Economic Models as Argumentative Devices

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Abstract. This article critically evaluates Itzhak Gilboa, Andrew Postlewaite, Larry Samuelson, and David Schmeidler's account of economic models. First, it gives a selective overview of their argument, highlighting its emphasis on similarity and their oversight of the role of idealizations in economics. Second, it proposes a sketch of an account of models as arguments and argumentative devices. This account not only sheds light on Gilboa et al.'s approach, including its shortcomings, but also identifies key challenges in model-based inference, suggesting a fresh perspective on the uses of models in economics for diverse objectives.

1. Introduction

Why do economists keep on building and using models that employ "false" assumptions despite their failures in prediction and policy advice? In their contribution to the present special issue, Itzhak Gilboa, Andrew Postlewaite, Larry Samuelson, and David Schmeidler (henceforth GPSS) argue that economists find these models valuable because they help them achieve a diverse set of goals such as explanation, critique, and methodological analysis, not simply prediction and recommendation (Gilboa et al. 2022a).

That models can serve several functions and be used for a variety of purposes is, of course, no news for philosophers of economics and more generally for philosophers of science. However, there is more to GPSS's contribution than just saying this. First, they illustrate how a model can be interpreted in several ways and how its interpretation can change. Second, they show how different interpretations of models can be tied to various questions and claims. Third, as practitioners in the field who work with models, they provide an insider view of the modelling

practices in economics. In sum, they provide a rare opportunity for philosophers of economics to see how the diversity of functions of models look from within the field.

There is no doubt that GPSS's insights enhance our understanding of the value and limits of modelling practices in economics. This commentary critically discusses GPSS's account to further this understanding. The second section summarizes GPSS's main arguments, particularly on 'analytical models' and the goals of explanation, critique, and 'methodology'. The third section focuses on the justification of model-based inferences, seeking insights in GPSS's work. It reveals that, for GPSS, the similarity between a model and its target plays a key role in model-to-world inferences (i.e., where a model is used to draw inferences about real-world phenomena). However, they use two notions of similarity. In their model of modelling, similarity amounts to a mapping between the elements in a model and elements of a description of the real world. This conception of similarity, however, neglects the role of idealizing assumptions in economics. Their account of models as analogies, on the other hand, uses a vague notion of similarity, which is not useful for practical purposes. The fourth section has two parallel aims: (i) to explore further the role of similarity judgements in model-based inferences and (ii) to sketch an account of models as arguments and argumentative devices. By examining the argumentative structure of model-based inferences, this section shows that while formal models can be conceived as valid deductive arguments, model-based inferences are not necessarily deductive arguments—even though they use models (i.e., deductive arguments) as inputs. Model-based inferences – such as a model-based explanation, policy advice and critique – can be better conceived as attempts at "rational persuasion or at influencing (or convincing) others by providing good reasons to justify a claim" (F. H. van Eemeren et al. 2014, 6). By underlining the components and structure of several examples of model-based inferences in economics, the fourth section (i) reveals some of the shortcomings in the GPSS account, (ii) demonstrates that similarity as GPSS conceive it is often insufficient for good explanation, policy advice or a critique, and (iii) shows how conceiving of models as argumentative devices can expose gaps, jumps and leaps in model-based reasoning. The fifth section concludes the article.

Before proceeding, note two key points: First, like GPSS, my focus is on theoretical, not econometric and applied models. Second, I use quotes around GPSS's terms 'analytical model' and 'methodology' to signal my reservation about their definitions, which I don't elaborate on because of space limitations.

2. Recap

GPSS's contribution to the present special issue is a follow up to a series of articles they wrote on the uses and value of unrealistic models in economics (Gilboa et al. 2014; 2018; 2022b; 2019). Their guiding question is:

> If the assumptions of economics are *all wrong*, why do economists keep using them? Why do they develop sophisticated mathematical models based on such flimsy foundations? (Gilboa et al. 2014, F513, emphasis added)

In their latest work, GPSS answer this question by saying,

> [E]conomists find value in theories and in specific mathematical results in ways that go beyond their use for prediction or recommendation. (Gilboa et al. 2022a, 2)

That is, they argue that economists use their models for various purposes such as explanation, critique, and 'methodology'—not simply for prediction and recommendation. To make this point, GPSS distinguish among *types of models*, positive, normative and 'analytical'; *types of modelling goals*, prediction, recommendation, explanation, critique, and 'methodology'; and *types of model targets*, economic phenomena and economics. GPSS's discussion does not exhaust the set of all possible model type-target-goal combinations. They explore only some, emphasizing (i) prediction and explanation for positive models, (ii) recommendation for normative models, and (iii) explanation, criticism, and 'methodology' for 'analytical models'. According to GPSS, what makes a model positive, normative or analytical is the way in which a model user decides to interpret and use it: a model could be *interpreted* as belonging to various model type-target-goal combinations.

GPSS emphasise 'analytical models', arguing that while economists often interpret their models this way, this interpretation has not received adequate attention. They introduce the concept using an example of a physical model, a maquette. A maquette, they say, can be interpreted in three ways: (a) as a positive model that describes an existing space like a town hall, (b) as normative model that illustrates a proposed design for the space, and (c) as an 'analytical model' that tests "the feasibility of a possible" design for the town hall. GPSS argue that even though the 'analytical' interpretation "does not have a widely accepted, 'official' title, […] economists often refer to this type of reasoning in explaining the value of their models" (Gilboa et al. 2022a, 4). GPSS also note that economists who propose an 'analytical model' often do not claim that it is the 'correct' model, but they see some value in it because it helps them *test whether a given result is consistent with a set of*

assumptions. As they see it, 'analytical models' can serve at least three goals: explanation, critique, and 'methodology'. Let us have a closer look at these goals.

2.1. Explanation

GPSS argue that models that have no use in prediction and recommendation might still have value as *explanatory* models that improve understanding, producing "a sense of understanding" or a "warm feeling inside" (Gilboa et al. 2022a, 7). Although GPSS are not very clear about this, it appears that both positive and 'analytical models' can spark this vague sense of understanding. How do unrealistic models do this? In a variety ways, according to GPSS. First, models might function *as theoretical cases* in that economists often learn from them "by drawing analogies between the model" and the real world "problem" (Gilboa et al. 2014, F513). In this sense, models enrich the pool of resources that economists use to draw *analogical inferences* from to explain phenomena. Second, 'analytical models' can contribute to the goal of explanation since they can help in "testing whether the standard assumptions are compatible with the phenomenon at hand" (Gilboa et al. 2022a, 5) and showing how "seemingly anomalous observations are indeed consistent with the common modeling framework" (Gilboa et al. 2022a, 6). GPSS argue that 'analytical models' can lead to "surprisingly simple *potential* explanations" (Gilboa et al. 2022a, 6, emphasis added). For similar reasons, they can also serve as tools for critique.

2.2. Critique

GPSS also argue that models can be used as *tools for critique* (Gilboa et al. 2018). For example, a model can help economists reject a given policy proposal, or at least raise questions about its viability, by showing that it cannot be accommodated within the preferred framework of economists, or that it is not consistent with a reasonable set of assumptions. Although GPSS are not clear about the differences, their discussion implies that both positive and 'analytical models' can be used for critique. The way 'analytical' seems to differ from the 'positive' is that while the latter starts from a description of some real-world target, the former does not. According to GPSS, 'analytical models' can also help in showing the availability of an alternative policy and highlighting the conditions under which a policy proposal can or cannot achieve its goals. By the same token, GPSS argue that models can be "powerful rhetorical devices" (Gilboa et al. 2019, 340; 2022b, 907).

2.3. 'Methodology'

In GPSS's view, models need not be only about economic phenomena. They can also be useful for examining economics and economic models, contributing to *economic 'methodology'*. In GPSS's account, 'methodology' is mostly the domain of 'analytical models' but they leave the possibility that other 'types' of models can be used for 'methodology' open. 'Analytical models' contribute to 'methodology' by

helping analyse the consistency of a given set of assumptions with a (set of) result(s) that economists find interesting (Gilboa et al. 2022b). For example, modelling can show whether the assumptions are too strong or too weak for a given result or whether a different set of assumptions can produce a given result . Since 'analytical models' are useful for doing all this, GPSS argue that their value should not be judged by how realistic their assumptions are but by how well they serve the goal of analysing economics and economic modelling.

In sum, GPSS argue that even though a model might look useless when interpreted as a positive or a normative model, it can still have value as an 'analytical model' that contemplates whether a given result or observation is consistent with a set of assumptions—a task which also advances the economist's goal of a unified theory.

3. Similarity's crucial role

GPSS neither aim to resolve the philosophical puzzles surrounding the epistemic value of models nor wish to reveal their own views about what justifies model-based inferences in economics. Nevertheless, their publications hint at their stance on what makes model-to-world inferences possible*: the similarity between the model and its real-world target*. This claim is supported by their model of economic modelling (Gilboa et al. 2018) and discussion of models as analogies (Gilboa et al. 2014).

