*Theoria, 2023, 38(2), 225-245*
*https://doi.org/10.1387/theoria.23849*

THEORIA

*ISSN 0495-4548 – eISSN 2171-679X*

# Model anarchism

*(Anarquismo de modelos)*

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**ABSTRACT:** This paper aims to articulate an anarchist challenge to a widespread assumption in the rapidly growing philosophical literature on models, modeling-practices, and model-based science. I argue that the various entities and practices called "models" and "modeling-practices" are too heterogeneous, too context-sensitive, and serve too many scientific purposes and roles, as to constitute unified scientific phenomena that would allow for useful epistemic and ontologies analyses. Just like Feyerabend once argued that there are no general useful inferences to be drawn about the method of "science", I argue that the same lesson will apply to "model-based science", hence calling my view model anarchism.

KEYWORDS: models, modeling, model-based science, anarchism, Feyerabend

***ABSTRACT:*** *Este artículo pretende articular un reto anarquista a una asunción extendida en la creciente literatura filosófica sobre modelos, prácticas modelizadoras y ciencia basada en modelos. Argumento que las diversas entidades y prácticas llamadas "modelos" y "prácticas modelizadoras" son demasiado heterogéneas, demasiado sensibles al contexto, y tienen demasiadas funciones y propósitos científicos, como para constituir un fenómeno científico unificado que dé lugar a análisis epistémicos y ontológico útiles. Del mismo modo que Feyerabend argumentó que no pueden hacerse inferencias generales y de utilidad acerca del método de la "ciencia", argumentó que la misma lección se aplica a la "ciencia basada en modelos", por lo que llamo a mi propuesta anarquismo de modelos.*

*PALABRAS CLAVE: modelos, modelizar, ciencia basada en modelos, anarquismo, Feyerabend*

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***How to cite:*** Veit, Walter (2023). «Model anarchism»; *Theoria. An International Journal for Theory, History and Foundations of Science,* 38(2), 225-245. (https://doi.org/10.1387/theoria.23849).

Received: 2022-08-02; Final version: 2023-05-26.

ISSN 0495-4548 - eISSN 2171-679X / © 2023 UPV/EHU

## *1. Introduction*

The recognition by philosophers of science that an exclusive look at theories and laws would lead to a misunderstanding of much, if not most, of what goes on in science, led to major shift in attention in the last decades towards "models", "modeling-practices", and "model-based science" (henceforth abbreviated as MMM). Indeed, these past decades have been pervaded by a sense of optimism not unlike that of earlier philosophers of science betting on theories and laws for discovering *the* method of science, as though model-based science constituted a unified kind. Despite the extreme heterogeneity of models and modeling-practices within science, many remain convinced that we will be able to arrive at general answers about the nature of models or derive general epistemic taxonomies into which we can usefully categorize different kinds of models.

The goal of this programmatic paper is to articulate a challenge to this optimistic working assumption within the field that MMM share enough family resemblance to allow for a general, or even intermediate, level of philosophical analysis. Instead, I will defend a thesis I dub "model anarchism":

> **Model Anarchism**: "models", "modeling-practices", and "model-based science" are too diverse, too context-sensitive, and serve too many scientific purposes and roles, as to constitute unified kinds that would allow for useful epistemic and ontologies analyses.

Importantly, model anarchism does not deny that philosophers can or should study particular instances of scientific activity, whether they are called "model" or "modeling", such as Maynard Smith's "Hawk-Dove model" in evolutionary game theory (1982), but that there is no real unified category here that can hope to be given a general philosophical account or taxonomy.

The title of this programmatic paper and thesis is a homage to Paul Feyerabend's (1975) famous, yet often misunderstood, monograph *Against Method* which challenged the straightjacket of methodological monism about the scientific method that was at that time common in the philosophy of science. Feyerabend's famous dictum of scientific anarchism —that "anything goes"— was not meant as a general principle, but rather as a self-ironic reflection to capture the horror methodological monists might feel when looking at the diversity of actual practice and history of science.[1] The then common philosophers' ideal of analyzing some aspect of science to derive general methodological rules he strongly resisted, instead urging us to adopt a radically pluralist stance in which mainstream philosophy of science activities at a high level of generalization come to be abandoned (see also Farrell 2013). In this spirit of a pluralist liberation from narrow epistemic straightjackets, I will likewise defend an anarchist approach to "model-based science", critiquing the idea that there is a general and unified phenomenon here that would allow for broad and useful epistemic generalizations.

[1] See Shaw (2017) for an excellent analysis of Feyerabend's radical endorsement of pluralism.

### 1.1. Article outline

This article is structured as follows. In Section 2, I will outline some of the history of the MMM literature, which will help us to contextualize the goals and arguments for model anarchism. In Section 3, I will take aim at the widespread idea that all of the heterogeneous things called "models" can be captured by a general representationalist account of models. Nothing at this level of generality, I argue, will provide us with meaningful epistemic insights into the workings of science. In Section 4, I take aim at the more mid-level ambition to provide useful typologies of models, arguing that this level of abstraction will obscure what is most important about models, i.e. their context and diversity. Finally, in Section 5, I will conclude the case for model anarchism and offer a further discussion of how embracing this position ought to impact the field.

## *2. Some history of the field*

Much of what now falls in the philosophy of MMM originated in the so-called *semantic view of theories*. I will therefore begin with a brief historical overview of the philosophy of MMM literature which, while sketchy and idealized, will allow us to acknowledge the historical context of the field and highlight a number of relevant features for my defense of model anarchism.[2]

The philosophy of science emerged as a stand-alone field that aimed to understand the success of science. Much of the early history of the field was focused on theories, which were understood as linguistic entities, i.e. collection of sentences. This so-called *syntactic view of theories* largely drew on work in the philosophy of language, employing concepts such as "reference", "representation", and "truth". In this context, the question of what makes a good theory appeared straightforwardly answerable in reference to the truth of the sentences making up that theory, which the logical empiricists tried to formalize in various ways in virtue of the relationship between scientific laws and observational statements.

