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Model Diversity and the Embarrassment of Riches

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Abstract

In a recent special issue dedicated to Dani Rodrik's (2015) influential monograph *Economics Rules*, Grüne-Yanoff and Marchionni (2018) raise a potentially damning problem for Rodrik's suggestion that progress in economics should be understood and measured laterally, by a continuous expansion of new models. They argue that this could lead to an "embarrassment of riches", i.e. the rapid expansion of our model library to such an extent that we become unable to choose between the available models, and thus needs to be solved to make 'model pluralism' viable. Drawing on Veit's (2019a) 'model pluralism' account, this paper argues that model pluralism as a thesis about the relationship between science and nature undermines the very idea of a general model selection framework for policy making.

Keywords: pluralism; models; model selection; policy making; Rodrik

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"It's *a* model, not *the* model." –
Dani Rodrik (2018, p. 276)

1 Introduction

Recently, a number of high-profile economists have weighed in on the question of how highly idealized models can be used to provide explanations and influence policymaking (Sugden 2000, 2009, 2011; Gilboa et al. 2014; Rodrik 2015, 2018). Among these, Dani Rodrik stands out for his particular emphasis on the importance of *model diversity* within economics. Rather than falsifying models, economists actively seek to expand our 'library of models', selecting the appropriate one for a specific context and purpose. Progress, Rodrik (2015) argues, is thus to be found not in the vertical replacement of old models with better ones in a motion towards the one true, perfect, and general model with universal applicability, but rather a horizontal expansion of our modeling toolkit addressing aspects of social phenomena previously unexplored. Yet, Rodrik himself recognizes the possible objections to his proposal:

> The multiplicity of models is economics' strength. But for a discipline with scientific pretensions, the multiplicity can also be viewed as problematic. What kind of a science has a different model for everything? Can a collection of cases, to use Gilboa and his coauthors' analogy, really amount to a science?
>
> – Dani Rodrik (2015, p. 72–73)

Rodrik thinks that these objections lose their bite once we realize that models are equipped with contextual information and background conditions regarding their appropriate use. Indeed, he goes so far as to argue that they "come with explicit user's guides—-teaching notes on how to apply them" (p. 73). While I think that Rodrik is overly optimistic here and model-world relationships are even in practice often severely underdetermined, there is something fundamentally right about Rodrik's call for model diversity that philosophers of models have failed to sufficiently acknowledge - i.e. the pluralist nature of scientific modeling.

In a recent special issue of the *Journal of Economic Methodology* dedicated to Rodrik's *Economics Rules*, Grüne-Yanoff and Marchionni (2018) argue that the plurality and diversity of models might lead to an *embarrassment of riches*.[1] While models are good, and more models arguably better, Grüne-Yanoff and Marchionni are worried about the conditions under which such "progress would turn into the production of non-processable white noise" (p. 273); i.e. the thought that our

[1] A popular idiom intended to signify that there is too much of a good thing.

modeling library could become so large that choice becomes impossible. Rodrik himself recognizes but ultimately dismisses this problem based on his idea of a user's guide: "in any specific setting, we can discriminate, at least in principle, between models that are helpful and models that aren't" (p. 73). I argue that reply won't work, but it will take more to show why. This is the target matter of **Section 2**.

Yet, despite the inadequacy of Rodrik's response, the present article is intended to debunk Grüne-Yanoff and Marchionni's embarrassment of riches argument (henceforth abbreviated as EoR). While they raise an important challenge to Rodrik's account - i.e., the idea that we would be dealing with such a wealth of models that we could no longer choose among them - this should not be considered a problem for model pluralism as a tenable view of actual scientific practice, nor for model diversity as a measure of scientific progress. There is little to go by in Rodrik if one is looking into his work for a "complete account if one could even call it that" (Veit 2019a, p. 108). After all, Rodrik (2018) does not intend his book to be a "treatise on economic methodology" (p. 276). Drawing on my own 'model pluralism account' (Veit 2019a), I argue that the EoR challenge can be debunked once model pluralism is properly understood as a thesis about the nature of science.[2] This will occupy us for most of **Section 3**. Finally, I offer some guidance for policy making under model pluralism and conclude the discussion in **Section 4**.

