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ON TWO MODES OF APPRAISAL OF ECONOMIC MODELS[1]

ABSTRACT

Economics became a model-based science. As economic modeling, namely model-building and model-appraisal, involves both informal craft and formal procedures, the aim of the article is twofold. Firstly, to enter into the discussion about the practice of economic modeling by drawing attention to the possibility of two implicit modes of model appraisal in economic science. We will start with a thought-provoking recent proposal by philosophically and methodologically sensitive economist Dani Rodrik, whose insights into the art of model selection has already triggered a number of in-depth philosophical commentaries, and has been the subject of a few successful formal reconstructions by philosophers of economics. Secondly, the aim is to supplement this philosophical view of model selection with an account that aims at underlabouring for the reconstruction of a distinct implicit mode of model criticism, which for various reasons has been pretty much absent in recent philosophical and economic discussions.

Keywords: philosophy of economics, economic model, model appraisal, model selection, model criticism.

JEL codes: B41, A14, B40

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The epistemic genre of creating and reasoning with models requires a craft skill working with highly formal instruments.
(Morgan, 2012, p. 399)

Economists need to err to be right, and if they get their errors right then they will be right and able to model with no error.
(Louçã, 2007, p. 255)

1. INTRODUCTORY REMARKS: ECONOMIC MODELING – BETWEEN SCIENCE AND ART

Since the publication of Mark Blaug's seminal book titled "Methodology of economics, or, how economists explain" (Blaug 1980[1992]), the philosophy of economics profession has put a lot of effort and invention into recovering the practical dimension of economic science (e.g. de Marchi, 1992; Backhouse, 1994; Boumans, Leonelli, 2013). This philosophical focus on the research practice in economic science was inspired by two conspicuous methodological and epistemic trends:

- the shift in the general philosophy of science from the analyses of the cognitive status and methodological features of scientific theories to the studies of theoretical and empirical models. Such a transition followed both focusing on how science is actually done, rather than on how science should be done, as well as questioning the received view of scientific theory (e.g. Hacking, 1983; Suppe, 1989; Cartwright, 1999; Zeidler, 1997);
- the growing plurality of models in economic science (e.g. Mäki, 1997; Sent, 2006; Dow, 2007; Dow, 2012; Gräbner, Strunk, 2018) due to which economics became a model-based science (cf. Morgan, 2012; Maas, 2014; Boland, 2014; Rodrik, 2015). An economic model has thereby gained the status of the basic unit of production, communication and utilization of economic knowledge in the intradisciplinary, interdisciplinary and extradisciplinary exchanges. This gradual transformation of economics into modeling science should not be surprising when we compare the path economics follows to the path already set out by number of natural, cognitive and computer sciences (cf. Gelfert, 2016; Magnani, Casadio, 2016; Magnani, Bertolotti, 2017). As it will be presented in this paper, the growing plurality of models in economics can be heuristically expressed by means of two metaphors – economic science as a library and economic science as a marketplace of ideas.

As a consequence of these two trends, the majority of philosophers of economics declare today the willingness to investigate in detail how economists actually practice their trade. On the one hand, since contemporary economics became

a model-based science, the nature, specificity and features of economic models, as contrasted with economic theories,[2] have become the objects of detailed philosophical investigations (see: Erkenntnis, 2009). On the other hand, since contemporary philosophy of science differentiates between scientific theorizing and scientific modeling (e.g. Hacking, 1983), some philosophers of economics have recently offered their accounts of what the economic modeling actually is (e.g. Mäki, 2013; Spiegler, 2015; Gilboa et al., 2018).

Still, there are some essential difficulties and substantial differences within the philosophy of economics profession[3] about how to philosophically and methodologically investigate the economic research practice (e.g. economic modeling) that to a large extent rests on the tacit (or implicit) type of knowledge, craft skills, knowing-how, connoisseurship or expert judgment. As it is not intended in this article to discuss the nature and features of economic models, the general focus will be placed on the practice of economic modeling. To do so, let us first make three specifications that will motivate, organize and guide our further investigations:

(1) the philosophical account of economic modeling that is acknowledged and used here is a revised version of Peter Spiegler's account, according to which: "an economist E uses model M to establish a certain likeness with the target T for intended purposes P and the success of E in accomplishing P is judged against disciplinary norms N" (Spiegler, 2015, p. 25; cf. Mäki, 2013, p. 91);

(2) the cycle of economic modeling is decomposed into two phases: (a) model-building (constructing, formulating) and (b) model appraising (testing, checking, validating, assessing), which fits into Spiegler's conceptual framework, as well as other traditional accounts from the general philosophy of science, philosophy of economics and economic methodology (e.g. Żytkow, 1999; Mäki, 2008; Boland, 2014). The important point to be made here is that the success of an economist in model-building and model appraising is judged against certain suitable intradisciplinary norms that are sufficiently shared in a certain way by the members of economics profession. It is then argued that in the context of model appraisal two sets of research

[2] For the discussion about the meaning of "theory" and "model" in economics see: (Klein, Romero, 2007; Goldfarb, Ratner, 2008).

[3] However, despite the shift towards the practice of economics, there are still three possible formulas of methodological investigations in the philosophy of economics: (i) normative investigation – that aims at elaborating recommendations (guidelines) regarding good research practices and thus at revising received set of practices, procedures and scientific products in a given discipline (e.g. Lawson, 1997); (ii) descriptive investigation – that aims at systematizing explicit declarations of researchers and thus at reporting the state of affairs concerning the set of practices, procedures and scientific products in a given discipline (e.g. Weintraub, 1989); (iii) reconstructive investigation – that aims at reconstructing implicit (tacit) presuppositions behind research practice in a given discipline, and thus to complement (supplement) a given set of practices, procedures and scientific products with a certain context, within which its specific construction (grammar) emerges (e.g. Mäki, 1994).

norms may come into play, standing behind the two implicit modes of model appraisal, namely norms of models selection and norms of model criticism.

(3) the community of economic modelers "in order to make the model work [is] using (a) a tacit, craft-based, knowledge as much as (b) an articulated, scientific, knowledge" (Morgan 2012, p. 25; see also: Morgan, Magnus 1999) and that claim not only conforms to Spiegler's account on disciplinary norms, but also enables to use some insightful philosophical contributions about two dimensions of scientific practice and dual nature of scientific knowledge by Percy Williams Bridgman (1959), Gilbert Ryle (1949) and Michael Polanyi (1966) or Harry Collins (2010). It is then argued that in the context of model appraisal one can distinguish between formal explicit criteria and informal implicit modes of model selection and model criticism.

By combining (2a), (2b), (3a) and (3b) we obtain a simple and tentative, yet helpful for philosophical investigation of economic science, typology (table 1).

Table 1. Typology for philosophical investigation of economic modeling

	model-building phase	mode appraisal phase
explicit dimension	formal methods	explicit criteria
tacit dimension	informal styles (genres, strategies)	implicit modes (ways of thinking, attitudes)

Source: own elaboration.

As the discussion about the explicit dimension of economic modeling (quadrant I and II) can in no way be restrained to the exclusive domain of the economics profession, the deliberations about the tacit dimension (quadrant III and IV) can by no means be delegated solely to the domain of the philosophy of economics. On the one hand, a number of philosophers of economics (more systematically) and some economists (more occasionally) have contributed to the discussion about styles (genres, strategies) of model-building and modes (ways of thinking, attitudes) of model appraisal in economic science. On the other hand, many economists (more systematically by either innovating new methods or importing existing ones from mathematics and statistics) and some philosophers of economics (more occasionally) have contributed to an increasing plurality of formal methods of model-building and explicit criteria of model-appraisal in economic science. An excellent example of the insights into discussion about both dimensions and phases of the practice of economic modeling is the one offered recently by philosophically and methodologically sensitive economist, Dani Rodrik in his extensively commented book titled *Economics rules. Why economics works, when it fails, and how to tell the difference* (Rodrik, 2015).

The aim of this article is twofold. Firstly, to engage in the discussion about the practice of economic modeling by drawing attention to the possibility of two implicit modes of model appraisal in economic science. This implies departing from occasional, yet thought-provoking, recent proposal by Rodrik, whose insight into the art of model selection has already triggered a number of in-depth philosophical commentaries, as well as has received so far a few coherent formal reconstructions by philosophers of economics. The second aim is to supplement this received philosophical view of model selection with an account that aims at underlabouring for further reconstruction of a distinct, implicit mode of model criticism which, for various reasons, has been pretty much absent in the reflection on economics.

