Realistic realism about unrealistic models

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My philosophical intuitions are those of a scientific realist. In addition to being *realist* in its philosophical outlook, my philosophy of economics also aspires to be *realistic* in the sense of being descriptively adequate, or at least normatively non-utopian, about economics as a scientific discipline. The special challenge my philosophy of economics must meet is to provide a scientific realist account that is realistic of a discipline that deals with a complex subject matter and operates with highly unrealistic models. *Unrealisticness in* economic models must not constitute an obstacle to *realism about* those models.

What follows is a selective and somewhat abstract summary of my thinking about economics, outlined from two perspectives: first historical and autobiographical, then systematic and comparative. The first angle helps understand motives and trajectories of ideas against their backgrounds in intellectual history. My story turns out to have both unique and generalizable aspects. The second approach outlines some of the key concepts and arguments as well as their interrelations in my philosophy of economics, with occasional comparisons to other views. More space will be devoted to this second perspective than to the first.

HISTORICAL

During my second undergraduate year at the University of Helsinki in 1971, I decided to become a specialist in the philosophy and methodology of economics. No such (institutionalized) *field* of inquiry existed at that time, but this was no reason not to make the decision. I studied economics and philosophy parallel to one another. I was attracted to economics by my perception of its importance and rigor, while philosophy was more a matter of intellectual passion. Having taken introductory courses in economics, I was, just like many other fellow students, deeply puzzled by utility maximization, perfect competition, and other assumptions that appeared bizarrely unrealistic about the social and mental world as we knew it. There was so much obvious falsehood in the models of the queen of the social sciences that I did not know what to make of it. Then I read Milton Friedman’s 1953 essay – cited in Richard Lipsey’s *Introduction to Positive Economics* that was at that time used as the major text – that argued in defense of such assumptions. The first encounter with Friedman’s essay made me feel like being intellectually insulted. I viewed its message as manifesting an irresponsible academic opportunism that did nothing to ease my puzzlement. On the contrary, it made the puzzlement deeper. These two experiences at an intellectually sensitive age were sufficient to result in a lifelong commitment and devotion.

Intermediate and advanced courses in economics did nothing to alleviate the puzzlement and discomfort. It would probably have been impossible for me to continue my studies in economics had I not also been a student of philosophy. I decided I would survive my economics studies by combining the two, by looking at economics from the point of view of the philosophy of science. I hoped this would help me understand the discipline that appeared so odd to this student. I was lucky as Helsinki was at that time one of the centers of frontline post-positivistic philosophy of science. Naturally, we learnt about Popper, Kuhn, Feyerabend, Laudan and Lakatos, but at the core of the attention was scientific realism as conveyed by two distinguished advocates, Ilkka Niiniluoto and Raimo Tuomela. I also studied the works of other scientific realists such as Wilfrid Sellars, Mario Bunge, Jack Smart, Clifford Hooker, Hilary Putnam, Richard Boyd, and others. We examined the structuralist (Sneed-Stegmüller) conception of theory structure and dynamics as well as the work by Leszek Nowak and the rest of the Poznan school on idealizations in scientific theorizing (Nowak 1980). In the latter part of the 1970s, I was also rather influenced by Roy Bhaskar’s first two books (Bhaskar 1975/78, 1979) and by some works by Rom Harré (e.g. Harré 1970).

So when the 1980s dawned and the new field of the philosophy and methodology of economics started taking shape, I had a very different ("post-Popperian") background than many of the visible players, such as Larry Boland, Mark Blaug, Neil DeMarchi, Bruce Caldwell, Wade Hands, and others. As I was to complain later, the field initially came to be dominated by Popperian and Lakatosian themes and conceptual tools (1985, 1990a). I was closer to Alex Rosenberg and Dan Hausman than to those others in my philosophical training, yet there were two things that distinguished me from these two pioneers: my strong anti-positivism due to my early exposure to European and Australian versions of scientific realism, and the problems on my agenda being primarily prompted by my experiences as an economics student. It was to become a major concern on my research agenda to look at the falsehood we students discovered in economic models from the point of view of a realist conception of science.

Soon after Rosenberg's *Microeconomic Laws* (1976) was published, I got hold of a copy of it (with quite some difficulties in that ancient world without Google and Amazon). I admired its argumentative qualities, but it did not very strongly connect with my concerns since its key issues were derived from those of general philosophy of science and philosophy of social sciences rather than from within economics. Hausman's concerns in his *Capital Profit, and Prices* (1981) were closer to mine, and have been ever since – even though here, too, I feel like being closer to the concerns of the students and practitioners of economics. This has shown, among other things, in my reluctance to talk about "laws" in a manner that joins the positivist legacy and that Rosenberg and Hausman have shared in their accounts of economics. I rather talk about causes, powers, mechanisms, and dependency relations in my ontology of economics.

The constitutive questions on my agenda have had less to do with Rosenberg's "is it a science?" and more closely linked to Hausman's "what kind of science is it?" My life-long preoccupation was to be with the more specific question that derived from the worries of an undergraduate economics student: "how does economics relate to the real world?" Virtually everything I have done somehow connects with this broad question that can be approached from a variety of angles.

From early on, I sought a connection between scientific realism and what I would later call the issue of *realisticness* in economics. In the early 1980s, I discarded Bhaskar's framework as I found it too simplistic to be of much help in understanding such a complex subject as economics (much later, Bhaskar was to be rediscovered by others in economic methodology). I moved on to developing a more nuanced discipline-sensitive scientific realism following a bottom-up approach that would be responsive to empirical and local discoveries about peculiar features of various research fields (some might call it a "grounded theory" approach at the meta level). This search was based on the conviction that such a local scientific realism must indeed be actively created given that no sufficiently rich and powerful version was available in the philosophical literature. (1989, 1996b, 1998, 2005a)

An early insight directed me away from a naïve condemnation of unrealistic assumptions. Economic models involve idealizations just like the most respectable physical theories do: just think of the idealizations of frictionless plane, perfectly elastic gas molecule, rigid body, planets as mass points, two-body solar system. So what's the trouble, if any, I asked. It became clear that falsehood in assumptions will not be sufficient grounds for an anti-realist instrumentalism about economic theory. This insight would drive and shape my later inquiries. The key idea that gradually emerged was that false idealizations often serve an important purpose, that of theoretically isolating causally significant fragments of the complex reality. Getting to this idea was helped by studying Marshall's writings on method, J.H. von Thünen's model of the isolated state, and Nowak's work on idealization. The analogy with experimental method also facilitated developing the idea: experimental isolation and theoretical isolation have a similar structure. (1992a, 1994a, 2004a, 2004b, 2005c) Among other things, these insights enabled me to overcome my early dislike of Friedman's essay and to develop a generous interpretation of it as a realist statement (1986, 1989, 1992b, 2003, 2008a). I have been rather lonely with this interpretation as most other commentators of Friedman have labeled him an anti-realist instrumentalist.

Alan Musgrave's seminal 1981 paper inspired me to revise and elaborate his suggestions as part of a larger attempt to develop a set of principles and categories that would help identify the various functions that unrealistic assumptions serve in models.

(2000a, 2004c) Indeed, it became a crucial idea in my framework that any assessment of an assumption (and more broadly of a model) must be dependent on a sound understanding of its function, and more generally on the pragmatics of research questions. These ideas have been helpful also in examining debate and change in economic theorizing: debate and change can often be redescribed in terms of (contested, requested, and suggested) isolation, de-isolation (supplementation) and re-isolation (replacement) (2004a).

