Mislav Žitko
Department of Philosophy
Faculty of Humanities and Social Sciences
University of Zagreb

Models, Fictions and Explanations:
A Study in Historical Epistemology of Economics

Abstract *This paper examines the standard criticism of the neoclassical economic theory that takes mathematical formalism and the practice of modelling as the most problematic aspect of orthodox economics. The aim of the paper is to explore the epistemic properties of models in science (particularly in economics), and to incorporate the insights from the recent debates in the philosophy of science into the framework of historical epistemology of economics. The main claim of this paper is that history is important for understanding how economic models operate and why have they been accepted as legitimate instruments of inquiry in economic theory. However, since modelling practice clearly is not limited to neoclassical economic theory, the difference between economic orthodoxy and heterodoxy has to be explained in a different way. The paper argues that the theory of fiction can provide an important clue inasmuch as the epistemological and political commitments are a part of the story that underpins the modelling practice in economics.*

Keywords: *models, explanation, economics, historical epistemology*

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In recent years considerable effort has been put into search for the origin of historical epistemology. Described by some as a 'success story' in the field of history and philosophy of science (Kusch 2010), historical epistemology has captured the theoretical imagination of a number of scholars as it apparently allowed them to concomitantly navigate through the labyrinths of disciplinary history and move across the disciplinary boundaries enshrined in the contemporary division of intellectual labor. Under the roof of historical epistemology Ian Hacking (2006) was able to investigate the erosion of determinism as an all-encompassing phenomenon connected with the rise of probability, while Lorraine Daston and Peter Galison (2010), under the same label, focused on the history of epistemic categories such as objectivity, which in turn structure the production of scientific knowledge. While there is no doubt that the further investigation of the history of historical epistemology can reveal something important about its content, as Hans-Jörg Rheinbeger (2010) was able to show by tracing the earliest contributions in the works of German physiologist Emil Du Bois-Reymond and Polish immunologist Ludwik Fleck, we will proceed with the

analysis of economic discourse using the notions and principles of historical epistemology in their original formulation. Rather than tracing them historically, we will attempt to explore their explanatory power through the study of modelling in economics, i.e., through examination of a particular feature of economic discourse loaded with theoretical and political disputes and ramifications.

Now, historical epistemology has in part emerged as a struggle to understand the questions of epistemology as the questions of social order. However, the point of this endeavour is not to simply write off certain disciplinary projects as ideological deviations, pure and simple. Quite the contrary, as Mary Poovey explains, the point is to move away from the modality of denunciation which uses the gestures of unmasking as a substitute or a shortcut for a more comprehensive and more difficult inquiry into the development of a scientific notion or a discipline, as if there is an implicit trade-off between political import and epistemological depth in the analysis of knowledge production. This is of a particular importance in the case of economic discourse, or, to be more precise, neoclassical economic theory. After a series of conceptual blunders and methodological dead-ends, neoclassical economics became the undisputed queen of social sciences in the second part of the 20th century, so much so that it is difficult for a layman to envisage where neoclassical theory ends and economic field begins. This brings about a series of epistemological issues which can by no means be reduced to the analysis of economic discourse. The progressive and regressive aspects of formalism in scientific inquiry, especially with regard to the deductive-nomological model, the use of mathematical methods in social sciences, or the consequences of idealization for the scientific conduct, all these issues tend to emerge in the dissection of economic discourse, and one can observe various ways in which they overlap with other investigations in the field of history and philosophy of science.

