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Peter J. Boettke

George Mason University

Christopher J. Coyne

Marquette University

John B. Davis

Marquette University, john.davis@marquette.edu

Francesco Guala

University of Exeter

Alain Marciano

Université de Reims Champagne Ardenne

See next page for additional authors

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Authors

Peter J. Boettke, Christopher J. Coyne, John B. Davis, Francesco Guala, Alain Marciano, Jochen Runde, and Margaret Schabas

Marquette University

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Where Economics and Philosophy Meet: Review of the Elgar Companion to Economics and Philosophy with Responses from the Authors

Peter J. Boettke

George Mason University Hampden Sydney College

Christopher J. Coyne

Marquette University

John Davis

University of Amsterdam

Francesco Guala

University of Exeter

Alain Marciano

Université de Reims Champagne Ardenne

Jochen Runde

Judge Business School, University of Cambridge

Margaret Schabas

University of British Columbia

Although Adam Smith's 1776 *Wealth of Nations* is often cited as marking the birth of economics, it was really not until after the second world war that economics became the distinctive, more or less unified, and largely separate discipline summarised in the textbooks of today. Even a mere fifty years ago, it was possible for the intelligent reader to move with relative ease between economics on the one hand and political economy, sociology and social theory, psychology and philosophy on the other. This is now no longer the case, and most young economists are taught to think of their discipline, not primarily in terms of the particular 'economic' social phenomena it was once taken to be about, but as a sophisticated and largely self-contained analytical approach to the investigation of social phenomena of any kind.

Even so, economics has never been able to separate itself entirely from its sister disciplines, even at the high tide of mathematical economics and positivism during the 1970s and 1980s, and many of the most active new areas in economics currently involve some form of boundary crossing (e.g. experimental economics, neuroeconomics and computational economics to name just three). With respect to the philosophy of economics in particular, the last fifty years or so have seen a steady expansion in scholarly investigation into different connections between economics and philosophy, with the emergence of new journals, professional associations, research networks and the like. There has been a great deal of work on epistemological questions in the wake of the decline of positivism, on boundary issues and the question of whether or not economics constitutes a science, and on the rhetoric of economics, ethics, value and, latterly, the ontology of economics ([Hands, 2001](#)).

It is against this background that, in 2004, three of us published the *Elgar Companion to Economics and Philosophy* (Davis, Marciano and Runde, 2004, henceforth the *Companion*), an edited collection aimed at documenting the current state of play in three important areas of the philosophy of economics:

1. Political economy as political philosophy;
2. The methodology and epistemology of economics; and
3. Social ontology and the ontology of economics.

The various authors represented in the volume were chosen on the basis of their having made distinctive contributions in these general areas, and they were asked, not only to survey the state of the field as they saw it, but also provide statements of their own positions and their perspectives on the field in question and its possible direction of development in the future. Imposed upon them, therefore, was the need both to mine the philosophical subject matter of economics and also to stand back from it to assess it in a manner that would do justice to the complex interrelations that exist between these different points of access as they saw them at the time. In effect, then, the authors were asked to anticipate the volume's appearance and its reviews, and were thus the first voice in the dialogue recorded below.

The *Companion* was subsequently the subject of a session at the meeting of the History of Economics Society, held at the University of Puget Sound in Tacoma, Washington between 24–27 June 2005, and the present 'review' comprises revised versions of the contributions to that session. The structure of the session followed the structure of the book, with separate commentaries on each of the three sections listed above, from people from a range of different perspectives: Peter Boettke and Chris Coyne (PB&CC, representatives of the Austrian School of economics), Margaret Schabas (MS, a philosopher and historian of economic ideas) and Francesco Guala (FG, a philosopher of science). The commentators were asked to concentrate on general issues that emerged in the papers comprising the section of the book they had responsibility for. Each of the commentaries

was then followed by a brief reply from the editor who had principal responsibility for that section in the book, and these responses are also reproduced below.

1. Political Economy as Political Philosophy

1.1. Comment: Modern Political Economy (PB&CC)

Political Economy has made a comeback. At the turn of twentieth century, the term political economy began to be purged from the scientific discipline of economics. This started to change with the economic analysis of politics in the 1950s and 1960s, when the tools of economic analysis began to be employed to analyse voting behaviour, bureaucracy, public policy decision making and constitutional choice. The new field of public choice political economy was born and in 1986 James M. Buchanan was awarded the Nobel Prize for his contribution in founding this line of research.

Although rational choice politics has become a major research field in both economics and political science, there was always something a bit odd about the research programme. It was born out of a frustration with an institutionally antiseptic theory of the market economy, politics and public policy. But when pursued to its extreme, it would once again result in an alternative version of an institutionally antiseptic state of affairs.¹ Political economy pursued via rational choice modelling would become what it had hoped to escape: a theory in which the behavioural assumptions of the model do the heavy intellectual lifting. Only when the programme of rational choice was pursued in a watered-down manner by weakening the strict demands on rationality associated with the mainstream, did political, legal, social and economic institutions again begin to matter in determining performance.² Rational choice as if humans were automatons suggests a view of the world as populated by programmable robots but a rational choice approach that emphasises the human element could be used to analyse a world populated by actors with all their imperfections. Such a world is less predictable than the automaton world but it is also one that can say something about beliefs, ideologies and norms.³

In this light, Part I of the *Companion* makes an important contribution to modern political economy by exploring the evolution of the behavioural assumptions employed within the discipline of economics. The authors adopt a looser notion of rationality than the conventional one, emphasising agent purposefulness and institutional contingency. Specifically, the emphasis on the interplay of cognition, economy and society in the essays by Marciano (Chapter 2), Heap (Chapter 3) and Frey and Benz (Chapter 4) highlight the importance of moving beyond the standard model of the perfect maximising agent. We seek to trace the implications of these essays for modern political economy.

