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# Models and Fictions in (Micro-) Economics

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# Models and Fictions in (Micro-)Economics\*

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**Abstract** How can an argument that is based on assumptions known to be false deliver any insightful conclusions let alone be used for policy recommendations? Over the years, a variety of concerns regarding (micro-)economic modelling and its relevance for real life have been expressed along these lines. Adding to this methodological discussion, I take seriously the recurrent comparison of economic models with literary fiction (fables, metaphors, parables,...) and argue that many of the concerns expressed can be alleviated in a coherent picture if one adopts the fiction view of models proposed by Frigg (2010a,b). In particular, I argue how adopting such a view not only opens a way for meaningful comparisons of economic models with reality but also relieves the pressure on assumptions to be empirically verifiable. Moreover, the argument suggests a distinctive role for strong mathematical theories such as expected utility theory (setting limits to the fictional world to be imagined), the often extensive story telling in economic modelling (adapting the model to a context and suggesting comparisons with reality) as well as for empirical studies putting economic modelling to the test (exploring properties of the real world and how they relate to properties of the fictional model-world). Thus, suggesting a coherent framework in which various aspects of the discussion can be accommodated, I hope to add some valuable structure to the ongoing debate about how to interpret (micro-)economic modelling.

*Keywords:* Fictions, Modelling, Microeconomics, Story Telling

*JEL codes:* B40, B41

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# 1 Introduction

How come that (micro-)economic theorists are often very confident that there are indeed lessons to be learned from their models or even give policy advice based on these models, despite the fact that these models are based on assumptions which are known to be false?<sup>1</sup> While not of primary concern for the daily research of most economists, the question about the realism of economic models of course touches on a vital core of the science. Therefore, it is not surprising that – in one or the other formulation – methodological concerns of this form have received considerable attention in both economics and philosophy, especially in recent years (e.g. Mill, 1843;<sup>2</sup> Friedman, 1953;<sup>2</sup> Gibbard and Varian, 1978; Sugden, 2000; Rubinstein, 2001; Schelling, 2006; Gilboa, Postelwaite, Samuelson and Schmeidler, 2014; Hausman, 1992, Mäki, 1992; Cartwright, 2009).

While I defer a more extensive review of the discussion to Section 2, it is worth noting that the approaches taken as well as the conclusions drawn differ in various ways. The discussion in philosophy, for instance, essentially tries to describe and eventually make sense of the scientific practice in economics, suggesting justifiable ways to understand economic models (e.g. Hausman, 1992; Mäki, 1992; Sugden, 2000;<sup>3</sup> Cartwright, 2009). By contrast, views expressed by economists themselves often express a stronger overall skepticism. The general scepticism notwithstanding, the conclusions reached in the more internal debate range all the way from rather optimistic ones, viewing knowledge about economic phenomena as generally case-based and models as one type of cases (e.g. Gilboa et al., 2014), over pragmatic views, judging models essentially by their predictive power (e.g. Friedman, 1953<sup>2</sup>), to rather pessimistic assessments essentially doubting the (direct) applicability of economic theory to daily life problems and suggesting to see it as a part of culture rather than science (e.g. Rubinstein, 2006).

Despite the differences in perspectives taken and the disagreements in the conclusions reached, however, it is possible to identify some common themes in the discussion. In fact, from my reading of the literature, it appears that what figures

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<sup>1</sup>Admittedly, not all micro-economists are equally confident. Ariel Rubinstein, for example, is one of the most prominent skeptics who has repeatedly expressed his concerns (e.g. Rubinstein, 2001, 2006). More generally referring to economic modelling, Axel Leijonhuvud (1973, p. 328) in his famous and admirable study “Life Among the Econ” mentions it as a fact that *most of these “modls” seem to be of little or no practical use.*

<sup>2</sup>As the discussion about models is comparably new, it should be mentioned that Mill (1843) and Friedman (1953), of course, refer more generally to theories.

<sup>3</sup>Robert Sugden, of course, is an economist. His contribution, however, is much closer to the philosophic discussion, which is why I mention him here.

prominently in the discussion are **(a)** comparisons of models with items of *literary fiction* (e.g. McCloskey, 1985; Sugden, 2000; Rubinstein, 2006; Cartwright, 2009), **(b)** a strong emphasis of the relevance of *background knowledge* in interpreting and judging theoretical models, where the background knowledge is usually implicitly referred to in the story-telling around the model (e.g. Rubinstein, 2006; Cartwright, 2008; Gilboa et al., 2014),<sup>4</sup> and **(c)** references to tendencies and/or isolated causal mechanisms, albeit in different guises and with varying strength (e.g. Mill, 1853; Hausman, 1992; Mäki, 1992; Cartwright, 2009). Last but not least, an additional recurrent theme, which arguably is more prominent in the internal discussion in economics, is **(d)** the relation between economic modelling (or theorising) and reality and empirical testing (e.g. Rubinstein, 2001; Gilboa et al., 2014).

As pointed out nicely by Sugden (2000), however, actual modelling is often not about generating precise predictions, thereby calling into question the instrumental perspective which focuses on predictive power (e.g. Friedman, 1953). Moreover, it often relies on assumptions which are not even approximately true, which makes it difficult to generally accept the isolation / inexact-deductions account (e.g. Hausman, 1992; Mäki, 1992); and the reference to fictional entities (e.g. Gibbard and Varian, 1978; McCloskey, 1983) still has to answer the question how something fictional can represent reality in a (scientifically) meaningful way (cf. Sugden, 2000). The last point, in fact, is particularly troublesome if inductive inferences, as often made, are to be justified. Sugden himself suggests to view models as credible / possible worlds, which due to their closeness to reality justify inductive inferences.<sup>5</sup> In fact, he does so despite his pointing to the decidedly unrealistic character of many common (auxiliary) assumptions, which rather suggests that the model worlds themselves may not be possible. Yet, even if we tentatively agree to view models as imaginary but credible / possible worlds, the question remains how something imaginary can actually represent reality in a scientifically meaningful way.

In the sequel, I therefore focus especially on the recurrent comparison of models with literary fiction, which I take as my point of departure. Starting from there, I show how the fictional account of scientific models proposed by Roman Frigg (2010a,b), which takes this comparison seriously and applies Walton's theory of representational arts (Walton, 1990) to scientific models, can be applied to (micro-)economics. Fur-

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<sup>4</sup>The importance of story telling in economic modelling is also convincingly demonstrated by Morgan (2001) or Grüne-Yanoff and Schweinzer (2008); while Morgan argues more generally, Grüne-Yanoff and Schweinzer focus on applications of game theory.

<sup>5</sup>In a later paper, Sugden (2009) in fact becomes more careful and mainly takes credible / possible worlds as supporting abductive inferences, i.e. inferring similar causes from observing similar effects.

thermore, I demonstrate how this allows to understand a variety of prominent issues concerning economics methodology in one coherent framework. In particular, I argue how such an interpretation of models renders it possible **(a)** to square the fictional character of models with their claim at providing relevant insights for reality and to better understand **(b)** the common emphasis of background knowledge / story telling, as well as **(c)** the frequent reference to isolated causal mechanisms in one consistent picture.<sup>6</sup> Moreover, based on my argument, I discuss how the fictional account of models can help **(d)** to both appreciate and clarify the different roles of theoretical models and empirical tests / experiments within economics. In fact, I believe that it is the constant interplay of both which eventually differentiates economic writings from pure works of fiction and which eventually lies behind much of the confidence economists have in their inductive inferences.

