloskey notes that 'the reflexivity of economics sets stringent limits on what we can predict and control' (p. 103). Here, McCloskey relies on some familiar arguments about the limitations of economics as a predictive tool. However, McCloskey's larger objection to social engineering is that it is illiberal. McCloskey's argument is a short and familiar one: that maximising utility is not necessarily consistent with maximising happiness or freedom. McCloskey sees this and the other vices of economists as symptomatic of modernism and wants a return to the 'bourgeois virtues' of the Scottish Enlightenment.

McCloskey's brief criticisms of economics as promoting the practice of illiberal social engineering raises some interesting issues, although she fails to draw out some of the implications. Classical liberals have been extremely

prominent in the development of modern economics. A significant proportion of the Nobel Prizes in economics in the post-war period have been awarded to those who have identified explicitly with the classical liberal tradition.

But it is also noteworthy that many of these same economists are better known for their contributions to political theory and philosophy than to economics. For example, the work of Hayek on trade cycles and Buchanan in public finance, for which they were awarded their Nobel prizes, is less well known than their contributions to the political theory and philosophical underpinnings of a free society. They have also been notable critics of over-adherence to formalism in economics and the role of the social sciences in encouraging attempts at social engineering.

An important implication is that much modern economics, especially at the academic level, proceeds from methodological and epistemological assumptions that are in conflict with key elements of the classical liberal tradition, such as adherence to methodological individualism and value subjectivism.

The identification of liberalism with economics is often at odds with the views of liberal economists. If liberalism has come to be overly identified with economics, this has more to do with their prominence in the field than with their actual intellectual contributions, which are often at odds with the mainstream of the discipline. While classical liberals often use economic arguments, they are for the most part not guilty of the methodological or epistemological errors typical of their more mainstream professional colleagues.

Many of the critics of liberal policy prescriptions aim their criticism, either directly or indirectly, at economics and its use in public policy. In so far as those policy prescriptions proceed from narrowly economic assumptions, this may be appropriate. It is interesting, however, that many of these criticisms often proceed directly from the pages of neo-classical welfare economics. For example, arguments about public goods and externalities, often considered to be fatal criticisms of 'free market' economics, are usually dependent on essentially the same neo-

classical assumptions. As McCloskey shows, this can make for particularly fruitless arguments over alternative policy prescriptions. These are not always the assumptions classical liberals make in applying economics to questions of public policy. Much of the criticism directed against both classical liberalism and economics in this regard is thus misplaced or beside the point.

What makes the classical liberal approach to economics distinctive is that, unlike many other approaches, it takes individual values seriously. Of course, this is exactly the criticism those outside tradition make of economics taken as identical with liberalism: that it does not pay due attention to values (although usually not individual values). The work of McCloskey and others in the classical liberal tradition provides important, although neglected, answers to many of these objections.

Some readers may be annoyed by McCloskey's more recent style. In particular, she has taken to wrapping some of her arguments in gender stereotypes and admonishing her academic colleagues using her 'Aunt Deirdre' persona. This reviewer found the latter quite amusing. It certainly adds to the provocativeness of her argument, however, it is more questionable whether it adds to its persuasiveness. But McCloskey knows the relevant academic 'speech community' better than I do.

Reviewed by Stephen Kirchner

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