As it is well known, there has been a substantial contention over the formalism and mathematization of economic analysis. Neoclassical economics, driven by what Mirowski (1989) named, in reference to Freud , "*physics envy*", has been at the forefront of this process. The commentators and critics of the neoclassical research program were quick to spot the vital link between neoclassical economics employment of mathematical techniques and its subsequent rise in terms of academic prestige, social authority and political power. However, the lines that divide orthodoxy and heterodoxy appear to be blurred upon closer inspection

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as one can find quite a quite a few critics with little or no sympathy for the heterodox economic theory. For example, Mark Blaug in his by now famous article titled *Ugly Currents in Modern Economics*, writes: "Modern economics is sick. Economics has increasingly become an intellectual game played for its own sake and not its practical consequences for understanding the economic world. Economists have converted the subject into a sort of social mathematics in which analytical rigor is everything and practical relevance is nothing". (Blaug 1997: 3) This, of course, does not mean that the analysis which highlights the methodological controversies and political content from the heterodox point of view are not worthwhile or beneficial for understanding the contemporary economic theory. The point is that one ought to work on the improvement of analytical tools and objects of inquiry, so that the lines blurred by pre-emptive strikes and controversial moves in the course of heated discussions and polemics may appear clear and distinct once again. Historical epistemology, as we will argue, can fortify the heterodox position as it is able to articulate the history of concepts, methods and categories, and thus allow one to incorporate those that will enable better or more progressive understanding of economic world.

Structuralist view and its discontents

The discussion about the place and function of models is by no means restricted to the field of economic theorizing, and the topic has obviously generated some interest in the field of history and philosophy of science. It is quite clear that the question of representation is the first one to arise when one starts to think about models in science, and, indeed, the recent discussions show that it is very difficult to outflank this issue. Now, the representational functions of the model can be considered as part of the discussion about model semantics, and it is understood that in the scientific practice one can use models in terms of representation of data or representation of phenomena (Frigg and Hartmann 2006). In the latter case, the representation of a particular phenomenon includes cases in which stable and general features of the world are transformed into objects of scientific probing. Some well-known examples of that sort are the billiard ball model of gas, the double helix model of DNA or the Mundell-Fleming model of open economy (ibid. 741).

The first question one could ask, having in mind the classical discussions in semiology, what is it about the model that allows it to stand in the place of something else, or, as Frigg and Hartmann put it, what is

the epistemic virtue inscribed in the models so that they can properly represent something else ? (ibid. 741)

To answer this question, those who are not satisfied with the syntactic view of theories (usually associated with the view of science as developed in the tradition of logical positivism) can always find shelter in the semantic view of theories. In the latter view, "theories are not linguistic, but rather abstract, set-theoretic entities – models of their linguistic formulations. A theory is a family of models: systems that satisfy the theoretical laws we commonly associate with scientific theories" (Chakravartty 2001: 326). To make matters a bit more clear, Frigg refers to this position, which assumes that "the relevant notion of model for the empirical sciences is the one we find in mathematical logic", as the structuralist view of models (Frigg 2002: 4). Thus, one can say that a model (M), which is a structure, represents a target system (T) if and only if T is isomorphic to M. The point about isomorphism is important inasmuch as structures by themselves do not relate to anything in the world, and it is only in virtue of the conjectural isomorphism that one can talk about representation in a sensible way.

However, there are further open questions as to whether isomorphism is the right kind of ingredient for understanding of models, and Frigg lists a series of arguments that point to the fact that even the best structuralist account ("*structure plus isomorphism*") cannot be used in the investigation of what models are, and how they operate in the empirical sciences. We find his arguments to be important for our present discussion since there is no room for highlighting the socio-historical features of a model in the static timeless framework offered by the semantic view.

The first point that Frigg makes is that isomorphism has the wrong formal properties that render it incapable to operate as the link that can secure the representational function between a model and a target system, i.e. "isomorphism is symmetric, reflexive and transitive while representation is not." (Frigg 2002: 10). For example, symmetry implies that if M is isomorphic to T, than T must be isomorphic to M. But, this obviously too strong condition in the context of present discussion and one easily can come up with numerous counter-examples: a floor plan may represent the disposition of rooms in a house, a house does not represent a floor plan. Maxwell and Boltzmann have both modeled *gas* molecules as colliding *billiard balls* in a box, and one could argue that billiard balls are