Three other perspectives have been important in my attempt to understand economic inquiry and its real world connections. One has been the ontology of isolation. Here I have emphasized causes (causal processes and mechanisms) rather than laws (while Hausman used to frame his arguments in terms of laws), and I have entertained an "essentialist" notion of the world having an objective structure, including ideas of stronger and weaker causes and connections as well as of real modalities of possibility and necessity. The idea of ontology entailing evidential constraints on theorizing naturally evolved, to supplement the focus on empirical tests by most other economic methodologists (2001b). Case studies on Austrian and New Institutionalist Economics have been of help here. Rosenberg's suggestions about the role of folk psychology inspired developing the generalized idea that economics deals with commonsense items such as preferences, households, prices (coined by me as "commonsensibles") by variously modifying and rearranging them. (1990b, 1992b, 1994, 1996b, 1997, 1998, 2002a, 2005a)

Another perspective has been provided by the insight that economists appreciate theoretical and explanatory unification. Economic theorizing and modeling are driven and constrained by the ideal of explanatory unification and the dislike of 'ad hoc' features in models. Economists pursue explanations that derive from a shared set of principles, or from an "*ur*-model" and its specific variations. A unified theoretical framework capable of accounting for a large variety of different kinds of phenomena has been established as a regulative ideal of economic inquiry. This has further ramifications for the expansion of economics to the domains of neighboring disciplines, as in the much disputed "economics imperialism". (1990b, 2001a, 2002b, 2004a, 2007; Mäki and Marchionni 2007)

The third perspective deals with the social conditioning or shaping of economic theorizing, including the rhetoric, sociology, and economics of economics. My friendly confrontations with Deirdre McCloskey have given me the opportunity and incentive to develop a realist account of rhetoric, one that accommodates the reality of rhetoric in scientific work while not compromising basic realist tenets (1988, 1993a, 1995, 2000a). My work in the sociology of scientific knowledge in the context of economics (1992c, 1993b) followed up on Bob Coats's pioneering work and may have served as a bridge from his work to that of Wade Hands (Hands 1994, 2001). Exercises in the economics of economics – examining economics itself in economic terms - have given rise to exciting issues of reflexivity (1993b, 1999, 2005b). Economic inquiry is social activity shaped by various social conditions, but as such this should not undermine its capabilities in accessing real world facts.

STRATEGIC

Most of this work, and its connection to scientific realism, has been motivated and guided by *general strategic principles* like these:

[A] Much of the criticism of economics is directed at wrong targets, and is based on the mistaken belief that criticism is easy – such as when inferring from unrealistic assumptions to models being incorrect. Economics is not at all flawless, but it is not easy to reliably identify its flaws (and even less easy to remedy them), almost regardless of how serious they are. Careful scrutiny is needed to locate the appropriate targets of criticism. Given that the most central set of issues deal with the relationship of economics to reality, an enriched and discipline-sensitive scientific realism offers an appropriate philosophical framework for the required scrutiny.

[B] The approach is bottom-up rather than top-down. Economics is a complex subject that has many peculiar characteristics in comparison to other scientific disciplines; it is different from natural sciences and from other social sciences, while it has similarities with both (themselves far from uniform sets of disciplines). One will not develop an adequate understanding of economics by way of imposing upon it some fancy doctrines borrowed from popular abstract philosophies of science. This easily leads to accounts that distort economics as it is actually done, or to methodological rhetoric that has little to do with actual practice (Popperian and Bhaskarian economic methodologies may have had this inclination). The alternative is to be sensitive to the complexities and peculiarities of economics, and to develop whatever philosophical tools may be required to accommodate those features. This approach is hoped to result in a *realistic* realism about economics.

[C] Many people have wondered about my obsession to render as much of economics as possible being in line with general realist intuitions. I admit the obsession. It is motivated by the hope that this will create – or help unearth - common ground for focused debate. It will help resist some of the unjustified and misdirected criticisms, while also supporting the elaboration of sound criticisms that will hit the target. Some of the misguided criticisms are based on attributing to conventional economics philosophical commitments that are not there (such as “positivism”) or on adopting perspectives that ignore the obvious “realist aspects” of much of economic inquiry (such as radical social constructivism). The obsession also seeks to block certain evasive justifications of unrealistic economic models, in particular those that complacently declare: all models are false anyway, they are to be judged only in terms of convenience and instrumental usefulness, so why bother taking any criticisms about their falsehood seriously! My hope is that once it is agreed that (at least many) economic models are (at least potentially) attempts to acquire truths about the real world, and that such attempts are fallible (but do not fail just because their assumptions are false), the debate will be focused on the right targets (on whether the attempts succeed or fail) and will improve in quality.

[D] The role of realism in my work can also be viewed from the point of view of debates within the philosophy of science between scientific realists and antirealists. Two kinds of issue arise. First, philosophy of science has a legitimate concern with the question: how does scientific realism as a doctrine within the philosophy of science cope with a peculiar discipline such as economics? Supposing scientific realism is an adequate philosophy of chemistry and geology, does it manage to accommodate economics? My answer is: yes. Second, scientific realism has traditionally been formulated as a global doctrine about all of (good or successful) science, but it is obvious that not all of science conforms to its conventional canons. Does information about the peculiar features of economics have implications for our understanding of the contents of scientific realism in general and for the appropriate strategies of arguing about it? My answer is: yes.

[E] My work has sought the attention of multiple audiences, including not just other philosophers and methodologists of economics, but also practicing economists, historians of economics, philosophers of science, and other social scientists. This has been a fascinating challenge, but also a source of difficulties. Each discipline and research community functions in an institutional framework with distinct values and norms, agendas and standards, languages and methods, conventions and traditions, loyalties and rivalries, rankings and authority structures. Having worked at economics and philosophy departments and having participated in many interdisciplinary encounters has helped me entertain several insider's perspectives, but obviously the success in addressing that multiplicity of audiences has been partial at most. This is a more general issue: any intrinsically interdisciplinary field faces similar challenges with their multiple audiences.

SYSTEMATIC

My work involves – builds upon and contributes to - an emerging systematic portrait of economics as an intellectual endeavor and as an epistemic institution. The disciplinary portrait is a vision that integrates a range of ideas, from those about the nature of economic reality and truth about it, through those about theoretical and explanatory structure and dynamics, to those about the institutions of economic inquiry. What follows is a summary outline of the basic ideas and their relationships in their current form. Throughout, the puzzle and struggle is over whether and how theoretical economics connects with the real world. The concept of model provides a useful point of departure. First, though, it is useful to offer a brief and simplified characterization of scientific realism as I see it.

[1] Scientific realism

My conception of generic scientific realism is rather thin and flexible. The world has an objective structure that is not created by scientists as they create their theories and models about that structure. Those theories and models are true or false in virtue of the ways of that objective structure – not in virtue of whether evidence supports them or whether we are otherwise persuaded to believe in them, for example. Finally, good science pursues theories that are true, while being prepared for the possibility of error.