It is our contention that the social science landscape can be depicted as follows:

	CLEAN EMPIRICAL WORK	DIRTY EMPIRICAL WORK
THIN THEORETICAL DESCRIPTION	Economic theory and econometrics	Analytic Narrative Political Economy
THICK THEORETICAL DESCRIPTION	Sociological and Political science econometrics	Anthropology, Cultural Sociology, and Institutional Political Science

(1)

[Methodological Predispositions and the Social Science Landscape]

In our view, the domain that modern political economy is best suited to occupy is the upper right quadrant. Economists have traditionally approached their subject matter by providing a parsimonious theory and then confronting that theory with as clean an empirical test as possible. The problem with this approach is that by stressing the universal in all human behaviour the specific is lost, whereas in asserting that all behaviour is specific as in traditional anthropology, the ability to communicate and understand across history and culture is lost. Thus thin/clean and thick/dirty will often fall short of providing fully satisfactory explanations of the world. But we contend that somewhere between the economist's penchant for the general (the thin and clean) and the anthropologist's demand for respect for the specific, there lies an approach which maintains the analytical structure of the economic way of thinking but respects the unique institutional arrangements that structure the rules of the game and their enforcement in any particular historical setting. We believe that this is the intellectual space where progress in modern political economy will be made in the coming decade.⁴

Ludwig von Mises analysed the implications of the dual assumptions of government agent omniscience and benevolence, and discovered that such assumptions led to the conclusion that the State was in a better position to succeed as compared to erring individuals ([1966](#), p. 692). Mises's point was that these assumptions had the affect of assuming what it is that the theorists of government planning had to prove, i.e., that the State was in fact in a position to assess the economic value of alternative productive activities rationally. In contrast, von Mises argued that the institution of state ownership and centralised economic planning would in fact run counter to making such assessments in an economically rational manner. James [Buchanan \(1969, pp. 77–92\)](#) followed this insight up by emphasising that the opportunity costs that entrepreneurs face within a market context are entirely different from the opportunity costs faced by those within a bureaucratic context. Both von Mises and Buchanan therefore called on people to question the behavioural assumptions of omniscience and benevolence. A political economy in which the actor is neither simpleton nor shining genius, neither pure sinner nor pure saint, would instead have to come to grips with how alternative institutional configurations impact on economic arrangements and performance.

As long as political economists are wedded to the behavioural assumptions of neoclassical economics, the persistent and consistent application of that model will result in analyses that neglect the very institutional arrangements that were to be studied by political economy.⁵ Only by moving away from model of the economic actor as a perfect maximiser towards one that adopts a more humble view of man's cognitive capacity, will the questions of how cooperation and coordination emerge in social intercourse ever be adequately addressed. The tendency has been to divide the discipline of economics into theory and empirics (whether historical or statistical). We contend that what we have learned in the evolution of modern political economy is that however important pure theory may be, the most pressing questions are to be found in the institutionally contingent theory that constitutes political economy. The shift forces the social scientist to move from a model in which the behavioural assumptions do most of the work to one in which most of the work is done by recognising the institutional specifications within which actors interact.

This theory is broadly conceived to include not just public choice, but also law and economics, economic sociology, new institutionalism, etc. (see the essay by Hodgson, Chapter 5). It is the evolution of institutions (including tinkering at the margins) and how these institutions provide the necessary filters in social intercourse that lead to either the promotion of social cooperation or inducement of social conflict that determines the state of affairs in any given society (see the essay by Vroman, Chapter 6). Institutions, not behavioural assumptions, explain how order emerges.

The purging of political economy at the beginning of the twentieth century was driven by methodological transformation of the social sciences in the philosophical realm, the rise of socialism and fascism in Europe and

the near collapse of the capitalist system in the Great Depression in the US and UK. Economics as a discipline was expected to provide answers to these social questions and economics as social engineering was born. The long history of economics as social engineering did not end with the fracturing of the Keynesian consensus in the 1970s or the collapse of communism in the 1980s. The task of economics as social engineering shifted, but the basic idea of economists at the levers of public policy to right what is wrong was maintained.

The difficulties of post-communist transition and fostering development in the third world started to raise doubts in the ability of economics to provide appropriate guidance on the (re-)construction of economic systems. In order to be relevant, the discipline had to be broader in orientation and take into account political, legal, historical, social and even religious factors. In the late twentieth century and early twenty-first century political economy was reborn. As Marciano's essay (Chapter 2) highlights, we have come full circle to the ideas of Smith and Hume, a more humble political economy, but perhaps a wiser one.

The issue of distributive justice (see the Fleurbaey essay, Chapter 7) perhaps best highlights the relationship between positive economic analysis and normative political economy. Economics proper cannot answer the question of whether profits are deserved or not. However, it can inform regarding the consequences of the answer that others provide to this question. [Hayek \(1976\)](#) argued that questions of distribution were often misplaced in political economy because they assumed a fixed pie that was being divided up according to rules of distribution that were judged as fair or not. Hayek's objection was not that fair divisions are not desirable. His criticism was that the rules of fair division are not neutral with regard to the incentives and information associated with production. The size of the pie being divided depends on how we divide the pie. As Buchanan emphasised in his work, public policy cannot be thought of as one of distribution, but is instead always a question of the choice over rules of the game that engender a pattern of exchange and distribution.

We have argued elsewhere that Buchanan is largely responsible for the rebirth of political economy as a scholarly agenda ([Boettke, 1998](#)). Buchanan's work in public choice is often interpreted as the quintessential case of economic imperialism. However as Sen ([1999](#), p.263) has pointed out, Buchanan should not be identified with economic imperialism. In fact, Sen states that Buchanan 'has done more than most to introduce ethics, legal political thinking, and indeed social thinking into economics' (quoted in [Swedberg, 1990](#), p. 263). Crucial to understanding Buchanan's system of thought is the distinction he made between politics and policy.

Politics is about the rules of the game, where policy is focused on strategies that players adopt within a given set of rules. Questions about what are 'good' rules of the game are in the domain of social philosophy, whereas questions about the strategies that players will adopt given those rules is the domain of economics, and it is the play between the rules (social philosophy) and the strategies (economics) that constitutes what Buchanan refers to as constitutional political economy. No assessment of modern political economy as political philosophy can proceed without recognising the recurring theme in the work of Buchanan, Coase, Hayek, North and Vernon Smith on how the rules of the game and their enforcement dictate how the social game of life will be played.