Feyerabend was one of the most outspoken critics of this project, but more moderate critics had a significantly larger impact on the field. One strand of opposition to this "received view" —perhaps most notably by Bas van Fraassen (1980)— agreed with the focus on theories, but maintained that models, not sentences, ought to be seen as *the* ingredients constitutive of theories. This challenge came to be known as the *semantic view of theories*, with theories being made up by a set of models supposed to represent the world (often by some form of isomorphism).[3] Surprisingly, the conviction that scientists deal in models rather than sentences did not move the philosophy of science initially all that much closer to scientific practice. As Godfrey-Smith (2006a) notes, the early development of the semantic view remained committed to a highly formal analysis of science, thus making the difference between the syntactic and semantic views merely one of emphasis, concerning

[2] Here, I largely draw on the longer historical overviews by Teller (2001); Godfrey-Smith (2003; 2006a); Downes (1992, 2011, 2020); Gelfert (2016); Frigg and Nguyen (2020); Frigg and Hartmann (2020).

[3] See Suppe (1977a) and the other contributions to his collected volume in Suppe (1977b).

which formal approach is to be taken. Both sides were eager to characterize their approach as *the* correct description of science.

Two ideas were central to the semantic view of theories: firstly, that *all* scientific theories ought to be reconceived as sets of "models", and secondly, that *all* "models" were meant to be captured in the mathematician's and logician's sense of the term (Godfrey-Smith, 2006a, p. 727). While this monist view faced immediate problems in capturing the heterogeneity of "models" in science, it was seen as a *superior framework* to the syntactic view, and thus endorsed by many - a feature that we will see time and time again in the continuous fragmentation of the field.

Later, some philosophers of science became inspired by the alleged superiority of the semantic view in describing science in terms of models, which turned them more closely to the actual usage of models by scientists. Here, Cartwright (1983), Hacking (1983), and Giere (1988), alongside others, spearheaded a significant shift in the philosophy of science towards scientific practice. While they continued to criticize the syntactic view, more and more attention was given to how scientists actually construct and use models.[4]

But what looked like progress in the following years, as Godfrey-Smith (2006a) rightly pointed out, made the term "model" incredibly ambiguous. This problem, Downes (1992) argued, stemmed from the failure of the semantic view to acknowledge important differences of "models" in logic from those in science, instead trying to provide something of a unified account. Important figures, such as van Fraassen (1980), despite admitting that their "usage is somewhat different" insisted that "the usages of 'model' in meta-mathematics and in the sciences are not as far apart as has sometimes been said" (p. 44). Godfrey-Smith described them as simultaneously trying to see "models" as "an important real-world scientific tool, and as a concept that could be used in an abstract way to describe all of theoretical science" (2006a, p. 728). The MMM literature was faced with the dilemma of endorsing a narrow mathematical sense of model that failed to describe what went on in the daily practice of science or stick with the ambition of an inclusive account and endorse some very deflationary sense of model as any form of representation, which failed to recognize substantive differences in the plurality of scientific theorizing. The ambition to provide a general account of models seemingly failed in the face of the heterogeneity of MMM. Yet, attempts to provide general answers continue until the present.

Philosophers of science continue to defend the idea that *all* models are some form of abstract mathematical structures, real structures isomorphic to a target system in the real world, *some* structure with an interpretation and a similarity-relationship to the world, or simply any kind of representation. To make sense of the representational nature of models, philosophers proposed various alternative such as *all* models being variously abstract structures, fictional entities similar to imaginary systems, forms of art, epistemic tools, concrete physical structures, and so forth (see Gelfert, 2016; Frigg and Hartmann, 2020; Downes, 2020). Parts of the literature can only be understood under the assumption that models must share a single property, with many papers existing that serve to simply deny one account in favour of a different generalist view of models. It is thus hardly surprising

[4] Importantly, the logical empiricists were also quite interested in scientific practice and had proto-semantic views of scientific theories. One might thus see this practice turn as something of a *Rückbesinnung*.

that a variety of philosophers have expressed skepticism that there is a general metaphysical answer to the question of what models are, instead seeking their unifying feature in their representational nature while maintaining that in principle anything can be a model (Swoyer, 1991; Teller, 2001; Suárez, 2004; Callender and Cohen, 2006; Giere, 2010; Odenbaugh, 2018).

I argue that such a deflated sense of representation cannot tie this literature back together. In the face of the incredible heterogeneity of "models" and "modeling-practices" it has become bizarre to think that a minimal sense of models as being some sort of representational device could provide unity to this literature, in addition to blatantly excluding a variety of models that should not be considered representational devices. I am thus closer in spirit to a suggestion by Goodman (1976) in the face of the heterogeneity of usages of the term "model": that it may "well be dispensed with in all these cases in favour of less ambiguous and more informative terms" (p. 172). As I shall argue, what is being called a model and what isn't is extremely relative to the social norms of particular scientific communities, but has comparatively little to do with how they function in science.

Frigg and Hartmann (2020) resist this move towards sociological factors in the face of the heterogeneity of models, noting that the problem of categorizing different types of models in science "cannot be dismissed as 'just sociology'". But this need not be disparagingly seen as *just sociology*: the concern that there is nothing for philosophers to do or that the entire literature ought to be discarded. Worries of this kind may have made model anarchism seem suspect and untenable, thus ultimately preventing philosophers from seriously investigating its merit.

There are thus two common views in the field that I will argue against here in the favour of model anarchism: (i) the view that all the things called "models" have the shared function of representation, and (ii) that we should develop more fine-grained typologies of models, rather than give up on the very idea of providing taxonomies of models altogether. While both points are related, I will address them separately here for the purposes of clarity.

## *3. Against monism about models*

Even after roughly 40 years of tremendous work of the highest intellectual calibre philosophy of science has to offer, Frigg and Nguyen (2020) note in their recent book on scientific models, there is "no stable terminology, no shared understanding of what the central problems are, and no agreement on what might count as an acceptable solution" (p. v). That there would not be any consensus forthcoming for the great plurality of things called "models" and "modeling" ought not to be surprising. That philosophers even tried to provide a general account of models as if they were a single thing can be seen as an unfortunate inherited tendency of philosophy towards monist thinking.