3.1. A model of economic modelling

GPSS's model of economic modelling (henceforth, MoEM) is presented as a general model of modelling that can account for different interpretations of models in economics (Gilboa et al. 2018). In MoEM, GPSS characterise a *model* as a *description* that contains sets of entities E, a set of predicates F "that are used to attribute properties to entities or to establish relationships between them," and a function d that specifies which predicates apply to which entities (Gilboa et al. 2018, 370). In a mathematical model, E and F would "denote the abstract notation" used by the economist (Gilboa et al. 2018, 370). On the other hand, in GPSS's account, a description of reality contains real-world entities E_r and their properties and relationships (F_r, d_r). Finally, modelling is represented as consisting of a description of reality (E_r, F_r, d_r), a model (E, F, d) and a mapping between the two, which is conceived of as *abstraction*. In an abstraction, "unimportant aspects of reality are excluded" while those aspects that seem important to the modeller are

included (Gilboa et al. 2018, 374).[1] GPSS assume that acceptable abstractions in the field are given (Gilboa et al. 2018, 376).

Figure 1 shows GPSS's depiction of "how a model and a theory are used to draw conclusions about reality" (Gilboa et al. 2018, 378). The process starts from a 'description of reality'. A model is built by abstraction in that some of the things we observe in the real world are incorporated into our model, while some are omitted. The second step involves a theoretical inference, which is about models, not about the description of reality. The theory tells us whether we can *extend* the model or not. As I understand it, theoretical inference in GPSS's account allows one to say 'if you observe such and such conditions or results in your model, such and such other things must be the case'—using the resources of the theory; based on what we know from various other models. To be clear, GPSS do not assume that theory will always lead to an extension of a model. In any case, the third step involves what GPSS call *interpretation*. If abstraction is a function that transforms the description of reality into the model, interpretation is the reverse of that, which gives us an extended description of reality. In sum, according to MoEM, one can make inferences about reality based on a model and theory.

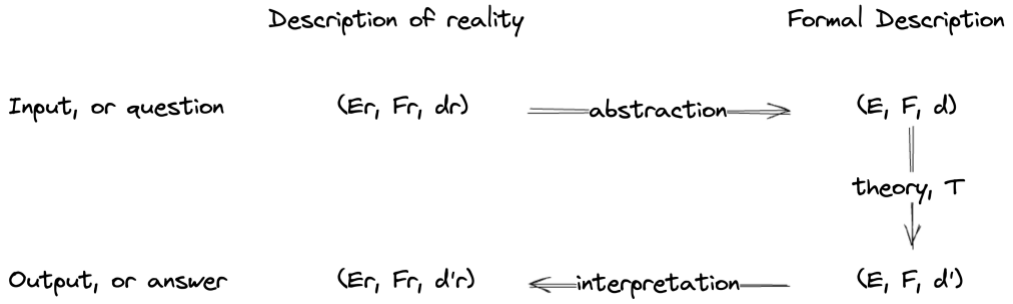

Figure 1. GPSS's MoEM (adapted from Gilboa et al. 2018, 378)

The important point for us is that the whole process is modelled with functions (i.e., abstraction and interpretation) that map the 'description of reality' to the model, the model to its theoretical extension, and the extension to the extended 'description of the reality'. This *mapping*, which is the *similarity* between the model and real-world target in this sense, makes model-based inferences about the real world possible.

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[1] A better term for what they mean is *isolation* (Mäki 1992, 321), which also allows for different levels of abstraction and generality—in line with what GPSS wish to capture.

GPSS recognize the difficulty with justifying the use of economic models with 'similarity' in this specific sense of a mapping between the model and a description of the real world due to a peculiarity of economics: economists do not always start modelling from a 'description of reality' (Gilboa et al. 2018, 374). Instead, they sometimes operate in reverse and start from an existing model, a modelling framework, or a theory. They then build a model, study it, modify or amend it, and finally derive the lessons learned from this analysis which might or might not include some indicative discussion of some possible real-world applications. This practice, which we might call 'analytical modelling' following GPSS, complicate the problem of inference since there is no explicit attempt to represent a real-world target involved. However, GPSS argue that economists find this practice valuable. Why? First, according to GPSS, such models could be interpreted as works in 'methodology'. As we will see, GPSS's views on 'methodology' can be interpreted in at least two ways. Under a narrow reading, 'methodology' is not about economic phenomena and does not involve inferences about the real world. Hence, similarity is not an issue. However, under a broader interpretation, 'methodology' also concerns statements about the real world, and a judgement of similarity will be required. Leaving this discussion to Section 4.2, the second reason why GPSS think that economists value such models is that they can serve as analogies or theoretical cases. However, as the next section will show, GPSS's depiction of models as analogies also relies on similarity—albeit with a vaguer concept of similarity.

3.2. Models as analogies

GPSS's account of economic models as analogies or tools for case-based reasoning (Gilboa et al. 2014) provides further evidence that they think that model-to-world inferences require similarity judgements. In this view, each model can be conceived as a *model world* created by an economist as a *thought experiment* (Gilboa et al. 2014, F517; cf. Morgan 2002; Mäki 2005) that others can observe and learn about. More model worlds mean more observations for economists; observations that are on a par with observations about the real-world, according to GPSS (Gilboa et al. 2014, F520).

For example, in discussing Akerlof's (1970) *The Market for "Lemons"*, they argue that Akerlof's model can be considered as a *theoretical case* where we can observe how idealised agents behave under the conditions specified in the model (Gilboa et al. 2014, F518). Put differently, when we study Akerlof's fictional automobiles market, which is populated by rational agents, we can observe how these agents behave under the conditions of symmetrical and asymmetrical information about the quality of the cars on the market, and see that under certain conditions lower quality cars will drive out the good cars from this fictional market (Akerlof 1970, 489–90).

How can we learn anything about the real world by studying Akerlof's model world? On what basis do we see ourselves as entitled to carry what we learn from the model world into the real world?[2] GPSS's answer is *similarity*.

> The relevance of this observation for prediction will depend on the perceived *similarity* between the idealised agents and the real agents one is concerned with, the *similarity* between the situation of the former and that of the latter, and so forth. An economist who is interested in real agents would therefore *have to judge* to what extent the situation he studies *resembles* the idealised situation in the 'case' reported by Akerlof […]. (Gilboa et al. 2014, F518, emphasis added).

It is clear from this passage that GPSS think that it is the similarity of a model to its target that entitles its users to make model-based inferences about a real-world target. However, in contrast to their discussion in MoEM, what similarity amounts to here is not clear. GPSS argue that similarity judgements are not merely judgements about the realisticness of assumptions, but might also involve assessments of how well the story that unfolds in the model world fits the real-world case at hand (cf. Sugden 2000). According to GPSS, a similarity judgement can also consider steps of the formal proof of a model result and ask whether they are similar to past or present real-world cases (Gilboa et al. 2014, F521-2). However, these remarks do not make this second conception of similarity any clearer. With this vague conception of similarity, anything can be said to be similar to anything else in some respect, causing similarity to lose its usefulness for most practical purposes.

This vagueness does not seem to worry GPSS, who argue that it is the task of the practitioner (i.e., model user) to make a similarity judgement and use the model accordingly (Gilboa et al. 2014, F519). They admit that economists do not provide enough guidance (Gilboa et al. 2022a, 9), but argue that the absence of guidance is not a good reason to dismiss a model. GPSS contend that even if there is no guidance about a model – and even if no one has yet found it similar to any real-world target – it will be added to the pool of theoretical cases waiting to be used

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[2] I use "model world" and "real world" to conveniently distinguish between the simplified scenarios of economic models and the more complex world we live in. This choice was made to facilitate easier reference and discussion with the aim of enhancing the readability of the text. However, this choice of words does not imply that these two worlds are completely independent or unrelated.

someday by someone who might find it (or parts of it) similar in some sense in some relevant ways to the real-world problem they are working on.[3]

We can conclude that in GPSS's view model-based inferences about economic phenomena (i.e., the real world) require similarity either in the sense of a mapping between the model and a description of the real world or in the vaguer sense of some form of resemblance between the two.

3.3. Missing idealizations and holistic distortions

GPSS's vaguer notion of similarity does not do much in helping us understand how unrealistic models can justify inferences about real-world phenomena. It just tells us that economists will use a model to make inferences about real-world phenomena when they think it is similar in some sense to the real-world problem at hand. GPSS's model of economic modelling (MoEM), on the other hand, uses a more precise notion of similarity (a mapping) and helps better to see how the results of a theoretical model can be carried to the real world. However, it has another important shortcoming: it overlooks the complications brought about by idealizations that distort representations.

MoEM conceives of models as abstractions or isolations from 'descriptions of reality'. Although this view contains a grain of truth, it is nonetheless an oversimplification. First, it assumes that both the target and the model are modular; that is, we can selectively pick up parts of the target, and represent these parts with a model without losing relevant information. Second, it assumes that the mapping between the model and the target is straightforward since it results from a series of abstractions. Third, it ignores the complications created by *idealizations* that might distort the picture of reality. Models not only selectively represent real-world entities and their attributes, but they also employ several idealizing assumptions such as perfectly competitive markets, rational utility maximizing agents and zero transaction costs (Mäki 1992). Moreover, they involve several idealizing assumptions which are employed for mathematical convenience or tractability. Fourth, and correspondingly, MoEM appears to assume that idealizations do not distort the representation of the relevant parts of the target. That is, it seems to assume that idealizations only influence the irrelevant features of a model and have no effect on what can be learned from the model. These assumptions are strongly contested in philosophy of science (e.g., Knuuttila 2005; Morgan and Knuuttila 2012; Grüne-Yanoff 2011; Potochnik 2017; Rice 2021). For example, speaking of idealized models in general, Colin Rice argues,

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[3] Note that there are also costs associated with having too many models of this sort. In particular, this might increase the difficulty of model selection (Grüne-Yanoff and Marchionni 2018).