1.2. Reply (AM)

In their rich and stimulating comment, Peter Boettke and Christopher Coyne (PB&CC) observe that 'political economy has made a comeback'. I take this to mean that, as is argued in the first section of the *Companion*, the distance that separates economics in its recent developments from its foundations in writings of the classical political economists has decreased over recent decades. That is to say, economics might once again be considered as a political or social philosophy that recognises that 'the rules of the game and their enforcement dictate how the social game of life will be played'.

It is interesting that (PB&CC) do not refer to the rather usual 'new' political economy but describe the revised version of political economy as 'modern' political economy – a term already used, for instance and amongst

others, by [Ruskin \(1862\)](#) or [Sidgwick \(1885\)](#) and that can be traced throughout the twentieth century. 'Modern' political economy is supposed to reflect a genuine return to the roots of the discipline. How does it differ from the 'new' political economy? The answer is that, despite its claims about reviving the heritage of David Hume and Adam Smith, new political economy remains closer to neoclassical economics than to classical political economy. The reason for this, as noted by [Wiseman \(1991, p. 154\)](#), is that new political economists 'are as yet reluctant simply to abandon the analytical aides furnished by neoclassical economists'. From this perspective, Buchanan's writings provide a clear example of the tension that frequently characterises the writings of new political economists. Thus while he sometimes argues that one has to distinguish 'between questions that can be analysed by treating the actor as equivalent to a rat (where neoclassical economics is most appropriate) and those that require the actor to be viewed as a creative individual (where the subjectivist economics of [Shackle \(1972\)](#) and the Austrians is more appropriate)' (as quoted by PB&CC in [Section 1.1](#)), he nonetheless maintains that 'there is a powerful argument that suggests the appropriateness of something akin to the *homo oeconomicus* postulate for behaviour' ([Buchanan, 1990](#), p. 15); see also ([Buchanan, 1987](#)). What Boettke and Coyne describe as Buchanan's difficult trick 'of appearing both a hard-boiled rational choice theorist and a social philosopher and political economist', clearly illustrates what [Wiseman \(1991\)](#) calls the 'uneasy cohabitation' between new political economy and the dominant paradigm.

Undoubtedly, then, the difference between new and modern political economy is closely connected with their respective conceptions of economic man. Whereas proponents of new political economy remain committed to the expected utility maximiser of mainstream economics, proponents of modern political economy should refer to a model 'that adopts a more humble view of man's cognitive capacity' (PB&CC, in [Section 1.1](#)) – a conception closer to that of Hume or Smith; see [Marciano \(2005\)](#). Thus, the idea that what makes 'modern' political economy distinctive is that it takes into consideration the rules in which 'the social game of life' is played is only partially true. The other characteristic of a genuine revival of the 'spirit' of classical political economists is that it makes room for human (radical) subjectivity and limited cognitive capacities, and acknowledges that individuals play 'the social game of life' as *homo sapiens* rather than merely *homo oeconomicus*.

This leads to a second set of remarks about PB&CC's distinction between politics being 'about the rules of the game' and economics being about 'strategies that players adopt within a *given* set of rules' (emphasis added). My own view is that this perspective tends to equate 'modern' with 'new' political economy. In fact, the assumption that 'economics' consists in the analysis of strategic behaviour 'within a given set of rules' indeed characterises new political economy (public choice, law and economics and even constitutional economics). This is clearly reflected in the standard distinction between constitutional behaviour – the definition of rules – and post-constitutional behaviour – behaviour within given rules. It then comes as no surprise that the kind of models advanced by new political economists should be much the same as standard neoclassical models that describe optimising individuals as choosing their most preferred element from a set of *given* alternatives or from a set of alternatives delineated by *given* rules. To this extent new political economy simply follows standard neoclassical economics in assuming away the complexities of human subjectivity and cognitive limitations.

The trouble is that these complexities cannot easily be ignored, that individuals use their personal experiences and limited capacities to understand the complex and ambiguous information that they receive from their environment, and that their behaviour thus rests on the way that they subjectively represent and imaginatively *create* their environment. Therefore, in contrast to what neoclassical and new political economists argue, individual behaviour is not so much about choosing within the set of alternatives defined by given rules, as it is about formulating (through subjective evaluation) alternatives and opportunities, thereby often altering the rules that frame them. In other words, playing the 'social game of life', individuals modify the rules that delineate the set within which they are assumed to behave. They do not (only) 'react' to constraints but (also) 'create' them (on reactive and creative choices, see [Buchanan \(1989\)](#)). This perspective thus certainly suggests

that institutions should be designed in order to let individual behave creatively ([Buchanan and Vanberg, 2002](#)). In particular, one evidently important type of creative behaviour is that of entrepreneurs. It can be argued that 'genuine' entrepreneurship is not so much about *discovering* possibilities that already (although implicitly) exist than it is about totally creating new and (for that reason) totally unpredictable alternatives ([Witt, 2004](#)). If so, attempts to modify the rules of the game should be envisaged as reflecting the positive creativity of free individuals rather than being described as mere opportunism. Accordingly, questions arise about the quality of the new rules that are created. This means that, from the perspective of economics as social philosophy, questions about what are "'good" rules of the game' and those 'about the strategies that players will adopt given those rules' do not belong to two different domains that should be analysed separately. Rather, they form the two branches of one discipline, namely modern political economy. To revive the 'soul' or 'spirit'— words of the utmost importance —'of classical political economists', as Buchanan himself notes, implies the development of analyses based upon a 'vision' of what a good society is or what institutional arrangements ought to be ([Buchanan, 2000; 1990](#), pp. 16–8).

Once again, these questions all result from a change in the behavioural assumptions used by economists. The 'spirit of classical political economy' will revive only to the extent that they are taken into account.

2. The Methodology and Epistemology of Economics

2.1. Comment (MS)

First, I commend the editors for compiling a very balanced overview of the subject of Economics and Philosophy. My job is somewhat harder than the other two reviewers, since there is already an *Elgar Companion* on the Methodology of Economics, and, as that volume makes manifest, there are many topics that have been excluded here. I was prompted to address the question of balance. I think the selection of topics is perfectly fine and representative, as samples go, but we all have our favourites. Of the ones that spring more readily to mind, and are not included here, are idealisation, laws and experimentation. An assessment of econometrics is notably absent. Reductionism as a general theme is analysed in some detail in Harold Kincaid's essay, but the more specific problems of bridging micro- and macroeconomics are central and specific to the discipline and would have benefited from more direct attention.