2 An Overabundance of Models

As the epigraph is meant to indicate, there is a central message in Rodrik's work that is about the elimination of a particularly harmful, yet widespread belief among economists. It is the still-dominant view that there is something like the perfect model that can solve all problems in economics, or at least within a particular domain of economics. Rodrik suggests the following formulation for this view:

> A model is an abstract, simplified setup that sheds light on the economy's workings, by clarifying the relationship among exogenous determinants, endogenous effects, and intermediating processes. Economic science advances

[2] I initially developed the view largely in response to a perceived need in economics, psychology, and biology (sciences dealing with complexity) to embrace a wider range of models (see Veit 2019b), and defended an early version of this view in Veit (2019c). I have since applied model pluralism as a more general view about science to a wide range of phenomena such as cultural evolution in economics (Schlaile, Veit, & Boudry forthcoming), climate modeling (Ortmann & Veit 2021), autism (Chapman & Veit 2020a,b), cognitive enhancements (Veit et al. 2020), health and disease (Veit 2021, forthcoming), animal welfare (Veit & Browning 2020, forthcoming), consciousness (Browning & Veit 2021; Veit 2021c), and conceptual engineering more generally (Browning & Veit 2020b). It is now time to revisit my view within the economic modeling literature where it has been originally articulated, to address its biggest challenge (EoR), and explicate it as a general thesis about the nature of science.

> by testing these models against reality, keeping those that do a good job and discarding the rest.
>
> – Dani Rodrik (2018, p. 276)

While he has no qualms with the view presented in the first sentence, Rodrik is vehemently opposed to that of the second one. Here, the influence of contemporary philosophy of science is noticeable. The Popperian idea – that we just need to come up with better models that are more general than previous ones and seek to falsify the old ones – is still alive and well in many of the sciences. This, Rodrik thinks, is a fundamental mistake, and here he draws on a number of influential philosophers of science (and economics) such as Nancy Cartwright and Uskali Mäki, in order to 'save' economics from both its critics and its practitioners.

The very idea that economists discard their models is illusionary, so claims Rodrik. The quote above is thus more of a representation of a popular myth among economists *about* economics than an actual truth - a normative view that fails to correspond to actual modeling practice, a problem that as I shall argue, likewise applies to the EoR problem introduced by Grüne-Yanoff and Marchionni. Economics, Rodrik thinks, is in a dire hold between the critics of economics and economists themselves. Yet, he thinks both misunderstand the nature of economic modeling and subsequently tries to set them straight. Both got wrong what economists are actually doing - yes, even the economists themselves. The critics of economics are wrong when they suggest that highly idealized models are the reason for the supposed 'failure of economics'. Economists on the other hand, Rodrik argues, are terrible defenders of their own discipline. They become strange bedfellows with outdated views in the philosophy of science such as those of Karl Popper and Milton Friedman, even though their own pluralist practice contradicts their apparent endorsement of these views.

As Grüne-Yanoff and Marchionni (2018) point out, Rodrik breaks here with an old tradition in economics that saw economic theorizing as the testing, verifying, and falsification of individual models. Instead, an alternative picture is offered according to which *actual* economists choose the right model for the right purpose. Model selection becomes a new and important ingredient. This picture nevertheless offers a moderate role for empirical testing in the evaluation of a model's fit to its purpose and the real world. Testing is here a less formal procedure, something halfway between the practitioners' idea that economics is engaging in simple falsification and the critics' thought that it is merely applied math (Rosenberg 1992), thus "shield[ing] its models from empirical evidence altogether" (Grüne-Yanoff and Marchionni 2018, p. 266).

Rodrik's picture has been praised by a number of philosophers of economics as a more realistic picture of economics (Aydinonat 2018b,a; Mäki 2018; Kuorikoski and Lehtinen 2018; Grüne-Yanoff and Marchionni 2018; Veit 2019a), yet there are

some significant lacunas regarding its details. What has been offered is an alternative - but one that needs to be refined and put to practice. In this, Aydinonat's special issue in the *Journal of Economic Methodology* does an excellent job in doing the essential brick-laying for future work. Grüne-Yanoff and Marchionni (2018) attempt this by formally spelling out the gaps in Rodrik's account in detail. It is an attempt to provide an idealized model of Rodrik's selection procedure which, as I will argue shortly, will ultimately prove unsuccessful.

