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Isolation Is Not Characteristic of Models

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Modelling cannot be characterized as isolating, nor models as isolations. This article presents three arguments to that effect, against Uskali Mäki's account of models. First, while isolation proceeds through a process of manipulation and control, modelling typically does not proceed through such a process. Rather, modellers postulate assumptions, without seeking to justify them by reference to a process of isolation. Second, while isolation identifies an isolation base—a concrete environment it seeks to control and manipulate—modelling typically does not identify such a base. Rather, modellers construct their models without reference to concrete environments, and only later seek to connect their models to concrete situations of the real world. Third, Mäki argues that isolation employs idealization to control for disturbing factors, but does not affect the factors or mechanisms that are supposed to be isolated. However, models typically make idealizing assumptions about the factors and mechanisms that are the focus of investigation. Thus, even the product of modelling often cannot be characterized as isolation.

1. Introduction

With his isolation account, Uskali Mäki made an important contribution to the philosophy of science, and in particular to the philosophy of economics. In a series of publications, Mäki (1992, 1994, 2004a, 2004b) has characterized isolation as a distinct method of theorizing, and has convincingly shown its usefulness for the explication of certain cases of economic theorizing.

In some recent publications, however, Mäki (2005, 2006, 2009a, 2009b) has sought to extend his isolation account to function as an explication of what scientific models are, and how they function. Although he focuses largely on examples from economics, Mäki clearly intends his account to have wider applicability. In this article, I argue

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against this account of models as isolations. Like Mäki, I also focus on economic models.

Beyond the specifics of Mäki's account, my argument may be of interest to a wider audience. A number of influential philosophers of science, among them Nancy Cartwright, Michael Strevens, and Robert Batterman, have suggested that models explain by representing only those factors that make a difference to the occurrence and essential character of the explanandum (cf. Weisberg 2007a). Mäki's account of models as isolations spells out certain views that defenders of such a minimalist model account seem to share. An in-depth discussion of Mäki's account may therefore also elucidate some of the issues that the minimalist account faces.

I start by recapitulating the main features of Mäki's isolation account in Section 2. Section 3 summarizes Mäki's attempts to characterize models as isolations. Against this characterization, I address instances in which models and isolations diverge. In Section 4, I argue that while isolations commence from a base, gradually eliminating certain features from it, models do not start from such a base. In Section 5, I argue that the process of eliminating or sealing-off, characteristic of isolation procedures, is not the common modelling process. In Section 6, I point out that models often idealize their very targets, and thus do not offer a 'neutral', isolating setting. Addressing these three questions, I conclude that Mäki's isolation framework cannot function as a general account of models and modelling. Instead, I argue in Section 7 that Mäki's account of isolation should be seen as a sufficient (albeit not necessary) criterion of model success. Section 8 concludes.

2. Isolation as a Form of Scientific Theorizing

In the early 1990s, Mäki described a form of scientific theorizing that had not received proper attention from philosophers of science. He termed this form of theorizing *isolation*, and characterized it as follows:

> Theoretical or ideal isolation ... is manifest when a system, relation, process, or feature, based on an intellectual operation in constructing a concept, model or theory, is closed from the involvement or impact of some other features of the situation. (Mäki 1992, 325)

Thus, isolation is a *process* that leads from a *base* to an isolated *product*. These three aspects are central to the notion of isolation.

First, isolation is a *process*, an 'intellectual operation' that consists in 'constructing' and in 'closing off from'. This process, Mäki suggests, is analogous to scientific experimentation. An experimenter causally intervenes in a process occurring in the world, and thus closes off the target entity from causal interferences of other entities. Yet while the experimenter causally manipulates real entities, the theoretical isolator manipulates representations (Mäki 1994, 151).[1] Mäki therefore conceives of theoretical isolations as thought experiments, as opposed to laboratory experiments: 'isolation takes place in one's ideas, not in the real world' (Mäki 1992, 325).

Yet Mäki also sees the limitations of this analogy: 'The theoretical method of isolation involves an imitation of a limited portion of the logical structure of experimental research capable of material isolation' (Mäki 1996, 443). While they differ on ontological grounds and epistemic powers, experiments and theoretical isolations share the same logical structure. But what are the properties that characterize this logical structure of isolation?

Isolation proceeds via two procedures. The act of *omission* excludes the impact of some factors by neglecting them in the representation. The act of *idealization* distorts the impact of some factors, by changing a parameter in the representation to a different value, typically to zero or infinity. Idealization thus yields a deliberately false representation (Mäki 1992, 324). To engage in idealization requires that one represents the idealised element, and that one knows that this element could be more accurately represented—even if the exact form of this more accurate representation is not spelled out.

Mäki addresses the procedural aspect of isolation explicitly when discussing Ronald Coase's methodology. He interprets Coase's rejection of the neoclassical tradition as 'blackboard economics' as rejecting too high a degree of vertical abstraction—i.e. as too high a degree of isolating universal aspects of things from their particular features. But Mäki observes that Coase himself employs vertical abstractions: after all, he wrote about the nature of the firm, not the natures of particular firms. To make sense of Coase criticism, Mäki acknowledges 'that the term "abstraction" in the vertical context designates two different concepts, namely the *level of abstraction* and the *process of abstraction*' (Mäki 1998, 15; emphasis in original). In a process of abstraction, a theorist moves from detailed knowledge of particular phenomena to vertically abstract notions of these phenomena. Coase, Mäki notes, performed a large number of case studies on industrial structure and behaviour, hence documenting the detailed knowledge from which he then derives his vertical abstraction.

