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BETWEEN ISOLATIONS AND CONSTRUCTIONS:

ECONOMIC MODELS AS BELIEVABLE WORLDS

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ABSTRACT. As the title of this essay suggests, my concern is with the issue of what are economic models. However, the goal of the paper is not to offer an in-depth study on multiple approaches to modelling in economics, but rather to overcome the dichotomical divide between conceptualizing models as isolations and constructions. This is done by introducing the idea of economic models as believable worlds, precisely descriptions of mechanisms that refer to the essentials of the modelled targets. In doing so I make use of the Woodward's (2002) conceptualization of mechanisms. It is shown that such models do not offer the perfectly true descriptions of the actual world but justified beliefs about the modelled, precisely they aim at maximizing truth and minimizing falsity in a large body of belief about the real world. The analysis throughout the paper is supported by in-depth examination of the Varian's (1980) model of sales that is here treated as a representative way of reasoning in neoclassical economics.

1. Introduction

Economics uses models extensively in explaining the workings of the markets. However, these models, if interpreted as representations of real economic phenomena, appear extremely unrealistic. Therefore, many ask how it is possible to make inferences from models to the actual world. Or, if these models help us to understand the world, and if so, how? This fact alone warrants curiosity of philosophers of science as well as practicing economists interested in methodological issues.

In recent years a lot has been done in terms of philosophical reflection on economic models (cf. Morgan and Knuuttila 2012; Morgan 2012; *Erkenntnis* 70 (1); *Journal of Economic Methodology* 20 (3))[1]. The authors contributing to this field can be roughly divided into isolationists and constructivists. The ones from the former group understand economic models as isolations that "represent the target systems as far simpler, as devoid of most of those proprieties and causal facts, highlighting of focusing on just a small fraction of them. […] they isolate a fragment of their target system" (Mäki 2006, p. 10). So, such models are constructed using the

[1] Also, many recent papers in the history of economic thought study the reasons for such a huge inflow of the modeling method into economics, including the role of strong ties between economics and physics (e.g., Morgan 2012; Boumans 2004, 2005).

rules of *ceteris absentibus, ceteris neglictis,* and *ceteris paribus* (Boumans 2005). In Mäki's account, a model builder is sealing-off the relations of interest from other influences in order to isolate the essence of the object (Mäki 1992, p. 344). This way of modelling draws heavily from the Poznan approach where idealization is usually complemented with the reverse process of concretization (Nowak 1994). On the other hand, the representatives of the latter group, treat models as constructions which are *parallel realities* and not the simplified pictures of some targets. Or, in Sugden's words: "[…] the model world is not constructed by starting from the real world and stripping out complicating factors: although the model world is simpler than the real world, the one is not a *simplification* of the other" (2000, p. 25). Although the divide between these two ways of interpreting economic models is not sharp, it can serve as a starting point in investigating the ways economists model the world.

Before going further, let me put emphasis on the fact that in these two approaches the goal of the modeler is to explain. In particular, I subscribe here to the realism movement in economics (Lawson 1997). Its important feature is the claim that the social world is layered in such a way that, on the one hand, we have higher-level events or facts (e.g., Polish inflation at the beginning of the 90s) and, on the other hand, underlying processes giving rise to the higher-level phenomena (e.g., price setting behavior of enterprises). Here I make also reference to the semirealism which nicely *combines* entity and structural realism, namely it claims that science tells us about the structure of the mind-independent reality, however, this structure is 'encoded' in the natures of its forming entities (Chakravartty 2007). Also, the elements of the structure are interrelated, i.e., changing a given component modifies the other. Thus, in this context, models are devices that enable an indirect representation of such structures. Therefore, my view on the role of models in economics conflicts with naïve empiricist ideas, since for me there are 'depths' in science (cf. Hausman 2012, p. 87).

As the paper's title suggests, what I claim is that economic models are neither pure isolations, nor pure constructions, but believable worlds depicting structures (in the above-described sense) that enable the workings of mechanisms that refer to the ones operating in the real world. Such an approach incorporates some elements of isolationism (only the *essential* mechanism is investigated) and constructivism (the mechanism can be at play between fictional entities – why not to model price distorting effects of asymmetric information using artificial markets inhabited by fairy-tale creatures?). The latter does not conflict with the central tenet of semirealism, since one may have different sets of particulars forming the same structure, or, in other words, different particulars can have the same natures, e.g., both in the nature of a Wall Street banker and an inhabitant of Hobbiton could be to maximize profits. So, imagination plays also an important role in model building (Morgan 2012).

The description of models in terms of believable worlds rests on the assumption that mechanisms described by models are similar to the ones in the real world. The truthlikeness (*verisimilitude*) of a given model depends on the closeness of the mechanism it contains to its real counterpart. Thus, truthlikeness covers both partial truth and similarity. It is important for our investigation of economic models, since one may conclude that even a false model *as a whole* can be claimed to be believable (truthlike). As Niiniluoto (2012, p. 71) argues that models can approximate the real system without being identical with it at any specific point. Therefore, models are taken here as fundamental elements of science, and hence they are structures that satisfy the linguistic elements of theories (van Fraassen 1980; Giere 1988; Suppe 1989). In such a "semantic view", theoretical claims are always true within models, but only partially true if referred to the outside-model world (cf. Zeidler 2013, pp. 30-35). For instance, in the model of perfect competition price always equals marginal cost of production at the equilibrium, however, such a claim referred to the real market loses the status of being universally true and gains the status of a belief – it is believable to claim that price is to converge towards marginal cost of production, however, other factors can be at play[2]. The more a given domain is closer to the model's structure, the less falsehoods a belief contains. Therefore, models are theory creating entities, however, these theories are just sets of beliefs if referred to domains beyond models (cf. Guala 2005). Or, in other words, representation should not be tied to the traditional notion of isomorphism, but rather it is sufficient that "the 'representational force' [mechanism] of model *M* points toward target *R*, and *M* allows competent and informed agents to draw specific inferences [theories] regarding *R*" (Niiniluoto 2012, p. 69). So, models need not be iconic and can be to a large extent constructed.

Since the contemporary philosophers of economics usually build their insights on inspecting the ways economists do economics, I start by the way of example, a typical approach taken by those investigating the nature of economic models, e.g., Sugden's (2000) analysis of Schelling's (1978) checkerboard model of racial segregation or Cartwright's (2009) insights based on Pissaride's (2000) labor market model, and I explore *A Model of Sales* (1980) by Hal Varian. Here I offer a more detailed analysis of what a belief *is* and in doing so I prove that what is described by Varian's model is a mechanism. This requires me to include in the paper not only insights on the ontology of models (what kind of entities are they), but also how they explain (epistemology), and how they relate to the real world. However, at this point, I disagree with Morgan and Morrison's (1999) claim that what makes a model is how it is used, not what kind of thing it is. Therefore, answering the ontological question should enable me to shed some light on the epistemological one. Subsequently, the idea of believable world is compared with the one of *credible world* by Sugden (2000; 2009). The last section of the paper uses my approach to resolve the explanatory paradox by Reiss (2012). Conclusions follow.