2.1 The Model Selection Model

Grüne-Yanoff and Marchionni's explicit goal is to provide a formal model in order to investigate the conditions under which Rodrik's account would fail to select a single model and thus lead to what they fear is an EoR. While Rodrik (2015) emphasizes modeling as a craft, rather than merely a logical analysis, their model nonetheless exposes a problem in Rodrik's account. Their attempt to provide a more "tenable pluralist position" by providing a general model selection procedure (p. 268), however, proves unsuccessful. One reason for this is that Rodrik does not intend to develop a complete account of model selection. As his account is thus intentionally incomplete and vague, it does not serve as a good foundation for Grüne-Yanoff and Marchionni's *general selection model.*

Grüne-Yanoff and Marchionni's model consists of an "ordered tuple $<A_1,...,A_n,T,D>$ of assumptions $A_1,A_2,...,A_n$, theoretical base T, and derivational rules D" (p. 268). They argue that some models, such as the Sakoda-Schelling model of racial segregation,[3] are not explicitly based on theory. I cannot make much sense of this claim, but there is certainly a sense in which some models are more theory-light than others – not relying on the dominant theoretical assumptions within the field. This seems to be a better interpretation fitting with the assertion by Grüne-Yanoff and Marchionni that the Sakoda-Schelling model "proceeds autonomously from any particular economic (or sociological) theory" (p. 268). I take it that their 'explicit' (theoretical base) quantifier is intended to solve this issue. T can then consist of any concepts and assumptions, some of which Grüne-Yanoff and Marchionni argue may have garnered strong evidential support. Similarly, for assumptions $A_1,A_2,...,A_n$, they can be supported by evidence. Here, they avoid having to deal with the now quite large literature on evidence in the philosophy of science and simply introduce evidence as "a set of propositions E_K that lend support to a given assumption" (p. 269).

[3] Though usually referred to as the checkerboard model or Schelling model after Schelling (1971), Hegselmann (2017) elegantly showed that James Sakoda (1971) as a victim of the Matthew effect, deserves at least equal credit and recognition for his earlier development of the idea.

D on the other hand, includes logical and mathematical rules of inference: "such as modus ponens and mathematical techniques such as the identification of local maxima" in addition to "fallible inference rules such as statistical inferences drawn from simulation runs" (p. 268). This set of rules is too narrow, however, as it leaves out important facts about context. Much recent work in the philosophy of science suggests that we need to take the context of models more seriously in order to draw inferences between a model and its real-world target (see for instance Teller 2001). This shortcoming will later reveal itself as the fundamental problem with Grüne-Yanoff and Marchionni's demand for a general selection procedure. Rodrik's defense, however, is likewise not satisfying and a closer examination of Grüne-Yanoff and Marchionni (2018) reveals why.

In addition to the initial set-up, Grüne-Yanoff and Marchionni suggest a number of constraints in the model-selection process such that the possibly uncountable number of possible models *M* becomes limited, leading to a set of candidate models M_C. Their primary concern is the idea of a model's purpose. Models are used for different purposes and different purposes will determine which models are relevant and which aren't. Grüne-Yanoff and Marchionni see this relevancy criterion R as quite narrow, however. They assume that a model's purpose will always include at least one input to output combination, such as the market price of a good and the policy of a price cap, thus offering something like a critical assumption. This does not mean that these relationships need already be part of the model, but that they can "be meaningfully introduced" and in which a change of the input variable has an effect on the relevant output variable (p. 268). While some may criticize this constraint as unrealistic, I see it as a useful and approximately true idealization in many cases of economic policy making. In any case, the point of their argument is to show that even if we have such a constraint there will be too many models left, making a choice impossible if we don't introduce some further details and constraints into the model selection process.

2.2 Modeling Selection Failure

Having given the core of their model we can turn to their formal definition of an EoR, which occurs "if the number candidate models, M_C, is not sufficiently constrained" (p. 273). This is where Grüne-Yanoff and Marchionni (2018) demand the necessity of a general selection framework, which "will fail to reduce this multiplicity to a manageable number if the critical assumptions are underdetermined by the empirical evidence" (p. 274). They illustrate three different ways in which such a failure may occur.

1. Robustness Analysis. Perhaps the primary way in which economics would become 'flooded' with an almost uncountable number of models would be to ignore the robustness of models. Here, Grüne-Yanoff and Marchionni worry that

robustness analysis will be unable to "sufficiently filter the number of models" (p. 274). For this, they provide us with the following scenario:

> This might happen with economic models that are not derivationally robust: if in the extreme, all the assumptions in the models belonging to the set are crucial to the conclusions, then no model is discarded. Of course, the lack of robustness also raises doubts about the reliability of the models in the first place (cf. Cartwright, 2007; Grüne-Yanoff, 2011).
>
> – Grüne-Yanoff and Marchionni (2018, p. 274)

As I shall later argue, the problem here rests ultimately in a misunderstanding of the role of robustness analysis in science.