an appropriate representation of gases that in turn inform our knowledge about kinetic theory of gases, although it is senseless to argue that gases somehow represent billiard balls (ibid. 12). Furthermore, Frigg argues that the multiple realisabilty of structures doesn't seem to be consistent with the notion that models ought to represent a particular target-system. For example, when it comes to economic theory the method of Lagrange multipliers used to tackle the problems of optimization under constraint can also be used in the field of physics. In that sense "we face the dilemma that the structure as a model must stand for one particular system (or one particular type of system), but as a bare structure it is isomorphic to all of them and there is nothing in the set-up that picks out one of the systems as the 'privileged' one of which the structure really is a model. The structure fails to indicate to which one of the structural isomorphic targets it should be applied." (ibid. 15). The integrity of the structuralist approach could be saved if a feature of intentionality is introduced, i.e. if we imagine that a user of the model has created it intentionally for specific purpose. In that case, the above mentioned objections would be neutralized to some extent. For example, the problem of multiple realisability would disappear inasmuch as one could argue that the user intended that the structure appears as the representation of a particular system, so that 'unintended representations' can be safely disregarded. (ibid. 18). One can readily agree with Frigg that this *ad hoc* invention does not save the structuralist account. However, the introduction of agency does point in the right direction, toward an approach more attentive to historical and social components of modelling.

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If our task here is to see the role that modelling plays in economic theory, it is of a particular importance to get a proper understanding of the relationship between a model system, i.e. "hypothetical system proffered as an object of study" (Frigg 2010: 254) and a target system. If the objections to the structuralist approach are to be taken into account, then there are no structures that relate to target system, via isomorphism or otherwise. If scientific practice is relevant for understanding how models work, then one can accept that, when they are using models, scientists are in fact introducing a hypothetical system which becomes the primary focus of their attention. Thus, modelling as a practice necessarily includes two key aspects: on the one hand, a relation has to be formed between the structures and the object of study. The structures have to be pertinent, in one way or another, to the hypothetical system, and this does not undermine Friggs' claim that structures don't

just wait to be picked up, i.e. that without modelling assumptions there can be no structures. We can agree that modelling assumptions (simplifications, idealizations, the introduction of artificial constraints etc.) are necessary first ingredient inasmuch as the modelling process involves a series of pragmatic choices. Still, it needs to be emphasized that the interplay between structures and hypothetical object of study is a two-way street, i.e. a particular structure can influence the modelling practice to develop in a certain way. This interplay produces the model systems which Frigg defines as "imagined physical systems, i.e. as hypothetical entities that, as a matter of fact, do not exist spatiotemporally but are nevertheless not purely mathematical or structural in that they would be physical things if they were real." (ibid. 253) That brings us to the other key aspect of modelling practice, namely the relation between model as a hypothetical physical system and the target system. In other words, once they are constructed and developed, model systems should be able tell us something about the world and it is assumed that the way they can do that is through their representation function.

However, thing are more puzzling than they might appear at this point. Reason for that lies in the fact that we need to account for (a) the epistemic status of the model, (b) epistemic status of the target system, and (c) their relationship in the face of the fact that there is something fictitious about the model by definition. The question is whether the fictitious nature of the model is a quality that has to be understood and accepted or a threat to scientific reasoning that has to be dodged or neutralized. Additionally, the status of target system also needs a further clarification. Do sciences proceed to modelling in hope to capture some phenomena that is simply out there? More concretely, do economic models tell us something about economic world, or is the economic world a realm pliable to the language of numbers and economic models?