The history of philosophy itself as a discipline at the highest level of abstraction is of routinely attempting to carve nature and provide clear definitions, often leading philosophers on wild goose chases for definitions that could not be obtained. Resistance to this methodology was often seen as resistance to philosophy itself. Admittedly, the philosophy of science suffers from this tendency less so than other areas of philosophy. Few domains of philosophy show a greater embrace of pluralism than the philosophy of science and hardly

any philosopher of science would categorize themselves as an essentialist or monist.[5] But despite the widespread acceptance that "models" are an extremely heterogenous cluster of things, perhaps more so than anything else going on in science, the great majority of participants in the MMM literature adopt something like a shared working definition of "models" as constituting representations or at least being used to represent. In his introductory textbook to the field, Downes (2020) expresses this sentiment succinctly:

> There is almost complete consensus among philosophers of science working on models on only one idea: models are representations or models represent. The idea is so prevalent that many do not think it requires supporting argument. Rather, the idea that models represent is the assumed baseline for *all* discussions of models. (Downes, 2020, p. 52) [italics added for emphasis]

Whether a model is a good one is then simply answered in virtue of whether the model succeeds at representation. But this starting assumption may not be a good one, regardless of how ubiquitous its endorsement may seem. Unfortunately, even the most pluralist and anti-essentialist thinkers within the field, such as Teller, endorse the view that all models are representations:

> I take the stand that, in principle, *anything* can be a model, and that what makes a thing a model is the fact that it is regarded or used as a representation of something by the model users. Thus in saying what a model is the weight is shifted to the problem of understanding the *nature of representation*. (Teller, 2001, p. 397) [italics added for emphasis]

Teller criticizes Cartwright (1983) among others for emphasizing the heterogeneity of "such things as idealizations, prepared descriptions, physics as theater, caricature, works of fiction, and simulacra, but gives no uniform account" despite repeatedly speaking of "models" as if they were a single thing (Teller, 2001, p. 396).[6] Against those demanding a general account of models that identifies something like intrinsic properties, Teller replies that "there are no such features" instead maintaining that it is *us* that "make something into a model by determining to use it to represent" (p. 397). That even something like a sentence could be a model, if used as a representation, is notably quite the detour from the origins of the MMM tradition in the semantic view.

To many this will undoubtedly already be "anarchic" enough, with ontologies of models allegedly becoming 'mere sociology' in comparison to a functional analysis of how models are used to represent and succeed at doing so. They want to resist the idea "that 'scientific models' is a catchall phrase for what is actually a heterogeneous collection of objects" (Contessa, 2010, p. 216). Here, I will challenge this last remainder of monism in the field. Not only will I argue that (i) a deflationary sense of representation fails to pick out a unified kind of scientific activity, but also (ii) that not all scientific models are representations; thus making this consensus both too broad and too narrow.

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[5] I am myself "guilty" of endorsing pluralistic views in a variety of debates within the philosophy of science (Veit & Browning, 2020; 2021; Ortmann & Veit, 2023).

[6] Cartwright does say that a model "is a work of fiction", but that should probably not be interpreted as a strong commitment to a fictionalist account of models, rather than a recognition that some models involve falsehoods and idealizations (1983, p. 153).

### 3.1. Why the deflationary account is too broad

Perhaps the most fundamental core problem with drawing on the concept of representation to try and unite the heterogeneous literature on modeling is that representation itself is an incredibly elusive kind of phenomenon and philosophical accounts differ wildly. As mentioned above, this heterogeneity is already reflected in the numerous attempts to cash out the representational relationship of models with the world. In effect, endorsements of a deflationary account of representation repeat the mistake of the early proponents of the semantic view to make their account able to cover all cases (including possible cases) where the term "model" is used, but unlike the precise proposals for such a view of models in terms of mathematical set-logic, leave nothing in its place. That models are representations is merely asserted in order to make sense of their scientific success. Successful models are those that successfully represent. But in the absence of even the most minimal kind of consensus of how a model can achieve this function, the representationalist view remains hollow and uninformative.

Just like when scientists study nature and learn that what was believed to be a single phenomenon turns out to consist of a heterogeneous cluster of different phenomena with very little family resemblance, we should give up on generalized accounts for the most heterogeneous cluster of things in science. In virtue of increasing the generality of our account of models to accommodate their diversity, we will inevitably be faced with trade-offs in precision or realism, which will ultimately make such an account uninformative.[7] This is precisely why certain strands of analytical metaphysics are derided as unhelpful: they are operating at such a high level of abstraction that it is unclear how such work can be useful. Nevertheless, metaphysics has recently been defended as a form of modeling in precisely the sense of developing highly abstract and idealized representations of the world (see Godfrey-Smith, 2006b; Paul, 2012). So it is hardly surprising that those who endorse the representationalist view of models, such as Michael Weisberg (2013), treat their work in the MMM literature as the building of models *of* modeling:

> 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. Nevertheless, by developing philosophical accounts of modeling, we can start to get a handle on theoretical practice. But just as in a *representation* of any other complex phenomenon, philosophical analysis will necessarily be partial and incomplete. Thus the accounts described in this book are themselves models of modeling. (Weisberg, 2013, p. 6) [italics added for emphasis]

Here, Weisberg notably admits the possibility of a position that has not yet been defended: model anarchism. Modeling practices appear so diverse as to not be amendable to a general philosophical analysis. His strategy of dealing with the heterogeneity of MMM was to provide more pluralist answers than the previous literature did: i.e. by providing more fine-grained typologies of models and evaluating models based on these categorizations. In the recognition of this diversity, however, Weisberg remains firmly committed to the idea that *all* models involve representations. That this is a problematic assumption has been force-

[7] See the work of Weisberg (2013) on trade-offs in model-building.