In contrast to standard conceptions of scientific realism in the philosophy of science, my generic or minimal conception does *not* include claims such as these: actual science has most of its theories (at least approximately) true; actual science is predictively successful; and the theories of actual science refer to unobservables such as electrons. In my view these things are empirical and local matters, they vary from case to case, from theory to theory, from field to field, from discipline to discipline. Those ingredients in standard formulations of scientific realism should perhaps be included in a realism about (the most successful) parts of physics, but maybe not in a realism about archaeology or economics. My scientific realism is *local realism* in regard to these and other specific issues. The *global or generic realism* I endorse is the simple and minimal composite idea that, descriptively, there is a fact of the matter concerning the ways of the world and whether our theories have got those ways right, and that, normatively, it is the task of science to get them right. (1990b, 1996b, 2001d, 2005a, 2008b)

[2] Models as representatives, or as surrogate systems

Economics is a modeling discipline alongside others that deal with a complex subject matter, such as biology and meteorology. Therefore, a key to understanding the practice of economic inquiry is to have a refined concept of model. This is not easy given the ambiguity of the term ‘model’ and the multiplicity of kinds of models, and of ways of describing them (2001c, 2005c, 2008c). I take models to be *representations* of some target (such as a real world system, a set of data, or a theory). And I take representation to have two aspects, the *representative* aspect and the *resemblance* aspect. The metaphor I have come to entertain to illustrate this idea is representative democracy: a small set of our fellow citizens are elected to serve as our representatives, and the chronic issue is whether, once elected, the goals they pursue as such representatives resemble our interests that we sought to express in the elections. Scientific models are like those representative citizens in that capacity: models resemble those citizens in interesting ways. So, in an obvious sense, representative democracy can be used as a model of theoretical modeling in science.

Scientific representation is not just a matter of some representative *M* possibly resembling some target *R*, such as a boxes-and-arrows-diagram or a set of equations having a fit with some real world structure. Representation involves an agent *A* (economist, scientific community) employing and using an object *M* as a representative of target *R* for some purpose *P*. This makes representation four placed, and there is now general agreement that this is how models as representations should be conceived. I have added two further components: an audience *E*, its expectations and background beliefs; and a commentary that is used for specifying the other components and aligning them with one another.

> Agent *A* uses object *M* (the model) as a representative of target system *R* for purpose *P*, addressing audience *E*, prompting genuine issues of resemblance between *M* and *R* to arise; and applies commentary *C* to identify the above elements and to coordinate their relationships.

An important feature of this account of models as representations is that it does not require resemblance with the target to obtain, it only requires *resemblance-related issues to arise (or to be capable of being raised) as genuine issues*. In order for them to be genuine issues, it is presupposed that a model has the capacity to resemble its target and that not just any arbitrary resemblances are considered. So a model is used as a representative of some target, and this potentially prompts issues of resemblance between the representative and what it is a representative of. A realist will take the stance that a successful representation does resemble its target in some desired ways – while these desired ways are relative to the relevant purposes and audiences.

The *representative aspect* of representation can be characterized in terms of *surrogate system*, in contrast to the real systems in the social world. Models are surrogate systems, and economic inquiry is *directly* concerned with the properties of such surrogate systems – rather than the properties of real systems. Much of economics consists of the investigation of the properties of model worlds ("let's check what happens in this model"). This is an important observation, since herein lies a major source of possible complaint, based on some sort of realist intuitions: economics (or this or that of its subfields) appears to be preoccupied with a study of imaginary model worlds only, detached from the real world. This is a suspicion that must be taken seriously, but meeting the challenge is not easy. It would be intellectually irresponsible and arrogantly complacent to dismiss it as being just based on ignorance about the achievements of economics or on misunderstanding the scientific method.

Economists build models by imagining and describing and manipulating model worlds populated by perfectly rational agents, games with two players, trade with two countries and two goods, perfectly competitive firms, representative agents, closed economies, zero transaction cost situations. There is no doubt that through their investigations – by manipulating assumptions, performing inferences, deriving results - economists learn a great deal about the properties and behavior of such model worlds. What is not a matter of self-evidence is whether they also thereby learn about the properties and behavior of the real world. This is what prompts the issue of resemblance.

[3] Models and the issue of resemblance

The main challenge derives from a worry about the other aspect of representation, that of resemblance. The suspicion is that viewed as representatives, as surrogate systems, (at least many prestigious) economic models do not resemble real systems in some desirable manner. And because of the lack of resemblance, and economists' lack of interest in it, economic inquiry becomes predominantly a matter of examining the properties of those imagined systems only. The accusation boils down to the thought that the study of model systems literally *substitutes for* the study of real systems.

In response to such charges, one must consider the representative and resemblance aspects of economic modeling together. If one is to please the realist intuitions of the critic, one must show how an interest in the properties of model systems not only does not rule out an interest in the properties of real systems, but also serves the satisfaction of the latter.

The key is to see the difference between direct and indirect access to one's subject matter, along the following lines: Economists build (or should build) theoretical models as representatives of real systems, as surrogate systems, the properties of which are *directly* examined in order to *indirectly* acquire information about real systems. Models are built and studied because there is no epistemically reliable "direct" access available to some deep facts of economic reality. All access is necessarily indirect and mediated by simple images of complex things. But in order for such indirect epistemic access to the real world to be possible and successful, model worlds must resemble the real world in some required ways.

In order to see the issue more clearly, we can draw a pragmatic distinction between models as surrogate systems and models as substitute systems. This is a pragmatic distinction in the sense that it is based on economists' attitudes and practices in relation to their models. *Surrogate systems* are treated as mediating vehicles in attempts to gain indirect epistemic access to the real world: surrogate systems are examined in order to acquire information about the real systems. The issue of resemblance remains a genuine shared concern. *Substitute systems*, on the other hand, are examined only for their own sake, with no further aim or wish of connecting with real world systems: the study of substitute systems substitutes for any interest in real systems. The issue of resemblance becomes neglected. (This revises the more neutral use of 'substitute system' in the terminology of some of my earlier work.)

This can be used to identify two kinds of legitimate criticism from a realist point of view. One kind of criticism attacks styles of inquiry that treat a model as a substitute system only, not even intending it as a means for gaining access to the real world. The alleged problem is that there is *no attempt*. The other kind of criticism acknowledges a model being treated as a surrogate system, but blames it for failing in accessing the social world. The alleged problem is that there is a *failed attempt*. The history of economics exhibits both kinds of criticism.

Three remarks are in order. First, it is not always easy to apply this distinction in practice. What appears to be a model treated as just a freely floating fictional substitute system may be defended as a surrogate system by arguing that its connections to real systems are just very indirect or that those connections will be created in a long enough run. Second, as a special case of the first, a model may be treated as a substitute system and as such it may serve as a test bed for developing concepts and techniques that may find useful applications in later models that will be treated as surrogate systems. The general principle is that there is no fully reliable way of identifying a model either as a surrogate system or as a substitute system in isolation from other models and its own development. Third, even if the distinction is primarily pragmatic, one in terms of attitudes and practices rather than in terms of the "intrinsic" properties of models, those intrinsic properties (together with the media of their description, such as mathematical equations or diagrams) often have pragmatic consequences for how the models are treated. Some models are easier to treat as surrogate systems than others, while these others may tend to invite attitudes that enable or even encourage treating them as substitute systems only (my conjecture is that using very demanding mathematical techniques for describing and manipulating a model is generally more likely – than, say, using diagrammatic methods - to discourage attention to real world systems and thereby encourage treating it as a substitute system only; but note that 'more likely' definitely does not imply 'necessarily' or the like).

Even if economists were ambitious and optimistic enough to treat a model as a surrogate system, there is still an objection or suspicion that naturally arises: models just do not resemble their targets, or do so only very remotely or in otherwise wrong ways. So how can models possibly help us acquire truthful information about the real world? In order to answer this question, we need to understand how the apparent gap or distance between models and the world is created, what this gap consists of, and how it is possible for this gap to help scientists gain epistemic access to the world – indeed, *what appears as a gap from one perspective may be a bridge from another.*

[4] Theoretical isolation by idealizing assumptions

Models characteristically are, or describe, imaginary situations. These situations are imaginary in two ways. First, they are imagined by economists. They are not observed or discovered, they are constructed by economists using their imagination. The power of imagination enables a second sense in which models are imaginary. Namely, those imagined situations are imaginary in that they do not include most of the ingredients in real situations; some ingredients are represented in very idealized form; and yet others may be added even though they do not have obvious correlates in real situations. There is no way that models can avoid being "unrealistic" in many such ways.