The section assigned to me also conjoins methodology to epistemology, and three of the entries (chapters 10–12) aim toward that end more than the purely methodological. None of them, however, truly grapples with what is known, or what can be known, about economic phenomena, to the same extent as some of the entries in the section on Ontology grapple with fundamental questions of existence. If anything, it is the latter set that brush up against the epistemological. Nor should one expect these topics to be neatly cordoned off from one another but it is at least worth mentioning that there is more to be found on methodology and epistemology in Part III. I will cover the entries in the order given by the book and then make some general comments on the set as a whole.

Roger Backhouse provides a solid and accurate account of the rise and fall of Lakatosian readings of economics but with the aim of salvaging the approach. He argues that it is incorrect to merge Lakatos with Kuhn or Popper, since Lakatos comes out of a very different set of traditions, namely that of philosophy of mathematics, Hungarian communism and, reaching further back, Hegelian-Marxism. While it is always the case that under the magnifying glass one can find unique attributes for anything, I think that compared and contrasted to other schools of interpretation, Lakatos is still rightfully grouped with Popper, and Kuhn for that matter. Popper's interests in physics, his own Austrian-Marxist heritage and, as it happens, Kuhn's position in Cold War America, suggest that future historians will lump them together with impunity. The questions they raised and attempted to answer, particularly the one of combative research schools, are here to stay. The one qualification to note is

that the lesser attention paid to the sociological dimensions by Popper and Lakatos (nature could shout back unequivocally for them) means that Kuhn's stock is rising, and to some extent this is reflected in the fact that all but one of the entries in Part II refer to his seminal book.

Wade Hands makes the solid point that the constructivist position of the Strong Program, namely that science is partly or wholly the product of social conditions, is to a large extent unexciting when applied to economics. We expect, after all, that economists are reflecting on the economic and social conditions of their time. Ricardian economics is partly about the recently industrialised British economy, just as Keynesian economics reflects the downturn of the 1920s and early 1930s. Moreover, as Hands acknowledges, much of the SSK (sociology of scientific knowledge) literature directs the sociological toward the economic. Whether neoclassical, institutionalist or Marxist, 'some version of (disciplinary) economics informs (implicitly or explicitly) much of the work of those writing in SSK' and is thus more accurately ESK, the 'economics of scientific knowledge' (p. 205). There are still many underdeveloped veins to tap however. Hands mentions in passing the difficulty of delineating 'the economy', and even more problematically, of what constitutes the scientific understanding of it. The difficulties are compounded once one is reminded that there are important physical and biological dimensions to economic phenomena, whether it be the role of time or the rate of population growth. The path-breaking work of SSK seems to have broken down certain conceptual barriers but, as Hands notes at the end of his fine account, there is much work to be done in sorting out the myriad number and circularity of factors that shape our knowledge of 'the economy'. Certainly any simple linear model has to be set aside.

Drucilla Barker makes a compelling case for the significance of feminist economics, and points to a burgeoning literature that addresses income discrepancies, unpaid household labour and the significance of the marketplace in reproduction (e.g. surrogate mothers). She also leaves the reader with the impression that things are still at a very preliminary stage in this particular subdiscipline. This is partly because engaging in feminist economics appears to require an ability to transcend the privileged state of a trained (and hopefully employed) economist. Barker points to the need to move away from liberal feminism that tends to pass over all too quickly the greater oppressions of race, class and nation. 'Feminist economists need to theorise a multiplicity of oppressions and examine the ways that subjectivities are produced and shaped by various, often contradictory, discourses, institutions, and the power relations inherent within them' (p. 228). I am far from persuaded by this claim. Mainstream economists do not seem at all bothered to apologise if and when they direct their attention to poor regions of the world, or the unemployed. If feminist economists are to have an impact on the mainstream accounts, then they can be equally culpable of belonging to the privileged. True, to do feminist economics means to question the very categories by which we organise our world, divide it into rich and poor, male and female for example. In this sense, the poststructuralist movement she describes that queries the very language used in the discourse and repudiates any pretence at objectivity, seems of value. At the very least, it seems, gender as a useful category is here to stay.

Because I know far less about postmodernism and the role of rhetoric, I will have to pass on commenting on Robert Garnett's detailed entry, but would like to add that I commend the editors for its inclusion. Deirdre McCloskey appears to be the main figure since the early 1980s, and the fact that she has also embraced a feminist stance, suggests a convergence there as well.

Marcel Boumans delves into interesting parallels between the use of models in physics and in economics. He endorses the position recently advanced by [Mary Morgan and Margaret Morrison \(1999\)](#), namely that models are like material objects (the analogy is still somewhat murky), and in that sense can be used as tools and modified as such. Understanding a particular phenomenon in science has come to mean building a model. This, Boumans suggests, was as true in the work of Galileo or Kelvin, as it is in Irving Fisher or Jan Tinbergen. What we have in this essay is a very expansive reading of the term model, such that they proliferate throughout the history of science. I myself would welcome a more fine-grained taxonomy, one that separates and distinguishes

the models, say of Samuelson or Akerlof, from game theory, or the work of David Ricardo. The ascription of models is ubiquitous in contemporary economics. If philosophers can help in rethinking basic assumptions, it would make sense to take issue with these ascriptions and develop more discriminating terms. But there is also merit in seeing the robustness of this conceptual tool as a central element to economic discourse and one can only welcome the recent contributions.

The piece on formalism co-authored by Peter Kesting and Arnis Vilks argues that formalism is a much broader category than mathematics, and could be nothing more than the use of a proof or logical derivation with or without symbols. They suggest, therefore, that formalism reaches far back in time, from Cantillon and Quesnay through to Ricardo and von Thunen. The crowning achievement, however, is Gerard Debreu's work of 1959. Where I think they go astray is in suggesting that formalism is synonymous with models. Again, models are a subset, but by no means exhaust the content of economic theory. I would submit that there are very few explicit or implicit models in economics before the twentieth century. In short, it is one thing to lay down postulates or propositions, and another to develop a model, and this is in consonance even with Boumans' more capacious construal of the term. I think much can be gained if the word model was not so diluted but, rather, given a more specific range of meanings.