To untangle this rather difficult set of issues a number of authors have concentrated on working out the connection between science and fiction. In model-based science there is what Godfrey-Smith (2010) calls a *deliberate detour through fiction*, and it appears that the most productive way to account for imaginary things such as frictionless planes, ideal gases or rational utility maximizers is to use existing knowledge about storytelling and literary fiction. In the literature we find Sudgen's "counterfactual worlds"(2009), McCloskey's depiction of economists as "tellers of stories and makers of poems"(1990), Cartwright's advice how "fables and parables help us understand the use of models"(2010), Frigg's

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"fiction view of model-systems"(2010) etc. To make things more plain let us see Frigg's take on the relationship between science and fiction: "The core of the fiction view of model-systems is the claim that model-systems are akin to places and characters in literary fiction. When modelling the solar system as consisting of ten perfectly spherical spinning tops physicists describe (and take themselves to be describing) an imaginary physical system; when considering an ecosystem with only one species biologists describe an imaginary population; and when investigating an economy without money and transaction costs economists describe an imaginary economy. These imaginary scenarios are telling like the places and characters in the works of fiction like Madam Bovary and Sherlock Holmes. These are scenarios we can talk about and make claims, yet they don't exist"(Frigg 2010: 101). We quoted here Frigg at length not because we necessarily agree with him, but in order to see

that it is possible to argue that model-systems have the properties and functions in a way similar to those one can find in literary fiction. We will return to this issue in the final section, but first we will try to grasp the conundrum of modelling through a more traditional set of questions.

The model trilemma: falsehood, explanation and truth

In his analysis of models in economics, Reiss (2012, 2013) proposed the following trilemma:

- – Economic models are false
- – Economic models are explanatory
- – Only true accounts can explain

Although all statements appear to be acceptable on first sight, they cannot all stand together since in that case end result would be internally incoherent. There are a number of ways one can approach this paradox, and it is enough to challenge at least one of the three points in order to cast doubt on the existence of the paradox. Before that, it is important to properly understand what Reiss means when he claims that models are false. He refers, of course, to the fact that they can misrepresent the target in different ways. That is true for all models, not just for those economists use. However, the difference that should be taken into account is the one between Galilean and non-Galilean idealizations. As Reiss explains: "In a Galilean thought experiment, the factor that has been 'assumed away' does not normally appear. The assumption of no air resistance cannot be read off the model. It only surfaces when we ask

'under what conditions would the result (given by Galilean thought experiment) be true?'" (Reiss 2013: 131). Moreover, Galilean idealizations usually play with fine-tuning of quantitative causal factors, and introduce causal factors that have natural zero, such as no air resistance in Galileo's case (ibid.). On the other hand, Reiss points out using the example of Harold Hotelling's principle of minimal differentiation that economic models proceed in a different manner. Hotelling's model basically shows that the optimal location is next to a competitor in the middle of a geographic or product space, which explains why supermarkets or restaurants are regularly located within a walking distance. The general problem with non-Galilean idealizations is that "they make the model result specific to the situation that is being modeled. There is no way to tell from just inspecting the model that it is one subset of assumptions that is driving the result rather than another." (ibid. 131) In other words, there can be no process of de-idealization for non-Galilean models, since making the assumptions more real would not result in the opportunity to check the implications of the model in a more 'real' surrounding, but would amount to completely different setting separated for the initial model conditions.

Therefore, economic models are not just false; they are false in non-Galilean sense. It is still possible to challenge the first point, and Hausman (2013) does just that by indicating that sentences or propositions can be true or false, but to claim that models are false is "a seriously misleading way to speak" (Hausman 2013: 250). Hausman brings attentions to the fact that models contain at the same time many falsehoods and an element of truth, and it is that fact that enables economists to rely on models in their search for explanation of the economic world. Notwithstanding the falsehoods, a good economic model captures the causal relation in a proper way, i.e. causal explanation requires only that "cited causes and mechanisms exist and result in the phenomena to be explained and that our description of the causes and mechanisms are approximately correct" (ibid. 253). Therefore, when Hausman provides us with a reiteration of the paradox,

a) Models (or assertions that employ models) contain falsehoods

b) Explanations in economics rely on falsehoods contained in the model, or contained in assertions about the work that employ models

