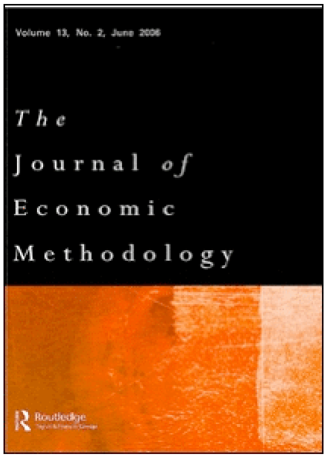
This article was downloaded by:[University of Helsinki]
On: 14 August 2007
Access Details: [subscription number 772113072]
Publisher: Routledge
Informa Ltd Registered in England and Wales Registered Number: 1072954
Registered office: Mortimer House, 37-41 Mortimer Street, London W1T 3JH, UK



Journal of Economic Methodology
Publication details, including instructions for authors and subscription information:
http://www.informaworld.com/smpp/title~content=t713704064

Models are experiments, experiments are models

Uskali Mäki [a]
[a] Erasmus University of Rotterdam,

Online Publication Date: 01 June 2005
To cite this Article: Mäki, Uskali (2005) 'Models are experiments, experiments are models', Journal of Economic Methodology, 12:2, 303 - 315
To link to this article: DOI: 10.1080/13501780500086255
URL: http://dx.doi.org/10.1080/13501780500086255

PLEASE SCROLL DOWN FOR ARTICLE

Full terms and conditions of use: http://www.informaworld.com/terms-and-conditions-of-access.pdf

This article maybe used for research, teaching and private study purposes. Any substantial or systematic reproduction, re-distribution, re-selling, loan or sub-licensing, systematic supply or distribution in any form to anyone is expressly forbidden.

The publisher does not give any warranty express or implied or make any representation that the contents will be complete or accurate or up to date. The accuracy of any instructions, formulae and drug doses should be independently verified with primary sources. The publisher shall not be liable for any loss, actions, claims, proceedings, demand or costs or damages whatsoever or howsoever caused arising directly or indirectly in connection with or arising out of the use of this material.

© Taylor and Francis 2007

Journal of Economic Methodology 12:2, 303–315 June 2005

Routledge
Taylor & Francis Group

Models are experiments, experiments are models

Uskali Mäki

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

Abstract A model is a representation of something beyond itself in the sense of being used as a representative of that something, and in prompting questions of resemblance between the model and that something. Models are substitute systems that are directly examined in order to indirectly acquire information about their target systems. An experiment is an arrangement seeking to isolate a fragment of the world by controlling for causally relevant things outside that fragment. It is suggested that many theoretical models are ('thought') experiments, and that many ordinary experiments are ('material') models. The major difference between the two is that the controls effecting the required isolation are based on material manipulations in one case, and on assumptions in the other.

Keywords: model, experiment, representation, isolation, control, assumptions, testing

1 INTRODUCTION

Models and experiments, as used in a variety of research areas, interact in many ways, they reciprocally inform and constrain one another. The following observations do not address the issues related to these interactions. They rather focus on the very concepts of model and experiment in relation to one another. The questions are: What are models? What are experiments? The answers are constrained by the strategy to consider them in relation to each other. What do we learn about the concept of model and that of experiment by viewing them from the perspective provided by the other – as well as by their relationship to the real world? A rather weak and unsurprising – and correct – answer would be to suggest that they are in many ways similar, that there are important positive analogies between them. A stronger and perhaps somewhat surprising answer will be to suggest a closer connection: models are experiments, and experiments are models. In order to ground such a claim, we need to examine the two concepts a bit more closely and to identify two more general background concepts – those of representation and isolation.

Journal of Economic Methodology ISSN 1350-178X print/ISSN 1469-9427 online
© 2005 Taylor & Francis Group Ltd http://www.tandf.co.uk/journals
DOI: 10.1080/13501780500086255

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

2 MODELS AS REPRESENTATIONS AND ISOLATIONS

For the present purposes, I take models to be representations (of theories, of data, or of reality, for example[1]). I take representation to have two major aspects: the representative aspect and the resemblance aspect (see Mäki 2001). Models are 'representatives' of what they represent: they represent by adopting the role of representative. Another way of putting this is to say that models serve as 'substitute systems' of the target systems they represent. They are substitute systems in the sense that one does not directly examine the target systems, one rather focuses on the properties and behaviour of the representatives as substitutes of the targets. The epistemic point of this activity is that the properties of such substitute or surrogate systems are directly examined in order to indirectly acquire information about the properties of the systems they represent. Animal subjects are representatives that are examined with the purpose of learning about human beings. Large systems of mathematical equations are representatives that are examined in order to learn about the Big Bang.

