STUDIA METODOLOGICZNE
NR 39 • 2019, 153–170
DOI: 10.14746/sm.2019.39.6

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Models and Beliefs they Produce

Abstract. This paper does not focus neither on models nor on modelling procedures but rather on the nature of knowledge about the world models give us. It puts forward the thesis that models are producers of beliefs about their targets. These beliefs may differ both in degree and scope. They are offered by various kinds of models, including models understood in terms of isolations as well as minimal models. This paper puts emphasis on what kind of entities beliefs produced by economic models are.

Keywords: philosophy of economics, models in economics, nature of laws in economics, beliefs.

1. Introduction

Economics is a modelling science. It uses models in order to understand the ways economies function. However, these model worlds are always not perfect pictures of the targets they refer to. But still they can explain. This fact alone warrants the curiosity of philosophers interested in analysing the interplay between models as well as their relationship to laws, theories, and empirical phenomena. First, many of them investigate the ways models can be unrealistic. Here enters U. Mäki's ideas of models' realisticness and unrealisticness [see, e.g., Mäki, 1998]. There are many kinds of unrealisticness. For instance, if a given model lacks its target, then we deal with the issue of referentiality [e.g., Mäki, 2017]. Second, as far as the connection between models and theories is concerned one may subscribe to the so-called semantic view and hence theories can be treated as families of models [e.g., van Fraassen,

1980; Giere, 1988]. However, such an approach poses many problems, e.g., it treats models and theories as interrelated entities but what is quite obvious from observing the ways researchers do science is that they often use models as autonomous agents [Morgan, Morrison, 1999]. And third, what many philosophers are interested in is the issue of models' targets – do we start building models by referring to empirical phenomena or maybe we construct models of artificial worlds and only later we try to refer models' insights to some possible facts [Grüne-Yanoff, 2013]? We may easily multiply such questions, however, what seems to be crucial here is to investigate the ways we learn from models.

This paper refers to the above-mentioned issue of learning from models in a very special way. It asks about what kind of insights models offer us. So, I treat models as producers of our knowledge about the world. What preoccupies me is the character of this very knowledge. Here I definitely do not subscribe to the view that the only kind of knowledge models can give us is a *theoretical* knowledge, namely that models are producers of theories. Consequently, the very traditional and dichotomical treatment of the interplay between models and theories offered by the syntactic and the semantic view of theories is definitely inadequate. If not theories, then what is produced by models? This is the issue I put emphasis on here.

In particular, what I try to show here is that models give us beliefs about the ways the world works. Consequently, section 2 starts with the presentation of two kinds of models, namely models of phenomena and minimal models, precisely models of possible and not actual processes, mechanisms, or events. Section 3 introduces the notions of possibilities and beliefs as the right descriptions of the kind of knowledge models give us. Next, in section 4, a further treatment of beliefs is offered. Conclusions follow.

2. Two kinds of models

If one takes the definition of models, for instance, from *Encyclopaedia Britannica*, then it is clear that models are various kinds of entities but all of them refer to real phenomena. It means that models are models *of something*. They are simplified pictures of their targets. Not only scientists use models

but we all do it – every map is a model of a given terrain. Therefore, models are idealizations, namely they take on board elements that are crucial from the researcher's perspective and they pass over the ones of secondary importance. Here we should refer to philosophical analysis of models developed by U. Mäki, N. Cartwright, and D. Hausman. They differ in many respects but in one they are compatible – they all refer to J.S. Mill's observation that causes of economic phenomena interfere mechanically and thus one can analyse them in isolation[1]. For Mill it was also clear that what is true inside models is not true vis-à-vis real phenomena. In his own words:

[…] the conclusions correctly deduced from these assumptions, would be as true in the abstract as those of mathematics; and would be as near an approximation as abstract truth can ever be, to truth in the concrete [Mill, 1836/2008, p. 45].