2. Lack of Empirical Evidence. In some policy decisions the evidence-base is scarce. This concerns rapidly occurring phenomena such as epidemiological events, like the Coronavirus outbreak in 2020, but also longstanding problems such as optimal education strategy. In educational design, for instance, we may have only sparse evidence on whether class size leads to better education (Stecher et al. 2001). Grüne-Yanoff and Marchionni (2018) suggest that the problem of lacking evidence - i.e. an insufficient number of propositions E_K supporting the model - may occur when we "are unable to experiment, or because of legal constraints on the data" (p. 274). In such situations, economists frequently invoke the robustness of their models. While they may be empirically underdetermined, they at least appear to be robust against a broad range of empirical assumptions. But this reply is simply an implicit endorsement of model pluralism. Grüne-Yanoff and Marchionni would have to pick their side: there is no important epistemological distinction between robustness analysis narrowly conceived and a much broader sense of robustness analysis. Here, we don't rely on the selection of a particular model, but rather are interested in the connections between a broad range of models giving us purchase on a complex world. It is the diversity of models that matters, rather than a particular model (see Ylikoski and Aydinonat 2014; Veit 2019a).

3. Pragmatic Limitations. The third reason they give for the occurrence of an EoR, are pragmatic considerations. When models are used and intended for policy purposes, there is an added time constraint that is due to urgency or lack of manpower. Policy-makers may not be able to delimit the number of relevant models even when they are not underdetermined by the empirical evidence and meet our robustness standards. Consider the 'noise' created by a multitude of empirical studies with contradictory results. In order to make an informed choice, we may have to consider them all, but this is often not possible given the practical constraints of policy making.

While Grüne-Yanoff and Marchionni (2018) recognize that Rodrik suggests a number of additional constraints himself, such as "narrative relevance, simplicity,

plausibility, reasonableness, and intuitiveness" (p. 268); these are largely left unspecified and thus do invite the charge that policy-makers will be faced with an EoR problem.

3 Model Pluralism and the Embarrassment of Riches

Despite the obvious shortcomings of Rodrik's account, this section is intended to debunk the EoR argument. Grüne-Yanoff and Marchionni (2018) argue that a necessary condition for the viability of *model pluralism* is that it is possible to "find a way of selecting, from amongst a plurality of models, the one that is right for a specific purpose" (p. 266). They also argue that "[i]f such a selection procedure were not available, and an arbitrary large number of models had to be considered adequate for a given target and purpose, then *model pluralism* would not be a viable position" (p. 273) at all. This, I shall argue, gets things backwards.

The mistake here is twofold. First, it suggests that model pluralism is just one possible view of how success, progress, and the structure of science could be characterized. But if the proponents of 'strong' model pluralism (Veit 2019a) are correct, there is no alternative to model pluralism. Due to inherent trade-offs between the many aims of scientific models (Levins 1966; Weisberg 2003, 2006, 2013; Matthewson and Weisberg 2009) and their context-sensitive nature (Veit 2020), multiple models are a necessary feature for scientific progress. They are not just a sign of an immature phase of science - science itself operates by continuously expanding its 'toolkit' of available models, whether in ecology, climate science, or economics. Due to the complexity of the world and our cognitive limitations, model pluralism properly understood is both a factual claim about how science is practiced, and a normative one about how science ought to proceed. The second problem in Grüne-Yanoff and Marchionni (2018) rests in the assertion that it will be possible to provide a general model selection framework. This, however, is wishful thinking. There is no way in which policy-makers could 'do it by the book'.

Before elaborating these two points, I would like to address a possible objection concerning the uncharitability with which Grüne-Yanoff and Marchionni (2018) is treated here. Grüne-Yanoff and Marchionni, after all, do not have the stronger model pluralism in mind that Veit (2019a) outlines in his "Model Pluralism" paper, nor the more moderate versions (see Aydinonat 2018a; Ylikoski and Aydinonat 2014) that seem to be able to withstand at least some of the brute force found in the EoR argument. They formulate model pluralism more narrowly as follows: "no model fits all situations" and "different models are right depending on the purpose to which they are put" (Grüne-Yanoff and Marchionni 2018, p. 266). Nevertheless, given that they assert they are attracted to Rodrik's pluralistic understanding of economics and modeling it seems at least awkward to suggest that

there is a problem for this position to overcome in order to be acceptable. Indeed, as a criticism of Rodrik, their arguments may very well be considered successful. As demonstrated in section 2, the original formulation of Rodrik is flawed in various respects, and Grüne-Yanoff and Marchionni do well to point these out. Yet, we should also recognize that Rodrik never intended his alternative picture of economics to be a fully adequate one. His book is not to be taken as "treatise on economic methodology" (Rodrik 2018, p. 276). As such, we should be free to follow Aydinonat (2018a) and Veit (2019a) in their attempts to improve the idea of "model pluralism" for if they are correct, then there is a much bigger gem hidden in Rodrik's embrace of model diversity – an idea that philosophers of science are well-advised to take seriously. If Grüne-Yanoff and Marchionni's arguments are successful, this is only to the extent that they take a naïve version of model pluralism as their starting point. But this view, as I shall demonstrate, fails to take seriously the more rigorous arguments for an embrace of model pluralism that have been offered elsewhere.

