<u>Dodging the Fundamentalist Threat (pre-print version)</u>

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1. Introduction

As demonstrated by the collection of essays in this volume (as well as Tahko (2012) and Novotny and Novak (2014)), Aristotelianism has recently been enjoying a revival in various subfields of philosophy, including metaphysics and the philosophy of science. To take an example that will play a motivating role in our discussion, the theme of "hylomorphism" has been widely discussed in analytic metaphysics (see Koons (2014) and references therein), as well as in the philosophy of quantum theory (Pruss (2017)), the philosophy of mind (Jaworski (2017)), and the philosophy of biology (Austin (2017)).

Although the term "hylomorphism" is sometimes used very loosely, Neo-Aristotelians of a strict observance will want to reserve this term for a theory of substance that avoids collapsing into either dualism or materialism. In particular, in order to avoid the latter collapse, Neo-Aristotelian substances must exhibit a strong form of unity, i.e. a unity such that the powers of a substantial whole cannot be completely grounded in the powers of its parts. (On this score, see Koons (2014), who argues for the more specific thesis that there must be a mutual relationship of partial grounding between a substantial whole and its parts.) More generally, many Neo-Aristotelians are interested in the possibility of a kind of emergent causal efficacy (such as the powers of an organism) which cannot be reduced to forms of causal efficacy that are sometimes regarded as more "fundamental" (such as the powers of the electrons that comprise the organism).

While it is a truism that one cannot directly read metaphysics off of science, the above Neo-Aristotelian position can still be threatened by weaker claims about the relationship between science and metaphysics. Consider for instance the metaphysical thesis called "fundamentalism", which holds that the laws of a (hypothetical) unified physical theory exhaustively govern all of material reality: on this view, it is difficult to see how the powers of substances could be anything other than entirely grounded in the powers of the entities of the unified physical theory; hence, this metaphysical picture is inconsistent with Aristotelian hylomorphism. But why should anyone believe in such a metaphysical thesis? It is at this juncture that many thinkers implicitly or explicitly invoke a connection with scientific practice:

> **Fundamentalist Unification**: The success of science (especially fundamental physics) at providing a unifying explanation for phenomena in disparate domains is good evidence for fundamentalism.

Thus, although Neo-Aristotelians are not directly threatened by anything in science, they will want to find ways of warding off this interpretation of the "unifying role" that physics plays with respect to disparate domains of phenomena.

The goal of this essay is to recommend a particular set of resources to Neo-Aristotelians for resisting Fundamentalist Unification, and thus for resisting fundamentalism. The set of resources in question originates in the work of Nancy Cartwright, who has famously drawn on the details of scientific practice in order to launch an argument against fundamentalism. We would like to urge two points in particular:

(i) Anti-fundamentalism is a live option, because genuine arguments in favor of Fundamentalist Unification are hard to come by, and the best (and most fully worked-out) argument for it rests on assumptions that beg the question against Cartwright's epistemology of scientific models.

(ii) Neo-Aristotelians should find Cartwright's epistemology of scientific models appealing because it adopts the broadly Aristotelian approach of prioritizing the concrete over the abstract—call this approach "concretism".

Bringing these two points together, we submit that Cartwright's approach offers Neo-Aristotelians a distinctively concretist epistemology of scientific models that has the added benefit of making room for robust forms of Aristotelian metaphysical doctrines such as hylomorphism. Nonetheless, we will also urge that Cartwright's approach is in many ways underdeveloped, and that it needs to be more fully worked-out if it is to be incorporated into a compelling Neo-Aristotelian picture of the relationship between metaphysics and science.