> […] the model's idealizations will often distort difference-making (that is, relevant) features of the model's target system(s). As a result, we cannot map the accurate parts of the model onto what is relevant and its inaccurate parts onto what is irrelevant. (Rice 2019, 194)

If models that employ many unrealistic assumptions are holistically distorted representations (Rice 2021) and if the idealizing assumptions are not easily eliminable from such representations (Batterman 2009), the mapping (similarity) between some parts of a model and its target will not be enough to justify model-based inferences. Moreover, one must also note that the criticism of the uses of models in economics is not always as simple as "these assumptions are false." It is also argued that since economists do not have many universal laws or general principles to rely on in their models and they employ many interlinked assumptions to secure the deductive validity of their models, it is hard to carry the conclusions of a model (i.e., what we learn from a model) into the real world (Cartwright 2007). In response to this last claim, one could argue that robustness analysis can increase our confidence in models by showing that a given model result is not an artefact of the idealizing assumptions (Kuorikoski, Lehtinen, and Marchionni 2010). However, holistic distortions and ineliminable idealizations also reduce the reliability of robustness analysis (Lisciandra 2017; Aydinonat forthcoming). To complicate this picture further, note that economists find some non-robust models such as Hotelling's (1929) location model very valuable (see Aydinonat and Köksal 2019).

In sum, according to GPSS, economists justify their model-based inferences about the real world by appealing to similarity. It seems that GPSS also believe that similarity justifies such inferences to some extent. I have noted, however, that the two notions of similarity they use have shortcomings: one is too vague to be useful and the other overlooks the role of idealizations and other difficulties with model-based inference. In the next section, we will see that model-based inferences—such as model-based explanation, policy advice, and critique—commonly have a more complicated structure than GPSS admits, and the problem of similarity is only a small part of it.

Before going further, I should note that this discussion does not indicate that there is something inherently wrong about the similarity view of models. Philosophers of science provided more nuanced accounts of what a model's similarity to its target amounts to (e.g., Giere 2004; Strevens 2008; Mäki 2010; Weisberg 2013). These accounts pay more attention to the goals of the model user, the task at hand, model commentary, and the context of modelling (Aydinonat forthcoming). Given GPSS's emphasis on the variety of modelling goals, their account might benefit

from a deeper engagement with the philosophy of models and representation (e.g., Frigg and Nguyen 2017).

4. Models as argumentative devices

Recall GPSS's claim that models are "rhetorical devices" (see Section 2.2.). This is a claim that they do not elaborate enough, but if I understand it correctly, they are saying that models are used to make a variety of arguments in economics. In this section, I present a sketch of an account of models that conceives them as arguments (Beisbart 2012; also see, Norton 1991; 1996) and argumentative devices (Aydinonat, Reijula, and Ylikoski 2021). This account will serve to introduce a perspective that aids us in navigating GPSS's distinctions and understanding the problem of model-based inference more clearly. Furthermore, I aim to demonstrate that being explicit about the structure of model-based arguments helps clarify what a model can and cannot convey. This approach offers an x-ray view of model-based inferences, revealing argumentative leaps and jumps.

4.1. Model as an argument

Let us start with the simple claim that the derivation of a model result can be represented as an argument: it will have the following form, where C is the conclusion, and *i* and *ii* are the premises.

i.	Assumptions of the model.
ii.	Mathematical theorems and logical principles.
C.	Model results (theorems).

For example, in international trade theory one can derive *the factor price equalization (FPE) theorem* (model result) from the assumptions employed by the Heckscher-Ohlin (HO) model (Samuelson 1948; 1949). This model result basically tells us that the factor prices will be equalized *in the model world* defined by the assumptions of the model. Typically, the HO model world has two countries, two commodities, and two factors of production, where each commodity is produced with two factors of production but will differ in the how intensively they use these factors. In this world, we have constant returns to scale and diminishing marginal productivity. Each country uses identical factors of production with the same technology. Commodities can move freely and without cost between countries, but factors of production are only completely mobile within countries, not between them. Also in this world, countries cannot completely specialize in the production of one good; they both produce both goods even after specialization. Given these conditions one can derive the result that after international trade "real factor prices must be exactly the same in both countries" (Samuelson 1949, 182). We can show how the result (FPE) is logically derived from the assumptions as follows.

i.	Assumptions of the HO model world (see above).
ii.	Mathematical theorems (e.g., Euler's theorem, see Samuelson 1949, 182) and logical principles.
C.	"factor prices must be exactly the same in both countries" (Samuelson 1949, 182).

Representing the derivation of the result of a mathematical model as a logical argument should be uncontroversial and in line with how economists see their models (e.g., see Samuelson 1949, 182–83, who argues that whether the model result holds or not is a purely logical question). It is clearly in line with GPSS's efforts to develop a model of economic modelling. It should also be clear that *conclusions derived from the premises of the model will be true only in the model world*—unless once can show that its premises are also true as statements about the real world.

Now let us see what 'methodology' in GPSS's view would look like as an argument.

4.2. 'Methodology' as an exploration of model worlds

According to GPSS, 'methodology' is primarily about discovering what conclusions one can derive from a given set of assumptions: "showing what a set of assumptions implies, or does not imply" (Gilboa et al. 2022b, 899). One of their examples is Arrow's (1950) impossibility theorem that according to GPSS asks whether there is a "function aggregating profiles of preference orders while satisfying certain (arguably intuitive) properties" and shows that there is no such function (Gilboa et al. 2022b, 900). This is in line with Arrow's formulation of the task, who asks whether "it is formally possible to construct a procedure for passing from a set of known individual tastes to a pattern of social decision-making, the procedure in question being required to satisfy certain natural conditions" (Arrow 1950, 328).

Here, as GPSS see it, one starts from a given model result and investigates whether it can be deductively derived from a given set of premises, including those stating the desiderata. That is, one asks whether it is possible to make the following argument.

i.	Definitions of the terms to be used (e.g., x, y, z, R_i, R, xRy, xR_iy in Arrow's exposition).
ii.	Assumption: individuals are rational in that individual ordering relations satisfy Arrow's Axioms I & II (1950, 331–32)
iii.	Definitions concerning the Social Welfare Function (SWF), which takes as input the *individual* orderings for alternative social states and produces as output a *social* ordering (1950, 335).

iv.	Premises stating the desired criteria, i.e., Arrows's *Conditions 1-5* (1950, 336–39). (1) "for some sufficiently wide range of sets of individual orderings, the social welfare function gives rise to a true social ordering." (2) "the social ordering responds positively to alterations in individual values or at least not negatively" (1950, 336). (3) independence of irrelevant alternatives (1950, 337). (4) individual freedom to choose or, no imposed choices (1950, 338), and (5) non-dictatorship (1950, 339).
?C	SWF that satisfies Axioms I & II and Conditions 1-5 exists.

This simplified presentation aims to make the simple point that Arrow is addressing a purely logical question: he shows that it is not possible to derive C from *i-iv*. GPSS present Arrow's model as a primary example of 'methodology'. It should be clear that 'methodology' in this case does not look much different from a model in terms of the argumentative structure.

Another example of 'methodology' in GPSS is Milgrom and Stokey's (1982) no trade theorem, which is the result of a deductive argument. Milgrom and Stokey use their model to show that the common knowledge assumption is crucial for the derivation of the result that "the receipt of private information cannot create any incentives for trade" (Milgrom and Stokey 1982, 1). As GPSS also argue, the no-trade result is not a statement about the real world, but a statement about the model world, showing that it is not possible to explain trade *within the model world* where agents have rational expectations and common knowledge. In this case too, 'methodology' does not look much different from modelling.

While it may seem trivial or obvious to reconstruct Arrow's or Milgrom and Stokey's models as logical arguments, doing so shows us one way in which we can interpret GPSS's view of 'methodology'. In both cases, neither the conclusion nor the model result is a statement about the real world: we merely learn about the logical implications of the model's assumptions—not about the real world. 'Methodology' viewed in this way does not lead to the epistemic problem that is the primary concern of philosophers since there is no model-based inference that concerns real-world phenomena and no statement about the real world. In this sense, if economists find 'methodology' valuable it can only be because 'methodology' improves the understanding of models or model worlds, and nothing more.

Even though GPSS present 'methodology' as being predominantly about proving theorems and deriving new model results, they also say that theoretical work can be considered as contributing to 'methodology' if it is "making a statement about the models used by economists" (Gilboa et al. 2022b, 904). Interpreted this way,

'methodology' would involve a wider variety of arguments in economics. First, most derivational robustness analyses (Woodward 2006) would qualify as 'methodology' since they inquire whether a given result can be derived from a slightly different set of assumptions (see Kuorikoski, Lehtinen, and Marchionni 2010, 547 who argue that "robustness analysis is a crucial methodological strategy" in economics). A template for such an argument could be the following.

i.	Model X employs a set of assumptions A and proves a result R.
ii.	Model X' which relaxes one of the tractability assumptions of Model X, shows that R no longer holds.
C.	The result of Model X, R, is sensitive to changes in its assumptions, A.

In this argument, the concluding statement is about a model and does not include any statement about the real world. The difference from the previous cases is that the argument involves at least two models, the original and the modified one. Note, however, that if robustness analysis is used as a tool to ask whether we can confidently use a model's results to support statements about the real world, then 'methodology' cannot be said to be only about model worlds anymore. I explore this point further below (also see Section 4.3).

