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# Model Anarchism

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## Abstract

This paper constitutes a radical departure from the existing 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 diverse, too context-sensitive, and serve too many scientific purposes and roles, as to allow for a general philosophical analysis. From this recognition an alternative view emerges that I shall dub *model anarchism*.

**Keywords:** models, modeling, model-based science, model pluralism, context, Feyerabend, scientific practice

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# 1 Introduction

The last 40 years in the philosophy of science demonstrated a radical shift towards the philosophical study of models, modeling-practices, and model-based science (henceforth abbreviated as MMM), and away from laws and theories. While this shift can be partially explained by Nancy Cartwright’s (1983) highly influential attack on the then orthodoxy in the philosophy of science, the ever-growing usage of the terms ‘modeling’ and ‘models’ by scientists to not only describe, but also to defend their work, plays an at least equally important role.

Indeed, it would be no stretch of the imagination to consider this shift as something akin to a ‘gold rush’ in the philosophy of science. After all, it *seemed* to many that progress in our understanding of models would be the key to unlock the answers to the many questions at the very heart of philosophy of science. While a philosophical investigation into the various things scientists call ‘models’ may not have promised any material fortune, the epistemic and reputational rewards *seemed* incredibly rich.<sup>1</sup>

These past decades have been pervaded by a sense of optimism not unlike that of gold miners in the latter part of the 19th century. Yet, even after 40 years of tremendous work of the highest intellectual calibre philosophy of science has to offer, Frigg and Hartmann (2018) conclude their Stanford Encyclopedia article on models with the resignation that “despite the fact that [models] have generated considerable interest among philosophers, there remain significant lacunas in our understanding of what models are and of how they work”. While the optimism within the literature appears to be unwavering, there has always been some more or less silent dissatisfaction with the literature at large among those philosophers who have been called ‘modelers’ themselves (both self-proclaimed and by others within and outside of philosophy).<sup>2</sup> The aim of this short article is to articulate this dissatisfaction.

The culprit, I suggest, is an underlying assumption within this philosophical literature that has rarely been challenged: that is, the very idea there is anything like a *natural kind* to the various things called ‘models’ and ‘modeling’. Here, I argue that this assumption, and the optimism that ac-

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<sup>1</sup>This is not to deny that there have been philosophical discussions of models for over a century, see for instance Boltzmann’s 1902 Encyclopaedia Britannica entry “Models”, later reprinted as “Model” (1974) in a collection of his essays.

<sup>2</sup>From personal conversation with Brian Skyrms, Cailin O’Connor, and Rainer Hegselmann.

companies it, is fundamentally misguided. The view that emerges from this recognition constitutes a radical departure in a literature that has attempted to carve out different kinds or types of models and modeling practices and provide general taxonomies of a ‘unified practice’. From this departure an alternative view emerges that I shall dub *model anarchism* in reference to Feyerabend (1975). The negative formulation of this view can be summarized as follows: ‘models’, ‘modeling-practices’, and ‘model-based science’ are too diverse, too context-sensitive, and serve too many scientific purposes and roles, as to allow for a general philosophical analysis.

Let me now sketch the organization of this article. For my argument to succeed, I first need to illustrate that there is an unquestioned assumption in the literature that has not received sufficient attention. Section 2 is intended to expose this unquestioned assumption. Secondly, I will need to show that this assumption is a problematic one in the face of apparent progress in the literature. This will be the target of Section 3. Lastly, I will sketch a future for a philosophical literature on MMM that rids itself from this problematic assumption and conclude the discussion in Section 4.

