Introduction

Uskali Mäki's realist philosophy of economics[1]

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1 Introduction

Mainstream economics abounds with complicated mathematical models that use a variety of unrealistic assumptions. Assumptions such as infinitely living individuals, perfectly balanced budgets, perfectly rational individuals, zero transaction costs, constant returns to scale, fixed preferences, fictional auctioneers, and societies with no government are regularly employed in economic modeling. Furthermore, some things such as institutions, habits, and changing preferences seem to lean on economic phenomena but are summarily set aside under the suspicious-sounding but omnipresent *ceteris paribus* clause. Given these features of economics, it is not surprising that other social scientists and the general public are often very critical of the whole approach.

The abundance of unrealistic assumptions raises questions concerning the nature of the enterprise: Are economists aiming at the truth at all, or are they just playing an intellectual game in which such assumptions are acceptable for some mysterious reason? Are they studying the economy for real? Are they simply uninterested in truth, or is there perhaps some other way of accounting for their modeling practices? Yet this modeling practice does constitute the mainstream, and this fact alone raises the question of why this is so. Furthermore, many economists are in a position to make major societal decisions. This raises further questions: Are the vast majority of economists simply deluded in engaging in such a weird practice? How can it be that models based on such unrealistic assumptions are assumed to be relevant to policy?

This question of unrealistic assumptions is perhaps the hottest topic in economic methodology. Milton Friedman (1953) famously argued that the realism of assumptions in economic theories did not matter as long as the predictions derived from them were correct. It is easy to see why Friedman's instrumentalist answer has been so popular among economists. If it is pointed out that an assumption is unrealistic, an instrumentally inclined economist may decline from justifying it and merely retort that 'it is of course just an assumption in a model that is not to be taken too seriously, and ... all models are false anyway'. Some economic methodologists (e.g., Boland 1979) are also perfectly happy with instrumentalism. We can at least easily explain the widespread use of false

assumptions if economic theories are not meant to be true, but are merely useful tools for predicting the future and for guiding economic policy.

There are, however, many reasons why most methodologists (and economists) have abandoned instrumentalism. Daniel Hausman's (1994) analogy with cars highlights one problem with it. If a car runs smoothly we do not need to know much about what is under the hood, but as soon as it does not function properly it may be necessary to look more closely at its internal workings. Another major issue is that economics also deals with explanation. From this perspective, it is not surprising that economists spend a considerable amount of time arguing for their assumptions and challenging their peers' assumptions in scientific conferences. It is difficult to make sense of these practices in terms of instrumentalism.

Given the deficiencies of instrumentalism and also in the Lakatosian and Popperian approaches to economic methodology, Tony Lawson and Uskali Mäki launched realism as a meta-theoretical approach more or less simultaneously at the turn of the 1990s.[2] They both agree that science is not just an intellectual game and that scientists should aim for truth. One of the main theses commonly associated with realism is that mature and advanced scientific theories are, by and large, true. Some versions of realism claim that science provides literally true accounts of reality. However, given the ubiquity of unrealistic assumptions, how can economics and realism be reconciled?

Mäki and Lawson part company here. Although both are waving the flag of realism, their approaches are fundamentally different. Lawson draws inspiration from Roy Bhaskar's critical realism and uses it as a platform for arguing that mainstream economics is not consistent with realism and ought to be changed accordingly. In contrast, Mäki's aim has been to show that, despite the seeming discrepancy between realism and unrealistic assumptions, scientific realism can be reconciled with many approaches in economics, both mainstream and not-so-mainstream. The starting point is that there is a difference between *realism* (a meta-theoretical doctrine) and *realisticness* (an attribute of scientific representations such as assumptions).[3] Scientific realism can thus be perfectly compatible with unrealistic assumptions as long as the *function* of these assumptions is compatible with realism, and the methodologist's task is to sort out the various functions that different assumptions have. The pivotal role of unrealistic assumptions and a deep commitment to scientific realism jointly determine Mäki's research agenda. The main question is: *what does it take to formulate an account of realism and of economics such that the two are compatible?*

Monographs (Lawson 1997, 2003) and anthologies (Cruickshank 2003; Fleetwood 1999; Fullbrook 2009; Lewis 2004) in which Lawson's account of realism is discussed are readily available. Mäki is a prolific writer on economic methodology, but has mostly published in specialized journals. His views are thus not equally accessible to a more general audience of philosophers and economists. He provided an intellectual history himself (Mäki 2009e), and revealed similar information in an interview conducted by his own students (Mäki 2008d), and his views have also been compared to Lawson's realism and/or rhetoric in

various contributions.[4] However, there is no comprehensive analysis of Mäki's philosophy of economics. This volume endeavors to correct this omission by focusing on Mäki's scientific realist account of economics.

Mäki has made contributions in various areas of the philosophy and methodology of economics, including unrealistic assumptions, the rhetoric of economics, Austrian economics, institutional as well as mainstream economics, the sociology of knowledge, economic models and the economics of economics.[5] Aside from his realist philosophy of economics, he is perhaps best known for his analysis of the method of isolation. In more general terms, his peers have certainly learned to recognize his careful analytical style, which his under-laborer conception (Mäki 2000b: 47) of the role of philosophy legitimizes.

The purpose of this introductory chapter is to show how the different parts of Mäki's work hang together through the notion of realism and to provide a description of his vision of economic methodology. Reading everything he has written (to which I had access) has been a revealing experience. I have come to realize that his academic production constitutes a unified framework in which a few central concepts and distinctions are applied, and at times slightly modified, in different contexts. He provides not just an isolated argument but rather a full philosophical system that is, nevertheless, constructed in a piecemeal fashion. In order to illustrate this unity I will also discuss his early work starting from the beginning of the 1980s, thereby covering topics such as *explanation as redescription* and *essentialism* about which he has not written since the beginning of the 1990s. I am doing this in the hope of being able to provide a deeper understanding of other topics that he does continue to discuss, such as *truth in models* and *commonsensibles*. In the last decade he has often claimed that he has realist intuitions (Mäki 2000c: 112; 2002a: 9; 2003b: 66; 2005b: 235; 2008c: 296). Although he might no longer subscribe to all of these older ideas, I believe that discussing them here will help in articulating what such intuitions might be.[6]

Although the aim is to provide an overview of Mäki's realist philosophy of economics, I also hope to cater for experienced readers by providing some new observations and interpretations of his work. Given the clarity of Mäki's writing, I cannot hope to do better than he does with respect to some key ideas. If you have not read them already, I particularly recommend his works on realism (1989, 1990d, 1992a), on models and assumptions (2000a, 2011a), and on the method of isolation (1992c, 1994b), and a particularly characteristic contribution that combines realism and isolation (2004a).

I will also introduce the various chapters included in this volume, giving them a context within Mäki's realist philosophy of economics. In a true realist spirit, the style of the articles will be that of critical engagement rather than outright advocacy. The authors take on various aspects of Mäki's realist philosophy of economics. Collectively, they provide a lively account of the scientific realist position that has influenced the philosophy of economics through Mäki's writings. This book should be of interest not only to philosophers of economics but also to social scientists and economists reflecting on the nature of their science.

The topics covered in this introduction are discussed in roughly the same chronological historical order in which Mäki presented them. I will start in Section 2 by presenting Mäki's vision of doing the philosophy of economics. He is famous for distinguishing between various kinds of realism and realisticness. I suggest in Section 3 that he is particularly critical of ontological and referential anti-realisms, and discuss why these aspects of realism are so important to him, given his account of explanation as redescription and essentialism. My aim in Section 4 is to sort out how his notion of *commonsensibles* is related to these issues and to represent his views on constructive empiricism. Section 5 is devoted to a discussion of Mäki's account of unification, and Section 6 to an exposition of his method of isolation. I consider his contributions on realisticness and kinds of assumptions in Section 7, and discuss his most recent work on models in Section 8.

2 The vision

The very notion of a vision of what one is engaged in doing is particularly important to Mäki. This aspect of his approach is clearly evident in the papers he wrote in Finnish during the early stages of his career. I hope that laying out the main contents of these early papers in the international arena will enhance understanding of how a devotion to realist philosophy can arise from such a vision in a fairly natural way.[7] He writes, for example:

> The starting point of the [philosophical] project is the conviction that economic methodology should not be based merely on prescriptive apriorism or descriptive empiricism ... but rather at least partly on scientific results and realistic metaphysics. ... Theoretical conservatism, persuasive argumentation, the monopoly of one paradigm or the free competition of approaches ... and the strict application of predictive power have been proposed as general methodological principles. ... In economics, conditions for these should be found from the nature of the economy and from the special relationship between economists and the economy. Metaphysical considerations may then give a partial explanation of descriptive methodology: Why do economists act as they do? Because the nature of the object, the beliefs concerning it, and a certain point of view towards it are prone to provoking such action. ... Why should economists proceed in a certain way? Why would it be rational? Because the nature of the object of investigation and a certain point of view towards it require such actions in order to achieve a certain goal.
>
> (Mäki 1981–1982: 177)[8]

Similarly:

> Consider your project simultaneously as an empirical and a theoretical one. Do not, for example, borrow anything from the philosophy of science without checking it in relation to those idiosyncrasies and without modifying it accordingly.
>
> (Mäki 1990c: 457)[9]

These programmatic declarations show that Mäki's vision was, from the very start, to engage in economic methodology in such a way as to respect the disciplinary peculiarities of the target discipline. Such naturalism is a feature that is sometimes associated with the realist philosophy of science (e.g., Boyd 1983). From this perspective, it is not surprising that he presents his most vehement criticism (Mäki 1994a) when he perceives that somebody's work in economic methodology does not pay sufficient attention to the particular features of economics.

Early book reviews (e.g., Mäki 1982) hint at another significant aspect of his vision. He criticizes the author for 'dilettantism in the philosophy of science, which shows in the large number of errors, and also in a lack of commitment to any philosophical school'. Similarly, he claims that 'it is difficult to avoid the impression of eclecticism' (Mäki 1980a: 335). The following quotation reinforces the importance of this issue to him:

> It is clear we cannot tackle economics with empty hands – with no philosophical concepts – just as economists cannot tackle economic problems without their theoretical notions. ... You cannot analyse economics without recourse to existing philosophical notions.
>
> Economists are not philosophers, and although they sometimes try to rationalize what they are doing in terms which have been borrowed from traditional philosophy, the relationship between what they rationalize and what they use for rationalizing is external and even arbitrary. Much of methodological work on economics suffers from the same problem.
>
> (Mäki 1987a)

The idea is that an advanced scholar in philosophy has to be committed to some particular philosophical doctrine in order to avoid shallow theorizing. In other words, it may be necessary to frame questions and arguments in terms of a broader philosophical doctrine in order to be able to conduct a structured discussion about them. Having a structured academic discussion implies that the discussants know what other arguments the opponents have presented in favor of their alternative doctrines and have understood the problems associated with them. This helps to focus the discussion on the moot points because the interlocutors may quickly endorse or reject the standard arguments and give their reasons for this.

At the turn of the millennium, Daniel Hausman leveled a criticism[10] at Mäki and Lawson's realisms. The critique rested not on the idea that realism itself is wrong, because Hausman is also a realist of sorts, but rather on the idea that one has to 'wave the realist flag'. Hausman thus precisely challenged the idea that it was necessary to frame all methodological discussion in terms of a doctrine such as realism. He noted that it was more illuminating to concentrate on the differences

between the various views in economic methodology than on their similarities, and that these differences did not typically concern realism. Thus, one interpretation of Hausman's position is that he, too, endeavors to focus the discussion on the moot points. However, he argues that framing methodological issues in terms of realism confuses rather than clarifies the issue. Indeed, given the fact that Mäki's and Lawson's main tenets are so different, one might wonder why either of them wished to continue waving the realist flag. For example, although others have criticized Mäki's account of isolation, Lawson is the only scholar in the philosophy of economics to have expressed major qualms about the *very idea* of such a method. He argues that it presupposes the kind of closure that is not to be found in the economy (Lawson 1997: 234–236). Would it not be better to not be associated with someone whose views are so entirely different from yours?

Nevertheless, it is perhaps not very surprising that Mäki continued to wave the realist flag after Hausman's assault. Mäki's main philosophical project is to work out a version of economics and a version of 'mainstream' realism such that the two can be reconciled with each other. Realism is a widely espoused position in the philosophy of science, and this is a good enough reason to see whether economics 'fits' with it. It would be interesting, after all, if economics could not live up to the standards of the most important philosophical meta-theory. Furthermore, Mäki considers his version of realism to be closer to the mainstream conception than that of Lawson, and justifiably so. Even though Bhaskarian critical realism is perhaps the best-known version among the social scientists, within the philosophy of science it is considered merely a particular strand of realism that is seldom discussed and which has a very limited number of supporters.

Even though critical realists share many of the fundamental tenets in Mäki's realist vision of economics, such as an emphasis on finding causal mechanisms, they believe that economic modeling cannot be reconciled with their version of realism. The aim in Kuorikoski and Ylikoski's chapter (this volume) is to evaluate critical realism with the conceptual tools of the current philosophy of science, and thus to discuss it alongside Mäki's philosophy. The focus is on the main positive critical realist proposal for a more fruitful economic methodology, namely contrastive explanation. In line with a recent paper by Tony Lawson, the chapter uses Akerlof's market for lemons as a case of a successful exercise in contrastive explanation.