fully expressed in an essay review of Weisberg by O'Connor and Weatherall (2016), two philosophers who notably are "modelers" themselves. Expressing their dissatisfaction with the literature, they argued that the core problem in the MMM literature is that the "analysis begins with the assumption that there is a single relationship that bears between models and the world" (p. 626). But in the absence of refinement of this representational relationship —e.g. moving from isomorphism to vague notions of similarity— it is unclear why this assumption should continue to be held:

> The more one digests examples of modeling practices across fields, the less plausible it seems to think that the same basic relationship holds between a mouse exposed to radon gas and humans suffering from cancer, as between a relativistic space-time and the universe over the course of its entire history, or as between a bargaining game and negotiations over Iran's nuclear program, or as between the BlackScholes formula and traders' expectations about market volatility. It is a sociolinguistic fact that scientists tend to use the word "model" often. But one cannot infer from this that there is a natural activity or category of practice that the term tracks. (O'Connor and Weatherall, 2016, p. 626)

It is of course tempting to find some unifying element between all the different entities and practices called "models" and "modeling" in order to explain the mysterious success of "model-based science". But the only account that we see able to accommodate this demand and the diversity of models, would categorize something as a model whenever *something* is used by a scientist for the purpose of representation. Weisberg maintains that the *structure* of the model is important, but unlike the early work in the semantic view of theories, leaves out any specification of what that would entail. But there simply isn't a general unified account of structure either that could cover any and all phenomena used to represent. As O'Connor and Weatherall (2016) note, this puts us in the uncomfortable situation in which "[o]ne might as well have an account according to which a model is a 'thing' and leave it at that" (p. 625). In trying to capture all the various things called "models" our general accounts become vacuous and unhelpful for the normative aspirations of the literature. Nothing is gained, either ontologically or epistemically, by operating at this level of abstraction, and we even obscure important features by emphasizing representation as the one and only thing that matters.

Unifying theories are of course attractive both in science and in philosophy. If all of science becomes a matter of model-building in the sense of trying to represent the world, then as Downes (2011) rightly notes, all of our epistemological problems seem to be solvable by developing the right account of representation. But such a general account of the method of model-based science may be a chimera, not something that can really be found, thus ultimately hindering us from truly investigating the diversity of epistemic purposes for which models are put to use. It is unfortunate that Downes (2020) has not been more critical of this tradition in his later introductory book to the field, but his goal may have been to remain fairly neutral in regards to the promises of a representationalist view of models.

Finally, we may also want to reject the representationalist view of models in virtue of recent attacks on representationalism itself. Some will want to maintain that it is obvious that models are representational at least in some sense, but this begs the question of whether this assumption is really one that can just be taken for granted or for that matter

will be useful. After all, representationalism has long been under attack and in recent years many alternative views have been developed in philosophical discussions outside the philosophy of science.

One philosopher of science who has attempted to bring this challenge to the MMM literature is de Oliveira (2021), who likewise described representationalism as "the view that scientific modeling is best understood in representational terms—is the received view in contemporary philosophy of science" and yet maintained that the view is "untenable and unnecessary, a philosophical dead end" (p. 209). He considers them to be mere intuitions that will falter once we investigate them more closely. Yet, it appears that many philosophers of science hold the view that representationalism needs to be true - for how could we otherwise possibly make sense of the success of models in science? Their success is explained in virtue of representing a real-world target. But how this representational relationship is supposed to be explicated is a matter of great controversy. Therefore, de Oliveira suggests that we should consider the possibility that these difficulties arise from the inadequacy of the analytic representationalist framework itself, rather than in a failure of philosophical ingenuity. His first goal is to show that representationalism is a flawed program, by drawing on general anti-representationalist work in the philosophy of mind by Myin and Hutto (2015).

If one endorses such radical views regarding the untenability of representationalist views (both mind-dependent and mind-independent versions), then it appears to straightforwardly follow that there is little hope for a representationalist view of models. While I share much of the criticism that is common among those who think strong representationalism problematic, one does not need to become a global anti-representationalist (in the sense of nothing called a "model" can be a representationalist device) to deny that this is a unifying feature of all models. More important is the emphasis of de Oliveira (2021) that "we *don't need* representationalism to answer it, so we might as well begin working on developing alternative ways of thinking" (p. 234). Unfortunately, he in turn urges us in the direction of general anti-representationalist views focused on scientific agents to make sense of the success of MMM: a suggestion to replace one general view with another.

Instead, I urge the consideration of a very different and admittedly also radical kind of alternative altogether, i.e. the elimination of the category "model" as a meaningful ontological or epistemic category at the level of science. It is this sense in which it is merely a sociological fact that some things are called models and others aren't. The deflationary account of models as representations is simply too broad to meaningfully offer a general insight into *the* method of model-based science. If almost every thing in science gets to count as a model and almost every activity can potentially be seen as modeling, the deflationary view may succeed in capturing all of MMM, but the account must ultimately be hollow and uninformative at such a level of abstraction - no less so than the commonly to Thales' attributed assertion that everything is water.[8]

[8] I note that Thales held a more moderate position that things come from water, not that all things coming from it *are* water itself. While this hypothesis might be just as wrong, it is informative and could be tested.

### 3.2. Why the deflationary account is too narrow

Another way of rejecting the representationalist view of models is to show that it is too narrow, i.e. that not all models are representations. Despite the apparent consensus that all models involve representations, there are several philosophers who have resisted this claim (see especially Downes, 2011). Here, I offer several examples of "models" that challenge the unquestioned assumptions of the consensus view.