In describing imaginary model worlds, scientists employ idealizing assumptions that are false if taken literally as statements about the real world. Perfect information, zero transaction costs, closed economy, ceteris paribus – these are characteristics of surrogate model worlds, not of the real world. Physical sciences employ such assumptions in abundance - just think of frictionless planes, mass points, rigid bodies. Idealizing assumptions are false, they distort the facts. But these falsehoods are *not errors*. They are not hypotheses or conjectures that are proposed with the hope that they will turn out to be true – and in case they turn out to be false, are to be rejected and replaced by others hopefully closer to the truth. Idealizing assumptions are deliberately employed, often with full awareness of their falsehood. At any rate they are not hypotheses conjectured to be true. What is the point? How does a realist accommodate these falsehoods?

The important thing to understand is that *idealizations are strategic falsehoods*. They serve some higher purpose. This purpose is that of theoretically *isolating* some important dependency relation or causal factor or mechanism from the involvement and influence of the rest of the universe. Consider Galileo's law of freely falling bodies, Milton Friedman's (1953) favorite example. The simplest statement of the law only cites time, distance, and gravity – while implicitly or explicitly assuming that air pressure is nil, magnetic and other forces are absent, and so on. The point of these idealizing assumptions is to help isolate the impact of the earth's gravity – gravity alone, undisturbed by anything else – on the falling body (1992b, 2003, 2008a).

This is also the point of much of economic modeling. Consider my favorite example, the very first economic model in the modern sense, J.H.von Thünen's (1826/1842/1910) model of agricultural land use in the Isolated State. It is built on highly unrealistic assumptions, such as a city without dimensions, no other towns, no rivers, no mountains and valleys, uniform fertility and climate, transportation costs a function of distance from the city alone (rather than of the availability of roads etc), the state closed off from the rest of the world, thus no trade, strict rationality of agents, and so on. In these imagined circumstances, a pattern of concentric rings emerges, representing zones of cultivation. But such a pattern cannot be observed in the real world, it is a feature of the model world only. And the idealizing assumptions that imply the pattern also fail to state any facts. So what's the point? The point of von Thünen's highly unrealistic simple model is to isolate one major causal factor that shapes land use patterns, namely distance (or transportation costs), and to show how it works its impact through to the outcome in the imagined conditions. (2004b, 2005c, 2008c)

Economists can be philosophical realists about their models even though these describe imaginary situations (von Thünen was a realist about his simplest model of the Isolated State). This is because it is possible that the mechanisms in operation in those imaginary situations are the same as, or similar to, those in operation in real situations. A model captures significant truth if it contains a mechanism that is also operative in real systems. This significant truth can be attained thanks to the false idealizations employed by the model. Capturing this truth does not require any de-idealization by way of relaxing those assumptions.

Unrealisticness in models is not intrinsically a bad thing. It is often a very good thing. It may even be necessary for achieving important epistemic goals. Unrealistic assumptions must be assessed in relation to their functions in modeling (such as fixing a causal background in contrastive explanations, see Marchionni 2006). Whether an assumption is duly or unduly unrealistic depends on its location in a theoretical structure and the functions it is designed or able to serve.

[5] Model and experiment

Theoretical models are structurally and functionally similar to ordinary experimental setups: both pursue isolation. The major differences lie in the methods of control used in effecting isolation; and in the nature of the materials that are being manipulated (e.g.

real people in contrast to imaginary agents). In an ordinary laboratory experiment, various causally efficacious measures are adopted to control for other things so as to neutralize their impact, while in theoretical isolation, the controls are effected by the force of assumption: those other things are assumed to be absent, constant, in normal states. In this sense, *much of theoretical modeling is a matter of thought experimentation* and the world of theoretical models is a kind of intellectual laboratory world (1992a, 1994a, 2005c). On the other hand, *material experimentation is a species of modeling*: it is a matter of building and examining surrogate systems as representatives of real systems out in the wild. Hence my slogan, "models are experiments and experiments are models" (2005c). In both cases, issues of resemblance arise. We have already discussed the case of theoretical models. Similar issues are involved in building and using experiments that employ causal controls, captured by the notion of external validity (see Guala 2005; cf. also Morgan 2005).

[6] Isolation and metaphor

Economists examining the rhetoric of their discipline, such as Deirdre McCloskey and Arjo Klamer, emphasize that models *include* metaphors. Sometimes they suggest that models *are* metaphors. While it is easy to agree on the former idea – just think of 'equilibrium', 'dictator game', 'human capital' – I would only go along with the latter if qualified as the weaker claim that models *are akin to* metaphors. The reason why I think so is that I take both to be isolations. Metaphors and models are essentially similar in that both highlight limited aspects of their targets. In the case of metaphor, it highlights those aspects of the target that are believed to be similar to some limited aspects of the source. Thus the metaphor of human capital helps isolate those aspects of education that are believed to be similar to, say, financial capital.

[7] Isolation as key to inexactness and separateness

Daniel Hausman (1992) has argued that, descriptively, economics is an inexact and separate science. I have argued that both of these characteristics, properly understood, are derivatives of theoretical isolation (1996a). Hausman says inexactness lies within economic theories (in their premises or laws), but I find it more natural to say that inexactness is a feature of the implications of theories or models. I would say predictive implications are inexact in two ways: they typically come out true only if formulated in terms of permissive degrees of approximation; and they often fail if presented in very precise quantitative terms. As a feature of theory's implications, inexactness is a consequence of a feature of theories or models, namely their incompleteness. Theories and models are incomplete just because they isolate small slices of the world, they capture just a small subset of the whole set of causal factors that in the real world shape the behavior of phenomena. So inexactness turns out to be a consequence of isolation.

The same can be said about separateness. As I read Hausman's notion of separateness, he mostly uses it for characterizing economics as a discipline that studies the consequences of rational greed, or more generally as one that employs a very parsimonious set of theoretical principles having a very broad scope. Separateness turns out to be based on a radical isolation of a small set of explanatory factors that also have an extended explanatory reach. In sum, theoretical isolation is the more basic notion that underlies those of inexactness and separateness.

[8] Capacities and lies

There are many similarities between Nancy Cartwright's (1989) account of economic theory and that of mine. We both believe (that economists believe) in capacities or causal powers: agents have powers of rational deliberation, money has purchasing power, price changes have the capacity to transmit information, smart fiscal policy has the capacity to smoothen business cycles. We both believe that the conception of laws as regularities is not a recommendable idea: regularities tend to break down as circumstances change. We both believe that the world is a rather messy place, in certain respects (we might differ somewhat about those respects). I believe that the notion of capacity is not sufficient for having a sound economic ontology, but that a separate notion of mechanism is needed, and that capacities are properties of both mechanisms and their component parts: much of economic modeling amounts to attempts to describe economic mechanisms (1992a, 1994a, 1998b, 2008c). And while Cartwright has thought that the theoretical models of economics lie just as the laws of physics lie (Cartwright 1983) when considered as claims about the happenings among the messy empirical phenomena, I think that if conceived as representations of capacities and mechanisms, economic models do not necessarily lie, they rather have a chance of being true. Much of the time, Cartwright seems to think that the applicability conditions of a model are given by its idealizations, including the associated ceteris paribus clauses; and that the model has a chance of being true only provided those highly restrictive conditions hold. I have defended the idea that simple and highly idealized models may be true of simple facts about the world while getting more complex facts wrong. I take this view to be part of the long tradition within economics, from Senior through J.S. Mill and J.E. Cairnes to Lionel Robbins and much of contemporary economics. (1994a, 2004c, 2008c)

[9] Ontological convictions and tractability conventions

The method of isolation may be motivated by ontological conviction, the belief that models with unrealistic assumptions are needed to isolate and describe causal factors or mechanisms and their characteristic ways of operation in the real world. Economists at least implicitly distinguish between surrogate model systems and real systems: those in which disturbances are absent and those in which they are present, or closed and open systems. These distinctions coincide, implying that economists believe real systems are open and model systems are closed. Another way of speaking about this is in terms of simplicity and complexity. Economists build simple models because they believe the world is complex. They don't build simple models because they believe the world is simple. They build models based on theoretical isolation because they believe this is the only or the best way to get access to the deeper causes of the phenomena in complex reality. All this is fine for my scientific realism. (Here my realist portrayal of economics seems different from that of Lawson 1997.)