Here we see Mill's emphasis on deduction and thus inference from the model to the world is of deductive nature. Whether this is feasible is a separate issue to which I am to come back soon. But before that let me present Mäki's modern treatment of models as isolations and precisely his following reasoning:

> Agent *A*
> uses object *M* (the model) as
> a representative of target system *R*
> for purpose *P*,
> addressing audience *E*,
> prompting genuine issues of resemblance between *M* and *R* to arise,
> and applies commentary *C* to identify the above elements and to coordinate their relationships" [Mäki, 2009, p. 75][2].

What is however important here is that the issue of resemblance is understood in a very specific way. It is not a perfect isomorphism between *M* and

[1] Mill was of course conscious that some elements of in the socio-economic may interact chemically and hence hinder any isolation.

[2] Cf. Giere [2006, p. 60].

R [cf. Suppes, 2002]; it is not a partial isomorphism [da Costa, French, 2003]; also, it is not a similarity between the two [Giere, 1998]. Rather it is a possibility that the issue of resemblance between *M* and *R* may arise. What is thus required is a researcher's attempt at making a model (potentially) similar to its target. But judging about the existence of this similarity comes only in the second step. If such a judgment is positive then we are equipped with surrogate systems (models) that "are treated as mediating vehicles in attempts to gain indirect epistemic access to the real world" [Mäki, 2009, p. 77]. Resemblance thus matters and importantly one can ask also if we can learn something about the world if a modeller did not make any attempt at making his model similar to some existing phenomena. I am to come back to this issue while analysing minimal models.

Now, what a proper isolation means? In Lawson's words "[abstraction] must be concerned with the essential rather than merely the most general" [Lawson, 1989, p. 69], and in Mäki's account "[…] an isolating theory or statement is true if it correctly represents the isolated essence of the object" [Mäki, 1992, p. 344]. However, even a proper isolation is not a perfect one. So, if assumptions of isolated models are always not perfect then intra-model deductions are not perfectly transferable between models and their targets. But still, if models are similar to experiments then we may claim that in ideal conditions (i.e., in models) we can investigate the way a given causal mechanism is to work. Thus, if intra-model deductions cannot be deductively transferred to our empirical domains then what kind of knowledge they are? Are we hence in need of referring to some blurred techniques based on induction and intuition?

Mill's sagacity can help us in answering the above questions. Our intra-model deductions can be treated as Mill's "abstract truth(s)" and these are only approximation to "truth(s) in the concrete". Since disturbing causes are always present in the concrete then one should not predict the actual result but rather a tendency to the result, precisely "a power acting with certain intensity in that direction" [Mill, 1836/2008, p. 56]. Thus models produce laws but not laws understood as Humean universal regularities but rather "laws of causation […] stated in words affirmative of tendencies only, and not of actual results" [Mill, 1843, p. 523]. So, for instance, instead of saying that lower interest rates stimulate investments one should claim that a decline in

the cost of money produces a tendency of investments to rise. So, in a sense, a statement affirmative of tendencies only can be treated as an approximation of Mill's "truth in the concrete".

Now, what is interesting is that Mill's tendencies can be interpreted in metaphysically rich manner. As Cartwright puts it "Substituting the word 'capacity' for Mill's word 'tendency', his claim is exactly what I aim to establish in this book […]. I suggest that the reader take my 'capacity' and Mill's 'tendency' to be synonymous" [Cartwright, 1989, p. 170][3]. Therefore, referring again to our example of price of money-investment interplay one can state that lower interest rates carry capacity to cause higher investments, or that in the very nature of lower interest rates is to cause a rise in investments. So, models understood in terms of blueprints of nomological machines create favourable conditions for the emergence of natures of given causes. Therefore, the knowledge they offer is again not about universal uniformities but rather nature's capacities. Please note here also that in both Mill's treatment of models' results and in Cartwright's one models inform us about possibilities in the world. In section 3 I am to present more insights into the ways we can understand possibilities in the context of models used to raise them.

After the above presentation of models treated as isolations I would like now to refer to a different kind of models, namely models conceptualized as constructions. In Sugden's own words:

It is essential […] that the model world is a construction of the modeller, with no claim to be anything other than this. Its specification is just whatever the modeller has chosen it to be. In particular, there is no claim that it has been constructed by stripping out some features of the real world and describing what remains [Sugden, 2009, p. 17].