The plan of the essay is as follows. Section 2 discusses the topic of what scientific practice-based reasons one might marshall in favor of fundamentalism. As we see it (and as the question has been understood in the literature), any practice-based attempt to adjudicate this issue has to reckon with two considerations that seem to pull in opposite directions. On the one hand, even our most impressive scientific theories seem to only apply to domains of reality in a "patchwork" way; and on the other hand, it is undeniable that much work scientific work consists in devising theoretical structures that are in *some sense* "unifying". Intuitively, the first consideration provides prima facie evidence against fundamentalism, and the second consideration provides prima facie evidence in favor of it. Thus, a successful argument for Fundamentalist Unification needs to provide a convincing account of "theoretical unification" that explains away the appearance of theories only having a patchwork application to reality. One such account that claims to be "practice-based" is that of Smith (2001), and we will consider his argument in favor of Fundamentalist Unification in this section.

In Section 3, we will argue that Smith's account of theoretical unification turns on a specific and controversial epistemology of scientific models. We then highlight how Cartwright's concretist account of the epistemology of models explicitly rejects Smith's assumptions, and leads to a different way of understanding substantive theoretical unification. Indeed, this rival epistemology forms the basis of Cartwright's famous argument against fundamentalism, which we then discuss.

Section 4 draws on our previous discussion to sketch some general guidelines for the project of developing a concretist epistemology of models. Although we take inspiration from Cartwright's anti-fundamentalist morals, we also highlight some ways in which the approach that we recommend diverges from hers.

2. Fundamentalism and its justification

Metaphysical fundamentalists believe that the universe is exhaustively governed by a limited set of principles, which are often called "fundamental laws of nature". The popularity of metaphysical fundamentalism presumably derives from the metaphysical hope—shared by various scientists and philosophers alike—that science will eventually discover the laws of nature that *exhaustively govern all of material reality*, from the causal agents implicated in quantum gravity to human and non-human organisms. These fundamental laws of nature are usually taken to be truths expressed in mathematical language, which accurately describe the behavior of all things in the world, at all times and places. However, they are typically not taken to be the actual laws of our most fundamental physical theories, but the laws of some future "Final Science" or "True Physics" that our current scientific efforts aim at (cf. Sklar (2003): Sec. 5; Hoefer (2010): 308).

In this paper, we will put aside armchair metaphysical speculation and instead focus on the following question: Based on the *practice of science*, what reasons might one have for accepting or rejecting metaphysical fundamentalism? By our lights, all parties to the debate will have to reckon with two practice-based facts that appear to be in tension with each other:

1. Scientific theories have the appearance of having "patchwork" domains of application
2. Theories have been successful at providing "unifications" of such domains

Regarding (1), both fundamentalists and anti-fundamentalists should agree that our explanatory and predictive practices in science suggest a much more "dappled" or "patchwork" picture of scientific activity than what the fundamentalist hopes for. For instance, biology and chemistry are scientific disciplines which seem to operate autonomously from physics: they often construct theories, give explanations and make successful predictions without taking into account any fundamental laws of physics. Furthermore, fundamentalists should concede that even upon restricting scientific activity to "physics", it often appears to be the case that different domains of phenomena are described by different physical theories: to give an elementary example, point particle mechanics and fluid dynamics are physical theories that apply to relatively disjoint sets of classical phenomena.

With respect to (2), fundamentalists and anti-fundamentalists should likewise agree that various theories have had empirical and theoretical success in playing a "unifying role" with respect to phenomena in different domains. The issue at stake is how such "unifications" should be understood, and what are we justified in inferring from them.

Let us briefly consider a fundamentalist narrative that emphasizes a particular understanding of (2) and uses this to explain away (1). Suppose that fundamentalists and anti-fundamentalists agree that, at least within the confines of certain experimental scenarios, we have good reason to believe in the truth of mathematical laws describing the behavior of basic kinds of particles/fields and their interactions. One fundamentalist strategy for describing the unifying role of particle physics is to then elaborate on the narrative as follows: We also have good reason to believe that everything in the physical world is made up of these same basic kinds of particles. So, from the fact that everything is made up of the same basic particles and that we have reliable knowledge of the behavior of this particles under some experimental conditions, it is plausible to infer that the mathematical laws governing these basic kinds of particles within the restricted experimental settings also govern the particles everywhere else, thereby governing everything everywhere (Hoefer (2010): 317-18). A fundamentalist of this stripe would resist claims that the "patchwork" picture of science constitutes *prima facie* evidence in favor of anti-fundamentalism by denying that the patchwork picture carves at the joints of reality. Thus, for instance, Sklar claims that although explanations in biology and chemistry describe real phenomena in the world and are certainly useful for predictive purposes, they are not characterizing how things "really are" (Sklar (2003): Sec. 4).