Harold Kincaid's entry on methodological individualism is very thorough and probing. He helps us to see the many different meanings employed in the commitment to methodological individualism as well as that there is no simple dichotomy between society and individuals. The advent of game theory, he points out, brings further complexity to the topic insofar as it posits multiple equilibria, and subpersonal strategies that appeal to ideal not real individuals. One conclusion he argues well is that the prospect of a successful or useful reduction to an individualist ontology is an empirical matter, and at this point there are only promissory notes. One of the only key issues here that Kincaid does not consider is the topic of personal identity that might make the individual a robust category in the first place. Because this entry spills into ontological issues, it provides a good segue into the next section of the *Companion*.

Now for some general comments. The methodology of economics is a burgeoning field, perhaps not the most exciting, but still one worth the effort. These pieces help us to see the need to do much more work, to unpack the extension of the term model, to reflect more deeply about how mathematics is or is not coextensive with formalism, and to compile case studies on the success of reductionist explanations from groups to individuals in economics. The epistemology of economics is at an even more preliminary stage, insofar as the generation that cut its teeth on Lakatos is only now acknowledging its demise, or those imbued with SSK are only now grappling with the murky lines between the social, the economic, and the natural. Add a dash of feminism or postmodernism and one has quite a heady brew of topics and problems.

In the very fine and thorough Introduction to the volume, the editors make a bold claim about the drift of methodology since the 1970s: 'While economists remained attached to traditional logical positivist methods and the empirical verification of theories, economic methodologists almost universally rejected them' (p. xvii). There is a large grain of truth to this, to the failure of methodologists to have their preaching penetrate the profession of economists, many of whom are still in thrall of Milton Friedman. While this may be the common plight of philosophers, one can only hope that this volume constitutes an important step in dissolving the divide.

2.2. Reply (JD)

Margaret Schabas' comments on the Methodology and Epistemology section in the *Companion* are most welcome, because she brings a philosopher's perspectives to chapters written almost entirely by economic methodologists. Thus, interestingly, she sees less emphasis on classical epistemological issues and more emphasis on characteristically methodological issues in chapters 10, 11 and 12, and indeed sees significant attention to epistemological issues in the ontology section of the volume. This might be said to reflect two

developments in the general field of economic methodology. On the one hand, the field of economic methodology in recent years has moved further towards trying to understand the engagement of methodology with economics *per se*, where classical epistemological issues are remote. On the other hand, ontological thinking by methodologists is both comparatively more recent in the field of economic methodology and also still somewhat removed from more direct engagement with economics, such that classical philosophical issues, including epistemological ones, are still foremost for many contributors. This difference may lessen as methodologists concerned with ontological issues seem to be increasingly focused on the ontological commitments economists make. Schabas' observation, then, might be broadened to say that methodologists in general may in the future come to trade less in classical philosophical issues *per se* and more involved in eliciting the kinds of implicit philosophical commitments to be found in economics.

Schabas makes many interesting observations but I will restrict my comments to those that follow upon my remarks above. One concerns Schabas' reading of Hands' chapter, which emphasises the evolution of methodologists' use of SSK in the direction of ESK. Hands argues that SSK almost always makes some use of some version of economics and is thus to some degree really an ESK. Such a conclusion could hardly be more natural for economic methodologists for whom scientists' behaviour – the object of SSK and ESK work – invariably has an economic dimension, albeit not necessarily understood in neoclassical terms. What thus seems interesting here, then, and consistent with the suggestion above that methodologists have become more engaged with economics, is what an ESK of economics would look like. Are neoclassical economists to be understood in terms of their own neoclassical models? In terms of other types of models? Intriguing reflexivity problems abound in these questions. And they might be taken to be paradigmatic of larger issues involved in understanding the complex relationship between economic methodology and economics.

Schabas also offers a number of interesting and valuable observations regarding the status of models in economics, as interpreted by economic methodologists. First, she asks for greater elucidation of the view that models are like material objects, as explained by Boumans following Morgan and Morrison. Modelling, of course, is arguably the dominant practice of contemporary economics, though perhaps less clearly so in earlier economics. What does saying models are like material objects imply? The claim is certainly an ontological one. But if earlier economists are less modellers than theorists, what was the ontological status of theory? Similar issues arise in connection with the Kesting-Vilks chapter that equates models with formalism. More generally, then, how is the ontological status of the practices and tools economists employ related to their own ontological status? These questions also direct us to look beyond the framework of the *Companion* in terms of its separation between the second and third parts.

All this brings us conveniently to the one chapter in part two contributed by a philosopher, Kincaid's discussion of methodological individualism. Methodological individualism is both an organising strategy for neoclassical economists and also an ontological commitment. Kincaid shows us how complicated the former becomes when seen in terms of the latter. If game theory posits multiple equilibria and subpersonal strategies, what basis is there for taking individuals as ontologically unified entities? But personal identity questions such as this are hardly on the minds of contemporary methodological individualists. In this regard, the apparent move of economic methodology towards greater engagement with economics and also with ontology has much to offer.

Schabas also offers a number of general comments. She correctly notes that the methodology of economics is a burgeoning field, and tells us throughout her comment that there are many unexplored avenues ahead. Many of these new possible directions could not have been anticipated a number of years ago. Backhouse, for example, goes beyond the standard view of Lakatos, Popper and Kuhn by providing a more historical and less prescriptivist reading of Lakatos. Barker shows feminist economic methodology to be concerned with its own engagements, specifically with questions of oppressions of race, class, and nation. What, then, is economic methodology today? I have suggested two recent directions and types of emphasis, and close with a comment

on one, the greater engagement with the practices of economics, in an attempt to give something of an answer to this last question.

