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ECONOMICS

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Economists about economics

Economics is a controversial discipline: the highly successful “queen of the social sciences” as well as the miserable “dismal science.” No wonder there is ongoing philosophical debate around issues of justification.

From Nassau Senior and John Stuart Mill in the 1830s to Lionel Robbins in the 1930s, there was a dominant conception among practicing economists about the structure and justification of economic theory. One idea was that the premises or postulates (later to be called “assumptions”) of economic theory were by and large true: they capture the key causal factors that are in operation in producing economic phenomena – such as the selfish pursuit of maximum wealth by agents and of diminishing returns in agriculture. While these were confirmed by ordinary experience, the predictions of economic theory are not typically well confirmed by evidence. The reason is that the theory is incomplete: it captures only a limited portion of the multiplicity of the causes that jointly influence the actual economic outcomes. The accuracy of predictions is thus not a reliable indicator of its truth. As Mill says, a theory may be true in the abstract – in the absence of disturbing causes – without being true in the concrete – when the disturbing causes are allowed to make their contribution. Another way was to say that a theory describes tendencies towards those outcomes rather than regularities among them or between them and their causes.

Confirmation of economic theory thus rested on the assurance that the premises are correct rather than on checking the predictive implications against evidence. Testing by implications as in the hypothetico-deductive view does not work in economics. One starts rather by isolating component causes and making well-supported claims about them, and then, in applying the theory, proceeds to add them to one another as in vector addition. The method is that of decomposition and composition, analysis and synthesis, or isolation and de-isolation. This method was often justified by appeal to a special characteristic of economics: we have relatively easy access to the key causes of economic phenomena by way of ordinary experience, thus there is no need for conjecture and lengthy inference about hidden unobservables as in the natural sciences.

Up to the present day, there have been critical voices insisting on a different approach. These included many German and British historicists (in the nineteenth

century) and North American institutionalists (in the early twentieth century). Their objection was that conventional theory has taken the decomposition of causes too far: the causes interact and constitute larger wholes, the parts of which cannot be isolated from one another without distorting important facts about social reality. Economics should have a broader scope and be flexible about its disciplinary boundaries. The critics also argued that the claims made about those component parts, especially about self-interested maximizing, were evidently incorrect descriptions of human behavior. Those charges remain common today. Many of the critics also insisted that economists should start their investigations by collecting lots of empirical data and only gradually generalize the regularities discovered therein.

From a Millian point of view, this is wrong. Empirical data manifest the functioning of multiple causes in irregular combinations and thus cannot provide a reliable basis for generalization (or testing). This was argued by Carl Menger (1883), who launched the famous *Methodenstreit* against the German historicists. He outlined a version of the Millian account with Aristotelian characteristics. Economic theory is about general *types* and *typical relations*. Of these, *exact laws* depict *de re* necessities that derive from individuals' economizing action; they are Aristotelian, second-order universals that connect first-order universals. Exact laws, such as the law of demand, do not permit exceptions, while empirically established regularities do. The historicist method was capable only of producing the latter. But exact laws are unable to yield reliable and accurate predictions of phenomena in the complex actual world: they are exceptionless only in the simple world of economic universals.

The discrepancy between what is predicted and what is observed has remained a chronic issue that shows no sign of going away. Nevertheless, the legend has it that the Millian tradition was left behind in the 1950s with Fritz Machlup's and Milton Friedman's contributions. The burden of justification was then put on the predictive implications rather than the assumptions of a theory. This can be seen as a strategic response to the challenges leveled against the neo-classical theory that they defended. In empirical studies carried out in the UK and the USA in the late 1930s and early 1940s, the profit-maximization assumption had been questioned. That gave rise to the "marginalist controversies" in which the issue was whether business managers maximize profits by producing quantities of goods that equate marginal cost and marginal revenue, and if they do not (and the empirical studies showed they indeed do not), whether this would undermine neo-classical theory. Machlup and Friedman put forward arguments suggesting that such assumptions do not need to be realistic in order for the theory to be just fine. All that matters is *predictive* performance. Methodological debate in economics in the 1950s and the 1960s was dominated by that theme, and it continues.