Models come first and only later we give them a kind of ex-post interpretation. In the earlier presented approach where models are treated as isolations the world comes first and models only later. The most well-known case of constructivism in model building is Schelling's checkerboard model. There are many research papers dealing with his model with Sugden's ones [e.g.,

[3] There is an interesting debate concerning the synonymous character of Mill's tendencies and Cartwright's capacities (see, e.g., Schmidt-Petri 2008), however, in the context of this paper its extended presentation is not necessary.

2000; 2009] among others. According to Sugden what Schelling's model offers its readers is not a set of isolations of some empirical phenomena but rather a construction of a very specific and abstract *parallel* world. In other words, it *parallels* the real world rather than isolates it [Grüne-Yanoff, 2009, p. 89]. But now, how one learns from such a model? Here inductive inference plays a crucial role and those using models "[infer from] a particular hypothesis, which has been shown to be true in the model world, to a general hypothesis, which can be expected to be true in the real world too" [Sugden, 2000, p. 19]. This very inference is of inductive kind. Although it can be employed both in explanations and predictions, Sugden claims that the most productive kind of inference from model worlds to real phenomena is the one of abduction. It goes as follows: in the model world, *R* is caused by *F*; *R* occurs in the real world, and by a kind of inductive leap we may state the following: there is reason to believe that *F* operates in the real world. So, we do not have any kind of deduction here but rather something more informal. Some even claim, including Sugden himself, that credibility in models is similar to 'credibility' in realistic novels. As Frigg [2010, p. 251] puts it clear "models share important aspects in common with literary fiction" and even Cartwright states that "a model is a work of fiction" [1983, p. 153] and an "intellectual construction" [ibid., p. 144].

But now let us go even further in the constructivist approach to model building, namely to minimal models. In Grüne-Yanoff's [2009, p. 84] words "[…] the representational structure [of such models] as a surrogate is merely declare, and no further claims are made about the truth of its assumptions, the epistemic status of the principles used in its construction, or the similarity of its economic interpretation (or parts of it) with the real word"; and earlier he notes the following: "[minimal models] lack any similarity, isomorphism or resemblance relation to the world, to be unconstrained by natural laws or structural identity, and not to isolate any real factor" [ibid., p. 83]. All isolationists, including Cartwright, Mäki, and most probably also Mill, obviously would claim that we do not learn from such models since they are referentially and representationally unrealistic. However, Grüne-Yanoff takes a different stance. His way of reasoning starts from a claim that minimal models can be used in assessing propositions about impossibility as well as necessity of hypotheses forming our knowledge. As for necessity, people often maintain certain hy-

potheses about necessary connections in nature (e.g., between unemployment and GDP growth). Similarly for impossibility. But to learn from such a model, (1) it should represent something at least potentially possible and (2) playing with such a model should modify, e.g., my impossibility hypotheses. Thus such models play important role in learning and they do not only play heuristic role in science as, for instance, Hausman [1992] in his account of "conceptual exploration" would claim.

Now, I would like to stress one important aspect of these models, namely that what they produce can be termed as beliefs about the world. Grüne-Yanoff puts it as follows: "[the minimal model] conceptualizes existing beliefs in a new way, and it draws inferences from thus established possibility, given all relevant beliefs" [Grüne-Yanoff, 2013, p. 853]. And most importantly, since this very model can be treated as a work of fiction then it offers us beliefs about possible entities, processes, proprieties, capacities, and so on. Thus appraising such models representationally is a dead end. And importantly, such possibility claims are about possibilities in our world and not in some *possible* worlds. So, we are close here to the ontology of possibilities and not of actual facts. But now, aren't we close again to Mill-Cartwright view of models' claims as statements of tendencies/capacitates only? Answering such a question requires me to introduce notions of 'how actually' and 'how possibly' explanations. It is done in the following section and in section 4 we are to come back to Cartwright's view.