## 2 An unquestioned assumption

The most elegant way of introducing this ‘anarchist’ account is by exposing the *unquestioned assumption* that lies at the heart of the optimism in the literature I am attacking here. One of the most influential papers in this literature, more so, in recent philosophy of science itself, is Peter Godfrey-Smith’s (2006) “The strategy of model-based science”. Indeed, it is Godfrey-Smith’s second most cited paper<sup>3</sup> – impacting an entire generation of philosophers of science, most notably Michael Weisberg (2013). The model anarchism account presented here denies that there is anything like *THE* strategy of model-based science. Unfortunately - and in spite of his recognition of the fact that hardly any term has been more contested in the philosophy of science than the term ‘model’ - Godfrey-Smith (2006) literally states that the search for a “[‘]natural kind[’] within the larger domain of theoretical science, is a guiding idea of [his] paper” (p. 729). In other words: the assumption that there is something like a unified kind or practice comes first and the philosophical investigation second. As I shall demonstrate, this talk of *natural*

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<sup>3</sup>Cf. Godfrey-Smith’s Google Scholar page [accessed on 26th of February 2020] [https://scholar.google.com/citations?hl=en&user=Lu\\_qQp0AAAAJ&view\\_op=list\\_works](https://scholar.google.com/citations?hl=en&user=Lu_qQp0AAAAJ&view_op=list_works).

*kinds* is an unfortunate choice, for it suggests that there is a sort of ‘essence’ to modeling – a word choice that Godfrey-Smith later came to regret, indeed found himself surprised to have used.<sup>4</sup>

Even if models “usually *seem* to be hypothetical objects of some kind” (2006, 727) [italics added for emphasis], it is one step too far to call such an idealized account of modeling *THE* strategy of model-based science. Here, we should heed Godfrey-Smith’s own warning that “[o]ne of the hazards of philosophy is the temptation to come up with theories that are too broad and sweeping” (2003, 5). One may justifiably see *model anarchism* as a careful warning against just this sort of practice within the MMM literature.

What then motivates this tantalizing step towards broad accounts that attempt to carve the various things called ‘models’ and ‘modeling’ into *taxonomies* and different *types* of models and modeling practices? As Godfrey-Smith illustrates, the temptation lies in the observation that widely different entities and practices *seem* to be unified, to the extent that the alternative is dismissed outright. The fervent optimism within the literature suggests that many philosophers of science appear to be under the impression that once we figure out what ‘models’ are and how they work, we can finally get a handle on all the other pressing questions in the philosophy of science. Indeed, the working hypothesis of many participants in the literature seems to be that the incredible variety of things scientists call ‘models’ or ‘modeling’ offers enough shared features to allow for an epistemically useful carving into different ‘types’ or ‘kinds’ of models. Consider Khosrowi’s (2020) hand-wavy dismissal<sup>5</sup> of what could be considered a challenge to the foundations of the MMM literature:

“I 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

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<sup>4</sup>Godfrey-Smith’s regret [from personal conversation] is unsurprising, for it seems to conflict with his own anti-essentialism. Dennett (2011) offers a similar criticism in a review of Godfrey-Smith’s later work.

<sup>5</sup>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.

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, 540) [italics and bold type added for emphasis]

This excerpt nicely illustrates the essentialist ambitions philosophers are easily attracted to. 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. But *seemings* do not have to track truth. In fact, the history of science and philosophy suggests that such pre-conceived, indeed, *a priori* notions are shown to be wrong time and time again. We should therefore at least stop for a moment and carefully consider whether the underlying assumption in the MMM literature is well-founded.

Though the intended target of Khosrowi’s criticism is Paul Teller’s (2001) contextual view of models, Teller denied that such radical conclusions follow from his view. Despite his strict opposition to the essentialism found in the MMM literature, Teller holds that the lack of strict necessary and sufficient conditions does not undermine the motivation to classify the multiplicity of models.<sup>6</sup> Khosrowi’s target, however, is not entirely misplaced. There is an alternative position that challenges Khosrowi’s assertions and the received view in the MMM literature at large. It is only that it has not yet been articulated. This omission shall be remedied here with an account I shall dub *model anarchism*.

### 3 A problematic assumption

It is one thing to show that there is a widely held belief among participants in a particular debate or discipline. It is quite another to proclaim that the dominant - or stronger, almost universally held - assumption is mistaken. Why then is this essentialist thinking about models problematic? The justification for this assertion is a critique of the MMM literature at large.

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<sup>6</sup>From personal conversation.

The arguments against this assumption are twofold. Firstly, this section defends the idea that the various entities and practices called ‘models’ and ‘modeling’ are too diverse and disunified to constitute a useful target of general analysis. Instead, we should take seriously the possibility that the scientific phenomenon of the rise of terms such as ‘models’ and ‘modeling’ is a sociological and linguistic development, rather than an epistemic one. The second argument is more general in character and draws on anti-essentialist arguments in philosophy at large. As I shall show, the MMM literature cannot escape the force of these arguments.