Testability and progress: Popper and Lakatos

Prior to the 1970s, philosophical and methodological reflection on economics was provided mostly by working economists. During that decade, the philosophy and methodology of economics started taking shape as a separate research field, prompted

by changes in economics and in the philosophy of science. Themes and concepts adopted from Karl Popper and Imre Lakatos first became popular. This was largely due to authors who worked at the interface of the philosophy and history of economics and who were concerned that economists had accepted theories without sufficiently strong evidential warrant. Others looked for ways of discriminating between schools of economic thinking that again had started to proliferate. Popper and Lakatos seemed to offer appropriately stringent standards for assessing – and improving – a discipline that aspired to be an empirical science.

In 1938, Terence Hutchison had incorporated *falsificationist* elements in his otherwise logical positivist account of economic theory. Between years 1957 and 1963, Popper's falsificationism was more seriously entertained by the "M²T" group of economists and philosophers at the London School of Economics. They examined a variety of economic theories against falsificationist standards, and concluded that there is an irresolvable tension: economic theories are not strictly falsifiable. One of them had to go, and it was falsificationism that was to be sacrificed (see De Marchi 1988).

In the 1970s, falsificationism made a comeback, both in Popper's and in Lakatos's modified versions. It was again soon concluded that any simple version of falsificationism in economics would be descriptively inadequate and normatively utopian, thus in a sense itself falsified – even though commentators, like Mark Blaug (1992 [1980]), kept insisting that economists should just try harder to meet falsificationist standards. Others, like Larry Boland, have defended Popper's more general doctrine of critical rationalism.

Lakatos's *methodology of scientific research programs* (MSRP) has enjoyed a longer life. It was introduced to economics by Spiro Latsis, a student of Lakatos, soon to be adopted by others (Latsis 1976). Fifteen years later, it was almost unanimously dropped (De Marchi and Blaug 1991). Meanwhile, numerous applications and case studies were undertaken and employed in arguments either about economics or about Lakatosian methodology. Research programs were identified by formulating their hard cores, protective belts, and heuristics, and they were assessed in terms of progress (De Marchi and Blaug 1991).

The MSRP had obvious advantages. It helped see that the unit of assessment is larger than a single hypothesis or theory, and that not all parts of a theory are equally flexible when confronted with empirical evidence. It helped highlight the ongoing adjustment of theories in economics. The idea of predicting novel facts captured a notion held by many economists: predicting data that were not used in the construction of a model yields it greater support than predicting data that were so used.

When applied to economics, the MSRP suffered from obvious problems. The identification of research programs – choosing their scale and drawing their boundaries – turned out to be somewhat arbitrary, making it unclear how one can assess their relative performance in a sensible way. The hard cores of many candidate programs are not as hard as the MSRP would require. The MSRP lacks other resources needed for recognizing programs as rivals that can be reasonably compared; that would require pointing out their shared goals. This is made harder by further problems in reliably identifying cases of progress and degeneration. And, again, economists' actual

decisions as to whether to accept or reject a program seemed to have little to do with their apparent progress or degeneration.

Popperian and Lakatosian frameworks were dropped also because they lacked the resources needed for addressing many core issues in the philosophical reflection on economics. Much has happened after (and parallel to) this episode, such as philosophical analyses of causation in macro-economics and econometrics, and of experimental economics, by specialists like Kevin Hoover and Francesco Guala (see Further reading). In what follows I select four other core themes: theoretical models; rhetoric of economics; use of economics in the socialization of the philosophy of science; and the interdisciplinary relations of economics.

Models and their assumptions

There are many kinds of models in economics, such as large-scale econometric forecasting models and small-scale theoretical models. Forecasting models have their own associated philosophical issues, but the focus of most of the philosophy of economics has been on *theoretical* models.