3 Referential realism, explanation as redescription, and essentialism

Mäki is no intellectual rebel because he has always subscribed to the mainstream view of his home university in Helsinki, in other words to scientific realism. It is interesting to note that he has only once given an explicit definition of a generic version of realism that he seemed to endorse[11]:

1 The world is out there, independently of our mind but knowable to our mind.

2 The essential nature of what there is in the world is mostly inaccessible to our everyday experience and commonsensical thinking.
3 This is why we have to construct scientific theories to find out what there is and what is its essential nature.
4 The theoretical terms in our theories are to be understood as putatively referring expressions and theories themselves as at least approximately true representations of the real world. Theories thus conceived cannot be reduced to some observational language nor be taken as mere tools of inference.
5 Observable phenomena should be explained as causal manifestations of some underlying generative powers or mechanisms, which are grounded on the essential nature of real things. It is the task of scientific theorizing to reveal this causal basis of the world.

(Mäki 1983a: 257–258)

It is difficult to tell whether he would still subscribe to all facets of this definition, or even whether he thinks that trying to give such an all-encompassing definition makes sense. In any case, at the time he wrote it, he did subscribe to it, and he also specified what this might be taken to mean: 'To recommend SR [scientific realism] as an interpretation is to recommend it as a foundation of real research practice and vice versa (ibid.: 262)'. This amounts to a *prescriptive* use of realism. He also seemed to subscribe to a *descriptive* version: 'Scientific realism can be understood as a descriptive thesis as to what kind of theories are actually held in management research' (ibid.: 259).

A few years later he developed a breath-taking number of distinctions among different realisms (for example ontological, referential, representational, semantic, veristic, methodological, and epistemological),[12] and it may be difficult to determine which one he now subscribes to. I offer my interpretation here.

Mäki (1989) introduces some of these doctrines as follows. Ontological realism with respect to economics holds that its objects exist. Referential realism holds that terms in economic theories refer to something real. Representational realism requires that economic theories or terms represent entities in the sense that they tell us what those entities are like and how they behave. If the theories and terms can be claimed to be true by virtue of the reality, we arrive at veristic realism.

An important feature of the classification is that the different realisms are lexicographically ordered. One cannot be a realist with respect to some items on the fringe without being a realist with respect to the core items. In the innermost core is ontological realism (Mäki and Oinas 2004), which is followed by referential realism, then various semantic realisms such as representational and veristic.

Sometimes one gets the impression that Mäki is trying to *maximize* realism (Mäki 2005b), in other words that he is trying to find the strongest possible realism that fits a given discipline. From this perspective, the point of the strategy of classifying various kinds of realisms is not so much to find the most

suitable one that would fit all possible purposes and contexts, but rather to provide weak enough versions that can be made to fit more broadly. Given that Mäki has never explicitly argued against any particular version of realism, and given this maximizing conception of realism, he seems to subscribe to every kind of realism as a normative thesis, including the stronger versions. Showing that some kinds of descriptive realism are not compatible with economics does not constitute an argument against realism per se, but merely delineates the scope of the different versions. Maximization is thus constrained, in the sense that the applicability of descriptive realisms depends on the features of actual research conducted in the particular target sciences.[13]

The closer one gets to the core, the less willing Mäki is to swerve. One of the noteworthy features of the definition of realism given above is that it includes the idea that the theories and terms should be considered as *referring* to the real world.[14] At around the same time he wrote: 'Against fictionalism and instrumentalism, scientific realism considers scientific theories as (hypothetical) descriptions of the world, not just derivation devices. The postulated entities must be understood to exist at least possibly' (Mäki 1984: 81).[15] He thus seemed particularly reluctant to accept ontological and referential anti-realisms in either their descriptive or their normative versions.

Evidence of this interpretation comes from the fact that, for example, he explicitly argued (Mäki 1999a, see also forthcoming) against Machlup, who claimed that assumptions in economics contained theoretical terms (Mäki 1983–1984: 241) and thus did not have truth values (Mäki 1980a: 333–334, 1998d: 254). In the following section I will attempt to spell out some possible reasons why Mäki seemed to be so unwilling to accept referential non-realism. One such reason is that if the terms of theories do not refer, the notion of explanation he has propounded, *explanation as redescription*, becomes impossible: 'Under an instrumentalist conception we cannot talk of a "redescription" of business firms because they do not have semantic properties' (Mäki 1985: 121).

What, then, is explanation as redescription? Mäki borrowed the notion from central realist philosophers such as Wilfrid Sellars. He wrote: 'To describe a thing is to attribute to it properties by means of some conceptual framework' (ibid.: 121). Theories are the vehicles of redescription (ibid.), and indispensable to it (Mäki 1990d: 321). Explanation involves the redescription of explananda, and the idea of something being redescribed presupposes an antecedent description of that something. 'The events and their co-occurrences and sequences, as well as the entities involved in them, are redescribed in terms of theory as what they are believed to be, namely as manifestations of "underlying" entities and processes' (Mäki 2001e: 371–372).

Theoretical redescription is intrinsically related to essentialism. It means describing the essence of the object (Mäki 1992b: 44).

> Objects of empirical descriptions at the level of Erkenntnis are redescribed at the level of Verständnis as something else. This something else is purported to be what those objects really are. This is often referred to as the 'essence' or 'nature' of objects. There is a relation of natural necessity between the powers and the nature of a thing. For example, the instances of money have the powers and capacities they do by virtue of the real essence of money
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> (Mäki 1990a: 301).

The point is that we attempt to explain the way an entity is by describing what it is (Mäki 1990d: 320–321). To explain a thing or a phenomenon is to refer to its essence or inherent nature (Mäki 1987b: 109). Theoretical redescription gives us the best possible clue as to what empirically described objects really are, i.e., what their nature or essence is. The way the objects behave is dependent on what they are (Mäki 1990a: 304). The view that seems to emerge from these characterizations is that the essence of objects accounts for their behavior; they behave as they do because they are what they are. Essentialist realism posits that scientific theories may have essences as their real objects, and that they may be true about those essences.

Mäki must have discovered at the beginning of the 1990s that the same object of investigation was taken to have a different essence by different scholars. For example, one institutional theory describes transaction costs as the essence of the economic institutions of capitalism (Mäki 2004c: 340). On the other hand, entrepreneurial alertness is best understood as a causal power (Mäki 1991b: 14), and the Austrians describe the 'market process as a purportedly realistic representation of the essence of the market' (Mäki 1992b: 36), and 'entrepreneurship as the essence of the market' (Mäki 1992b: 54), whereas neoclassical economists would consider the essence of price behavior to be a manifestation of maximization (Mäki 1992a: 189). Furthermore 'Friedman could argue – though he does not – that the neoclassical theory of the firm is a 'descriptively false' representation of the appearances of real business firms (i.e., it is unrealistic in this sense), but that it is also a true representation of the essence of "the fundamental structure" of firms (i.e., is realistic in this other sense) (Mäki 1989: 186–187).' Thus, the essence of economic institutions is transaction costs, but particular institutions such as the market and the firm have different essences. Finally, the firm's essence is both entrepreneurship and the maximization of profits. If an essence describes the main features of a thing independently of the context, firms seem to have several essences, or none at all.

It seems to me that if Mäki continues to subscribe to essentialism, it is an essentialism that is modified precisely with respect to this issue of the context. In his more recent papers on models (Mäki 2009b, 2011a, 2011d), which I discuss more fully in Section 8, he argues that modelers' purposes and audience in part determine what is taken to be causally important.[16]

A second possible reason for holding steadfastly onto referential realism is that the notion of truth aptness is so important for Mäki's brand of realism. Because veristic realism presupposes referential realism, he is not willing to make concessions to the latter. In fact, he does not accept entirely fictional entities, forces, or relations at all in economic theories: 'It is the isolations, closures,

and simplifications involved in economic models that are artificial rather than at least all the economic entities, relations, and forces that are postulated' (Mäki 1992d: 95). His idea seems to be that if the terms in scientific theories do not refer, we cannot assign truth values to them either. However, it is obvious that truth-bearing propositions can be deduced from theories that contain such fictional elements. Machlup (1955) claims, for example, that even though marginalist firms are mere fictions, one may derive propositions concerning how a market consisting of such firms would respond (in terms of output or price, for example) to various changes in circumstances (such as taxes, weather conditions, or changes in technology). However, this solution seems to be entirely unacceptable to Mäki.[17] He might think that considering the results of theories and models to be the relevant truth bearers is too instrumentalist. As I will show in Section 7, he identifies (thoughts about) mechanisms rather than the model's conclusions as the crucial truth bearers. Presumably, mechanisms and results cannot both be the *crucial* truth bearers of the *same* model. Mäki's strategy is thus consistent in that, given all the other things he says about models and theories, he has to insist that only real entities be included in models, otherwise it would be impossible to provide an account of truth that has mechanisms as the relevant, real, and only truth bearers.

It is also worthwhile pointing out that Mäki criticizes the redundancy theory of truth[18] on the grounds that 'there is no property for truth bearers to bear, thus there are no truth bearers' (Mäki 2004b: 18). As will be pointed out in Section 8, this aspect continues to be important in his recent publications on models. Given the importance of truth aptness and of specifying what truth is about, it is not very surprising that he is an avowed supporter of the correspondence theory of truth, which posits that a sentence, model, or whatever, is true if it represents things in the world as they are.

Arguing for the correspondence theory, and particularly for a non-epistemic conception of truth, is one of the dominant themes in Mäki's critique of the rhetoric of economics and social constructivism (e.g., Mäki 1988a, 1993b, 2003b). Indeed, he seems to consider rhetoricians and social constructivists (rather than constructive empiricists, for example) his main opponents. This may be because they tend to subscribe to some versions of coherence or consensus theories of truth,[19] which are not based on the idea that there is an independent reality that determines the truth or falsity of assertions. For example, he calls McCloskey's account, rather disparagingly, 'the angel theory of truth' (Mäki 1995), because attaining the truth would seem to require superhuman qualities from the participants in an ideal discussion.

Jesús Zamora Bonilla (this volume) argues that the 'rhetoric of science' debate between McCloskey and Mäki has left some very important questions unanswered. Why are some persuasion strategies successful? What is the connection between the use of certain rhetorical strategies and the actual attainment of other goals? Why is a rhetorical strategy more successful in certain circumstances than in others? Zamora Bonilla claims that his game-theoretical models provide answers to some of these questions.

4 Commonsensibles and constructive empiricism

A third reason for the centrality of referential realism is related to Mäki's claim that economics deals with *commonsensibles*. The idea presented here is that the ubiquity of commonsensibles in economics provides an argument for its referential realisticness: if it is essentially about commonsensibles, the terms of economic theories refer to them, and thereby the thesis of descriptive referential realism (about economics) is vindicated (see, e.g., Mäki 1996a).

> Instrumentalists are taken to treat unobservables as non-referring expressions. ... Most of economics seems to be dealing only with observables of sorts ... preferences, and objectives, beliefs and expectations, goods and their prices, costs, benefits, money and market exchange ... are very much part of our commonsense experience.
>
> (Mäki 1998c: 307)

It is sometimes claimed that some of these items, such as preferences and beliefs are unobservable. However, according to Mäki, they are observables of a sort because they concern the common-sense furniture of the human world.

The notion of commonsensibles is based on, or at least is closely related to, the Sellarsian notion of redescription: 'Such commonsensibles are represented in economic theory rather differently from the way they are represented in our common-sense understanding of the world' (Mäki 2002d: 95). The idea is that we already have an understanding of commonsensibles (Mäki 2005b: 247–248), but economic theory modifies and rearranges them (Mäki 1996a, 2009e, 2011c). Most social science is a study of the manifest image, the realm of commonsensibles (Mäki 2005b: 249). It is not their existence that is the issue, but rather their causal role and relevance in the functioning of social systems. The prominent issues of realism in the 'sciences of commonsensibles' thus deal with the existence of causal relations, and with the truth of causal hypotheses phrased in terms of theoretically modified commonsensibles (see also Mäki 2000c: 112). 'Scientific realism about these units of science is not about the existence of theoretically postulated unobservables nor about whether we are entitled to believe in the approximate truth of theories of them' (Mäki 2005b: 250).

Although he introduced the substantive term 'commonsensible' in 1998 (Mäki 1998a: 307), he had already mentioned the basic idea during the previous decade.

> According to this conception [Friedman's], nothing follows from acceptance of a theory about its truth and about the existence of its objects. Beliefs about these questions ... are formed on grounds independent of accepting or rejecting a theory. This is possible, provided that the objects of economic theory are regarded as commonsense objects, i.e., objects that are accessible to us just by means of our everyday experience and commonsense frameworks. ... We have information about firms, and this information permits Friedman to make the judgment that neoclassical theory is unrealistic.
>
> (Mäki 1989: 194)

The context in which Mäki discussed commonsensibles in 1989 and 1992 provides interesting clues about what his realism amounted to. Constructive empiricism is often considered the main challenge to scientific realism. It holds that theories are about real things and truth-valued, but we are never entitled to say that they are true about unobservables. We are at most justified in asserting that they are empirically adequate. Surprisingly, Maki's realism has some commonalities with constructive empiricism. This becomes more evident if we highlight the differences between his realism and the conception of realism that has become standard in the philosophy of science.