Second, isolation commences from a *base*, closing off 'the involvement or impact of some other features *of the situation*' (Mäki 1992, 325; my emphasis). By 'base' I here mean any description of an environment that the isolation seeks to manipulate and control. This may be a description of the actual world. For example, Mäki interprets Coase as first establishing case studies of real firms, and then isolating certain features from these detailed description. Here, through isolation, a 'situation is simplified by removing items from the actual situation' (Mäki 2004b, 1725).

Alternatively, a base may be another level of theory or quasi-theory. For example, Mäki (1996) postulates the existence of three 'hypothetical levels of thought': general folk views, folk economics, and scientific economics. The entities in all three of these levels remain the same, 'entities with which economists and others are familiar on the basis of ordinary experience' (Mäki 1996, 434). Yet when moving from one level to the next, these entities are modified and rearranged. By moving from the general folk view to folk economics, 'emotions are excluded in favor of rational deliberation. Making love is excluded in favor of making money' (Mäki 1996, 435). By moving from folk economics to scientific economics, properties of these entities are 'selected', 'abstracted', 'idealized', 'projected', or 'aggregated' (Mäki 1996, 435). Thus, an instance of the general folk view would be a base for folk-economic isolation, and an instance of folk economics would be a base for a scientific-economic isolation.

Furthermore, even within the realm of scientific economic theories, bases of isolation can be found. When discussing debates and changes in economic theorising, Mäki (2004a) suggests that debate and change can often be reinterpreted in terms of isolation and re-isolation. Thus a critic may suggest a new theory in which the focus of isolation of a prior theory is shifted towards another feature, or in which the isolation is narrower than in the prior theory. The prior theory then is the base of isolation. The new theory is developed from it by a process of omission or idealisation.

Whether a description of phenomena or a theory, what is relevant is that the base has a higher degree of complexity, with the interaction of various entities, so that there is something to be isolated from. Additionally, this base must be, in some sense, represented. Isolation is a manipulation of representations (Mäki 1994, 151); hence not only the result of the isolation is a representation, but also the base from which isolation starts.

Third, isolation yields a *product*, 'a concept, model or theory' that is appropriately isolated. In laboratory experiments, a target entity is shielded from the causal interferences of other entities, yielding a *material isolation*. Theoretical isolations, in contrast, yield a representation in which some entities are 'sealed off' (Mäki 1992, 321) from the influence of everything else.

Conceptually, two kinds of isolation products can be distinguished. An abstraction is 'a universal ... isolated from particular exemplifications' (Mäki 1992, 322). For example, a theorist may abstract from the linear form of the functional equation $q = a + bp$, instead choosing $q = f(p)$ for a theoretical representation. Mäki therefore calls abstraction *vertical isolation*. A *horizontal isolation*, in contrast, isolates while keeping the level of abstraction constant. For example, a theorist may isolate the influence of p on q from other influences by representing it in the form $q = f(p)$ instead of $q = f(p, p', p'', \ldots)$. Notably, only horizontal isolation corresponds to isolation in experiments, while vertical isolation is merely realizable as a manipulation of representations.

Mäki carefully distinguished the isolation process, which may involve idealisation, from the product of isolation. When isolating a factor F from intervening factors $G1, \ldots, Gn$, one may either omit or idealize the operations of the Gi, but not the operation of the factor F itself. This way, one makes false claims about the Gi; but the purpose of the theoretical process—to isolate the operation of F—remains intact. Idealisation thus is a procedure applied to entities one isolates from, but not to entities that one intends to isolate (Mäki 1992, 328); and idealization is used as an auxiliary technique for generating isolation, yet it is not part of isolation itself (Mäki 1992, 325). Thus the product of isolation—the isolated factor—is never idealized.

Mäki originally developed the isolation account in order to support a realist interpretation of economic theories (Mäki 2009a, 70–72). The core of this argument is isolating theories are representations of the world that *partially* resemble the world, and hence are true. Assuming that (i) theories consist of separable elements, and

(ii) the world is partitioned into the aspects some of which resemble the theory elements, one can say that the partial resemblance consists in a resemblance of certain aspects of the world—in particular causal factors active in the world (Mäki 1994, 149–152). Assuming further a correspondence notion of truth, this resemblance between aspects of theory and world allows Mäki to say that 'an isolating theory or statement is true if it correctly represents the isolated essence of the object' (Mäki 1992, 344).[2]

This of course is a non-standard use of the correspondence notion of truth, as it yields the theory to be true (by resembling an isolated essence), while at the same time it is also false (in its statements about idealized entities). Consequently, Mäki distinguishes two truth concepts, where a theory making *only* true statements expresses 'the whole truth', while a theory making true statements about the intended isolated objects is 'nothing-but-true'.

The use of isolation as a defence of realism makes substantial ontological assumptions about the relation of isolating theory and parts of the real world. I will therefore call it the *essential* account. In particular, it claims that the factors and causal mechanisms represented by a successful isolating theory are real, in the partition that the theory proposes. Whether one can hope to find such ontological assumptions to hold in the social realm seems largely an open question (cf. Reiss 2009).