The reasons for employing and examining substitute systems are various, including ethical (using animals in medical research instead of human subjects since the latter would be ethically objectionable), economic (examining the target system directly would be too expensive), and epistemic (the target systems are inaccessible in full since they are too small, too large, too far away in space or time or too complex). A familiar way of putting one major idea is to say that the systems in the real world are characteristically all too complex to be tractable targets for direct examination, therefore they are represented by much simpler model systems. It is within, and in terms of, such simple representatives that questions about the complex real system can be recast so as to make them tractable and answerable. 'Results' are inferred within model systems, and this is supposed to illuminate some facts about the real world. Theoretical practice in economics is in line with this notion of representation: 'let us examine what happens in this model' thereby hoping to gain some insight into the 'whys' and 'hows' of what happens in the real world.

Not just any arbitrary substitute systems will successfully serve the purpose of learning about those target systems of which they are representatives. It is also required that models represent by resembling those real systems in certain respects and to certain degrees. Thanks to the resemblance, the direct study of models may succeed in indirectly providing information about what they represent. The resemblance aspect of representation is far more contested and difficult to deal with than the representative aspect. While medical researchers may often regard animals as adequate representatives of human beings, major problems arise amongst economists and other social scientists as to whether there is sufficient resemblance between animal behaviour and human behaviour to justify

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

using the former as representatives of the latter. In economics, the main methodological issue for the last two centuries has been whether the resemblance between theoretical models and reality has been sufficiently close. It is relatively easy to construct tractable and convenient substitute systems, models as representatives, and to examine them so as to generate definite solutions, but it is somewhat harder to do this while meeting the constraint of resemblance. There is a long tradition of blaming economics for failing to meet the latter requirement and for focusing its efforts on building theoretical model worlds far removed from the real world – and then for conflating those substitute systems with the far more complex real systems.

I want to suggest that these criticisms and controversies, and more generally the propensity or capacity to prompt questions of resemblance, may play a criterial role in identifying something as a model. Whether something is a representative of what it represents, whether it is a model as representative, is often revealed by whether it gives rise to questions or issues of resemblance. Obviously, resemblance issues should not arise in cases where the thing in question is not intended as a representative of some other thing but something unrelated to it, and where the two things are identical or close to identical. Considerations of resemblance presuppose that a system is employed as a representative, but on the other hand those considerations may serve as a criterion that helps identify a system as having the status of a representative.[2]

Thus models involve a semantic aspect (characterised by the notions of representation and resemblance) and an epistemic aspect (characterised by the aim of indirectly acquiring information about the target system by examining a representative substitute system). In addition, models also essentially involve pragmatics, dealing with their functions. Models conceived as representations can be considered four placed: Model M is an entity used by agent A to represent target system S for purpose P. The inclusion of purpose or function suggests pragmatic constraints on the required respects and degrees of resemblance. The desired sort of resemblance is a function of the uses to which models are put, the purposes they are supposed to serve.

These ideas about the general characteristics of models imply that the ontology of models must be very flexible (see Mäki 2001). Almost anything can serve as a model of almost anything else. Models may be made of almost any stuff, as it were. They may be physical objects, diagrams, drawings, verbal statements, systems of mathematical equations, abstract objects and so forth. With a broad enough conception of the world, we may say that bits of the world (models) are designed and used by other bits of the world (modellers) to represent further bits of the world (modelled). This flexibility is among the preconditions of the models=experiments equation to be argued.