3. Possibilities and beliefs

In his illuminative chapter on three conceptions of explaining using how possibly clauses Persson [2012] refers to an insightful Dray's opinion which is definitely worth citing here:

The demand for explanation is, in some contexts, satisfactorily met if what happened is merely shown to have been possible; there is no need to go on to show that it was necessary as well. To put the point another way, I shall argue that although, as Professor Toulmin puts it, to explain a thing is often to 'show that it might have been expected', the appropriate criterion for [how-possibly

explanations] is broader than this; for to explain a thing is sometimes merely to show that it need not have caused surprise [Dray, 1957, p. 157].

We are in need of how possibly explanations if a given element of explanandum, say *X*, is ruled out by our existing belief system – it is simply impossible to have *X* (cf. impossibility hypotheses referred to earlier). Now, coming back to Grüne-Yanoff's insights, to learn about the world (e.g., the one *possibly* consisting of *X* among others) means to change somebody's confidence in impossibility hypotheses. More precisely, and taking into account the above Dray's opinion, how possibly explanations can account how events that are considered impossible (for instance, our *X* above) could have happened. Such a view was not unknown to Hempel who stated the following:

[…] some of the beliefs we hold concerning relevant matters of fact seem to us to make it impossible or at least highly improbable that X should have occurred [Hempel, 1970, p. 428].

Having the above in mind it is clear that on the other hand how actually explanations account how actual events have come into existence. But in what way how possibly explanations (for simplicity let us call them HPE henceforth) can be defined more precisely? Here Persson [2012] writes about three kinds of such explanations. First, they can show that *X* is not epistemologically impossible. For instance, Schelling [1971] just shows that it is not impossible that patterns of segregations may be present even without a strong preference for segregation. Second, HPE may offer us a kind of possibly how explanations of *X*. Here a nice example can be taken from biology where Darwin himself analysed various possible explanations of how birds wings have evolved. He offered some possible how explanations of this very fact without presenting convincing empirical evidence for some of them [Resnik, 1991]. Third, we have partial how explanations. Here one searches for possible mechanisms giving rise to phenomena in question. In economics we (possibly) have many mechanistic explanations [Hardt, 2017][4].

In the context of the above it is important that in science that are models that produce HPE, namely they offer us possibility claims. As Persson [2012]

[4] It is definitely beyond the scope of this paper to investigate whether these mechanical explanations are of ontic or epistemic kind [see, e.g., Illari, Williamson, 2012].

rightly stresses these claims are of epistemic nature, precisely they refer a claim under consideration to the current and known state of our world, e.g., *X* can be epistemically possible if it may be true for all that we know. However, and interestingly, in the context of models producing HPE one may refer also to alethic possibilities [Grüne-Yanoff, 2017]. For instance, one may claim the following: "It is possible that the author of this paper was born in London". Is this possibility claim true? Yes, it could be, however, since I was born somewhere in Poland it contradicts current knowledge. Now, coming back to Schelling's model and assuming that in actual cities racial segregation is caused by strong preference for self-segregating, then his model's claim that segregation may possibly arise event without such a preference degenerates into a kind of alethic possibility. However, and to sum up, both epistemic and alethic possibilities influence our beliefs regarding the world. And this is how we learn, since to learn means to change one's beliefs' structure. Let us give here again the floor to Hempel:

[…] questions of the form 'why is not the case that p?' […] might well be rephrased as 'how-possibly' questions: 'How could it possibly be the case that not-p?' […]. A pragmatically adequate answer again will have to clear up the empirical or logical misapprehensions underlying this belief" [Hempel, 1970, p. 429].

So, and again, we have beliefs at the very centre of the process and the way we learn about the world. But let me now draw my reader's attention to another philosopher who put beliefs at the centre of his theory of how we apprehend the world. Here we have to refer to Charles Peirce, the author of *The Fixation of Beliefs* (1877), who famously states the following:

Doubt is an uneasy and dissatisfied state from which we struggle to free ourselves and pass into the state of belief; while the latter is a calm and satisfactory state which we do not wish to avoid, or to change to a belief in anything else. […] the irritation of doubt causes a struggle to attain a state of belief. I shall term this struggle *inquiry* [Peirce, 1877, pp. 66-67].