Van Fraassen's famous characterization of realism reads: 'Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true' (1980: 8). Mäki (1992a: 182; 1993b: 25) sides with the constructive empiricists on this issue in arguing for a difference between accepting and believing a theory. Even though epistemic optimism is often considered one of the key characteristics of realism, Mäki seems to think that at least the 'generic' or 'minimal' conception does not subscribe to it:

> 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.
>
> (2009e)[20]

Some realists (e.g., Boyd 1990; Psillos 2000) might conclude that Mäki is not a realist at all if he is willing to make such extraordinary concessions in order to accommodate economics. Standard characterizations of *epistemic realism* demand that the mature theories of science are, by and large, true. Mäki's characterization of this concept includes only what is presupposed in the stronger standard notion: 'The epistemological realist holds that the world is knowable'[21] (2001d: 12820). He seems to interpret the issue of whether one is justified in considering a theory true, or whether one should suspend judgment, as one that is relevant primarily to scientists rather than to philosophical theories. Thus, when he argues that it is advisable to suspend judgment, he differs from van Fraassen in terms of who (i.e., the scientist or the philosopher) is supposed to formulate a pessimistic or optimistic opinion regarding this issue, as well as what the judgment is about (a particular theory rather than mature theories in general) (Mäki forthcoming).

In another major concession to empiricism Mäki implies that the underdetermination argument and the Duhem-Quine thesis are cogent (Mäki 1993a: 80; 2001a: 9; 2009d: 314). Consider, for example, the following:

> If modeling were just a matter of a formal exercise with the goal of showing that a stylized fact can be derived from a set of premises, then economics would be an all-too-easy intellectual game. For any given stylized fact, there is an infinite number of possible models that entail it.
>
> (Mäki 2002a: 15)

On the other hand, he proposes the typical realist antidote of using extra-empirical virtues to resolve the underdetermination issue, and also claims that they provide rational grounds for believing in the theory (Mäki 2004a: 1761–1762; 2009f: 106). Examples include simplicity, completeness, unifying power, mathematical elegance, lack of ad hoc characteristics, and coherence with other established theories. He also suggests that ontological commitments may be a criterion for theory choice (Mäki 2001a, 2001e).

Mäki makes an interesting related observation about constructive empiricism: 'If economics is mainly about commonsensibles, then the detail of, and the motivation behind, the constructive empiricist position do not seem to apply very well to economics' (2004b: 20). He continues arguing that according to constructive empiricism, 'there is no way to determine whether the unobservable posits of scientific theories are real and whether those theories are true or false.' He thus seems to be suggesting that since commonsensibles are observables of sorts, there is no reason for epistemic skepticism about their reality or existence. Furthermore, although there might be good reason to withhold judgment concerning the truth of theories that employ commonsensibles, the reason for such skepticism does not derive primarily from the fact that they are unobservable.

I am not so sure whether anything he has written should be taken as an effort to argue for realism in the sense of epistemic optimism concerning the truth of theories that involve commonsensibles.[22] As he points out:

> We appear to have information on commonsense objects independently of what economic theory says about them. It is this information that permits Friedman and others to make the judgment, from a commonsense realist point of view, that the assumptions are unrealistic.
>
> (Mäki 1992a: 183)

However, it seems clear that even though we may have theory-independent information about commonsensibles, this information does not yet provide any assurance that the causal relations assumed in the theories are correct, or that they are true. After all, the commonsensible entities are represented in various ways including by idealizations, simplifications, and abstractions. Thus, Mäki's observation about the lack of motivation for constructive empiricism seems reasonable if the primary content of this sort of empiricism consists of a skeptical attitude concerning the *reference* of theories that deal with commonsensibles. However, if constructive empiricism is primarily an epistemic thesis concerning the *truth* of theories, the ubiquity of commonsensibles will not make it ill-motivated in economics.

Why does Mäki consider rhetoricians and social constructivists his main opponents when the traditional discussion on realism is framed in terms of

unobservables and constructive empiricism? As pointed out above, he seems to think that constructive empiricism is irrelevant in economics due to the prevalence of commonsensibles. One further reason could be that he does not consider any empiricist school in economic methodology worth taking seriously. Boylan and O'Gorman's (1995) 'causal holist' account is a version of constructive empiricism (see also Lagueux 1994, 2010). They argue as follows:

> Moreover, at the epistemic level, the constructive empiricist has no objection in principle to the introduction of Friedmanite hypotheses or assumptions which are 'widely inaccurate'. The construction of a theory which is empirically adequate may require the introduction of theoretical terms, such as idealizations, which are highly implausible or unrealistic. However, the constructive empiricists will not condone any statement in their economic models which is false with respect to some observable economic phenomena. For instance, neoclassical economics says that consumer preferences are stable and transitive, and this statement is either true or false. According to some commentators this claim is factually false. If this is so, then neoclassical economics is empirically inadequate.
>
> (Boylan and O'Gorman 1995: 150)

Boylan and O'Gorman seem to endorse the idea that unrealistic assumptions are acceptable insofar as they concern unobservables, but as soon as they concern observables they should be literally true. If they are not, models containing such assumptions should be rejected. If this criterion were to be consistently applied, there would be no economic models left because assumptions about observables are virtually always false. What they say about Mäki's main focus of interest, namely unrealistic assumptions, is indeed rather underdeveloped.

Note that, as Boylan and O'Gorman argue (1995: 120–121), constructive empiricism, among a host of other approaches, is perfectly compatible with many of the weaker forms of realism identified by Mäki, and in particular with referential realism. Given that Mäki has been willing to show that Friedman is a realist, and recently even proposed in a conference presentation that Hans Vaihinger was a realist on precisely these same grounds, why does he not want to show that constructive empiricism is consistent with realism? There may be several reasons. Empiricists tend to emphasize empirical adequacy, but Mäki wonders whether it is possible to test economic theories empirically by deducing statements from them:

> Once it is understood what *ceteris paribus* clauses or provisos are like, it becomes clear that the very idea of a determinate deductive implication of a given theory becomes undermined, and along with it goes the hope of doing other things that are made dependent on this notion, such as verification, falsification, as well as demarcation and empirical content or meaningfulness defined in their terms. ... The crux of the matter is that the deductive structure of explanatory inferences – also supposedly used for purposes of epistemic appraisal, demarcation, provision of empirical content, etc. – is being undermined. Test statements are not deduced from theories.
>
> (2003c)[23]

Although Mäki (unlike Lawson) has not been openly hostile to econometric testing of theories, the views expressed above might explain his relatively scant attention to empirical testing. It also seems clear that, unlike constructive empiricists, Mäki believes that explanation is to be viewed as an epistemic rather than a purely pragmatic enterprise, that economics reasonably evokes many important unobservable mechanisms,[24] and that seeking truth rather than empirical adequacy is the proper goal of economic theorizing (e.g., 2008d). Given that Friedman's goal of prediction is instrumentalist, the issue of *methodological realism* does not seem to be critical. Although (unlike Lawson) I do not remember Mäki ever having *emphasized* the unobservable character of mechanisms,[25] or having argued for the epistemic role of explanations, he does seem to consider these issues important enough to justify his unwillingness to show that realism and constructive empiricism are compatible.

Mäki's ideas on commonsensibles have been challenged in various ways. In Hausman's view the irrelevance of observability issues in economics should make scientific realism just as irrelevant as constructive empiricism (1998, 2000). He thus appears to agree with Mäki about the prevalence and nature of commonsensibles, but not about what consequences they have for economic methodology. Other authors (e.g., Hoover 1995) challenge the idea that there are no important unobservables in economics. The two papers on commonsensibles in this volume continue this latter line of criticism.

Wade Hands' contribution (this volume) challenges Mäki's argument in a case study from contemporary microeconomics – contemporary revealed preference theory – in which the meaning of terms such as 'preference', 'utility', and to some extent 'choice' is radically different from their common-sense meaning. On the other hand, Hands ends up arguing that, in this particular case, the incompatibility between Mäki's realist account and contemporary revealed preference theory speaks in favor of the former rather than the latter.

Francesco Guala (this volume) also challenges some of Mäki's (and Hausman's) arguments in favor of commonsensible realism, claiming that it is an unstable philosophical position with a tendency to collapse into forms of behaviorism (such as revealed preference theory). In fact, behaviorism may turn out to be the only defensible interpretation of rational choice theory that avoids explicit reference to unobservable theoretical entities. The price to pay for this return to the old orthodoxy, however, is to deny that preferences have a causal role in the explanation of action, and to sever the economic theory of choice from research in psychology and the cognitive sciences.

5 Unification

Many economists are willing to formulate a unified methodological approach that is broadly applicable but at the same time parsimonious, and aim for unified

theories of the economy. Mäki's concern with unification is motivated by realism. From the start he distinguished between different versions of unification in terms of whether or not they were compatible with realism. First he distinguished between *ontological* and *logical* unification:

> Ontological unification emerges as a result of identifying phenomena with aspects or manifestations of a common set of objects. ... Logical unification is brought about when more and more statements within a discipline become derivable from the same set of axioms, or when the same set of statements become derivable form a smaller set of axioms.
>
> (Mäki 1990d: 330–331)

'Derivational unification' later replaced 'logical unification' (Mäki 2001b). *Derivational unification* is a matter of deriving large classes of explanandum sentences from a parsimonious set of theoretical sentences or inference patterns. The explanations are basically understood as arguments (Mäki 2001b: 494). In contrast:

> Ontological unification is a matter of redescribing large classes of apparently independent explanandum phenomena as forms or manifestations of a common system of entities, causes, and mechanisms. It is based on the representational capacities of theories in depicting such underlying systems Explanations are construed as descriptions of the order of things, or goings on, in the world. Theories are regarded as purportedly true pictures of the simplest mechanisms and processes of the world's workings; phenomena are regarded as manifestations thereof.
>
> (Mäki 2009a: 367)

Note how closely the definition of ontological unification resembles theoretical redescription. It is thus natural that ontological unification has been an integral part of Mäki's account of realism since the beginning of the 1990s.[26] In 1990 he subscribed to the view that unification is crucial for explanatory power:

> I suggest that acceptance of an economic theory be based on explanatory power and that the explanatory power of a theory be analyzed in terms of its capacity to bring about ontological unification among apparently independent phenomena.
>
> (Mäki 1989: 195)

On the other hand, he later claims that explaining is not tantamount to unifying (Mäki 2001b: 504). Mäki's writings on ontological unification thus allow him to discuss topics that are closely related to theoretical redescription without having to commit himself to the questionable claim that one or the other of them has something to do with explanation.

Various scholars have different intuitions about how unified the world actually is. Mäki is undoubtedly anxious to find a few cases in which he can show

that a branch of research has been ontologically unified.[27] Ultimately, however, even if the world were to seem like a rather disunified place, he would not be swayed: 'There is no direct argument from the observation of diversity to the denial of unity and the pursuit of unification. The point of unification is precisely to redescribe such diversity as something else' (Mäki 2001b: 504). On the other hand, he has suggested that ontological unification is ultimately a matter of discovery (Mäki 2001b: 502). His current position is that unification is a norm of science, something for which scientists ought to aim (Mäki 2009e: 93), but only insofar as reality admits. On the other hand, mere derivational unification is unsatisfactory, and unification is ultimately acceptable only if it is of the ontological kind. It is an interesting question for future inquiries whether there is some tension in considering ontological unification as a normative ideal on the one hand, and unity as an empirically ascertainable feature of the world on the other.

Mäki has used the notion of unification to serve at least two purposes. First, he argued that its pursuit better conveyed what was going on in economics than Hausman's notion of separateness (Mäki 1996b, 1998i).

> He [Hausman] characterizes the idea in terms of four claims: Economics is defined in terms of a limited set of causal factors; those causal factors predominate in the distinctive domain of economics; the laws of those factors are reasonably well known; thus, economics provides a unified and complete account of its domain (Hausman 1992: 90–93). ... These more fundamental characteristics constitute economics as a science which subscribes, not to separateness directly, but to the ideal of theoretical and explanatory unification, the pursuit of maximal scope employing a parsimonious set of fundamental claims.
>
> (Mäki 1996b: 27)

Second, he has characterized economics imperialism in terms of unification. '*Economics expansionism* is a matter of a persistent pursuit to increase the degree of unification provided by an economic theory by way of applying it to new types of phenomena' (Mäki 2009a: 359). '*Economics imperialism* is a form of economics expansionism where the new types of explanandum phenomena are located in territories that are occupied by disciplines other than economics' (ibid.: 360). Mäki argues that economics imperialism is acceptable if and only if it is ontologically grounded (2009a: 366). Acceptable forms of it thus satisfy the *ontological constraint*: imperial economic applications unify the world by showing that the same kinds of causal processes and mechanisms are at work in different circumstances (see also Mäki and Marchionni, forthcoming b).

Kuorikoski and Lehtinen (2010) criticize Mäki's account of economics imperialism, noting that the distinction between ontological and derivational unification is difficult to apply in concrete cases. John Davis (this volume) gives a critique that is similar in spirit: he considers the development of a whole new set of research programmes within economics that carry the imprint of other

disciplines. These developments have produced approaches that significantly depart from the post-war neoclassical economics paradigm. With this further 'data point' in mind, Davis then questions whether Mäki's deductive type of argument produces a successful account of the phenomenon of disciplinary imperialism. He claims that Mäki's three constraints on imperialism (the ontological, the pragmatic, and the epistemological) are not likely to be satisfied.