One notable example is Cartwright (1999) herself, whose minute attention to scientific practice has influenced my view here (see also Veit, 2021b). As Downes (2020) notes, Cartwright neither sees theories as representative nor the models that are constructed from them - calling these *interpretive models*. Instead of representing actual target phenomena, these models are meant to "concretize relations between abstract concepts" in our theories (Downes, 2020, p. 64). Downes uses the *harmonic oscillator model* to demonstrate Cartwright's distinction between representational and interpretive models against those like Giere (1988) who maintain that this model represents the horizontal motion of actual physically realized pendulums. Here, Cartwright (1999) maintains that this view is mistaken since these model fail to even approximately represent real world pendulums, with corrections of model to better represent the real-world targets often not being consistent with the theory, nor suggested by it (p. 251).[9] In her recent monograph, Cartwright (2019) again emphasizes that there is typically no guideline, no way one could follow a book with instructions, no opportunity for "reading off" in trying to translate a theory into real-world models. And this gap between theory and reality that can be found in different kinds of "models". Notably, interpretive models can also be found in the social sciences, such as in economics, with various attempts at theoretical refinements of rational choice theory. The goal of much of the work on rational agent models constructed from rational choice theory is not to accurately represent real-world agents, but to make the theory itself more concrete and to explicate abstract concepts of rationality, consistency, and transitivity (Okasha, 2018). Attempts to reconceptualize such work as a form of representation fails to recognize genuine differences in scientific practice. Another example Downes emphasizes in his introduction to the field is a distinction between 'models-of' real target systems and "models-for" the purposes of intervention such as the CRISPR-Cas systems due to Evelyn Fox Keller (2000).[10] Both Cartwright and Keller, as Downes (2020) nicely emphasizes, "find nonrepresentational models at opposite ends of the theory–experiment continuum" (p. 64).

The point made by these authors isn't so much that these models cannot contain representations, but rather that a focus on these properties would be irrelevant to understanding their epistemic contributions to science. The representationalist view of models is too narrow to recognize what really matters epistemically about these cases. Here, it is not enough to defend the representationalist picture by trying to shoehorn these 'kinds' of models into a representationalist picture. If we idealize away features from any set of phenomena, it can be all too easy to group them together. What proponents of the representationalist camp need to show is that we haven't idealized away important features - particularly features that may be important features to understand the function and epistemic

[9] Morrison (2015) offers a critical discussion of this (pp. 130-136).

[10] The distinction has been further developed by Emanuele Ratti (2020).

success of "models" in science. The burden of proof must lie with those that deny the radical diversity of models, not those who emphasize the diversity of actual scientific practice.

Once we look at the myriad roles that models play in science, however, there is little reason to think that anything that would be useful at a general level of analysis of models across all of science is forthcoming. Such general accounts are doomed to failure because there is simply nothing epistemically important that unifies them into a single phenomenon. Here, it is useful to emphasize the detailed case-study work of Tarja Knuuttila (2005, 2011), who has also been critical of the idea that all models involve representation. She argues that our accounts of models should not be centered around the notion of representation, which she maintains will not help us to make as much progress as many within the field believe. Instead, she has defended a view of models as epistemic tools or artifacts, distinct both from experiments and theories, which are used in a variety of ways to promote the ends of science. The problem, however, with her account (and others) of models as artefacts, stems once again from the mistaken idea of providing a general alternative to the representationalist view of models.

Once we pay attention to the actual diversity of models in science and the different ways they are used, there is less hope that a definition of these entities as 'scientific tools' is going to be useful. And what epistemic lessons could possibly be gained by understanding models at this level of generality? Just as there is no general account of science to be defended, there will be no general account of scientific tools. Godfrey-Smith (2006a) once argued that the search for a *natural kind*-like phenomena of modeling in theoretical science was the goal of his work on models (p. 729). But the mere fact that there is a common use of the word "model" does not show that there is a unified phenomenon. And as I shall argue in the next section, this lack of unity ultimately undermines even attempts to provide typologies of models at an intermediate level of grain.

## *4. Against Typologies of Models*

Once we admit that some models aren't representations, the floodgates seem to be opened to the idea that nothing hangs on the issue of whether something is called a "model" or not. Science is diverse and we'd do well, as those in the practice turn have long urged, to admit that nothing of value is to be found at this level of abstraction. Monism about models is doomed to failure. But once we abandon the idea that all "models" must be representations, the more mid-level approach of those trying to develop taxonomies of models becomes similarly problematic. Without a general view of what models are, it seems hard to see how it even makes sense to engage in the activity of distinguishing different types of models. In the section, I will therefore defend the idea that we should also give up the hope of developing even fine-grained typologies of models.

The idea that the entities and practices scientists call "models" and "modeling" are diverse is not a novel one. As I noted above, Downes (1992) argued early on that the literature downplays the differences between the different kinds of things scientists refer to as "models", calling for more pluralist accounts of models (see also Downes, 2011). And indeed, this call seems to have been answered in the last decade. Philosophers of science have paid increasing attention to the diversity, richness, and multiplicity of MMM, making explicit calls for more pluralism in the debate (see Mitchell, 2003, 2020; Weisberg, 2013;

Gelfert, 2016; Potochnik, 2017; Veit, 2019, 2020, 2021a). As I have previously illustrated, there is now an enormous diversity of distinctions being drawn between different types of models, putting us in a very different situation from that Feyerabend found himself in when he wrote his appeal for scientific anarchism (and pluralism). All of this is great to the extent that we are moving away from monism. As the MMM literature becomes ever-more fine-grained, we are well on the way towards an embrace of model anarchism, and my goal in this programmatic paper may well be seen as an attempt to accelerate this development by criticizing the typical move towards increasingly going a bit more fine-grained in the face of inadequate typologies of models, rather than giving up the motivation for such generalizations altogether and focusing on the actual context of scientific practice.

Take for instance Gelfert (2016), who like Weisberg (2013) considers himself to be a pluralist and suggests that the diversity of roles and functions of models is *the* "key to answering any of the more general philosophical questions" (2016, vi) concerning scientific models. I of course agree with this, but the conclusions to be drawn are less of an answer and more of an outright denial that there is sufficient family resemblance to warrant any general answers about MMM. Their multiplicity should be taken as a call to consider eliminativism rather than an ever-more fine-grained approach to different kinds and taxonomies of models. Recall Teller, who maintained that all models involve representations, but denies the possibility of providing useful taxonomies of models. It is the context that explains the success of "models", not their membership of a particular type or group, and I thus deny the possibility of going beyond the case-by-case type of study of scientific practices. Unsurprisingly, such context-sensitive views are in their nature anarchic and have therefore been criticized by Contessa (2010) and Frigg and Hartmann (2020) for failing to show that useful taxonomies cannot be provided. But once we recognize that not all models are representations, the force of context-sensitive views returns with even greater force. Unfortunately, context-sensitive views tend to be discarded without much consideration.