There is another possible motivation behind economic models and their idealizing assumptions that is a little more difficult for a realist to accommodate. Some assumptions are made to facilitate the formal treatment of a model. They increase or enable the tractability of the problems cast in terms of the model, thus they could be called *tractability assumptions* (Hindriks 2005, 2006). The primary motivation in such a case is pragmatic convenience, constrained by a given mathematical technique or framework. Often this is no problem, provided the assumptions that serve a tractability function serve as harmless formal auxiliaries rather than distort an actually held deeper worldview.

Sometimes this is not the case, which gives rise to a serious issue. In such situations, pressures of tractability override important ontological considerations, and the values of formal rigor take over in shaping the focus and strategies of research. This is a worry about contemporary economics that many commentators share (e.g. Mayer 1993; Blaug 2002). A few decades ago economists lacked the mathematical tools for dealing with increasing returns and monopolistic competition in a general equilibrium framework. This violated the ontological convictions of many economists working on development issues: these economists conceived of (major parts of) the economy as being governed by positive feedback mechanisms and market imperfections. In case a conflict between ontology and tractability is resolved in favor of tractability while suppressing ontology, the obvious suspicion is that the models that ensue are (or are to be) treated as substitute systems only. Such a situation may or may not create a disturbing tension that motivates building further models that relax at least some of the relevant tractability assumptions, such as in recent developments in growth and trade theory that now employ models with increasing returns and monopolistic competition. Even though there is no full harmony established here between ontological conviction and the properties of the new surrogate systems, it seems that the process has been at least partly motivated by ontological constraints. In general, I believe *the tension between tractability conventions and ontological convictions is one of the driving forces of progress in economics*. If the tension were to be systematically resolved by privileging tractability and formal rigor while suppressing ontological convictions, economics would be on the wrong track.

[10] Paraphrasing assumptions so as to give them a chance of being true

Assumptions serving to exclude factors that are irrelevant or negligible from the point of view of the purpose of inquiry are often formulated as deliberately false idealizations. So formulated, they appear to make false claims about the absence, constancy, or zero strength of a variable. But they can often also be transformed into claims about properties such as the *negligibility* of a factor, and as such claims, they are given a chance of being true (Musgrave 1981). What first appears as a false claim about the absence of a factor *F* can sometimes be paraphrased as a potentially true claim about a property of *F*, namely its negligibility. This property is relational in that it connects a causal fact of the matter (*F* has impact *C* on some further variable *G*) with a pragmatic fact about our purposes and interests (such as the required accuracy when predicting the value of *G*). Thus a negligibility assumption claims that *C* is negligibly small given our purposes. Such a claim may be true and it may be false. (2000b, 2004c)

If we were to consider von Thünen's model of the Isolated State as a predictive model, the assumptions of no rivers and uniform fertility cannot usually be paraphrased as true negligibility assumptions: the impact of rivers and variation in fertility are not negligible for most predictive purposes. On the other hand, in two-body models of the solar system the exclusion of interplanetary attraction can usually be interpreted as a negligibility assumption because for most accuracy preferences, the strength of that attraction is negligibly small compared to the attraction between a planet and the sun. Similar issues can be raised about, say, models of two-country trade, asking whether other trade relations are negligible; or about models of closed economy, asking whether all trade relations of a given economy are negligible for some legitimate purpose. In answering such questions, one first has to fix the purposes that a model is expected to serve. This determines the upper limit of causal impact that can be neglected. Thereafter, the challenge is the empirical one of estimating the actual impact and checking whether it is below or above that limit.

In case a factor excluded by an assumption is not negligible, the options include relaxing the assumption and paraphrasing it as an *applicability assumption*. There is more on the former below in [11]. In the latter case, what starts out as an assumption about the absence of factor *F* may be transformed into a claim about the applicability of the model to situations in which *F* indeed is absent or at least negligible in its impact, and about its inapplicability to situations in which this is not the case. A closed economy model may be claimed to be applicable to large economies in which the role of foreign trade is negligible for a given purpose of model use. Applicability is a relational property of a model, connecting the model with a domain in the world. Claims about applicability and inapplicability can be true even though assumptions about the absence of *F* were false.

A realist should have no complaint about the above procedures of turning apparent falsehood into truth. But consider other possible uses of the art of paraphrase. A false assumption *A(F)* about the absence of *F* might be paraphrased variously as true claims that make no reference to the real world, such as these: *A(F)* serves useful pedagogical purposes; *A(F)* helps build aesthetically pleasing models; *A(F)* facilitates calculations about the model; *A(F)* is a precondition for accepting a paper about the model for publication; *A(F)* manifests a gender bias in economic inquiry. These paraphrased claims may well be true, and may reveal very interesting facts about economics. But from the point of view of model/world relations, they are true about wrong sorts of thing, so fail to please the realist in looking for a justification for apparently false models. They are about scientific practices only, not about the real world subject matter of economics (2000b, 2004c). If those kinds of paraphrase dominate as the only available options, then the suspicion may arise that the respective models are nothing but substitute systems with little or no contact with the real world.

[11] The needs and roles of de-isolation and re-isolation

I have defended the thought that false idealizing assumptions and the highly isolative models they help build are not as such problematic for a realist. This is because, subject to some further conditions, this style of inquiry may promote the attainment of small yet significant truths about the real world. In some cases, false assumptions may be paraphrased as potentially true assumptions about negligibility and applicability. But economists often also relax some of the unrealistic assumptions, they practice de-isolation by de-idealization, adding further causal factors on top of previously isolated ones. Or they may re-isolate by re-idealization, which is a matter of removing previously included factors and replacing them with previously excluded factors. These procedures treat the original assumptions as *early-step assumptions* that give way to later-step assumptions. These practices have their reasons, too.

One may want to generate more detailed explanations or predictions of phenomena than is possible without adding further factors and complexity into one's model: de-idealization is needed. More generally, one may want to explain a different aspect of the phenomenon, which requires an adjustment in the explanatory factors by de-isolation or re-isolation (2004a). Or one performs de-idealization for the purpose of testing and confirming a model. This is because the empirical data characteristically manifest a multiplicity of causal influences, while a theoretical model typically isolates just one or a few causal mechanisms. In order to align the two with one another, so as to ensure that the data help test what one wants to test, either the data have to be adjusted (by data mining) or the model is de-isolated, or both. Testing may also proceed by way of the theoretical manoeuvre of checking the robustness of certain presumed facts to various assumptions or the factors they depict. One relaxes an assumption and thereby determines whether the conjectured fact is sensitive to that assumption (see Lehtinen and Kuorikoski 2007; Kuorikoski, Lehtinen, Marchionni 2007). Another way of putting this is to say that robustness tests are ways of checking whether an assumption can be treated as a true negligibility assumption. Sometimes the discovery may be devastating, requiring a major revision in one's assumptions. For some important research questions, one is forced to relax earlier idealizations and replace them by others such as when assuming increasing returns, asymmetric information, or positive transaction costs simply because assuming otherwise will yield models that miss causally powerful factors that are far from negligible.