2. Hal Varian's Model of Sales and the Idea of Believable Worlds

[2] Such an understanding of the relationship between these two kinds of claims can be traced back even to Mill's remarks on the nature of economics. In his 1836 essay *On the Definition of Political Economy* he states the following: "The conclusions of geometry are not strictly true of such lines, angels, and figures, as human hands can construct. But no one therefore contends that the conclusions of geometry are of no utility, or that it would be better to use Euclid's *Elements* as waste paper" (p. 46).

Varian's paper *A Model of Sales* (1980) is widely cited in economics. It discusses the issue of persisting price dispersion in many markets, or, in other words, the cases in which the "law of one price" seems not to hold. Also, its author is not only a renowned theoretical economist, but also the author of bestselling microeconomics textbook (*Intermediate Microeconomics*, 1st. ed. in 1987). Therefore, his approach to economic modelling impacts the ways new generations of economists build and use models[3]. So, my concern is with whether Varian's model tells us something about the real world, and if so, what? But first and most importantly an ontological question is stated – what kind of entity the Varian's model is?

The starting of Varian's paper is rather in a manner of the so-called internally driven research program: after declaring that "the law of one price is no law at all" (1980, p. 651), Varian presents some research done by theoretical economists explaining this phenomenon. Only later he refers to the actual market:

> One does not have to look far to find the real world analog of such behavior. It is common to observe retail markets where stores deliberately change their prices over time – that is, where stores have *sales*. A casual glance at the daily newspaper indicate that such behavior is very common. A high percentage of advertising seems to be directed at informing people of limited duration sales of food, clothing, and appliances (emphasis added) (ibid.).

So, the relation between the Varian's model and the real world is of the analogical kind. However, his use of the term 'analog' does not mean that his model is analogical – we do not have a change of medium while reproducing the original (cf. Mäki 2001, p. 9932). What he expresses is that in the world beyond the model one can notice price movements analogical to the ones present in the model. So, his model in not the analogue of the real economic system. We do not find any inhabitants of his model behaving like real stores but at the same not being the real shops. Therefore, metaphors are not present in Varian's model. What we have in his model are consumers' and stores' descriptions. The rest of the model is formed by some assumptions about both its objects (consumers and stores) and mechanisms regulating the interplay between them as well as their behavior. These assumptions are not crafted so as to resemble the ones identified on the real market. Also, they are quite general, e.g.: "each week, each store randomly chooses prices according to its density function $f(p)$ […]. Finally, the stores are characterized by identical, strictly declining average cost curves" (Varian 1980, p. 652). Definitively these characteristics of stores' behavior were not constructed by the method of idealization: just what do we have to seal off to make a real market – say, Berkeley's Fourth Street shopping area – become like a Varian's model? On the contrary, these characteristics are just tractability assumptions that make the workings of the model possible (cf. Alexandrova 2006). Consequently, Varian constructed an imaginary world – he did not attempt to describe any real market, say Walmart stores in his home town Berkeley. Finally, he concluded that:

[3] Varian is quite explicit on this in his how-to do economics manuals, e.g., his 1999 paper *How to Build an Economic Model in Your Spare Time* (constantly updated on his webpage). Also, in 1978 he published a paper (co-authored with A. Gibbard) on *Economic Models* which appeared in *The Journal of Philosophy*.

> […] the features of the model here described may have some relevance in explaining real world retailing behavior (Varian 1980, p. 658).

So, he is very cautious in making any model-world inferences – a rather typical position of modelers in economics and beyond (Sugden 2009, p. 16). He even claims that "the theoretical examination of these motives is left for future work" (Varian 1980, p. 652). The logic of his paper suggests that by *motives* he understands not *all* possible factors influencing firms' behavior in setting prices but rather the ones identified by him in form of regularities-like-statements, e.g. "more uninformed consumers cause the average price paid by the uninformed consumers to rise" (ibid., p. 657). However, these statements are not stated in the form of explicit and testable hypothesis. Here I meet with Sugden's (2000) reading of Akerlof's paper and especially his claim that many economic models connect *real* causes (in Varian's model, e.g., the different levels of consumers' knowledge about prices on the market) to *real* effects (in Varian's model, e.g., frequent price changes of a given good).

Let me now comment on the nature of this *connection* between causes and effects. First, the construction of the Varian's model is such that enables some comparative statics (see, Table 1, p. 657 in his paper). For instance, the model links the average price the uninformed consumers pay ($\bar{p}$) with a consumer's reservation price – the maximum price any consumer will pay for the good (denoted by r), a density function $f(p)$ (informing about the probability with which a given store charges price p), and the average cost of supplying the good for all consumers (denoted by p^*), and thus (p. 657):

$$\bar{p} = \int_{p^*}^{r} pf(p)dp$$

So, the above relation is invariant to the changes in the background conditions, e.g., we do not have any information about the possible impact of changes in the interest rates on the average price paid by the uninformed consumer. But also, the above *connection* is non-sensitive with respect to the range of values the variables in explanans (here: in the above equation) can take without breaking the explanatory relationship (cf. Hardt 2011, p. 127)[4]. However, it should be noted that the density function is chosen solely by the firm so as to maximize its profits. Now, the interesting question is the following: what is described by mathematical formulas from Varian's model? As the very first sentence in the summary of Varian's paper indicates the goal of the model was to "show how stores may find it in their interest to randomize prices in an attempt to price discriminate between informed and uninformed consumers" (1980, p. 658). Thus, my question is the following: *what* makes shops to randomly price their products? My reading of the Varian's paper makes me think of his model as an *isolation* of a mechanism that is responsible for price dispersion. In this context, to explain means to discover the mechanism (e.g., Steel

[4] The very idea of non-sensibility as an important virtue of good explanation is due to Ylikoski and Kuorikoski (2010).

2011, p. 123). But how philosophers understand mechanisms? First, there is a general consensus that mechanisms require structures enabling their functioning. As Reiss (2008b) claims:

> Whether or not a mechanism operates depends on whether or not there is a system or structure in place that has the right proprieties such that, if it *were* to persist, it could operate undisturbedly and, if the sequence was triggered repeatedly, then a regularity *would* ensue […]. Without such a setup, no mechanism can operate (p. 108).

So, a mechanism is something in the world which gives rise to a specific causal relationship (regularity). Such a perspective corresponds well with my initial remarks about the structural character of the world (semirealism). However, we should make a distinction between a real mechanism and its description. Here Reiss's (2008b) insights can help us again, namely:

> *Casual Mechanism (CM)*. A causal mechanism for a causal relationship between (aggregate, macro or social variables) *X* and *Y* is a set of entities and proprieties that are such that, if they were embedded in a stable structure, could operate unimpededly and, if *X* fired regularly, then *Y* would follow regularly (p. 109).

The stability of the relation between *X* and *Y* is due to the fact that CM operates on the artificially built structure (*if they were embedded*). Therefore, such a structure resembles Cartwright's nomological machine which is used to produce stable behavior[5]. Thus a model of CM or a blueprint of nomological machine can be conceptualized as follows:

> *Model of Casual Mechanism (MCM). A model for causal mechanism is a representation of a causal mechanism according to CM (Reiss 2008b, p. 109).*

Although some definitions of mechanisms do not explicitly refer to structures in which they are embedded (e.g., Machamer et al. 2000, p. 3), I think that structures are indispensables and implicitly present in such conceptualizations. When Machamer et al. (2000) claim that "The organization of these entities and activities [in mechanisms] determines the ways in which they produce the phenomenon. <u>Entities often must be appropriately located, structured, and oriented</u>, and the activities in which they engage must have a temporal order, rate, and duration" (emphasis added) (p. 3), in fact, they refer to structures, since entities must be structured[6]. Other authors offer more complex definitions. For instance, Woodward (2002) proposes the following one:

> **(MECH)** a necessary condition for a representation to be an acceptable model of a mechanism is that the representation (i) describe an organized or structured set of parts or components, where (ii) the behavior of each component is described by a generalization that is invariant under interventions, and where (iii) the generalizations governing each component are also independently changeable, and where (iv) the representation allows us to see how, in virtue of (i),

[5] Nomological machine is defined as "[…] a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behavior that we represent in our scientific laws" (Cartwright 1999, p. 50).