From the anti-fundamentalist's perspective, the problem with this assertion is that it takes fundamentalism for granted, and then uses it to dismiss the *prima facie* evidence for anti-fundamentalism, viz. (1). If fundamentalists want to provide non-question-begging arguments for their position, they cannot assume that fundamentalism is true, since that would just be a statement of their (metaphysical) faith in fundamentalism. Instead, what fundamentalists should do is to provide *evidence* in the form of an analysis of scientific practice that supports (2) and dispels the worries raised by (1). In other words, what is primarily at stake in the debate is whether Fundamentalist Unification is true.

Smith (2001) essays to provide just such an account, albeit one that is limited to the domain of classical phenomena, i.e. systems for which the units of action are large in comparison with Planck's constant, and whose speeds are small relative to the speed of light. He focuses on the question of whether classical phenomena are theoretically unified by a single theory, viz. Classical Continuum Mechanics (CM). Although the focus on the case of classical mechanics might be seen as incurring a loss of generality, this is a particularly relevant case study for the debate about Fundamentalist Unification for two reasons. First, it focuses on well-established scientific theories that have been at the epicenter of this debate from the beginning (the origin of the debate can be traced back to Cartwright (1994)). And, second, this case does not involve inter-level relations or relations among different sciences, but focuses exclusively on a particular set of physical theories for which the length/energy scale is held fixed. Accordingly, this is arguably the case in which it is *easiest* for the fundamentalist to demonstrate the unity of the domain of classical mechanics, because there are no complicating factors such as complex inter-level relations, or a lack of well-established scientific theories that operate in that domain. Conversely, if we can show that the fundamentalist fails to even make it plausible that the domain of classical physics is unified, this would provide a strong case for the plausibility of anti-fundamentalism.

In constructing his argument, Smith draws on the work of Clifford Truesdell (1991), who sought to place Continuum Mechanics (CM) on a deductive foundation akin to that of Euclid's deductive axioms for geometry. Recall that in the theory of fluids and continuous media, "constitutive equations" are used to describe the response of a particular material to external stimuli—in other words, such equations provide a formal specification of some particular material. Truesdell aspired to provide a general theory of the constraints that should be satisfied by "physically reasonable" constitutive equations such as "determinism", "local action" and various invariance principles; let us call these the "Truesdell Constraints". With that in mind, we are now in a position to lay out Smith's argument in favor of the following classical version of the Fundamentalist Unification thesis:

> **Classical Fundamentalist Unification (CFU)**: The unificatory success of classical continuum mechanics demonstrates the truth of fundamentalism about

the classical domain, viz. the laws of continuum mechanics govern the behavior of all phenomena within the classical domain.

Smith's Fundamentalist Argument for CFU:

(F1) A theory only applies to a domain insofar as it provides a principled way of generating a set of models that are jointly able to describe all the phenomena in that domain.

(F2) The theory of Continuum Mechanics **(CM)** admits of an infinite number of constitutive equations (or *formal* models of materials).

(F3) The admissible constitutive equations of (F2) satisfy a small number of simple laws, viz. the Truesdell Constraints, which ensure that they obey some set of "physically reasonable" axioms.

(F4) By (F2) and (F3), CM has an infinite number of principled constitutive equations/models.

(F5) The constitutive equations of (F2) suffice to describe almost any classical phenomenon.

(F6) By (F1), (F4) and (F5), CM applies to the whole classical domain.