### 3.1 ‘Models’, ‘Modeling’, and ‘Model-Based Science’

The idea that the entities and practices scientists call ‘models’ and ‘modeling’ are incredibly diverse is not a novel one. 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 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 Weisberg 2013; Gelfert 2016; Potochnik 2017; Veit 2019b, 2020).

Take for instance Gelfert (2016), who suggests that 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. This is right, of course, 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. 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’s (2013) equally pluralist monograph *Simulation and Similarity*: “[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” (614).

This is a strong assertion that does not seem to square well with the observation that the philosophy of science has undergone tremendous progress since its shift from laws and theories towards models as its primary target of analysis. If I were to deny this progress, one might be justifiably tempted to stop reading here. Let me therefore attempt to shift the evidential burden to those who dismiss *model anarchism* without argument.

Firstly, *model anarchism* is not at odds with progress in our understanding of science, not even progress in our understanding of models. The very reasons Weisberg (2013), Gelfert (2016), Potochnik (2017), and Veit (2020) cite for more pluralist and fine-grained accounts in the MMM literature should give us reason to doubt that philosophical analysis at this level is a useful target to begin with. Instead, we may have to recognize that there is nothing at this level of generality that could serve useful epistemic roles. Once, we take seriously the challenge of Weisberg (2013) and Potochnik (2017) to look at actual scientific modeling practices, there is less and less reason to think that philosophical investigations into MMM are going to yield epistemically useful and general accounts or taxonomies. Recast in the light of the alternative account provided here, the progress in the literature is entirely explained as a move towards anarchism and scientific practice. Now, one may want to hold, as does Khosrowi (2020), that we merely need to go more fine-grained – but this is only a reasonable position to hold if the underlying assumption in the literature is an appropriate one.

Consider the following assertions by Godfrey-Smith and Weisberg in which they, I think prematurely, assume that there is something distinctive and unified to the entities and practices called ‘models’ and ‘modeling’. Early on in his paper, Godfrey-Smith (2006) asserts that “[s]howing that there is *something* real and distinctive in that location is easy to do” (729). Similarly, Weisberg (2013) recognizes while that his unified account “may not always be simple and tidy” he believes “that it can help us make sense of this important and distinctive theoretical practice” (175). Model anarchism is importantly not the denial that we can make sense of the various entities and practices called ‘models’ and ‘modeling’, but it is the careful warning that we should not become overly optimistic and postulate allegedly distinctive epistemic types and categories.

Godfrey-Smith (2006) argues that the sociological facts of assessment, training, and job descriptions for ‘modelers’, distinctive languages and different disciplinary norms among ‘modeling communities’ should be enough to maintain that there is *something* real. He even recognizes but quickly dismisses the potential charge that this ‘category’ might be an “epistemically shallow one” (730). O’Connor and Weatherall (2016) raise the problem with this type of argument forcefully in their review of Weisberg (2013), suggesting that though it “is a sociolinguistic”, and I may add, historical, “fact that scientists tend to use the word [‘model’] often [...] one cannot infer from this that there is a natural activity or category of practice that the term

tracks” (626). Nelson Goodman (1976) may have been the first to anticipate this futility of trying to provide a unified account of models and modeling practices:

“Few terms are used in popular and scientific discourse more promiscuously than [‘]model[’]. A model is something to be admired or emulated, a pattern, a case in point, a type, a prototype, a specimen, a mock-up, a mathematical description – almost anything from a naked blonde to a quadratic equation – and may bear to what it models almost any relation of symbolization.” – Goodman (1976, 171)

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.” If one replaces the words ‘model’ and ‘modeling’ with the sketched alternatives provided here, these statements appear to lose credibility; if not among their peers then at least among the public. In this regard, my analysis is more sociological than philosophical.