To make progress in investigating the issues of empirical testing, one needs to have an understanding of what exactly is being tested, and what kind of performance it is tested for. This is a precondition that has not been fully met by the Popperian-Lakatosian episode that can be seen as a detour that ignored the Millian heritage. That heritage has been upheld – but not in one choir – by Daniel Hausman (1992), Nancy Cartwright (1989), and myself (1992), of whom the last two have also been influenced by the work of the Poznan School on idealizations and the Aristotelian tradition.

The most central issue in the philosophy of economics derives from the popular complaint that economics employs imaginary models with highly unrealistic assumptions, therefore failing to offer true accounts of the real world. Economists often react by saying that all models are false anyway. Or they follow Milton Friedman's influential advice (1953): it does not matter even if the assumptions of a model are false, provided its predictions succeed. Those responses have inspired the conclusion that economists generally are inclined towards an *instrumentalist* conception of theory and model. Friedman's view is instrumentalist, and so are others appealing to Friedman's arguments. Among the premises of this interpretation are these two: the truth-value of a model is essentially dependent on the truth-values of its assumptions; and instrumentalism views models as false tools of inquiry.

Such conclusions have been hasty in that they are not based on any detailed examination of the structure of models and the various roles that assumptions play in those models. The first premise of the received view of economic instrumentalism must be rejected: the truth-value of a model cannot be derived from the truth-values of its assumptions. There are other ways of interpreting economic theory and model.

While some thinkers (such as Mary Morgan – see Further reading) have focused more on how models function in actual research practice, others have tried to analyze the structure of models from the point of view of the question of how they are – or

fail to be – connected to the real world. In Hausman's 1992 account, a model as such contains no claims about the real world, it is rather a definition of a predicate given by the assumptions of the model, and such definitions are not truth-valued. Models as bundles of assumptions define predicates such as "... is a Keynesian system" and "... is a general equilibrium system," and economists examine the properties of such predicates in exercising *conceptual exploration*. On the other hand, *theoretical hypotheses* are truth-valued claims about the applicability of the models to real economic systems. "The Greek economy is a Walrasian system" is one such true-or-false hypothesis.

My worry about this account is that the original suspicions about utterly false economic models would remain intact. Theoretical hypotheses would not perform any better in truth acquisition than if models were directly considered as truth-bearers. They would turn out to be false just as often as models would. The source of this trouble is the same: models play a role in both approaches in their entirety, including all their unrealistic assumptions. "The Greek economy is a Walrasian system" is as false as the general equilibrium model described in terms of the usual, highly unrealistic, Walrasian assumptions if the model is indiscriminately taken as a unitary truth-bearer.

The alternative is to take the truth-bearer to be more limited and to be clear about the special roles played by false assumptions. The intended truth-claim when using a model is often about a real dependence relation or a powerful causal mechanism, its structure and characteristic way of functioning. The role of false assumptions is to help isolate that mechanism from other influences. This allows us to reject popular beliefs held by economists: "This model is based on false assumptions, therefore the model is false" is as false as the claim "No model can capture the whole complexity of the real world, therefore all models are false."

The key is to examine the roles that assumptions play within models. Their role is not one of assertion. No truth-claim, no belief in their truth is involved, not even a conjecture that they might be true. The function of many assumptions is to neutralize other factors the influence of which is not considered in the model – and thereby to isolate a limited set of factors for closer inspection. One makes idealizing assumptions about the absence, zero-strength, constancy, and normalcy of those other things. Such assumptions are believed to be (always or much of the time) false if considered as truth-claims.

On that account, theoretical models are analogous to ordinary experiments in which such isolations are based on causally effective material controls. In theoretical models, such controls are accomplished by idealizing assumptions. In both cases, the goal is to acquire truthful information about some major dependence relation or the operation of a causal mechanism (Mäki 1992). Employing this way of framing things, the received interpretation of Friedman (1953) as an instrumentalist statement can be questioned: he defended unrealistic assumptions from a realist point of view.