The structure of this paper is as follows. In the introductory remarks we sketch the basic distinction between formal methods and informal styles of model-building, as well as between explicit criteria and implicit modes of model appraisal. In the next section we explore the metaphor of economics-as-a-library-of-models, as recently offered by Rodrik. It is then argued, that, by making use of the heuristic potential of this metaphor, Rodrik attempts to redirect the economics profession's attention from formal methods of model-building to the neglected informal mode of model appraisal, namely, the art of model selection (section 2). As this shift has been also addressed by some contemporary philosophers of economics, we will summarize a formal reconstruction of the art of model selection by Till Grüne-Yanoff and Caterina Marchionni (section 3). Afterwards we will argue that the mode of model selection does not exhausts the very art of model appraisal in economic science. There are good philosophical, theoretical and statistical reasons to distinguish then the second implicit mode of model appraisal, namely the art of model criticism. The discussion of the latter will start with delineating the metaphor of economics-as-a-marketplace-of-models (section 4). This metaphor is a useful starting point to turn to both the philosophical tradition that interprets science as a process of detecting, collecting and correcting errors, as well as the statistical and econometric tradition that explicitly differentiates model selection and model criticism (section 5). Such an account makes it possible to address in concluding remarks the question of peculiarities of the art of model criticism in economic science, analogically to Rodrik's insights into the art of model selection.

2. EXPLORING THE METAPHOR OF ECONOMICS AS A LIBRARY OF MODELS

As it has been already pointed out, Dani Rodrik in his broadly discussed and thought-provoking book made a diagnosis of the state of the art in contemporary economic science. According to him, "models are both economics' strength and its Achilles' heel; they are also what make economics a science" (Rodrik, 2015,

p. 5). Furthermore, as Mary Morgan convincingly put it, since the discipline of economics has always been considered both an art and a science (Morgan, 2012, p. 400), it may then prove useful to apply the latter distinction to the practice of modeling and to recognize that economic modelers "depend upon both tacit and articulated knowledge in making sense of their (…) findings and judging their relevance" (*ibid.*, p. 34). When we look at, for example, judging the relevance of economic models, the success of an economist in model-building and model appraisal is assessed against intradisciplinary methodological and epistemic tacit norms, as designed by insiders to insiders and mutually negotiated by them. These norms are commonly shared to a sufficient degree by the members of the economics profession. However, it is the articulated knowledge about formal methods and explicit criteria that was traditionally of primary interest to economic methodologists and philosophers of economics. Given both Michael Polanyi's famous dictum that for all knowledge that is intersubjectively communicable and controllable there exists some tacit and unarticulated knowledge (Polanyi, 1966) and taking into account growing economists' and economic methodologists' interest in the nature and function of tacit knowledge in economic decision-making (Perraton, Tarrant, 2007), it is puzzling that only a few economists and philosophers of economics focused their attention on the unspecifiable or hard-to-specify art of economic modeling. It is even more striking, when one considers that the tacit knowledge, skills and connoisseurship may play in economics the function of "a flexible methodological glue for doing that science" (Morgan, 2012, p. 399).

Having said that, one has to take up the challenge of addressing an initial question about how to investigate in a philosophical and methodological way (in contrast to sociological, psychological and ethnographical ways) the tacit dimension of research practice (e.g. modeling) in economics. The philosophical and methodological literature on economic modeling provides us with at least three possible ways of addressing the question about how to investigate the economic research practice that to a large extent is based on tacit knowledge:

- according to the first, "this can be done by studying the documents one will find at the sites of practice. These documents can include a variety of printed materials: almanacs, dictionaries, guides, handbooks, instructions, reports, teaching materials, tutorials, yearbooks and anything else one can find on desks and shelves at the research site or in the corners to where they have been thrown away out of frustration or because they became redundant. For a philosophy of science-in-practice, these documents have proven to be very informative sources to gain a deeper understanding of specific research practices, particularly of those where theories do play a minor role (…) and where knowledge is to a large extent intuitive" (Boumans, 2016, p. 30);
- according to the second, this can be done by running a controlled field trial experiment that aims at assessing different ways of doing economics and econometrics. "The basic idea of the experiment is very simple, namely to

take a specified data set and let several researchers carry out the same set of applied econometrics tasks but with different methods, approaches and beliefs" (Magnus, Morgan, 1997a, p. 460). In order to learn about how tacit knowledge "fills the gap between methodological treatises and successful applications" (p. 462), "we asked all participants to join us in an attempt to throw light on this important aspect of econometric research. As a mechanism for keeping track of the process we suggested the 'logbook'. It is commonplace in other scientific fields to keep lab notebooks. These record the procedures used, the various steps taken as the research progresses, false avenues, interim results, and other details the scientist wishes to keep track of. These records, directly or indirectly, can reveal much about the research process. We asked all our participants to keep such notebooks, which we call 'logbooks' and which form an important element of the experiment" (Magnus, Morgan, 1997b, p. 471);

- according to the third way, this can be done by exploring the genesis, structure and function of metaphors that the practitioners of scientific modeling constructed and utilized in their attempts to conceptually grasp a certain, but not yet fully articulated in the formal language, component of research practice.[4] The traditional philosophical view of metaphors in science admitted them a certain heuristic role but only at the early stage of formulation of unclear intuitions in the research process (Zeidler, 2014, p. 40). This situation changed when the interaction view of metaphor (Black, 1962), as well as the cognitive view of metaphor (Lakoff, Johnson, 1980) were elaborated. As a consequence, the role of metaphor in scientific practice started to be conceived as an important cognitive factor on various stages of the process of production of scientific knowledge.[5] An example of an account that makes use of a certain metaphor is the one offered by economist Dani Rodrik (2015). Although he does not explicitly refer to the very term of "metaphor" while discussing the state of affairs in economic science, he does implicitly make use of the metaphor of economics as a library of models (Rodrik, 2015, pp. 5, 46, 84).

[4] As there is no direct access in philosophical/methodological investigations to the tacit dimension of economic research practice the exemplification of the tacit dimension depends on the understanding of the "tacit". This term can be defined as either (1) "unarticulated" or (2) "unaware". Concerning (1), the philosopher/methodologist is able to reconstruct the tacit dimension by focusing on either (1a) informal (verbal) procedures or (1b) suggestive (verbal) hints. Concerning (2), the philosopher/methodologist is able to reconstruct either (2a) accepting (declarations) or respecting by the researcher the basic set of normative beliefs. These beliefs constitute the so-called methodological consciousness. The structure of researchers' methodological consciousness consists of five major components: epistemological (cognitive norms), methodological (directives), meta-scientific (aim of scientific cognition), metaphysical (world-view), and anthropological (vision of Man). The methodological consciousness can be seen as a dimension in which the culture intersects with scientific practice.

[5] For the role of metaphors in economics and interactions between models and metaphors in economic research practice see: (Hardt, 2016).

The Rodrik's attempt to metaphorically grasp economic science as a library of models will now be shortly discussed. Such a discussion is necessary to underline the role the metaphor of library plays in drawing the economics profession's attention to the neglected art of model selection. The genesis of the metaphor of library will be only signaled.[6] Instead, the main focus will be on the structure and, especially, the function of this metaphor. To present the structure of the metaphor, it is helpful to recall below some relevant quotations from Rodrik's book (2015):

- "Rather than a single, specific model, *economics encompasses a collection of models*. *The discipline advances by expanding its library of models and by improving the mapping between these models and the real world*. The diversity of models in economics is the necessary counterpart to the flexibility of the social world. Different social settings require different models. Economists are unlikely ever to uncover universal, general purpose." (Rodrik, 2015, p. 5; emphasis added);
- „*models enable the accumulation of knowledge*, by expanding the set of plausible explanations for, and our understanding of, a variety of social phenomena. In this way, *economic science advances as a library would expand: by adding to its collection* (*ibid*., p. 46; emphasis added);
- "[e]conomics advances by expanding the collection of potentially applicable models, with newer ones capturing aspects of social reality that were overlooked or neglected by earlier ones." (*ibid.*, p. 183);
- "[e]conomics advances also by better method of model selection [and] this is more craft then a science" (*ibid.*, p.183–184);
- „[economic] *methods are as much craft as they are science*. Good judgment and experience are indispensable, and training can get you only so far." (*ibid.*, p. 83; emphasis added);
- "economics as a collection of models, along with *a system of navigation among models*" (*ibid*., p. 208; emphasis added).