From another perspective, a need for de-isolation or re-isolation may arise when there is reason to believe that previous isolations violate the ontic unity in the world: they impose divisions where the world is indivisible. Just as biologists will fail in representing a system such as the human organism if they consistently exclude the brain or the heart from their theory, economists might fail in representing an economic system for certain explanatory purposes – such as for explaining the performance of a developing economy - if the isolations they employ exclude the role of institutions. Sticking to such ontologically ungrounded isolations would be tantamount to dealing with models that are nothing but fictional substitute systems.

These concepts can be used to deal with debate and progress in economics. Indeed, much of the difference, disagreement, debate, change, and progress in economics can be described in terms of rival and complementary isolations as well as requested and suggested de-isolations (whereby an assumption is relaxed so as to incorporate an additional causally relevant factor in one's model) and re-isolations (whereby the isolations of a model are revised so as to replace previously isolated factors by different ones). Such changes may contribute to progress in the sense of scope expansion (whereby new kinds of phenomena are successfully explained in terms of the same explanatory principles) or causal penetration (whereby black boxes are opened so as to reveal deeper causal mechanisms responsible for the phenomena to be explained). The debates and progressive moves in and around transaction cost economics exemplify these categories (2004a).

[12] Explanation, mechanism, and unification

Much of the time, economists describe their activities in terms of prediction, but there is no doubt that they also engage in explanatory practices. These practices, as *explanatory* practices, are still not very well understood, but a few things can be safely said about them. Explanatory practices typically involve theoretical modeling, and theoretical modeling is typically a matter of isolating causal mechanisms. Indeed, 'mechanism' is one of economists' favorite words, used in a variety of contexts such as kinds of market mechanism, incentive mechanism, and transmission mechanism. Characteristically, mechanisms reside inside input-output systems, they serve as the mediating causal chains between the input and output phenomena. A simple model is supposed to depict the bare skeleton of such a mediating economic mechanism. A mechanism in a successful surrogate system is sufficiently similar to the mechanism in the modeled real system. This is also what makes a model explanatory. By representing a mechanism inside an input-output system, an economist not only conveys knowledge *that* the input and the output are connected, he also conjectures *how* the input, together with the mechanism, produces the output. Answering *how*-questions (how does input *I* produce output *O*?) enables the economist to answer *why*-questions (why *O*?). And answering such how-questions enables the economist also to be more assured *that* there is a causal connection between *I* and *O*, thereby establishing a causal relationship where there appeared to be mere correlation or empirical regularity. Representing mechanisms may therefore also promote confirmation.

Much of theoretical model building in economics aims at explaining patterns of some generality – "stylized facts" - rather than singular events. It proceeds abductively, often attempting to answer the question, "What mechanism could have generated this pattern?" Such a model gives a possible (partial) explanation for the pattern by isolating a possible mechanism that could be causally responsible for, or could have significantly contributed to, the pattern. Much of economic modeling aims at inference to a possible explanation – rather than inference to the best explanation. A scientific realist should find such *how-possibly* explanations perfectly appropriate stages in an intellectual process towards *how-actually* explanations that describe the mechanisms and processes that actually have brought about the *explanandum* phenomenon. But if a how-possibly explanation appears to be the final destination rather than a phase on the way towards a how-actually explanation, the realist will raise questions about whether the exercise is leaning too much towards examining mere substitute systems. However, there is no denial that even a true how-possibly explanation may convey information about the modalities of the real world. Naturally, this presupposes that what the model describes is something stronger than just logical and physical possibility, namely some sort of real social and cognitive possibility.

Much of explanatory activity in economics is driven by the ideal of *unification*: the urge to explain much by little, to explain many kinds of phenomena in terms of the same parsimonious explanatory principles. This shows in the insistence on micro foundations, in the avoidance of “ad hoc” explanations, and in the expansion of the explanatory endeavors of economics beyond its traditional disciplinary boundaries. (1990, 2001a, 2002b, 2007; Kincaid 1997) For a long time, the ideal of *intra*-disciplinary unification has motivated and constrained practices of theorizing and explanation within economics, exemplified by Paul Samuelson’s 1947 book *Foundations of Economic Analysis* and by the later doctrine of rational expectations. In the course of the last half a century, *inter*-disciplinary explanatory expansion has become increasingly popular, as in Gary Becker’s “economics imperialism”. Explanatory unification may be based on more abstract explanatory principles and have a more universal reach, such as in the practice of insisting on rational choice micro foundations in economics, sociology, and political science. Or it may be less abstract and have a more local or regional reach, such as in using increasing returns and monopolistic competition for unifying (the phenomena explained by) location theory, trade theory, growth theory, and so on (Mäki and Marchionni 2007).

Explanatory unification is a generally respectable ideal of scientific theorizing. But unification is neither uniform nor uniformly praiseworthy. Scientific realism can be taken to imply a constraint on preferred kinds of unification. If the accomplishment is mere *derivational unification* by way of deriving a large number of *explanandum* sentences from a parsimonious set of *explanans* sentences or from a compact sentential scheme, this as such is not yet to be celebrated. The realist will hail an accomplishment that makes claims about the real world, not just about logical relationships between sentences. The goal and achievement should be *ontological unification* whereby an explanatory theory unifies what previously appeared to be different kinds of phenomena by establishing an ontic unity between them, by showing that they are of the same kind after all. Ontic unity between phenomena may be due to being constituted in the same way (all matter is made of atoms), or to being caused by the same causal mechanisms (falling apples and planetary motions are governed by the force of gravity). (1990b, 2001a, 2002b)

It is one thing to be able to derive sentences about prices and price levels, wages and unemployment, marriage and politics, crime and addiction, from other sentences about constrained optimization or interactive rational choices in a market. It is quite another thing to establish a unity between these phenomena by showing that they all are manifestations of such choices in the real world. Meeting the latter challenge may benefit from derivational achievements, but mere derivational connections are insufficient for ontological unification. Mere derivational unification without ontological grounding gives rise to justified suspicions of the unifying models being mere substitute systems. This much is at stake when judging whether Samuelson's and Becker's achievements are comparable to those of Newton.

[13] Commonsensibles and the way the world works

The ontological constraints on economic theories and models are shaped by the peculiar ontology of economics as a social science. Physical sciences view the world as populated by quarks and photons, magnetic forces and black holes. The world as depicted by physics cannot be observed by human senses nor is it part of our familiar everyday world of ordinary experience. The ontology of physics takes departure from the commonsense world of stones and trees, chairs and tables. No similar ontological departure from the commonsense realm takes place in economics. Economics views the world more ordinarily, largely comprising familiar entities of folk psychology and commonsense social observation. The world of economics is the ordinary world of firms and households, preferences and expectations, money and prices, wages and interests, contracts and conventions, inflation and unemployment, imports and exports. Economic models refer to a world furnished with what I have coined *commonsensibles*, thus they do not postulate unobservables in the same sense that much of physics does (1990b, 1992b, 1996b, 1998, 2005a).