From another perspective, one can show that apparently false assumptions can often be paraphrased so as to turn them into candidates for true claims. Many assumptions appear falsely to assume that some quantity is zero (closed economy, zero transaction costs, time needed for adjustment). Some of them serve to remove from

consideration factors that are supposed to be causally weak or otherwise irrelevant, in which case the assumption may be used to make the true assertion that such a factor is negligible for the purposes at hand (negligibility assumption: actual foreign trade makes a negligible impact on the outcome). In case such a factor is causally strong, the claim intended may be to suggest that it will be included later on by relaxing the false assumption (early-step assumption: the closed economy assumption is to be relaxed in later versions of the model); or else to use the assumption for fixing the domain of application of the model (applicability assumption: the model applies only to circumstances in which foreign trade has a negligibly small effect) (Musgrave 1981; Mäki 2000).

Consider then the very concept of model. Models can be viewed as representations in that they serve as representatives of what they represent – as surrogate or substitute systems of the target systems. One directly examines the model in order to acquire indirectly information about the target system. Animal subjects are examined to learn about human beings; miniature airplanes are examined in artificial wind-tunnels to learn about the prospective behavior of real airplanes in non-artificial conditions; systems of mathematical equations are studied in order to learn about the Big Bang; imagined simple $2 \times 2 \times 2$ worlds containing only two countries, two goods and two factors of production are studied to learn about the mechanisms of comparative advantage in international trade. Indeed, models are of a broad variety of kinds, and they can be described using a variety of media, such as mathematical equations, flowchart diagrams, and verbal stories. An implication of this account is that ordinary material experiments also count as models.

The long tradition of blaming economic models for being out of touch with the real world can be translated into the suspicion that models are treated as nothing but surrogate worlds, without the right kind of further connection with the real world. Accordingly, economists take the easy route of examining the properties of the model systems while not bothering themselves with the effort of determining how they are related to the properties of real systems. Economists would be missing another aspect of models as representations: *resemblance*. It is trivial that models do not resemble the target systems in all respects and in all details, hence the thought that all models are false. But a model has to resemble the target system in relevant respects and in sufficient details in order to serve as an adequate representative. As soon as one is clear about what exactly a model is intended to represent – such as one tiny mechanism among many others – the question about its truth can be raised. The whole truth is not the goal. One pursues only truths about partial aspects of a total situation. Models may in principle be true – nothing-but-true – about such partial aspects.

Robert Sugden's account of models as "credible worlds" (2002) fits in that framework. Models have to be such that the imaginary worlds they describe – such as segregated housing markets in Thomas Schelling's checkerboard models of cities – are factually possible worlds in that what causes those imaginary worlds to work the way they do is plausible, given our beliefs about their constituent elements and causes in the actual world. This enables an inductive move from model worlds to the actual world: by examining a number of closely related model worlds (e.g., checkerboard cities) one

discovers the same mechanism producing the same outcome (thereby establishing its robustness) and infers to the conclusion that the mechanism is in operation also in the actual world (in real-world cities). If one wants to call this "testing," it is different from testing according to hypothetico-deductivism.

Rhetorical persuasion and truth

The frustrations with falsificationism in economic methodology not only gave a boost to a renewed interest in the Millian tradition and its elaborations, but they also encouraged the spread of emerging social constructivist trends: given that the fate of theories is not determined by incorrupt empirical evidence, there is ample room for social factors to play a role. An early start was made by the rhetoric of economics, on which the work by Deirdre McCloskey and Arjo Klamer (see Klamer, McCloskey, and Solow 1988) has become the subject of an extended debate. Their claim is that much, or almost all, of what scientists do is a matter of attempting to persuade their various audiences (colleagues, students, administrators, funding agencies, political decision-makers, lay audiences).

One of their contributions has been the identification of various *rhetorical ploys* and textual strategies used by economists, such as the use of attractive metaphors, and appeals to authority and mathematical brilliance. Another characteristic is a *conversational model* of rhetoric: persuasion takes place in a conversation that is very much akin to exchange in a marketplace. "Honest conversation" abides to the *Sprachethik*, and this is included in the very concept of rhetoric: "Don't lie; pay attention; don't sneer; cooperate; don't shout; let other people talk; be open-minded; explain yourself when asked; don't resort to violence or conspiracy in aid of your ideas." A third characteristic is an overt *anti-methodology*: no space for the traditional concern with methodological principles and rules. Good economics will be promoted just by raising the awareness among the practitioners about the rhetorical features of their conversations and by persuading them to adhere to the *Sprachethik*. Further "methodological intervention" would do nothing but harm.

