JOHN B. DAVIS, ALAIN MARCIANO and JOCHEN RUNDE (eds.), *The Elgar Companion to Economics and Philosophy*, Edward Elgar (2004), 509+xxii pp., ISBN 1-84064-964-X

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DESPITE ITS TITLE, this *Companion* contains little economics. Rather, it presents topics from the philosophy of economics. The title may still be appropriate, as the philosophy of economics addresses many issues that should be of great interest and importance to economists. However, many authors of this volume seem to understand their project as *opposed* to economics (or rather, what they call 'mainstream' economics). This is unfortunate. Philosophy of economics is dependent on the science that it purports to be of. It discusses economics' specific epistemic, conceptual and normative problems, and intends to contribute to their solution or at least clarification. Naturally, such a project requires a critical perspective. But it must be friendly criticism. If philosophers reject the core of contemporary economics, and 'seek to re-orientate the economics discipline' as a whole (Lawson in this volume, 322), they will be confined to a state of irrelevance: ignored by economists busy building their science, but unable to produce a serious alternative themselves.

Nevertheless, the *Companion* includes many highly informative and at times provocative papers on important philosophical questions about economics. It contains twenty-three papers, categorized into three parts concerning political economy, methodology and ontology. The political economy part discusses the use of economic tools for the end of political philosophy, namely to understand and justify social order. Two papers from this part challenge contemporary economic theory to live up to this task. Hargreaves Heap points out that the rational choice model only provides incomplete explanations of institution formation. Coordination games used for this purpose typically have multiple Nash equilibria, and selecting the one that will or should be played requires reference to factors that are outside of the standard model. In particular, Hargreaves Heap argues, reference to convention alone is not enough: what motivates people to select one equilibrium is not only dictated by what they think what others will do (in accord with historical precedent), but what they think is the 'fair', 'just' or 'honourable' thing to do. He further argues that such normative beliefs are substantiated only by the judgement of others, so that individual motivation with respect to institutional formation is essentially influenced by *shared* normative beliefs. Such a conception of shared beliefs does not replace, but rather complements the standard rational choice model: it explains how preferences for institutional formation are formed by shared normative beliefs. This is an important extension of rational choice theory, and the arguments Hargreaves Heap presents deserve more attention. However, his presentation remains unclear about the structure of normative beliefs. As Biccheri showed (at the hand of behavioural experiments), people do not behave altruistically or reciprocally across the board.[1] Rather, they show sensitivity to shared normative beliefs in very specific contexts. Thus, for successful explanations of institutional formation based on people's choices, the structure of normative beliefs requires more elucidation.

In his survey of Institutional Economics, Hodgson presents a related concern. The new institutional economics, he argues, has focussed on the aim of explaining institutions solely in terms of the properties of individuals, and has neglected the importance of rules and social norms in this process. He therefore suggests borrowing from the old institutional economics, with its insistence on the malleability of agents' preferences and beliefs. In particular, he proposes a mechanism of *habituation* that explains how institutions influence agents, and lead to their relative conformity in purposes and preferences, which in turn strengthens the institution in question. Regrettably, Hodgson's notion of habit remains largely unanalysed: does he mean to refer to conventions alone? Or is there a normative element involved, as he seems to suggest when he mentions social norms? Answers to these questions would help understanding the conditions under which habits are activated, and when they are discarded.

Two other papers from this section are worth mentioning, as they provide excellent surveys. Frey and Benz review how economics opened itself to inspirations from other social sciences. They see these inspirations coming in three types of constraints: as limits on the rationality model, as an expansion of intrinsic motivations (in particular, considerations of justice, fairness and identity), and as empirical findings from happiness research informing new concepts of well-being. Fleurbaey sur-

1 Bicchieri, C. (2006) *The Grammar of Society*, Cambridge University Press.

veys recent trends in normative economics. In particular, he traces two solutions to Arrow's Impossibility Result: either by returning to cardinal utility aggregation, with a better grip on the aggregating functional and a resource-based conception of utility, or by merging the theory of fair allocation with the theory of social choice.

The methodological part addresses problems that economists encounter in their daily work. Boumans argues that empirical models must be appraised by a methodology of their own, separate from theories. Sketching the development of the model concept from physics to economics, he shows that models – although they are mathematically specified structures today – still function as if they were material objects. Models are pictorial representations of the world, but they are also instruments, in a similar way as microscopes are a tool for the biologist. They are image and investigating instrument in one. Further, they are constructed by integrating empirical facts right into them, so that they have their justification 'built-in'. Only a methodology that acknowledges these particularities will be appropriate for the assessment of models. This point is of great relevance, but Boumans avoids discussing any specific criteria for model assessment. One can only suspect that his understanding of models as pictorial representations would lead him too quickly to embrace a criterion of similarity. An understanding of models as maps that allow to draw inferences about what is mapped, in contrast, may sit more easily with his view of models as instruments employed to reach specific epistemic goals.