As demonstrated nicely by Frigg (2010a,b), the main point to note is that we have to find answers to very similar questions if we are to talk about literary fiction, or works of arts in more general, and scientific models (see Section 3 for a more extensive review of the argument). In both cases, we are urged to / allowed to imagine certain things, but not others; we face questions as to judging truth in the fiction (the model, which is based on false assumptions), relations between works of fiction (models) or the comparison of fictional entities (items of the model) with real entities. In a nutshell, Frigg (2010a,b) argues, based on Walton's earlier discussion (Walton, 1990), that models can be understood as authorised games of make-believe. Upon seeing certain props (triggers) there is a set of commonly agreed, i.e. authorised, rules saying in which way the reader is allowed to entertain imaginations and to judge the correctness of statements within the model. Moreover, these rules also say which assignments of properties to objects are allowed, thereby providing a way to compare the imaginary system, i.e. the model, with the target system, i.e. reality, namely by comparing possession of properties.

As I argue in Section 4, this description, once applied to economics, covers a good many of the common features of economic modelling. In particular, to give a preview of the argument, one might see expected utility theory (EUT) and possibly, to a lesser degree, also game theory as powerful general rules of imagination in the background – the background knowledge – and the common story telling around specific models

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<sup>6</sup>In the sequel, I nevertheless only allude to the discussion concerning causality, though. I believe that a dispositional interpretation of economic models along the lines instructively described by Cartwright (1989) nicely fits the picture. Yet, it is beyond the scope of this paper to delve into the depth of this intriguing discussion and, in fact, possible to stay essentially agnostic on this matter for the present argument. For some further arguments see also Walzl and Wichardt (2014).

as suggesting additional less formal and less general but still necessary rules tailored more to the problem at hand (suggesting parameter ranges etc.). Moreover, slightly extending Frigg's proposal, I argue that the stories told in economics not only serve as description of the (fictional) model and prompters of desired imaginations. Instead, I suggest that they also provide rules for the comparison of (the properties of) theoretical entities and structures with real ones, thereby building a powerful bridge between the model and the target system. Thus, it is the underlying principles referred to in the story which indicate the background knowledge necessary to judge truth within the model. And it is the real life scenarios mentioned which provide the required guidance with respect to how to actually compare the model to reality.

In view of comparisons with reality, it should be perfectly clear, though, that the model world is a simplified one – compared to reality. It is used to tell fictional stories about special characters (such as the rational agent) accounting only for certain (causal) properties and mechanisms. Nothing is said about the interaction effects that will arise once (causal) properties and mechanisms, which have been neglected, are added. And some of the entities / properties of the model world may well not have real counterparts as they are assumed essentially to facilitate the argument. This is also why the behavioural patterns described in a certain model are almost never exactly found in reality. However, the occasional disagreement with empirical evidence in itself is not surprising once we observe that the perspective taken is a very special one. The fictional (economic) model world simply is not meant to account for all relevant aspects and will therefore not be suitable to study all possibly interesting effects of individual economic behaviour (see Giere, 2010, for a discussion of scientific perspectivism); especially social aspects of the interaction have been shown to be difficult to capture (Camerer, 2003, provides a summary of much of the evidence).

To wrap up, my objective in writing this paper is to add some hopefully clarifying structure to the methodological discussion about economic models by taking seriously the comparison of economic models with items of literary fiction. In doing so, I hope to show how a good many of the common concerns expressed about economic modelling can be gathered under such a perspective and how some open questions may indeed be answered. In order to do so, I first briefly review the methodological discussion about models in economics – without any claim at completeness in Section 2. In Section 3, I present Frigg's account of models as fictions before I then argue, in Section 4, how this can be applied to and partly dissolve the concerns about modelling in Economics. I provide some further discussion about how different earlier views can be

accommodated in the argument and what the possible lessons for the interpretation of economic theory or the role left for experiments might be in Section 5. Section 6 concludes with a brief summary and some final comments on how I believe the present discussion renders it possible to understand some of the main idiosyncracies of economics from one coherent perspective.

## 2 The Trouble with Models in Microeconomics

In the sequel, I provide a somewhat more detailed review of the methodological discussion about economic modelling both in economics and in philosophy. It should be said directly, though, that the review focuses on the most prominent positions and makes no claim at completeness. For expositional purposes, I decided to separate issues essentially according to whether the respective papers were published in Economics or Philosophy. The line, of course, is imperfect and blurred and I repeatedly trespass the borders thus set. Yet, I hope that the choice made helps to get a better view of the different perspectives taken.

### *Concerns from Economics*

Regarding the discussion in economics, I will focus on arguments being concerned with the relation of models and their assumptions to reality. I do so, because I believe that, especially in microeconomics, the mechanics of the model and its relation to reality lie at the heart of the matter (see, for example, Sugden, 2000; see also further below). It is worth mentioning though, that there is also a strand of literature which is less concerned about the realism in the modelling but instead is emphasising essentially the importance of predictive power; Friedman (1953) is a well-known case in point.<sup>7</sup>

A prominent example highlighting the common reliance on false assumptions and questioning the value of the results thus derived are Gibbard and Varian (1978). Emphasising the combination of stories and structure, they argue that models are used to talk about central tendencies. Moreover, while admitting that the assumptions made are not exactly true (and, hence, not meaningful to test), they claim that the statements of applied models are still meant to be (roughly) true and testable. In particular, they distinguish two types of (descriptive) models, namely approximations – with assumptions sufficiently close to reality so that conclusions might also be – and caricatures – exaggerating and isolating one particular feature in order to explain the role of a certain factor.

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<sup>7</sup>Again it should be mentioned that Friedman's focus were theories not models as the latter have only recently gained more attention; see also Footnote 2.



A related but decidedly more sceptical position on the matter is advanced by Rubinstein (2001, 2006). Similar to Gibbard and Varian (1978), he emphasises the dichotomous nature of models in economics being comprised of mathematics, i.e. structure, and interpretation. Different from Gibbard and Varian, however, Rubinstein emphasises in particular the absurd conclusion which the assumptions made in economic modelling commonly allow (see Rubinstein, 2001, for a telling example); here he, in fact, emphasises the importance of (non-formalised) background knowledge to discern the limits of the models. Accordingly, he rather suggests for empiricists to test assumptions and general stories told instead of conclusions reached, claiming that theory makes no accurate predictions. All in all, Rubinstein expresses a strong scepticism about the use of economic theory and compares economic models with fables, acknowledging only that *something significant remains* (Rubinstein, 2006, p. 881). This of course implies that, all problems notwithstanding, there still is some general lesson worthy of being learned. What this lesson is or how it can be found, however, is not made explicit.