Second, "making a statement about the models used by economists" (Gilboa et al. 2022b, 904) could also be interpreted as involving statements about a model's representational adequacy, generality, or an economist's uses or misuses of a model for a given task. Under this broader interpretation 'methodology' would also extend to relations between models and the real world, and to statements about the real world. Consider the following example.

Hotelling (1929) uses a model, Model H, to explain why "buyers are confronted everywhere with an excessive sameness" (1929, 54). Using this model, which assumes a fictional bounded linear market, Hotelling claims to show the stability of prices in duopoly (stability of competition) and illustrates that as a result of competition, profit maximizing sellers will locate in the middle of the market next to each other (minimum differentiation). However, on a closer examination of Model H, it can be shown that "nothing can be said about the tendency of both sellers to agglomerate at the center of the market" because "no equilibrium price solution will exist when both sellers are not far enough from each other" (D'Aspremont, Gabszewicz, and Thisse 1979, 1145). With a modified version of Hotelling's model, Model H', which assumes a quadratic transportation cost function rather than a linear function, one can derive the stability of competition, but with this slight modification the minimum product differentiation result no longer holds (D'Aspremont, Gabszewicz, and Thisse 1979). Hence, based on Model H', it can be argued that Hotelling's model *fails to explain* minimal product

differentiation (in the model world) and any inference based on the model should be carefully examined. This argument can be represented thus:

i.	Model H employs a set of assumptions A and proves a result R.
ii.	Model H' which relaxes one of the assumptions of Model H, assuming a quadratic transportation cost function rather than a linear function, shows that R no longer holds.
C.	The result of Model H, R, is sensitive to changes in its assumptions, A.

This example is derived from a famous article by Claude d'Aspremont et al. (1979), who set their task as carefully studying Hotelling's original model. They test the logical consistency and the robustness of Hotelling's model but do not make any claims about examining economic phenomena *directly*, neatly fitting GPSS's 'methodology' definition. However, their model has implications about how one might use Hotelling's model for real-world tasks. Based on their model, Claude d'Aspremont et al. conclude:

> The preceding example, far from confirming the minimal differentiation principle, suggests that this principle cannot be based on spatial competition. Certainly many *comments derived from Hotelling's contribution should be carefully reexamined* before taking them as granted (D'Aspremont, Gabszewicz, and Thisse 1979, 1149, emphasis added)

Note the additional statement ("comments derived from Hotelling's contribution should be carefully reexamined") that Claude d'Aspremont et al. make based on the result of their analysis, which presumes that the non-robustness of a model reduces the confidence in inferences based on it. Comments derived from Hotelling's model contain claims concerning the model world such as the stability of competition as well as statements that relate to the real world such as the ability of a model to explain or provide reliable policy advice. Thus, under this broader interpretation, 'methodology' could be about a model's ability to function as advertised and to teach us about the real world.

If we accept this broader definition of 'methodology' many important works in economics will fall under it. For example, consider the Coase theorem. The *argumentative strategy* in Coase's renowned article can be summarized as follows:

(i) *Statement of the claim to be examined.* Using a model of perfect competition where transaction costs are zero, Pigou (and others) argue that the problem of negative externalities can be solved by assigning liability for the

externalities, taxing the producer of the externality, or restricting the activity causing the externality (Coase 1960, 1).

(ii) *Model-based demonstration: the claim does not follow from the stated premises.* The two models presented by Coase (1960, 2–8) show that "in a regime of zero transaction costs, an assumption of standard economic theory, negotiations between the parties would lead to those arrangements being made which would maximise wealth and this irrespective of the initial assignment of rights" (Coase 1992, 717).

(iii) *Rejection of a key premise.* "[…] that there were no costs involved in carrying out market transactions […] is, of course, a very unrealistic assumption." (Coase 1960, 15)

(iv) *Casting further doubt.* Discussion of the shortcomings of the Pigovian argument, together with an examination of several cases (Coase 1960, most of the article).

(v) *Rejection of the Pigovian claim.* Given that *i-iv* cast serious doubt on *i*, "both the analysis and the policy conclusions which it [the Pigovian tradition] supports are incorrect" (Coase 1960, 39).

(i) *Statement of the methodological claims.* "[…] the failure of economists to reach correct conclusions about the treatment of harmful effects […] stems from basic defects in the current approach to problems of welfare economics. What is needed is a change of approach" (Coase 1960, 42).

Based on this argument, Coase further argues that designing policy for negative externalities (i.e., to solve real-world problems) requires studying the "the world of positive transaction costs" (Coase 1992, 717). Since Coase is presenting two simplified models to make "a statement about the models used by economists" (Gilboa et al. 2022b, 904) and a comment on how economics should be done, his contribution can be interpreted as 'methodology' in GPSS's terms. However, in this case, 'methodology' is used to reject statements about the real-world (e.g., the problem of real-world pollution can be remedied with taxes) as well as to suggest better ways to approach real-world problems. Note also how Coase uses modelling as a part of a complex argument and how 'methodology' in this case is like *critique* in GPSS's terms (see Section 4.3.3 below).

This exposition also makes it clear that, whether a modelling attempt is interpreted as 'methodology' will depend on many factors, including the interests of economists at the time, the theoretical framework, and the modelling goals—which is in line with the GPSS comment that context including "the current state of the discipline and the perspective of the reader" matters (Gilboa et al. 2022b, 912). However, this broader interpretation of 'methodology' blurs the distinctions that

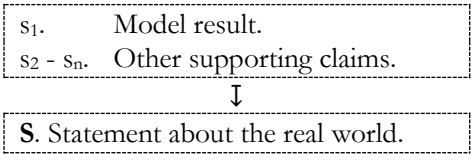
GPSS make between 'methodology' and economics proper, and between 'methodology', critique, and policy advice.

In sum, reconstructing examples of 'methodology' as arguments helps us see the differences between the narrow and broad interpretations of 'methodology' in GPSS's account. It will also serve as a stepping stone to show that arguing about theorems and making real-world assertions—such as those in explanations and policy advice—represent fundamentally different challenges. The latter often involves many additional steps, each susceptible to potential flaws and leaps in reasoning. The next section provides a more detailed examination of this contrast.

4.3. Models and statements about the real world

In GPSS's view 'analytical models' can also be used to ask "whether the standard assumptions are compatible with *the phenomenon at hand*" or investigate the "the consistency of the assumptions with some *stylized facts*" (Gilboa et al. 2022a, 5, emphasis added). In our terms, 'analytical models' can be used to derive, to support, to infer *statements concerning the real world*. First, notice the additional step required to make such *model-to-world inference*s. A model only shows that a set of *model results* can be deductively derived from its premises. To use the model to derive or support a statement about the real world, one would at least need to show why the model is relevant for the case at hand. Second, note that in contrast to the case of models as arguments, model-based arguments that involve model-to-world inferences are not necessarily deductive arguments. They are arguments in a more general sense, providing reasons why a given claim is reasonable or justifiable. Evaluating such arguments requires not only the evaluation of the validity of the model, but also assessment of the other supporting statements or claims.

Consider the HO model again (see Section 4.1). Can we say that there will be complete factor price equalization in the real world *solely* based on the HO model? It should be obvious that the answer to this question is negative because of the restrictive assumptions employed by the model. Other premises are needed, which could include statements concerning the similarity of the model world to the real world, empirical tests and observations, and perhaps other models that derive the same result under different assumptions. In any case, the argument will look like the following.



The strong assumptions employed by the HO model do not allow us to carry the model's result (complete FPE) into the real world unaltered. For example, knowing that products cannot move completely freely and without a cost in the real-world, and that countries use different technologies in production, one can conclude that it is *unlikely* to have complete factor price equalization in the real world. Nevertheless, one might still be inclined to argue that the HO model supports the modified statement that as a result of international trade factor prices will converge (factor price convergence, FPC). Such an argument could have the following structure:

s_1. HO model shows that factor prices will equalize as a result of international trade in the model world.
s_2. The model's assumptions concerning behaviour of economic agents in response to international trade are reasonable.
s_3. The model also makes some strong assumptions concerning trade and production that cannot be expected to hold in the real world.

↧

S. Complete FPE cannot be expected in the real word, but FPC is likely.

In this rather simplified example, the concluding statement S is supported by the model result and an evaluation of the reasonability of its assumptions (i.e., similarity judgements). The model user weakens the model result by presuming that its strong assumptions will prevent complete FPE and settles for FPC, making an argumentative jump without providing other reasons. We are not even told whether relaxing the assumptions will bring about FPC in the model. It has been argued that robustness analysis can strengthen such arguments (Kuorikoski, Lehtinen, and Marchionni 2010). For example, if the relaxation of some of the strong assumptions of the model implies FPC but not complete FPE, this might increase one's confidence in the model-based inference. Consider the following.

s_1. The HO model implies complete FPE (result of the original model).
s_2. Modified HO models with relaxed (more realistic) assumptions do not imply complete FPE, but FPC (results of multiple models; robustness analysis).
s_3. The core assumptions of these models that 'explain' the movement of prices are reasonable (similarity judgement).

↧

S. Complete FPE cannot be expected in the real word, but FPC is likely.

The argument from robustness says that s_2 increases our confidence because it gives information about the conditions under which FPE and FPC hold. Put differently, 'methodology' (in GPSS's sense) can indeed contribute to model-based inferences if it is used as input in another argument that concerns the real world.