Such concerns may have motivated Mäki in the early 1990s to develop the isolation concept in a more formally characterized way, less reliant on an essentialist ontology. This account characterizes isolation through the three properties of the theory construction process discussed in this section, namely isolation base, procedure, and product. It is used as a theory of how economists theorize: their ways of theorizing is influenced by how they *believe* their theories relate to the world, but is independent of how their theories relate to the world. Isolation then becomes a meta-theoretical framework that helps organize and understand the development of a particular theory (Mäki 2004a). In this usage, the framework spells out what properties make a theory an isolation, and how it influences scientists' theorizing choices. To distinguish this account from earlier essential isolation, I call it *formal* isolation.[3]

Based on this formal account, Mäki argues that isolation is a method of theorizing that has particular centrality in the sciences. It includes abstraction as a special form of isolation; and because idealization is a means for isolation, isolation holds primary status over idealization (Mäki 1992, 332–333). Yet throughout Mäki's earlier work (Mäki 1992, 1994, but also 2004a), isolation is presented as one method of theorizing among many.

In recent publications, Mäki has disavowed the procedural aspects of the formal account. Instead, he defends a *minimalist* account by claiming what matters is the product—that is, that the model is an isolation—and not that the modelling is identifiable as an isolation process from an isolation base.

> I take theoretical isolation to be a central characteristic of an important class of models, akin to isolation in material experiments ... What the two procedures share is the *goal* or function of closing a system by neutralising a number of factors that are not included in the isolated system. This outcome is essential for

> isolative modelling, while the precise *way* in which isolations are implemented is inessential. (Mäki 2009b, 31; emphasis added)[4]

One might be sceptical about such a minimalist notion, on the grounds that intuitive notion of isolation includes a procedural aspect, and requires a connection to a base. From such a point of view, to claim that a theory is an isolation, and to link it to material experiments is flatly contradicted by the consecutive claim that such a theory is not the result of any specific isolation process.

I will not press this intuition here. Instead, I take the minimal account as an alternative to the formal account in the following analysis. The arguments in Sections 4 and 5 against isolation as a characteristic feature of modelling then apply only to the formal account, while the arguments presented in Section 6 apply to both.

3. Models as Isolations and Modelling as the Process of Isolation

In more recent publications, Mäki (2005, 2006, 2009a, 2009b) has proposed the isolation account not only as a meta-theory of one mode of theorizing, but as a general account of economic modelling. Crucially, Mäki has conceived of his account as universal: *every* model is fruitfully explicated as an isolation. In particular, isolation supposedly characterizes the representational function of models, their relation to experiments and their truth.

Mäki's account of models as isolations and surrogate systems (MISS account) distinguishes between the representative and resemblance aspects of representation, and embeds models in a pragmatic context that includes the modeller's purposes, audiences, and commentary. More specifically, a model is an object used by an agent as

> a representative of some target system *R* for purpose *P*, addressing audience *E*, prompting genuine issues of resemblance to arise; and applies commentary *C* to identify and align these components. (Mäki 2009b, 32)

Of these characterizing elements, the notion of representative is most relevant for the present purpose. A model is a representative of some target in the sense that it stands for that target as its *surrogate*. A model functions as a surrogate for a target in the sense that instead of seeking to acquire information about target *R* by examining *R* directly, one examines the properties of the model, thus hoping to indirectly acquire information about *R*.

For a model to fulfil the function of a surrogate of *R*, the model must resemble the target system *R* in suitable respects and sufficient degrees.[5] Yet Mäki has argued that a model being a representation of *R* does not require resemblance. Rather 'it only requires issues of resemblance to potentially arise' (Mäki 2009b, 32). This requires that model shave the likely capacity to resemble, and that 'irrelevant resemblances do not count'.

Mäki has contrasted models as surrogates with models as *substitute systems*. A model is a substitute system, if it does not raise issues of resemblance at all. This may be the consequence of model users focusing their attention merely on examining the properties of the model without any interest in the resemblance aspect of representation. The model then

> becomes a substitute system, a freely floating subject of inquiry, unconstrained by any concern as to how it might be connected to real-world facts. It substitutes for the real system rather than serves as its surrogate. (Mäki 2009b, 36)

Thus, the issue of resemblance is central to Mäki's MISS account. It is here that the notion of isolation comes in, as it explains how models resemble their target systems.

> Models represent the target systems as far simpler, as devoid of most of those properties and causal facts, highlighting or focussing on just a small fraction of them. I have attempted to capture this feature of models by saying (following Marshall and other economists) that they *isolate* a fragment of their target systems. (Mäki 2006, 10; emphasis added)

The issue that the representative function of models raises is that of partial resemblance, explicated as the isolation of certain features of the target by the model. Isolation thus becomes one of the central tenets of the MISS account.

Mäki has explored the relevance of the isolation notion for the understanding of models and modelling further by suggesting an analogy between models and *experiments*. Models, he argued, are constructed to create 'a simple and controlled mini-world in contrast to the complex and uncontrolled maxi-world'. The way they are constructed proceeds via manipulation: 'a system of entities is manipulated in order to accomplish effective isolations of a limited set of properties and causal relations from the rest of the world' (Mäki 2005, 306). The only difference between experiments and models lies in what exactly is manipulated:

> While material experimentation employs causally effected controls, theoretical modelling uses assumptions to effect the required controls. Assumptions are used to neutralise, in the model worlds, the involvement of other things by assuming them to be constant, absent, of zero strength, negligibly small, in a normal state, within certain intervals, and so on ... The structure of experimentation, involving controls and isolation, is the same, while what is different is the way these controls and isolations are effected: by way of thinking and assuming, and by way of material or causal manipulation. (Mäki 2005, 308–309)

Mäki thus drew on his earlier analogy of material experiments and theoretical isolations to characterize models. Models are a special kind of theoretical isolation, and modelling is a special kind of theoretical isolating.