b) Explanation requires approximate truth

one is able to exclude (b') by showing that models indeed can explain as long as falsehoods are not driving the results (ibid.). From our perspective this reformulation offers much better prospects for understanding the scope and function of models than strategy implemented by Alexandrova and Northcott (2013). They set to demonstrate that (economic) models are not explanatory at all, and that our pro-explanatory intuitions are wrong. Specifically, by using Salmon's (1984) distinction between ontic and epistemic conception of explanation, they claim that the latter reduces "our surprise at an outcome, making it more evident to us why that outcome occurred" (2013: 265). The former consists in proper identification of the cause-and-effect relation, and it has nothing to do with subjective or epistemic aspects. Thus, it is, inasmuch as it is focused on the objective features of phenomena, superior to epistemic view of explanation. Models can have, in the outlook of Alexandrova and Northcott, other useful purposes like heuristic function, but no matter how well they are constructed, one should not use them for the task of providing explanations.

Nevertheless, as Reiss (2013) and Sudgen (2013) indicate, models are designed to give explanations about phenomena, and thinking about explanations in causal terms (as given in Salmon's dichotomy, for example) is but one option. Indeed, Sudgen (2000) nominates for that task the mode of explanation which is based on inductive inference, and exemplified in the following schema:

E1. In the model world, *R* is caused by *F*
E2. *F* operates in the real world
E3. *R* occurs in the real world
Therefore, there is reason to believe:
E4. In the real world *R* is caused by *F*

If it is understood that *R* stand for some kind of regularity that may occur in the world, and *F* stands for set of causal factors, one can try by means of a careful inductive inference to point out that causal factors denoted in the model are responsible for regularity *R*. Of course, one has to be careful and look out for logical fallacy, so the main question is "what factors might influence one's degree of confidence" to jump from E1., E2. and E3. to E4 (Sudgen 2013: 241). In Sudgen's view then the solution of this paradox lies in the re-examination of third premise which he, consequently, rejects. His account of the models as credible worlds will allow us to return to the analysis of ambivalent relationship between models and fiction. The point here is to see the content

of the notion of *true account* in the premise *c* (only true accounts can explain). A credible world is a counterfactual construction that is to a greater or a lesser extent similar to the 'real' world, so that the credibility of the model is dependent upon similarity with the target system: "The greater the similarity between the model world and the real world, the more confidence we can have in inferences from the former to the latter" (ibid. 241). This formulation by Sudgen is, in fact, a response to what he believes was Reiss' inappropriate depiction of his position. In the book Philosophy of economics (2013) Reiss proposes that the notion of credibility is something that "characterizes good economic models in virtue of which they are acceptable by the economics community" (Reiss 2013: 137). The problem that emerges consists in the question whether a model that the group of economists find credible also has explanatory power. It seems clear that once we relate credibility to a particular group socially verified to perform a specific intellectual task, other (in the traditional Mertonian sense) non-rational factors come to define credibility. This is, in our view, the precise reason why Sudgen claims that the fundamental explanatory concept in his account of models is *similarity*, rather than *credibility*, although he later makes clear that judgments about similarity are somewhat subjective, and that ultimately explanatoriness can be considered as "a psychological notion" (Sudgen 2013: 242).

For our purposes here, the most interesting point that Sudgen makes is that despite the subjective character of similarity that grounds the explanatoriness of the model, knowledge produced in the model-based science is not just social construction. One can, furthermore, ask any community of researchers, "with its given history and its evolving pattern of characteristic modes of enquiry, theoretical preferences and similarity judgments, how far it has been successful in discovering unexpected but predictable regularities" and they will, to the extent that they can, show success in terms of their own discipline, and not in terms of norms imposed by "philosophers of science, logicians or decision theorists" (ibid. 242). We tend to agree with both Sudgen and Reiss, and will argue that there is a missing piece of the puzzle. The missing piece involves examination of the third premise through the prism of model – fiction relation.

Models and fiction?