[6] Here I disagree with Reiss's (2008b, p. 110) interpretation of Machamer's definition.

(ii) and (iii), the overall output of the mechanism will vary under manipulation of the input to each component and changes in the components themselves (p. 375).

Although no direct references to 'regularities' can be found in the above definition, they are embedded in 'generalizations', and also 'invariance under intervention' plays the same role as 'regularities' in Reiss's definition. That is due to the fact that a generalization can be invariant within a certain domain even though it has exceptions outside that domain (Woodward 2000, p. 199). Similarly in Reiss's approach where regularities hold only for a given stable structure. The virtue of Woodward's (2002) definition is due to its applied character – one is offered with a detailed set of criteria for assessing whether a given model can be treated as a model of mechanism. Also, these criteria are built on rich philosophical underpinnings concerning the issue of invariance (see, e.g., Woodward 2005). So, if (i)-(iv) are met for a given representation (here: the Varian's model of sales), then this representation is an acceptable model of a mechanism. Before moving further in order to check whether Varian's model satisfies these conditions, let me offer more insights on each clause.

Ad. (i). Mechanism consists of components that are structured according to some principles, or, as Glennan's reminds us: "A mechanism underlying a behavior is a complex system which produces that behavior" (1996, p. 52). Woodward offers here an example of a block sliding down an inclined plane. So, the block is subjected to two forces – a gravitational force and the one due to friction that opposes the motion of the block. Thus we can easily get the net force on the block along the plane. But still, we have two components (forces) and each is capable of producing regular changes (regularities). If the movements of the block had been caused by one force only, then one could not have named the process producing these movements a mechanism (at least in Woodward's sense). Also, each component of the mechanism produces its effect not due to direct causal law, but rather according to some approximate empirical relationship (Woodward 2002, p. 369). Even if one names it law, it would rather be a Millian tendency law – such law is true of the *tendency* to produce its characteristic results, so even if the effect is not the one predicted by the law, this law still may hold since the tendency may be present and the result may occur in spite of this very tendency (Reiss 2008a, p. 267)[7]. So, regularities *inside* mechanism are not of the universal kind and often the only feature they share with laws is the one supporting (some) counterfactuals (Woodward 2002, p. 369). In the below comment on (ii) I develop more insights on this issue.

Ad. (ii). In Woodward's terms the invariance of the generalization regulating the behavior of each component of the mechanism is defined as follows: "[…] for a generalization to be invariant all that it is required is that it be stable under some changes and interventions. It is not required that it be invariant under all possible changes and interventions" (2002, p. 371). The last sentence gives further arguments for non-universal character of regularities present in

[7] A tendency is defined by Mill as "a power acting with certain intensity in that direction" (1836, p. 67, cited in: Schmidt-Petri 2008, p. 67). Such an understanding of laws has been popularized by Cartwright with her conception of 'capacities' which is synonymous to Mill's 'tendencies' (Cartwright 1989, p. 170).

mechanisms. So, the invariance of the generalization should be understood as a capacity of *X* to alter *Y*. For instance, manipulating the air pressure (*X*) may contribute to the creation of storm (*Y*), but changing the readings of the barometer does not have capacity to influence the weather. Surely, the strength of the X's impact on Y depends on external factors (e.g., air humility, temperature, relief of the terrain, and so on), however, even if these factors are to nearly cutoff the casual relation between *X* and *Y*, still the air pressure would have the capacity to alter the probability of storm's occurrence. So, the invariance is understood rather *weakly* (cf. Woodward 2000).

Ad. (iii). The independently changeable character of generalizations governing each component of mechanism comes down to the requirement "that the components of a mechanism should be independent in the sense that it should be possible in principle to intervene to change or interfere with the behavior of one component without necessarily interfering with the behavior of others" (Woodward 2002, p. 374). For instance, in the above example of air pressure and storm this condition does not hold since according to Boyle's law changing the pressure of gas affects its temperature and hence the workings of *temperature* ➔ *(probability of) storm* casual channel. On the contrary, in the example of a block sliding down an inclined plane the (iii) is met, since even if one changes the kinetic friction (e.g., by greasing the surface), then the gravitational force component is not to be altered at all. Such a system is modular (cf. Woodward 1999). The (iii) is required so as to allow tracing out the consequences of (possible) changes in any of components for the overall behavior of the mechanism. Now, if any change is to alter the remaining generalizations and thus disallowing us from assessing the individual contributions of mechanism's subparts for the overall effect, then our proposed decomposition is incorrect. The (iii) is thus hardly met in organismic systems. This has a profound impact on the applicability of Woodward's conception of mechanism to particular sciences. However, in neoclassical economics which is to a large extent built on classical physics the (iii) seems to be *naturally* met since in this science causes are usually composed in analogy with the law of vector addition of forces in physics (Cartwright 1998, p. 45). But still, even in economics a checking whether we deal with a 'mechanistic' and not an 'organismic' model is needed, if one wants to identify mechanisms in the sense of **MECH**[8].

Ad. (iv). The interpretation of (iv) is rather straightforward, since it means that, if (i)-(iii) are met, then a necessary condition for a representation to be an acceptable model of a mechanism is that it enables us to see the overall output of the mechanism. Or, in other words, the representation should enable the modeler to identify the composite effect of the changes of input

[8] In this context it is worth referring to Mäki's comments on the applicability of the method of isolation in economics: "This point refers to a major problem involved in the method of isolation as used in studying social and economic phenomena. This is the question of whether the causes of economic phenomena combined 'mechanically' or 'chemically', to use Mill's phrases. When causes combine 'mechanically', their effects can be 'added up' like vectors […]. On the other hand, when causes are combined 'chemically', some qualitative novel, emergent outcomes ensue. It is easier for the user of the method of isolation to deal with the domain of 'mechanics' than that of 'chemistry'. No wonder, therefore, that standard neoclassical economists do their work most of the time as if economics were 'mechanics'" (Mäki 1992, p. 349; cf. Lawson 1997, p. 132).

to each component and changes in the components themselves. So, the mechanism should be characterized by a capacity to aggregate the workings of its components.

So, in what follows, I check whether the model of sales by Varian (2002) can be thought of as a model of mechanism in the above described sense.