More recently, Gilboa et al. (2014) advocate a more positive position regarding economic models. In particular, taking into account the constraints of human knowledge acquisition, they argue for knowledge as being case-based rather than rule-based; in fact, they even provide a technical model to support their argument. According to their account, models are (unrelated) cases, i.e. instances of experience (in this case theoretical ones), as are empirical results or lab experiments, which by some form of analogy reasoning, generate knowledge about the real world. What actually determines analogies, how the *unreal* model cases can actually be meaningfully related to the *real* world, and why there is no difference between “model cases,” “empirical cases” and “reality” (or, if there is, where the line goes), however, is left open – eventually all of them deliver cases of equal value in their model. Thus, while the idea of case-based reasoning is certainly an important one, the recurrent reference to similarity judgements in the overall discussion being supportive of this view (see, in particular, Sugden, 2000), I believe that the position put forward in Gilboa et al. (2014) still sidesteps some of the central aspects of the problem.

#### *Main Points Raised in Philosophy of Science*

Moving from the economists perspective to the philosophical discussion, I want to start with some general comments on the perception of theoretical science and models. As pointed out by Frigg (2010a,b), whose more general position is presented in the next section, the predominant understanding of theories today is referred to as the semantic

view of theories according to which theories are essentially a collection of models.<sup>8</sup> As argued by Giere (1988), these models are not a simplified version of some real system but constructions which themselves make no claims about reality. Instead, they have to be linked to the target-system by the scientists who, for example, add hypotheses asserting certain similarities between the models and the world, i.e. the target system (see also Giere, 2004). This view, naturally, goes not without objections and internal discussion. Most pertinent to the present argument, perhaps, is the view expressed by Morgan and Morrison which describes models as autonomous agents mediating between theory and reality; in doing so, however, they also emphasise the importance of additional, non-theoretical information to establish the link to reality (Morrison, 1999; Morrison and Morgan, 1999).

It is, of course, beyond the scope of this paper to adjudicate between these views or to comment on the subtleties of the broader philosophical discussion. Yet, I believe it is still reassuring to note that the more fundamental discussion in philosophy acknowledges the special role and the non-real character of models which makes them hard to judge without further information / hypotheses. At least when seen from the outside, this discussion indeed already seems to be in line with many of the above mentioned concerns expressed in economics itself, most of all perhaps those expressed by Rubinstein (2006).

There is, of course, also a more specific literature focusing on the problems of economic modelling in particular. A prominent theme in this discussion is that economic theory is concerned with the study of single causal factors in behaviour. For example, Hausman (1992) argues that economic models are build to explore tendencies,<sup>9</sup> but that deductive arguments given are based on inexact generalisations – which is why he refers to Economics as an inexact science. In a similar vain, Mäki (1992) argues for models as tools devised to study certain behavioural mechanisms in isolation. And Cartwright (2008, 2009) compares models with (over-constrained) Galilean thought experiments, which are the basis for stories told about capacities.<sup>10</sup>

A notable point in this discussion, which is particularly prominent in Cartwright (2008), are the possible limitations of conclusions drawn / inductive inferences made from economic models, in particular with respect to applications. According to Cart-

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<sup>8</sup>The opposing view being the syntactic view of theories according to which theories are interpreted logical calculi.

<sup>9</sup>The idea of tendency laws, in fact, was already expressed by Mill (1843).

<sup>10</sup>A rough and imprecise description of a capacity would be to describe it as a contingent causal tendency. To cite a standard example, Aspirin may be said to have the capacity to relieve pain; they work for many but not for all (see Cartwright, 1989, for an extensive and illuminating discussion).

wright, this is due to the over-constraining assumptions commonly made in order to derive the results, such as adding auxiliary assumptions, fixing parameters or simplifying for reasons of technical tractability.<sup>11</sup> It is beyond the scope of this paper to comment on this issue in greater detail (see Walzl and Wichardt, 2014, for further discussion). However, it is worth mentioning that Rol (2008) has already argued for the possibility to gain truth by further abstraction. In fact, this point is also acknowledged by Cartwright (2008) who, referring to Lessing (1759), argues that we indeed need the concrete to learn about the abstract so that (over-constrained) models might indeed be just a way to fit out a more abstract story lying behind. Put differently, while economic models may arguably be very specific and “false” in their particularities, they may still be a way to learn something about a “true” more abstract general mechanism.<sup>12</sup>

Furthermore, Sugden in an influential paper (Sugden, 2000; see also Sugden, 2009) argues that models create credible / possible worlds. Observing that many theoretical models in economics are silent about specific predictions, Sugden suggests that these models are used to demonstrate causal mechanisms in a simplified but credible, with respect to its closeness to reality, world. Relying on an admittedly subjective notion of similarity the idea, then, is that causal mechanisms which give rise to certain phenomena in the model are likely to also be at work in sufficiently similar real situations;<sup>13</sup> notably, in doing so, he also uses the comparison with realistic novels.

Last but not least, there is a strand of literature which focuses in particular on the role of story telling in (especially applied) economic modelling (e.g. Morrison, 1999; Morrison and Morgan, 1999; Morgan, 2001; Grüne-Yanoff and Schweinzer, 2008). The common thrust of the arguments put forward in this discussion is that models are special – autonomous agents, as Morrison (1999) puts it – in that they not only rely on mathematical structure but, being intended to be closer to reality than pure theory, rely heavily on the stories told around them. On these accounts, the stories

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<sup>11</sup>As also pointed out by Cartwright (2009), economists often perform robustness checks to assess how far a claim made in a model can be extended. Yet, she claims, most of these robustness results essentially show very little except that the respective dimensions are irrelevant to the problem at hand.

<sup>12</sup>In this sense, the position of Cartwright may indeed be seen as somewhat related to the argument for a case based knowledge put forward by Gilboa et al. (2014). My reading of Cartwright, however, is that she rather sees the models as being connected by this abstract story, which is looming in the background, rather than as independent cases.

<sup>13</sup>While not of central concern for the present argument, it is worth pointing out that the restrictions regarding predictive power which are inherent in this account are one of the major concerns expressed by Cartwright (2009) about Sugden’s account. While I agree with Cartwright’s critique, I still believe that Sugden’s argument captures many important aspects of the problem; but see below.

are important in order to fill the gap between the formal discussion and reality, e.g. by filling abstract concepts with meaning or suggesting a range of applications.

### *Summing Up – The Main Themes*

As I hope the above review, which does not claim to be complete, has shown, the discussion about models in economics has many facets, pitfalls and interesting tangents to possibly take off at.

For the argument to follow, it is useful to single out some recurrent themes, namely **(a)** the comparison of economic models with objects of literary fiction and the idea that models are primarily concerned with objects which are not real (the main focus of the argument to follow),<sup>14</sup> **(b)** the emphasis of the importance of story telling and background knowledge about the situation at hand, and **(c)** a recurrent reference to causal mechanisms, tendencies or dispositions (which I will only take up briefly). Moreover, there is **(d)** the omnipresent question about the relation of models to “real life” and the related question about the connection between (theoretic) economic modelling and empirical testing.