Don Ross (this volume) argues that Mäki's philosophy of economics leads to misidentification of the scope of the discipline, and therefore fails to shed accurate light on the relationships between economics and other disciplines. His criticism focuses specifically on the borderland where economics meets psychology and neuroscience, and on Mäki's claim that economics differs fundamentally from physics in considering manifest, as opposed to 'deep', aspects of reality. Ross further argues that putting the emphasis on successful reference to commonsensibles gives a misleading picture of the interdisciplinary relations between economics, psychology and neuroscience, each of which has a relevantly different notion of choice and preference.

6 The method of isolation

When Mäki introduced the 'method of isolation' in 1992, the term 'isolation' was already fairly widely used in economics and economic methodology. He recalls how the idea that false idealizations may serve the important purpose of theoretically isolating causally significant parts of the complex reality came from von Thünen, Marshall, and Nowak (Mäki 2009e: 71), and some aspects of the method of isolation are certainly to be found in Mill.

It seems that the basic idea of studying the contribution of a major factor to a causal system was not new. Consider, for example, what Nancy Cartwright (1989: 191) wrote: 'When all other disturbances are absent, the factor manifests its power explicitly in its behaviour'. Mäki expresses this basic idea thus: 'In an isolation, something, a set X of entities, is 'sealed off' from the involvement or influence of everything else, a set Y of entities' (Mäki 1992c: 318), and 'a set of elements is theoretically removed from the influence of other elements in a given situation' (Mäki 1992c: 321). One of the fundamental ideas in the method of isolation is thus that isolative theories will always violate the *whole truth* about some phenomenon, but if the isolation is successful it may tell *nothing but the truth* about the functioning of the isolated factor (ibid.: 343).[28]

I am emphasizing the abundance of predecessors of the method of isolation not in order to dismiss its importance but rather to point out that Mäki was able to make a lasting contribution to a topic that had already been widely discussed. This highlights the fact that, given the acclaim it has received, in addition to the descriptive title there must be something quite right about the details of his account.

Nowak's (1980) account of idealization was particularly germane because he shared Mäki's essentialism. However, when Mäki introduced the notion of the 'method of isolation' he seemed to vacillate on whether it could be formulated in

essentialist terms. On the one hand, he wrote: 'When trying to describe ontic cores, or essences or essential layers, one deliberately omits most facts about the actual world. Such a description is an attempted theoretical isolation of the ontic core from peripheral factors' (Mäki 1991a: 88). On the other hand, he also wrote: 'It is precisely the point of theoretical redescription of buying and selling by means of identification statements ... to specify those aspects of market transaction that are believed to have *causal relevance*' (Mäki 1992b: 51).

The notion of causal relevance is considerably weaker than that of an essence, and in his discussion of the method of isolation he distinguishes between the two: 'It is not, metaphysically speaking, particularly informative to equate the notion of basic determinants ... that is those which affect most strongly the phenomenon under consideration ... with the notion of 'what is real' or the 'inner structure of the phenomena' (Mäki 1992c: 339). He was thus admitting that the method of isolation was not committed to essential realist ontology. However, he quickly qualified this claim: 'Still, I agree that the method of isolation and some kind of ontological essentialism are often related and that their relationship is far from artificial' (Mäki 1992c: 340).

Figure I.1 represents Mäki's isolation scheme as he presented it in 1992 and 1994.

In Mäki's scheme an *idealization* is a representation that can be formulated in terms of extreme or limit values such as zero, infinity or one (1992a: 176; 1992c: 323; 1994b: 150, 154; 2003a: 502; 2004a: 1724; 2004c: 321). Abstraction, in turn, is a *subspecies* of isolation (1992c: 322), and idealizations and omissions are *means for* achieving it (Mäki 1992c: 325; 1994b: 150, 152; 2004b: 25; 2009e, 2009f: 99) or 'techniques of isolation' (Mäki 1992c: 327; 1994b: 152). Abstractions correspond to two different 'kinds of isolation' (Mäki 1992c: 322–323), 'vertical' and 'horizontal'.[29]

It would be instructive to compare Mäki's scheme to an alternative account that has appealed to many students of idealization and abstraction. Jones' (2005) Cartwright-inspired framework presents them as two basic categories in science, whereas isolation is the fundamental category in Mäki's framework. Jones defines idealization as the misrepresentation of the target, and abstraction as omission. Although Jones' framework is highly intuitive and simple, Mäki's account has more expressive power.

According to Mäki, a *universal* is isolated from its particular exemplifications in an abstraction, in other words it is the process of formulating a universal. Abstraction takes place if a production function is expressed in terms of symbols that do not refer to any particular place or time, for example: $Q=F(L, K)$ (Mäki 1992c: 322). In that *vertical isolation* is defined as isolation that changes the level of abstraction, it is identical to abstraction. Although this seems to lead to a certain redundancy in the framework (vertical isolation = abstraction),[30] and although Mäki's notion of abstraction is rather specific, this way of putting things has the distinct advantage that one can distinguish between the aim of theorizing (isolation) and the means (idealization and omission). Furthermore, it provides a method for analyzing cases in which an operation is not analyzable in

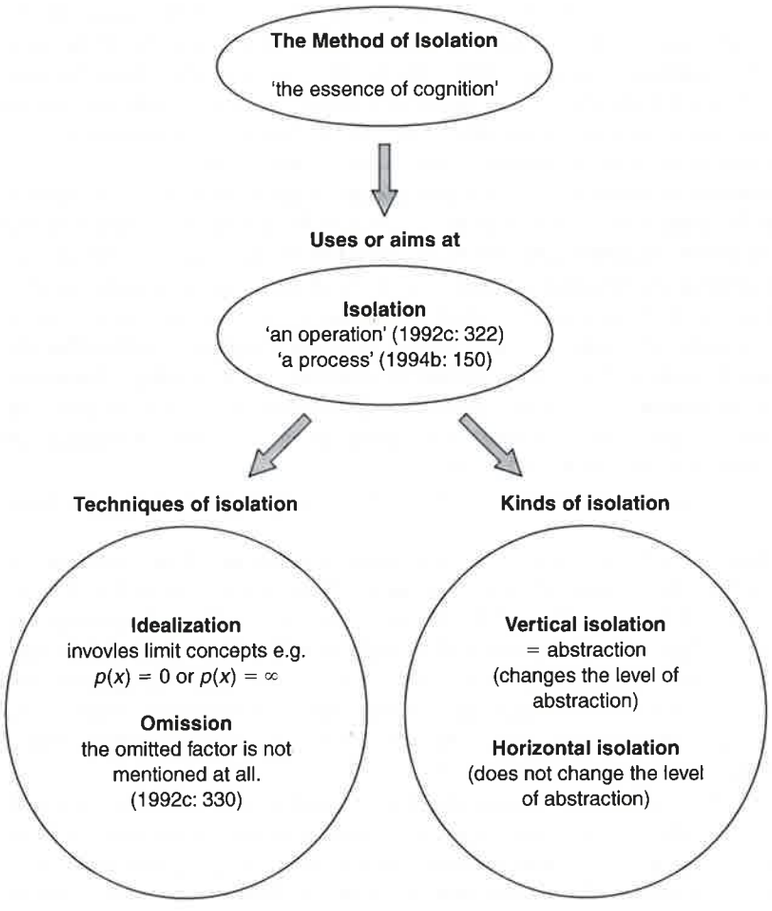

Figure I.1 Mäki's account of the method of isolation.

terms of idealization because no limit concepts are used, but which does not change the level of abstraction either. Such cases belong to the important category of *horizontal isolation*. Mäki's example (1992c: 322) involves moving from a case where the demand q_1 for a good x_1 depends on the prices $(p_2, \ldots, p_n)$ of the complements and substitutes of x_1:

$$q_1 = f(p_1, p_2, \ldots, p_n),$$

to a case in which the demand only depends on the price of the good itself:

$$q_1 = f(p_1).$$

Given that the prices of the substitutes and complements lie on the same level of abstraction as the price of the good itself, the example does indeed show that there are cases in which distinguishing between vertical and horizontal isolation is necessary. This simple example also illustrates how omissions are typically the means for achieving horizontal isolation: some items such as the prices of substitutes and complements are not even mentioned.

> An omission is a kind of implicit idealization in that the factor omitted is not mentioned at all in the presentation of the theory or model ... an omission is not a false representation even though it can be transformed into an idealization by introducing an explicit statement to the effect that the 'weight' of the element is nil.
>
> (Mäki 1992c: 330)

Although I do not remember Mäki ever characterizing the so-called negligibility assumptions (discussed in more detail in the next section) in terms of omissions, I think there is a close affinity between the two concepts.

At times he emphasizes that an idealization is unrealistic (Mäki 2004a: 1726) or false (Mäki 2003a: 500, 502), but he also presents an argument against the idea that idealizations are false by definition. Consider a typical idealization in economics:

> **[B] 'The government has a balanced budget' (i.e., the surplus and the deficit are zero).**

Although it is perhaps not particularly common, a country may have decided to keep the budget exactly balanced, or it may be balanced by accident. That is to say, [B] may be true even when it is not meant as a negligibility claim (Mäki 1994b: 155). One possible way of dealing with this issue is to distinguish between idealizations that may be true (but usually are not) and idealizations that cannot be true. This distinction corresponds to Vaihinger's distinction between *hypotheses* and *fictions*, which Mäki (1980b) noted early on.[31]

Robert Sugden (2000) leveled a major criticism at the method of isolation, arguing that modelers rarely start with a real-world target, and then start isolating what they consider important by way of idealization and omission. Modeling is rather predominantly a matter of *constructing* a representation of a system. If isolation is mainly considered a process or an operation, this would seem to be a devastating criticism – at least insofar as practicing economists really do not think about the process of building models in terms of isolation.

Mäki (2009b: 32) recently responded to Sugden's criticism by distinguishing between the *process* and the *product* of isolation. Talking about the product allows the use of the isolation framework to explain what is epistemically important while avoiding the claim that isolation must be a process that occurs in a

particular way. I believe, however, that part of its attraction lies precisely in the fact that at least some economists feel that it provides a correct account of what they are trying to do, and at times even of how they go about it. From this perspective, it might be inadvisable to deny that the process can ever be described in terms of an attempt to isolate.

Sugden's criticism affects Cartwright's (1989) framework more directly than Mäki's. The reason is twofold. First, Cartwright's presentation makes it perfectly clear that one starts with a concrete object of investigation: 'In idealisation we start with a concrete object and we mentally rearrange some of its inconvenient features – some of its specific properties – before we try to write a law for it. The paradigm is the frictionless plane' (ibid.: 187). In contrast, although Mäki's early account also presents isolation in terms of a *process* or an *operation*, he never explicitly requires the process to start from a concrete object. Second, Cartwright is committed to the idea that the process of subtracting factors and bringing them back in must be based on some well-defined principles (1989: 208; see also 2006, 2009). In contrast, as far as I can tell, even though Mäki discusses de-isolation[32] (i.e., the reverse of isolation, bringing explanatory factors back into the model or correcting a distorting idealization), he has never expressed any similar demands. He merely notes that 'the need for de-isolation ... is ontologically grounded' (Mäki 2001e: 383). It is not evident however, that Mäki's ambiguity on this issue is to be counted as an argument in his favor.

My impression of what he means by this is the following. Economists are typically perfectly aware of making unrealistic assumptions. The motivation for making them is that they are necessary for tractability reasons in the early stages of the development of an account that captures the workings of an important economic mechanism. However, economists typically know which assumptions are the most problematic in their models – if they are not evident, as is often the case, their colleagues are sure to point them out. Thus the need for de-isolation is ontologically grounded in the sense that economists often recognize what features a more realistic model would have, and they would build one if they only knew how (see also Mäki and Marchionni, forthcoming a).

As noted, Mäki argues that idealizing and simplifying assumptions are made in order to study the workings and effects of one mechanism in isolation. Theoretical disputes about some theory in a discipline are typically ignited by critiques of such theoretical isolations. De-isolation consists of supplementing items in the original set of explanatory variables with new ones (which thus amounts to extending the set), whereas re-isolation involves replacing or substituting an explanatory variable in the original set with another one. Jack Vromen (this volume) suggests that Mäki's notion of de-isolation neatly captures the dynamics in the dispute over the deficiencies of standard price theory in explaining the so-called crowding-out phenomenon. He also shows, however, that recent models advanced to explain these phenomena go beyond Mäki's framework of supplementing or substituting items of the original set of explanatory variables with new items. These models not only specify what new items are supplemented or substituted, they also point out how the items in the new set of explanatory variables can interact with each other to produce the crowding-out phenomenon.

Till Grüne-Yanoff (this volume) points out that Mäki has proposed three different notions of isolation that have different functions in his overall philosophical project, and that they are to be evaluated and criticized bearing these differences in mind. The early 1990s was characterized by 'essential isolation', meaning that isolation was supposed to apply only to some particular kinds of theories. When it became 'formal' this restriction was dropped, and in the latest work on models, it has become 'minimal' in the sense that the only relevant property is that the product of isolation is never itself idealized.

7 Truth, realisticness, and kinds of assumptions

The role of idealization became widely acknowledged in the philosophy of science during the 1980s. The prevalence of idealizations was initially taken to be a problem for scientific realism. After all, if idealizations are typically false, how can this be reconciled with the realist claim that science is able to provide a literally true story about its objects?