Let me start with (i). If the Varian's model is to be treated as a mechanism, then it should be composed by at least two interrelated parts/components. This is for sure, since what we have in the model are descriptions of consumers and shops, including some insights on the rules regulating their behavior. The buyers "behave in rational manner" (p. 651) and the sellers maximize their profits (p. 654). The two groups play a special kind of a game in which they interact (see, Table 1, p. 657) and the price dispersion follows. Now, do we have any extra crucial components of the model beyond consumers' and sellers' rules of behavior? It seems that these two (utility maximization by consumers and profit maximization by stores) are the only ones. The rest consists of tractability assumptions such as the one that each firm chooses the same price strategy (p. 652) or that its average cost curve is strictly declining (ibid.). The two previously mentioned components are generally accepted by the majority of economists and they form the core of neoclassical economics. In this context, it is worth mentioning that economics faces the problem of *overconstraint*, namely that it has very few uncontroversial principles at its disposal and hence its models must do a lot with a little (Cartwright 2009, p. 48). However, the ones of utility and profit maximization are relatively uncontestable, but they should be rather treated as approximate empirical relationships and not as the universal regularities. Thus the (i) seems to be met in the Varian's model.

As far as the (ii) is concerned, we should check whether consumers' and stores' impact on the workings of the market are separately invariant under interventions. Starting with *consumers* ➔ *market dynamics* causal channel, it is enough to say that in the neoclassical setup the utility function of a given consumer (or in Varian's terms "rational behavior") is stable, namely it does not change in reaction to adjustments in the context. Even if we allow for context dependency of preferences, then still the elements of neoclassical utility maximizing behavior are to be present (cf. Kahneman and Tversky 1981). So, it is hardly imaginable how one could break the explanatory relation between (invariant) consumers' behavior and market dynamics in Varian's model. The same holds for profit maximization of firms, and thus (ii) is met.

Another important issue is to check whether the two above mentioned *forces* are independently changeable (iii). Or, in other words, whether the model enables us to interfere with the behavior of one component without necessarily interfering with the behavior of others. For instance, is it possible to investigate the effect of profit maximization behavior of firms on the *shape* of price dispersion in cases of varying behavior of consumers, including violations of the principle of utility maximization? First, and very intuitively, if we agree that the firm follows the rule of maximizing its profits, then it is to do so even if consumers would behave non-rationally, partly rationally, and so on. Secondly, and now in the context of Varian's model, the impact of stores' behavior on the market does not interact with the consumers' decisions, however, the model enables us to determine the join effect of two groups on the workings of the market[9]. This is very similar to the Woodward's case where the description of gravitational force does not contain the coefficient of kinetic friction. However, does it hold for the symmetrical relation? Does a hypothetical change in stores' behavior, e.g., they are now not interested in profit maximization but only in achieving satisfactory profits, is to impact consumers' decisions and hence market dynamics? What changes now is the price-setting behavior of stores, and hence consumers face different prices (or, more precisely, different probabilities of prices), but they can still behave in rational manner. So, the symmetric relation holds and thus the (iii) is met.

The presence of the (iv) condition in Varian's model is more difficult to check. This is so because in economics a general *law of composition* that dictates the final result of the workings of mechanism's components refers only analogically to a simple vector addition where causes are summed, but rather it is the requirement that all the equations of simultaneous-equation models must be satisfied at once (Cartwright 2009, p. 50). So, equations like the one describing the net force on the block along the plane in Woodward's 2002 paper can hardly be found in economics. Nevertheless, economic models compose the individual *forces* usually by saying that the equilibrium outcome is achieved when consumers and producers are maximizing utility and profits respectively. In the case of Varian's model the picture is rather nuanced, since on the first reading it seems that the stores play the very first role in shaping prices (see, e.g., eq. no 12). However, on analyzing the model more carefully it occurs that stores are constrained by consumers' characteristics, primarily their reservation prices[10]. So, the overall output of consumers' and stores' interactions can be determined and thus (iv) is met.

Since conditions (i)-(iv) of Woodward's definition are met, thus Varian's model can be treated as a model of mechanism in **MECH** sense. Now, the following questions are worth asking. First, how such a model explains? Second, how it enables us to learn about the actual world? Three, what criteria one should apply in choosing the *right* model of price dispersion in case of having multiple models fulfilling the **MECH** conditions? The next section deals with each of these questions.

3. Learning from models

Historically, in economics, the nomological deductive model of explanation was a dominant one. It equated explanations with deductions of descriptions of economic phenomena from premises including scientific laws. Although there were (and still are) some controversies about the nature of scientific laws, philosophers generally agree that such laws are "nonaccidental generalization

[9] This refers to the fact that in Varian's equation no 12 (p. 656) describing profit maximizing density function we do not have consumers' preferences, but only fixed costs, number of informed and uninformed consumers, and a consumer's reservation price (cf. earlier models of price dispersion by Salop and Stiglitz (1977) as well as the one by Shilony (1977)).

[10] It is even noticeable on inspecting the eq. no 12 where for price equal to r (reservation price) the denominator is not defined, and for $p>r$ consumers' demand equals zero (see, proposition 1 in Varian's paper).

that made no reference to particulars and supported counterfactual claims" (Kincaid 2012, p. 137). The deductive nomological model of explanation was finally rejected due to fundamental problems in distinguishing scientific from nonscientific laws as well as the general issue of many examples showing that derivation from a law was neither necessary nor sufficient to explain. Therefore, many alternative theories of explanation emerged (for an interesting overview, see, e.g., Salmon 1989). It is for sure beyond the scope of this paper to present all of them, however, a common trait among them is that they do not explain by invocations to universal laws.

As far as economics is concerned, I should comment now on a typical way economists defend the centrality of laws in economic theory. It is interesting that they do it by referring to modelling practices of economics. In other words, they ask how laws produced by models with false assumptions can explain. They answer that these laws are not false but qualified as *ceteris paribus*. As Kincaid (2012, p. 145) rightly notices, that view is supported by Cartwright's analysis of science, especially physics, since its fundamental laws are just *ceteris paribus* laws, e.g., the force on a body due to gravity is equal to mass times acceleration only assuming no other physical forces are present. The same holds for economics where a typical way of reasoning is to have models as producers of theories that are always true *in* models but only true in *ceteris paribus* sense if referred to the actual world. For instance, in the neoclassical model of consumer choice, the higher the price, the less a given consumer is willing to buy (the law of demand). This law is always true in the model, but not *vis-à-vis* the actual world where its *ceteris paribus* interpretation is claimed to be true, namely all other things being equal or held constant (*ceteris paribus*), the higher the price, the less a given consumer is to buy. However, philosophers of economics have several objections against such a way of defending the centrality of laws in model-based explanations. First, claims qualified as *ceteris paribus* laws seem to be unfalsifiable (Earman et al. 2002). Second, if one attempts to spell out *ceteris paribus* laws, then one arrives at the nomological deductive model of explanation which is however claimed not to be the adequate one. Thus, postulating the existence of *ceteris paribus* laws does not offer an acceptable answer to the question on how models with false assumptions explain. Here the semantic view can help us in solving such a puzzle, since it claims that this is a separate empirical question whether there is anything in the world corresponding to the abstract entity (e.g., a mechanism) described by the theory and embedded in a theoretical model. Therefore, the question on how we may have good empirical evidence for models is distinct from the question on how models explain. I am to return to this former question later, but now let me continue my reflections on the latter. The question on how literally false models explain has gained a considerable attention in philosophy of science with such answers as the following ones (according to Kincaid 2012):

1. Models provide "insights". This is a common informal rational given by social scientists in defense of particular models.
2. Models unify, i.e., they show how different phenomena might be captured by the same model (Morgan and Morrison 1999).
3. They serve as instruments – we do things with models (ibid.).