It helps economists understand better the conditions under which a given result holds, increasing their confidence in their models. However, note that s_1 and s_2 are a collection of model results; i.e., inferences to statements about model worlds. Hence, they cannot be sufficient to change our confidence about a statement concerning the real world. Other premises are required. These could be statements connecting the set of models to the real world. In the argument above, we have statements about robustness and similarity. The question is whether the conclusion *S* is warranted solely based on these. Because of the abstractions and idealizations employed – or, in GPSS's terms, because of "false" assumptions – the answer to this question is likely to be negative unless we have additional good reasons to believe that the model worlds that we explored are reliable guides to what happens in the real world. For example, if one has good reasons to believe that the *invisible-hand mechanism* (Aydinonat 2008) that forms the backbone of HO models is working in the real world, one will have more confidence in the argument. Needless to say, such good reasons must be based on some empirical evidence.

> "Economists who believe that markets work well (or better than any alternative) believe the general argument that free trade will tend to equalize the return to similar factors internationally. Economists who are suspicious of markets will not trust free trade to create a more equitable international distribution of income. For each group, empirical testing may be more critical than theoretical refinement." (Rassekh and Thompson 1993, 11)

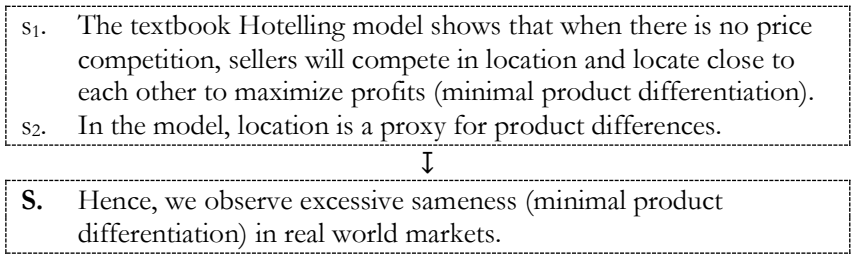
In sum, when S is a statement about the real world and the model involves many idealizations, it is difficult to justify the model-based inference on the grounds of similarity in the sense of a mapping between the elements of a theoretical model and its target *alone*. Empirical evidence, however, can strengthen model-based inferences and help "identify circumstances in which the model is useful and other circumstances in which it is misleading" (Leamer 1992, 2).

Recap: similarity – in the sense of a mapping between a model and its target – alone is not a good justification for model-based inferences; robustness analysis could sometimes help, but even in this case some empirical support would be necessary to jump from model worlds to the real world. Recall that in GPSS's framework, model-based inferences include a *theoretical inference* (see Figure 1, Section 3.1). If the multiple models (e.g., due to robustness analyses) and accumulated knowledge concerning empirical work can be interpreted as being parts of the accepted theory, we can perhaps argue that additional steps involved in model-based inference can be interpreted as cases of theoretical inference in GPSS's terms. One must admit, however, that if this is what GPSS wished to argue, they have packed a lot into the notion of 'theoretical inference'. The perspective offered here, on the other hand, aims to unpack all that is required for the model-based argument to work. It helps

us see that when a model is used to make inferences about the real-world many additional premises, including those expressing empirical support, will be needed to arrive at a sound argument. This contrasts with the case of models as arguments (Section 4.1) where everything needed to derive the model result was included in the model. As we will see in a moment, such unpacking contributes to a better understanding of the structure of model-based explanation, policy advice, and critique. Let's start with explanation.

4.3.1. Explanation

Conceiving of a model as a deductive argument makes it easy to see that *a model explains its own results* by showing how they can be derived from its premises. The HO model explains why FPE holds in the HO-model world, the textbook Hotelling model explains the minimum product differentiation in the linear bounded market in the model world, etc. Can the HO model explain factor price movements in the real world? Can the Hotelling model explain the excessive sameness we observe in real world markets and tell us why products in the market are extremely similar to each other (think of the SUVs, jeans, and even cafeterias that look like the same, but not exactly the same)? We have already seen that model-based inferences supporting statements about the real world often require additional premises that are not a part of the model and that they do not necessarily have the argumentative structure of a deductive argument. Explanation is no different. Consider the following example.



If this argument is supposed to explain why we observe excessive sameness in the real world, it is missing quite a lot. If we were to conceive of an explanation as a deductive argument, the explanation above would at best be considered as an incomplete potential explanation in the Hempelian sense (Hempel 1965). Potential because we do not know whether the premises of the textbook Hotelling model are true. Incomplete because it is missing premisses that are needed to logically derive S; it is elliptically formulated, partial and sketchy. If we were to adapt a causal account of explanation, this explanation would not be satisfactory because it does not provide a clear account of causal dependencies that bring about the explanandum. The model suggests some potential explanatory dependencies, but it is not shown that the minimum product differentiation is the result of these dependencies. That is, the explanation above is an incomplete potential explanation

in a more general sense. One plausible way to describe what the model does in this explanatory context is to say that model offers a set of factors (*explanans*) that *could* explain the phenomenon to be explained (*explanandum*), and a *sketch* of what the explanation might look like. It could be interpreted as offering a *how-possibly explanation* (Ylikoski and Aydinonat 2014) or an *open formulae* that can be used to formulate an explanatory hypothesis (Alexandrova 2008).

In their work, GPSS also mention in passing that when a model is interpreted as an 'analytical model' it is likely to produce a potential explanation: one starts with an observation to be explained and then tries to find a model, a set of assumptions, that are consistent with the given observation (e.g., see Gilboa et al. 2022a, 11). Our discussion shows that this task is more complex than it first seems because of the *gap* between what the model can prove (i.e., model result, a statement about the model world) and a given statement about the real world. Without additional premisses the potential explanation is likely to be incomplete at best. Showing that the model result can in fact accommodate a given observation requires figuring out the additional premises that warrant the conclusion—if the premises are true. Providing a *true* explanation requires more steps. Arguing for the similarity of the model to its target may be one of these steps, but often this will not be sufficient. Consider the following example.

Suppose that we have observed that competing airlines schedule their departure times close to each other and proposed the following explanation:

s_1. The textbook Hotelling model shows that when there is no price competition, sellers will compete in location and locate close to each other to maximize profits (minimal product differentiation).
s_2. The Hotelling model makes some reasonable assumptions about the behaviour of sellers (similarity judgement).
s_3. The schedules of the departing flights can be interpreted as locations on a timeline akin to the locations of sellers on a straight line in the model (interpretation based on a similarity judgement).
s_4. The competing airlines only compete in price and location, and nothing else (idealizing assumption).
s_5. If the airlines cannot compete in price, they will compete in location, and vice versa.
s_6. In the observed period, a regulation (fact) impeded price competition.

↧

S. The competing airlines scheduled their flights close to each other—because they were not able to compete in price.

The important thing to notice in this made up but otherwise reasonable explanation is the number of additional steps (including additional idealizations such as s_4) required to move from the model result to the explanation. Note that

even with these additional steps, it is hard to argue that this explanation is complete It remains a *potential explanation* since the truth of the premises is yet to be determined. To arrive at a true explanation, one needs to check the truth of the premises, verify that the critical assumptions of the model hold, and the mechanisms identified by Hotelling are *actually* bringing about the minimum differentiation we observe. This would require some empirical work (e.g., as in Salvanes, Steen, and Sørgard 2005; Borenstein and Netz 1999). When GPSS argue that economists find their models valuable because models can be used to explain, they do not sufficiently emphasize the 'distance' between the statement of the model result and the statement about the real world that needs to be explained. Model-based explanatory inference is more complex than it appears in GPSS's work. What makes it even more complex is the fact that economists often use multiple models to explain, and arriving at a good explanation often involves several steps where models play multiple roles, including the production and testing of potential explanations (Aydinonat 2018).

It is perhaps because GPSS do not take these complexities into account that they equate explanation to "sense of understanding" or "a warm feeling inside" while they think of prediction in terms of "accuracy" (Gilboa et al. 2022a, 7). Reconstructing model-based explanation as an argument – that gives reasons why it is likely to be true – not only helps to see the gaps and jumps in argumentation, but also shows that explanations can be factive and an ideal explanatory relation – i.e., between the *explanans* (what explains) and *explanandum* (what is to be explained) – is an objective relation that can be evaluated using objective criteria instead of subjective 'a-ha' moments or 'warm feelings' (on "understanding" versus "sense of understanding", see Ylikoski 2009).

4.3.2. Policy advice

Policy advice or recommendation can be viewed similarly. Consider the simple model that forms the backbone of Peltzman's (1975) famous empirical study of the seatbelt regulation. According to the model, increasing road safety would decrease the cost of driving and hence will lead to an increase in driving speed and more accidents. Now consider an economist who is arguing, using this model, that the net benefit of the safety regulation will be very small at best—since increasing the safety of the cars would increase the number of accidents (hence the costs). Based on this model-based inference, suppose that the economists' policy advice to the government is to cancel or postpone the regulation. The argument will look something like this:

s_1. Incentives matter (Econ 101 principle).
s_2. Peltzman's model shows that increased safety will increase the number of accidents (model result) since drivers will respond to the changing costs of driving (s_1)
s_3. The model makes reasonable assumptions concerning drivers' behaviour and other conditions that influence driving behaviour (similarity judgement).
s_4. An increased number of accidents will increase the costs both for drivers and the government (fact).