In response, one may acknowledge the presence and power of rhetoric in science, as well as the importance of some ethical principles of research and communication, while insisting that the *Sprachethik* should not be included in the general concept of rhetoric – but will be fine as part of the idea of appropriate rhetoric – and that the awareness of the ploys used in the ongoing rhetorical persuasion cannot replace methodological principles.

The fourth feature of this project has been its outright anti-realism, variously self-identified as relativism, pragmatism, social constructivism, postmodernism. This is part of a larger current of rejecting the ideas of objective reality and objective truth. Whatever there is in the world, and whatever is true about it, are merely the results of persuasion. Truth is equated with persuasiveness, and thus truths are "made" rather than "discovered."

The alternative, realist, view is to take truth to be independent of any rhetorical efforts. A model is not made true (false) by being found persuasive (unpersuasive) by

a cohort of economists with a certain educational background and academic incentive structure. Background beliefs and institutional structure shape what is found persuasive and what is regarded as true at any given time – and even the likelihood of tracking truths about the world by a community of inquirers. The distinction is between what is true (or real) and what counts as true or is believed to be true (in some culture or group, or at a certain time). We do not have to think that the *reality* of the natural rate of unemployment or the *truth* of our theory of it is a function of rhetorical persuasion even if we think that our *belief* in its reality and in the truth of our theory of it can be influenced by rhetoric. The recognition that rhetoric is real and effective also in scientific communication is relatively neutral regarding its philosophical implications and presuppositions.

There is an extreme reading of the McCloskey–Klamer conception that would help resist the above charges. Economics as it is currently practiced is nothing but a rhetorical game of persuasion, perhaps one that chronically violates the *Sprachethik*. Being “nothing but rhetoric” would suggest that realism is unfit, since economics is presently not in the least interested in generating truthful information about the real world (perhaps it is preoccupied just with the study of the surrogate worlds of theoretical models). Even if this were true of some parts of current economics, it is unlikely to be true of all of it. And the natural remedy would be to preach not just rhetorical awareness and the *Sprachethik*, but to preach them *together with realism*.

Economics as a resource for the philosophy of science

In line with the larger currents in the social studies of science and social epistemology, economics is now customarily viewed as a form of social activity. The theoretical resources for highlighting the social aspects of economic inquiry are derived not just from rhetorical studies but also from sociology – and from economics itself. The contributors include Wade Hands, Philip Mirowski, Esther-Mirjam Sent, Roy Weintraub, and others. A special portion of this work suggests reversing the roles of economics and philosophy in their interactions.

Economics is playing an increasingly important role in the *naturalization* of the philosophy of science: philosophical accounts of various aspects of science are to be informed by the best scientific accounts of matters related to those aspects. Its social aspects call for *socializing* philosophy by appealing to social sciences. The question is how to choose the theoretical resources for this purpose given the variety of social science disciplines as well as the theoretical variety and disagreement within and between those disciplines.

The earlier, simplified, description of good science was in terms of disinterested scientists thrown in an institutional vacuum, and pursuing nothing but truthful (or otherwise epistemically virtuous) information about the world. Using economic concepts, philosophers such as Alvin Goldman, Philip Kitcher, and Jesus Zamora now portray scientists as being driven by self-seeking desires in a competitive market for ideas: scientists strategically seek to maximize their own fame and fortune, credibility and prestige, and other such non-cognitive social goals that enhance their personal

utility. Scientists make investments and expect returns, suffer costs and enjoy benefits, acquire property rights and respond to incentives, and do these things within an "industrial organization" of scientific production governed by the rules of the game with a contractual structure.