Consider Khosrowi's (2020) hand-wavy dismiss[11] of what could be considered a challenge to the very foundation of the MMM literature:

> We consider the [context-sensitive] view to be an unsatisfactory view as it suggests that there can be no *general taxonomy* of different types of models and modelling strategies that can successfully single out epistemically significant commonalities of tokens of these types with respect to how models relate to targets and in virtue of what kinds of relations they tend to be epistemically successful. It *seems* that there are ways, even at relatively coarse-grained levels of classification, to distinguish between different *types* of modelling activities concerning the respects in which, the particular ways in which, and the degrees to which models involved in these activities need to be suitably related to targets for epistemic success to be likely. (Khosrowi, 2020, p. 540) [italics added for emphasis]

This excerpt nicely illustrates the ambition toward generalizing that philosophers are easily attracted to. Tellingly, the main goal of Khosrowi's paper is to demonstrate the failure of Weisberg's (2013) general *feature matching account* of model-world relationships and ap-

[11] In this Khosrowi is in good company, however, for almost every participant in the literature holds, if not explicitly, at least implicitly, the intuition that there is *surely* a useful general taxonomy to be drawn.

peal to a more pluralist account of model-world relationships. O'Connor and Weatherall (2016) likewise question the idea that a single relationship obtains between model and world, but Khosrowi does not want to take the "radical" step from the heterogenity of models to the denial that we can offer meaningful accounts.

Why not go along with Khosrowi's (2020) suggestion go a bit more fine-grained? This is a trap supposedly "pluralist" thinkers often seem to fall into. In light of the disunified nature of the various entities colloquially called "models" in science, we should be much more reluctant to assume that some general (even if fine-grained) account is in the offing, and we should not try to provide one, due to the inherent risk of idealizing away other important, yet unrecognized relationships. Pluralism is not going to help us with the "understanding of the relation in virtue of which successful models are successful" (Khosrowi, 2020, p. 525), rather it is the denial that any such monist relationship exists. Importantly, Khosrowi does not provide an argument that the context-sensitive view is wrong. Instead, there is explicit reference to what modeling "seems" to be like. It *seems* as if general classifications are possible. It *seems* like we can carve up the diverse practices called modeling. It *seems* like such a carving would allow us to tell, at least to some extent, whether particular kinds of models are going to afford epistemic success. I do not deny that it may *seem* like this to most participants in the MMM literature. But *seemings* do not have to track truth. In fact, the history of science and philosophy suggests that such preconceived notions are shown to be wrong time and time again.

As naturalist philosophers have long preached, our intuitions are worth little if they come to be challenged through investigations of the actual world. We should therefore at least stop for a moment and carefully consider the underlying assumption in the MMM literature that there is sufficient family resemblance of the heterogeneous cluster of 'things' called "models" and "modeling-practices" for useful philosophical abstractions even at a reasonably low level of grain. In this vein, the present article has the goal of developing a concern expressed by O'Connor and Weatherall (2016) in a footnote of their book review of Michael Weisberg (2013): "[o]ur worry is perhaps more basic, since we do not see enough of a family resemblance to justify understanding 'models' as a fruitful unit of analysis at all" (p. 614). Developing this worry in more detail will lead us straight to model anarchism.

### 4.1. Why "models" lack a sufficient degree of family resemblance

In the absence of a unifying feature that all models share, one may still hope to derive something like a property-cluster account of models, perhaps one that clusters models into different categories that have more resemblance with each other than with those in other groups. After all, there are many other concepts in the philosophy of science that are meant to capture heterogeneous phenomena, such as "explanation", "understanding", "experiment", that have not been eliminated after we realized that a more pluralist approach to these notions will be the right one.[12] The difference between "models" and these other notions is simply a vastly different degree of family resemblance. Models are more like science itself; an incredibly diverse set of things and activities, which Feyerabend rightly argued will

[12] I thank a reviewer for urging me to make the difference from these other cases explicit.

not be able to be captured in a single philosophical account. The list provided by Frigg and Hartmann (2020) already suggests that there are too many different kinds of activities going on under the term "modeling" as to allow for a neat division. The various categories appear to overlap in various ways without any 'real' dividing lines between different models. How to categorize a model appears to be a mere choice reflecting the interests of the philosopher, or for that matter, scientist. These may be helpful for offering new ways of seeing "models" and their diversity, but it is only in contrast to more monist accounts that such taxonomies constitute useful correctives.

Weisberg (2013) has attempted to remedy the problem of this wealth of distinctions by classifying all models intro three types: mathematical, computational, and concrete models, and distinguishing model practices into targetless modeling, hypothetical modeling, and generalized modeling. But even here, O'Connor and Weatherall (2016) are right to criticize that these seemingly clear-cut boundaries are not so clear after all once we take a closer look at models such as the Sakoda-Schelling model of racial segregation[13], replicator dynamics in evolutionary game theory, the Lotka-Volterra model, the Black-Scholes formula, model organisms, the double helix DNA model, and scale models such as the San Francisco Bay Delta model. Their discussion of these models in their review of Weisberg is not aimed at providing a more fine-grained analysis of models, but rather to show that it is a mistake to assume the commonalities of different models as those that are epistemically important for understanding their role and success in science:

> The problem with this is not that a broad range of models, and indeed perhaps all models anyone has ever case studied, cannot be shoehorned into a tripartite taxonomy with sufficiently many subparts. Rather, the problem is that the more one appreciates the richness of modeling practices in science—a richness Weisberg has done more than anyone to highlight—the less compelling it is to think that the philosophically and scientifically important features of models are the ones they have in common. The term "modeling," much like the term "science," picks out a set of practices that do not constitute any sort of natural category. For this reason, studying models in science at the level of generality and abstraction attempted here is not just herculean but quixotic. (O'Connor and Weatherall, 2016, p. 614)

In effect, O'Connor and Weatherall make here a claim about model-based science, similar to how Feyerabend criticized the philosophy of science for treating "science" as a unified phenomenon, rather than an incredibly pluralistic endeavour that resists simple categorization. To be a model anarchist is to resist the motivation to re-focus the philosophy of science towards models while keeping the older ambition to derive general accounts of science.