The structure of Rodrik's reasoning by means of the library metaphor is as follows: he starts with acknowledging the analogy between a science and a library. Just as a library is a collection of books, economic science is viewed as a collection of models, i.e. the basic units of scientific knowledge production, communication and utilization. The library of economic models is thus growing

[6] Before the structure and function of this metaphor, as constructed and applied by Rodrik to analyze in an original manner the state of affairs in economic science, will be explored, it may prove useful to indicate the genesis of the library metaphor. The metaphor in questions has its long history, especially in philosophical essays and novels. For example, it appears in Jorge Luis Borges' *The Library of Babel*, Umberto Eco's *The Name of The Rose* or Stanisław Lem's *Solaris*.

through a continuous model-building process, that is, via the production activities of economic modelers. It is then the economic model-building practice that contributes to the expansion of the library of models. But what is necessary to the accumulation of knowledge in economic science is the method of model selection. The latter are necessary because economists in their daily practice, as Rodrik argue, deal with a variety of models at the same time. So the informal procedure of model selection comes into play in order to decide which model to apply. The informal nature of the way economists often proceed speaks for the view according to which model selection is more a craft then science. Moreover, Rodrik is fully aware of that the accumulation of knowledge in economics, if really occurred, concerns not only articulated and explicit knowledge, but also tacit and craft-based. The latter is of major importance especially in making models useful.

The idea Rodrik advocates for draws support from both the history of economic thought and methodology of statistics. In the first case, it was John M. Keynes who pointed out that "economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world" (Keynes, 1938). In the latter case, "[t]he current statistical methodology is mostly model-based, without any specific rules for model selection or validating a specified model" (Rao, 2004, p. 2). It is quite clear that by making use of the heuristic potential of the metaphor of economics as a library of models, Rodrik attempts to redirect the economics profession's attention from formal methods of model-building to the neglected informal mode of model appraisal, namely, the art of model selection and craft of navigating among multiple models.

3. A RECONSTRUCTION OF THE ART OF MODEL SELECTION IN ECONOMICS

Rodrik's initial diagnosis and description of the very art of model selection in the practice of economic modeling attracted attention of philosophy of economics' profession. His remark that economists in their crafting the navigation among models act often "informally and suggestively" (Rodrik, 2015, p. 184) has motivated Till Grüne-Yanoff and Caterina Marchionni (2018) to take an attempt to reconstruct the art of model selection more "formally and conclusively" (Rodrik, 2015, p. 184), or, to put it in authors terms, to reconstruct such an art as a "mechanical procedure" (Grüne-Yanoff, Marchionni, 2018, p. 4) containing built-in, at least, potential "decidability" criterion (pp. 1, 2, 5). In what follows we will shortly discuss this methodological contribution. However, due to both the detailed character of this contribution and size limitations of this article, it is hardly possible to deal with all of the interesting and thought-provoking analyses offered there. As the aim of this paper is to investigate the modes of model

appraisal in economics, we will focus only on first step in Rodrik's remarks on the art of model selection, as made by modeling practitioner':

- the informal procedure of selecting the set of candidate models from those epistemically available (Rodrik, 2015, p. 93);
- the informal procedure of selecting the set of critical candidate models from the set of candidate models (*ibid.*, p. 94–98);
- the informal empirical tests via four verification strategies (*ibid.*, pp. 93–94).

As we focus on the first stage, we will take into considerations only the problem of so-called inclusion criteria for the choice of the set of candidate models, as Grüne-Yanoff, Marchionni (2018, p. 6) call it.

Let us now proceed to a short discussion of Grüne-Yanoff's and Marchionni's account. In their conceptual framework they integrate three items: concept of model pluralism[7], ModRep[8] and set-theoretic toolbox[9]; this framework enables them the reconstruction of informal procedure of model selection, informal because of both its form – Rodrik's verbal and underspecified description and its genesis – Rodrik's unsystematic and unmethodical reflection on modeling experience (Rodrik, 2018, p. 3). From the ModRep Grüne-Yanoff and Marchionni pay attention to two components: model's target (T) and model's purpose (P). This way they can specify the very concept of model pluralism as the following:

> "The core idea is in the form of model pluralism, according to which multiple models of the same target T are acceptable as long as one model of T

[7] By making use of the concept of model pluralism they directly refer to the Rodrik's metaphor of economics as a library of models. However, indirectly they refer to the distinction made by Uskali Mäki (1997, pp. 37–39) between "plurality of *Xs* and pluralism about *X*". For example, a statement that there is a plurality of economic models is a descriptive one, whereas any statement about economic model pluralism is of normative character. The normative character of the statement about pluralism takes the form of either justification or prescription. So it is in the case of the idea of model pluralism – it either justifies an existing plurality of economic models or in case of its absence (or insufficient degree) prescribes plurality of economic models by appealing to some ontological or epistemological reasons, e.g. progress in economics by adding to collection of models and improving methods of model selection.

[8] ModRep is here the shortcut for the philosophical accounts on (economic) modelling. Actually, it is worth to mentioned two proposal: first was elaborated by Ronald Giere, according to which "[s]cientists use models to represent aspects of the world for specific purposes" (Giere, 2004, p. 742). Later on, this account was extended by Uskali Mäki, according to which "[ModRep] Agent A uses (imagined) object M as a **representative** of (actual or possible) target R for **purpose** P, addressing **audience** E, at least potentially prompting genuine **issues of resemblance** between M and R to arise, describing M and drawing inferences about M and R in terms of one or more **model descriptions** D, and applies **commentary** C to identify and coordinate the other components" (Mäki, 2013, p. 91).

[9] What is, however, interesting in their general approach is that they, on the one hand, conduct their formal reconstruction in set-theoretic terms and, on the other hand, admit they have no clear attitude towards the semantic view of model. There seems to be an analogy between their "mechanical procedure" with built-in "decidability" criterion and the semantic view, according to which the intended scope of application is built-in in a model. Whether this analogy, if right at all, exposes some consistency difficulties, is left open.

is more useful for one purpose P, and another model of T is more useful for another purpose P'" (Grüne-Yanoff, Marchionni, 2018, p. 1).

Such a formulation enables them to draw attention to the neglected function of confronting a model with empirical evidence, namely "selection of the appropriate model for a specific target T for a particular purpose P" (Grüne-Yanoff, Marchionni, 2018, p. 1; cf. Rodrik, 2015, p. 90).

As we have already mentioned, they re-described the informal procedure of selecting the set of candidate models from those epistemically available in terms of the inclusion criteria. Such an decision is supported by references to Rodrik's contextual usage of terms such as "intuitive", "intuitively", "intuition", "reasonable", "reasonably", "simplicity", "simplified", "simplistic", "plausible", "story lines", "narratives". Rodrik's, formally underspecified, usage of these terms is identified by Grüne-Yanoff and Marchionni as the first gap in his description. To fill this gap they enumerate five basic terms as an inclusion criteria (Grüne-Yanoff, Marchionni, 2018, p. 4) and next they explicate, what is quite puzzling, only four (p. 6). We will come back to this puzzling omission later on. Below the four specified and separate criteria for selecting the set of candidate economic models will be presented (p. 6):

- "intuitiveness: T is a member of a theoretical basis $\boldsymbol{T}$, which is intuitive;
- reasonableness: D is a member of the set of reasonable derivation rules $\boldsymbol{D}$;
- plausibility: $A_1, \ldots, A_k, \ldots, A_n$ are members of the set of plausible assumption $\boldsymbol{A}$;
- narrative relevance: $A_1, \ldots, A_k, \ldots, A_n \notin \boldsymbol{R}$ ($\boldsymbol{R}$ is a representation of model user's purpose), i.e. inferences are not trivial."

The criterion omitted in their reconstruction, yet repeatedly appearing in Rodrik's informal description, is the criterion of simplicity. Below we will make an attempt to correct this omission by both recalling the standard formulation of the logical simplicity criterion as referred to scientific theory, as well as by offering a tentative non-standard formulation of such an criterion that utilizes Grüne-Yanoff's and Marchionni's solutions.