Two philosophically interesting issues stand out. First, viewing science as an economy means transferring the familiar ideological and political issues from economics to science theory along the dimension of hands-off free market to hands-on regulation. The capacity of science to reach whatever epistemic or other goals depends on its industrial organization, market structure, regime of regulation, or governance structure. This has a theoretical aspect: which structure of academic institutions is the most conducive to epistemic success? And it has a policy aspect: how is that structure to be designed and implemented? Philosophy of science becomes more explicitly political.

Second, naturalizing science theory in terms of economics is taken by some to imply dispensing with traditional issues in scientific methodology, replacing it with a social science of science (see Hands 2001). I do not think that the traditional issues in the philosophy and methodology of science – or of economics – are dead at all, for two reasons. The first is that the familiar philosophical questions about *the target of science* remain as alive as ever. If we portray a science as an economy and scientists as economic agents seeking their own non-cognitive goals, it will be difficult to answer further questions, such as whether and how such an activity will be able to generate knowledge and cognitive progress, and what are the rational grounds of belief and settlement of disagreement in science. On such issues, economics offers theoretical resources that have been employed to alleviate emerging concerns: the market of science is in operation with the capacity to coordinate individual scientists' activities so as to transform them into epistemically virtuous outcomes as if by an invisible hand. But this requires a troublesome translation from economic theory to the philosophical vocabulary of knowledge and its growth.

The second reason why familiar philosophical issues will not go away is perhaps even more obvious. If we portray science in economic terms, we are employing a theoretical resource that is supposed to supply very demanding services. Not just any such possible resource or tool will do. Only the best and most reliable tools should be adopted in the service of purposes of such importance. Questions arise about *the credibility and reliability of economics itself* as such a candidate tool. Economics is unable to justify itself: a reflexivity test of that kind lacks the required power (Mäki 1999). And it will not do to appeal to the prestige of economics as a social science given that it is such a controversial discipline. Instead of replacing or eliminating familiar philosophical concerns, the reverse is true: the use of economics as a resource in the naturalization of our accounts of science only makes those concerns more pressing.

Explanatory expansionism and interdisciplinary relations

Economics is currently participating in interdisciplinary interactions in two ways: as an imperialist imposer; and as a humble learner. The first is a relatively new (post-1950s)

trend, while the second means returning to the nineteenth-century ways of flexible or non-existent disciplinary boundaries, with intellectual traffic flowing freely. These appear to pull in different directions. The first is more conservative of the conventional contents of economic theory, while the second is reformist, even revolutionary. Such trends respond to traditional complaints about economics being closed-off from other disciplines. Borders are now being opened, in both directions.

One such direction is a version of the urge to explain as many kinds of economic phenomena as possible in terms of the same small set of causal factors or explanatory principles. This manifests itself in the *intra-disciplinary* unification of theories and fields (such as trade, growth, and location theories), and in the micro-foundationist project of reducing all economic phenomena to individual constrained maximization. Economics is no exception within the family of scientific disciplines: unification is a driving methodological ideal (see Mäki 2001). An *interdisciplinary* version of explanatory unificationism is economics imperialism, the aspiration of using economic concepts and explanatory principles in accounting for phenomena that traditionally belonged to the domain of disciplines other than economics (such as law, political science, sociology, anthropology, history, human geography, science studies). Almost all human behavior is now depicted as self-interested, rational choice in a market or market-like social setting — such as the marriage market and the market for votes, religions, and scientific ideas. The more controversial applications include explaining phenomena such as serial killing and drug addiction as informed self-regarding rational choice that balances the relevant utilities, harms, and the associated likelihoods (of one's own early death, for example). Emotions, morality, routines, and social norms are not supposed to play a role in such explanations if they cannot be redescribed in terms of self-regarding instrumental rationality.

Are we here witnessing empirically supported progress towards a more unified social science that succeeds in capturing the real unity of human action and social structure, thus establishing ontological unification of the variety of phenomena regardless of their previous disciplinary home domains? Or is it rather a matter of one limited discipline colonizing its neighbors by way of dubious maneuvering of flexibilities in testing, including the questionable redefinition of key concepts (such as *cost* and *market*) so as to enable only ontologically uncommitted derivational unification?