Several lines of realist responses have been pursued. First, weaker notions of truth such as truthlikeness as well as approximate and partial truth have been formulated. Second, it has been argued that although an idealized theory is not true, it becomes more so as false idealizations are removed (Laymon 1980, 1982). Third, it has been argued that various falsities are useful in tracking the truth (rather than in deriving predictions). Of these three approaches, Mäki has contributed to the first by formulating a particular notion of partial truth, and to the third by elaborating and extending Musgrave's (1981) framework, but he has explicitly and repeatedly argued against the second (Mäki 2009c, 2011a). His notion of partial truth essentially provides an account of how models that contain unrealistic assumptions may nevertheless contain truth if they depict the workings of an important mechanism, and the basic point of the framework that derives from Musgrave's work is that some unrealistic assumptions should not be taken as literal claims about the target being modeled. Rather, if they are reformulated in terms of their function in the model, they may be true. Hindriks (this issue) gives descriptive names to these two strategies by calling the former 'the significant-truth' strategy and the latter 'the truth-of-paraphrase' strategy. Mäki's contributions on kinds of assumptions and their truth are discussed in this section, and his strategy of significant truth in the next.

Musgrave (1981) argues that economics uses various different kinds of false assumptions. *Negligibility* assumptions posit that some causal forces or entities or relationships can be ignored when analyzing certain research questions because they are sufficiently irrelevant to them. *Domain* assumptions specify the applicability of a given assumption or model. According to Musgrave, one could interpret the same description of an assumption first as a negligibility assumption and then as a domain assumption. In some cases, however, the economist knows that a factor is not negligible, and does not wish to argue that assuming

its absence defines the scope of the theory. In such cases he or she may first assume something in order to be able to derive consequences from the model, and then later modify the assumption in order to make the model more realistic. Musgrave called such assumptions *heuristic*, presumably because they are dispensable and their role is to assist in formulating the model, and argued that all three kinds of assumptions ought to be true.

Musgrave was not particularly clear about heuristic assumptions. Mäki (1994d, 2000a) thus provided a modification by paying particular attention to their temporality: *early-step* assumptions are first made and subsequently relaxed. Hindriks (2005, 2006, 2008) modified heuristic assumptions in a different way by focusing on their role in making models mathematically *tractable*. Mäki also argued that, whereas negligibility and domain assumptions ought indeed to be true, this is not the case with early-step assumptions. The same could surely be said about tractability assumptions. Mäki and Hindriks tend to present their terminology and analysis as a clarification of and improvement on Musgrave's heuristic assumptions, but they do not seem to agree on which account best illuminates them.

These views could be reconciled by acknowledging that heuristic, early-step and tractability assumptions are all different. Although Musgrave does not refer to Fritz Machlup's (1955, 1956) notion of *heuristic postulates* when he introduces heuristic assumptions, he does refer to concepts such as *indirect confirmation*, which are of significance in Machlup's account. The primary examples of heuristic postulates include profit and utility maximization, which Melitz (1965) called 'generative assumptions'. They are useful tools for generating significant results, but according to Machlup and Friedman they need not be justified with evidence that bears on them directly. They are dispensable in the sense that they are used as long as they are found to be useful. Heuristic postulates have been relaxed in mainstream economics only in the last few decades. They could thus be interpreted as early-step assumptions only in the very long term. They do have a tractability aspect, but they are clearly very different from tractability assumptions that are highly specific, such as that production functions are of the Cobb–Douglas or of the CES form. If Musgrave's account is taken to provide an explication of heuristic postulates, it is possible to make sense of his claim that heuristic assumptions also ought to be true while still subscribing to the view that early-step and tractability assumptions need not be.

Mäki also clarified what Musgrave was trying to say by introducing the notion of *paraphrasing* (Mäki 2000a, 2004b, 2009f, 2011d). Suppose, for example, that we are trying to work out the tax incidence of a raise of three percentage points on the VAT on groceries. Suppose, further, that our economic model employs assumption [B] (recall that it states that the government has a balanced budget), among others, to study this question. Even though this assumption may well be false, we could somewhat safely assume that [B]'s divergence from the truth is negligible in a model that studies tax incidence. Thus, the truth of [B] does not matter, but the truth of the paraphrased version [NB] does (Mäki 2000a: 330; see also 2009f: 103; 2011d):

[NB] A given budget imbalance has negligible effects on the tax incidence.

Unfortunately, the term 'paraphrase' was not particularly well chosen because the idea is not to reformulate the assumption or to express it in other words, but rather to point out its truth-related function in a model. In other words, an assumption and its paraphrase make two different claims. The former says something false about the budget balance whereas the latter says something true about its negligibility. There is a fact of the matter concerning whether NB is true, and it may well be true. To put it differently, budget imbalance could be negligible or non-negligible in a study of tax incidence. Furthermore, it is clear that this assumption ought to be true.

It is of some interest that Musgrave (1981) once used the term 'paraphrase', but he did so only with respect to so-called 'as-if' assumptions. His reason, I suspect, was that using an 'as-if' formulation allowed for *reformulating* some assumptions of the model in such a way that their intended truth claim became evident. Consider the following pair of assumption statements:

[M] Economic agents consciously maximize utility.

[MAI] Economic agents behave as if they consciously maximized utility.

Whereas [M] is usually false, [MAI] may be true, particularly if there is some more general theory, such as evolutionary selection, explaining why agents' behavior might *in fact* be similar to that which would ensue if they maximized utility, even though they do not consciously do so. Mäki's contribution with respect to as-if assumptions is to point out that when they are used as part of the method of isolation, they can be interpreted as being consistent with scientific realism, contrary to what is commonly believed (1992a, 1998b, 2000a, 2003a, 2004a, 2009f). He provides the following example (1992a, 2003a, 2009f) of the function of such usage:

> [A] Phenomena behave as if certain ideal conditions were met: conditions under which only those real forces that are theoretically isolated are active.

Here the as-if assumption refers to the idea that various disturbing factors that are found in the real world are assumed to be absent. The central forces identified in the model are depicted as if they were the only forces. As Ross (this volume) notes, the method of isolation is not intrinsically related to realism however, and Mäki's argument is only that the as-if expression is consistent with realism, not that there is something intrinsically realist about formulating negligibility assumptions.

Negligibility assumptions are clearly the most useful kind of paraphrase, and the basic point of the paraphrasing strategy is particularly sound with respect to them. Nevertheless, the strategy can also be used for analyzing domain, heuristic, early-step, and tractability assumptions. This raises the problem of determining the right way to paraphrase any given assumption in some model. It is a problem that is far from trivial because negligibility assumptions are crucial for isolating important truths in models, although the 'truth' of early-step and tractability assumptions has very little to do with the truth of the model. Mäki recently suggested calling the former 'paraphrases' and the latter 'meta-claims' (Mäki 2011d).[33]

Mäki proposed in a recent discussion on models and modeling that the notion of *commentary* takes care of specifying which assumptions are to be considered for their truth. Roughly, the idea is that the commentary nominates some parts of the model as candidates by aligning the items in the description in the right way, and it does so by taking into account the ontology of the model objects and the pragmatic goals of the modeler. This does not tell us how exactly to employ the paraphrasing strategy in concrete cases, but at least it tells us what kind of resources we could be using.

Musgrave's contribution, Mäki's subsequent papers (Mäki and Piimies 1998; Mäki 2004b, 2009f, 2011d) and Hindriks' (2005, 2006) are based on a realist philosophy of science in two respects. First, given that instrumentalists are supposedly only interested in the pragmatic usefulness of assumptions, they may be interested in classifying them according to the different ways in which they help in deriving predictions, but not in classifying them into kinds for which we have different truth demands. The second consideration is related to the first: Musgrave's argument and Mäki's distinction between paraphrases and meta-claims do not make any sense unless the idea that some particular kinds of assumptions ought to be realistic is accepted. A hard-headed instrumentalist might simply dismiss the point of the paraphrasing strategy because he or she simply does not care whether or not the assumptions are realistic. Thus the very point that some kinds of assumptions and thereby some assumptions ought to be realistic is fundamentally realist. Although Mäki would undoubtedly consider the Musgrave–Mäki–Hindriks typology a realist line of research, I believe he obfuscates this fact by giving the impression that realisticness and realism have next to nothing to do with each other:

> The abstract construal of the issue is often accompanied by the idea that the dividing line between people holding rival views concerning the desirability of realistic or 'more' realistic assumptions distinguishes those holding a realist position from those who are non-realists (instrumentalists, conventionalists, etc.). Accordingly, the idea goes, realists prefer realistic assumptions to unrealistic assumptions, while non-realists are either indifferent or have their preferences the other way round ... there is a need for reorientation. ... The issue should not be construed as one of realistic versus unrealistic assumptions in the abstract but rather over which specific assumptions are and should be unrealistic or realistic, and over rival ways in which they are or should be so. ... The advocacy of more or less realistic assumptions per se does not yet make anybody a realist or non-realist about economic theories.
>
> (Mäki 1994b: 239)

In itself, this admonition to redirect the discussion on unrealistic assumptions is useful, at least insofar as there are still scholars who think, rather simplistically, that the relevant question is whether or not any kind of unrealistic assumptions are acceptable.

Frank Hindriks (this volume) discusses two strategies for defending realism in economics. He calls them the 'truth-of-paraphrase strategy' and the 'significant-truth strategy.' Both allow for non-negligible falsehoods – a concession that compromises the realist ideal of true theories. Hindriks argues that these strategies can be replaced by two other strategies that come closer to the realist ideal in that they do not require us to make such a compromise. They are referred to here as 'the future-truth strategy' and 'the truth-of-the-counterfactual strategy', and are illustrated using examples from both physics and economics. In the latter case they help us to see that there may be more truth in economics than meets the eye.

8 Models and truth

Mäki's primary motivation for developing an account of models and modeling is to show that there are ways in which models can be taken to be true even though they contain various false assumptions and even when their predictions are false. He is intent on developing an alternative to

> 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!
>
> (Mäki 2009e)

Given that Mäki is not willing to claim that models are true in their entirety, and because 'the truth of a model is not reducible to the truth of its assumptions nor to the truth of its predictions' (2006: 14), Mäki ends up proposing that some privileged parts of models may be true. This is why he has developed an account he calls 'the functional decomposition approach'. The general idea is that models contain various different parts that have different functions, and in particular, some elements are to be evaluated for their truth whereas others are not.

Mäki calls his account of models MISS: Models as Isolations and Surrogate Systems. He views models as *pragmatically* and *ontologically constrained representations* that *isolate* the workings of some important causal *mechanism*, thereby linking the discussion on models to that of representation. He distinguishes between the *representative* and the *resemblance* aspects of representation (Mäki

2001c: 9936). The former refers to the idea that models stand for their target systems as representatives, and the latter to the idea that in order for a model to serve as a good representative, it must resemble its target system in relevant respects and to sufficient degrees (Mäki 2006: 9). He provides the following characterization (Mäki 2009b: 32; 2009e: 75; 2009c, 2011a, 2011d):

> Agent A uses object M (the model) as
> a representative of some target system R
> for purpose P,
> addressing audience E,
> at least potentially prompting genuine issues of resemblance between M and R to arise,
> describing M and drawing inferences about M and R in terms of one or more model descriptions D,
> and applies commentary C to identify and align these components.

This clearly seems to be an account of modeling rather than of models. Mäki does not explicitly define what models are, but he seems to accept the part of Giere's account positing that they are abstract objects.[34] The two aspects of representation are embedded in this pragmatic account. He explains what 'prompting genuine issues of resemblance between M and R to arise' means as follows.

> By requiring that the issue be 'genuine' I mean to put forth two ideas. First, genuine issues are about non-utopian resemblances: *M* or its modifications should have the capacity to resemble *R* so that successful resemblance does not appear as an unattainable utopian goal, but should instead lie within the horizon of our cognitive possibilities. Second, genuine issues are not about just any of the numerous arbitrary ways in which *M* and *R* do (or do not) and might (or might not) resemble one another, but rather about specific respects and degrees of resemblance that meet the pragmatic constraints.
>
> (Mäki 2009c: 180)

It seems to me that the seven points above are to be considered necessary conditions for genuine surrogate modeling. This interpretation derives from the fact that if some crucial item from the list is missing from a modeling practice, according to Mäki's account there is no model or no modeling going on. For example, if there is no intended target, there is no model. This, I take it, is part of what it means by saying that models are ontologically constrained. Similarly, if no genuine issues of resemblance arise, again there is no model. This, I take it, is what it means to be pragmatically constrained. Thus, despite its generality in other respects, Mäki's account of modeling rules out a large number of practices that some other scholars would be willing to call 'modeling.' In particular,

merely manipulating models by examining model descriptions does not yet count as modeling if there is no attempt at representation. In such cases, according to Mäki, models are treated as *substitutes* (for reality), whereas genuine representative modeling treats them as *surrogates* (for reality).

There appears to be a tension in the methodological thinking of econometricians: they are pulled in the directions of both constructivism and realism. Kevin Hoover (this volume) suggests that Ronald Giere's (2006) perspectival realism provides a starting point for such a charitable interpretation and a resolution of this tension, which is ultimately a form of pragmatism. He draws on the original pragmatism of Charles S. Peirce, which supports an account of realism that both enriches Giere's account and suits the metaphysical attitude of econometrics.