The discussion so far has indicated the problem with economic models in terms of their properties on the one hand, and their epistemic tasks,

on the other. It has been pointed out by Reiss that there is some sort of paradox involved when one attempts to pull them together. However, most of the discussion between Reiss and his critics has been concentrated on the relationship between models and epistemic norms, and much less on equally thorny issue of relationship between models and the world. At this point, the theory of fiction comes in, but instead of Frigg's fiction view of model-systems, we will lean on Morgan's account in which she explains that what one needs to understand about models and model-making practices is their close affiliation to the storytelling practices. It is her claim that "the way models help us to describe and to understand the economic world is by telling stories about the world" (Morgan 2002: 178). Moreover, Morgan's account of models is set within the framework of historical epistemology, which allows her to specify, i.e. historically determine how "modelling as epistemic genre came into economics and what difference it made" (Morgan 2012: 14). The clue is taken from Crombie's analysis of the six styles of scientific thinking, labelled later on by Hacking as styles of scientific reasoning (Hacking 2002). The six styles of reasoning are:

– The simple method of postulation exemplified by the Greek mathematical sciences
– The deployment of experiment both to control postulation and to explore by observation
– Hypothetical construction of analogical models
– Ordering of variety by comparison and taxonomy
– Statistical analysis of regularities of populations, and calculus of probabilities
– The historical derivation of genetic development

So, when one talks about modelling in economics one is talking about a particular style of reasoning that has had a significant role in transforming the economic discourse. From verbal expositions which had a prominent role in the classical political economy to mathematical idioms that are regularly used in contemporary economic analysis, one can trace a series of epistemic mutations that had influenced the way economists see their objects and use the intellectual instruments at their disposal. As Morgan points out "the introduction of this new kind of scientific object – namely models – involved not just the adoption of new languages of expression into economics (such as arithmetic or geometry), but of introducing a new way of reasoning to economics. And having moved

from a verbal to a model-based science, economists no longer depicted their knowledge in terms of a few general, but unseen, laws, but expressed it in a multitude of more specific models. As models replaced laws, so economists came to interpret the behavior and phenomena they saw in the economic world directly in terms of those models" (ibid. 4).

Since there is nothing self-evident about the models as, at the same time, *objects and instruments* of scientific inquiry, one *cannot* simply propose that a model is explanatory and that it represents a true account without attention to a particular story or narrative within which that particular model is built. It would be rather difficult to find out something about Quenesay's *Tableau Economique* or the IS/LM model (Investment Saving–Liquidity Preference model, common pedagogical device in standard economic textbooks) just by examination of their formal properties, i.e. "history matters whenever we are discussing any particular example of a model, for models are contingent not timeless: we need history to understand how any particular model was built, how it was used, and what understanding economists gain from it" (ibid. 19). When a model is used to represent or to capture a problem in the target system, there is a narrative that provides a framework for that style of reasoning, and the explanatoriness of the model cannot be assessed without taking it into account. This point leads us back to the question of scientific community and its role in the production of models and their underlying narratives.

Again, we have to stress that it is a two-way street. On the one hand, as Morgan explains, there are two aspects of modelling practice, i.e. economic modelling gives form to our ideas about the economy and it makes them formally rule bound (ibid. 20). The practitioners in the field are free to use and develop all legitimate methods and instruments at their disposal, but, on the other hand, these choices constitute a form barrier that separates the possible from the impossible in a given model. In that sense, models display a feature that Morgan and Morrison (1999) call autonomy of the model. In their account, there is a connection between autonomy of the models and their ability to function as instruments, and the partial autonomy stems from the fact that models cannot be constructed entirely out of theory or out of data. (Morgan and Morrison 1999: 10). Type of independence that is peculiar for models is the feature that enables economists (and other scientists) to use models as mediators between theory and the world. To make things more clear, we can use, following Morgan and Morrison, the analogy

with correlation in statistics. With perfect correlation there is little new knowledge to be acquired since the two sets of data will share the same variations, while with zero correlation there is even less to learn since the two sets of data have nothing in common. It is only in between the extreme values that something more can be argued about the two data sets, and a meaningful research can begin. Similarly, if models were just an expression of data, or a mirror image of theory, they would be quite useless for the purpose of scientific research. (ibid. 19) This is the productive side of modelling, for it allows one to participate in construction of a theory (or its improvement), and under certain condition it can help us to understand a feature or a problem in the world a little better.