### *Open Questions*

As already mentioned in the introduction, the arguments given by Sugden (2000) nicely demonstrate that many of the suggestions made regarding how to understand economic modelling, while clearly providing important insights, still also suffer from various shortcomings. And that arguably makes it difficult to agree with more than aspects of them. Instrumentalism, for example, has difficulties to answer why so many models do not make precise predictions. The isolation / inexact-deductions perspective, in turn, faces the challenge to explain how it can be that many aspects in the construction of a model may not even be approximately true. In fact, I believe that Sugden's own credible worlds account faces a similar problem, at least to some extent, as it relies on (subjective) similarity of the model world with reality and assumes the imaginary, credible world to be possible. Once we compare economic models with imaginary worlds or, more specifically, items of literary fiction – be it caricatures, parables or fables – the question remains how something fictional can meaningfully represent reality in way that would count as scientific. And as long as these questions are not answered in a satisfying way, it is difficult to justify the inductive inferences often made in connection with economic models.

A central question which, thus, remains to be answered is how economic models

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<sup>14</sup>Note that the comparison with literary fiction already suggest that models in economics are about more than pure mathematical structure.

represent reality and how we can actually learn something about reality by exploring these in many respects apparently rather unrealistic models. In the sequel, I take up this question and argue how economic practice can be understood in a meaningful way and from one coherent perspective if we adopt Frigg’s fictional view of scientific modelling to economics (Frigg, 2010a,b). To begin with, however, I first have to give a more extensive account of what it means to perceive models as fictions in the sense of Frigg.

### 3 Models as Fictions

In recent work, Roman Frigg has compared scientific models with works of fiction (Frigg, 2010a,b). Borrowing from Walton’s pretense theory of the representational arts (Walton, 1990), Frigg presents an illuminating way to structure the debate about models, clearly differentiating and putting into place the questions about truth within the model as well as truth when it comes to comparative statements linking the model to some target system. In the sequel, I give a brief summary of his argument; for a more extensive discussion as well as an instructive defence of his against the structural account of models (emphasising essentially the importance of story telling), the reader is referred to the original work by Frigg.<sup>15</sup>

In comparing models with fictions, Frigg<sup>16</sup> aims to provide an answer to two main questions which, he argues, ought to be differentiated in the discussion about models and modelling, namely “What is the model?” and “How does it represent?”. In order to avoid confusion with earlier approaches as well as casual discussions of the problem, Frigg avoids the term “model” – thereby of course avoiding to give a direct answer to the first question – and instead refers to the *model-system* (the hypothetical object of study), the *model-description* (introducing the model-system), and the *target-system*. The term *modelling*, in turn, is used to describe the whole practice of inventing, presenting and using the model system (see Frigg, 2010b, p. 99).

The main point of Frigg’s argument, then, is to note that there are important parallels between scientific modelling and literary fiction. Both, he argues, stir certain imaginations which, in significant ways, are not subjective but shared by many (we all

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<sup>15</sup>See Frigg and Votsis (2011) for an excellent discussion of structural realism. Regarding the discussion of economics, the position taken by Schelling (2006), emphasising the role of models as templates to compare with reality, can supposedly be interpreted in this fashion.

<sup>16</sup>If not mentioned otherwise, all references made in this section refer to Frigg (2010a,b); both works present the same argument with Frigg (2010b) being essentially more detailed.

supposedly have roughly similar ideas about Sherlock Holmes or Hamlet or, in case we are economists, about “the rational agent”). Once this parallel is acknowledged, he suggests that, taking the analogy one step further, the theory of literary fiction may indeed provide helpful clues as to how to understand scientific model-systems and the way they represent.

In a nutshell, Walton (1990) argues that works of fiction stir imaginations using certain prompters of cues, referred to as *props*; the leading example given by Walton (1990) is a children’s game where stumps are to be perceived as bears (here the stumps are the props). Moreover, with respect to those aspects relevant for the piece at hand, imaginations usually follow certain rules (Sherlock Holmes is to be imagined to live in London and the imaginary world around it as having the same physical properties as the real one etc.). What is more, the rules set are essentially agreed upon by all readers of the respective piece of literary fiction (in relevant respects, we imagine the London of Sherlock Holmes in the same way; this is different for the children’s play, in which stumps are bears, where rules are ad hoc). Once the rules of imagination are agreed upon, or authorised, Walton says that whenever we react to the props and, upon perceiving them, engage in the respective rule-based imaginations we play an *authorised game of make-believe*.

Taking up the ideas put forward by Walton (1990), Frigg roughly argues that scientific modelling is like playing an authorised game of make-believe in that there is a common understanding of what to imagine upon certain cues, i.e. props (here descriptions of the model), and how to do that. Moreover, the rules of the game – in science – are such that everyone knowing them can, in principle, start off to explore the corresponding imaginary world. This is interesting because the fictitious world is not fully described by the model-descriptions, which is why studying a model-structure can be informative – there is something to be discovered.

What is more, while the objects to be imagined of course do not exist, the rules of generation for imaginations still tell us which properties we are allowed to imagine these object to have. Thus, and this is crucial, we can actually judge if a proposition  $p$  is true in fictional world  $w$ , the model-system, namely if the  $w$ -prop together with the principles of generation of  $w$ , i.e. the rules of the corresponding game of make-believe, prescribe  $p$  to be imagined (Frigg, 2010b, p. 119). And, being able to assess the truth of propositions within a model-system, we can also judge whether two model systems are identical, which they are if the sets of propositions which are true within the respective model-systems are identical.

Moreover, observing that science is usually interested in the properties of a model-system and not so much in its objects (which, as pointed out by Frigg, are ontologically more problematic), the approach also opens a way to judge comparative statements between different model-systems, i.e. different fictional worlds. As Frigg (2010b, p. 119) points out, *the truth conditions of transfictional statements in the context of scientific modeling come down to truth conditions for comparative statements between properties, which are unproblematic in the current context.*<sup>17</sup> And, by the same token, we can judge comparative statements between a model-system and the target-system, namely by identifying the truth conditions for comparative statements between the properties of these systems.

To take stock, the fictional account of scientific modelling describes modelling as the setting of rules which allow to generate certain imaginings but not others in combination with the fictional world thus described.<sup>18</sup> Moreover, it allows to assess when two model-systems are identical, comparing the sets of propositions true in these models, and more generally to judge statements between different model-systems or between the model-system and the target-system, namely by judging the truth conditions of comparative statements about properties of these systems.

Finally, regarding the question how models represent, Frigg first of all observes that common parlance often confuses representation of entities within the model and representation of the target-system. – In economics, for example, talking of a market may refer to either a model market (with rational agents) or a real market (with say less rational, real agents) and economists, in common discourse or writing, often do not clearly distinguish between the two. – For the sake of clarity, the former, which is an aspect of the model-description, is referred to by Frigg as *p-representation*, i.e. p-representation refers to the connection between the model description and the entities within the model-system, while the latter is called *t-representation*, i.e. t-representation is about how entities in the model-system relate to entities in the target-system; Figure 1 provides a graphical representation of Frigg’s account.