In economic theorizing and modeling, commonsensibles are theoretically modified (selected, isolated, idealized, abstracted, simplified, aggregated): firms and households as strictly maximizing units without internal organization, complete and transitive preferences, infinitely lived agents, 2x2x2 economies, two-player games, zero transaction cost economies, and so on. Enabled by their modifications, commonsensibles are also rearranged. Rearrangement amounts to revising the commonsense understanding and replacing it by a theoretical picture of the causal structure of the world. A commonsense picture is replaced with a scientific picture that economists hope will get the causal and other dependencies right, such as in arguments for trade as against protectionism and in dealing with collective action dilemmas – or generally by postulating various invisible-hand mechanisms between intentional action and unintended aggregate or collective outcomes. What from a commonsense point of view appears as paradoxical is turned less so by making the mediating mechanisms transparent. Realism about economics is a combination of commonsense realism and scientific realism (1990b, 1996b, 1998a, 2005a; but see Ross 2005).

The fact that economic models are about commonsensibles has epistemological consequences. Classics in economic methodology, such as John Eliot Cairnes and Lionel Robbins, believed that economics is in a better position than physics in having more or less direct access to the basic constituents and causes of economic phenomena. This judgment is in need of elaboration. There may be commonsense access to the realm within which those constituents and causes reside. But the common sense alone is unable to identify them *as* the basic constituents and causes. Economic theory and inquiry is needed for this. (1990c) Nevertheless, what remains significant is that the concepts, theories, and models of economics are strongly constrained by economists' commonsense intuitions, including introspection. This is only natural: after all, economists live their lives as observing and interpreting agents in the economies they model.

This idea of theories and models being constrained by background beliefs can be extended beyond mere common sense. Economists generally hold, at least implicitly, ideas or visions of *the way the world works* (*www*). The sources of information contributing to the contents of the *www* conception are various, ranging from commonsense experience and empirical data to academic education, scientific theories, metaphysical convictions, political and moral ideologies. The contents of this worldview deal with human behavioral dispositions (the role and degree of rationality, selfishness, sociality, morality), functioning of market mechanisms (whether they are predominantly negative or positive feedback mechanisms), boundaries of the economic realm (whether the economic realm is sharply separate from the realms of biology and sociology, for example), and many other things. The important point is that the *www* conception operates as an *ontological constraint* on economic theories, models, and explanations. The constraint mainly functions negatively: proposed theories, models, and explanations that do not meet the *www* constraint will be considered unfavorably – or will not be considered at all – by those holding the respective *www* conviction (or "intuition" - to use a popular phrase). This is how the ontological constraint starts playing an epistemological role, ruling out ideas not worthy of belief, acceptance, or further exploration. Much of disagreement and criticism between economists of different persuasions boil down to differences in their respective *www* convictions. Therefore, those disagreements cannot be understood nor resolved by way of empirical testing only; the call is for ontological investigation and argument. (2001b, 2008b)

[14] Folk psychology and predictive progress

My suggestion that economics is about (modified and rearranged) commonsensibles can be seen as an extension and modification of Alex Rosenberg's favorite idea that economics is formalized folk psychology. Folk psychology is the commonsense conceptualization of human action in terms of intentions and beliefs, desires and expectations, hopes and fears – all these familiar mental terms that the humankind has kept using for millennia. Economic models formalize selected parts of folk psychology in terms of preferences and subjective probabilities, maximization and rational expectations, etc.

Rosenberg (1992) believes this is a weakness and a source of failure of economics as an aspiring empirical science. His criterion of scientificity is predictive progress. Economics fails in generating predictive progress. The reason for its failure is its dependence on folk psychology. This is the reason because folk psychology fails to capture the deeper causes of human behavior. As long as economics is committed to the folk psychological framework, it cannot become a science.

I have not been a fan of this diagnosis (1996a). I believe progress can be generated by way of further modifications and rearrangements of commonsensibles, including folk psychological items. One may make progress by moving from certainty to uncertainty in decision-making, from unbounded to bounded rationality, from maximization to satisficing, from symmetric to asymmetric information, from fixed learning rules to evolving learning rules, from emotionally cold to emotionally ordinary agents, from asocial and amoral agents to ones with social and moral awareness, and so on. Such modifications among the commonsensibles pertaining to agents prompt further modifications among the social and institutional commonsensibles (including their inclusion in the models), such as firm structure, market structure, constitutional structure, incentive structure, and so on. Given that "predictive progress" can take on a variety of forms (that Rosenberg does not analyze), many such changes may amount to progress of some such forms. For example, a move from symmetric to asymmetric information or from zero to positive transaction costs in one's model may predict the emergence of an institutional structure that was beyond the horizon of previous models.

[15] Realism and the reality of rhetoric

Rhetoric is real, and it matters. This is what a realist has to acknowledge in order to be realistic about actual science. Rhetoric is a matter of communication and persuasion, of an agent conveying meanings and beliefs to an audience. This is part of the social dimension of inquiry that is also built into my account of models. The presumed background beliefs and anticipated deliberations and responses of an audience – or several audiences – constrain the construction and use of models, their form and contents. Since a model had better be received as comprehensible and persuasive by the relevant audiences to be of any relevance to scientific inquiry and perhaps to policy purposes, the model is shaped by its anticipated and actualized reception. Note that the notion of an audience can be generalized so as to comprise the "internal audience" of the economist himself. Indeed, the economist carries out a lot of "pre-testing" of his model in his private mind before submitting it to the verdict of external audiences. In both cases, the model is judged for its intelligibility and plausibility. (1992c, 1993b)

Much of what economists do is done with the purpose of persuading an audience – or is done in a way that persuades an audience. There are many kinds of audience to be addressed directly or indirectly: colleagues in the same research field, students, journal editors and referees, department chairs and university administrators, other social scientists, lay people, the media, politicians. These audiences are persuaded to adopt this or that belief, such as of the scientificity or topicality of a theory or technique, of the efficacy of a piece of policy advice, of the excellence of the expertise of the author. Various rhetorical ploys are employed to persuade audiences, such as the use of accessible (sometimes inaccessible) language, illuminative metaphor, appeal to academic authority and trendiness, exhibition of mathematical brilliance, appeal to "intuition" and commonsense experience.

In McCloskey's (19853, 1994) seminal work on the rhetoric of economics, these ideas have been used for downplaying the ideas of objective reality and objective truth (Arjo Klamer seems recently to have retreated from such an antirealist position, see his 2007). What there is in the world and what is true of it becomes nothing but results of rhetorical persuasion. Truth *is* persuasiveness, so truths are collectively constructed in a rhetorical conversation. Truths are *made* amongst those who are eligible to participation in the conversation – namely the well-educated and well-behaved economists, those who abide with the *Sprachethik*, subscribing to canons such as, "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." These canons define the notion of 'honest conversation' in McCloskey's image of economics. Economics is made better by persuading economists to observe the *Sprachethik* and to raise their self-awareness of the rhetorical character of their activities – not by imposing methodological rules on their conduct.

In my alternative realist account of rhetoric, the world and truths about the world are not dependent on persuasion amongst economists and their audiences. I reject the presumption that the occurrence of rhetorical persuasion alone rules out the possibility of attaining and communicating persuasion-independent truths about economic reality (1988, 1993, 1995, 2000a, 2004c). We need to distinguish between what is true and what counts as true (in some culture or group, or at a certain time), or between truth and plausibility. While what is plausible and what counts as true can be manipulated by rhetorical persuasion, what is true cannot. The same applies to what is real. We do not have to think that the *reality* of the connection between minimum wage and employment 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. A model – or a statement made in using it – is not made true (false) by being found persuasive (unpersuasive) by a cohort of economists with a certain educational background, academic incentive structure, and moral standards. Background beliefs and the institutional structure of economic inquiry play a very important role in what is found persuasive and in what counts as true at any given time. They also shape the likelihood of successfully tracking truths about the world by a community of inquirers. But they have nothing to do with what is and is not true. This is implied by my rhetorical realism.