Lastly, Mäki argued that models can be true if they are isolations. They can be true, he said, by isolating a real causal force, and showing its characteristic way of functioning (Mäki 2006, 14). This reading is further supported by another article, where Mäki stated:

> Economists can be philosophical realists about their models even though these describe imaginary situations ... This is because it is possible that the mechanisms in operation in those imaginary situations are the same as, or similar to, those in operation in real situations. (Mäki 2009a, 79)

The diagnosis that economists can be realists about some parts of their models clearly presupposes that these models are isolations, and may be successful isolations. So Mäki embraces the view that economic models (by and large) are isolations.

To conclude, Mäki characterized models as isolations and modelling as isolating. By appealing to this characteristic of models, he strives to answers the question what models are, how they can be true, and how to understand their use. Against this, I will argue that models and the practice of modelling in economics often do not go well together with Mäki's isolation account.

4. Lack of Base

The first aspect in which models and isolations differ is what they are constructed from. Intuitively, and in accord with Mäki's formal account, an isolation is an isolation *from* something else. This is obvious in material isolations. Experimenters commonly believe that the same causes are at work in their experiments as in the relevant parts of the real world, because they take the 'stuff' of the real world and examine it under controlled conditions in the laboratory. An experimental asset market, for example, includes real human actors, but has them trade only a small number of stocks, with a given budget, for a limited time, in specified channels of communication under certain information. But beyond this general relation to the world through using its material, experimenters typically identify the specific base or bases from which their experiments isolate. Take the following example:

> Our immediate objective in the present series of experiments was to determine whether agents would actively trade an asset when all investors faced identical uncertain dividend payout schedules. The *previous cited asset experiments* pay different dividends to different investors on the grounds that investors have different opportunity costs. But if this is so, subject agents ought to have their own homegrown differences in opportunity cost (*as in field environments*). Consequently it is an open question whether artificially inducing different dividend values on subject investors is a necessary condition for observing trade. (Smith, Suchanek, and Williams 1988, 1122; emphasis added)

Vernon L. Smith, Gerry L. Suchanek, and Arlington W. Williams here identify two bases from which their experiments isolated: previous experiments and field environments. Furthermore, they identify the specific feature exhibited in both bases, from which their experiment isolates: different dividends to different investors. Because almost all actual asset markets exhibit this feature, they do not need to specify any concrete instance of it; while they are much more specific about the alternative laboratory experiments that exhibit this feature.

To have a base, then, an isolation at least needs to refer to those properties that it seeks to isolate from. Genuine experiments do this by listing all the factors that are explicitly controlled for—the assumption being that any non-listed factors are the same as in the base. Only then is there a reason to assume that experiments treat the same 'stuff' as found in the target system, and only then might there be reason to have more confidence in experiments than e.g. in 'merely formal' simulations (cf. Morgan 2003, 231; Guala 2005, 215).

Yet theoretical models in economics often lack such a base. Economic modellers commonly do not refer to the relevant property of such a base when constructing a model; nor do they refer to such a link once the model has been constructed. Thomas C. Schelling gives a good illustration of this when describing how he came to develop his famous chequerboard model. As he recounts in a recent note, he was interested in how people's interactions led to unintended consequences, for example spatial patterns resulting from 'preferences about whom to associate with in clubs, classes, or ballparks, or at dining tables'. After searching the sociological literature, he concluded 'I found nothing I could use, and decided I'd have to work something out for myself' (Schelling 2006, 249). This 'working out', alas, was entirely devoid of references to the specific properties of those clubs, classes, or dining tables, in particular to properties that the model isolated from.

Instead, Schelling 'experimented' with pen and paper, with copper and zinc pennies, and finally with tokens on a chequerboard. The experiments consisted of defining one- or two-dimensional spaces, randomly distributing symbols, coins or tokens over it, and then letting them 'move' according to some predefined rule. The moves yielded robust patterns that interested Schelling: 'I experimented with different sizes of "neighbourhoods" ... and got results that fascinated me ... We kept getting the same kind of results, and the dynamics were intriguing' (Schelling 2006, 250). It becomes clear from this quotation that Schelling did not isolate his model from any base. First, it is obvious that he did not have any concrete urban space in mind when he constructed the model. But that may be too strong a criterion, as Smith et al. (1988) in the above example do not refer to concrete asset markets either. Yet unlike them, Schelling does not refer to any properties of actual city types, either—there is no reference to data or the result of data analysis, nor to background knowledge of any form. Schelling may of course have used such background knowledge in his modelling choices—but the degree to which he did remains mere speculation, as he chose not to document it anywhere. The real world, with its concrete neighbourhoods or generalizable observations about urban environments, is thus not the base for the chequerboard model, at least not in any detectable way.[6]

Rather, Schelling developed first a paper-and-marks, and later a coin-and-chequerboard system because (a) they offered an interesting interpretation (allowing one to interpret elements as 'blacks', 'whites', and 'neighbours'), and (b) they allowed interesting manipulations ('moving them around', 'deciding where one can go'). It was the formal properties of the representation device that gave rise to the properties of the surrogate system, and not any explicit link to a real-world base.