4. Models are isomorphic to the phenomena of interest (Giere 1988).
5. Models are nomological machines and theoretical models are just blueprints for such machines (Cartwright 1983).

Now, how about the kind of explanation the above-described Varian's model offers us? It is for sure that his model does not unify (Ad. 2) and it is not used as a typical instrument (Ad. 3). Also, it is not isomorphic to the phenomena of interest (Ad. 4). Thus, the very first ("insights") and the last ("machines") of the five above mentioned options seem to be worth investigating. Some insights are given by Varian himself in his 1978 paper where the emphasis is put on the *causal* fit between the model and the real world:

> The goal of causal application is to explain aspects of the world that can be noticed or conjectured without explicit techniques of measurement. In some cases, an aspect of the world (such as price dispersal […]) is noticed, and certain aspects of the micro-situation are thought perhaps to explain it; a model is then constructed to provide the explanation. In other cases, an aspect of the micro-world is noticed, and a model is used to investigate the kinds of effects such a factor could be expected to have (emphasis added) (Gibbard and Varian 1978, p. 672).

An important virtue of explanation is its simplicity: "[…] it is important that one be able to grasp the explanation. Simplicity, then, will be a highly desirable feature of such models. Complication to get as close as possible a fit to reality will be undesirable if they make the model less possible to grasp" (ibid.). Sugden (2000, p. 13) interprets this passage from Varian's paper in terms of simplicity as a model's characteristic which makes communication with the audience easier. Also, for him simplicity serves as device legitimizing the presence of highly unrealistic assumptions in models. I disagree with such an interpretation of Varian's ideas. In my reading of his paper, I treat simplicity as a suggestion for searching essential mechanisms explaining the real. Or, even more, a good model tells a story (one story) (Gibbard and Varian, p. 666) which must give an answer that is right in its essentials (p. 669) and unrealistic assumptions "are chosen not to approximate reality, but to exaggerate or isolate some feature of reality" (p. 673). So, the mechanism (treated in the sense of **MECH**) is such an answer, since it refers to the essentials of a given systems. Also, my analysis of his *Model of Sales* supports the above interpretation of *simplicity* from his 1978 paper.

Here enters my conception of a believable world under which a model is an entity containing mechanisms that are believed to be similar to the ones operating in the real world. They are similar because a model of a mechanism demonstrates the reality of mechanism by isolating it. According to **MECH** what we find in mechanisms are not the universal regularities but rather a set of Millian tendencies or capacities (natures) as Cartwright would name them. They are crucial for the mechanism's ability to explain: "Our most wide-ranging scientific knowledge is not knowledge of laws but knowledge of the natures of things" (Cartwright 1999, p. 4) and later she adds that "Idealizations and the inference to natures form a familiar two-tiered process that lies at the heart of modern scientific inquiry" (ibid., p. 83). If "capacities are real" (Cartwright 1989, p. 1), then mechanisms are real too. Also, the reality of capacities leads us to the acceptance of the idea of the world as a world of powers (capacities) (Mumford 2013, p. 17). This is a typical realist claim and thus anti-Humean about powers. In such a conceptual schema instead of referring to capacities in the form *in-the-nature-of-sth.-is-to-produce*, one may say *it-is-believable-that-sth.-is-to-produce*.

If models are understood as believable worlds, then the theoretical insights they produce are beliefs[11]. The very category of belief is present in contemporary epistemology where the quality of understanding that the models offer is taken as a measure of their goodness (cf. Suarez 2010). It is important to note that understanding means having true beliefs about the world, or, more precisely, believing truths and not believing falsehoods. So, understanding is defined in terms of belief and not in terms of universal knowledge. Let me explain. Invocations to knowledge are much absent from contemporary epistemology (David 2001, p. 152). What contemporary epistemologists value most is connecting justification to the non-epistemic concept of truth. As Alston states it:

> Epistemic evaluation [justification] is undertaken from we might call 'the epistemic point of view'. That point of view is defined by the aim at <u>maximizing truth</u> and <u>minimizing falsity</u> in a large body of belief [...]. For a <u>belief</u> to be justified is for it, somehow, to be awarded high marks relative to that aim (emphasis added) (Alston 1985, pp. 83-84)[12].

The aim is usually defined in terms of searching for truth, however, with emphasis on searching, since arriving at universal knowledge that is free from doubts is hardly possible (cf. Lehrer 1990, pp. 20-38). While searching for the truth we successively produce some often vague descriptions of the reality giving us insights into the way the world works (max truth) and does not work (min falsity). In the same vain we may say that the goodness of a model's fit to the reality can be conceptualized in virtue of the extent to which a given model offers us a justified belief about the real, namely the belief aiming at maximizing truth and minimizing falsity about the model's target. If our model meets the **MECH** requirements and refers to the essentials of the modelled, then it is a believable world, and thus:

DEF1: Any model which meets the **MECH** requirement and refers to the essentials of the modelled is a believable world.

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[11] Here the idea of models as believable worlds differs from the hypothetical one of models as possible worlds (cf. Nowak 1992, pp. 9-10). In the case of the latter, the theoretical claims built upon a given possible world "[do] not intend to speak about reality. A pure theory is just a picture of a possible world which does not actually exist" (Händler 1982, pp. 74-75, making this observation in his discussion of the empirical applicability of the general equilibrium theory in economics). In the case of the former, from the very beginning the model is constructed in such a way as to mimic its real target, so it intends to speak about reality. However, on the other hand, the Nowak's claim that "[T]he smallest is the distance between the intended possible world of the kind and the actual world, the truer the counterfactual is" (1992, pp. 9-10) underlines some similarities between these ideas. But still more research is needed in comparing these ideas, since, for instance, the counterfactuals that are made in models of believable worlds are different from the ones of models of possible worlds (cf. theory of counterfactuals by D. Lewis, e.g., 1973). I would like to thank an anonymous referee for bringing the idea of possible world to my attention.

[12] This claim can be treated as a symptom of Alston's general denial of deflationism in the theory of truth. However, an in-depth study on this issue is beyond the scope of my paper.

It is worth noticing that meeting the **MECH** requirements only is not enough for a model to give us a good understanding of the way the world works (max truth and min falsity). The model must correctly make distinctions between essential explaining items (including mechanisms) and the ones of secondary importance. This is particularly important, since we may have a set of models (in **MECH** sense) that offer different explanations of the phenomena of interest. This is precisely the third question from the reminder of the previous section, namely what criteria one should apply in choosing the *right* model of price dispersion in case of having multiple models fulfilling the **MECH** conditions? But before focusing on this question, let me conclude my analysis on the ways economic models explain. Here I subscribe to the view that explaining means identifying the cause(s) of a given phenomenon. However, as Steel (2011, p. 122) claims:

> (M) *X* is a cause of *Y* if and only if there is a mechanism from *X* to *Y*. (M) is not intended as a universally true principle regarding causality, since there is presumably some "rock bottom" level of physical causation below which no mechanism lie.