(Note that although our reconstructed argument focuses only on the case of contact forces, i.e. forces acting on the surface of a body, such as the force of the wind, a completely analogous argument can be constructed to cover the case of body forces, i.e. forces that act throughout the body, such as gravitation.)

The argument's conclusion, i.e. (F6), is that CM applies to the whole domain of classical mechanics, in the sense that for any possible phenomenon in that domain, the theory has a "principled" model that accurately describes that phenomenon. Thus, the argument attempts to establish Fundamentalist Unification by providing an *interpretation* of the unifying power of continuum mechanics that allows one to deduce CFU.

Notice that (F1) defines the conditions under which we can consider a particular domain to be unified by a theory. At a schematic level, (F1) is a shared premise between fundamentalists and anti-fundamentalists; however, fundamentalist and anti-fundamentalist may disagree on the determinate interpretation of what counts as a principled model, which will in turn inform their judgments about when a domain counts as theoretically unified. In particular, (F2-4) assume a particular understanding of what it is for a theory to generate a model in a principled manner. For Smith, all that is required to generate such a principled model is to show the existence of a constitutive equation that satisfies the Truesdell Constraints (which define an infinite class of formal models for materials) (Smith (2001): 471). (F5) then asserts that such models suffice to provide a good description for "almost" any classical phenomena, thus effecting the desired "theoretical unification" of the classical domain. The next section elaborates on, and interrogates, the underlying assumptions of this particular account of theoretical unification; we will argue that once these assumptions are uncovered, Smith's argument loses whatever initial plausibility it might have had as a non-question begging defense of CFU.

Section 3: Anti-fundamentalism and Cartwright's "concretist" epistemology of models

In Section 3.1, we will first show that Smith's argument in favor of CFU assumes a particular (and controversial) epistemology of models. We then sketch a rival epistemology of models that is due to Cartwright (1999)—we call this "concretism". Section 3.2 then explains how concretism is deployed by Cartwright in her argument against CFU. The moral of this section, then, is that CFU (and Fundamentalist Unification, more generally) is extremely sensitive to one's preferred epistemology of models. Moreover, the Neo-Aristotelian has little reason to accept anything close to the epistemology that Smith proposes.

Section 3.1: Aristotelian Concretism about Models

As noted above, the main point of contention between Smith and Cartwright is their interpretation of a "theoretical unification". Consider that by invoking the notion of a "principled" model, premise (F1) of Smith's argument is referring to a set of criteria that need to be met in order for a theory to count as "unifying" a particular domain: theories only apply to a domain *D* insofar as they can provide principled models for phenomena in *D*; thus, a *unified* theory of *D* needs to be able to provide principled models for *all* phenomena in *D*. In its schematic form, (F1) is shared by fundamentalists and anti-fundamentalists alike, because both parties agree that not just any model will count as principled! However, disagreements arise once we get into the specifics of the criteria according to which a model will count as principled, which will in turn determine what a "theoretical unification" consists in.

We will now explore Smith's understanding of what it takes for a model to be principled. Recall that in his Fundamentalist Argument, premises (F2) and (F3) jointly describe the existence of an infinite set *S* of models that satisfy physically reasonable assumptions that we called the Truesdell Constraints; however, these premises do not yet claim that such models count as "principled" ways in which the theory can be applied to describe classical phenomena. Indeed, as far as these premises go, such a characterization might be consistent with the models being purely *ad hoc*, or phenomenological (relative to the theory). The additional claim that the models in *S* are principled is made in (F4), and the claim that any classical phenomenon will be described by a model in *S* is made in (F5)—these two premises, then, are the controversial ones that we will now interrogate.

Why should one think that (F4) is true? A plausible way of reconstructing Smith's reasoning here is as follows: Evidently, we have a general method for demonstrating the existence of constitutive equations which satisfy the Truesdell Constraints; indeed, we know that there is an infinite set *S* of such models (cf. Smith (2001): 471-2). Now, several of these models happen to be *explicitly constructible* and have been applied with great empirical success; the success of these models can then be taken to justify other models in *S* (Smith (2001): 474).