Schelling's chequerboard model is considered by many to be an exemplary case of a good theoretical economic model (Sugden 2000; Aydinonat 2007). Of course, it cannot stand for all of the many different kinds of models employed in economics or the sciences more generally. But because many economists consider it an important model worth aspiring to, it is sufficiently significant an example to pose a problem for

Mäki's account. Furthermore, Mäki himself analyses this model and admits that it does not have a base:

> Schelling's Commentary does not go far enough beyond such possibilities to allow him to state that the mechanism isolated by his model is actually in operation and responsible for a segregated pattern. The model isolates a possible mechanism. (Mäki 2009b, 38)

Mäki suggests that Schelling's model isolates a social mechanism, but admits that the accompanying commentary leaves it open whether such a mechanism exists in any real-world situation. In Mäki's analysis, all that Schelling claims is that it is possible that this mechanism is in operation in some real-world situation. This implies, however, that in the model construction process no situation or type of situations was identified in which this mechanism was in operation, and from which this mechanism was consecutively isolated.

Thus, taking Schelling's description as a typical case rather than an aberration, models are distinct from isolations: while experiments (and hence material isolations) necessarily start from a (concrete) base, models do not necessarily do so.

If isolations commence from a base, as intuition and Mäki's early account suggest, then models differ from isolations in this way. Models are typically constructed without reference to a base. Unlike experiments, and *pace* Mäki, they are not the result of 'stripping' or 'sealing off' elements of a base. In model construction, and sometimes in model use, there is a conspicuous absence of any base from which they could have been isolated.

5. The Lack of Process

The second aspect in which models and isolations differ is the process by which they are constructed. Intuitively, and in keeping with Mäki's formal account, isolations are constituted by isolating processes. This is obvious in laboratory experiments. By some detectable causal intervention, an environment is controlled, an impulse given, a reaction channelled in certain ways. Through such interventions, experiments are clearly distinguished from quasi-experiments (also called natural experiments) or observation studies. Scientific actions without such a process of consecutive interventions would not be considered controlled experiments. Similarly, it seems, with theoretical isolations: why speak of isolation if an isolation process is lacking?

Indeed, Mäki suggests that modelling proceeds through the manipulation of entities in a way structurally similar to isolation in experimentation:

> Assumptions are used to neutralise, in the model worlds, the involvement of other things by assuming them to be constant, absent, of zero strength, negligibly small, in a normal state, within certain intervals, and so on. (Mäki 2005, 308)

In the words of Mäki's earlier publications, thought experiments are performed through idealisation and omission processes: 'the vast number of items is excluded . . . neglected . . . omitted . . . nullified' (Mäki 1994, 149–151), 'modified' and 'purified'

(Mäki 2005, 306). One finds a particularly clear expression of this idea in Mäki's analysis of Coase's methodology.

> The Coasean research process involves changing levels of vertical abstraction, while Coase might argue that this does not happen in blackboard economics which sticks to a high level of vertical abstraction without an ongoing process of abstraction. (Mäki 1998, 15)

A process of abstraction or vertical isolation thus involves changing levels of abstraction.

This implies that states of the representation previous to the final model must be existent in some form. To neutralize a property, there must be a suggestion of the variable's non-neutral state. To change it into a stable state, there must be some trace of its dynamics. To neglect something, a hint of its possible existence is required. Mäki here acknowledges both that the presence of such processes may have important epistemic consequences for models, and that different modelling methodologies can be distinguished by the presence or absence of such processes.

Yet in most practices of theoretical modelling in economics, such processes and sequences of changes are notably absent. Take Schelling's case. It gives no evidence of the modeller neutralising, changing, or neglecting anything. No levels of abstraction different from the proposed models are documented. Rather, Schelling sticks with one level of abstraction of the chequerboard.

The contrast to the practices of those *not* involved in modelling may make this point clearer. Those scientists go through the processes of data measurement, data clean-up, data refinement, and the construction of 'data models' or 'phenomenological laws'. In their papers, they extensively document how they proceeded, and what the sequence of changes is. Most theoretical modellers in economics, however, do no such thing. Their papers commonly lack any evidence of how they arrived at their models. In the rare cases where modellers speak about their modelling practice, they say something like Schelling: they start with an 'idea', they played around with a formal system, they used the system as a stand-in for the idea. What they do not report is that they developed the system in a number of stages, changing an initially complex system into something simpler and more isolated. Thus, modelling practice lacks detectable processes of manipulation that seem so characteristic of isolation.

Some authors have argued that the lack of isolation or abstraction processes is characteristic of scientific models generally. Weisberg (2007b; see also Godfrey-Smith 2006) illustrates how models as indirect representations contrast with direct theoretical representations. When developing his famous predator–prey model, Vito Volterra did not proceed by identifying patterns and structures of a real-world phenomenon, working out which of these properties are essential and which ones can be abstracted away. Rather, he proposed a completely abstract mathematical system as a possible object of study for the Mediterranean fish population, without ever having studied the actual fish populations before (Weisberg 2007b, 14). Volterra's differential equations are thus an exemplary case of modelling. In contrast to this, direct theoretical representations identify patterns in data, and develop these patterns to organize, simplify (and hence isolate) hypotheses. As typical examples of such direct theoretical representations, Weisberg suggests Mendeleev's periodic system and Darwin's theory of coral reefs. Thus Godfrey-Smith and Weisberg suggest the lack of an abstraction or isolation process as *the* central characteristic of modelling, and hence agree with my diagnosis that models and isolations diverge in this respect.