So, models explain by depicting structures which enable the working of mechanisms, or models are just mechanisms' descriptions. And such models of mechanisms *produce* beliefs about the real world and thus these beliefs are always true in models producing them (cf. "semantic view" on models). Here my answer to the question on how models explain refers closely to the Cartwright's idea of models as blueprints of nomological machines that produce "insights" about regularities present in the actual world. Although the identification of a mechanism between *X* and *Y* is a necessary condition for explaining *Y*, it is not a sufficient condition for having a believable world (in sense of **DEF1**) of *X*–*Y* causal interplay. What is needed is high level of *essesimilitude* of such a model (cf. *essentials* in **DEF1**) which is understood as likeness or closeness of model's mechanism to its real counterpart (cf. Mäki 1991)[13]. This is what the method of isolation aims at in economics. However, economists often isolate so strongly that a phenomenon is isolated in its "pure" form without disturbing factors. As Niiniluoto (2002) rightly claims such situations are not "parts" of the real world and thus theoretical claims describing the model world are not true in its target. My solution of this problem is that they are *partially* true, namely they are beliefs about the target.

Here, an interesting insight emerges from my analysis. It seems that models being constructed in such a way as to comply with **DEF1** are inherently isolations. Although more in-depth research is needed to confirm such a proposition**,** the basics arguments in favor of it are based on the following: 1/ a mechanism must have subparts and hence a researcher is obliged to identify a set of *forces* making the overall effect of the mechanism and consequently the risk of concentrating on the wrongly chosen single *force* is minimized; 2/ the requirement that each component of mechanism is described by a generalization that is invariant under interventions

[13] Although in philosophical literature the notion of *essesimilitude* refers usually to theories, I think that one may use it (at least analogically) in reference to models, if models are understood as entities producing theoretical claims (cf. Mäki's (2012) insights on models and truth). Here I assume that models capturing the "essence" of the target are to give rise to theoretical claims about this very "essence" (cf. Niiniluoto 2002, p. 218). However, an in-depth study on this issue is beyond the scope of this paper.

guarantees that only *influential forces* are to be taken into account what immediately excludes false generalizations such as the one that manipulating the barometer is to cause the storm; 3/ the independently changeable character of mechanism's components assures the modeler not to include in the description (model) the *forces* that change their rules of work in the presence of other *forces* and also enables the modeler to assess the strength of each *force*, and thus the risk of taking into account some *pseudo forces* is minimalized; 4/ the (iv) condition of **MECH** assures the model to be similar to the real in that sense that it enables *summing up* the effects of the workings of all forces and hence arriving at the overall effect of the mechanism. On a more philosophical level the justification for the **MECH** models ability to capture the essentials is due to the fact that in the real world mechanisms have the status of unobservables and thus their existence is only manifested on the empirical and factual levels of reality[14]. Consequently, a given mechanism's manifestation (in the real world) and a given mechanism's description (in the model world) refer to the very same mechanism. However, the *fit* is perfect in the case of the actual world (manifestations are products of the true and acting mechanism) and imperfect in the case of the model (e.g., a model of mechanism does not capture *all* the ingredients of the mechanism operating in the real). But still the goal of explanation "is to posit a mechanism (typically at a different level to the phenomenon being explained) which, if it existed and acted in the postulated manner, could account for the phenomenon singled out for explanation" (Lawson 1997, p. 212). So, both in the real world and in the model the mechanism is somehow *hidden* – in the case of the former behind its manifestations and in the case of the latter behind its description[15].

Now, the second question should be asked: how the mechanism described by the model relates to the one operating in the real world? Or, what is the nature of model-world inference? First, I agree with Hausman that the point of models in empirical science is to assist scientists in making claims about the world. In doing so, however, we do not test the model as such *vis-à-vis* the real world, but just "an *application* of a model, a hypothesis stating that certain elements of a model are approximately accurate or good enough representations of what goes on in a given empirical situation" (Guala 2005, p. 219). Next, he adds:

> The fact that a model turns out not to work under certain circumstances does not count as a refutation of the model but only as a failed test of its applicability in a given domain (ibid., p. 220).

The above is relatively intuitive: the closer a given empirical domain to the model's structure is, the higher *probability* that the model's insights are to correctly explain the workings of such a

[14] I refer here to the idea of three levels reality by T. Lawson (1997, p. 21), namely the empirical (experience and impression), the actual (actual events in addition to the empirical), and the real (structures, powers, mechanisms, and tendencies).
[15] Cf. Steel's (2011, p. 130) thesis that mechanisms are of central importance for learning about cause and effect in social sciences. This follows from his assertion that if there is a mechanism from *X* to *Y*, then *X* is a cause of *Y*.

domain[16]. However, the fit is never perfect and thus the idea of models' insights as beliefs about the targets. But how the very act of making inferences from models in **MECH** sense to the real world looks like? Let me start by naming the mechanism responsible for price distortion (**F**, henceforth) and described in Varian's 1980 paper as **MECH1**. Also, the set of regularities between variables of his model is depicted here as **R** (take, for instance, relations between model's parameters as presented in Table 1 of Varian's paper, e.g., the claim that more uninformed consumers cause the average price paid by the uninformed consumer to rise). Therefore, the structure of model-world inference is as follows:

E1 – in the Varian's 1980 model, **F** is caused by **MECH1**, and this model produces **R**;

E2 – **F** occurs in the real world ("[…] the form of the resulting price strategy […] does not seem out of line with commonly observed retailing behavior" observes Varian 1980, p. 658);

E3 – **MECH1** operates in the real world ("[…] some aspect of economic life is noticed" says Gibbard and Varian 1978, p. 673);

Therefore, there is reason to believe that:

E4 – in the real world, **F** is caused by **MECH1**, and thus statements taken from **R** are believed to adequately describe the real world. In other words, they are beliefs about the target.

Now, the interesting question is why knowledge about a mechanism for a causal relationship between shops' behavior and price distortion can be inferred more reliably than the causal relationship as such between these two factors (cf. Reiss 2008b, p. 113)? It is particularly important, because economists quite often explain and make theory-world inferences on statistically inspecting patterns in empirical data-sets. However, such an approach is possible only if one has data, but also knowledge about possible confounders as well as the relationship between causality and probability. Practicing economists know well that such an econometrically informed way of doing economics is often hardly possible. This is also the case of Varian's model – we do not have a statistical data analysis here but rather a conceptual exploration into various causes of price dispersion. This way of making model-world inferences not only diminishes the risk of doing economics without theoretical underpinnings, but can help econometricians by telling them, for instance, what kind of data is necessary for explaining given phenomena (cf. critique of theoretical emptiness of econometrics by McCloskey 1985). In this context, a must to be cited piece from economic literature is an interesting passage from Friedman and Schwartz monumental work on *The Monetary History of the US* (1963):

> However consistent may be the relation between monetary and economic change, and however strong the evidence for the autonomy of monetary changes, we shall not be persuaded, unless we

[16] I put the term 'probability' in italics, since by using it I refer to the idea of verisimilitude – we are interested in theories ("models insight's") with high degree of verisimilitude (closeness to truth) (cf. Popper's insights on the origin of the idea of verisimilitude (in contradistinction to probability) and its closeness to the idea of belief (as opposed to truth), e.g., Popper 1963/2002, p. 540).

> can specify in some detail the <u>mechanism</u> that connects the one with the other (emphasis added) (p. 229).

What I should add is that an identified mechanism, e.g., the one Friedman refers to, does not enable us to learn about the real world itself. It does its job by giving us insights (beliefs) about the target. As it was stated earlier, the closer the model to the target is, the more accurate beliefs it produces are. So, the remaining question is how we can compare different **MECH** models of a given phenomenon.