3.1 Taking Model Pluralism Seriously

What Grüne-Yanoff and Marchionni see as a fatal flaw of model pluralism, Aydinonat (2018a) and Veit (2019a) see as its greatest strength. Yet, both sides call for a revision of Rodrik's ideas in order to make them usable. Here, it might be useful to draw on a distinction I have previously drawn between strong, moderate and weekly moderate model pluralism (Veit 2019a).

Strong model pluralism is the view I endorse and what I see as the natural endpoint of a continuing trend in the philosophy of science to paying more attention to the context of models: "For almost any aspect x of phenomenon y, scientists require multiple models to achieve scientific goal z" (Veit 2019a, p. 6). This position is considerably stronger than any other found in the literature, such as Weisberg (2013), Ylikoski and Aydinonat (2014), Potochnik (2017), and Aydinonat (2018b). These authors are defenders of *moderate model pluralism*: "There exists an aspect x of a phenomenon y such that scientists need multiple models to explain/predict x" (2019a, p. 6). Finally, I have ascribed a *weakly moderate model pluralism* to Rodrik, Levins, and the large majority of philosophers engaged in the philosophy of models literature: "Each phenomenon has many different aspects, and scientists need different models to explain/predict these different aspects of a single phenomenon" (2019a, p. 6). Aydinonat thinks that moderate model pluralism is the most valuable contribution within Rodrik's monograph, yet it is doubtful whether he ever defended such a view. While Rodrik (2018) admitted that "Aydinonat is right" and his monograph "is slippery on my preferred version" (p. 278), he does not seem to fully grasp the more nuanced points Aydinonat attempts to make, seemingly treating it as the mere stitching together of multiple models. But this is not what Aydinonat had in mind.

Grüne-Yanoff and Marchionni (2018) give Aydinonat's suggestions slightly more attention, considering his position as a footnote towards an alternative view on model pluralism: "[...]could also be interpreted as recommending the use of several models in the understanding of a specific target" asserting that a "more complicated version of our selection procedure could be employed to select the 'right set of models' for the purpose" (p. 274). But a general model selection framework will always fail, regardless of whether it is intended to provide a single model or a set of models. More importantly, however, I think that it is a mistake to treat a model-selection framework for a set of models as a mere issue of scale. Selecting *the* model, and selecting the right set of models are different problems. The latter requires paying attention to the context and the relation between different models, a factor Grüne-Yanoff and Marchionni largely omit in their paper. But if one takes the 'family of models' perspective seriously - whether in Ylikoski and Aydinonat (2014) or the 'population of models' idea in Veit (2019a) - than it is in precisely these connections and context that we find the reason for why modeling in science is so strikingly successful even in the absence of a model-selection procedure. It is not merely a stitching together of models. It is thus not clear that their model of model selection can simply be extended to capture the actual pluralist views in the field.[4]

Indeed, if one accepts the strong model pluralism in Veit (2019a, 2020), model pluralism can no longer be seen as a mere mode of investigation as more moderate views would have it (e.g. Aydinonat 2018a). It is a view about the nature of the world, science, and us as cognitively limited agents, in which pluralism isn't just *a* strategy, but the only possible avenue for those who want to gain knowledge about the world (Veit forthcoming; Teller 2020; Pearce 2013). This form of pluralism can be understood as both an epistemological *and* a metaphysical thesis with close ties to the doctrine of perspectivism. While there is no agreed upon definition of perspectivism or perspectival pluralism, they all share the view that the world is too complex and our cognitive capacities too limited as to avoid a plurality of perspectives (see Veit and Browning 2020a). Pluralists, such as Giere (2006), Wimsatt (2007, 2012), Van Fraassen (2008), Massimi (2012), Chang (2012), Mitchell (2020), Teller (2020), and Veit (2019a, 2020) have long argued, that multiple scientific methods, models, experiments are a source of strength rather than a weakness.