The standard formulation of the criterion of simplicity in science was offered, for example, by the methodologist of empirical sciences Jan Such (1982). Such distinguished two types of simplicity – mathematical and logical. Between these two types there is a negative correlation. The logical simplicity of a theory can be presented in the following form (Such, 1982, p. 119):

$$\text{logical simplicity} = \frac{\text{informational content}}{\text{number of initial assumptions}} \tag{1}$$

where an informational content of a theory is "determined by the set of its implied theorems (being logical consequences of its postulates)" and initial assumptions "are mutually independent".

Let us briefly comment on this formula: the more logical consequences a theory contains, the logically simpler it is; the less initial assumption a theory has, the logically simpler it is.

Now, the question arises whether we can extend such an account on logical simplicity to the case of the inclusion criteria for candidate model selection. A careful exploration of the four criteria specified by Grüne-Yanoff and Marchionni brings a tentative solution, according to which the omitted criterion of simplicity may be expressed as follows (own elaboration):

$$\text{simplicity} = \frac{\text{narrative relevance}}{\text{plausibility}}. \tag{2}$$

Such a formulation may explain the omission of the criterion of simplicity in the discussed reconstruction due to derivative character of this criterion in Grüne-Yanoff and Marchionni proposal. However, this problem surely deserves a deepened and separate examination.

Let us close this section with recalling Rodrik's remark that "[e]conomics advances also by better methods of model selection" (Rodrik, 2015, p. 183). Exposing that economic model selection is "more a craft then a science" (*ibid.*, p. 184) means that economists' navigation among models proceed sometimes "informally and suggestively". As we have pointed out, Grüne-Yanoff and Marchionni in their formal reconstruction focused on the first element of this expression, namely on "informally" (Rodrik, 2015, pp. 89, 90). For further investigation it is helpful to systematize the interpretation of the term "informally" and understand it as:

- either "verbally":
  - "economists employ a wide a range of strategies (…) from the informal and *anecdotal* to the sophisticated and quantitative" (*ibid*., p. 109, emphasis added);
  - "practitioners' views about the real world develop much more heuristically, as a by-product of informal *conversations* and socialization among themselves" (*ibid*., p. 171, emphasis added).
- or "enthymematically":
  - "the idea that economists have to carry multiple models *in their heads* simultaneously" (*ibid*., p. 84, emphasis added);
  - "the core of model-based science is the representational capacity of the model, which is deployed to serve the modeler's purpose with the norms of the discipline *in mind*." (Spiegler, 2015, p. 25, emphasis added).

As Grüne-Yanoff and Marchionni left aside the second element of the expression, namely that economists in their research proceed often "suggestively" and did not make any systematic reference to it, we will take an attempt to clarify this element. Our view is that "suggestively" may relate to at least two, formally underspecified and philosophically underdeveloped, peripheral issues in economic modeling:

- presupposing theoretical background for a given economic model that enables and secures the tractability of model and contributes to its soundness. Such a presupposed economic-theoretical background has something in common with intuitiveness criterion, as specified by Grüne-Yanoff and Marchionni;
- implicating scope of application of a given economic model that secures against misapplication of such a model and indicates its intended application. Such an implicated applicability has something in common with establishing a model's purpose that aims at delimiting the intended scope of model's application. We will come back to this issue later on.

Foregoing considerations were intended only to clarify the idea that model appraisal in economics involves not only formal methods and informative tests but also informal modes and suggestive hints.

4. EXPLORING THE METAPHOR OF ECONOMICS AS A MARKETPLACE OF MODELS

The expression of economics-as-a-library-of-models, as coined by Rodrik to draw economics profession's attention on the art of model selection and craft of navigating among economic models that has long been underspecified at the very heart of economic science, is a metaphor, not the metaphor. Therefore, the art of model selection, as informally expressed by Rodrik and formally reconstructed by Grüne-Yanoff and Marchioni, does not exhausts the very art of model appraisal in economic science. There are good philosophical, statistical and economic-theoretical reasons to distinguish the second implicit mode of model appraisal, namely the art of model criticism. The discussion of the latter will depart from constructing and exploring the metaphor of economics-as-a-marketplace-of-models.

Let us, however, start our exploration with discussing a more general metaphor, namely that of science as a marketplace of ideas. As the philosopher of economics Jesus Zamora Bonilla correctly pointed out, "the thesis that 'science is like a market' has often been taken as an assumption about the working of some 'epistemic invisible hand' mechanism behind the process of scientific research. This vision of science as a 'marketplace for ideas' was not originally a technical notion in the analysis of scientific research, but rather a common metaphor 'floating in the air'" (Zamora Bonilla, 2012, p. 15). This primary metaphor, once carefully extended, may prove useful to heuristically grasp economics science as a marketplace of models. Analogically to section 2, the genesis of the metaphor of marketplace of ideas will be only signaled. Instead, the main focus will be on the structure and, especially, function of this metaphor as related to economic science. Let us start the discussion with the genesis of the metaphor in question.

The genesis can be traced to the works of philosopher, logician and political economist John Stuart Mill, especially, to his classic paper titled *On liberty* (1859). According to Mill, an open society simply benefits from freedom of expression

and speech contributing to the creation, presence and pertinence of a wide range of alternative ideas, let it be scientific ones. It was philosopher of science Karl Popper who explicitly applied this way of thinking to science when he stated that "the advance of science depends upon the free competition of thought, and thus upon freedom, and that it must come to an end if freedom is destroyed" (Popper, 1959[2005], p. 279).

Such a view was further developed by another philosopher of science, Paul Feyerabend who claimed to be following Mill's tradition, at least, in elaborating his pluralistic view of science. According to Feyerabend, there are two main rules that guides scientific research: the principle of tenacity that claims that scientists stick to their theoretical strategies as long as possible, despite initial problems they encounter, believing in potential of their theories; and the principle of proliferation that claims that scientists produce and supply theoretical alternatives, the more the better for scientific progress, and the number of which should be maximized (Feyerabend, 1970; Bschir, 2015). In a situation when the proliferation effect dominates over the tenacity effect, proliferation of scientific products contributes to the marketplace of ideas. What is, however, missing in Feyerabend's account of science as marketplace of ideas is the lack of some possible principle for "rejection or elimination of ideas" (Godfrey-Smith, 2003, p. 116). It was William W. Bartley who developed the metaphor of marketplace of ideas one step further. He did not argu that the primary function of the market is to reject or eliminate some ideas. Instead, he focused on the function of recognizing errors. As he convincingly pointed out, "the university is a marketplace of ideas where new ideas are welcome, and falsehoods can be challenged without recrimination (…). Markets are particularly useful in directing attention to error. The detection of error is the dismal function of the marketplace" (Bartley, 1990, p. xvi). A metaphor that underlines the detection, and not rejection, of error in science accommodates some recent accounts of research practice in economics. On the one hand, according to Rodrik, "[m]odels rarely get rejected in economics" (Rodrik, 2018, p. 2). On the other hand, as Lawrence Boland stressed, "[t]esting by attempting to falsify someone's theory or explanation is just one of many types of criticism. And it is criticism or more specifically, a critical attitude that is the hallmark of science" (Boland, 2003, p. 235).

As we have already said, Rodrik opted for the way of thinking in economics profession about model appraisal that he labeled as informal art of "model selection". Doing this, however, he explicitly dismissed another, *prima facie* traditional and declaratively supported by most economists, way of thinking about model appraisal, according to which economics profession's "party line holds that economics *advances by improving existing models and testing hypotheses*. Models are continually *refined* until the true universal model comes into view. Hypotheses that fail the test are discarded; those that pass are retained" (Rodrik, 2015, p. 84; emphasis added). What Rodrik meant by "the party line" in economic science is the falsificationist ideal widely attributed do the works of the philosopher of science Karl Popper (Rodrik, 2018, p. 1–2).

According to Rodrik, this program of methodology, when applied to economic research, does not accurately describe the way economic modelers practice their trade i.e. it does not accurately describe the way economic knowledge is accumulated (e.g. learning process), as well as the way scientific progress in economics occurs. Behind Rodrik's expression there is the old question of whether Popper's falsificationism is a descriptive or a normative program, question recently raised by various philosophers of economics. Following a broad discussion, there is a consensus among philosophers that falsificationism in Popper's version is not a descriptive stance, so neither the history of science, sociology of science nor economics' insiders' reports can invalidate it. Still, if it is a normative position, there remains the problem of whether Popper's falsificationism passes the test of self-reflexivity (whether falsificationism is falsifiable?) (see: e.g. Nowak, 1992). As the detailed discussion about rights and wrongs (or pros and cons) of falsificationist program of economic methodology is not the purpose of this paper, it is sufficient to close this section with a remark that Popper's approach has still some untapped potential for the philosophy of economics. As Wade Hands put it, a "context dependent criticism is a prime desideratum" (Hands, 2004, p. 152).