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

Another precondition of the models=experiments equation is based on recognising a prominent feature of conventional experiments: they involve manipulation. An experimental design typically suggests ways of manipulating 'other things', a number of potentially influential variables of a larger system so as to neutralise them, to prevent them from making an impact on what forces are in play and what happens in experimentally controlled situations. In other words, 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. An isolated system is a simple and controlled mini-world in contrast to the complex and uncontrolled maxi-world. The isolation of such controlled mini-worlds is accomplished in order to utilise and enhance the capacity of such experimental systems to serve as epistemically successful substitute systems, as resembling representatives. The equation models=experiments is supposed to hold precisely for such manipulable and manipulated systems. The equation holds for representations which involve the representative and resemblance aspects and which are subject to manipulations that effect isolations.

3 EXPERIMENTS ARE MODELS

Let us first approach the equation by examining the notion of experimental system in the conventional sense, denoting an arrangement of things typically in a laboratory. Such experimental systems, having been first designed and then built, are systems that are not, at the end, examined for their own sake. They are artificially designed and constructed substitute systems, controlled mini-worlds that are directly examined in order to indirectly generate information about the uncontrolled maxi-world outside the laboratory – such as economic systems and behaviour 'in the wild' (they can also be used for generating information about theories about such systems and behaviours). Given the account of models given above, this means such experimental systems are representatives of some real non-experimental systems: they are material models of aspects of the rest of the world. This is not to say experimental set-ups are unreal other than in the sense of being artificial, man-made. They are bits-in-the-world and at the same time bits-about-the-world. As Plott suggests (see also Wilde 1980; Smith 1982):

> The relevance of experimental methods rests on the proposition that laboratory markets are 'real' markets in the sense that principles of economics apply there as well as elsewhere. Real people pursue real profits within the context of real rules. The simplicity of laboratory markets in comparison with naturally occurring markets must not be confused with questions about their reality as markets (Plott 1982: 1520).

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

In order to construct a substitute system, the experimenter modifies and purifies some relevant chunks of the world. Experimental models isolate fragments of the world by way of controlling for the rest of it – or more accurately: for other potentially causally significant things - by causally manipulating those other things, thereby preventing them from interfering with what causal forces operate and what happens in those substitute systems.

Vernon Smith's version of experimental economics proposes an elaborate vocabulary and a set of conditions that highlight the ideas of isolation and control. In his vocabulary, theory (or a theoretical model) depicts three components – environment, institution, and behaviour – and yields results concerning behaviour given the environment and the institution. In testing such a model against field data, the environment and the institution are not controlled, thus we have a composite test of the theory's assumptions about all three components jointly. Failures and successes in such tests are not conclusive about any of the three components. In an experimental set up, on the other hand, one seeks to control the environment and the institution and proceeds to test assumptions about behaviour (Smith 1989: 154). The relevant connections between the three components are isolated from possible disturbances by employing various controls. On Smith's early account, four conditions must be met to ascertain that some of the potentially causally most significant things are neutralised (Smith 1982): non-satiation of preferences, saliency or lucidity of the connection between rewards and choices, dominance of payoffs in the laboratory over other possible costs or benefits, and privacy of choices. By way of manipulating the situation so that these conditions hold, relatively effective material isolations are hoped to be forthcoming.

Experimental economists with a more behavioural bent may question the details of these conditions, so as to enable experiments to generate information about individual preferences and behaviour that may be more context- and procedure-dependent (see Loomes 1999). But even if one's experiments were not so designed as to control for the context and procedure in order to neutralise their influence, obviously many things have to be controlled for to effect an isolation of the major processes of interest that shape individual preferences and behaviour. The disputes within experimental economics are concerned not with whether to isolate, but with what to isolate and what conditions to control (see Binmore 1999; Loewenstein 1999).

Experiments operate with substitute systems that are manipulated for certain purposes. For many purposes, the issue of resemblance between such material models and what they are representatives of 'in the wild' cannot be escaped. Do the conclusions of the examination of the substitute systems also apply to the target system? Are the causal properties discovered in isolated mini-worlds transferable to non-isolated maxi-worlds? The resemblance aspect of representation is a particularly pressing issue in experimental economics: do experimental systems resemble real economic systems 'in the wild' in sufficient respects and to sufficient degrees, given the purposes of experimentation? This is the issue of 'external validity' or 'parallelism' (see Loewenstein 1999; Guala 1999, 2002; Starmer 1999; Siakantaris 2000). This is another feature suggesting that experimental systems qualify as models: they meet the criterion of giving rise to the question of resemblance between the representative and the represented.