For example, Morrison (1999) shows how a pendulum model was used as a vehicle for improvement of Newtonian mechanics, as it enabled description of harmonic motion that could not be derived on the basis on Newton's laws alone. Closer to the field of economics, Reuten (1999) provides us with the analysis of the Marx's schema of reproduction in the second volume of Capital and the consequences that came out of construction the first two-tier macroeconomic model in history. "Marx had to" as Morgan and Morrison suggest "deliberately set aside key elements of his theory in order to order to fix the model to demonstrate the transition growth path from one stable growth path to another. On the other hand, it seems that Marx became prisoner to certain mathematical conditions implied by his early cases which he then carried through in construction the later versions." (Morgan and Morrison 1999: 16). These two examples point just what modelling as a practice entails. In using the model to, for example, make an inquiry about the world or explore the limits of a theory, models will through a series of choices made by the practitioner regiment the reasoning in a certain way. To reason with a model means that the formal rules and the subject rules will come together to produce a framework for understanding the object of investigation (Morgan 2012: 27). Storytelling, in turn, over-determines the construction of a model as it would be, as we have argued, rather difficult to use a model without a general purpose implicitly or explicitly held, i.e. without theoretical conundrums or questions that can only arise out of particular narrative about the world.

Storytelling, or political commitments behind the scene

Storytelling, to use Morgan's term, is conditioned with historical development of the disciplinary field. Moreover, it is conditioned by political

and epistemological commitments, which one becomes aware as soon as the question about the credibility of a counterfactual model world comes up. In our view, stories or narratives that are required to make sense of the model in practice cover both model-systems and target systems. As far as economic models are concerned, there is no raw economic world out there waiting to be modelled or investigated in other ways. The very notion of economic field as an object for economic theory is a product of the historical development that made possible to conceive the economy as an autonomous and separate field. The movement, by which the economy has been dislocated from household (οἶκος /oikos), and the autonomous field of production and exchange is formed, is the subject of countless discussions (see, for example, Foucault 2009). In this context it is enough to stress that target system should not be thought of as the invariable background against which economists can pitch their claims, using the models as research instruments. To make these arguments more transparent let us look at the base-multiplier model in economics. In neoclassical theory this model has had a prominent role as it corresponded well with the quantity theory of money, one of the pillars of the orthodox economic reasoning.

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The base-multiplier model was formed in order to explain the process of money supply determination within neoclassical theory. Here, we will first give a formal account of the model using the version provided by Bain and Howells (2003). After formal presentation we will return to epistemological and political issues that have surfaced in the course of this discussion. The model starts by defining two stocks:

$$M \equiv Cp + Dp \qquad \text{...1.1}$$

$$B \equiv Cp + Cb + Db \qquad \text{...1.2}$$

In these expressions M stands for broad money and it consists of notes and coin in circulation (Cp) plus non-bank public holdings of bank deposits (Dp). Monetary base B consists of notes and coin plus notes and coin held by banks Cb plus banks' deposits at the central bank Db. It follows that $Cb + Db$ are in fact banks' reserves, and if we denote them by R we can write:

$$B \equiv Cp + R \qquad \text{...1.3}$$

At any point in time one can devise a ratio of broad money (that in practice usually corresponds to M1 monetary aggregate) to base (that in practice corresponds to M0 monetary aggregate):

$$\frac{M}{B} \equiv \frac{Cp+Dp}{Cp+R} \qquad\qquad \text{...1.4}$$

If we divide the ratio by the non-bank public's holdings of deposit we can write:

$$\frac{M}{B} \equiv \frac{\frac{Cp}{Dp}+\frac{Dp}{Dp}}{\frac{Cp}{Dp}+\frac{R}{Dp}} \qquad\qquad \text{...1.5}$$