But interdisciplinary influences also travel in the other direction. Economics increasingly gives up its disciplinary autonomy and, under interdisciplinary pressure, modifies narrow conceptions of rational action and market adjustment. New branches of economics (such as institutional economics, behavioral economics, neuroeconomics, and evolutionary economics) are dependent on consulting other disciplines (such as sociology, experimental psychology, neuroscience, and evolutionary biology) for information and insight. They recognize the importance for economic action and outcomes of cognitive limitations, social norms, emotions, moral commitment. Next to rational deliberation, people are moved as much, if not more, by routine and affect, by considerations and feelings of fairness and reciprocity, shame and esteem, trustworthiness and retaliation, and they keep making systematic mistakes. This enriched folk psychology has neurobiological correlates, the investigation of which shows that the

capacity of the human brain for rational choice is much weaker than economic theory or our ordinary introspective judgment would suggest.

Among the more radical conclusions that have been drawn is that standard rational choice remains an exceptional special case, thus should lose its dominance in economic modeling. On the other hand, those defending standard economics argue that its framework of rational choice is flexible enough to accommodate a broad range of mental dispositions and that it is not contradicted by neurosciences, due to differences in disciplinary domains and their constitutive explanatory questions. The debate will continue, and its analysis will require adopting and developing a rich range of philosophical tools as yet untried in the philosophy of economics.

See also Critical rationalism; Idealization; Mechanisms; Models; Realism/anti-realism; Relativism in science; Representation in science; Social science; Social studies of science; The structure of theories.

References

- Blaug, M. (1992 [1980]) *The Methodology of Economics*, 2nd edn, Cambridge: Cambridge University Press.
- Cartwright, N. (1989) *Nature's Capacities and Their Measurement*, Oxford: Oxford University Press.
- De Marchi, N. (1988) "Popper and the LSE Economists," in N. de Marchi (ed.) *The Popperian Legacy in Economics*, Cambridge: Cambridge University Press, pp. 139–66.
- De Marchi, N. and Blaug, M. (eds) (1991) *Appraising Economic Theories: Studies in the Methodology of Research Programmes*, Aldershot: Elgar.
- Friedman, M. (1953) "Methodology of Positive Economics," in *Essays in Positive Economics*, Chicago: University of Chicago Press, pp. 3–43.
- Hands, D. W. (2001) *Reflection Without Rules: Economic Methodology and Contemporary Science Theory*, Cambridge: Cambridge University Press.
- Hausman, D. M. (1992) *The Inexact and Separate Science of Economics*, Cambridge: Cambridge University Press.
- Klamer, A., McCloskey, D., and Solow, R. (eds) (1988) *The Consequences of Economic Rhetoric*, Cambridge: Cambridge University Press.
- Latsis, S. (ed.) (1976) *Method and Appraisal in Economics*, Cambridge: Cambridge University Press.
- Mäki, U. (1992) "On the Method of Isolation in Economics," *Poznan Studies in the Philosophy of the Sciences and the Humanities* 26 (special issue): 319–54.
- (1999) "Science as a Free Market: A Reflexivity Test in an Economics of Economics," *Perspectives on Science* 7: 486–509.
- (2000) "Kinds of Assumptions and Their Truth: Shaking an Untwisted F-Twist," *Kyklos* 53: 303–22.
- (2001) "Explanatory Unification: Double and Doubtful," *Philosophy of the Social Sciences* 31, 488–506.
- Menger, C. (1883) *Untersuchungen über die Methode der Socialwissenschaften und der Politischen Ökonomie Insbesondere*, Leipzig: Duncker & Humblot.
- Musgrave, A. (1981) "'Unreal Assumptions' in Economic Theory: The F-Twist Untwisted," *Kyklos* 34: 377–87.
- Sugden, R. (2002) "Credible Worlds: The Status of Theoretical Models in Economics," in U. Mäki (ed.) *Fact and Fiction in Economics: Models, Realism and Social Construction*, Cambridge: Cambridge University Press, pp. 107–36.