So, the very essence of learning is a transition from the state of doubt to the one of belief. But still, a given belief which is formed after abandoning some doubts may again be put into question. Hence degrees of beliefs [ibid.]. Peirce analyses four methods of fixing beliefs, i.e., tenacity, authority, the a priori

method, and 'the method of science'. Of these four the last one is privileged. This is so since for Peirce scientists observe mind-independent reality: "There are real things, whose characters are entirely independent of our opinion about them; those realities affect our senses according to regular laws" [ibid., p. 74]. Thus the crucial role of empirical evidence in forming beliefs. Peirce is hence optimistic about the possibility of having *true* beliefs in science. However, if we do not have laws of nature in economics, and if we as economists influence the world we investigate, then 'the method of science' is not to offer us high degree beliefs [cf. Hardt, 2017].

Interestingly, the celebrated 1877 work by Peirce was discussed in the context of economics by R. Backhouse in his 1994 paper which appeared in the inaugural issue of *The Journal of Economic Methodology*. In this very paper Backhouse put forward a thesis that in economics disagreements are impossible to overcome. This is so since (1) empirical evidence provides weak constraints on theorizing and economics is centred on theorising; (2) economic theories change the world economists try to understand; (3) "[…] and great problems are imposed by the changing nature of the economic world" [Backhouse, 1994, p. 41]. While I agree with Backhouse's thesis, I do not accept some of his arguments backing it. First, economics is centred on models rather than on theories. Second, markets definitely evolve and thus are constantly changing but it does not imply that natures of elements they consist of are also in a permanent change. If one, as myself, subscribes to the world of powers and capacities, then natures of given entities do not evolve [Cartwright, 1999, p. 49]. For instance, let me claim the following: in the very nature of lower cost of money is to stimulate investments. This nature is not to change while we modify circumstances but in some contexts it is to manifest itself and in some it will be dormant. Why not to interpret in such a *realist* spirit the following sentence from Peirce's 1877 paper? So, it states the following:

Belief does not make us act at once, but puts us into such a condition that we shall behave in some certain way, when the occasion arises [Peirce, 1877, p. 67].

I am to come back to the above issue later in the next section. Now, what about changing focus of inquiry from theories to models? In such a case beliefs cease to be elements of theories but they are rather products of models. There-

fore, Backhouse is not right in claiming that the vagueness of many statements in economic theory is due to its detachment from empirical domains. Here he gives examples taken from Bloor and Bloor [1993], e.g., 'It seems likely that money causes inflation', or 'I wish to suggest that money causes inflation'. In my perspective where models are first and theories only later such elusive statements are nothing wrong. As it was shown in section 2, models' claims about the world are often about tendencies, probabilities, or capacities, and rarely if ever Humean regularities.

Now, referring again to Peirce's idea of the fixation of beliefs one may ask what does this very fixation mean if beliefs are agents' *opinions* about the world that are offered by models. First, let me take the case of *models of something*, namely models understood as isolations or as imperfect pictures of some targets. In such a situation one can only take fixation as something local and not general. In other words, beliefs produced by a given model can be fixed in relation to this very model. So, the more a given situation similar to the one described by a model in question is, the higher degree of beliefs about this very situation we have. As Guala puts it: "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" [2005, p. 220]. I put forward the idea of models as believable words elsewhere and I do not want to go into details here [Hardt, 2017, pp. 33-168].

But what about minimal models where the issue of referentiality is not in place? As I have mentioned earlier these models help us to learn about the world by revising our beliefs. So, now the following question is worth asking: are beliefs in Grüne-Yanoff's sense [e.g., 2013] similar to the ones appearing in Peirce's seminal 1877 work? I would give here a rather positive answer, however, some reservations are definitely worth making. First, Peirce generally claims that while beliefs nearly always are imperfect descriptions of the world they can nevertheless attain (however rarely) the status of true beliefs [cf. Almeder, 2014, p. 105]. In my perspective beliefs can only be true inside models used to produce them. Second, pragmatists, including Peirce himself, generally put emphasis on prediction and control rather than on explanation. In Grüne-Yanoff's take on models they are more treated as entities aiming at explaining than simply on predicting. Third, pragmatists are generally treated

as fallibilists. Therefore, given beliefs may become wrong. But we should remember that these are beliefs that turn out to be wrong in a given context and not necessarily a particular model as such. Once you change a context your belief may turn out to be correct. Despite the above mentioned differences there are striking similarities between Peirce's beliefs and beliefs understood as claims about the world produced by models, including economic ones.