An extreme line in resisting rhetorical realism and supporting antirealism could suggest that economics as it is currently practiced is *nothing but a rhetorical game of persuasion*, perhaps one that systematically violates the *Sprachethik*, that it is not in the business of generating truthful information about the real world. It may be solely preoccupied with the study of the substitute worlds of theoretical models, while the so-called empirical tests would be just rhetorical exhibits. Empirically, I would respond by saying that even if this gloomy picture were correct about some parts of current economics, it is unlikely to be true about all of it. And normatively, the natural remedy would be to preach not just rhetorical self-awareness and the *Sprachethik*, but to preach them *together with realism*.

[16] Economics of economics

Economists customarily describe their scientific activity in terms of building and modifying models, and testing them by checking their refutable implications against empirical data. This portrays the activity as conforming to the canons of what is supposed to be the scientific method. The initiative to study the rhetoric of economics questions these traditional philosophical portrayals, but the attack from rhetoric is not in terms of economics itself – but neither are those traditional portrayals! Recently, some economists and many others, including a few philosophers of science, have started using economic ideas in depicting scientific activity itself – another imperialistic extension of economics as it were. 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 adequate) information about the world. Now, scientists are portrayed as being driven by self-seeking desires in a competitive scientific market: they seek to maximize their own fame and fortune, credibility and prestige, and other such noncognitive personal utilities. This trend is taken by some to imply dispensing with traditional issues in scientific methodology, replacing it with a social science of science. (See Hands 2001)

While I do not think that traditional issues in methodology are dead at all, I am rather fascinated by these new developments towards expanding the domain of economics. Perhaps the most intriguing perspective is provided by what I have called the *economics of economics*, the attempt to look at economics itself in economic terms. Depending on one's choice of economic theory in depicting economics itself, the consequences may be dramatic. One of my own contributions has been to look at Coasean transaction cost economics in its own lights (1999). This is in line with Coase's own advice of doing methodology as an exercise in economics; indeed, he is all in favor of economics of economics. The result appears paradoxical.

Coase dislikes what he calls "blackboard economics" that is detached from real world issues, or economics that deals with models as substitute systems only, as we could say.

Coase would prefer economics to engage in case studies so as to generate information about the details of real economies. But a higher-order application of his own transaction cost accounts to the study of economic science reveals the embarrassing result that blackboard economics is more transaction cost efficient than Coasean transaction cost economics: the intellectual and academic transaction costs of measuring and monitoring scientific performance are likely to be lower in formalized work examining substitute systems on the blackboard than in less standardized case studies of the complexities of real world situations. Therefore, on Coasean grounds, blackboard economics is to be preferred – an outcome Coase flatly rejects!

In my view, the tension must be resolved by way of institutional design guided by realist tenets. The proposal would be to redesign academic institutions so as to shape the (production and transaction) cost structure of economic inquiry in a way that fortifies those academic incentives that function in support of efforts to build models as surrogate systems with the ambition of revealing significant truths about real world economies.

[17] How (not) to criticize economics

What the above observations reveal is that economic model building is driven and constrained by a variety of factors. It is actually constrained by principles, standards, ideals, conventions, and incentives that economists explicitly or implicitly accept as honorable. There are tradeoffs between some of the constraints, thus choices have to be made such that not all constraints will be equally met by a given model or style of modeling. These tradeoffs must be interpreted and choices be made in a justifiable manner. In my philosophy of economics, there is a super constraint entailed by a commitment to realism that should not be easily compromised. Here is a summary of what this means.

Economists build and use unrealistic models with unrealistic assumptions aiming at isolating possible mechanisms that also unify much while persuading various audiences. Model building is constrained by factors such as available data, mathematical tractability, theoretical tradition, rhetorical conventions, intellectual production and transaction costs, academic power relations, and other institutions of economics, as well as intellectual milieu and moral and political ideologies. These goals and constraints do not yet guarantee that the models are more than just fictional substitute systems.

Further rules are needed to ensure that the above sorts of constraint serve as favorable rather than unfavorable means in pursuing truthful information about the world. These include ideas such as the desirability of ontological (instead of mere derivational) unification; accepting any particular consideration of mathematical tractability only temporarily, not as a permanent constraint, and not as suppressing major ontological convictions; epistemically significant persuasiveness being dependent on a sufficiently open and democratic structure of the institutional conditions of rhetoric; and more. On such conditions, some of these constraints may serve justificatory functions. It is in the form of such rules that my realist methodology becomes more broadly normative.

From my realist point of view, there is no general problem with unrealistic models with unrealistic assumptions or the method of isolation by idealization. This means that the locus of appropriate criticism of any chunk of economics does not mostly lie at the level of general philosophical description of method, but rather at the level of how the method is used and how its use is constrained and what results it produces. For example, there may be worries about the contents of any particular *www* constraint, which should prompt ontological argument about the general worldviews driving and constraining economic modeling. There may be worries about particular methods being inappropriate or being inappropriately used in economic investigation, resulting in some systematic distortion of major facts about the social world. And there is a chance that the various pragmatic constraints such as tractability and rhetorical conventions, or academic incentive structures more generally, shape or suppress some appropriate *www* constraint in undesirable ways. The issues are mostly about realisticness, not about realism. (1994b)

So one has to examine the social conditions under which economics makes claims about the world. The institutional-industrial organization of economics contributes to shaping the models and styles of modeling that are favored or are out of favor. Sometimes one may feel that it is easier to point out flaws in the social structure of economic inquiry than in that inquiry itself directly, by arguing that the institutional-industrial structure does not meet the ideal conditions of epistemically virtuous and successful science. This may be taken to have consequences for our assessment of its epistemic strategies and achievements: since economic theories, explanations, and policy advice are produced in institutionally imperfect conditions, we may expect those products to be imperfect as well. The academic and other reward structures may encourage epistemically low-ambition research in encouraging quantitative productivity (but on the other hand, normal science is supposed to be pretty much like this). They may encourage dogmatism and arrogance while suppressing critical and deviant voices (but on the other hand, a suitable degree of dogmatism is supposed to be a precondition for cumulative research). Once one starts listing such possible flaws, and is then reminded of the other side of the coin, things become more complicated again.

This is where the challenge lies – and it is a double challenge. We want to have economic models that provide us with truthful information about the real world. In order for economics to be capacitated and disposed to produce such models, economics had better operate in institutionally ideal conditions. In order for us to describe such ideal conditions and to estimate the distance between the actual and ideal conditions, we need another set of (metascientific) models of those conditions, capable of conveying truthful information about them. But if the former set of models are built in institutionally imperfect conditions, it is likely that the latter set of meta-models – models of modeling - are also produced under imperfect conditions. Our judgments about the institutional conditions required for economics to produce truthful models of social reality therefore seem to be on no firmer ground than the economists' models themselves. (1993b, 1999, 2005b)

There is no reason for despair or nihilism. This is just what science is like, including the scientific and philosophical investigation of science itself: an imperfect human endeavor. The chance of error is there, particularly pronounced in sciences dealing with very complex subject matter; and science itself is a very complex socio-epistemic system, thus the possibility of error in making meta-level claims about science is also considerable. My scientific realist philosophy of economics entertains *epistemic ambition and optimism*, but – as any reasonable realism should – also subscribes to *fallibilism* as the super rule. This rule suggests that a systematic investigation of its possible errors be put on the research agenda of economics. Any discipline should openly recognize and examine its characteristic imperfections. Honesty and modesty is power. Arrogant and pretentious over-confidence would be an expression of weakness.

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