This part contains some further interesting essays. Kesting and Vilks discuss the formal method of economics, and identify the assumption implicit in the now ubiquitous set-theoretic formalism. They ask how these assumptions can be given an interpretation that makes them relevant for saying anything about the real world. They suggest that formal models, even if they do not represent, can be useful as training tools of the economist's intuition about the real world. Kincaid offers an epilogue to the long discussion of methodological individualism in the philosophy of the social sciences. Analysing the concept into five categories, he concludes that in none of them, *a priori* arguments for methodological individualism hold water. Instead, the methodological virtues of individualism, so Kincaid, can exclusively be assessed by empirical investigation – a conclusion that deflates the assumptions much of the microfoundations literature is founded on. For my taste, this part could have included more papers of this sort. Recent developments in methodology tackling issues like e.g. behavioural experiments, computer simulations, elicitation of causes from statistical data and the relevance of economic models for policy formation would have been desirable subjects.

Instead, a few papers of the second part consider the development of methodology itself. The one that sticks out is Hand's, who proposes a shift of question away from philosophy of science to the social determination of scientific beliefs. This is certainly an important question in its own right, but one may ask why this is relevant for methodological considerations? What can an economist learn *for* (in contrast to: *about)* her work from the investigation of such a question? How could it improve economics as a science? The only answer I can imagine is that it may uncover social biases and partial interests – but Hands touches the issue what distinguishes proper versus improper social determination only briefly.

The ontological part discusses, so the anthology's introduction says, 'the nature and structure of the socio-economic realm' (xx). To understand this project, a distinction between descriptive and revisionist metaphysics may be helpful. The aim of descriptive metaphysics is to clarify the structure of our thought about the world. Applied to special sciences, it spells out how a science conceptualises the world through its theories and practices. Only two of this part's papers fall into this category: Faulkner and Runde analyse economic models of imperfect knowledge. They argue that these models still make substantial assumptions about the epistemic capacities of agents, and that they cannot cover violations of Bayesian axioms (e.g. in the Ellsberg experiment) or situations where the agent cannot survey the space of all relevant states of the world. McCann analyses four different theories of probability and investigates their conceptual and epistemological assumptions. In neither case do the authors make claims about how the world is.

The rest of the papers, in contrast, make claims how aspects of the socio-economic world *are*. Some of these offer more or less detailed theories that specify how they relate to empirical observation. Davis for example argues that we-intentions of a group cannot be reduced to individual we-preferences, because such we-preferences cannot explain the particular binding forces of some groups. Instead, he claims, only group intention based on shared normative beliefs can explain such phenomena. This paper, I think, is a successful example how philosophical analysis can contribute to the development of scientific theory.

The largest share of this part's papers, however, engages in revisionist metaphysics. They make claims how the world is (e.g. 'intrinsically dynamic', 'internally related', 'structured' – Lawson, 330) and reject 'mainstream' economic theory as incompatible with this way the world is supposed to be. Lawson's paper may serve as the representative example here. He suggests 'that the formalistic methods of economists are being applied to materials for which they are not appropriate' (324). He argues for his claim by (i) analysing economic theories to find out what objects they are appropriate for (it needs to be mentioned here that Lawson makes little effort to differentiate between economic theories. Rather, he tends to lump it all together under the heading of 'economic mainstream' – a habit that is unfortunately widespread in this *Companion*). Then, (ii), he elaborates 'a conception of the broad nature and structure of ... reality' (324). Last, (iii), he shows that the thus determined nature of reality does not match with the objects 'mainstream' economic theories are appropriate for. The most controversial part of this argument, of course, is the second one. Lawson purports that the way to achieve his ontological result is 'to look to the social realm directly, and to transcendentally infer the social conditions of human practices directly' (329). How one is supposed to proceed in this fashion he does not detail. In particular, it remains a mystery to me how one can look 'directly' at reality, without first determining the methods and instruments with which one looks. The 'transcendental argument', as briefly as it is sketched in this paper, does not give an answer to this question. Looking at other papers in this category only increased my scepticism. Pratten claims that 'the stipulation from a realist orientation is that procedures and techniques should be tailored to the chosen object of study' (345). How, one may wonder, can such a method avoid vicious circularity? The only way I see is to claim some form of privileged access to knowledge about the social realm. Lawson suggests that the source for this access may lie in exercising 'pure reason...on the basis of *a posteriori* conceptions of historically rooted practices' (328). This sounds like an appeal to common sense. But hasn't 'mainstream' economics successfully contradicted such common sense judgment many times? E.g. haven't we learned from 'mainstream' economics that raising tax rates may reduce government revenue? Or that introducing minimum wage may harm the poor? Folk economics, like folk physics or folk biology, needs to be treated with caution. That it should serve as the basis for rejecting the methods of 'mainstream' economics therefore remains at least a controversial view, which the papers in this part do not sufficiently support.

In the *Companion's* introduction the editors stress the importance of philosophical ideas *in* economics. This turns out to be an ambivalent description of the envisaged division of labour. Some contributors understand their role as supporting working economists in solving methodological and conceptual problems that appear in their work. Others seem to understand their role as *replacing* those working scientists, and starting pretty much all over again. Adopting the latter view, it seems to me, would be an unfortunate move for the philosophy of economics.