As metaphysical thesis, though this is an optional element, perspectival pluralism is located between the two extremes of metaphysical realism and anti-realism in science (Massimi and McCoy 2020). Cartwright (2019) herself, who appears to be a major influence on Grüne-Yanoff and Marchionni has expressed such a view in her recent monograph *Nature, the Artful Modeler*. She maintains that models are what makes science successful, while acknowledging that we have little grasp on how

[4] I thank one of my reviewers for convincing me to press Grüne-Yanoff and Marchionni (2018) on this point in more detail.

to make sense of this metaphysically. She proposes to see nature *itself* as a modeler, who arranges the world in such a way as to make models epistemically successful. Talk of nature as modeler is best interpreted metaphorically (Veit forthcoming). If nature is understood as complex and ever-shifting, we should not expect that we could ever find a general model selection procedure that conceives of nature like a book of secrets - only to be revealed once we discover the right rules. Nature is not like a book for which we would just need to find the right model in order to translate its secrets into a form we can understand (see also Veit & Ney 2021 for the importance of metaphors in science).

What pluralist views on models - such as those found in Ylikoski and Aydinonat (2014) and Veit (2019a) - point to is a certain kind of anarchism with respect to single models. In a previous paper on what I dubbed "model anarchism" (Veit 2020), I argue that we should not expect a philosophical analysis at the level of generality typical in many philosophical discussions, to successfully lead to useful generalizations across models. Both nature and the various entities and activities we call 'models' and 'modeling' are simply too complex and diverse as to allow such approaches to be successful. Ideas such as the notion of a 'family of models' (Ylikoski and Aydinonat 2014) and 'population of models' (Veit 2019a) are an attempt to highlight the complex relations we find between models and the world. The mappings between these domains are many-to-many and cannot be understood without taking the context-sensitive and pragmatic nature of what 'modelers', or rather all scientists, are engaged in into account. If we idealize away from this 'messy' nature of actual scientific practice our philosophical accounts are bound to be uninformative. Weisberg similarly alludes to Feyerabend when we are faced with the philosophical problem to make sense of the success of modeling practice:

> Just as theorists offer incomplete, idealized models of their targets, so must philosophers. Theoretical practice is rich and multilayered, and the world is often uncooperative. Paul Feyerabend's dictum that "anything goes" in science often seems true of theoretical practice. [...] philosophical analysis will necessarily be partial and incomplete.
>
> – Michael Weisberg (2013, p. 6)

If one takes such pluralist views on science seriously, Grüne-Yanoff and Marchionni's demand for a general selection framework would almost appear as the assertion that science needs to solve the problem of choosing between diverse models for a specific purpose, for science itself to be tenable. But as Weisberg rightly notes, philosophical analysis on such a level of generality will necessarily be partial and incomplete. If model pluralists are correct, there will simply not be a non-pluralist alternative that isn't likewise faced by the problem of choosing the right model. Under-determination is simply a feature of the complexity of nature and the scientific

activity we are engage in, which is precisely why we require a plurality of methods to deal with this complexity of the world.

In a footnote, Grüne-Yanoff and Marchionni state that the "availability of such a selection procedure is not sufficient to justify model pluralism. Even when the procedure successfully selects one model, or a manageable set of models, doubt remains whether the model so selected should be legitimately trusted for the purposes of prediction, explanation, and intervention" (p. 274). But even if one subscribes to the received view of economic modeling practice with a focus on case-studies of different singular models, it is unclear how the problem Grüne-Yanoff and Marchionni raise for model pluralism is a novel problem for science. Economists looking for the right 'general' model are still faced with the question of what the appropriate model is. In fact, if the arguments presented here are valid, the problem is much more severe for anti-pluralist views of modeling. Economists rarely discard models completely, but if the goal is to find one model that is supposed to satisfy diverse and mutually incompatible goals, such as simplicity and completeness, irrespective of the context, they are faced not with an embarrassment of riches, but with an *embarrassment of impossibility*. It is thus misleading to paint it as a problem for pluralism, since it is a problem for science itself. EoR cannot be overcome by dropping pluralism, since model pluralism itself arises out of the perspectivist recognition that we *need* the diversity of models to even begin to make sense of the world. If anything, model pluralism serves as a partial solution to this problem of diversity, by narrowing this 'huge' set of models down to a smaller set fitting the task at hand.

3.2 Facing the Embarrassment of Impossibility

The mistake in the EoR lies in the assumption that it is possible to provide a general model selection procedure. The goals for which models are put to use are too diverse to allow for a unified account of model selection. This does not entail a naive model anarchism in which 'anything goes' and policy-makers would be free to choose any model that they see fit, but rather points to a much more pluralist and context-sensitive view that takes the tacit knowledge and practice of policy making seriously. Let us return to Grüne-Yanoff and Marchionni's three paths towards an EoR and illustrate how model pluralism turns the problem into an *embarrassment of impossibility*.