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

4 MODELS ARE EXPERIMENTS

Models are the laboratories of economic theorists. This is a claim most economic theorists will accept, and many of them have explicitly made it. Just as laboratory scientists design and examine the artificial worlds of experimental situations in their laboratories, economic theorists design and examine the artificial worlds of their theoretical models. Lucas draws this analogy explicitly:

> One of the functions of theoretical economics is to provide fully articulated, artificial economic systems that can serve as laboratories in which policies … can be tested out … (Lucas 1980: 696).[3]

I am not suggesting that all sorts of models are to be viewed as experiments. But as implied by what I said earlier, there is a species of models that possess the key characteristics of experiments: those based on theoretical isolation (see Mäki 1992). In earlier work, I have adopted the vocabulary of 'isolation' using it of processes and products of theoretical inquiry, partly inspired by the analogy with material isolation as in experimentation (and partly also by Marshall's early use of the term to characterise partial analysis). The expression I have used, the 'method of isolation' is intended to generalise, to capture what is shared by conventional experimental isolation and theoretical isolation.

Consider material experimentation as based on causally isolating fragments of the world from the rest of it so as to examine the properties of those fragments free from complications arising from the involvement of the rest of the world. The analogy with theoretical modelling is obvious: 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. Assumptions play a key role in the construction of theoretical models as substitute systems. Such idealising assumptions, if interpreted as statements about the real world, are characteristically false. Yet they are necessary for effecting the required theoretical isolations. Unrealistic assumptions are the indispensable tools of the experimental theorist.

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

I have called models based on theoretical isolation 'thought experiments' in analogy to 'material experiments'. This seems justified given the strong structural similarities between material experimentation and theoretical modelling. 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 1992).[4] Mary Morgan has pursued similar ideas and suggested further classifications (see Morgan 2002a, 2002b, and this issue).

Given this difference in the means of controls, it is not surprising that theoretical models are capable of effecting isolations more stringently than material models. In theoretical modelling, one simply assumes away all disturbances and complications. As Vernon Smith rightly notes, 'the abstractions of the laboratory are orders of magnitude smaller than those of economic theory …' (Smith 1982: 936). In thought experiments, the controls can be made as tight as one wishes, whereas material experiments are forced to leave many of the possible interferences uncontrolled or just weakly controlled (failure in causally effective controls can then be compensated for by way of assumptions).

The issue of resemblance is the hottest methodological issue in and about theoretical economics. Models and their assumptions are being criticised for being unrealistic and defended as sufficiently realistic or inconsequentially unrealistic. (For the very complex issues of truth and falsehood of models and their assumptions, see Musgrave 1981; Mäki 2000, 2004c; see also Mäki 2004a, 2004b.) The traditional complaint is that the representatives do not sufficiently resemble what they represent, and that the gap between the two is ignored by treating the substitute systems as if they were the real system. Lucas is aware of these issues and guards against this latter error:

> To serve this function [as a laboratory] well, it is essential that the artificial 'model' economy be distinguished as sharply as possible in discussion from actual economies. Insofar as there is confusion between statements of opinion as to the way we believe actual economies would react to particular policies and statements of verifiable fact as to how the model will react, the theory is not being effectively used to help us to see which opinions about the behavior of actual economies are accurate and which are not (Lucas 1980: 696).

5 TESTING MODELS IN TERMS OF MODELS

It is often thought that a major, if not the only, purpose of economic experiments is to test economic theories. Of course, experiments serve other purposes as well, but let us briefly focus on the testing function. An interesting implication of the suggestions above is that in testing theories in

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

terms of laboratory experiments, one tests in terms of a hierarchy of models. One tests a theory by testing its theoretical models that are its representatives with a sufficient resemblance between them and the theory (this assumes a distinction between theory and theoretical model, not always made by economists). Testing those theoretical models may then take place in terms of suitable material experiments. In this part of the hierarchy one tests (theoretical) models in terms of other (experimental) models. But this model-model structure of testing is not restricted here. For such testing purposes, material experiments are supposed to generate data that serve as relevant evidence. But in order to employ such data as providing relevant evidence, one needs to have models of those data. Models of data are representations that reduce the richness and complexity of experiential materials into simpler and manageable portions of information (models of data are unavoidable in all contexts where empirical data are being systematically used for epistemic purposes). All this means that in testing theories in terms of experiments one actually tests them in terms of at least three layers of models. It is a matter of testing representatives by other representatives, while at each level trying to make sure that the resemblance between the representatives and what they represent is sufficiently tight. Since it is never perfectly tight, this introduces slack to the structure of testing.