Regarding t-representation, Frigg suggests a comparison of model-systems with maps, the latter commonly representing aspects of the real world in an abstract way,

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<sup>17</sup>In theoretical physics, for example, it also seems that what counts are the properties ascribed to theoretical entities (such as electrons) and the observation of these properties in reality but not so much the entities themselves (the electrons).

<sup>18</sup>Note that the rules of the game are notoriously incomplete. This, however, need not pose a problem as additional rules can, and often are, added when needed (I am grateful to Roman Frigg for pointing this out to me). Eventually, the imaginary scientific world is in constant change (cf. Neurath, 1921).

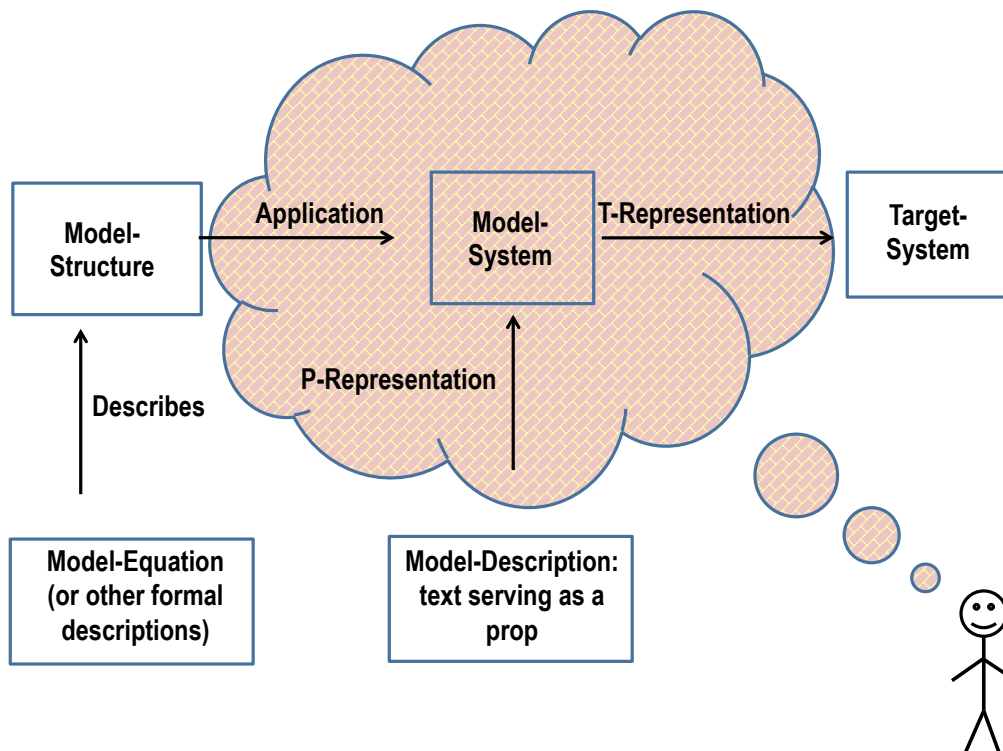


Figure 1: Elements of scientific modelling (following Frigg, 2010b).

i.e. neglecting many aspects of reality. As maps, however, which come with a legend informing the reader how to interpret the map, model-systems have to come with a key informing the reader how to relate the model-system to the target-system, i.e. endowing the model with representational power as Frigg (2010b) put it. As I will argue in the next section, this does not pose a problem for economic model-systems but rather provides an intuitive explanation for the abundant story telling in economics.

## 4 Models as Fictions – Application to Economics

In the sequel, I start out with a kind of case-study in order to demonstrate how the fictional account of scientific models actually fits common practice in economics (Section 4.1). In a second step, I proceed to discuss some more general aspects related to the comparison (Section 4.2). In the general discussion, I suggest to extend Frigg's



(2010a,b) proposal by emphasising the double role of the extensive story telling around model-descriptions in economics. In particular, I argue that these stories serve both as props in the corresponding game of make-believe *and* as (part of) the key which is necessary to understand how the described model-system (t-)represents reality.

## 4.1 A Case-Study

The main reference example, I use in order to demonstrate how the fictional account of scientific models fits common practice in economics is Akerlof's famous paper "The Market for "Lemons": Quality Uncertainty and the Market Mechanism" (Akerlof, 1970). The reason for this choice is that the paper, which was also used as one of the main reference examples by Sugden (2000), offers a very clean case with a comparable simple technical model and abundant story telling around it.

In brief, the point made by Akerlof (1970) is that asymmetric information may lead to market failure. In order to make this point, Akerlof creates a model market for used cars of two possible types: good ones and bad ones (lemons). Moreover, he assumes that the quality of a car is only observable by its owner and that buyers and sellers value good cars higher than bad ones. In such a market, Akerlof argues, no good cars may be offered even in cases where the price buyers would be willing to pay for good cars is higher than the reservation price of sellers of such cars (so that trade could make both parties better off). The reason for this is that, depending on the distribution of car types in the market, the price buyers would be willing to pay for a car of unknown quality could actually be below the reservation price of the sellers of good cars.

In view of the present discussion, it is instructive to first of all note that the paper consists of a (a) short introduction mentioning the topic of study and providing some brief allusions to real life markets, (b) a description and analysis of the model using the market for used cars as the reference example, (c) a long discussion of different possible real life applications, (d) a brief mention of counteracting institutions and (e) some concluding remarks (which I will neglect in the following). Moreover, it is noteworthy part (b) contains a model-description and a technical analysis of the model-system whereas all other parts of the paper actually refer to real life markets.

Focusing on part (b) first, the model-system is even introduced by saying that the (stylised) reference example "*.. is chosen for its concreteness and ease in understanding rather than for its importance or realism.*" (Akerlof, 1970, p. 489). Furthermore, in describing what types of cars are traded in the model-system, Akerlof again points

out that the restriction to four types (new and used as well as good and bad) is made “*for the sake of clarity rather than reality*” (Akerlof, 1970, p. 489). In describing the model-system, Akerlof states how market demand and supply depend on quality and price, that both will equal each other in equilibrium, what determines the utility of the agents (money and their valuation of the car), what agents are supposed to know about the expected quality, that agents are von Neumann-Morgenstern expected utility maximisers, and that what is looked for is an equilibrium. Yet, as already observed by Sugden (2000), there is no clear instruction how to relate the analysis of the – apparently unrealistic – model-system to the real life cases.

Note, however, that at the time the paper was written expected utility theory (EUT), equilibrium (market) analysis and to a lesser extent also game theory,<sup>19</sup> which had been introduced to economics in 1944 (von Neumann and Morgenstern, 1944), were already well known ingredients of economic modelling. The way the model is described by Akerlof, thus, most likely will have prompted the following imagination among its readers (or something roughly similar): there is world in which people only care about the value of the four types of cars and money (i.e. only outcomes and their monetary value matter and there are no relevant personal relations between sellers and buyers or social norms to be considered); agents are rational in pursuing their goals (they will not get sidetracked and optimisation procedures are no problem) and they are able to perfectly assess the objective probability to obtain a good car in different situations (i.e. in equilibrium expectations are correct); there is only one market (that for used cars); and there are no other external processes interfering.