It is instructive to compare Mäki's account to Hausman's Giere-inspired account of models (Giere 1988; Hausman 1992). Mäki accepts a central tenet of Giere's account, namely that models are to be distinguished from model descriptions. Given that non-propositional abstract objects and predicates are not usually considered to be truth-apt, Giere and Hausman adopt the notion of a *theoretical hypothesis* in order to provide an element that carries truth values. A theoretical hypothesis is a truth-valued statement about the relationship of similarity between a model and a real system. Mäki (2004b: 27) doubts that using theoretical hypotheses helps in the task he sets for himself. The problem is that in economics at least, theoretical hypotheses such as 'the Dutch economy is a general equilibrium system' regularly come out as flatly false. In this sense, 'it does not seem to matter whether one directly views models as truth-valued or whether one takes theoretical hypotheses to be the relevant truth bearers'. Mäki argues against Hausman's account of models as definitions of predicates on the grounds that it does not allow the models to be true any more than Giere's account does. This example is problematic for two reasons.[35] First, the general equilibrium system is quite an unusual model in economics, and certainly not one that is meant to be directly applicable in this way. Second, theoretical hypotheses do not relate whole models to whole economies, they rather relate aspects of models to aspects of economies. It is thus possible that if more appropriate examples of theoretical hypotheses are to be found, they might be true after all.

In Giere's account the model descriptions define the model, and the model is what makes the descriptions true. This truth relation is uninteresting, however, because the model is true by definition. Furthermore, given that the model is comparable to a predicate, it is not the kind of entity that can have truth values (Giere 2006: 64–65). It makes no sense to say, for example, that 'Red is true.' Giere and Hausman thus need to introduce another element, namely the theoretical hypothesis, into the model in order to relate models to reality and truth. Given that Mäki rejects theoretical hypotheses, however, he needs to build the truth content into his account in some other way.

He also argues that one should not take model descriptions at face value. Although they are typically false with regard to the intended target, this does not matter because they do not provide the relevant truth claims: the paraphrased

assumptions do so instead. The notion of *commentary* is thus absolutely crucial to Mäki's account because it specifies which parts of the model are to be assessed for truth by describing how to paraphrase the various assumptions. The pragmatic aspect is now easier to see: even a given model description such as [B] may be assigned different roles in determining the truth of the model in different contexts.

The notion of a theoretical hypothesis overlaps the notion of commentary in some respects: 'A commentary is needed because no model is itself able to specify how it relates or is supposed to relate (or fails to relate) to its (or any) target or targets' (Mäki 2011d: 218). The commentary also 'helps determine the respects in which resemblance between the model and the target is be sought' (Mäki 2011a). If 'similarity' is substituted for 'resemblance', are not these two things exactly what theoretical hypotheses are supposed to do? The difference between a commentary and a theoretical hypothesis seems to be just that the latter is *in itself* supposed to be truth-valued but Mäki does not specify whether or not a commentary is. Commentaries help in specifying which similarity features are important, whereas theoretical hypotheses are contentful claims about similarity.

It seems to me that the notion of commentary is not yet fully developed. It includes various different features that one might prefer to keep separate. However, Mäki is using it in an attempt to capture something rather important about modeling practice. Economists often need to justify their assumptions, and their justifications are not usually mere instrumental appeals to their usefulness. Mäki's notion of commentary is a first stab at making sense of these practices.[36]

What, then, is truth-valued in Mäki's account? It is the thought expressed in the model concerning the central mechanism that is isolated by the model (Mäki 2008e, 2011a). In the example of von Thünen's model, the mechanism is that which shows how distance from the center explains the concentric pattern of land use through transportation costs and land rent. There is 'truth in the model' if there is a mechanism in the real world that resembles what is depicted in the model, and this is also 'the truth of the model' (Mäki 2011a). If the model depicts the functioning of such a mechanism correctly, Mäki is willing to say that there is truth in the model and that this is the truth of the model, and even that the 'model is true' (2009c). He will not say, however, that the 'model is true', *tout court*, because its assumptions are false, and in the case of his favorite example, von Thünen's model of the isolated state, the most important prediction that economic activity is spatially aligned in concentric rings around cities is also false. Mäki's account thus facilitates discussion about the truth *in* and *of* models but it does not change the fact that, strictly speaking, all models are false in the sense that they contain false assumptions.

Note that it is possible for a mechanism to be described by means of a number of different assumptions, none of which is literally and exactly true. Thus, even in the midst of unrealistic assumptions, Mäki has been able to find a glimmer of truth. There is no doubt that his account of models captures something that

economists consider valuable in their own models. This is a significant achievement even though other philosophers might prefer not to use the notion of truth to express the idea. In getting to this result, Mäki rejected the idea that truth bearers must be linguistic, for example (2001c: 9936). Note also that, just as he wished, his notion of partial truth is entirely static in that von Thünen's original model is just as true as later models studying the same mechanism. Indeed, he does not even mention increasing truthlikeness in his discussion of scientific progress (Mäki 2002c).

Daniel Hausman (this volume) argues that Mäki's account of Models as Isolations and Surrogate Systems (MISS) explains the enormous diversity of models, while at the same time offering a specific analysis of the notion. In this chapter, Mäki's account is compared to the author's preferred account of theoretical economic models as theoretical predicates, the implications of these differences for what we learn from models and how we learn them are discussed.

Ilkka Niiniluoto (this volume) argues that the concept of truthlikeness or verisimilitude is a useful tool in the defense of a realist position about theories and models in science. The difference between analogical models and idealized models is emphasized: the former are surrogates and simulations that allow direct analogical inferences to a real-target system from the model, whereas the latter include counterfactual assumptions and lead to realistic conclusions only through concretization. These considerations help in assessing Uskali Mäki's MISS account of economic models and Robert Sugden's account of models as credible fictional worlds. Niiniluoto gives support to Mäki's realism-inspired thesis that models must represent some actual target systems.

9 Guidance for the reader

This book is divided into four parts. The ordering of the themes presented in this introduction roughly follows the historical development of Mäki's realism, whereas the essays that follow are placed in thematic groups. Part I (Isolating truth in economic models) deals with notions of truth (Hindriks, Niiniluoto), models (Hausman, Grüne-Yanoff), and the method of isolation (Grüne-Yanoff, Vromen). Part II is devoted to the notion of commonsensibles (Guala, Hands), and Part III (The proper domain of economics) to the scope of economics (Ross, Davis). Finally, the focus in Part IV (Rethinking realism(s)) is on alternative meta-frameworks to Mäki's scientific realism (Hoover, Zamora Bonilla, Kuorikoski and Ylikoski).

The editors have not solicited responses to the essays from Mäki. The primary reason for this is that we are quite sure that if we succeed in keeping the project of writing this book hidden from him until its inauguration after the International Network for Economic Methodology (INEM) conference on 4 September 2011 in Helsinki, finding out that we have put together a book that concentrates on his account of economics will give him a pleasant surprise. Furthermore, knowing Uskali's way of doing things, we are convinced that he will respond in one way or another on a later occasion.

Notes

1 I am grateful to Till Grüne-Yanoff, Francesco Guala, Frank Hindriks, Clemens Hirsch, Jaakko Kuorikoski, Caterina Marchionni, Ilkka Niiniluoto, Samuli Pöyhönen, and Petri Ylikoski for their insightful comments on various versions of this paper. The usual disclaimer applies.
2 Mäki discusses Lakatos in (2008b), and Popper and Lakatos in (Mäki 2008a).
3 Mäki first made this distinction in 1989. Its centrality is evident from the number of contributions in which he discusses or mentions it (Mäki 1989: 176; 1990a: 291; 1990b: 80; 1992b: 38; 1994b: 154; 1996a: 429–430; 1998c: 304; 1998e: 6, 19; 1998f: 266; 1998g, 1998h, 2000a: 319; 2000c: 110–111; 2002d: 90; 2011c: 2).
4 E.g. Boylan and O'Gorman (1995: 112–129), Deichsel (2011), Hands (2001: 328–333), Hodge (2008) and Peter (2001). Mäki has never compared his account to Lawson's in writing, but Mäki and Oinas (2004) provide a discussion that could be considered relevant for such purposes and Mäki (2011c: 2–3; 2011d) briefly discusses Bhaskarian realism.
5 Of these topics, Mäki's writings on economics of economics (1999b, 2005a) will not be discussed at all in this Introduction, and his writing on rhetoric as well as sociology of knowledge only very cursorily.
6 In Mäki (1991a: 85) he avows that he has not only realist but also essentialist intuitions.
7 The choice of the Finnish language in the early stages of his career seems to have been more than a matter of convenience. Given his current international orientation, I was somewhat surprised to find that he had written a rather extensive historical overview of Finnish economic methodology (Mäki 1983b).
8 All translations from Finnish in this paper are my own.
9 See also Hausman and Mäki (1998).
10 Hausman (1997, 1998, 2000); see also Hands (2001) and Hausman (2009).
11 He discusses another generic version (Mäki 1996a), but only to show that economics does not fit with *that* version of realism.
12 He first presented most of these distinctions in his dissertation (Mäki 1990e: 16–37) and in Mäki (1989).
13 Mäki (1996a) shows that some standard arguments (technological manipulation and the non-miracle) for realism cannot be reasonably applied in economics. He does not seem to think, however, that this should be taken as a criticism of realism because there are other ways of establishing the conclusions of these arguments.
14 In Mäki (1988c: 80) he claims that according to scientific realism 'those entities that have been assumed to exist in scientific theories do in fact exist'. This is tantamount to giving scientific realism a referential gloss.
15 He continues to hold similar but weaker views: '... in modifying commonsensibles by various simplifications and idealizations the theorist does not thereby introduce entirely new kinds of entities and properties' (Mäki 2011d).
16 He avowed at the beginning of the 1990s that he had come to doubt essentialism on the basis of discussions with Daniel Hausman. On the other hand, he has never explicitly rejected essentialist ideas in his writings, and he even said recently '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' (Mäki 2009e). Furthermore, he has thus far not endorsed any particular theory of explanation other than the redescription account.
17 He has recently analyzed Machlup's argument (Mäki and Sappinen 2011) and briefly in Mäki (2011d). Given that the former article is an overview of the notion of homo economicus, he does not present any criticisms. In the latter he re-asserts his argument on reference originally presented in Mäki (1999a).

18 Mäki uses this term for what is perhaps better known as the 'disquotational theory of truth', according to which asserting that a statement is true is completely equivalent to asserting the statement itself. Thus, the notion of truth is redundant.
19 He explicitly says that 'constructivism [rather than instrumentalism or empiricism] in various guises now constitutes the major challenge to realism' (Mäki 2000c: 113). This view also shows in his emphasis. He discusses constructive empiricism briefly (Mäki 1989, 1992a, 2004b), whereas other papers focus on rhetoric or social constructivism (Mäki 1986, 1988b, 1988a, 1992d, 1993a, 1993b, 1994c, 1995, 2000b, 2003b, 2009d, 2010).
20 Mäki also expresses similar views (2005b: 241–242; 2001d: 12818–12820;1998g: 407).
21 Recently Mäki wrote: 'it is enough [for minimal realism] if a theory has a chance of being true, and that it is true or false in virtue of how the world works. I take anti-realism to deny this and to claim that theories have no chance of being true in this sense: either no talk about truth makes sense or truth should be conceived in terms other than how theories relate to the world (such as usefulness, coherence, or consensus)' (2011b: 43). Mäki thus does not seem to count constructive empiricists as anti-realists at all.
22 As he said in the interview 'My scientific realist philosophy of economics entertains epistemic ambition and optimism' (2009e: 94). On the other hand, in addition to his denials of epistemic optimism referred to in note 21, he is very explicit about not using the 'no miracles' argument as a justification for realism in economics (Mäki 2002d, 2005b).
23 This extract is taken from an English version of the paper that was published in Spanish. I am grateful to Luis Manuel Valdés-Villanueva for providing this document.
24 In contrast, Boylan and O'Gorman argue thus: 'Realist generative non-empirical mechanisms play no epistemic role in causal holism. Rather, an indispensable epistemic aim of a scientific theory is the construction of models which furnish accurate descriptions of the real observable causes' (1995: 7), and that 'the domain of empirical evidence, which is all we have at our human disposal, is confined to the observable and no amount of knowledge about this domain can legitimate any inference about the characteristics of the unobservable in principle' (1995: 63).
25 It is clear that his ontology includes unobservable entities: 'A radical empiricist would not accept essences into the ontology of economics, since they are not available to sense experience' (Mäki 1998a: 592). A paper written jointly with Caterina Marchionni also includes the claim that mechanisms are metaphysical concepts (Mäki and Marchionni 2009: 190).
26 Mäki discusses unification in the following papers: (Mäki 1990d, 1992a, 1996b, 1998i, 2001b, 2002b, 2002c, 2003a, 2004c, 2009a; Mäki and Marchionni 2009; see also Mäki 1997a).
27 His actual claims are weaker, however. In Mäki and Marchionni (2009), for example, the authors claim that ontological unification is *pursued* but not necessarily achieved in geographical economics.
28 The distinction between the *whole truth* and *nothing but the truth* is from Sen (1980).
29 He also presents several other distinctions within the kinds category (internal–external, material–theoretical etc.). To save space, they are not discussed here.
30 Mäki seems willing to make some distinction between the two but I am not sure what it is exactly: 'Abstraction may be thought of as an operation in 'vertical isolation' whereby universals are isolated from the particular features of the objects that instantiate them' (Mäki 1997b: 487, fn. 11). In Mäki (1998e: 15) he distinguishes between the *level* and the *process* of abstraction, but it remains unclear how this distinction is to affect the overall structure of the isolation account.
31 As Niiniluoto (this volume) notes, Mäki (1994b: 153) also uses the term 'quasi-idealization' for the former.