This relates to what was said above about the tightness of controls: in theoretical model experiments the controls can be made as tight as one wishes just by assuming them to be so, while in material experiments many of the possible interferences are left uncontrolled or just weakly controlled. An unsurprising implication of this slack between the two – fortified by the role of data models - is that the testing of theoretical models in terms of material experiments is bound to be imperfect.

Nevertheless, one may take testing theories to be the sole or primary role of experiments. On one account, material experiments may not be taken as simple models of complex real world systems in the wild maxi-world, but rather as models of theoretical models. Here is a lucid expression of this idea:

> Once models, as opposed to economies, became the focus of research the simplicity of an experiment and perhaps even the absence of features of more complicated economies became an asset. The experiment should be judged by the lessons it teaches about theory and not by its similarity with what nature might happen to have created (Plott 1991: 906).

To function in this way in relation to theory, the causal controls of the material experiment must attempt to imitate and implement the assumed controls in theoretical models (but they can only imperfectly succeed in this as indicated above). Thus the material experiment becomes a model of the theoretical model (rather than, or in addition to, a model of some real

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

system). It is explicit in Plott's statement that experimental models are not to be assessed as resembling representatives of the uncontrolled maxi-world. On this account, then, the issue of external validity of experiments does not arise at all – or it may arise at most in the narrow form of transferability of predictive success from the controlled mini-worlds to the uncontrolled maxi-world (see Wilde 1980; Smith 1982; Starmer 1999).

Suppose we want to have economic models – theoretical models, experimental models, data models – that help us have epistemic access to some relevant facts about the uncontrolled mental and social maxi-world. We must pose the question of what these theoretical and material representations are – or are intended to be – about: what are they representatives of, and exactly what kinds of realities might they resemble? 'Aboutness' is an essential condition for something to qualify as a representation, but this does not yet tell anything about the sort of thing that any given representation is intended to be about. This is a crucial issue for debates around 'external validity' in experimental economics.

As for the representative aspect, we are presupposing that both theoretical and experimental models are representatives of the economy in the wild – that impure mixture of all sorts of ingredients and influences where the conditions of isolation do not hold (or hold at most partially, approximately, temporarily). But what about that wild world are the models supposed to resemble? In the isolated mini-worlds of theoretical and experimental models, things are shown to happen in a more or less orderly fashion. Is it these orderly happenings that are claimed to resemble what happens in the non-isolated maxi-worlds, implying that the models can be expected to exhibit good predictive performance also in non-isolated circumstances? In many cases this is not a very good idea. Another possibility would be to suggest the counterfactual idea that the happenings in the model worlds resemble what would happen in the real world if only those isolations were implemented. Yet another possibility – one that I am attracted to – is to conjecture that, at least for some purposes, the causal powers and mechanisms isolated and identified by the models resemble those that function outside those theoretical and material models (cf. Sugden 2002). Experimentation alone is insufficient for testing such conjectures for their truth and falsity. But there are models that again do play a role in judging these options: the options involve metaphysical conjectures, and these conjectures are described in terms of metaphysical models of the basic constitution of the social world.[5]

6 CONCLUSION

I have suggested that models=experiments insofar as theoretical models and material experiments share the characteristics of representations that are manipulated in order to effect isolations. In this regard they share the

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

general characteristics of models as well as the general characteristics of experimentation. There are two major differences. One is qualitative: the manipulations in one case are causal, while in the other, they are theoretical. The other is one of degree: theoretical models are capable of providing much tighter isolations than experimental models. This is possible thanks to the qualitative difference: those tighter isolations are easy to implement as they only have to be assumed.