1*. Robustness Analysis. In the case of robustness analysis, the EoR is the easiest to dissolve. Grüne-Yanoff and Marchionni argue that the model selection problem needs to be solved in order for model pluralism to be viable. Yet, robustness analysis is merely a narrower form of model pluralism. Each change in a particular model creates a new one, with further changes and alterations creating a genuine genealogy of models. As the model pluralists argue, it is the very presence of this multiplicity of models that gives robustness analysis its strength (Ylikoski and

Aydinonat 2014; Aydinonat 2018b; Veit 2019a). Grüne-Yanoff and Marchionni thus demand that model pluralism needs to apply model pluralism (in the sense of robustness analysis) in order to be viable. The problem is: there is no alternative to a reliance on a diversity of models, since science derives its strength, not its weakness, from the existence of an abundance of models. Lehtinen (2016, 2018), for instance, argues that we can gain indirect evidence for models through robustness analysis - thus undermining the idea that models always need to be underdetermined. It is precisely through the use of models with a wide range of different assumptions that we will get a better grip on the causal patterns of the world we are faced with. Robustness analysis is often misleadingly conceived as the confirmation of a particular model. However, it is not the *individual* model that matters, but rather the entire population it is part of (Veit 2019a). It is from a population of models that we make progress in understanding the complexity of the world, *not* just from a particular model. This is why it is a mistake to see a 'failure' of robustness analysis to determine a single model as an embarrassment of riches. The goal of selecting a single model is usually not the purpose of robustness analysis to begin with.

2*. Lack of Empirical Evidence. Grüne-Yanoff and Marchionni suggest that the lack of empirical evidence for models and their assumptions makes model pluralism particularly problematic. This, I think, gets things backwards. Models are not only underdetermined by the evidence when it is impossible to create a testing-scenario for the model, or moral and legal constraints constrain our empirical investigations. Models are *always* underdetermined by the evidence. This is why model pluralism is needed to deal with the evidential uncertainty. Faced with an abundance of underdetermined models, it would be too high of an epistemic risk to put all our eggs in one basket and rely on only one model, since we will never have the kind of confidence in a single model that a general selection procedure would demand. This is not a problem *for* model pluralism, but again one for science more generally and is precisely the reason why we rely on robustness analysis. Model diversity is the only way to deal with this problem (see also Mitchell 2009), since it acknowledges this complexity of the real world. This is not to say that we will find easy solutions, but rather that we are actively trying to find models that map onto these complex relations instead of trying to find one general model that captures everything.

3*. Pragmatic Limitations. While Grüne-Yanoff and Marchionni's final argument for the EoR was only a sidenote in the entirety of their paper, it is now the only remaining candidate. Here, however, again their argument is undermined by the goal of providing a general model selection framework. Pragmatics entirely depend on context – an issue that they deliberately chose to omit for the sake of their idealized model. But it is here that their idealization leads to a misrepresentation of actual scientific modeling practice. While they assert that they would address the EoR "from a more pragmatic perspective at the end of Section 5" (p. 266), they fail to do so in the end. Let us therefore conclude this paper by addressing this point ourselves.

4 Concluding Remarks

In the last decades, philosophers of science have shifted much of their attention away from laws and towards models, modeling-practices, and model-based science. Much of this work, however, has been undertaken from a detached armchair position. Indeed, unfortunately little has been written on scientific modeling practices and the roles of models in policy making. Models, however - in particular in the social sciences - are often intended to inform and for use in public policy making.

Perhaps, part of the avoidance among philosophers in engaging with the conceptual problems of model-based policy making, has rested in the naive idea that scientists are in the business of supplying policy-makers with the ONE true model, that they can then straightforwardly apply, i.e. 'read off policies'. Due to the efforts of numerous philosophers of science, such as Douglas (2009) and Mitchell (2009), little if any credibility remains for this conception of the relation between modeler and policy-maker. The rapid proliferation of modeling practices within science raises the challenging question of how policy-makers should deal with the ever-growing number of models. Due to the incredible diversity of models, it is simply no longer possible for policy-makers to simply 'read off' the appropriate policies from science, if it ever was to begin with (Cartwright 2019; Veit forthcoming). There could never be a successful, yet context-free, model selection procedure. To idealize away from the context-sensitivity and relationships among models in order to achieve a general model of model selection, will necessarily lead to a sacrifice in realism and precision - something Levins has already pointed out in 1966. This does not prohibit the possibility of some useful heuristics in narrow cases such as epidemiological outbreaks, weather forecasts, or models for future economic crises, but these will always be context-dependent and need to be sensitive to the "values, judgments, and evaluations of an ethical, political, or practical nature" (Rodrik 2015, p. 211).