Thus, when introducing his model, Akerlof puts his argument into the broader context of what might be referred to as the abstract world of the rational economic agent. In doing so, he neglects all aspects of the problem that, although undoubtedly relevant in the real world, would only obscure the point to be made (which is why Akerlof himself describes the reference example as unrealistic). What is more, the reference to equilibrium and EUT sets clear limits on how to explore this world, namely by conducting a technical maximisation exercise where only outcomes matter and agents have unrestricted calculation abilities. Thus, we are told how to judge truth in the model and our experience with the underlying model world already induces some confidence in the inferences to be made from the model at hand.

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<sup>19</sup>Akerlof’s argument does not rely on any game theoretic concepts but rather refers to market equilibrium (see Akerlof, 1970). The common reliance of game theoretic arguments on equilibrium notions, however, and its proliferation in economic arguments over the years are why I decided to add this more general reference already at this point.

In the introductory as in later examples, parts (a) and (c), Akerlof then presents some real life cases of markets for which he argues that the properties in these markets, especially regarding the information structure, are essentially the same. Moreover, in doing so, Akerlof implicitly provides some guidance for the reader which properties of the model are to be compared with which properties of the real world. Thus, in the end, what is implicitly suggested is that the processes which lie behind the (derived) properties of the model-system are also present in the real world. This inference to the same cause is just the abductive reasoning emphasised by Sugden (2000, 2009). Yet, on the present account there is no need for the model world to be actually possible to allow for this kind of reasoning. It suffices for it to represent the relevant features of the real world in a way that allows the scientist (Akerlof in this case) to make his point.

Moreover, the section on counteracting institutions, part (d), mentions some properties real markets may have (institutions that may be present, such as brand names), which are not present in the model and which would prevent the processes described for the target system to take effect. In order to appreciate the actually confirmative role of this part for the overall argument, recall that the model itself, as presented by Akerlof is not realistic – and Akerlof indeed never claims it to be. In fact, it neglects a variety of aspects of reality which are likely to interfere with the process described by Akerlof. The way the model-system is introduced and embroidered with real life examples including counteracting institutions, however, provides the reader with the information necessary to understand how the model is meant to represent reality and where to look for similar effects and where *not* to.<sup>20</sup>

To wit, the point I want to make is that the way Akerlof presents his model-system is filled with references to standard economic concepts – most of all EUT, equilibrium (analysis) and markets – thereby providing an authorised set of rules of generation for imaginations and telling the reader what type of world to imagine and how to explore it. Moreover, the analysis of the model-system proceeds to establish certain properties of the model-system which are deductible from the assumptions but not entirely obvious *ex ante* and which are pertinent to the overall argument being made. The extensive discussion of examples, finally, is used to emphasise the presence of these properties also in real markets, i.e. the target-system. Thus, while not made explicit, the overall story telling not only serves as a prop for the game of make-believe being

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<sup>20</sup>The mention of potential counteracting institutions, in fact, also ties in well with a causal dispositional interpretation of economic theory (e.g. Cartwright, 1989). Here, the counteracting institutions would be antidotes prohibiting certain dispositions from being activated.

played (referring more or less explicitly to the rules of the game being played, which are commonly understood, by mentioning markets, EUT, etc.). They also provide a key as to how to interpret the model-system with respect to the real world (which properties to focus on), i.e. they endow the model-system with representational power; see Figure 2 for an illustration.

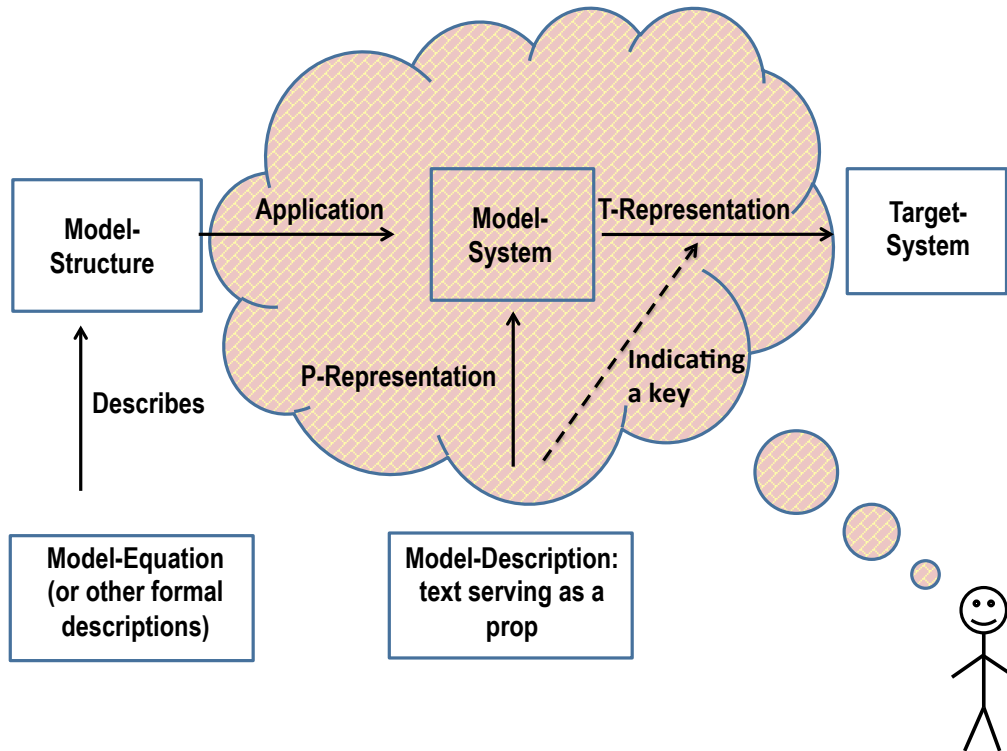


Figure 2: Elements of scientific modelling also indicating the key-providing role of story telling in economic modelling.

## 4.2 Models as Fictions in Economics

Leaving the concrete example and moving to the more general practice in economics, the overall structure in which Akerlof (1970) proceeds in making his argument is very common in economics. There usually is a clear reference to standard work horses such as EUT (indicating “selfish” utility maximisation) – and often also to game theory (restricting attention to the most prominent / relevant possible actions and suggesting

a focus on outcomes and static equilibrium analysis rather than procedures) – thereby setting the, already authorised, general rules of generation for the imaginings allowed to be made. This reference, then, is extended by some additional assumptions which fit out the abstract framework and make it applicable to a (more or less) specific situation. And there mostly is extensive story telling, especially in introductions, providing an implicit key as to how to compare the properties of the model-system to be analysed to the real world – the target-system.

Of course, the degree to which stories are told within the process of modelling depends on how applied the model itself is. Questions which are entirely concerned with the exploration of properties of the fictional world, mostly published in pure theory journals, will commonly provide very little story telling. In these cases, what is important is the rules of the game in order to know which world is to be explored. These, however, can be introduced briefly, and usually only few words – if any – are needed in such studies to indicate how to compare the model with reality. Papers which deal with less abstract / more applied questions (such as the one by Akerlof), by contrast, need to spend more time on story telling as the comparison with reality is of primary interest to them. Accordingly, it is important to clarify not only which model-world is to be explored but also how this is meant to represent reality (cf. Giere, 2004); and the latter, as I have already argued, is what a good deal of the story telling is needed for.