↧

S_1. The net benefit of the safety regulation will be small in comparison to the advertised policy outcome arising from the offsetting effect of the change in drivers' behaviour.

↧

S_2. The government should cancel the safety regulation (policy advice)

First, let me repeat my earlier point to prevent any confusion: whereas valid models can be conceived as valid deductive arguments, model-based inferences such as this are not necessarily valid deductive arguments. In the case above, the premises (s_1-s_4) are better conceived as providing reasons to support the conclusions of the argument. Second, consider what is required to make this a reasonable argument. We have already seen that without something like s_3, the first conclusion would not be warranted. S_1 is a relatively weak statement since it does not say how strong the offsetting will be, but it nevertheless implicitly assumes that at least some group of drivers will change behaviour. As in the case of explanation, the truth of the premises in this argument needs to be established before one can rely on this argument to guide policy. Luckily there is a ton of empirical work on the topic. The evidence on the existence of offsetting behaviour is mixed (e.g., Cohen and Einav 2003), which creates some doubt about whether the reasoning leading to S_1 is justified (Aydinonat 2012). But even if the evidence were clear about offsetting behaviour, policy conclusion S_2 would have required additional evidence that shows that the model is a good guide for the particular case at hand given the policy goals, and institutional and other constraints. The point is that moving from a model result to statements about the real world requires many additional steps, which are often left implicit in practice—especially in policy debates. Recall that GPSS argue that a model may be valuable as an explanation even if it cannot be a reliable guide to policy. This is true on the face of it, but this does not change the fact that for both purposes similar challenges have to be tackled: there is no simple 'function' to turn a model into an explanation or policy advice.

4.3.3. Critique

Critique often starts from a policy proposal and evaluates how it fits the accepted theoretical framework in economics. For example, consider a proposal that

suggests making wearing face masks mandatory in public spaces. Also assume that in support of the proposal, the public authority presents evidence that shows that masks reduce the risk of infection from airborne viruses. Now assume that an economist, who is armed with Peltzman's model, argues that the policy proposal does not take the offsetting effect of increased safety into account. For this reason, the economist asks the public authority to postpone the regulation until it can provide evidence that the benefits of this regulation will exceed its costs considering the offsetting behaviour. Consider the following reconstruction of the critique.

s_1. The proposal for mandatory face masks assumes that this will reduce the number of infections based on evidence about the protection masks offer against virus transmission.
s_2. Masks reduce the risk of infection but do not eliminate it.
s_3. Peltzman's (1975) model shows that increased safety leads to increased risk-taking, which could offset the benefits of the regulation.
s_4. The proposal does not take the Pelzman effect (offsetting behaviour) into account.

↧

S_1. The proposal lacks the evidence that the regulation has net benefits considering the Peltzman effect.

↧

S_2. The policy-maker should provide evidence that the regulation has net benefits considering the Peltzman effect.

As the readers might recall, we have seen many similar arguments during the Covid-19 pandemic (e.g., Geloso 2020). Given the intuitiveness of the Pelzman effect, many are likely to find this type of critique compelling. However, we must note that the reason why many find the result intuitive arises because there are other sources of information (i.e., other than the model) that support it. Since this other information is not provided in the critique above, it remains incomplete. Moreover, the critique above has many implicit assumptions, the most important one for us being about the Pelzman model's applicability to this case. It also presumes – or asks us to believe – that the policy proposal does not consider the Peltzman effect. All in all, presented this way, the above argument appears to have many jumps and gaps. But perhaps the most interesting thing about this type of model-based critique is that it puts the burden of proof on the opponent despite the fact that the only "evidence" the critic provides is the result of a simplified model. Interestingly, GPSS also argue that a model-based critique places the burden of proof on the opponent.

One of GPSS's examples for 'models as critique' is the first welfare theorem in welfare economics, which shows that competitive equilibria are Pareto efficient. GPSS argue that when interpreted as critique, "the first welfare theorem sets the terms of the policy debate, *placing the burden of proof on interveners* to identify a market failure and to argue that the proposed intervention will lead to an improvement" (Gilboa et al. 2022a, 12, emphasis added). They say that

> In this view, the first welfare theorem becomes an exercise in critique, arguing that we should not be persuaded by a claim that 'this regulation will improve welfare' that cannot identify a market failure and that does not examine whether the regulation introduces further distortions. […] However, applying the first welfare theorem as critique, one would reasonably expect the proposer to point to some glitch in the market (perhaps monopsony power on the part of employers, or an inability for workers to borrow against their human capital in order to acquire skills) and to argue that the increase will not have prohibitively deleterious employment effects (perhaps, again, because of monopsony power, or because workers will become more valuable by acquiring more skill). (Gilboa et al. 2022a, 12)

It is appropriate to start commenting on this by stating the obvious, echoing Joseph Stiglitz, who argued, "The Welfare Theorems are just that: theorems, the conclusions of which follow inevitably from the assumptions" (Stiglitz 1991, 5). In our terminology the fundamental theorems of welfare economics are model results that are derived from a set of strong assumptions such as perfect competition, perfect information, and no externalities. As we have seen, using a model result in support of a policy or to explain is not a straightforward task; just citing the model result is rarely sufficient. Recalling GPSS's original question, we can ask: if we know that the assumptions used to derive the theorem are "false", why should we take the critique based on this assumption seriously? And why should the burden of proof be on the interveners?

First, note that *solely* pointing out the first welfare theorem is not likely to be considered as a challenge by anyone who proposes an interventionist policy, since they are likely to start from the observation that markets are not working as advertised in models of competition. Second, in the passage quoted above, GPSS say that the first welfare theorem entitles economists to ask the policy maker for evidence of monopsony power or some other market imperfection, but they make no demands on the economist to provide evidence that a model that assumes away all possible market imperfections is a good guide to policy design and critique. The reason this approach to critique appears misguided is that it does not mention the additional premises that enter the critical argument. The additional premisses include other models in economics and an accumulated pool of empirical evidence.

For example, there are many models in economics that show what happens when the assumptions behind the welfare theorems do not hold. Economists have investigated a wide variety of model worlds and gained knowledge about what happens under a wide range of conditions in their models. They have showed, for example, that under the conditions of monopsony a minimum wage policy could be desirable (e.g., Manning 2004). These models, some of which could be more reasonable approximations to the case at hand, give us *some* idea about *possible* results that *might* obtain in the real world. In addition, there is often a vast amount of empirical research on almost any chosen topic. Thus, what GPSS present as a critique based on a single model, in reality involves many other models and empirical evidence. As mentioned earlier, it seems that GPSS's 'theoretical inference' step in MoEM is too opaque to reveal the importance of these elements of model-based inference.

The general point is that whereas a single model with unrealistic assumptions, which looks nothing like the real-world case, cannot reliably serve as a tool for critique by itself, a diverse set of models that explore a variety of *what-if* scenarios could open one's eyes to *potential* results and pitfalls (Ylikoski and Aydinonat 2014) and with some additional evidence can convince policy-makers to be more careful.

In conclusion, a theoretical model is an argumentative device that can play a variety of roles in an argument, be it an explanation, policy advice, or criticism. However, the idealizing assumptions it employs make it rather a limited device, since often model results do not easily translate into statements about the world and additional argumentative steps are needed to reach conclusions about the real world. In practice, many model-based explanations, policy proposals and critiques are not explicit about these steps and hence argumentative leaps and jumps can be hidden under powerful rhetoric or conventional ways of doing things (e.g., due to epistemic and non-epistemic norms in the field or policy environment). Seeing models as argumentative devices can help us identify mistaken and deceptive arguments, and clearly see the limits and power of models. I hope the sketch of an account of models as argumentative devices that I provided in this section has given some directions in which to develop GPSS's account and their claim that models are can sometimes function as rhetorical devices.

5. Concluding remarks

There is no doubt that Itzhak Gilboa, Andrew Postlewaite, Larry Samuelson and David Schmeidler's account of economic modelling provides an insider perspective on economic modelling, enriching the literature on economic methodology. In the preceding pages, I presented a critical overview of their views and sketched a fresh perspective from which to view model-based inferences in

economics, with the hope of advancing the conversation. By pointing out the shortcomings of their similarity view and emphasizing the roles that models play in arguments, I aimed to identify focal points for future discussion. My main point was that conceptualizing models as argumentative devices simply and straightforwardly—with an appreciation of their diverse functions and interpretations—facilitates the identification of gaps and leaps in reasoning within model-based inferences. I contend that little progress can be achieved by evaluating models in isolation and exclusively focusing on similarity. Instead, paying closer attention to how economic models are used in practice for explanation, policy advice, prediction, 'methodology', and critique is the way forward.