5. UNDERLABOURING FOR THE RECONSTRUCTION OF THE ART OF MODEL CRITICISM IN ECONOMICS

The metaphor of economics-as-a-marketplace-of-models, as discussed in the previous section, constitutes a heuristically useful basis to draw inspiration in our preliminary exploration of the art of model criticism in economic science from three theoretical traditions:

- the philosophical tradition that interprets science as a process of detecting, collecting and correcting errors by way of systematic criticism (e.g. Popper, 1959[2005]; 1972[1994]; Bartley, 1982; Mayo, 1996; Boumans, Hon, Petersen, 2014);
- the statistical tradition that explicitly differentiates two phases of statistical modeling, namely model selection and model criticism, as well as elaborates respectively to these two phases different statistical tests and formal procedures (e.g. Box, 1976, 1980; Mayo, 1996; O'Hagan, 2001[10]; Rao, 2004; Staley, 2012);
- the economic-methodological tradition that investigates the way economists make the potential errors during economic modeling more transparent (e.g. Morgenstern, 1963; Mayo, Spanos, 2004, 2010; Reiss, 2008; Louçã, 2007).

[10] "Model criticism may be defined briefly as the process of checking the model assumption (…) without reference to alternative models or assumptions. It is intended as an open-minded phase of investigation to identify any problems with the model. Formulation of explicit alternatives comes after the model criticism phase has identified some problems." (O'Hagan, 2001, p. 1)

Let us start our analysis of the art of model criticism in economic science by discussing the approach within the philosophy of science that interprets science as a process of detecting, collecting and eliminating errors, mistakes and flaws by means of the systematic criticism (or to put it differently, by exercising critical attitude and making use of critical tests). Popper's idea that science is entirely based on the method of "trial-and-error" (or "conjecture and refutation"), occupies a special place within this tradition:

> "The way in which knowledge progresses, and especially our scientific knowledge, is by unjustified (and unjustifiable) anticipations, by guesses, by tentative solutions to our problems, by *conjectures*. These conjectures are controlled by criticism; that is, by attempted *refutations*, which include severely critical tests. They may survive these tests; but they can never be positively justified: they can neither be established as certainly true nor even as 'probable' (in the sense of the probability calculus). Criticism of our conjectures is of decisive importance: by bringing out our mistakes it makes us understand the difficulties of the problem which we are trying to solve. This is how we become better acquainted with our problem, and able to propose more mature solutions: the very refutation of a theory – that is, of any serious tentative solution to our problem (…) is how we can learn from our mistakes." (Popper, 1963, p. vii).

Such a view has already been and, actually, still is widely discussed. As summarizing this discussion goes beyond the aim and scope of this paper, it is sufficient to state that the problem with interpreting, tailoring for special sciences and applying Popper's "trial-and-error" (or "conjecture and refutation") method is that it can be either easily understandable or a complex issue. In either case, however, there are still some controversies. In what follows, it will be argued that Popper's "trial-and-error" idea can be easily understandable (but it does not mean that widely shared) when it is referred to the question of growth of scientific knowledge[11].

Popper formulated a schema of the growth of scientific knowledge through error-elimination by way of systematic rational criticism. According to him, this general tetriatic schema is found "more and more useful as a description of the growth of theories", and, what is important, this "tetriatic schema can be elaborated in various way" (Popper, 1972, p. 287). Below, a number of variants of this schema will be gradually presented:

Diagram 1.

$$PS \rightarrow TS \rightarrow EE \rightarrow PS$$

Source: Popper, 1972, p. 243.

[11] The question of growth of scientific knowledge, as stated and answered by Popper, has its equivalent in Rodrik proposal, namely that there occurs an accumulation of knowledge in economic science due to the process of model-building and model selection.

where:
PS – problem-situation the scientist is facing,
TS – tentative solution to the problem-situation,
EE – error-elimination that controls tentative solutions,
→ – diachronic relation.

Still, this sequence is not intended by Popper to be a cycle, therefore: *PS* (on the left) ≠ *PS* (on the right):

Diagram 2.

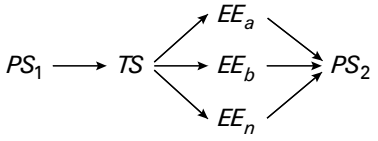
$$PS_1 \rightarrow TS \rightarrow EE \rightarrow PS_2$$

Source: Popper, 1972, p. 243.

where:
PS_1 – old problem-situation,
PS_2 – new problem-situation.

Still, there can be a multiplicity of trials for error-elimination focused on only one tentative solution:

Diagram 3.

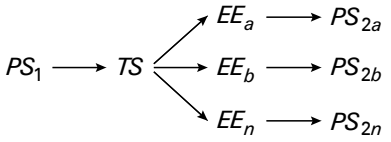


Source: own elaboration.

where:
$<EE_a, EE_b, \ldots, EE_n>$ – a finite and non-empty set of trials for error-eliminations that jointly contribute to the emergence of one new problem-situation.

Still, there can be a multiplicity of trials for error-elimination that independently contribute to emergence of many new problem-situations:

Diagram 4.

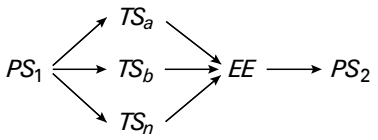


Source: own elaboration.

where:
$<EE_a, EE_b, \ldots, EE_n>$ – a finite and non-empty set of trials for error-eliminations which independently contribute to the emergence of a number of separate new problem-situations;
$<PS2_a, PS2_b, \ldots, PS_{2n}>$ – a finite and non-empty set of new problem-situations.

Still, there can be a multiplicity of tentative solutions offered and considered by scientist, therefore *TS* is not a unit set:

Diagram 5.

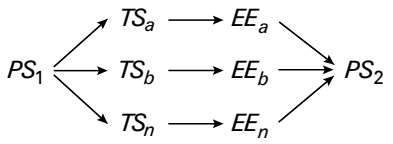


Source: Popper, 1972, p. 243.

where:
$<TS_a, TS_b, \ldots, TS_n>$ – a finite and non-empty set of tentative solutions.

Still, there can be a multiplicity of trials for error-eliminations taken by a given scientist or other scientists:

Diagram 6.

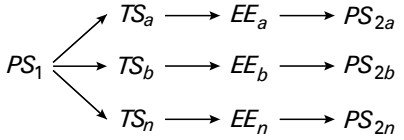


Source: own elaboration.

where:
$<EE_a, EE_b, \ldots, EE_n>$ – a finite and non-empty set of trials for error-eliminations that jointly contribute to the emergence of one new problem-situation.

Still, there can be a multiplicity of new problem-situations that emerge as a consequence of different and separate trials for error-elimination:

Diagram 7.



Source: Popper, 1972, p. 287.

As it has been already pointed out, Popper's "trial-and-error" (or "conjecture and refutation") method can be easily understandable or a complex issue. The simple enumeration of various schemas of growth (in a diachronic sense) of scientific knowledge can be seen as a relatively easily understandable task. But even this Popper's proposal and achievement has been widely discussed and often put into question (cf. Worrall, 1995). As it is not the aim of this paper, we will not attempt to answer the question whether these various schemas accurately describe the growth of knowledge in economics. However, Rodrik's usage of the

library metaphor indicates he seemingly opted for the approach, according to which the accumulation of knowledge in economics occurs by adding to the collections of models. In any case, the problem of growth of knowledge certainly deserves a separate treatment. However, what is more important here for us is to consider the usefulness of the general method of trial-and-error by way of systematic criticism for the reconstruction of the art of model criticism in economic science. Interpreting and applying (in a synchronic sense) Popper's ideas to the question of scientific (here: economic) modeling is surely a complex issue. By labeling such a task a complex one, one is expected to skillfully tailor Popper's general method of trial-and-error to the practice of economic modeling. To meet this expectation the reconstruction of the art of economic modeling in general and model criticism in economics in particular is needed. Such a reconstruction will proceed in one preliminary and three main consecutive steps (respectively: supplementation, specification and refinement).