Further reading

The classic work in the Millian tradition that also defines economics as the (almost universally applicable) science of choice in conditions of scarcity is Lionel Robbins, *An Essay on the Nature and Significance of Economic Science* (London: Macmillan, 1935). An analysis of Menger's earlier version in the same tradition mentioned in the text is U. Mäki, "Universals and the *Methodenstreit*: A Reexamination of Carl Menger's Conception of Economics as an Exact Science," *Studies in History and Philosophy of Science* 28 (1997): 475–95. Logical positivist ideas have influenced T. Hutchison, *The Significance and Basic Postulates of Economic Theory* (London: Macmillan, 1938). On contributions and debates in the Popperian and Lakatosian traditions, one should consult D. W. Hands, *Testing, Rationality, and Progress: Essays on the Popperian Tradition in Economic Methodology* (Totowa, NJ: Rowman & Littlefield, 1993), and R. Backhouse, *Explorations in Economic Methodology* (London: Routledge, 1998). The philosophical analysis of economic models is now spreading in many directions. For Mary Morgan's important contributions, one may start with M. Morgan and M. Morrison (eds) *Models as Mediators: Perspectives on Natural and Social Sciences* (Cambridge: Cambridge University Press, 1999). An outline of my account is in U. Mäki, "Models Are Experiments, Experiments are Models," *Journal of Economic Methodology* 12 (2005): 303–15. The classic work on the rhetoric of economics is D. McCloskey, *The Rhetoric of Economics* (Madison: University of Wisconsin Press, 1985). My critique and alternative view are outlined in U. Mäki, "Diagnosing McCloskey," *Journal of Economic Literature* 33 (1995): 1300–18. For themes mentioned but not covered in the text, one should consult K. D. Hoover, *Causality in Macroeconomics* (Cambridge: Cambridge University Press, 2001) and F. Guala, *The Methodology of Experimental Economics* (Cambridge: Cambridge University Press, 2005). There are good collections of essays that provide accessible introductions to the philosophy and methodology of economics. They fall in three categories. Among handbooks, there are J. B. Davis, D. W. Hands, and U. Mäki (eds) *The Handbook of Economic Methodology* (Cheltenham: Edward Elgar, 1998); H. Kincaid and D. Ross (eds) *The Handbook of the Philosophy of Economics* (Oxford: Oxford University Press, 2007); and U. Mäki, *The Handbook of the Philosophy of Economics* (Amsterdam: Elsevier, 2007). The first of these contains numerous shorter entries, while the other two consist of fewer and longer essays. Anthologies of previously published representative papers include B. Caldwell (ed.) *The Philosophy and Methodology of Economics*, 3 vols (Cheltenham: Edward Elgar, 1993); J. B. Davis (ed.) *Recent Developments in Economic Methodology*, 3 vols (Cheltenham: Edward Elgar, 2006); and D. M. Hausman (ed.) *Philosophy of Economics: An Anthology* (Cambridge: Cambridge University Press, 2007). The third category includes edited volumes on more focused themes. R. Backhouse, D. M. Hausman, U. Mäki, and A. Salanti (eds) *Economics and Methodology: Crossing Boundaries* (London: Macmillan, 1998) is a collection of case studies that was designed to turn philosophy of economics in a more empirical direction. R. Backhouse and A. Salanti (eds) *Macroeconomics and the Real World*, 2 vols (Oxford: Oxford University Press, 2000) is devoted to methodological issues in macroeconomics. U. Mäki (ed.) *The Economic World View: Studies in the Ontology of Economics* (Cambridge: Cambridge University Press, 2001) collects work on economic ontology. U. Mäki (ed.) *Fact and Fiction in Economics: Realism, Models and Social Construction* (Cambridge: Cambridge University Press, 2002) approaches the core conundrum from a variety of angles, while U. Mäki (ed.) *The Methodology of Positive Economics: Milton Friedman's Essay Half a Century Later* (Cambridge: Cambridge University Press, 2007) does the same, focusing on Friedman's controversial and poorly understood essay.