It is possible, though, that modellers proceed through such an isolation process, yet do not report it. Then Mäki's claim—that theoretical modellers isolate in a way similar to experimenters—may still help one understand models and modelling better. Yet there are further reasons to doubt this analogy.

In experiments, the process of controlling and isolating itself has an epistemic function. Experimental manipulation processes are highly restricted by the causal properties of the manipulated materials: liquids cannot be heated beyond their boiling points, animals endure only limited toxic doses, and people react to opponents only if they know of their existence. Experimenters' attempts to heat a medium, reach a drug dose level, or create an interactive environment are frequently frustrated, because the causal properties of the materials resist such manipulations. Such failed manipulations, occurring in the isolative process, are important sources of learning for experimenters, and are frequently reported as such. Crucially, such failures not only tell us about research strategy or experimental design, but also about properties of the actual materials experimented with.

In contrast, modellers do not report such epistemic gains from the process of modelling. This is not surprising. Models, in Mäki's view, are representations. But representations are established, first, by constructing an artefact, second, by declaring this artefact to be a representation of a target, and third, by establishing some connection between the artefact and the target (Knuuttila and Voutilainen 2003). In this order of construction, the target imposes *no* constraints on developing the artefact: the modeller can imagine any object, property or relation at her whim. Thus the construction process of theoretical models cannot reveal any actual constraints of the real world, but only discovers the implications of the model's assumptions. Hence modelling does not offer the same opportunities of learning as experimenting.

To use the above example again, Schelling's choice of a chequerboard was not constrained by his intention to model certain spatial dynamics. He could have later found out that the dynamics produced by this model were not interesting or relevant for his epistemic purposes. But there was nothing in the model construction process that would have prevented him from choosing the chequerboard, or setting up the rules in the way he did; and hence, there was nothing he could have learned about the world in this process.

Thus, while experimenting involves negotiating actual constraints of the real world, modelling does not involve such constraints. This is an important disanalogy between experimenting and modelling, and puts into further doubt Mäki's claim that modellers isolate in ways similar to experimenters.

To conclude, models and experiments differ with respect to the processes of their construction. Experiments are constructed through consecutive steps of causal manipulation and control. In contrast, modelling does not commonly involve such a process. Indeed, some philosophers of science consider it a central characteristic of models that they are not constructed by such a process from any base.

6. Inseparability of Isolation and Idealization Q1

The criticism advanced in Sections 4 and 5 rests on Mäki's formal isolation account, which stresses the procedural aspect of isolation. This interpretation is supported by (i) Mäki's claim of an analogy between isolation and experiments, and (ii) intuitions derived from standard uses of the term isolation. Yet in recent publications, Mäki has disavowed such a procedural understanding of isolation. Instead, he now defends a minimal models-as-isolations position. According to this position, models are isolations because they 'close a system by neutralizing a number of factors that are not included in the isolated system' (Mäki 2009b, 31). What matters for the characterization of models as isolations are the model results, not the modelling processes.

This leads to the question what a characterization of models as the product of isolation implies, and what such a characterization would explicate. For the sake of argument, let's assume that the concerns of Sections 4 and 5 are irrelevant. Does Mäki's models-as-isolations account then still give a characterization of what models are in terms of the models' properties themselves? I argue that it does—but that unfortunately, this *positive* characterization is at odds with the reality of modelling.[7] I will argue for this claim in the remainder of this section. In the next section, I will then show that instead it should be seen as a *normative* characterization of models.

Idealization, in contrast to isolation, falsifies its objects: to idealize an object *X* means to (consciously) make false claims about it; to isolate the same object *X* means that one may make false claims about *X*'s environment, etc., but not about *X* itself. Idealization, in Mäki's account, is therefore a means of the isolation process, but it is not part of the isolation result. If idealizations and isolations were not clearly delineated, Mäki would have to admit that isolations also make false claims about the world, putting his defence of realism in doubt. He therefore insists that idealized representations are elements of the excluded field, and not the isolated one (Mäki 1992, 328). Against this claim, I will argue that the core of a model—what is reasonably assumed to be its isolating elements—is often and indeed usually contaminated with various idealizations.

The distinction between isolation and idealization quite intuitively applies to the interpretation of experimental results. Experiments take entities of the real world and examine them under controlled conditions. In successful experiments, the assumption goes, such controls will not affect the workings of the causal powers under investigation. In asset market experiments involving human agents, for example, we commonly assume that limiting the number of stocks, or changing their dividends, will leave unchanged the agents' ability to maximize utility under constraints. The experiment can thus examine the workings of this causal capacity under changing conditions. To apply these experimental results to the real world, the experimental conditions need not be identical to those of the real world. Rather, the experimental conditions may diverge from those of the target situation—i.e.

may be idealisations—because the successful experiment tells us about the operation of this power in isolation.

A model, however, isolates not by manipulating material entities, but by manipulating representations. Every part of the model is such a representation—i.e. both isolated as well as idealized features—which are pinned down through some form of description, be it mathematical, graphical, or through prose. Thus there is no prima facie reason to believe that a model correctly represents a certain mechanism. It all depends on the assumptions of that model, and on how the world really is.