Above, Khosrowi (2020) justified the motivation to build taxonomies of models in virtue of the ability to single out important epistemic commonalities of tokens that will enable us to distinguish different model types and understand which features make them epistemically successful. But this cannot be justification *enough*, as we are able to find some epistemic commonalities between almost any 'thing' used in science. Indeed, such a re-

[13] Usually the model is referred to as the checkerboard model or Schelling model after Schelling (1971), but Hegselmann (2017) elegantly shows that James Sakoda (1971) was a victim of the Matthew effect, deserving at least equal credit and recognition for his earlier development of the idea.

sponse is hardly worth mentioning in response to eliminativistist as opposed to pluralist views on any concept. What drives eliminativist debates about concepts is typically not the absence of any similarities —these are too easy to come by— but the presence of important differences. The question must be whether this warrants the delineation of a type, which will inevitably force us to shoehorn very different "models" into a category and disregard the many things that make them distinct. If it is their differences, rather than their commonalities, that explain their scientific success then an abstraction away from these features would positively move us away from a better understanding of science. In drawing arbitrary boundaries between some shared properties we neglect what is most important, i.e. the context of particular models.

Unfortunately, this danger has been given little consideration. Yet, where if not for the most heterogeneous kind of entity and practice within science (or possibly anywhere) should we take this danger seriously? Philosophers of models will continue to come up with more distinctions and more fine-grained typologies, but for however many distinctions we could come up with, they will not exhaust the option space for important epistemic differences between models.

One way critics may respond to my proposal of model anarchism is to point out that the MMM literature makes definite progress. I do not deny this - in fact, I think that it supports my thesis. What is seen as progress in the literature towards greater pluralism and more fine-grained taxonomies really turns out to be a mere recognition that we have discarded important differences that have come to be idealized away in the quest for general, or at least mid-level accounts, of models. As model anarchists we can accept that there is progress without thereby refuting the anarchist stance. The epistemic progress in the field stems precisely from a move towards anarchism: a recognition that it is the particular models we must focus on and the context in which they are used. Nothing of importance hangs upon the trivially true fact that they share some commonalities with other "models", when it is their differences that bear out epistemic success in extremely heterogeneous kinds of contexts. This brings me to the final point: what's the harm in theorizing (in a general manner) about "models"?

### 4.2. Why we should abandon the philosophy of "models"

One excellent objection to my criticism is to ask why we should abandon the philosophy of "models". After all, even if what I have argued has a high probability of being true, philosophers have often examined positions, such as panpsychism, that might seem pointless to others in the field that view these positions as absurd. There are several responses to this objection.

Firstly, while I do indeed think that a lot of work in the MMM literature is guilty of working on a mistaken assumption, I do not believe that this article will be the end of the more generalist work in the field, any more so than the work of Daniel Dennett has meant the end of philosophical defenses of dualism. Secondly, while I am convinced of model anarchism that of course does not mean that I do not welcome criticism of my view. Indeed, I would cherish any attempt that manages to convince me that my criticism has gone too far. Nevertheless, as I shall argue in the last section of this paper, I hope that the MMM literature will undergo a significant shift that abandons the working assumption that there are important epistemic and ontological commonalities between the various things and

practices called "models" and "modeling". Instead, much more effort should be spent on the analysis of particular case studies, without attempting to generalize it to some general category such as say "exploratory models". The remaining general work within the field should make much harder efforts to demonstrate that these exist, rather than just present one generalist account as being more reasonable than another, which would require a very different kind of bar for publication to be passed.

Finally, I hold that there is an argument for the abandonment of the very generalist work within the field that distinguishes it from arguments against other philosophically controversial views. That is, as I have mentioned in the introduction, that this work has an inherent danger to misrepresent science and put scientific straightjackets onto scientists. Model anarchism is thus not just a call to abandon a research project because it is deemed to rest on a mistaken assumption, but more importantly that philosophers of science working in the field are in the dangerous position to hinder or promote scientific progress.

When we abstract away from the important epistemic features of different scientific instances of what is sometimes labelled with the term "model" to misleadingly attribute the epistemic success of the various things called "models" to whatever they may allegedly share, we will inevitably offer scientists epistemic straightjackets instead of a huge palette of different kinds of "tools" or "activities" they can use for their work. Worse, as philosophers "of models" at the other end of the general taxonomy to case-study continuum have long argued, the generalist work has neglected the importance of context and the widely varying goals of "modeling" activities and scientific research at large (see also Veit & Browning, forthcoming). Making scientists put less emphasis on these factors that are the real drivers of epistemic success to focus on more general ones, such as say accurate representations, that are meant to unite all theoretical activities captured the umbrella of "modeling" could inevitably diminish the toolkit of science.

One might here, of course, object that philosophers despite their hopes to arrive at normative conclusions and guidelines for sciences typically do not influence how scientists do their work. But I have not infrequently encountered modelers at "philosophy of models" conferences that became convinced that their fields have become too promiscuous in the "tools" and "activities" they employ, looking for normative guidance on how to restrict these methods. Here, however, I see a danger that the true driver of scientific progress, i.e. the diversity of scientific methods and creativity of scientists, comes to be neglected. After all, much work in contemporary philosophy of science explicitly tries to exorcise strongly Popperian thinking out of the minds of scientists who treat the philosophy of science as if it had been fixed once and for all in the twentieth century. Furthermore, even if most work within the philosophy of science will never influence scientists, our goal should remain to develop a better understanding of science and its methods that could at least in principle help scientists to do better science, rather than constrain them. Even a small risk for such an effect should thus be taken seriously.