If indeed economic methodologists are more concerned with the state of economics than previously, we need to ask what the state of economics is if we are to say something about what the state of economic methodology is. Economics post-1980, however, seems increasingly diffuse and multi-directional. Whereas neoclassical economics was dominant in the postwar period through the economics imperialism forays of the 1970s, in recent decades experimentalism, behaviouralism, evolutionary economics and complexity theory, to name the most prominent new approaches, have changed the landscape of economics. Thus economic methodology in its engagement with economics has a changing object of investigation. But lest we think this makes matters more difficult for methodologists, we might note that the overall prospects for economic methodology seem to be enhanced by the fragmentation of mainstream economics. That is, as new approaches in economics crowd one another for attention, their proponents find themselves making recourse to methodological arguments to justify their respective orientations. These may not come in the form of traditional methodological discourse but methodological they nonetheless are.

So the identity of economic methodology seems unavoidably a working project. Schabas must be thanked for asking her probing questions to this end and for bringing the question of future directions to the attention of all.

3. Social Ontology and the Ontology of Economics

3.1. Comment (FG)

Ontology is one of today's buzzwords. It is back in fashion in analytical philosophy and Artificial Intelligence, and major 'ontological' projects and research centres get funding around the world (cf. e.g. the Buffalo Centre for Ontological Research, the Laboratory for Ontology in Turin, the Institute for Formal Ontology and Medical Information Science in Saarland). In the philosophy of science ontology has arguably always been a key area of research, under the guise of 'The foundations of ...' (physics, biology, chemistry etc.). Economics however is an exception. Because of economics' hybrid status, philosophers' interest in its foundations has traditionally focused on *normative* issues – typically on the theory of rationality that lies at the core of the neoclassical paradigm. What is relatively new, then,⁶ is the current growth of interest for the metaphysics of economics as a *descriptive* scientific discipline; see e.g. [Mäki \(2001\)](#).

Given economic ontology's relatively recent history, this is inevitably the most present-centred and forward-looking part of the *Companion*. And consequently there is also more potential for disagreement over the choice of topics. Having said that, in my view the editors have done a good job in mapping the current state of research in this area. There are some classics, like probability and money; some indisputably 'hot' topics like collective intentionality. I would have liked to see a chapter on causation and related notions (tendencies, powers, counterfactuals) – but you cannot have it all, I guess.

The allocation of space seems to reflect the levels of activity in the various areas during the last decade or so. The most vocal supporters of an 'ontological' turn in the philosophy of economics have been the members of the so-called Critical Realist school, whose contributions take up a big slice of Part III of the *Companion* (three contributions out of eight are 'orthodox' Critical Realism, I would say, and another three show some traces of influence). The opening essay by Tony Lawson is a programmatic statement of the method and role of ontological research according to Critical Realism, and provides a useful point of entry for somebody who is not acquainted with this body of work. It is also a good starting point for a critical discussion, for in ontology broadly conceived (in analytical philosophy, AI and philosophy of science) people are often busier getting on with their research than reflecting on what they are doing.

Let us begin with an obvious concern. Ontology is the theory of what there is, or of the fundamental constituents of being. In our culture the ultimate authority concerning the nature and structure of reality is science. So, is there room for a non-scientific, or extra-scientific theory of what there is? According to some philosophers, including Lawson and the Critical Realists, ontology tackles *pre*-scientific questions or, using a famous Lockean analogy, 'clears the rubbish' from the 'construction site' *before* the 'edifice of science' is built. Ontology is the '*under-labourer* of science'.

There are at least two versions of this claim, weak and strong. Both move from the historical failure of the project of specifying informative universal rules of scientific method. It is generally agreed nowadays that the reliability of ampliative (non-deductive) inferences cannot be established *a priori*. Scientific inferences from data to hypotheses are contingent, and success is dependent on the context, or the background conditions, that support the inferences themselves. An obvious implication is that scientific inferential methods must be chosen with an eye to the context of their application. They must be 'right' for their subject, appropriate for the 'stuff' one is investigating, and in general for the conditions in which the investigation takes place.

According to the weak version, ontology highlights potential mismatches between the subject matter of scientific investigation (the ontology of ...) and the method chosen to investigate it (the methodology of ...). Under this reading the role of ontology is merely one of issuing warnings, or pointing to *possibilities*: 'Look', the ontologist says, 'if the world is like *this*, your methods are not going to work; for them to work, the world has to be like *that*'. The weak version does not attempt to describe how the world is; it merely issues conditional claims of the 'if ... then ...' kind.

Despite its modesty, the weak version faces some serious obstacles. To begin with, scientific theories and scientific methods are usually ontologically *opaque*. Their ontological commitments, in other words, can rarely be read off unequivocally from their axioms, principles, or methodological rules. Take the theory of probability and its various interpretations reviewed by Charles McCann in chapter 22 of the *Companion*. There is arguably no single correct ontology of probability but a set of possible interpretations, each one with its own problems and limitations, each one more or less adequate depending on the use one intends to put the notion of probability. This problem is magnified whenever a literal reading of a given theory/method is uncharitable or blatantly implausible. Any ontological exercise then is likely to involve a certain amount of *reconstruction*; see [Carrara and Varzi \(2001\)](#).

Reconstruction means potential disagreement. So, unsurprisingly, Lawson's own interpretation of the ontological commitments of the deductivist/formalist method widely used in neoclassical economics has met quite a lot of scepticism, for other philosophers and economists see different ontological commitments behind the neoclassical paradigm. But Lawson has in mind an even more ambitious task for ontological analysis. In the strong version of the 'under-labourer' view, the ontologist actually puts forward some *unconditional* claims about the way the world is. The structure of the argument is the following: '(1) If the world is like *this*, your methods are not going to work; (2) For them to work, the world has to be like *that*; (3) But the world *is* like this; therefore (4) your methods cannot work'. The ambitious step is of course (3): despite Lawson's claims to the contrary, such a step places ontological research on the same level as science. Not perhaps in *direct* competition with science, because the ontologist is concerned with very general claims such as 'the social world is open and structured at different levels', 'individual beings are influenced by and simultaneously influence social structures', and so on; Lawson is right to say that claims like these are often *presupposed* rather than justified by scientific research. But still, they are descriptive empirical claims about the way the world is.