Of course, if the model corresponds well with our ontological convictions, we may have good reasons to believe that this model indeed isolates a certain power or mechanism. For example, a physicist who models the motion of a falling body might disregard the effect or air resistance on that body. Depending on what kind of body is modelled, such a model may give a good approximation of the body's falling, or not. But independently of whether the model is true or false in this sense, the modeller might maintain that the model correctly isolates gravitational forces, and that the idealization of no air resistance has no effect on the operation of this isolated factor. The modeller can rest her argument on the conviction that gravity and air resistance indeed are two separate forces, and that theoretically neutralizing the effect of the second has no effect on the first.

It is a lot harder to make such an argument in the social sciences, because there are few ontological convictions to rest it on. As an example, take the completeness assumption of expected utility theory. Wherever modern microeconomics refers to the utility function, it implicitly assumes the completeness of the underlying preference ordering. The main micro-concepts, from standard demand functions through Nash equilibrium to social welfare functions, depend on completeness. Yet it is clear that actual preference orderings are often incomplete.

In Mäki's reading, completeness is an idealizing assumption to isolate the causal influence of preferences on choice. Preference incompleteness is a strongly disturbing causal factor in decision-making, as it adds features of information search, and judgement processes. The completeness assumption adds an excessively powerful mental capacity to the model in order to remove these extra real-world features from the model. This isolation allows investigating microeconomic phenomena in the absence of such disturbances (cf. Mäki 2009a, 78–79).

I have three reasons to doubt such a reading. First, completeness is constitutive of utility models in a way that e.g. disregarding air resistance in a model of falling bodies is not. Standard utility functions are not defined for incomplete preferences. Thus, the completeness assumption not only removes disturbing influences on a factor of interest, but partially constitutes the representational means by which this factor can be studied in the first place. Without completeness, we would loose many of the central micro-concepts, and we would not be able to express results as conditions on the utility function.[8] The whole representational framework of microeconomic theory would break down. This is not the case when modelling falling bodies. Neglecting air resistance is not to overlook something constitutive of our representation of falling bodies. Rather, it is more convenient, serves our scientific purposes better and makes solving equations possible or easier.

Second, because completeness is constitutive of utility models, there is no way to de-isolate the model. Demand functions, Nash equilibrium and social welfare functions are all based on utility functions that are continuous, continuously differentiable, and unique up to linear transformations. Most of the interesting results of theoretical microeconomics are conditional on properties of these utility functions, like being an increasing function, being quasi-concave, or being homogeneous of degree 1 (Mas-Colell, Whinston, and Green 1995, 46–50). If completeness were relaxed, the utility function is not defined any more, and we would have no way of knowing whether an (incomplete) utility function would satisfy these properties or not. Again, this differs notably from the physics example. It is imaginable that the effect of air resistance cannot be exactly specified, and that without disregarding it, we would not be able to solve the equations describing the motion of a body. But clearly, we have a good idea how such a de-isolated equation would look like, even if we cannot fully specify the disturbance's strength and direction. For an incomplete utility function, by contrast, we do not have such a similarly good idea at all.

Third, our ontological convictions are of little help in the case of incomplete utility functions. True, we have folk intuitions about preferences; but our (hedonistic) folk intuitions about utility are not reflected in the (ordinal) utility notion used in economic theory. This ordinal notion is a functional representation of complete preferences under uncertainty, and the properties of this measure (like being continuous or quasi-concave) cannot be understood through intuitive folk notions. Yet this makes it hard to understand what a complete utility function isolates from: we do not have intuitions about a separate power or force that preference incompleteness exudes. Thus, there is no way to separate the two functions, and hence no way to know whether the causal influence of preference incompleteness could in principle be isolated.[9] Instead, we are forced to admit that in our standard micro-models, completeness is inevitably linked with the derived results. It is an idealization of decision-making itself, necessary to make the study of it possible under the currently standard representational framework.

The preceding example is by no means exceptional. Philosophers of science have pointed out that models involve idealizing assumptions not only for the purpose of isolating certain objects, properties, or relations, but also for the purpose of handling modelling relations. Such idealizations can take the role of *derivation facilitators* (Alexandrova 2005). They facilitate derivation of model results (e.g. continuity of a distribution, differentiability of a function). Yet such a continuity or differentiability assumption idealizes elements not only of the excluded field but also of the isolated field. Or they may take the role of *tractability assumptions* (Hindriks 2006). A tractability assumption imposes simplifying conditions without which a problem cannot be solved. Yet again, it is often properties of elements of the isolated field that must be idealized in order for the problem of interest to remain tractable. Finally, Cartwright has argued that many model settings *over-constrain* the causal power of interest—they constrain it to a narrower set than those permitted by just the assumptions necessary to ensure that there are independent causes at work (Cartwright 2007).

To conclude, idealizations are not restricted to the excluded field of model elements. For a number of reasons, it is common to find the very core elements of models—those that the models are purportedly intended to isolate—to be idealized, too. **Q1** Thus, models not only nullify, neutralize, and purify, they are also augment, bias and pollute. This makes it doubtful that even a narrow focus on the result of modelling is adequately characterized by the isolation account.