The foregoing has suggested that theoretical models are experiments and that material experiments are models. To justify this thought I did not need much in the way of conceptual preliminaries: just the notion of representation as involving the representative and resemblance aspects, and the observation that experiments characteristically involve isolation by manipulation. The equation models=experiments is not suggested to hold for all specifications of the two concepts, that of model and that of experiments. The equation rather boils down to two more specific claims: many theoretical models=experiments, and many material experiments=models.

As implied already, we can choose to let all this shape our vocabulary. We can talk about two kinds of models: theoretical and material models; about two kinds of experiments: theoretical experiments (or thought experiments) and material experiments; and more generally about two broad classes of representations: theoretical and material representations. When talking about economic experiments, we need to be somewhat more relaxed about the meanings of 'material' than when referring to experiments in physics or chemistry. The extension of 'material' must be taken more broadly than just pertaining to matters of physical matter.

There is a broader upshot that I have not here set out to investigate systematically. Recognising the indispensable role of further kinds of models – of which I have mentioned data models and metaphysical models - we cannot escape the observation that in scientific practice, economics being no exception, models are everywhere, and they appear in very many sizes and shapes and colours and materials and roles. Models definitely deserve the attention they have started to receive from methodologists.

Uskali Mäki
Erasmus University of Rotterdam
umaki@fwb.eur.nl

ACKNOWLEDGEMENTS

Earlier versions have been presented at the workshop on 'The Role of Experimental Methods in Economics' at the University of Nottingham, 13–14 September 2003; and at the INEM Conference in Amsterdam, 10–11 August 2004. Materials from the paper have also been presented at the

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

workshop on philosophy and economics at the University of Pisa, 2 December 2004. I am grateful to the three audiences for inspiring reactions, and in particular to Mary Morgan and Robert Sugden for exceptionally extensive written comments.

NOTES

1 The linguistic practice of economists often does not distinguish between 'theory' and 'model' but for many purposes it is useful to think of models not only as representing the world but also as representing theories, as their reduced or enlarged representatives. Models in this sense 'mediate' between theories and the world or the data (see Morgan and Morrison 1999). My focus here is on models as representations of the world.

2 One might also think, as Robert Sugden has suggested to me in reaction to an earlier version of this paper, that it is sufficient for something to qualify as a model if it is intended or treated as a representative that resembles its target: no issues of resemblance need arise, it is sufficient if the designer or user of a model intends it to resemble. I can think of two lines of response. First, consider a situation where the designer or user of a model is a radical instrumentalist: the model is definitely not supposed to resemble any real system in any sense. Whether or not there really are such instrumentalists around, the implication would be that there is no model in this situation. For there to be a model at all, it should be possible at least to raise questions of resemblance, and these questions may be posed by the model users themselves or by other members of the relevant community of inquirers. Note that this formulation does not require a debate over issues of resemblance (there may be a consensus or a tacit and suppressed disagreement on this), and that questions of resemblance do not need to be actually raised (they may be just potentially raised). Nevertheless, this thought reveals a slight realist bias in my account of models. Second, resemblance is not an unambiguous and uniform notion: there are many varieties of resemblance as things may resemble one another in various respects and degrees. It often happens that the designer or user of a model intends it to resemble the target system in one way, while some other members of the relevant community may wish it to resemble in some other way. This may give rise to questions about, and perhaps debates over, resemblance. Thus questions, criticisms, and controversies are compatible with the model user's intention to go for resembling representatives.

3 The full quote refers to the relatively low costs of theoretical modelling as one of its justifications: 'One of the functions of theoretical economics is to provide fully articulated, artificial economic systems that can serve as laboratories in which policies that would be prohibitively expensive to experiment with in actual economies can be tested out at much lower cost'.

4 An obvious qualification is needed. This is because the expression 'thought experiment' is often used in a more restricted sense, denoting mental operations that deal with particular cases, whereas this restriction is not a condition for something to qualify as theoretical modelling. A thought experiment in this narrow sense is put in terms of concrete illustrations. This makes the analogy between such concrete thought experiments with ordinary material experiments quite strong for the simple reason that the latter are also conducted in terms of concrete particularities, in particular space-time situations dealing with concrete materials and set-ups. A negative analogy is that the idea of control is not

among the key elements in the narrow notion of thought experiment. The notion of thought experiment that I have been employing is not necessarily concrete in such a way, and it essentially involves the element of control. All that is needed for something to qualify as a thought experiment in my sense is that it is a mental operation of representation, isolation and manipulation, employing a theoretical framework, and having the generic structure of experiment.