According to Lawson there is an independent, non-scientific way of validating ontological claims, by means of *transcendental reasoning*. A transcendental argument in the critical realist sense is heavily de-Kanted: it does not aim at establishing the *necessity* of the conclusion, it is fallible, and always revisable. Lawson's argument

moves from 'conceptions of generalised social practices' ('agency' is a favourite of Lawson's) and concludes by way of abductive inference to an ontological claim (e.g. 'social structures (norms, rules) exist'). I will not comment on the details of Lawson's transcendental argument for social structures, I have done it elsewhere ([Guala, 2005](#)). I will just note that its distinctive character cannot lie in the inferential method, for scientists do abductions all the time; hence, only its premises can differentiate it from ordinary scientific reasoning. These must contain the framework of reference for the ontological investigation. The premises of the transcendental argument are 'conceptions of generalised human practices' or, more prosaically, 'folk' social science. Below the jargon, the transcendental argument looks a lot like a search for the most likely explanation of some 'evident' features of social reality (we seem to make free choices, we are constrained by social norms), informed by our commonsensical understanding of the world.

But is it wise to take commonsense as our point of departure? Psychologists teach us that our commonsensical intuitions about social reality are as unreliable as our folk physics. We are not so good at interpreting our own and others' motives, we tend to misread even fairly simple causal relations, and in general we are affected by a wide range of systematic biases. Consider agency, for example: it is well known that we tend to exaggerate the extent to which other people are responsible for their choices, and conversely we underestimate the power of external constraints. We also tend to ignore the context-specific (often cognitive) factors that trigger norm following and so on and so forth. At best, our commonsensical understanding of the social world should be backed up by a good deal of empirical evidence.

This means that there is a continuum, rather than a sharp divide, between 'ontological' (but we might as well just say 'theoretical') speculation and empirical research, and one can hardly proceed without the other. Despite dressing up as a metaphysician, the under-labourer of science must inevitably make use of (some) science – a lesson that Carnap and Quine taught us a long time ago. By artificially stressing the divide between philosophy and science moreover, one runs the risk of never convincing the (inevitably, scientifically minded) audience that one's views are worth taking seriously. If there is a continuum, the battle against dubious science (the neoclassical paradigm or whatever) must be fought across the whole front. And in fact there *are* philosophers and social scientists who fight it that way, by intelligently combining empirical investigation with philosophical analysis; the work of people like [Sugden \(1998\)](#), [Bicchieri \(2006\)](#), [Hacking \(2002\)](#), or [MacKenzie \(2004\)](#) is what I have in mind here.

These are friendly critiques. I am raising these questions not to dismiss ontological research but because I think they are important for the direction that economic ontology will take in the near future. A collection of quotations showing that some new institutional economists are against formalism while others are not (chapter 17) is *not*, in my view, an interesting contribution to ontology (although it might be an interesting contribution to something else).

Fortunately some chapters of the *Companion* avoid the insulation from science. Faulkner and Runde (chapter 21) in their review of the concepts of information, knowledge, beliefs, intentions, provide a nice example of how to combine general philosophical analysis with an overview of the relevant evidence provided by psychologists and behavioural economists. Similarly Geoffrey Ingham (chapter 23) illustrates how an ontology of money (which revealingly he calls a 'theory of money') cannot but be deeply informed by existing historical investigations in the origins and development of the things that over the centuries have been considered 'money'. John Davis (chapter 19) also argues convincingly that some philosophical disagreements concerning the intentional foundations of social institutions ('I-intentionality' vs. 'we-intentionality' foundations, in the current philosophical jargon) are not to be solved by means of conceptual analysis. There probably are examples of both kinds of institutions in reality, and which one is more common or robust is essentially an empirical matter.

To sum up: ontology is probably here to stay and this is a welcome development. It is important, however, that some key methodological issues are clarified before we proceed. Philosophers of economics must be allowed to tackle fundamental questions that are currently ignored by economists. And in fact it is not by chance that some important new areas of research have been first explored by philosophers. But we must also be aware that the purely speculative approach has some limitations and can only go thus far unless it is complemented by other methods. We must also be careful not to be fooled about the status of ontological analysis by trendy words ('metaphysical', 'transcendental', 'essential') which just hide philosophers' unwillingness to get their hands dirty with the business of studying reality as it is – as opposed as how it should be according to their Oxbridge intuitions. Many social scientists outside economics have been busy studying rules, norms, institutions, beliefs, and the other entities that are at the core of ontological debate. In the end the boundaries between economic ontology, sociology, economic psychology, economic and social history, and so forth should turn out to be more arbitrary than we currently think.

3.2. Reply (JR)

I am very grateful to Francesco Guala for his comments. It is natural that he should concentrate on Tony Lawson's chapter in the volume, as this is devoted to developing a general position on the nature and purpose of ontological research in economics. As Guala observes, Lawson advances a version of the Lockean conception of the philosopher as under-labourer for science. Guala makes an interesting distinction between strong and weak versions of the under-labourer idea. The weak version looks for mismatches between method and subject matter but tends to deal in possibilities rather than attempting to describe the world as it is. The strong version, in contrast, does attempt to make claims about how the world is.

Guala sees difficulties with both interpretations. The weak version faces the problem that the methods and models used by economists are 'ontologically opaque', that is, that economists' ontological commitments cannot be read directly off the tools they use. The strong version faces the additional problems that it strays into doing science and thereby runs into conflict with the under-labourer idea, and, at least as represented in Lawson's writings, is too reliant on folk psychology.

The problem of ontological opacity is a real one. It is indeed not possible to read economists' ontological commitments off their models in any direct and simple way. Economists who routinely assume one-agent economies, perfect competition, common knowledge of rationality and the like in their modelling work, would be surprised if such assumptions were interpreted as a sign of their being committed to the existence of these phenomena as real features of the world. The same goes for the deeper, more abstract kind of assumptions typically not mentioned when models are formulated. For example, I suspect that most economists would balk at the atomistic or deterministic structure of their models being interpreted as their denying that the social world is characterised by internal as well as external relations or that it evolves in a non-deterministic way. The conventional view is simply that both sorts of assumptions, be they explicit or the more hidden, unstated kind, are idealisations that represent a price worth paying for what they facilitate in any modelling endeavour.