But what is now necessary to complete the analysis of beliefs is to focus more on models' users, namely the ones internalizing beliefs about the world given by models. It is done in the next section.

4. Beliefs and people possessing them

If beliefs are propositional attitudes, as analytic philosophers would generally claim, and if these attitudes are mental models of having some opinions, then it is worth analysing beliefs abstracting from the ways they are produced and taking more into consideration the ones possessing them. So, beliefs are responsible for the productionof behaviour. If I believe that doing *X* is to harm me, then I am to abstain from *X*. However, *X* does not harm in every circumstances. For instance, if *X* stands for lighting a match then I am definitely not to do it if I believe that I am near some flammable materials. So, I will be characterised by a disposition of trying not to do it. But this disposition may be dormant if I am not surrounded by such materials. As Schwitzgebel [2015] puts it "[…] the demand for an absolutely precise specification of the conditions under which a disposition will be manifested, without exception, may be excessive". As Cartwright [1983] has noted, even perfectly respectable claims in the physical sciences often hold only ceteris paribus or 'all else being equal'. So, we are close to the so-called dispositionalism about beliefs, precisely if somebody believes *Y* then this person possesses a particular disposition pertaining to *Y*.

So, in the context of the above it is now worth making a distinction between occurrent and dispositional beliefs . Take, for instance, the following statements: (1) 'The author of this paper runs a ten mile race', and the second one, (2) 'The author of this paper is running a ten mile race'. The former one

is a dispositional statement and the latter a statement about a particular occurrence. Here (1) can be true even if the author is asleep. So, (1) can be true even if (2) is false. But (1) cannot be true unless there are circumstances under which (2) would be true. Schwitzgebel [2015] explains it as follows while taking into account an agent having these two kinds of beliefs: "A subject dispositionally believes *P* if a representation with the content *P* is stored in her memory or 'belief box'. When that representation is retrieved from memory for active deployment in reasoning or planning, the subject concurrently believes *P*. As soon as she moves to the next topic, the occurrent belief ceases". Such a dispositional view of beliefs is shared by many philosophers, including D. Armstrong and D. Hume, among others. So, in the two forthcoming paragraphs I am to comment on their ideas concerning beliefs.

In his 1973 book on *Belief, Truth, and Knowledge* Armstrong analyses three possible and interrelated answers to the very question on the nature of beliefs, precisely they can be treated as conscious occurrences in the believer's mind, they may be understood as dispositions of the believer, and they can just be states of the believer's mind. Let us focus here on the second understanding of beliefs. Here Armstrong clearly distinguishes between thing's disposition and the manifestation of that disposition. For instance, between the brittleness of a piece of glass and its actually breaking. Therefore, our knowledge consists of two parts, i.e., capacities (or dispositions to use Armstrong's word) and their manifestations. We are so very close to Cartwright's approach to human knowledge. She states the following: "Our most wide-ranging scientific knowledge is not knowledge of laws but knowledge of the *natures* of things" [Cartwright, 1999, p. 4], and in her earlier book: "The generic causal claims of science are not reports of regularities but rather aspirations of capacities, capacities to make things happen, case by case" [Cartwright, 1989, pp. 2-3]. If beliefs are produced by models, and if they are understood as dispositions, then a powerful conclusion follows: models inform us about entities' dispositions or, in other words, about nature's of things/states/processes/etc. Thus Armstrong's ideas concerning beliefs can be situated close to the Cartwright's ones.