As it was just mentioned, models are used as producers of theoretical insights about their targets. So, in assessing models one in fact is to check to what extent theories brought upon by models survive transition from the world of the model to the real world. After all that have been said above, it should be clear that theory which is true *inside* the model is never (totally) true vis-à-vis the real world. Let me refer again to the final part of the Varian's paper, namely the conclusion that "Although this causal empiricism can hardly be conclusive, it suggests that the features of the model described here may have some relevance in explaining real worlds behavior" (p. 658). Before offering such a statement, Varian describes his own observations on how shops in his neighborhood area use sales. So, for him, his model should at least offer some theoretical insights explaining these initial empirical observations. However, what is needed is a systematic empirical investigation into the applicability of the model's theoretical claims to a particular domain. It should be noted, however, that from the outset these claims are not to be suitable for every domain. So, a given model offers a specification of the conditions that make its insights potentially applicable in real situations. For instance, since in Varian's model shops can freely set prices thus this model's insights will be more appropriate in free markets (e.g., in the US) than in highly regulated market environments – it is for sure unreasonable to expect North Korean shops to behave in a manner described by Varian. So, the above mentioned statistical empirical investigations of the validity of a given theory should be conducted in domains at least slightly similar to the conditions of the model that was used to produce such a theory. It is thus unreasonable to test models' insights in environments far beyond the ones specified by models' structures.

Now, let us look at the idea of the believable world by comparing it to the concept of a credible world by Sugden (2000). According to him such a world is significantly similar to the real one (p. 23). Also, a credible world can be understood as a description of how the world *could* be (p. 24) and credibility in models is like credibility in 'realistic' novels (p. 25). Next, credibility in economic models means that they are coherent (assumptions of models are not arbitrary chosen) as well as "they cohere with what is known about casual processes in the real world" (p. 26). However, in summing up his arguments Sugden concludes that "[he] cannot give anything remotely like a complete answer" (ibid.) to the question of how a credible world should be defined. In his 2009 paper on *Credible Worlds, Capacities, and Mechanisms* he clarifies his ideas and claims the following:

> Credibility is not the same thing as truth; it is closer to *verisimilitude* or *truthlikeness*. We perceive a model world as credible by being able to think of it as a world that *could be* real […]. One crucial difference between a credible world and an isolation is that a credible world may be constructed around general empirical regularities – one might say, empirical laws – that are merely postulated. For all we know, these regularities may *not* be part of how the world really works. All that is required is that, in the current state of knowledge, they are credible *candidates* or truth (p. 18).

In the subsequent parts of his 2009 paper Sugden offers two sections – no 6 on models as isolating tools and no 7 on social mechanisms. Although he is quite ambivalent whether to accept Cartwright's approach to isolation and Schelling's ideas of social mechanisms, he somehow sees a need to conceptually link the ideas of credibility, isolation, capacity, and mechanism. Finally he concludes that we do not have such an overwhelming framework and hence "there is still a gap to be crossed and that requires inductive inference" (p. 26).

If I am to compare now my idea of believable world to the one of credible world, I should start by saying that these two are not identical. Mine is more sharp and thus less general that the Sugden's concept of credible words. Also, it is more applicable, since it is based on the **MECH** definition. Moreover, it attempts to resolve the problem of inductive leap in learning from models by claiming that gaining knowledge about the real world rests on assuming the similarity between the model and its target what guarantees the applicability of model's insights to such a domain. But still more research on **MECH** models is needed in order to check the validity of this approach in accounting for the ways economists model the real world. In this context it is worth checking how the above developed ideas can help us in solving the Reiss's (2012) paradox. This is done in the following section and serves as an assessment of the validity of my approach to economic modelling.

4. The Explanatory Paradox by J. Reiss (2012) and an Attempt at its Resolution

Reiss's 2012 paper brings together many issues philosophers of economics are interested in. It is particularly intriguing for those dealing with economic models. Its value rests in showing that the great many contemporary philosophical approaches to economic models are themselves contradictory. Also, it raises the issues so fundamental that they should be of interest not only for philosophers of economics. The Reiss's message is nicely given in the form of the following trilemma:

(1) Economic models are false.
(2) Economic models are nevertheless explanatory.
(3) Only true account explain.

My main point of disagreement is with the trilemma's first point not because I claim the contrary, but because I think that economic models are neither true, nor false, but rather they aim at maximizing truth and minimizing falsity in a large body of belief about the real world. They are producers of such beliefs. So, truth and falsity *mix* in insights (beliefs) models give us about the real world and thus the idea of models as believable world. This leads also to my disagreement with the trilemma's third point. I begin with the first claim of the paradox.

Reiss saying that economic models are false does not mean that models have truth values, since according to him models are not sentences and only sentences are true or false, so when he speaks about truth and falsity of models he "speaks elliptically" (p. 49). Further he clarifies his views in the following way: "[…] the slogan 'all models are false' […] draw[s] attention to the undisputed fact that all models also *mis*represent their targets in a myriad of respects" (ibid.). In what follows Reiss argues against Mäki's way of resolving the problem, precisely his claim that a model can be true despite containing many falsehoods. So, for Mäki a model is always 'false' in many unimportant respects, but 'true' in what captures the causal factor of interest, e.g., the Earth's gravitational pull in Galileo's case. Reiss's cites here the following passage from Mäki's accounts of von Thünen's model of the isolated state:

> If there is a natural truth bearer here, it is neither this model as a whole nor just any arbitrary parts of it. It is rather a special component of the model, namely the causal power or mechanism that drives this simple model world: the Thünen mechanism. This truth bearer has a fair chance of being made true by its truth maker, the respective prominent causal 'force' or mechanism in the real system. It is the mechanism that contributes to the transformation of distance into land use patterns through transportation costs and land values (Mäki 2011, p. 60).

So, in Mäki's approach, models are not true as such but may contain truths about its various parts and most notably casual powers of mechanisms. However, Reiss denies the validity of 'isolation by idealization' tradition by claiming that economic models do not isolate in the Galilean sense for three reasons: 1/ Galilean idealizations are absent in Galilean thought experiments[17]; 2/ Galilean idealizations are quantitative, not categorical[18]; 3/ Galilean idealizations have natural zero[19] (Alexandrova and Northcott 2013, p. 263). Therefore, Reiss (2012) develops his final and fundamental criticism of Mäki's approach first by claiming that "the models of economics […]

[17] To be honest I share with U. Mäki (2013, p. 274) difficulties in fully understanding the Reiss's distinction between Galilean and non- Galilean assumptions, so in here presented clarifications I just offer some insights taken rather straightforwardly from Reiss (2012). While claiming that Galilean idealizations are absent in Galilean thought experiments, Reiss states that the factor assumed away does not normally appear in the latter: "The assumption of no air resistance cannot be read off the model […]. By contrast, the non-Galilean assumptions Hotelling uses are all explicit part of the model, and they are assumptions without which no result could be calculated at all" (p. 51). According to him, the majority of assumptions economists are making in their models are similar in kind to the ones of Hotelling.