The preliminary step is to establish the base-scheme for further reconstructive operations. The idea is to choose one schema from the available set (see: diagrams 1–7) previously presented. The schema that fits best to our investigations is diagram 6.

Let us now move to the first main step. As we want to tailor and apply this schema to a special science such as economics, we have to supplement this framework with an account of economic modeling. To supplement means to complete a given component of the scheme with a certain context, within which its specific disciplinary grammar emerges. To be specific, it is the initial problem-situation (*PS*1) that is going to be specified. What is advocated here is that the problem-situation (*PS*1) in economics, which is a model-based science, can be understood as follows: "an economists E uses model M to establish a certain likeness with the target T for intended purpose P and the success of E in accomplishing P is judged against disciplinary norms N" (cf. Spiegler, 2015, p. 25; cf. Mäki, 2013).

As we want to focus on the model appraisal phase of economic modeling, we have to specify (clarify) how actually theoretical economic models are appraised. On the ground of recent developments in the philosophy of economics and economic methodology, the dominating view is that theoretical economic models "are not appraised by ex post empirical testing. Such models are assessed by whether they satisfy their purpose" (Boumans, 2005, p. 15).[12] This is in line with the supplementary Spiegler's account on economic modeling, but to demonstrate how such a view corresponds to Popper's traditional account on method of criticism in scientific practice, according to which tentative theories (solutions) are

[12] "There are several reasons why economists and others appraise theories and models. They may want to judge whether theories and models are worthy of study, whether one can rely on them in research and practice, or whether one can judge them to be true or false or predictively adequate. Different purposes may call for different appraisals." (Eels, Hausman, 2008, p. 248).

tested empirically, is quite challenging. To disentangle this puzzle let us recall the argument made by another philosopher of economics, Francesco Guala (2005).

As Guala, during his investigation into the peculiarities of theoretical modeling and experimenting in economics, pointed out (Guala, 2005, pp. 219–220), tentative theoretical models[13] cannot be tested empirically in the same way as Popperian tentative solutions (theories, hypotheses) can be. In the trial of validation of theoretical models an element that was absent in the traditional discussions about theory testing, namely fitting the modeler's purpose, comes into play. Establishing a purpose by the economic modeler aims at delimiting the intended scope of model's application. It is so, because theoretical models, in contrast to what the semantic view of theories offers, do not have any built-in indicators or instructions where and how to apply it. The latter question is delegated to the intended purpose established by economic modeler. While discussing the art of model criticism it is also important to note that inferring the domain of application from theoretical models by trained economists is to large extent rather a matter of informal economist' craft than formal procedures. Let us come back to the question of appraisal and empirical testing of economic models. It is a given application of the model, as indicated by modeler in the purpose, that can be empirically tested and not the theoretical model *per se*:

> "Scientists are pragmatic people, and although some paradigmatic applications are considered more important than others, a model is always useful to a degree, as long as it is applicable to *some* situation (or, more precisely, as long as it is more helpful in understanding a certain situation than are other rival models). 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." (Guala, 2005, p. 220).

What Boumans and Guala have advocated for does not automatically mean that in our underlabouring for the reconstruction of the art of model criticism in economics we should abandon the Popperian concept of error-detection and error-elimination. What is needed here is the refinement of this concept so that it becomes more tailor-made for the contemporary research practice in economics.

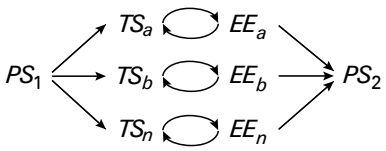
This is how we arrived at the third step, which actually, consists of two stages. During the first one, in order to deal with the current practice of economic modeling, we have to take a synchronic approach. To do so, a new element has to be introduced into the base-scheme (6), namely the feedback between *TS* and *EE*. By adding the feedback relation, we can, at least tentatively, consider the synchronic view. The refinement we postulate here will proceed by utilizing an

[13] For the interesting account on the tentative epistemic strategies as adopted by researchers in applied sciences in the process of constructing models (see: Carrier, 2004).

account on scientific modeling in general and the model verification in particular as elaborated by formal methodologist of empirical sciences, Jan Żytkow. According to Żytkow, a scientific modeling is a multilevel feedback process (Żytkow, 1995, 1999). As discussing his entire philosophical account on scientific modelling in details is not necessary here[14], the main focus will be on the model appraisal which Żytkow calls "model verification". It is important to know that, for Żytkow, "[m]odeling can be effective only if verification accompanies each cycle in model construction, providing feedback long before the final solution is reached" (Żytkow 1995, pp. 177–179). This idea is in line with Boumans' claims that in a number of contemporary practices of theoretical modeling in economics the model appraisal (assessment, validation) is intertwined with model-building (Boumans, 2005, p. 3). By interpreting scientific modeling as multi-layer task, Żytkow is able to conceptually integrate the idea of negative or positive feedback into his model of scientific modeling. As he summarized his own account on the model appraisal: "The most efficient evaluation occurs at the levels prior to the final verification, according to an AI principle: 'evaluate partial solutions as early as possible'." (Żytkow, 1999, p. 323).

As it has been already pointed out, by integrating the feedback relation into the base-scheme (diagram 6) we can, at least tentatively, receive the synchronic view concerning the method of trial-and-error:

Diagram 8.



Source: own elaboration.

An attempt to incorporate the feedback relation to the base-scheme of Popper's method of trial-and-error is not something that is illegitimate as having no support in Popper's writings. To support this claim let us refer to Popper's statements[15]:

> "*Criticism* (…) *is* always an attempt to find (and to eliminate) a mistake, a flaw, or an error within the theory. It is (…) *the negative feedback by which we control the construction of our* theories" (Popper, 1994, p. 162; emphasis added).

[14] "The process of model construction meanders through many feedback loops, as solutions to problems specific at a given step may require changes made to the constructions at the earlier steps" (Żytkow, 1999, p. 322).

[15] By focusing on the base-scheme of Popper's method of trial-and-error it is the tentative solution that is a certain variable which represents a variety of scientific outcomes (theories, hypothesis, models).

This is how we arrive at the second stage of our attempt to refine Popper's concept of error-correction (elimination), so that it better fits the contemporary practice of economic modeling. On the one hand, what was essential to scientific investigations, according to him, is the criticism practiced by scientists. Exercising scientific criticism aims at two error-detection (identification) and error-elimination (correction). But still, Popper taking the normative approach to methodology, did not examined in details how actually scientists practice their criticism or to put it in differently, how they craft "the art of recognizing and avoiding errors", as Paul Feyerabend (1970, p. 18) called it. According to the latter, "scientist who works in a particular historical situation must learn how to recognize error and how to live with it, always keeping in mind that he himself is liable to add fresh error at any stage of the investigation. He needs of a *theory of error* in addition to the 'certain and infallible' rules (…) A theory of errors will therefore contain rules of thumb, useful hints and these suggestions to historical episodes so that one sees in detail how some of them have led some people to success in some situation (…) Good books on the art of recognizing and avoiding errors will have much in common with good books on the art of singing or boxing" (*ibid.*, p. 18–19).

On the other hand, Popper often differentiated between "critical or severe tests" and "critical attitude or susceptibility to criticism", both being features of the scientific approach. However, Popper in his canonical view is not entirely clear about how to understand both the relation between "critical tests" and "critical attitude" and the specific function that "critical attitude" may play in scientific research. This state of affairs gives us some room for an interpretation. Our starting point is that these two concepts, "severe tests" and "critical attitude", were, and still are, subjected to further philosophical processing quite independently. It is because these concepts refer to two distinct dimensions of scientific practice: "severe test" relates to the explicit dimension consisting in formal methods and explicit or articulated criteria of model-building and model appraisal, whereas "critical attitude" relates to the tacit dimension that consists in informal strategies and implicit modes of model-building and model appraisal. Various conceptual difficulties arise from the fact that in daily practice of scientific (here: economic) modeling these two dimensions inevitably overlap, as Mary Morgan convincingly stated (Morgan, 2012, p. 25).