7. Isolation as a Success Criterion

If modelling cannot be properly characterized as an isolation process, and models cannot be properly characterized as isolations, Mäki's framework may still be relevant for models as a criterion of success. In other words, while models may often not be isolations, what makes a model a *good* model is that it successfully isolates. This perspective needs to be specified further, but I believe that in it lies the central contribution of Mäki's framework to the discussion of models. **Q1**

A model may successfully isolate either in the substantial or the formal sense. In the substantial sense, a model is successful if it 'resembles the target while meeting the pragmatic constraints' (Mäki 2009b, 35), where the pragmatic aspects concern in what ways and for what purposes the resemblance is necessary. I have no doubt that if a model satisfied these properties, it would be a good model: it would make a significant contribution to our knowledge about the world.

Yet this substantial success criterion only clarifies the metaphysics of success. It does not give any indication of how how a model must be (in contrast to what the model–world relationship must be) in order to be a successful model. **Q1** As a success criterion, substantial isolation is thus more of philosophical than of methodological interest.

However, one finds a more ready criterion of success in a property of the model alone, and not a property of the model–world relationship: namely, Mäki's claim that 'the model must have a likely *capacity* to resemble' (Mäki 2009b, 32–33; my emphasis). Mäki does not explicate what he means by this capacity, and does not discuss the issue further. I suggest that we find a partial explication of this capacity in Mäki's formal account of isolation: the absence of idealizations in the isolated field is a crucial condition for the model's capacity to resemble.

If elements of the isolated field are idealized, they are intentionally falsified in ways that make them unlikely to resemble anything in the real world. Yet keeping idealizations out of the isolated field is hard, whether due to the need to keep the model tractable, or its results derivable, or due to the simple fact that modellers have to construct **Q1** something concrete, with some kind of structure, where they simply do not know what an appropriate structure may be. Nevertheless, modellers (and model users) are able to see whether elements of the isolated field are idealized: they simply need to check whether deliberately false assumptions are made about the object, property or relation to be isolated. Hence absence of idealizations in the isolated field is a necessary (if not sufficient) condition for isolating real factors. Many models fail to satisfy this condition, and the success or failure of the model to meet this condition is detectable. Q1 So it makes for an important success criterion if the goal is to construct models that have a capacity to resemble.

While the separation of isolation and idealization constitutes an important part of a sufficient criterion for the success of models, it is not a necessary condition. Mary S. Morgan and Margaret Morrison (1999), Robert Sugden (2000), Mauricio Suárez (2004), Tarja Knuuttila (2008), and Till Grüne-Yanoff (2009) have all offered accounts of how models can be epistemically successful, *without* having to isolate parts of the world. Given these alternative accounts, one can learn from models that are not isolations. Formal isolation is not a necessary criterion for the success of models. Rather, it is a success criterion only for those models that are intended to isolate.

8. Conclusion

In this article, I have argued against Mäki's attempt to characterize models as isolations. First, I showed that modelling differs from the process of isolation. This served to distinguish models from isolations according to many standard interpretations of isolation, and shows that Mäki must develop his non-procedural notion further if he wants to defend his models-as-isolations account. Second, I argued that models often make idealizing assumptions about the very factors they are intended to represent or investigate. Because the isolation notion centrally relies on the distinction between isolation and idealization, this served to show that models often create an augmenting, biasing and polluting environment, and do not offer a 'neutral', isolating setting. Finally, I proposed that Mäki's account of isolation should be seen as a sufficient (albeit not necessary) criterion of model success. This leaves an important role for isolation. But it insists on isolation being just one method among many – in modelling just as in other forms of theorizing. It therefore cannot be a universal characterization of models. Modelling is not isolating, and models are not isolations.

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Notes

[1] Because there are material models, theoretical isolation may consist in the manipulation of a real entity that functions as a representation. In most social sciences, however, material models are insignificant.

[2] Note that when speaking about an 'isolated essence', Mäki refers to the way the world is partitioned, not about the way representations are isolated. Thus, he acknowledges the substantial ontological claim he is making.

[3] A related distinction can be found in McMullin (1985, 255). He distinguishes between 'construct idealizations', where the simplification is worked on the conceptual representation of the object, and 'causal idealization', where the simplification is worked on the problem situation itself.

[4] For an in-depth comparison of Mäki's three concepts of isolation, see Grüne-Yanoff (forthcoming).

[5] In personal communication, Mäki clarified that there is no conceptual difference between his term 'resemblance' and the more commonly used term 'similarity'.

[6] This lack of reality base is often diagnosed in economic models. About Lucas's 'model economy', for example, Knuutilla says: 'the assumptions made are patently artificial in the sense that it is difficult to imagine how they could have been drawn from the economic reality' (Knuutilla 2009, 63).

[7] There is, however, an alternative and much weaker reading. The above quotation may be read as a merely intentional characterization: modellers intend to close a system. Such a characterization would point not to a property of the model, but merely to a property of the modeller. This would jeopardize any intersubjective criterion for the explication or assessment of models and modelling practice. I think it is worthwhile to resist such a reading, and ask for a stronger characterization of models and modelling. I also think that Mäki would want to resist such a reading.

[8] Recently, axiomatic representations of incomplete preferences as *sets* of utility functions have been developed (Dubra, Maccheroni, and Ok 2004). However, it is unclear what sort of demand functions, equilibra concepts or welfare functions could be derived from such representations; it is only clear that these notions would be significantly different from the existing ones, and that the properties defined for unique functions do not apply to sets of functions.

[9] In particular, robustness analysis would not be of help here. Analysing the robustness of model results under varying assumptions requires varying these assumptions. But as I argued, completeness is constitutive of many other microassumptions, and hence cannot easily be varied without affecting the whole set of model assumptions.

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