32 See Mäki (1994b, 1998e, 1998f, 2004c, 2011a). Although some other writers use the term 'concretization' (see Niiniluoto in this volume) for something similar to de-isolation, Mäki has told me in private discussion that he thinks this is a misleading term. Indeed, even though the reverse process of abstraction does make a model more concrete, if de-isolation occurs via de-idealizing, it is not clear that the resulting model is more concrete than the original one.

33 Hindriks (2006) introduced the term 'meta-claim' but his use of this notion is slightly different from that of Mäki.

34 He refers to the model as 'an imagined object', an 'imagined model world' (Mäki 2009c), an 'imagined abstract object' (Mäki 2009b), and 'an imagined system' (Mäki, forthcoming). Although imagined objects and systems are not equivalent to abstract objects, given that he does not explicitly criticize this aspect of Giere, my interpretation is that he accepts it.

35 I am grateful to Francesco Guala for sharing these observations with me.

36 Another closely related issue is the stories that economists tell when they present their models (Grüne-Yanoff and Schweinzer 2008; Morgan 2001).

References

Boland, L.A. (1979) 'A Critique of Friedman's Critics', *Journal of Economic Literature*, 17: 503–522.

Boyd, R.N. (1983) 'On the Current Status of the Issue of Scientific Realism', *Erkenntnis*, 19: 45–90.

Boyd, R.N. (1990) 'Realism, Approximate Truth, and Philosophical Method', *Minnesota Studies in the Philosophy of Science*, 14: 355–391.

Boylan, T.A. and O'Gorman, P.F. (1995) *Beyond Rhetoric and Realism in Economics: Towards a Reformulation of Economic Methodology*, London: Routledge.

Cartwright, N. (1989) *Nature's Capacities and their Measurement*, Oxford: Oxford University Press.

Cartwright, N. (2006) 'The Vanity of Rigour in Economics: Theoretical Models and Galilean Experiments', in P. Fontaine and R.J. Leonard (eds). *The 'Experiment' in the History of Economics*, London: Routledge, pp. 135–153.

Cartwright, N. (2009) 'If no Capacities then no Credible Worlds. But can Models Reveal Capacities?' *Erkenntnis*, 70: 45–58.

Cruickshank, Justin (2003) (ed.). *Critical Realism; the Difference that it Makes.* London and New York: Routledge.

Deichsel, S. (2011) 'Against the Pragmatic Justification for Realism in Economic Methodology', *Erasmus Journal for Philosophy and Economics*, 4: 23–41.

Fleetwood, Steve (1999) (ed.). *Critical Realism in Economics: Development and Debate*, London: Routledge.

Friedman, M. (1953) 'The Methodology of Positive Economics', *Essays in Positive Economics*, Chicago: University of Chicago Press, pp. 3–43.

Fullbrook, Edward (2009) (ed.). *Ontology and Economics: Tony Lawson and His Critics*, London and New York: Routledge.

Giere, R.N. (1988) *Explaining Science: A Cognitive Approach*, Chicago: University of Chicago Press.

Giere, R.N. (2006) *Scientific Perspectivism*, Chicago: University of Chicago Press.

Grüne-Yanoff, T. and Schweinzer, P. (2008) 'The Role of Stories in Applying Game Theory', *Journal of Economic Methodology*, 15: 131–146.

Hands, D.W. (2001) *Reflection without Rules: Economic Methodology and Contemporary Science Theory*, Cambridge, New York and Melbourne: Cambridge University Press.

Hausman, D.M. (1992) *The Inexact and Separate Science of Economics*, Cambridge and New York: Cambridge University Press.

Hausman, D.M. (1994) 'Why Look Under the Hood?' in D.M. Hausman (ed.). *Philosophy of Economics: An Anthology*, Cambridge: Cambridge University Press, pp. 17–21.

Hausman, D.M. (1997) 'Theory Appraisal in Neoclassical Economics', *Journal of Economic Methodology*, 4: 289–296.

Hausman, D.M. (1998) 'Problems with Realism in Economics', *Economics and Philosophy*, 14: 185–213.

Hausman, D.M. (2000) 'Realist Philosophy and Methodology of Economics: What is it?' *Journal of Economic Methodology*, 7: 127–133.

Hausman, D.M. (2009) 'Laws, Causation, and Economic Methodology', in H. Kincaid and D. Ross (eds). *Oxford Handbook of the Philosophy of Economics*, Oxford: Oxford University Press, pp. 35–54.

Hausman, D.M. and Mäki, U. (1998) 'Introduction', in R.E. Backhouse, D.M. Hausman, U. Mäki, and A. Salanti (eds). *Economics and Methodology: Crossing Boundaries*, London: Macmillan Press, pp. xv–xxi.

Hindriks, F.A. (2005) 'Unobservability, Tractability and the Battle of Assumptions', *Journal of Economic Methodology*, 12: 383–406.

Hindriks, F.A. (2006) 'Tractability Assumptions and the Musgrave–Mäki Typology', *Journal of Economic Methodology*, 13: 401–423.

Hindriks, F.A. (2008) 'False Models as Explanatory Engines', *Philosophy of the Social Sciences*, 38: 334–360.

Hodge, D. (2008) 'Economics, Realism and Reality: A Comparison of Mäki and Lawson', *Cambridge Journal of Economics*, 32: 163–202.

Hoover, K.D. (1995) 'Is Macroeconomics for Real?' *Monist*, 78: 235–258.

Jones, M.R. (2005) 'Idealization and Abstraction: A Framework', *Poznan Studies in the Philosophy of the Sciences and the Humanities*, 86: 173–218.

Kuorikoski, J. and Lehtinen, A. (2010) 'Economics Imperialism and Solution Concepts in Political Science', *Philosophy of the Social Sciences*, 40: 347–374.

Lagueux, M. (1994) 'Friedman's 'Instrumentalism' and Constructive Empiricism in Economics', *Theory and Decision*, 37: 147–174.

Lagueux, M. (2010) *Rationality and Explanation in Economics*, London and New York: Routledge.

Lawson, T. (1997) *Economics and Reality*, London: Routledge.

Lawson, T. (2003) *Reorienting Economics*, London: Routledge.

Laymon, R. (1980) 'Idealization, Explanation, and Confirmation', *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, 1: 336–350.

Laymon, R. (1982) 'Scientific Realism and the Hierarchical Counterfactual Path from Data to Theory', *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, 1: 107–121.

Lewis, Paul A. (2004) (ed.). *Transforming Economics: Perspectives on the Critical Realist Project*, London and New York: Routledge.

Machlup, F. (1955) 'The Problem of Verification in Economics', *Southern Economic Journal*, 22: 1–21.

Machlup, F. (1956) 'Rejoinder to a Reluctant Ultra-Empiricist', *Southern Economic Journal*, 22: 483–493.

Mäki, U. (1980a) 'Methodology of Economics and Other Social Sciences' [Book Review of Machlup 1978, in Finnish], *Kansantaloudellinen aikakauskirja*, pp. 331–335.
Mäki, U. (1980b) 'Vaihinger on Fictions in Science', in I. Patoluoto, M. Sintonen, and L. Taiminen (eds). *Semi-Ramistic Studies*, Helsinki: Reports from the department of philosophy, University of Helsinki, pp. 32–37.
Mäki, U. (1981–1982) 'Positiivisen taloustieteen metodologia modernin tieteenfilosofian valossa' [the Methodology of Positive Economics in the Light of Modern Philosophy of Science, in Finnish], *Taloustieteellisen seuran vuosikirja*, pp. 157–182.
Mäki, U. (1982) 'Metodologisen kirjallisuuden uusin aalto' [the most Recent Wave in Methodological Literature, in Finnish], *Kansantaloudellinen aikakauskirja*, 4: 393–403.
Mäki, U. (1983a) 'How does Scientific Realism Manage as a Metatheory of Management Research?' *Methodology of Management and Business Research*, Helsinki: Helsinki School of Economics, pp. 257–264.
Mäki, U. (1983b) 'Metodologisen ajattelun juonteita suomalaisessa kansantaloustieteessä' [Streams of Methodological Thinking in Finnish Economics, in Finnish], in H. Koskenkylä, J. Pekkarinen, A. Räikkönen, and P. Vartia (eds). *Sata Vuotta Suomalaista Kansantaloustiedettä*, Porvoo: Kansantaloudellinen yhdistys, pp. 109–140.
Mäki, U. (1983–1984) 'Keskustelu Milton Friedmanin metodologiasta: opetuksia ja ongelmia' [the Discussion on Milton Friedman's Methodology: Results and Problems, in Finnish], *Taloustieteellisen seuran vuosikirja*, pp. 237–251.
Mäki, U. (1984) 'Tieteellinen realismi ja marxismi' [Scientific Realism and Marxism, in Finnish], in Ilkka Niiniluoto and Esa Saarinen (eds). *Vuosisatamme Filosofia [the Philosophy of our Century]*, Porvoo: Werner Söderström, pp. 78–116.
Mäki, U. (1985) 'Issues in Redescribing Business Firms', *Problems in the Redescription of Business Enterprises*, Helsinki: Helsinki School of Economics, pp. 121–129.
Mäki, U. (1986) 'Rhetoric at the Expense of Coherence: A Reinterpretation of Milton Friedman's Methodology', *Research in the History of Economic Thought and Methodology*, 4: 127–143.
Mäki, U. (1987a) 'Afterword', *Research in the History of Economic Thought and Methodology*, 5: 237–239.
Mäki, U. (1987b) 'Explanation as Redescription', in H. Holz and J. Manninen (eds). *Vom Werden Des Wissens*, Cologne: Pahl-Rugenstein, pp. 109–114.
Mäki, U. (1988a) 'How to Combine Rhetoric and Realism in the Methodology of Economics', *Economics and Philosophy*, 4: 89–109.
Mäki, U. (1988b) 'Realism, Economics, and Rhetoric', *Economics and Philosophy*, 4: 167–169.
Mäki, U. (1988c) 'Taloustieteen metodologia' [the Methodology of Economics, in Finnish], in M. Virtanen, I. Rahunen, and P. Pajunen (eds). *Tutkimus Kehityksen Tukena*, Helsinki: Helsinki School of Economics, pp. 60–81.
Mäki, U. (1989) 'On the Problem of Realism in Economics', *Ricerche Economiche*, 43: 176–198.
Mäki, U. (1990a) 'Mengerian Economics in Realist Perspective', *History of Political Economy, Annual supplement*, 22: 289–310.
Mäki, U. (1990b) 'Methodology of Economics: Complaints and Guidelines', *Finnish Economic Papers*, 3: 77–84.
Mäki, U. (1990c) 'Realistius ja legitiimiys: näkökulma erään tieteenalan krooniseen kiistakysymykseen' [Realisticness and Legitimacy: A Perspective on a Chronic Issue about Economics, in Finnish], *Kansantaloudellinen aikakauskirja*, pp. 452–457.

Mäki, U. (1990d) 'Scientific Realism and Austrian Explanation', *Review of Political Economy*, 2: 310–344.
Mäki, U. (1990e) *Studies in Realism and Explanation in Economics*, Helsinki: Suomalainen Tiedeakatemia.
Mäki, U. (1991a) 'Comment on Hands', in N.B. De Marchi and M. Blaug (eds). *Appraising Economic Theories: Studies in the Methodology of Research Programmes*, Aldershot: Edward Elgar, pp. 85–90.
Mäki, U. (1991b) 'Practical Syllogism, Entrepreneurship and the Invisible Hand: A Critique of the Analytic Hermeneutics of G. H. Von Wright', in D. Lavoie (ed.). *Economics and Hermeneutics*, London: Routledge, pp. 149–176.
Mäki, U. (1992a) 'Friedman and Realism', *Research in the History of Economic Thought and Methodology*, 10: 171–195.
Mäki, U. (1992b) 'The Market as an Isolated Causal Process: A Metaphysical Ground for Realism', in B.J. Caldwell and S. Boehm (eds). *Austrian Economics: Tensions and New Directions*, Dordrecht: Kluwer Academic Publishers, pp. 35–59.
Mäki, U. (1992c) 'On the Method of Isolation in Economics', in C. Dilworth (ed.). *Intelligibility in Science*, Atlanta and Amsterdam: Rodopi, pp. 319–354.
Mäki, U. (1992d) 'Social Conditioning of Economics', in N.B. de Marchi (ed.). *Post-Popperian Methodology of Economics: Recovering Practice*, Dordrecht: Kluwer, pp. 65–104.
Mäki, U. (1993a) 'Social Theories of Science and the Fate of Institutionalism in Economics', in U. Mäki, B. Gustafsson, and C. Knudsen (eds). *Rationality, Institutions and Economic Methodology*, London: Routledge, pp. 76–109.
Mäki, U. (1993b) 'Two Philosophies of the Rhetoric of Economics', in W. Henderson, T. Dudley-Evans, and R.E. Backhouse (eds). *Economics and Language*, Routledge, pp. 23–50.
Mäki, U. (1994a) 'Economics and the Philosophy of Science – Redman', *Economic Journal*, 104: 973–976.
Mäki, U. (1994b) 'Isolation, Idealization and Truth in Economics', in B. Hamminga and N.B. de Marchi (eds). *Poznan Studies in the Philosophy of the Sciences and the Humanities; Idealization VI: Idealization in Economics*, Amsterdam; Atlanta: Rodopi, pp. 147–168.
Mäki, U. (1994c) 'Methodology might Matter, but Weintraub's Meta-Methodology Shouldn't', *Journal of Economic Methodology*, 1: 215–231.
Mäki, U. (1994d) 'Reorienting the Assumptions Issue', in R.E. Backhouse (ed.). *New Directions in Economic Methodology*, London: Routledge, pp. 236–256.
Mäki, U. (1995) 'Diagnosing McCloskey', *Journal of Economic Literature*, 33: 1300–1318.
Mäki, U. (1996a) 'Scientific Realism and some Peculiarities of Economics', in R.S. Cohen, R. Hilpinen, and Q. Renzong (eds). *Realism and Anti-Realism in the Philosophy of Science*, The Netherlands: Kluwer, pp. 427–447.
Mäki, U. (1996b) 'Two Portraits of Economics', *Journal of Economic Methodology*, 3: 1–38.
Mäki, U. (1997a) 'The One World and the Many Theories', in A. Salanti and E. Screpanti (eds). *Pluralism in Economics: New Perspectives in History and Methodology*, Cheltenham: Edward Elgar, pp. 37–47.
Mäki, U. (1997b) 'Universals and the Methodenstreit: A Re-Examination of Carl Menger's Conception of Economics as an Exact Science', *Studies in History & Philosophy of Science Part A*, 28: 475–495.
Mäki, U. (1998a) 'Against Posner Against Coase Against Theory', *Cambridge Journal of Economics*, 22: 587–595.