Regarding the concept of "critical or severe tests", besides of numerous methodological specifications being already offered in the broad literature, it was creatively developed by philosopher of science and statistician, Deborah Mayo. She is a supporter of the so-called error-statistical philosophy of science, a tradition that seemingly prioritizes analyses of "critical or severe tests" over "critical attitude". However, as it will be presented later on, it leaves some room to assume that critical attitude may play some function in scientific research. What is more important at that moment is that Mayo treats her "account of testing as implementing and improving upon the Popperian idea

of critical rationalism by cashing out the notion of a severe test" (Mayo, Spanos, 2010, p. 114). Certainly, a severity is the formal requirement of a test of a given theory, model or hypothesis. According to Mayo, in the search for methods of criticism that would be more accurate to contemporary practice of scientific modeling:

> "we need to distinguish what critical rationalism has been since Popper – *Popperian critical rationalism* – and a forward-looking theory of criticism, which we may call *progressive critical rationalism* (…) Progressive critical rationalism would proceed by developing tools for severe tests. Such tools seek reliable probes of errors." (Mayo, Spanos, 2010, p. 117).

The concept of error occurring in science occupies a prominent position in Mayo's project of progressive critical rationalism aiming at refinement of efficient methods of criticism. Mayo supports Popper's initial idea that "*all* our knowledge grows *only* through the correcting of our mistakes", but, nevertheless, she argues that his approach is lacking an explicit theory of error.[16] The latter is expected to conceptualize the genesis, types and functions of errors, or, to put it in a different way, "conditions of error, the various kinds of error and variety of effects errors can have" (Boumans, Hon, Petersen, 2014, p. 19). Mayo's main focus can be thus expressed by five basic questions: why is it useful to study the art of dealing with errors in research practice? What are the major stages of the process of dealing with error? What are the types of errors? What does it mean that scientists can learn from errors? And, finally, how does this type of learning proceed in science?[17]:

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[16] As John Worrall strongly put it: "What exactly constitutes scientifically valuable criticism, for example? Does producing the most valuable criticism involve holding all theories equally open to correction? How exactly is 'error' established in science? What exactly do we learn from our mistakes ('truer' theories or only ones that have higher empirical adequacy)? Can some theories, although always strictly speaking tentative, nonetheless become probable to a reasonably high degree? Are successive 'trials' informed by the successes and failures of previous ones? And, if so, exactly how? (…) The two criticism of Popper's own attempt to fill out the details of the general scheme that I shall discuss here. The first is that he basically mischaracterized the process of 'error-elimination' in science. And the second is that he basically mischaracterized the process by which 'tentative theories' are proposed. Put baldly: Popper's view was that science is entirely based on the method of 'trial-and-error', 'conjecture and refutation', and yet – so these criticisms allege – he seriously misidentified the nature both of the process of identifying error in science and of the process of theory-production or 'conjecture'." (Worrall, 1995, pp. 76–77).

[17] "The view that we learn from error, while commonplace, has been little explored in philosophy of science. When philosophers of science do speak of learning from error-most notably in the work of Popper-they generally mean simply that when a hypothesis is put to the test of experiment and fails, we reject it and attempt to replace it with another. Little is said about what the different types of errors are, what specifically is learned when an error is recognized, how we locate precisely what is at fault, how our ability to detect and correct errors grows, and how this growth is related to the growth of scientific knowledge" (Mayo, 1996, p. xii).

> "The history of mistakes made in a type of inquiry gives rise to a list of mistakes that researchers either work to avoid (before trial planning) or check if committed (after-trial checking). For example, when inferring the cause of an observed correlation, such a repertoire of errors might include a set of questions: Is the correlation spurious? Is it due to an extraneous factor? Are we confusing cause and effect? Corresponding to such a repertoire of errors is a "reservoir of models." I call them models of error." (Mayo, 2004, p. 321).

As we defined science as a process of trial-and-error, not only the traditional stages of error-detection and error-elimination have to be specified, but also a new intermediary stage has to be introduced. According to philosopher of science and epistemologist Douglas Allchin, one of the most significant, yet insufficiently addressed, issues concerning the scientific research, is to catalog or collect actual and potential errors (Allchin, 1999). In practice, researchers usually assemble a discipline-specific informal catalog of past mistakes, flaws or errors. Thereby, they arrange and keep track of an error repertoire (Mayo, 1996, pp. 5, 18). It is therefore a challenging task for philosophers of science to analyze different types of errors in different disciplines and recognize some patterns across disciplines, as well as for methodologists of the special sciences (here: economics) to draw more on a latent discipline-specific reservoir of errors and reconstruct the way scientists actually cope with errors by building and using formal models of error and informal potential error scenarios (cf. Staley, 2014, p. 40).

Distinguishing three stages of dealing with errors in scientific research (detection, collection, elimination) does not, however, give us a complete enough picture of the process of trial-and-error. What is needed is a fourth stage, namely learning from error. It will be argued that the learning from error is to some extent a matter of "critical attitude", that is, rather of an informal and implicit kind. It may prove useful here to recall and briefly address three questions: what are the types of errors?[18] what does it mean that scientists can learn from errors? and how does the learning from error occurs in scientific research?

As types of error are to a large extent discipline-specific, the focus will be on the special science of economics. As it has been already mentioned, there are some economic-methodological works that investigate the way economists make errors occurring in their daily modeling practice more transparent. An example of such an account is given by Francisco Louçã (2007). Louçã in his contribution analyzed the history of the concept of error in economic theory, models and equations, as well as changing economics profession's epistemic strategies to

[18] Mayo introduced four standard or canonical types of error in statistical modeling and experimental sciences: (i) mistaking chance effects or spurious correlations for genuine correlations or regularities; (ii) mistakes about the quantity or value of a parameter; (iii) mistakes about a causal factor; (iv) mistakes about experimental assumptions (Mayo, 1996, p. 18).

handle them. He distinguished seven types of error as characteristic to economic science and research (*ibid*., p. 214), namely:

- measurement errors;
- influence of omitted variables;
- intrinsic randomness in human agency;
- theoretical misspecification of the model;
- functional misspecification;
- general inadequacy of the model;
- irregularities ('aberrations').

As long as there are different types of error, what is required are different, i.e., discipline-specific strategies for detecting, collecting and eliminating them (cf. Mayo, 2014, p. 59). However, in order to successfully avoid errors or, at least, to formulate potential error scenarios a scientist has to be open to learn from errors. This is how we arrive at the questions of what it means that scientists can learn from errors and how the learning from error occurs in scientific research? A possible answer, advocated in this paper yet surely not exclusive, is that, the learning from error, being a purpose of criticism, is "often done relatively informally" (Cox, Mayo, 2010, p. 285) or, to put it differently, it follows "an informal pattern of reasoning" (Mayo, 2000, p. 322). Once we consider criticism not only as targeted at detection, collection and elimination of errors, but also as aimed primarily at informal and implicit learning from error, we can cash out Popper's notion of critical attitude. This argument has support in Popper's writings:

> "[W]hat characterizes the scientific approach is a highly critical attitude towards our theories rather than a formal criterion of refutability: only in the light of such a critical attitude and the corresponding critical methodological approach do 'refutable' theories retain their refutability." (Popper, 1968, p. 94).

Still, there remains the question of so-called learning effect of criticism as it actually takes place in daily practice of scientific research. Leaving this question open, there are good reasons to regard criticism in scientific research as having rather an oscillatory character. As William Bartley insightfully pointed out, "[c]lumsily applied eradication of error may also eradicate fertility. Criticism must be optimum rather than maximum, and must be deftly applied." (Bartley, 1982, p. 133). So, the main purpose of model criticism in economics that is targeted, at least, at making transparent (exposing) the latent errors, is to secure and not to reduce the possibility of learning from modeling failures, blind alleys and obstacles by the widest possible audience on the marketplace of economic models. As long as "[s]cientists are rarely fully explicit about or even aware of why their methods and strategies work when they do" (Mayo, 2014, p. 77), the key is to reconstruct both how economic models are insured against errors and how economic modelers practice their art of recognizing and avoiding errors.