Evidently, I am convinced that developing and applying the models-as-argumentative-devices view presented in this article (and in Aydinonat, Reijula, and Ylikoski 2021) is a promising strategy. However, the sketch I presented here needs to be further developed, possibly using the resources of argumentation theory (e.g., Toulmin 1958; F. H. V. Eemeren and Grootendorst 2003; F. H. van Eemeren et al. 2014; Walton, Reed, and Macagno 2008), and the argumentative theory of reasoning (Mercier and Sperber 2011; Mercier and Heintz 2014), building on the already extensive literature on model-based reasoning in economics (e.g., Gibbard and Varian 1978; Hausman 1992; Mäki 1992; Morgan and Morrison 1999; Sugden 2000; Guala 2001; Alexandrova 2006; Cartwright 2007; Aydinonat 2008; Alexandrova 2008; Boumans 2009; de Donato Rodríguez and Zamora Bonilla 2009; Kuorikoski and Lehtinen 2009; Hedoin 2012; Marchionni 2012; Morgan 2012; Ylikoski and Aydinonat 2014; Elster 2015; Hands 2016; Marchionni 2017; Aydinonat 2018; Herfeld 2018; Mireles-Flores 2018; Mäki 2020; Verreault-Julien 2021; Lisciandra and Korbmacher 2021; Jhun 2023)[4]—and, perhaps with some further inspiration from older debates in economic methodology on the rhetoric of economics (e.g., Donald N. McCloskey 1983; Deirdre N. McCloskey 1998; Caldwell and Coats 1984; Mäki 1988; 1995).

6. References

Akerlof, George A. 1970. "The Market for 'Lemons': Quality Uncertainty and the Market Mechanism." *The Quarterly Journal of Economics* 84 (3): 488–500. https://doi.org/10.2307/1879431.

Alexandrova, Anna. 2006. "Connecting Economic Models to the Real World." *Philosophy of the Social Sciences* 36 (2): 173–92. https://doi.org/10.1177/0048393106287210.

[4] Obviously, this is just a small sample from a vast literature.

———. 2008. "Making Models Count." *Philosophy of Science* 75 (3): 383–404. https://doi.org/10.1086/592952.
Arrow, Kenneth J. 1950. "A Difficulty in the Concept of Social Welfare." *Journal of Political Economy* 58 (4): 328–46.
Aydinonat, N. Emrah. 2008. *The Invisible Hand in Economics: How Economists Explain Unintended Social Consequences*. London: Routledge.
———. 2012. "The Two Images of Economics: Why the Fun Disappears When Difficult Questions Are at Stake?" *Journal of Economic Methodology* 19 (3): 243–58. https://doi.org/10.1080/1350178X.2012.714142.
———. 2018. "The Diversity of Models as a Means to Better Explanations in Economics." *Journal of Economic Methodology* 25 (3): 237–51. https://doi.org/10.1080/1350178X.2018.1488478.
———. forthcoming. "The Puzzle of Model-Based Explanation." In *The Routledge Handbook of Philosophy of Scientific Modeling*, edited by Tarja Knuuttila, Natalia Carrillo, and Rami Koskinen. London: Routledge. https://philarchive.org/rec/AYDTPO-4.
Aydinonat, N. Emrah, and Emin Köksal. 2019. "Explanatory Value in Context: The Curious Case of Hotelling's Location Model." *The European Journal of the History of Economic Thought* 26 (5): 879–910. https://doi.org/10.1080/09672567.2019.1626460.
Aydinonat, N. Emrah, Samuli Reijula, and Petri Ylikoski. 2021. "Argumentative Landscapes: The Function of Models in Social Epistemology." *Synthese*, no. 199, 369–95. https://doi.org/10.1007/s11229-020-02661-9.
Batterman, Robert W. 2009. "Idealization and Modeling." *Synthese* 169 (3): 427–46. https://doi.org/10.1007/s11229-008-9436-1.
Beisbart, Claus. 2012. "How Can Computer Simulations Produce New Knowledge?" *European Journal for Philosophy of Science* 2 (3): 395–434. https://doi.org/10.1007/s13194-012-0049-7.
Borenstein, Severin, and Janet S. Netz. 1999. "Why Do All the Flights Leave at 8 Am?: Competition and Departure-Time Differentiation in Airline Markets." *International Journal of Industrial Organization* 17 (5): 611–40. https://doi.org/10.1016/S0167-7187(97)00058-1.
Boumans, Marcel. 2009. "Understanding in Economics: Grey-Box Models." In *Scientific Understanding: Philosophical Perspectives*, edited by Henk de Regt, Sabina Leonelli, and Kai Eigner, 210–28. Pittsburgh: University of Pittsburgh Press.
Caldwell, Bruce J., and A. W. Coats. 1984. "The Rhetoric of Economists: A Comment on McCloskey." *Journal of Economic Literature* 22 (2): 575–78.
Cartwright, Nancy. 2007. "The Vanity of Rigour in Economics: Theoretical Models and Galilean Experiments." In *Hunting Causes and Using Them Approaches in Philosophy and Economics*, 217–35. Cambridge: Cambridge University Press.
Coase, Ronald H. 1960. "The Problem of Social Cost." *Journal of Law and Economics* 3:1–44. https://doi.org/10.1086/466560.
———. 1992. "The Institutional Structure of Production." *University of Chicago Law Occasional Paper* 82 (28): 713–19. https://doi.org/10.1007/978-3-540-69305-5_3.

Cohen, Alma, and Liran Einav. 2003. "The Effects of Mandatory Seat Belt Laws on Driving Behavior and Traffic Fatalities." *Review of Economics and Statistics* 85 (4): 828–43. https://doi.org/10.1162/003465303772815754.

D'Aspremont, C, J Jaskold Gabszewicz, and Jacques-François Thisse. 1979. "On Hotelling's 'Stability in Competition.'" *Econometrica* 47 (5): 1145–50. https://doi.org/10.2307/1911955.

Donato Rodríguez, Xavier de, and Jesús Zamora Bonilla. 2009. "Credibility, Idealisation, and Model Building: An Inferential Approach." *Erkenntnis* 70 (1): 101–18. https://doi.org/10.1007/s10670-008-9139-5.

Eemeren, F. H. van, Bart Garssen, E. C. W. Krabbe, Arnolda Francisca Snoeck Henkemans, Bart Verheij, and Jean Hubert Martin Wagemans. 2014. *Handbook of Argumentation Theory*. Dordrecht: Springer Reference.

Eemeren, Frans H. Van, and Rob Grootendorst. 2003. *A Systematic Theory of Argumentation: The Pragma-Dialectical Approach*. 1st ed. Cambridge University Press. https://doi.org/10.1017/CBO9780511616389.

Elster, Jon. 2015. "Tool-Box or Toy-Box? Hard Obscurantism in Economic Modeling." *Synthese*, July, 1–26. https://doi.org/10.1007/s11229-015-0836-8.

Frigg, Roman, and James Nguyen. 2017. "Models and Representation." In *Springer Handbook of Model-Based Science*, edited by Lorenzo Magnani and Tommaso Bertolotti, 49–102. Dordrecht: Springer.

Geloso, Vincent. 2020. "Masks, Seatbelts, and Peltzman Effects." AIER. July 22, 2020. https://www.aier.org/article/masks-seatbelts-and-peltzman-effects/.

Gibbard, Allan, and Hal R. Varian. 1978. "Economic Models." *The Journal of Philosophy* 75 (11): 664–77.

Giere, Ronald N. 2004. "How Models Are Used to Represent Reality." *Philosophy of Science* 71 (5): 742–52. https://doi.org/10.1086/425063.

Gilboa, Itzhak, Andrew Postlewaite, Larry Samuelson, and David Schmeidler. 2014. "Economic Models as Analogies." *Economic Journal* 124 (578): 513–33. https://doi.org/10.1111/ecoj.12128.

———. 2018. "Economics: Between Prediction and Criticism." *International Economic Review* 59 (2): 367–90. https://doi.org/10.1111/iere.12273.

———. 2019. "What Are Axiomatizations Good For?" *Theory and Decision* 86 (3): 339–59. https://doi.org/10.1007/s11238-018-09685-1.

———. 2022a. "Economic Theories and Their Dueling Interpretations." *Journal of Economic Methodology* 0 (0): 1–20. https://doi.org/10.1080/1350178X.2022.2142270.

———. 2022b. "Economic Theory: Economics, Methods and Methodology:" *Revue Économique* Vol. 73 (4): 897–919. https://doi.org/10.3917/reco.736.0897.

Grüne-Yanoff, Till. 2011. "Isolation Is Not Characteristic of Models." *International Studies in the Philosophy of Science* 25 (2): 119–37. https://doi.org/10.1080/02698595.2011.574854.

Grüne-Yanoff, Till, and Caterina Marchionni. 2018. "Modeling Model Selection in Model Pluralism." *Journal of Economic Methodology* 25 (3): 265–75. https://doi.org/10.1080/1350178X.2018.1488572.

Guala, Francesco. 2001. "Building Economic Machines: The FCC Auctions." *Studies in History and Philosophy of Science Part A* 32 (3): 453–77. https://doi.org/10.1016/S0039-3681(01)00008-5.

Hands, D. Wade. 2016. "Derivational Robustness, Credible Substitute Systems and Mathematical Economic Models: The Case of Stability Analysis in Walrasian General Equilibrium Theory." *European Journal for Philosophy of Science* 6 (1): 31–53. https://doi.org/10.1007/s13194-015-0130-0.

Hausman, Daniel M. 1992. *The Inexact and Separate Science of Economics*. Cambridge: Cambridge University Press.

Hedoin, C. 2012. "Models in Economics Are Not (Always) Nomological Machines: A Pragmatic Approach to Economists' Modeling Practices." *Philosophy of the Social Sciences* 44 (4): 424–59. https://doi.org/10.1177/0048393112458715.

Hempel, Carl G. 1965. "Aspects of Scientific Explanation." In *Aspects of Scientific Explanation and Other Essays in Philosophy of Science*, 331–496. New York: Free Press.