The expression can be simplified if we take that $Cp/Dp = \alpha$ and $R/Dp = \beta$

$$\frac{M}{B} \equiv \frac{\alpha+1}{\alpha+\beta} \qquad\qquad \text{...1.6}$$

98   The model points to the relation between base and broad money, and that relation is dependent upon α and β, the public's cash ratio and banks' reserve ratio (fixed by legal requirements), respectively. Under the assumption that those ratios are stable expression 1.6 can be rearranged:

$$M = B\ \frac{\alpha+1}{\alpha+\beta} \qquad\qquad \text{...1.7}$$

In a contemporary banking system known in the literature as fractional reserve system the expression $(\alpha + 1) / (\alpha + \beta)$ is a money multiplier. The point is that the base as defined in 1.2 consists of liabilities of the central bank, so that the central bank can manipulate those liabilities if it chooses to do so. Thus, the model shows that the money stock is determined by base money, which means that the central bank can via multiplier determine the money supply in the economy (1.8). In, for example, the case of U.S. "The Fed is posited to be able to affect the quantity of banks deposits, and thereby the money stock, by determining the nominal amount of the reserve base or by changing the reserve multiplier" (Moore 1983: 538).

$$\Delta M = \Delta B\ \frac{\alpha+1}{\alpha+\beta} \qquad\qquad \text{...1.8}$$

The base-multiplier model is used to describe a monetary system where the money supply is exogenously determined, so that the way the monetary system will operate depends upon the central bank (the exogenous element) and its deliberate policy adjustment. Now, two points must be

emphasized. First, given the equation 1.8, monetarists as the main proponents of this model in economic history could have just as reasonably argued that the fluctuations of the money stock M is the source of variations in high-powered money B, but instead they preferred the reverse case because they wanted to argue that the monetary authorities can successfully implement their policies and keep the banking sector in check (Lavoie 1984). Second, there is nothing inherently wrong with the identities used in the base-multiplier model nor is the formal model the source of the problem. The problem lies in the neoclassical story that lurks behind the model, i.e. the set of questions and assumptions that inform this hypothetical model world. The dividing line between economic orthodoxy and heterodoxy can be found on the level of storytelling and not on the level of formal model-system. To be sure, the Post-Keynesian economists have developed a different description of the money supply process and a different model using almost the same components and techniques as those found in the base-multiplier model. The important difference lies in the depiction of model and target system relation, i.e. in the narrative that circumscribes their epistemological and political commitments.

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Mislav Žitko

Modeli, fikcije i objašnjenja: studija iz istorijske epistemologije ekonomije

Sažetak

U ovom se radu razmatra standardna kritika neoklasične ekonomske teorije koja pretpostavlja da su matematički formalizam i izgradnja ekonomskih modela najsporniji aspekt ortodoksnog ekonomskog diskursa. Cilj je rada istražiti epistemička obeležja modela u nauci (posebno u ekonomskoj nauci), te uklopiti uvide iz recentnih rasprava u polju filozofije nauke u okvir istorijske epistemologije ekonomije. Središnji argument u ovom radu jeste da povest predstavlja važan momenat u razumevanju ekonomskih modela, načina njihovog funkcionisanja i razloga zbog kojih su prihvaćeni kao legitimni istraživački instrumenti u ekonomskoj teoriji. Ipak, budući da izrada modela nije ograničena samo na neoklasičnu ekonomsku teoriju, razlika između ortodoksne i heterodoksne pozicije mora se objasniti na drugi način. U ovom radu se tvrdi da teorija fikcije pruža važnu smernicu za taj zadatak utoliko što su epistemološki i politički ulozi sastavni deo pripovesti koja mora biti prisutna kako bi praksa izrade modela zadobila primeren okvir.

Ključne reči: modeli, objašnjenje, ekonomija, istorijska epistemologija

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