6. CONCLUDING REMARKS: MODEL SELECTION AND MODEL CRITICISM IN ECONOMICS – COMPLEMENTS OR SUBSTITUTES?

The aim of this article was to juxtapose and consider two theses about economic modeling. According to the first one, practices of theoretical economic modeling, namely model-building and model-appraising, involve both formal procedures and informal craft. Concerning formal procedures, philosophy of economics takes into account formal methods and techniques of economic model-building and explicit criteria of economic model-appraisal. When skilled craft is allowed into consideration, and that was of primary interest in this paper, informal styles (genres, strategies) of economic model-building and implicit modes (ways of thinking, attitudes) of economic model appraisal come to the fore in philosophical investigations. According to the second thesis, the practice of economic model appraisal involves, apart from formal and explicit criteria, implicit and informal modes of model appraisal. These are the joyful, yet long forgotten in economics, art of skillful navigation among models (as recently recalled and discussed e.g. in: Rodrik, 2015, 2018) and the dismal, yet potentially useful for economics, art of agile or optimal learning from errors occurring in modeling (as discussed e.g. in: Mayo, Spanos, 2010).

The art of smart navigating among economic models has already triggered a number of in-depth philosophical commentaries, as well as has received so far a few formal reconstructions by philosophers of economics. As a results of the intellectual exchange between economists and philosophers of economics, the formal procedure of model selection in economics was reconstructed and exposed for further discussion (see: section 3). Certainly, answering the question of how economic modelers actually choose among models is an urgent and practical issue. As Uskali Mäki stated, "[b]oth Keynes and Rodrik think that economists have difficulties with the art of model selection, which gives rise to major wrongs in economic modeling" (Mäki, 2018, p. 17). Still, an equally burning question is the question of how economic modelers actually revise (refine or re-specify) existing models. This problem was not discussed by Rodrik in his book to a satisfactory degree though. What is more, it looks like Rodrik's rather negative outlook on the practices of improving existing models and testing hypotheses was rooted in his belief that the methodological "party line" does not adequately describe what is really going on in economics (see: section 4). Nevertheless, once we think (in a synchronic not diachronic way) about the question of how economic modelers actually refine or re-specify their model, then the art of skillful learning from modeling and from errors, as well as the mode of model criticism comes to our mind and may become a useful conceptual scheme for further philosophical investigations into the science and art of economic modeling.

As the underlabouring for the reconstruction of such an art of model criticism in economic science has already been carried out (see: section 5), what is needed next is a more detailed and based on a case study formal reconstruction of the way economists practice the model criticism by, at least, making the potential errors during their modeling more transparent and neutralized. Although, it was not intended in this paper to offer such a reconstruction, it is possible to briefly discuss the main characteristics and stages of such an account:

- "an economist E uses model M to establish a certain likeness with the target T for intended purposes P and the success of E in accomplishing P is judged against disciplinary norms N" (see: Spiegler, 2015);
- an economist E by using model M learns from modeling and the object and effect of learning depends on the disciplinary-specific norms (cf. Grüne-Yanoff, 2009);
- the disciplinary-specific norms are conceptualized as the norms of model criticism or as the norms of model selection that, if only strictly followed, organize the process of navigating among models (see: Rodrik, 2015) or the process of learning from mistakes, flaws and errors occurring in modeling (cf. Mayo, Spanos, 2010);
- giving priority to the norms of model criticism, an economist detects, collects, eliminates errors and learns from them by running the model specification (*S*), mis-specification (*M*-*S*) and re-specification (*R*-*S*) tests and analyses (see: Spanos, 2010; Staley, 2014);
- detection (at least, making transparent), collection (at least, informal), elimination (at least, neutralization) of errors and learning from errors involve both articulated knowledge and craft-based skills.

Following these enumerated characteristics and stages may prove useful to guide the reconstruction of the art of model criticism in economic science. Recent literature on modeling in the field of methodology of statistics and econometrics (cf. Box, 1976, 1980; Mayo, 1996; O'Hagan, 2001; Rao, 2004; Mayo, Spanos, 2004; Staley, 2011) seems to provide motivation to distinguish two phases of economic modeling, namely model selection and model criticism. Moreover, in focusing on the very art of model criticism we can utilize some arguments delivered by recent works of economic methodologists who analyze the way economists deal with errors in the practice of modeling (e.g. Mayo, Spanos, 2004; 2010; Reiss, 2008; Louçã, 2007). There seem to be enough reasons to philosophically investigate the dismal art of model criticism by means of systematic detection, collection, elimination of errors and learning from them, as exercised by economists. However, a separate treatment and case study would be needed to address in details the questions of how economists actually refine and re-specify their

models and how they actually learn from errors. That is why the respective reconstruction should be left for another occasion.

What may be, however, addressed here is the question of whether the art of model selection, as discussed by Rodrik (2015, 2018) and Grüne-Yanoff, Marchioni (2018), and the art of model criticism are substitutes or complements. The answer depends on whether we juxtapose and analyze these two modes in a diachronic or a synchronic way:

- from a diachronic point of view, when we deal with the problem of the growth of knowledge in economics as a model-based science, model selection (in Rodrik's formulation) and model criticism (in a traditional Popperian formulation as expressed in base-schema – diagram 6 from the section 5) seem to be more like substitutes. This interpretation is supported by Rodrik, according to whom "economic science advances by expanding its library of models" and "models enable the accumulation of knowledge." (Rodrik, 2015, p. 46) On the other hand, in the Popperian view, according to which the growth of knowledge is not a cumulative process, science (here: economics) advances through the process or error-elimination;
- from a synchronic point of view, when we deal with the problem of stages (phases) in the cycle of modelling in economic science, model selection (in Rodrik's formulation) and model criticism (in a refined version as expressed in the scheme (8) from the section 5) seem to be more like complements. This interpretation is supported, on the one hand, by Żytkow (1999), who conceptually integrated the idea of positive and negative feedback into his framework of model evaluation. On the other hand, by Mayo and Spanos, according to whom the first stage of model criticism, namely "the problem of (…) model specification is a distinct from model selection, insofar as (…) the presence of misspecifications jeopardizes the ground for model selection" (Mayo, Spanos, 2004, p. 1008).

This article was an attempt to distinguish two informal modes of model appraisal in economic modeling. Disciplinary-specific norms of model appraisal may be conceptualized as norms of model selection or norms of model criticism, against which a tentative theoretical solution that economic modeler arrived at is judged. If an economic modeler (or a community of modelers) is more interested in adding to the library of models, then what is prioritized is the practice and upgrading of formal procedures or craft of model selection. If an economic modeler (or a community of modelers) is more interested in contributing to the marketplace of models, then improving formal severe tests or art of model criticism is prioritized.

To sum up, as Uskali Mäki has inconclusively, yet thought-provokingly, stated recently, "[t]ime will tell whether Dani Rodrik's *Economics Rules* (perhaps in its next editions) will win a place in the series of honoured treatises on the nature

of economics written by practicing economists, next to works such as J.E. Cairnes's *The Character and Logical Method of Political Economy* (1875/88), Lionel Robbins's *The Nature and Significance of Economic Science* (1932/35) and Milton Friedman's *The methodology of positive economics* (1953)" (Mäki, 2018, p. 1). A growing number of interesting commentaries, as well as systematizing formal reconstructions triggered by Rodrik's art of navigation among economic models by means of following the model selection procedure, suggest the answer to Maki's statement could be positive. Still, there is a part of philosophy of economics that in order not to be exposed to the charge of decrying how economic modeling should be carried out, looks forward to a treaty on the dismal, yet constructive, art of model criticism by means of learning from errors, written by some practicing economist.

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O DWÓCH TRYBACH OCENY MODELI EKONOMICZNYCH

STRESZCZENIE

Ekonomia stała się nauką opartą na metodzie modelowej. Modelowanie ekonomiczne, obejmujące zarówno konstruowanie modeli, jak i ich ocenę, wymaga znajomości formalnych procedur oraz nieformalnego rzemiosła. W związku z tym cel niniejszego artykułu jest podwójny. Po pierwsze, wpisanie się w dyskusję dotyczącą praktyki modelowania ekonomicznego przez zasygnalizowanie funkcjonowania dwóch nieformalnych trybów oceny modeli. Będzie to oznaczać

omówienie propozycji Dani'ego Rodrika dotyczącej nieformalnej sztuki selekcji modeli, która wywołała pogłębione komentarze filozoficzne i stała się obiektem formalnych rekonstrukcji dokonanych przez metodologów ekonomii. Po drugie, uzupełnienie tego ujęcia poprzez przygotowanie gruntu pod rekonstrukcję odrębnego trybu oceny modeli wyrażającego się w nieformalnej sztuce krytyki modeli.

Słowa kluczowe: filozofia ekonomii, model ekonomiczny, ocena modeli, selekcja modeli, krytyka modeli.

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