Now, let me refer to Hume, and especially his observations about the nature of beliefs. In the *Treatise* he says that the goal of scientific inquiry is "a degree of belief, which is sufficient for our purpose" [Hume, 1740/2000,

p. 122]; however, a belief which would be free from any doubts is almost not possible: "Belief, being a lively conception, can never be entire, where it is not founded on something natural and easy" [ibid.]. As Hardt [2017, p. 26] explains "[…] the idea of belief was very important for Hume, since it was used as a tool for overcoming the conflict between knowledge and probability". Please thus take into account the following Hume's opinion:

Knowledge and probability are of such contrary and disagreeing natures, that they cannot well run insensibly into each other, and that because they will not divide, but must be either entirely present, or entirely absent [Hume, 1740/2000, p. 119].

The above is very interesting: beliefs should not be understood in terms of probabilities. This is so mainly because beliefs refer to prototypical proprieties of things and not to statistical *normality*. For instance, I can still claim that in nature of birds is to fly even if due to some natural disaster all birds cease to fly. What can be claimed here also is that the above dispositional account (including Armstrong's ideas) and here presented Hume's understanding of beliefs can be employed to the economic realm. As Cartwright puts it: "[…] our typical methodologies and our typical applications, both in the natural and in the social sciences, belong to a world governed by capacities, and indeed cannot be made sense without it" [Cartwright, 1999, pp. 1-2]. Here we find a clear influence of Aristotelian ideas on her thinking. Take, for instance, the following sentence from *The Nicomachean Ethics*: "Now fine and just actions, which political science investigates, admit of much variety and fluctuation of opinion, so that they may be thought to exist only by convention, and not by nature" [Aristotle, 1995, p. 1730]. However, the above does not mean that we should treat Hume as the one subscribing to such a metaphysically rich treatment of beliefs. On standard reading he is the one subscribing to the deflationary metaphysical claim that there is nothing more to causation than constant conjunction. And thus his beliefs being in-between knowledge and probability do not refer to some specific *ontology of beliefs*[5].

[5] It is however worth noticing that some Hume's interpreters treat him even as a „quasi realist", namely the one analysing operations of nature [e.g., Blackburn, 2000]. But such voices still are rare [cf. Hardt, 2017, pp. 102-107].

Now, let me come back to the topic of beliefs and people possessing them. Here one issue is very important, especially in the context of the previously discussed minimal models, namely of whether one may have beliefs without explicit representation. We should thus focus on the possibility (*sic!*) of implicit beliefs, namely such beliefs which while being possessed by an individual do not possess representations. Dennett [1978] claims in this context that we all have such beliefs and that they are derivable from explicit beliefs, namely the ones representing some existing phenomena. Now, Grüne-Yanoff's statement that "[minimal model] conceptualizes existing beliefs in a new way"[2013, p. 853] can be treated as the one related to the formation of implicit beliefs. However, one cannot also exclude the situation in which the previously implicit belief shifts into the explicit one once a context changes. Here enters Grüne-Yanoff's idea that in order to learn from minimal models they should represent entities at least potentially possible. Therefore, it seems to be useful to link literature on minimal models and beliefs they produce or modify with contributions studying the very nature of explicit and implicit beliefs in the human cognitive apparatus.

5. Conclusions

The existing literature in the philosophy of economics which deals with models focuses on both modelling and models but put less emphasis on the nature of knowledge models produce. It was shown in this paper that this very knowledge is best understood as beliefs about the world. Moreover, these beliefs should be treated as statements about dispositions, capacities, and natures. Therefore, they should not be interpreted in terms of probabilities but rather as claims about prototypical characteristics of entities being under investigation. However, beliefs are true within models producing them. Interestingly, products of very divers types of models can be analysed in here proposed framework. However, models treated as isolations (in Mill's tradition) produce *new* beliefs and models-constructions, and especially minimal models, rather modify existing beliefs. Nevertheless, the difference is rather a difference in degree rather than in kind.

Acknowledgments

The research here presented was financed by a grant from the National Programme for the Development of Humanities (grant no. 0266/NPRH4/H2b/83/2016).

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