Herfeld, Catherine. 2018. "Explaining Patterns, Not Details: Reevaluating Rational Choice Models in Light of Their Explananda." *Journal of Economic Methodology* 9427:1–31. https://doi.org/10.1080/1350178X.2018.1427882.

Hotelling, Harold. 1929. "Stability in Competition." *The Economic Journal* 39 (153): 41–57. https://doi.org/10.1007/978-1-4613-8905-7_4.

Jhun, Jennifer S. 2023. "Multi-Model Reasoning in Economics: The Case of COMPASS." *Philosophy of Science*, April, 1–19. https://doi.org/10.1017/psa.2023.69.

Knuuttila, Tarja. 2005. *Models as Epistemic Artefacts: Toward a Non-Representationalist Account of Scientific Representation*. Philosophical Studies from the University of Helsinki 8. Vantaa: Department of Philosophy, University of Helsinki.

Kuorikoski, Jaakko, and Aki Lehtinen. 2009. "Incredible Worlds, Credible Results." *Erkenntnis* 70 (1): 119–31. https://doi.org/10.1007/s10670-008-9140-z.

Kuorikoski, Jaakko, Aki Lehtinen, and Caterina Marchionni. 2010. "Economic Modelling as Robustness Analysis." *The British Journal for the Philosophy of Science* 61 (3): 541–67. https://doi.org/10.1093/bjps/axp049.

Leamer, Edward E. 1992. "Testing Trade Theory." *NBER Working Paper Series* Working Pa.

Lisciandra, Chiara. 2017. "Robustness Analysis and Tractability in Modeling." *European Journal for Philosophy of Science* 7 (1): 79–95. https://doi.org/10.1007/s13194-016-0146-0.

Lisciandra, Chiara, and Johannes Korbmacher. 2021. "Multiple Models, One Explanation." *Journal of Economic Methodology* 28 (2): 186–206. https://doi.org/10.1080/1350178X.2021.1892800.

Mäki, Uskali. 1988. "How to Combine Rhetoric and Realism in the Methodology of Economics." *Economics & Philosophy* 4 (1): 89–109. https://doi.org/10.1017/S0266267100000353.
———. 1992. "On the Method of Isolation in Economics." In *Idealization IV: Intelligibility in Science (Poznan Studies in the Philosophy of the Sciences and the Humanities 26)*, edited by C. Dilworth, 317–51. Amsterdam: Rodopi.
———. 1995. "Diagnosing McCloskey." *Journal of Economic Literature* 33 (3): 1300–1318.
———. 2005. "Models Are Experiments, Experiments Are Models." *Journal of Economic Methodology* 12 (2): 303–15. https://doi.org/10.1080/13501780500086255.
———. 2010. "Models and Truth: The Functional Decomposition Approach." In *EPSA Epistemology and Methodology of Science: Launch of the European Philosophy of Science Association*, edited by Mauricio Suárez, Mauro Dorato, and Miklós Rédei, 177–87. Springer.
———. 2020. "Puzzled by Idealizations and Understanding Their Functions." *Philosophy of the Social Sciences* 50 (3): 215–37. https://doi.org/10.1177/0048393120917637.
Manning, Alan. 2004. "Monopsony and the Efficiency of Labour Market Interventions." *Labour Economics* 11 (2): 145–63. https://doi.org/10.1016/j.labeco.2003.09.003.
Marchionni, Caterina. 2012. "Geographical Economics and Its Neighbours - Forces Towards and Against Unification." In *Philosophy of Economics. Handbook of the Philosophy of Science*, edited by Uskali Mäki, 13:425–58. Amsterdam: Elsevier. https://doi.org/10.1016/B978-0-444-51676-3.50015-4.
———. 2017. "What Is the Problem with Model-Based Explanation in Economics?" *Disputatio* 9 (47): 603–30. https://doi.org/10.1515/disp-2017-0020.
McCloskey, Deirdre N. 1998. *The Rhetoric of Economics*. 2nd ed. Univ of Wisconsin Press.
McCloskey, Donald N. 1983. "The Rhetoric of Economics." *Journal of Economic Literature* 21 (2): 481–517.
Mercier, Hugo, and Christophe Heintz. 2014. "Scientists' Argumentative Reasoning." *Topoi* 33 (2): 513–24. https://doi.org/10.1007/s11245-013-9217-4.
Mercier, Hugo, and Dan Sperber. 2011. "Why Do Humans Reason? Arguments for an Argumentative Theory." *Behavioral and Brain Sciences* 34 (2): 57–74. https://doi.org/10.1017/S0140525X10000968.
Milgrom, Paul, and Nancy Stokey. 1982. "Information, Trade and Common Knowledge." *Journal of Economic Theory* 26 (1): 17–27. https://doi.org/10.1016/0022-0531(82)90046-1.
Mireles-Flores, Luis. 2018. "Recent Trends in Economic Methodology: A Literature Review." In *Research in the History of Economic Thought and Methodology*, 36:93–126. https://doi.org/10.1108/S0743-41542018000036A008.

Morgan, Mary S. 2002. "Model Experiments and Models in Experiments." In *Model-Based Reasoning: Science, Technology, Values*, edited by Lorenzo Magnani and Nancy J. Nersessian, 41–45. New York: Kluwer Academic/Plenum Publishers.

———. 2012. *The World in the Model: How Economists Work and Think*. Cambridge ; New York: Cambridge University Press.

Morgan, Mary S., and Tarja Knuuttila. 2012. "Models and Modelling in Economics." In *Philosophy of Economics, Handbook of the Philosophy of Science. Volume 13*, edited by Uskali Mäki, 49–87. Amsterdam: Elsevier.

Morgan, Mary S., and Margaret Morrison. 1999. *Models as Mediators: Perspectives on Natural and Social Science (Ideas in Context)*. Vol. 1. Cambridge University Press.

Norton, John D. 1991. "Thought Experiments in Einstein's Work." In *Thought Experiments in Science and Philosophy*, edited by Tamara Horowitz and Gerald Massey, 129–44. Savage, MD.: Rowman and Littlefield.

———. 1996. "Are Thought Experiments Just What You Thought?" *Canadian Journal of Philosophy* 26 (3): 333–66. https://doi.org/10.1080/00455091.1996.10717457.

Peltzman, Sam. 1975. "The Effects of Automobile Safety Regulation." *Journal of Political Economy* 83 (4): 677–726.

Potochnik, Angela. 2017. *Idealization and the Aims of Science*. Chicago: The University of Chicago Press.

Rassekh, Farhad, and Henry Thompson. 1993. "Factor Price Equalization: Theory and Evidence." *Journal of International Economic Integration* 8 (1): 1–32.

Rice, Collin. 2019. "Models Don't Decompose That Way: A Holistic View of Idealized Models." *The British Journal for the Philosophy of Science* 70 (1): 179–208. https://doi.org/10.1093/bjps/axx045.

———. 2021. *Leveraging Distortions: Explanation, Idealization, and Universality in Science*. Cambridge, Massachusetts: The MIT Press.

Salvanes, Kjell G., Frode Steen, and Lars Sørgard. 2005. "Hotelling in the Air? Flight Departures in Norway." *Regional Science and Urban Economics* 35 (2): 193–213. https://doi.org/10.1016/j.regsciurbeco.2003.10.001.

Samuelson, Paul A. 1948. "International Trade and the Equalisation of Factor Prices." *The Economic Journal* 58 (230): 163–84.

———. 1949. "International Factor-Price Equalisation Once Again." *The Economic Journal* 59 (234): 181–97.

Stiglitz, Joseph E. 1991. "The Invisible Hand and Modern Welfare Economics." *NBER Working Paper*.

Strevens, Michael. 2008. *Depth: An Account of Scientific Explanation*. Cambridge, Mass: Harvard University Press.

Sugden, Robert. 2000. "Credible Worlds: The Status of Theoretical Models in Economics." *Journal of Economic Methodology* 7 (1): 1–31. https://doi.org/10.1080/135017800362220.

Toulmin, Stephen E. 1958. *The Uses of Argument*. 2nd ed. Cambridge University Press. https://doi.org/10.1017/CBO9780511840005.

Verreault-Julien, Philippe. 2021. "Explanation in Economics." In *The Routledge Handbook of Philosophy of Economics*, edited by Conrad Heilmann and Julian Reiss, 1st ed., 300–315. New York: Routledge. https://doi.org/10.4324/9781315739793-27.

Walton, Douglas, Christopher Reed, and Fabrizio Macagno. 2008. *Argumentation Schemes*. 1st ed. Cambridge University Press. https://doi.org/10.1017/CBO9780511802034.

Weisberg, Michael. 2013. *Simulation and Similarity: Using Models to Understand the World*. Oxford University Press.

Woodward, James. 2006. "Some Varieties of Robustness." *Journal of Economic Methodology* 13 (2): 219–40. https://doi.org/10.1080/13501780600733376.

Ylikoski, Petri. 2009. "The Illusion of Depth of Understanding in Science." In *Scientific Understanding: Philosophical Perspectives*, edited by Henk W. de Regt, Sabina Leonelli, and Kai Eigner, 100–119. Pittsburgh: Pittsburgh University Press.

Ylikoski, Petri, and N. Emrah Aydinonat. 2014. "Understanding with Theoretical Models." *Journal of Economic Methodology* 21 (1): 19–36. https://doi.org/10.1080/1350178X.2014.886470.