In fact, while I believe that the fictional account of scientific models proposed by Frigg (2010a,b) fits the practice in economics quite well – a belief which I hope the above discussion corroborates, albeit based on just one example – a point that in my view should be added to the discussion provided by Frigg is that model-descriptions, at least in economics, usually do more than just describe the model-system (serving as props). In particular, I believe that an important additional aspect of the verbal model-descriptions in economics, the story telling, is to indicate the key (often implicitly) telling the reader how the model is meant – by the writing scientist – to (t-)represent the real world. In fact, the Akerlof example is a nice case in point as the model-description not only refers to the underlying rules of imagination (EUT and rational equilibrium analysis) but also extensively describes some simplified real world scenario, which, later on, serves as the leading reference example throughout the whole text: the market for lemons.

## 5 Discussion

### 5.1 What is it All Good for?

Of course, a valid question to ask is what it is all good for – from an economists point of view. In the remainder of the section, I will therefore try and argue how the the fiction view on economic models may indeed help to clarify the role of economic theorising and the role of theory in relation to empirics – especially economic experiments.

#### What to Learn about Theory

The preceding discussion proposes to take a perspective for the interpretation of economic theory the benefit of which, I believe, is at least twofold. First and foremost, in differentiating different parts of economic modelling, it clarifies which parts belong to it, why they are necessary and how they belong together. And being aware of these aspects, of course, is helpful to understand common practice in economic theory – not least in view of its applicability.

In fact, knowing what the rules of the game of make-believe played in economic theory are – as indicated by references to (the community’s technical background knowledge about) EUT, markets, game theory and some auxiliary assumptions tied to the problem at hand – also makes it easier to recognise the limits of the imaginary world. And knowing these limits, arguable makes it easier to judge which comparisons of the model-system with the real world are meaningful and which are not. What is more, it also helps to differentiate between those limits which are set by the general game of make-believe being played (the general principles of imagination referring, for example, to the rational agent) and those which are due to the particularities of a certain model-system (the auxiliary assumption). Thus, it may also help to understand why, in common seminar discussions in economics, concepts such as EUT and game theory are rarely questioned – being essentially authorised – while debates over other auxiliary assumptions tend to be far more frequent and heated.

Moreover, the dual role ascribed to the stories told in economic papers – describing a model-system but also suggesting a key for how it (t-)represents – suggest a simple answer to the question why theoretical economic papers, despite an often very technical kernel, commonly also come with a rather chatty part. They are simply needed to introduce and justify necessary auxiliary assumptions and to indicate a key that relates the model-system to the real world. Yet, while it is important for the stories told to allude to the real world, they are – on the proposed account of economic modelling

– not to be understood as direct recommendations for policy applications; they only provide suggestions for how the model-system discussed actually may (t-)represent a target-system in the real world. Whether and how policy recommendations based on the respective model-system should at all be made, in fact, depends on the representational power of the model which to establish, after all, is an important challenge for empirical studies.

### **Economic Theory and Empirical Testing**

Once the perspective on economic theory suggested by the fictional account of scientific modelling (Frigg, 2010a,b) has been clarified, I believe that also the role empirical testing (be it in the lab or elsewhere), another common theme in the methodological discussion about economics, becomes easier to appreciate. In fact, as (t-)representation is about comparing properties of a model-system with those of the real world, the natural role for empirical testing, according to the present argument, would be to focus on certain properties of the model-system, assumed ones as well as derived ones, and to try and compare these with relevant target-systems (as suggested by the key which goes with the modelling). If some congruence is found, this gives empirical support to the practical relevance of the corresponding model-system which, of course, is important for a social science such as economics.

Moreover, another important role for empirical studies in more general would be to “simply” explore the target-system and to study, for example, which properties are important for the target-system in order to be meaningfully represented by a certain model-system (see also Cartwright, 2006, for an argument about the importance of the context). For example, it may be a worthy exercise to find out which properties different real life contexts have to share in order for certain types of economic model-structures to be compared to them in a meaningful way.<sup>21</sup> Anonymity or experience of decision makers with the context, for instance, typically tend to increase the empirical fit of economic model-structures based on EUT and game theory while a more pronounced emphasis on social aspects tends to reduce it (see, for example, Camerer, 2003, for evidence).

In any case, however, it should have become clear that, on the present account, empirical studies differ from theoretical ones. In particular, while theory is exploring a fictitious world by playing an authorised game of make-believe, empirical studies

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<sup>21</sup>This point is related to the one made by Rubinstein (2001) that empirical studies should try and find general patterns of behaviour rather than single instance effects.

explore the (t-)representational power of such model-systems by comparing the properties of the fictitious model-systems with properties of the real world, or else, they may pose new questions to theory by identifying persistent regularities in behaviour which so far have not been described in the fictional (model-)world (cf. Rubinstein, 2001). Yet, it should also be clear that emphasising the differences does not intend to make any judgements as to their relative importance or value. In fact, I believe that it is just the constant interplay between modelling (theory) and empirical testing which contributes to the general confidence in the rules of the game and which eventually differentiates economic writings from pure works of fiction.

## **5.2 How the Earlier Discussion Can be Accommodated.**

In order to conclude the present discussion, I want to briefly revert to the earlier summary of issues debated in connection with economic modelling (cf. Section 2). In particular, I want to argue how the issues raised there might be accommodated within the perspective on economic modelling proposed in the preceding part of the paper.

As should have become clear from the discussion, the present view on economic modelling easily accommodates any earlier references made to fables or other objects of literary fiction (e.g. Gibbard and Varian, 1978; McCloskey, 1985; Rubinstein, 2006). In fact, one of the main points of the present discussion is to explore what can be learned if this comparison is taken seriously on an abstract level and what it actually means to take it seriously. Nevertheless, I hope to also have shown how economic modelling due to the stricter rules in its writing, the permanent interplay with observed data and the necessity to eventually be empirically testable, in fact, can be conceived of as scientific in spite of its similarity with fiction.

As I have argued, applying Frigg's (2010) fictional account of scientific modelling to economics actually outlines a comparably clear picture of what modelling consists of – a rule-based story telling about some fictitious world. In fact, it also clarifies how theoretical economic modelling differs from empirical studies – which either try to identify properties of the target-system and/or to compare these with the properties of some model-system, i.e. the fictional world. Moreover, it clarifies how similarity or analogy between different model-systems or a model-system and reality can be judged, namely by the comparison of properties. Naturally, which properties are to be considered depends on the context and the questions asked; and this is where the background knowledge comes into the picture. Thus, I believe the argument nicely demonstrates how different models can be seen under one heading – following



the same rules of imagination and exploring the same fictional world (albeit with potentially differing inclinations towards applicability); and how theory and empirics play different but still equally important roles in making the game a useful one – some exploring the fictitious world and the others comparing it with the real one.