[18] Reiss (2012, p. 51) explains it as follows: "Galilean assumptions usually concern quantitative causal factors. Different media produce different degrees of resistance. Hotelling's assumptions are categorical. Different geographies are different kinds of thing and not the same kind of thing to a different degree".

[19] Here he states that "Galilean assumptions usually concern a causal factor that has a natural zero. No air resistance is such a natural zero" (Reiss 2012, p. 51), but the ones in economics, e.g., the geographical shape of the market, do not have natural zero.

are by and large very much unlike Galilean thought experiments [‘isolations by idealizations’]” (p. 51), and consequently “we do not know where to look for ‘truth in the model’” (p. 52).

Mäki (2013) rejects Reiss’s denial of the possibility of isolating by idealizing in economics. For instance, he does not claim that having a natural zero is a necessary condition for an assumption to be of a Galilean kind. More fundamentally, in his research Mäki presents many cases of economic models that include many idealizing assumptions. The only thing Mäki acknowledges in regard to the Reiss’s criticism is a need for checking the role a given assumption plays in the model. If, for instance, one assumes a balanced state budget, then it may be interpreted as an assumption defining the cases in which this model can be applied – if you have a balanced budget, then you can use that model. We have a domain assumption here. On the other hand, saying that a budget is balanced may be interpreted that the state of the budget does not have any importance for the process we try to explain. So, in this case we have a negligibility assumption (Musgrave 1981). I share with U. Mäki his strong conviction that the method of isolation is not only possible, but that is the central method of science, including economics (Mäki 1992; 1994; 2009; 2011; cf. Cartwright 1989). The possibility of isolating in its broadest sense is a necessary condition also for my idea of a believable world, since without being able to isolate, it would be hardly possible to distinguish between mechanisms’ parts and identifying mechanisms as such would not be feasible.

Now, let us come back to the first point of the Reiss’s trilemma but now in contrast with the idea of a model as a believable world (in sense of **DEF1**). Such models give raise to beliefs containing falsehoods as well as truths about their targets. So, the question is the following: do models as believable worlds have truth values? Does Mäki’s (2011, p. 60) claim that a mechanism *inside* a given model can be qualified as a truth bearer imply that models of mechanisms (in **MECH** sense) have truth values? I would not say that for at least two reasons. First, I do not believe in the ability of models to perfectly represent the mechanisms operating in the real world. Even I doubt whether we can have purely true and purely false elements in such models. What I claim is that we can only assess the extent to which a given model offers us a justified belief (not knowledge) about the real. This is a virtue of maximizing truth and minimizing falsity about the model’s target. So, each element of a given model is in some *distance* from truth. The aim of the modeler is to reduce this gap. Or, in other words, to include in mechanisms only these forces that are approximately correct and thus reducing the gap between the model’s mechanism and its real counterpart (cf. Hausman 2013, p. 253). For instance, in the case of Woodward’s (2002) block sliding down an inclined plane it means to include two forces only (gravitational and the one due to frictions) and not some extra ones, e.g., the force exercised on the block by the wind[20]. Such an approach guarantees also the stability of the explanation – the behavior it describes is stable across different environments, so robustness testing is possible

[20] Adding of such a force would break the condition of the independently changeable character of generalizations governing each component of mechanism, since wind is definitively to impact the workings of the kinetic friction. So, **MECH** requirements ease the process of idealization.

in case of believable worlds (cf. Grüne-Yanoff 2013, p. 255). Thus, for believable worlds, the Reiss's trilemma can be restated as follow:

(1`) Believable worlds offer insights that maximizes truth and minimizes falsity in a large body of belief about the real world.

(2`) Believable worlds are nevertheless explanatory.

(3`) Only beliefs that are *candidates* for true explain.

The first above statement recapitulates what have been said earlier. The second follows from **DEF1** (cf. discussion in section 2). So, the third only requires now some more comments. Here, instead of Reiss's 'account(s)' I refer to beliefs and '*candidates* for true' replaces the simple true from the original trilemma. What remains is the invocation to explain, since even on briefly inspecting the most important economic models their authors usually put explanation as the main research goal while crafting and using models, e.g., the very last sentence from the Varian's model is the following: "[…] it suggests that the features of the model described here may have some relevance in explaining real world retailing behavior" (1980, p. 658). Although the very notion of 'accounts' from Reiss's trilemma is imprecise, the detailed reading of his paper suggests that by true accounts he understands first credible worlds and next he checks whether the (3) holds for models as unifying entities (in the sense of Kitcher's (1981) accounts of unification). He concludes that neither credible worlds, nor the letter approach can account for models as being explanatory, since they are not true and only true account explains. Thus, according to him, the paradox remains.

In my formulation of Reiss's (3) the emphasis is put on saying that it is the justification of the belief that makes it a *candidate* for true and the highly justified beliefs are the ones that maximizes truth and minimizes falsity about the model's target. Importantly then is to conclude that we cannot have a purely true belief, or, a model perfectly representing the real, but what we should aim at is the model whose structure and its parts (together giving raise of mechanism) are as close as possible to the characteristics of its target. For instance, in case of the Varian's model of sales what explains is a set of regularities between variables of his model and these regularities are beliefs about what is going on in the real world. The model-world inference is thus due to the identification and subsequent description of the mechanism responsible for price dispersion and it proceeds from E1 *via* E2 and E3 *and then via inductive leap* to E4 (see, section 3). As it was explained in section 2, beliefs based on regularities produced by models do not have a status of purely true descriptions of mechanisms operating in the real, but rather these descriptions are as close as possible to their empirical *ideals*. Thus, (1`), (2`), and (3`) are all true and hence mutually consistent and thus the paradox is resolved.

5. Conclusions

The goal of this paper was to shed some light on the dichotomical treatment of models as isolations and constructions. In doing so I was able to show that what characterizes the great many of economic models is that they are not (only) idealizations, nor (only) constructions, but believable worlds, precisely models of mechanisms giving us justifiable beliefs about the way the world works. The mechanisms are defined here using the ideas of Woodward (2002). The example of Varian's model of sales was presented as a typical case of such a model. Also, the question on how models explain was answered. It was shown that models explain by producing theoretical insights that are always true within models but they are just beliefs if claimed to accurately describe the real world. Thus, such beliefs are more credible if the target is close enough to the model's structure. This enabled me also to shed some light on the issue of model-world inferences. The validity of my approach was tested by using it in an attempt at resolving the Reiss's paradox of explanation. It was done in by reformulating it in the following way: 1/ models are not true or false but rather they maximize truth and minimize falsity in a large body of belief about the real world; 2/ such beliefs can never function as perfectly true but rather as *candidates* for true; 3/ finally, these models are explanatory. The ability of believable worlds to explain lies in the fact that they refer to mechanisms that operate in the real world. However, more research is needed in order to further justify the claim that constructing models of mechanisms (in Woodward's sense) raises the probability of identifying candidates for crucial mechanisms and hence explaining economic phenomena well. This paper offers same basic ideas supporting such an assertion. Also, the concept of economic models as believable worlds better suites with the claim that in economics we do not have universal laws but rather Millian tendency laws (capacities) or empirical regularities of no-law status. In this context, a lot more in terms of philosophical refection need to be done in order to refer the here proposed ideas to the one of models as blueprints of nomological machines (Cartwright 1999). I think that the research endeavors sketched above can enhance our knowledge about the status of models in economics. This paper is an invitation to take up this challenge.

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