On the other end, some may object that anarchism allows an "anything goes" mentality, but really it is a call to pay attention to the detail, diversity, and creativity of the work going on that gets to be called "modeling", which I fear comes to be neglected in attempts to develop philosophical accounts of different types of models. As I see it, philosophers of models ought not to be in the game of constraining the work of "modelers", but rather to embody the Feyerabendian spirit of uncovering new and diverse ways in which science can

be pursued. In the absence of a general account of models, whether representationalist or other, we should simply embrace the (perhaps philosophically unwelcome) conclusion that MMM share too little family resemblance as to function as a useful investigation at the level of abstraction typical to philosophical investigations. Let me thus end this section with a quote from the book review of O'Connor and Weatherall that summarizes this sentiment and inspired me to write this programmatic paper to begin with:

> [U]ltimately, "scientific models" is simply not a fruitful unit of analysis, at the epistemic level or any other. To work at this level of abstraction forces one to group together models so dissimilar in terms of their structure, their function, their interpretation, their role in practice, and so on, that one is left either making claims that cannot really apply to everything in the category or else with generalities that reveal very little. (O'Connor and Weatherall, 2016, p. 624)

## 5. *Conclusion and further discussion*

The goal of this programmatic paper was to articulate my dissatisfaction with the MMM literature. The philosophy of models has been plagued by overly ambitious attempts to abstract away from myriad features of models to develop general accounts, typologies, and the like, which has distracted from the most important features of models in science, i.e. their diversity and context. There is no unified phenomenon here that would allow for a general philosophical analysis. I have no doubt that some philosophers within the field working at the case-study end of the continuum will think that this is no news to them, whereas those working on general accounts of models will strongly object to my view. However, as even the most granular work in the field often appears to have to respond to the need to justify itself in terms of generalizations across "models", I saw the need to write an article that challenges the working assumption in the MMM literature that there are unified kinds here that would allow for the general epistemic and ontological insights about "models", "modeling practices", and "model-based science".

While some may object that similar claims could be made about other philosophical debates on such notions as "experiment" or "explanation", I hope that this paper has made clear that "models" and "modeling practices" are very distinct as the most promiscuous terms used in science without any important shared commonalities (whether epistemic or ontological). While philosophers of science may likewise have been too ambitious in trying to provide very general accounts of "explanation", there are at least strong epistemic similarities between its usages across different sciences that make it tenable to speculate about the possibility of a general account or taxonomy. While different scientific fields might have different understandings of notions such as "theory", "hypothesis", and "confirmation", they have at least a sufficiently precise and widely shared understanding of them within their own disciplines. For "models" and "modeling practices", however, even this minimal agreement within disciplines, such as biology and psychology, is simply lacking. As I hope to have made clear, even philosophers within the MMM literature have recognized that these terms are used so promiscuously to cover almost any scientific object and activity.

It might even be considered surprising that despite the fact that many of those philosophers who have highlighted the great diversity of "models" and "modeling practices"

and consider themselves pluralists have been tempted to make generalizations about models that at best improve upon previous monist thinking, but at worst will be harmful to the understanding of science. In no other field within the philosophy of science do we find such a mismatch between confessed pluralism and the simultaneous ambition to draw general epistemic and ontological conclusions. In some places I have suggested that my view here may alternatively be considered a sort of "model eliminativism", since I believe that a strong case could be made in favor of the abolishment of the concept of "models" as a unified scientific phenomenon within the philosophy of science. Yet, I have not used this alternative title, precisely because eliminativism is so often misunderstood. The claim I am defending isn't that models such as the Sakoda-Schelling model don't exist, but rather that there is no unified phenomenon here, no typology to be drawn that captures important categories in science. The apparent unity of "models" is a sociological artefact of the way the term is used in science, not reflective of some deep epistemic commonalities.

The very promiscuity of these terms should make us doubt that there is a unified phenomenon in place only waiting to undergo philosophical analysis. Perhaps one must recognize that these terms are nothing more than attractive shorthands for scientists to use in order to avoid saying: "we have developed this scientific tool, construct, theory, equation, and what not [replace all for: model] which provides us with valuable scientific insights" or "we have done 'something' [replace: modeling] that provides us with scientific insights." The mere usage of these terms does not imply that they are picking out epistemically insightful properties; their extension is too heterogeneous and may well reflect non-epistemic goals. In this regard, my analysis is more sociological than philosophical, and resists the Lakatosian ideal to prioritize a 'rational reconstruction' over a psychological-sociological explanation. I am providing an alternative kind of explanation for why these terms are found in science without referring to the epistemic properties of models that would help us to better understand the progress of science.

If I convince participants in the field to more strongly resist grand unifying accounts of models and instead make them move closer to providing much more fine-grained and context-sensitive analyses of the various scientific instances of things called "models" and practices called "modeling" in science, than my model anarchism will have been successful. Just as there remain only few of the kinds of philosophers of science that Feyerabend originally meant to address, so do I hope that model anarchism will help to establish a healthy skepticism of over-generalization and a demand to appreciate the extreme plurality of ways science is conducted. Here I strongly resist the common picture of the philosopher of science as a critic of scientific practice. We ought to remove straight-jackets, not make them tighter. We are looking in the wrong place by comparing a particular model to what we consider to be an "ideal" instance of a model, or for that matter why a "failed" model is bad one. It is in the context of scientific work and not the membership of a heterogeneous class of scientific tools and practices called "models" that the real explanatory work is being done. No useful prescriptions can be drawn for the practice of science by the mere declaration that *something* is a model alone. Just as Feyerabend maintained that there are no general useful rules to be discovered about the method of science, I have here argued that the same may apply to model-based science. Finally, I hope there is some truth in the analysis I have offered, and that model anarchism will provide a useful corrective to the normative-theoretical ambitions of much of the field.

*Acknowledgements*

I would like to thank Peter Godfrey-Smith, Maureen O'Malley, Samir Okasha, Heather Browning, and the reviewers of this paper for their helpful comments. I am also thankful for audience feedback I received in Bristol and Bath. Apologies to anyone I forgot to mention. This paper is part of a project that has received funding from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (Grant No. 101018533).

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