Given the incredible diversity and richness of models and modeling practices, we may wonder why no one has provided an account that denies the underlying assumption in the MMM literature. The explanation I suggest is itself a sociological one. Unlike many disputes between historians, sociologists, and philosophers of science, there has been very little work on the history and the sociology of models.<sup>7</sup> While many debates in the recent philosophy of science have developed more sympathy for the tools and insights of historians and sociologists of science, there has not been any equivalent corrective to the MMM literature. Philosophy, after all, has long been conceived as the study of the general and abstract – attempting to find unification and cleanliness in the messiness that is the real world. Some potential criticism towards my model anarchism account may come from those who are opposed to merging the boundaries between philosophy and sociology: “Why should philosophers do the sociologists’ job?”

I do not find such a response satisfying. There are numerous reasons to be quite hesitant in equating philosophy of science with a certain methodology or set of methods. While we may allow a certain degree of division of labour in the specialization and use of our methods, I deem it more fruitful

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<sup>7</sup>One rare example is found in Schichl (2004).



to distinguish philosophy by the more complex, difficult, or abstract questions it seeks to answer. If the philosophical question we are interested in, e.g. “what is the ‘essence’ or ‘natural kind’ of models, modeling, and model-based science?”, would benefit from drawing on the tools of other sciences, we should be happy to do so. It is an unfortunate fact that there is very little sociological work on the rise of the words ‘model’ and ‘modeling’, but just as work in the sociology and history of science undermined the idea that there is a unified set of tools and practices to ‘science’, so too should we take seriously the possibility that ‘models’ simply do not constitute an appropriate target for philosophical analysis at this level of grain. This leads us to a more general philosophical debate, that is *the problem with essentialism*.

## 3.2 The Problem with Essentialism

Essentialism is at the very heart of philosophy and this very reason constitutes the primary problem with it. The nature of philosophy itself – the attempt to carve nature and provide clear definitions – often led philosophers on wild goose chases for definitions that could not be obtained. Rather than recognizing the problem with essentialism itself, philosophers have progressively weakened their demands for essentialist definitions. Dennett (2016) recently provided an elegant summary of the history of philosophy that strikingly emphasizes this point:

“Ever since Socrates pioneered the demand to know what all Fs have in common, in virtue of which they are Fs, the ideal of clear, sharp boundaries has been one of the founding principles of philosophy. Plato’s *forms* begat Aristotle’s *essences*, which begat a host of ways of asking for *necessary and sufficient conditions*, which begat *natural kinds*, which begat *difference-makers* and other ways of tidying up the borders of all the sets of things in the world.” – Dennett (2016, 9)

For Dennett this is intended to signal the overdue demise of essentialism<sup>8</sup> - a mistake philosophers have committed since the origin of our own discipline. Teller is right, of course, when he denies that the lack of strict necessary and sufficient conditions does not directly undermine all attempts to classify entities, as is nicely illustrated by the various and apparently conflicting classification schemes biologists have developed. The justification for these classifications, however, needs to come from the roles such classifications

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<sup>8</sup>Hence the title of his paper.

come to hold in science and this needs to be demonstrated and not merely asserted. *Prima facie*, any attempt to classify some set of entities may *seem* useful, but the allure of this essentialist thinking should be met with more resistance.

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 I see able to accommodate this demand and diversity of models would categorize something as a model whenever *something* is used by a scientist for a scientific purpose.<sup>9</sup> This does not *seem* satisfying precisely because the words ‘model’, ‘modeling’ and ‘modeler’ are held with esteem within science. Yet, this is the only real essence to be had. A definition that leaves out any information regarding the epistemic properties that make models successful.

Weisberg (2013) raised this complaint against what he considers to be deflationary accounts of ‘models’ and ‘modeling’ more generally. Criticizing an early version of Levy (2015) he observes that “[n]ot only does Levy’s view present a deflationary account of the nature of models, it also seems to present a deflationary account of the practice of modeling” none of which might be considering damning on its own, but Weisberg insists that “[f]ar from explaining the special uses to which models can be put, Levy says that there aren’t any models at all” (64). What is it then? Is everything a model or nothing? For the purposes of finding a philosophical essence to models these statements are equivalent. It becomes an epistemically useless category. Model anarchism might now sound like a sort of *model nihilism*. This is not my intention, for the term signals a sense of gloom and distrust. Model anarchism could not be further from that, but I will have to elaborate my account a bit more in order to bring this point home.