Mäki, U. (1998b) 'As if', in J.B. Davis, D.W. Hands, and U. Mäki (eds). *The Handbook of Economic Methodology*, Cheltenham: Edward Elgar, pp. 25–27.
Mäki, U. (1998c) 'Aspects of Realism about Economics', *Theoria*, 13: 301–319.
Mäki, U. (1998d) 'Instrumentalism', in J.B. Davis, D.W. Hands, and U. Mäki (eds). *The Handbook of Economic Methodology*, Cheltenham: Edward Elgar, pp. 253–256.
Mäki, U. (1998e) 'Is Coase a Realist?' *Philosophy of the Social Sciences*, 28: 5–31.
Mäki, U. (1998f) 'The Problem of Social Coase: Between Regulation and Free Market in Economic Methodology', in S.G. Medema (ed.). *Coasean Economics: Law and Economics and the New Institutional Economics*, Kluwer Academic, pp. 249–269.
Mäki, U. (1998g) 'Realism', in J.B. Davis, D.W. Hands, and U. Mäki (eds). *The Handbook of Economic Methodology*, Cheltenham: Edward Elgar, pp. 404–409.
Mäki, U. (1998h) 'Realisticness', in J.B. Davis, D.W. Hands, and U. Mäki (eds). *The Handbook of Economic Methodology*, Cheltenham: Edward Elgar, pp. 409–413.
Mäki, U. (1998i) 'Separateness, Inexactness, and Economic Method', *Journal of Economic Methodology*, 5: 147–154.
Mäki, U. (1999a) 'Representation Repressed: Two Types of Semantic Scepticism in Economics', in R. Favretti, S.G. Rossini, and R. Scazzieri (eds). *Incommensurability and Translation: Kuhnian Perspectives on Scientific Communication and Theory Change*, Williston, Vt.: Elgar; distributed by American International Distribution Corporation, pp. 307–321.
Mäki, U. (1999b) 'Science as a Free Market: A Reflexivity Test in an Economics of Economics', *Perspectives on Science*, 7: 486–509.
Mäki, U. (2000a) 'Kinds of Assumptions and their Truth: Shaking an Untwisted F-Twist', *Kyklos*, 53: 317–335.
Mäki, U. (2000b) 'Performance Against Dialogue, Or Answering and Really Answering: a Participant Observer's Reflections on the McCloskey Conversation', *Journal of Economic Issues*, XXXIV: 43–59.
Mäki, U. (2000c) 'Reclaiming Relevant Realism', *Journal of Economic Methodology*, 7: 109–125.
Mäki, U. (2001a) 'Economic Ontology: What? Why? How?' in U. Mäki (ed.). *The Economic World View*, Cambridge: Cambridge University Press, pp. 3–14.
Mäki, U. (2001b) 'Explanatory Unification: Double and Doubtful', *Philosophy of the Social Sciences*, 31: 488–506.
Mäki, U. (2001c) 'Models: Philosophical Aspects', in N.J. Smelser and P.B. Baltes (eds). *International Encyclopedia of the Social & Behavioral Sciences*, Oxford: Pergamon, pp. 9931–9937.
Mäki, U. (2001d) 'Realisms and their Opponents: Philosophical Aspects', in N.J. Smelser and P.B. Baltes (eds). *International Encyclopedia of the Social & Behavioral Sciences*, Oxford: Pergamon, pp. 12815–12821.
Mäki, U. (2001e) 'The Way the World Works (www): Towards an Ontology of Theory Choice', in U. Mäki (ed.). *The Economic World View*, Cambridge: Cambridge University Press, pp. 369–389.
Mäki, U. (2002a) 'The Dismal Queen of the Social Sciences', in U. Mäki (ed.). *Fact and Fiction in Economics*, Cambridge: Cambridge University Press, pp. 3–34.
Mäki, U. (2002b) 'Explanatory Ecumenism and Economics Imperialism', *Economics and Philosophy*, 18: 237–259.
Mäki, U. (2002c) 'Scientific Progress: Complexities of a Contestable Concept', in S. Böhm, C. Gehrke, H.D. Kurz, and R. Sturn (eds). *Is there Progress in Economics?* Cheltenham: Edward Elgar, 123–130.

Mäki, U. (2002d) 'Some Nonreasons for Nonrealism about Economics', in U. Mäki (ed.). *Fact and Fiction in Economics*, Cambridge: Cambridge University Press, 90–106.
Mäki, U. (2003a) '"The Methodology of Positive Economics" (1953) does Not Give Us the Methodology of Positive Economics', *Journal of Economic Methodology*, 10: 495–505.
Mäki, U. (2003b) 'The Archaeological Construction of the Past: Some Realist Moderations', in M. Sintonen, P. Ylikoski, and K. Miller (eds). *Realism in Action*, The Netherlands: Kluwer Academic Publishers, pp. 51–68.
Mäki, U. (2003c) 'Ceteris Paribus: Interpretaciones e Implicaciones', *Revista Asturiana de Economia*, 28: 7–32.
Mäki, U. (2004a) 'Realism and the Nature of Theory: A Lesson from J H Von Thünen for Economists and Geographers', *Environment & Planning A*, 36: 1719–1736.
Mäki, U. (2004b) 'Some Truths about Truth for Economists, their Critics and Clients', in P. Mooslechner, H. Schuberth, and M. Schurtz (eds). *Economic Policy-Making Under Uncertainty: The Role of Truth and Accountability in Policy Advice*, Cheltenham: Edward Elgar, pp. 9–39.
Mäki, U. (2004c) 'Theoretical Isolation and Explanatory Progress: Transaction Cost Economics and the Dynamics of Dispute', *Cambridge Journal of Economics*, 28: 319–346.
Mäki, U. (2005a) 'Economic Epistemology: Hopes and Horrors', *Episteme: A Journal of Social Epistemology*, 63: 211–220.
Mäki, U. (2005b) 'Reglobalising Realism by Going Local, or (How) Should our Formulations of Scientific Realism Be Informed about the Sciences', *Erkenntnis*, 63: 231–251.
Mäki, U. (2006) 'Remarks on Models and their Truth', *Storia del Pensiero Economico: Nuova Serie*, 3: 7–19.
Mäki, U. (2008a) 'Economics', in M. Curd and S. Psillos (eds). *The Routledge Companion to Philosophy of Science*, London and New York: Routledge, 543–554.
Mäki, U. (2008b) 'Method and Appraisal in Economics, 1976–2006', *Journal of Economic Methodology*, 15: 409–423.
Mäki, U. (2008c) 'Putnam's Realisms: A View from the Social Sciences', in S. Pihlström, P. Raatikainen, and M. Sintonen (eds). *Approaching Truth. Essays in Honour of Ilkka Niiniluoto*, London: College Publications, 295–306.
Mäki, U. (2008d) 'Realism from the "lands of Kaleva": An Interview with Uskali Mäki', *Erasmus Journal for Philosophy and Economics*, 1: 124–146.
Mäki, U. (2008e) 'Scientific Realism and Ontology', *The New Palgrave Dictionary of Economics*, London: Macmillan.
Mäki, U. (2009a) 'Economics Imperialism: Concept and Constraints', *Philosophy of the Social Sciences*, 39: 351–380.
Mäki, U. (2009b) 'MISSing the World: Models as Isolations, Representations, and Credible Worlds', *Erkenntnis*, 70: 29–43.
Mäki, U. (2009c) 'Models and Truth: The Functional Decomposition Approach', in M. Suárez, M. Dorato, and M. Rédei (eds). *EPSA Epistemology and Methodology of Science*, Dordrecht: Springer, pp. 177–187.
Mäki, U. (2009d) 'Neuroeconomics: Issues of Scientificity and Interdisciplinarity', in G. Margues (ed.). *Rationalidad, Economia e Interdisciplinariedad*, Buenos Aires: Facultad de ciencias económicas, Universitad de Buenos Aires, pp. 299–307.
Mäki, U. (2009e) 'Realistic Realism about Unrealistic Models', in H. Kincaid and D. Ross (eds). *Oxford Handbook of the Philosophy of Economics*, Oxford: Oxford University Press, pp. 68–98.

Mäki, U. (2009f) 'Unrealistic Assumptions and Unnecessary Confusions: Rereading and Rewriting F53 as a Realist Statement', in U. Mäki (ed.). *The Methodology of Positive Economics. Reflections on the Milton Friedman Legacy*, Cambridge: Cambridge University Press, pp. 90–116.
Mäki, U. (2010) 'When Economics Meets Neuroscience: Hype and Hope', *Journal of Economic Methodology*, 17: 107–117.
Mäki, U. (2011a) 'Models and the Locus of their Truth', *Synthese*, 180: 47–63.
Mäki, U. (2011b) 'Puzzled by Realism: A Response to Deichsel', *Erasmus Journal for Philosophy and Economics*, 4: 42–52.
Mäki, U. (2011c) 'Scientific Realism as a Challenge to Economics', *Journal of Economic Methodology*, 18: 1–12.
Mäki, U. (2011d) 'The Truth of False Idealizations in Modeling', in P. Humphreys and C. Imbert (eds). *Representation, Models and Simulations*, London: Routledge, 216–233.
Mäki, U. (forthcoming) 'Realism and Antirealism about Economics', in U. Mäki (ed.). *Philosophy of Economics*, Dordrecht: Elsevier BV.
Mäki, U. and Marchionni, C. (2009) 'On the Structure of Explanatory Unification: The Case of Geographical Economics', *Studies in History and Philosophy of Science*, 40: 185–195.
Mäki, U. and Marchionni, C. (forthcoming a) 'Economics as Usual: Geographical Economics Shaped by Disciplinary Conventions', in J.B. Davis and D.W. Hands (eds). *The Elgar Companion to Economic Methodology*, Cheltenham: Edward Elgar.
Mäki, U. and Marchionni, C. (forthcoming b) 'Is Geographical Economics Imperializing Economic Geography?' *Journal of Economic Geography*.
Mäki, U. and Oinas, P. (2004) 'The Narrow Notion of Realism in Human Geography', *Environment & Planning A*, 36: 1755–1776.
Mäki, U., and Piimies, J. (1998) 'Ceteris Paribus', in J.B. Davis, D.W. Hands, and U. Mäki (eds). *The Handbook of Economic Methodology*, Cheltenham: Edward Elgar, pp. 55–58.
Mäki, U., and Sappinen, J. (2011) 'Homo economicus ja marginalismin perintö' [Homo Economicus and the Legacy of Marginalism, in Finnish], in R. Heiskala and A. Virtanen (eds). *Talous Ja Yhteiskuntateoria [Economy and Social Theory]*, Helsinki: Gaudeamus, pp. 291–321.
Melitz, J. (1965) 'Friedman and Machlup on the Significance of Testing Economic Assumptions', *The Journal of Political Economy*, 73: 37–60.
Morgan, M.S. (2001) 'Models, Stories and the Economic World', *Journal of Economic Methodology*, 8: 361.
Musgrave, A. (1981) '"Unreal Assumptions" in Economic Theory: The F-Twist Untwisted', *Kyklos*, 34: 377–387.
Nowak, L. (1980) *The Structure of Idealization: Towards a Systematic Interpretation of the Marxian Idea of Science*, Dordrecht and London: D. Reidel.
Peter, F. (2001) 'Rhetoric Vs Realism in Economic Methodology: A Critical Assessment of Recent Contributions', *Cambridge Journal of Economics*, 25: 571–589.
Psillos, S. (2000) 'The Present State of the Scientific Realism Debate', in P. Clark and C. Hawley (eds). *Philosophy of Science Today*, Oxford: Clarendon Press, pp. 59–82.
Sen, A.K. (1980) 'Description as Choice', *Oxford Economic Papers*, 32: 353–69.
Sugden, R. (2000) 'Credible Worlds: The Status of Theoretical Models in Economics', *Journal of Economic Methodology*, 7: 169–201.
Van Fraassen, B.C. (1980) *The Scientific Image*, Oxford: Clarendon Press.

Part I

Isolating truth in economic models