How then should policy-makers use models in their practice? This is no easy task. But it is doubtful that an easy solution to this problem was ever on the table to begin with. Here we should follow Cartwright (2019), Rodrik (2015) and others, and see modeling as more of an art or craft, something that requires actual policy making experience, extensive background knowledge, and a number of often unarticulated skills in the implementation of models. Rodrik (2015) expresses this point elegantly:

> But as the *science of trade-offs*, economics deftly enlightens us on both sides of the ledger—the costs and benefits, the known and the unknown, the impossible and the feasible, the possible and the likely. Just as social reality admits a wide range of possibilities, economic models alert us to a variety of scenarios. Disagreements among economists are natural under the circumstances, and humility is the right attitude all around. It is better for the

> public to be exposed to these disagreements and uncertainties than to be lulled into a *false sense of confidence* about the answers that economics provides.
>
> – Dani Rodrik (2015, p. 209) [italics added for emphasis]

To make our model selection sensitive to realism and at the same time precise enough to actually narrow our choice of models will involve a sacrifice in generality. Economics as the *science of trade-offs*, should be sensitive to the necessary role of trade-offs within modeling itself. Consider the analogy of a craftsman who receives calls to fix x. x could be anything, and every instance of it may substantially differ from others. Even if it seems initially appealing to provide a general selection procedure for electricians, plumbers, etc. to optimize their work, there is simply no such general framework that wouldn't ultimately hinder rather than help the agents in action. While there might very well be a heuristic role for such frameworks for those who are starting to learn a craft, it should be considered a scaffold meant to be ultimately discarded. At this point, our able craftsman has developed a vast skillset of diverse methods, yet might never be able to list them all if asked to do so. We should not convey a *false sense of confidence* that the complexity of the phenomena economists study can simply be boiled down into a simple and general model-selection framework from which we could simply read off the appropriate model. Policy making isn't about choosing *the* right model, it is about using the richness of economic models to make a call about the best possible policy.

In his book, Rodrik discusses the illustrative case of the Nobel prize winner Jean Tirole, who left journalists frustrated by not being able to state the results of his research in a single sentence (pp. 209-210). There was not one big idea that brought everything together in a way that could satisfactorily summarize his work with a simple slogan. Instead, Tirole created a rich set of diverse models for a variety of problems, denying that railroads could be regulated in the same way as intellectual property. He took it to be an absurdly simplistic viewpoint of the nature of economic science that we could come up with a single model to satisfy all the purposes of policy making irrespective of context. The devil, as so often, lies in the details. In a paper with Ricardo Huasman and Andres Velasco, Rodrik demonstrates this pluralist way of doing economics by criticizing standard models for economic growth. They argued that in order to provide growth diagnostics, it is simply not enough to try "to come up with an identical growth strategy for all countries, regardless of their circumstances" (Hausmann et al. 2008, p. 326). This should not be taken as a signal for the great ingenuity of the modeler, but rather as a striking alarm bell that something is seriously wrong with it. Attention needs to be paid to the particular situation, history, and constraints faced by countries such as El Salvador, Brazil, and the Dominican Republic. The necessity of gathering empirical data about the particularities of the specific target systems simply precludes the possibility of developing a general model without losing much information as a result.

Clearly, there are limits to the extent to which policy-makers can rely on the diversity of models in economics. Individual economists can become specialized in a certain set of models, without undermining the essential tenet of model pluralism: that it is the entirety of models that truly matters. Rodrik thus urges economists to be more humble - to recognize how little they know and how specialized their knowledge really is, rather than push beyond their own limited expertise and give economics a bad reputation in the process (pp. 209-211). Economics, in many ways, should be considered a collective endeavor. It's not the individual economist that matters but the discipline as a whole - and the same goes for the economic models they construct. An individual policy-maker, of course, neither has the luxury to have an overview of all of economics, nor to be specialized in a particular domain of economic research. But here we should similarly recognize that policymaking is not an individual feat. A complex network of agents is usually engaged in the decision process for every single policy. Social epistemologists have long highlighted the fact that such epistemic processes don't occur on a mere individual level. The collective can make use of the knowledge and skills of the individuals involved. This is why diversity among actors in such a network is so beneficial. And it is the very same reason we should embrace a diversity of models - it is a necessary and unavoidable part of science as a human enterprise. This is why an expansion of our modeling toolkit should be seen as a measure of success, not a sign that something has gone seriously wrong. Ought implies can and Grüne-Yanoff and Marchionni (2018) fail to recognize that model pluralism, as a thesis about the nature of the world and the collective nature of science, denies that there is any other path for science to (successfully) proceed.

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