## 4 A future without this assumption

If there is no essence, no natural kind, no real borders, no taxonomies, nor types of models – then what should participants in the MMM literature do? Is model anarchism the dismissal of an entire literature? It is not. But it signals a radical shift to what has come before. First of all, I remain open to

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<sup>9</sup>Similarly, O’Connor and Weatherall (2016) argue that since Weisberg leaves his concept of structure undefined “[o]ne might as well have an account according to which a model is a [‘]thing[’] and leave it at that” (625).

be convinced that advances in our understanding of MMM can be gained by grouping, for instance, all ‘mathematical’ models together into a ‘type’. But I highly doubt this is likely to yield epistemic insights given their diversity of purposes and roles within science. I do not mean to imply philosophical analysis of MMM is impossible, but rather that a particular (i.e. essentialist) way of thinking about models is misguided. What is needed is an alternative way of thinking about models. Luckily we can start here with precisely the same words Weisberg (2013) uses to set out the goal of his monograph:

“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. Thus the accounts described in this book are themselves models of modeling.”  
– Weisberg (2013, 6)

*Models of modeling.* An elegant choice of words Weisberg unfortunately does not mention more than once in his entire book – worse, seems to ignore precisely when falling prey to the essentialist lure for a unified account. What one might have expected from the title of this paper is interestingly also found in the introduction of Weisberg: a reference to Paul Feyerabend. The key to Feyerabend (1975) is his commitment to diversity and pluralism in science – and the creativity and imagination of individual scientists. These are properties the model anarchism account is intended to capture. Let me therefore now sketch the ingredients of what I take to be a future without essentialism about models:

Firstly, (i) the various entities and practices called ‘models’ and ‘modeling’ are too diverse and disunified to constitute a useful target of general analysis. Secondly, (ii) due to this lack of general taxonomies and types of models/modeling-practices there cannot be any general epistemic evaluation of models within science. That there is no general answer may not be the end of the world. Perhaps more threatening is the third ingredient (iii) that even with a specified context and all the purposes of a model there is going to be a degree of underdetermination between an epistemically successful model and the target that was intended to be explained.<sup>10</sup> Teller (2001) is under the impression that the context is just going to tell us which features

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<sup>10</sup>Cf. models in animal welfare science (Browning 2020), evolutionary biology (Veit 2019a) and autism research (Chapman and Veit 2020).

lead to epistemic success. This picture is not realistic. Therefore, and this is our fourth ingredient, (iv) scientists must be creative and make use of their imagination to bridge this epistemic gap of uncertainty – models are often ingeniously created and thrown at the world, rather than meticulously created for a highly specific purpose such as in, say, the design of Apollo 11. Looking at the history and context of a model is simply *not* going to tell us all the epistemic features that made a particular model successful. Less so, enable us to postulate an *ideal type* of a model that can be used to evaluate and help to construct others. In Veit (2020) I have raised a similar point, criticizing the MMM literature for treating the Sakoda-Schelling model of racial segregation<sup>11</sup> as a representative for an *ideal type* of a model, rather than a particular successful token instance of a model. Lastly, (v) model anarchism is importantly not the denial that categories *cannot* be drawn, but rather that such general carvings constitute *epistemically* useful ones. No one denies that we can group together all blue cars. Yet, it needs to be shown that this is a useful epistemic category. And while this particular category might indeed be epistemically useful under very special circumstances - imagine a police search for a hit-and-run driver in a blue vehicle - it is here the very context and special circumstances that make it a useful epistemic carving. The MMM literature got it backwards – assuming in advance what has to be shown.

Model anarchism allows us to recast the goal of this literature as an assembly of case studies, resulting in a highly diverse and rich set of ‘models’ about models, modeling, and model-based science, all of which, if successful only illuminate a partial aspect of the scientific enterprise. If the sketch of the model anarchism account I have provided here is correct, philosophers are entrusted with the meaningful task of illuminating the manifold functions of ‘models’ and ‘modeling’, and keep the allure of monist and essentialist thinking about models at bay.

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<sup>11</sup>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.

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