5 The very idea of model isolation (whether theoretical or experimental) is metaphysically sufficiently neutral to allow for all three metaphysical models.

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

REFERENCES

Binmore, K. (1999) 'Why experiment in economics?', *Economic Journal* 109(453): F16–F24.

Guala, F. (1999) 'The problem of external validity (or 'parallelism') in experimental economics', *Social Science Information* 38: 555–73.

Guala, F. (2002) 'On the scope of experiments in economics: comments on Siakantaris', *Cambridge Journal of Economics* 26: 261–67.

Loewenstein, G. (1999) 'Experimental economics from the vantage point of behavioural economics', *Economic Journal* 109(453): F25–F34.

Loomes, G. (1999) 'Some lessons from past experiments and some challenges for the future', *Economic Journal* 109(453): F35–F45.

Lucas, R. Jr. (1980) 'Methods and problems in business cycle theory', *Journal of Money, Credit and Banking* 12: 696–715.

Mäki, U. (1992) 'On the method of isolation in economics', *Poznan Studies in the Philosophy of the Sciences and the Humanities* 26: 19–54.

Mäki, U. (2000) 'Kinds of assumptions and their truth: Shaking an untwisted F-twist', *Kyklos* 53: 303–22.

Mäki, U. (2001) 'Models', in, *The International Encyclopedia of Social and Behavioral Sciences*. Vol.5, Oxford: Elsevier, p. 9931–37.

Mäki, U. (2004a) 'Theoretical isolation and explanatory progress: transaction cost economics and the dynamics of dispute', *Cambridge Journal of Economics* 28(3), May: 319–46.

Mäki, U. (2004b) 'Realism and the nature of theory: A lesson from J.H. von Thünen for economists and geographers', *Environment and Planning A* 36: 1719–36.

Mäki, U. (2004c) 'Some truths about truth for economists, their critics and clients', in P. Mooslechner, H. Schuberth and M. Schürtz (eds) *Economic Policy-Making under Uncertainty: The Role of Truth and Accountability in Policy Advice*, Cheltenham: Edward Elgar, pp. 9–39.

Morgan, M. (2002a) 'Model experiments and models in experiments', in L. Magnani and N.J. Nersessian (eds) *Model-Based Reasoning: Science, Technology, Values*, Dordrecht: Kluwer, pp. 41–58.

Morgan, M. (2002b) 'Models, stories, and the economic world', in U. Mäki (ed.) *Fact and Fiction in Economics. Models, Realism, and Social Construction*, Cambridge: Cambridge University Press, pp. 178–201.

Morgan, M. and Morrison, M. (eds) (1999) *Models as Mediators*, Cambridge: Cambridge University Press.

Musgrave, A. (1981) ''Unreal assumptions' in economic theory: The F-twist untwisted', *Kyklos* 34: 377–87.

Plott, C. (1982) Industrial organisation theory and experimental economics', *Journal of Economic Literature* 20: 1485–527.

Downloaded By: [University of Helsinki] At: 08:59 14 August 2007

Plott, C. (1991) 'Will economics become an experimental science?', *Southern Economic Journal* 57: 901–11.

Siakantaris, N. (2000) 'Experimental economics under the microscope', *Cambridge Journal of Economics* 24: 267–81.

Smith, V. (1982) 'Microeconomic systems as an experimental science', *American Economic Review* 72: 923–55.

Smith, V. (1989) 'Theory, experiment and economics', *Journal of Economic Perspectives* 3: 151–69.

Starmer, C. (1999) 'Experiments in economics: should we trust the dismal scientists in white coats?', *Journal of Economic Methodology* 6: 1–30.

Sugden, R. (2002) 'Credible worlds. The status of theoretical models in economics', in U. Mäki (ed.) *Fact and Fiction in Economics. Models, Realism, and Social Construction*, Cambridge: Cambridge University Press, pp. 107–36.

Wilde, L.L. (1982) 'On the use of laboratory experiments in economics', in J. Pitt (ed.) *Philosophy in Economics*, Dordrecht: Reidel, pp. 137–48.