Moreover, once we acknowledge that economics as a social science should indeed be concerned with real phenomena, it also becomes clear why the rules of the game eventually have to be such that the world to be imagined is a credible one. But, and this is different from Sugden (2000, 2009), on the present account it is sufficient for the model world to be close to reality in certain (relevant) respects. Eventually, the purpose of a model-system (in economics) is to convey some information about human behaviour. Accordingly, it is natural to expect it to have some intuitive congruence with our daily life perception of the world. However, also unrealistic assumptions need not be problematic. They only ought to be such that they do not diminish the representational power of the model-system. Thus, what is problematic and what is not will, in general, depend on the intentions of the modelling scientist (cf. Giere, 2004).<sup>22</sup>

In addition, the account also assigns a clear job to mathematical structure (setting the rules of the game of make-believe) and story telling (also setting the rules, prompting imaginations and suggesting adequate comparisons to be made between the model and the target system). Thus, the fictional view of models in economics proposed here indeed is very much in line with the general thrust of the arguments given, for example, by Gibbard and Varian (1978), Rubinstein (2001, 2006), Sugden (2000, 2009). Different from the earlier discussion, however, it is more explicit about how to compare fictional entities with real ones than are, for example, Rubinstein, Gibbard and Varian or Sugden. Moreover, it is more pragmatic than the credible worlds account of Sugden as it does not rely on the fictional world be actually possible or credible (see also Giere, 2004).

Furthermore, while not at the heart of this essay, it is worth noting that it also poses no problem to the current account to accommodate (causal) dispositions or tendencies as repeatedly referred to in the discussion (e.g. Mill, 1853; Hausman, 1992; Mäki, 1992; Sugden, 2000; Cartwright, 2009). As has been demonstrated by many empirical studies, the common behavioural patterns postulated by economic theorists

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<sup>22</sup>Note that, for example, the stories told in quantum physics usually will not be credible in the way stories are in economics. In fact, I believe that the idea of “credible” worlds is due to the fact that the objects economists are concerned with are simply closer to reality. Thus, the imaginary worlds considered are likely to be “credible.” This, however, is not a necessity for scientific modelling but rather an artefact of the particularities of economics.

are often very sensitive to context or framing effects (again I just refer to the general summary of evidence provided by Camerer, 2003). And this, arguably, fits well with the idea of dispositions as causal powers which need not take effect if circumstances are not right (see also Cartwright, 2002).<sup>23</sup> While it is not necessary for the current argument to take a stance in the debate, I still believe that it is possible and, in fact, very much in line with common parlance in economics, to view the overall fictional world as a thought experiment in which the effects of certain, few causal dispositions are studied while the influence of others is neglected. The more specific models in economics, then, fit out this abstract world by providing (slightly less abstract) stories which exemplify the possible effects of the underlying causal dispositions / capacities (cf. Cartwright, 2009). Thus, albeit often being very specific and over-constrained (e.g. Cartwright, 2008), these stories actually provide valuable insights into the abstract (causal) mechanisms of a (even more abstract) fictional world which, as I have just argued, can be meaningfully compared with reality despite it being far from real.<sup>24</sup>

In fact, I believe that seeing models as providing specific information about the possible effects of more abstract underlying causal disposition also suggests possible solution another problem, namely the justification of inductive inference. If we see models as fitting out a more abstract fictional world, I think it is possible to argue that positive experience with this underlying fictional world and its principles of generation for imaginations can indeed provide a justification even for inductive inferences somewhat along the lines suggested by Goodman (1983). To explore the details of this argument, however, is beyond the scope of this paper.

Finally, I want to stress that the present discussion is also compatible with arguments viewing models as different from theory (Morrison and Morgan, 1999). On the present account, theories (or general principles for that matter) set the general rules of imagination while the former also rely on additional – over-constraining as Cartwright puts it – assumptions to be made in order to make it applicable. In fact, the additional assumptions made can be seen as a way of testing different directions to fit out / extend the model-system, trying to find out how it would look like when using them and comparing the properties derived with reality.

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<sup>23</sup>An interesting case in point here also is the study by Schmutzler (2011) which nicely demonstrates that economists, while have problems with point predictions, often get the comparative statics right. Thus, once the incentives set change the strength of the response to these incentives increases, i.e. the stronger the stimulus the more likely the activation of the disposition.

<sup>24</sup>Akerlof's paper discussed in the previous section, for example, considers a very specific and reduced example. The point made, however, namely that asymmetric information may lead to market failure, describes a more abstract (supposedly causal) mechanism with rather general implications.

## 6 Concluding Remarks

Economists often perceive their science as very rigorous, especially when compared to other social sciences such as psychology or sociology. Yet, they commonly have only negligible reservations about devising complex theoretical systems, which they use to comment on many different real life problems, despite them knowing that the assumptions these systems are built on are empirically difficult to justify — to put it mildly.

In response to this observation, various arguments have been put forward as to how (not) to understand economic modelling and its relation to the real world (see Section 2). Some of these arguments, in fact, have even gone as far as to compare economic modelling with the telling of fables and suggesting that economics should rather be placed in the realm of culture than science (Rubinstein, 2006).

In the present paper, I have argued how it might indeed be possible to retain the analogy of economic modelling with fables, or literary fiction in more general, while at the same time arguing for economic practice to be scientific. In particular, I have suggested that the fictional account of scientific modelling (Frigg, 2010a,b), which roughly speaking takes scientific modelling as creating fiction albeit with strict rules of generation in order to be acceptable for the respective community and with a need to actually (t-)represent and to be empirically testable, fits well with common economic practice.

More specifically, I have outlined an argument which renders it possible to distinguish between different aspects of economic modelling (model-descriptions, model-system,...), indicates how economic model-systems are endowed with representational power (in the course of the story telling around them), provides an explanation for the abundant story telling in economics (referring to the rules of imagination for the fiction and providing a key for how to compare it with reality), and which even allows to interpret economic modelling causally (exploring the effects of certain causal dispositions in a fictional world, while ignoring others). In addition, I have discussed how economic models relate to empirical evidence and how the latter serves an inherently different but nonetheless important purpose. In particular, I have argued how, on the present account, empirical studies are necessary to explore the properties of the target-system (the real world) and to compare properties of the theoretical model-structures with structures in the real world. And I have suggested that is this interplay of theory and empirics which eventually differentiates economic writings from pure works of fiction.

Of course, I have no intentions to claim that the perspective on economic modelling proposed in the preceding discussion answers most (let alone all) of the questions raised in connection with economic practice and its empirical relevance. Yet, I hope that it will contribute to the ongoing discussion by adding some hopefully clarifying structure to it. In recent years, economists have added various new branches to their field (such as behavioural economics or neuroeconomics) all of which, to some extent, have raised questions about their relevance for the science. While I have nothing to contribute to this discussions here, I still believe that structuring the debate about economic modelling and empirical testing on a more abstract level might be a fruitful exercise also in view of the arguments exchanged elsewhere.

In any case, I hope that having suggested an argument that eventually retains economics in the realm of science may at least have rescued some confidence in the policy advice given by economists – although it is important to keep in mind that this advice is always given from the special perspective of an economist (cf. Giere, 2010), which derives from the experience with a very specific and simplified fictional world.

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