One response to all this is to argue that economists' methods and models reflect particular ontological presuppositions irrespective of the beliefs of those who create and use them. But here too different authors tend to emphasise different things. For Lawson the distinguishing feature of mainstream economics is its emphasis on formal modelling, which he sees as reflecting a view of science that presupposes the existence of sharp regularities between events ('closed systems' in his terminology, which seem to be notable for their absence in the social realm). This position is of course merely one instance of a broad position that has been stated many times before and is, to this extent, perhaps less of an outlier than Guala's comment may suggest. But there are indeed always differences over details and it is certainly true that different authors often point to different things when characterising mainstream economics, such as a commitment to individualism, rationality,

equilibrium and so on. As Guala observes, any ontological exercise is inevitably going to involve some degree of reconstruction.

The diversity of views on the ontological presuppositions of the methods employed in economics should therefore not be surprising, and indeed much of the literature is devoted to debating the differences. All this should not be seen as a problem, as such debates can be very useful in generating critical perspectives, teasing out hidden presuppositions and so on, that lead to a better understanding of the subject. But the ontology of economics is about more than the properties and presuppositions of economic models. It is also increasingly taking the form of descriptive ontology that attempts to investigate, specify and conceptualise directly, different aspects of the socio-economic world, such as capabilities ([Martins, forthcoming](#)), money ([Ingham, 2004](#)) and institutions ([Searle, 2005](#)) to name just three. A characteristic feature of this work is that it proceeds to build theories in terms of propositions and statements that, while necessarily partial, are intended to be descriptively accurate of the phenomena concerned. That is to say, it is work that privileges abstraction over the kind of idealisation that characterises standard economic analysis ([Runde, 1996](#)), much of which is presented in terms of ideal and limit-types that everyone knows to be descriptively false of the world.

It may well be that the ontology of economics will run into conflict with the under-labourer idea, especially in pursuing the latter path. Whether it actually does so or not of course depends in part on where science is seen to begin and end. For Lawson science begins where researchers begin to address specific concrete questions and attempt to reproduce causes, for Guala where researchers start making empirical claims. My own view is that attempts to establish a hard and fast line demarcating the ontology of economics from economics proper are unlikely to succeed and, more importantly, that such attempts are likely to be counterproductive. There was a time when economists could write extensively on the nature of economic phenomena without being perceived as doing anything other than contributing to economics. This is no longer the case and economics is the poorer as a result. The kind of descriptive ontology mentioned above offers a glint of hope in this regard and the last thing it needs at this stage is to be parcelled off in a way that renders it yet another specialist activity that is tangential to mainstream economic research.

One last point. Guala appears to suggest that the premises of the kind of transcendental arguments favoured by proponents of critical realism are necessarily restricted to 'conceptions of generalised human practices' or 'folk science' derived from our commonsensical understanding of the world. This seems to me a misunderstanding. In the first place, while conceptions of generalised human practices will often be the place to start in socio-economic analysis, not least in studies of what economists themselves do, this is not a matter of necessity. As Lawson himself puts it in his chapter in the *Companion*, his strategy has been to explain various generalised 'features of experience *including* human actions, and so to uncover generalised insights regarding the structure or nature of reality' (p. 328, emphasis added). Exactly where the analysis starts depends on the question being raised and may not include human practices at all, such as the symptoms of mad cow's disease (a favourite example of Lawson's), particular crops failing and others not, and so on. Second, there is nothing in Lawson that restricts the objects of analysis to items of folk psychology. They might just as well be features of regularity that have to be teased out with close empirical, and perhaps even statistical and/or experimental, work. Guala is surely right to say that at best, 'our commonsense understanding of the social world should be backed up by a good deal of empirical evidence'. But it would be wrong to suggest that Lawson would have it any other way.

4. Concluding Note

Economics and philosophy, as a domain of investigation, constitutes a set of inquiries into the nature and status of economics that come from both inside and outside the discipline. In some cases these inquiries stem from philosophical problems that arise within economics itself. In other cases the impetus comes from outside economics, such as where philosophers aim to test the boundaries and assess the principal results of the

disciplines they examine. Either way, the subject offers significant scope for multiple dialogues and invariably reflects a broad range of perspectives. One of the primary aims of the *Companion* was to encourage this kind of multi-perspectival discussion by bringing together a diversity of voices on the state of the philosophy of economics. The exchanges between the reviewers of this volume and its editors recorded above, with both philosophers and economists involved, constitute a further round of the discussion. We look forward to the ones to follow.

Footnotes

1. On how rational choice politics can neuter the public choice critique of government; see [Stigler \(1992\)](#) and more recently [Wittman \(1995\)](#). For a critique of rational choice politics on empirical grounds; see [Green and Shapiro \(1994\)](#).
2. James [Buchanan \(1982\)](#) distinguishes between questions that can be analysed by treating the actor as equivalent to a rat (where neoclassical economics is most appropriate) and those that require the actor to be viewed as a creative individual (where the subjectivist economics of [Shackle \(1972\)](#) and the Austrians is more appropriate). Buchanan is thus able to perform the difficult trick of appearing both a hard-boiled rational choice theorist and a social philosopher and political economist.
3. On the importance of ideology in providing the raw material for economic analysis see [Schumpeter \(1954\)](#). Also see the chapters in the *Companion* by Cardoso (Chapter 1) on natural law and political economy, and Leroux (Chapter 8) on ideology. Leroux asks why economists have paid so little attention to ideology, and we would suggest that at least part of the answer is a consequence of a commitment to the rationality assumptions deployed in mainstream economics that render irrelevant such discussions in so-called scientific economics.
4. A first attempt can be seen in the collection by [Bates et al. \(1998\)](#). [Elster \(2000\)](#) warns that this approach risks running into the problem of twisting the narrative to fit the analytic structure.
5. See the work of German sociologist Hans [Albert \(1979\)](#). Albert's intellectual agenda has been pursued in more recent years by Vanberg ([1994](#), [2001](#)).
6. The 'relatively' accounts for old die-hard problems like methodological individualism, which span across the methodology/ontology divide; see Kincaid's essay in Part II of the *Companion*.

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