Based on this line of thought, let us spell out more precisely what Smith means by a "principled" model. According to Smith, a model is principled relative to a particular theory if it (i) fulfils the formal constraints that the theory sets for the generation of models (i.e. the Truesdell Constraints), (ii) these constraints represent some relevant physical aspects of the phenomena in the domain (i.e. general physical properties of materials), and (iii) some models that satisfy the constraints have been successfully used to describe, predict, and explain phenomena in that domain. This account of "principled" is not a purely formal one, because *some* models of *S* need to successfully predict empirical phenomena for this account to get off the ground; however, it is still highly formal in the sense that once this minimal empirical requirement has been met, the warrant of those models is transferred to *S* as a whole by very formal means, i.e. simply in virtue of the models in *S* satisfying the Truesdell constraints; the models in *S* are then judged to be principled. We shall call this general approach to the epistemology of models an "abstractionist" epistemology, since it is willing to count models as principled on the basis of highly formal criteria.

It seems to us that this "abstractionist" epistemology of models should raise the suspicions of Neo-Aristotelians, as well as others who philosophers who share the intuition that warrant does not transfer in virtue of the kind of formal relationship that Smith wishes to lean on. As a toy example, consider the relationship between the following two models A and B in some set *S* of models which satisfy "physically reasonable constraints": Let A be a model that has been extremely fruitful and empirically well-confirmed, such as the simple harmonic oscillator, and let model B simply be some other element of *S* whose existence we can perhaps only guarantee abstractly. Why should the empirical confirmation that attaches to A (in virtue of which it intuitively counts as "principled"), or some part thereof, transfer to B simply in virtue of their both being elements of *S*? Plausibly, the fact that they both satisfy "physically reasonable constraints" cannot be made to bear the weight of such a transfer of warrant; and furthermore, it is at least conceivable that many models in *S* might simply fail to apply to anything in reality (or even in possible worlds that are "close" to the actual world). By contrast, A and B usually have a much tighter relationship in cases where we think the warrant has some chance of transferring, e.g. a case in which A is the simple harmonic oscillator and B is a modification of A to include a damping term. Returning now to the case of CM, one might reasonably expect that the grounds on which a model is justified should include knowledge of how that model is related to some actual set of (real) materials and experimental settings, i.e. knowledge that goes beyond the idea that all "reasonable" constitutive equations should satisfy the Truesdell Constraints. Thus, from the concretist point of view, all that Smith has shown is that CM has the resources to articulate some very schematic degrees of freedom (i.e. the abstract notion of a constitutive equation satisfying the Truesdell constraints) which can be "filled in" in many different ways. Upon being filled in, they may or may not be applicable to real phenomena, and even if they are applicable, the extent to which such an application counts as "principled" should be taken to be an open question.

Let us now shift our focus to (F5): Why should we think that the set of constitutive equations compatible with the Truesdell Constraints is able to describe *all* real materials within the classical domain? According to Smith, the fact that there is an almost infinite number of constitutive equations that satisfy the Truesdell Constraints is good reason to believe that each phenomena of the domain will be described by some constitutive equation (Smith (2001): Sec. 4.2). However, just pointing out that the set of models that satisfy the Truesdell Constraints is infinite does not show that this set will contain all the models needed to describe each phenomenon in the domain. After all, it is reasonable to think an infinity of potential models might not be able to account for the diversity of real materials that one encounters in nature. Thus, an argument is needed here in order to show that any real material can be modeled (in a principled manner) by an element of *S.* We now consider an argument to this effect that is implicit in Smith's remarks.

Smith thinks there are reasons to be optimistic about not just the number but also the variety of models contained in *S*—they are so various, he thinks that it is almost unthinkable that they could fail to model a real material in a principled way. When discussing the potential infinity and variety of contact forces, he presents as an example that he calls "Lindsay's function", which is a function introduced in Lindsay's classic textbook on CM. Lindsay's function is an exponential "ansatz" that can account, via Fourier series theorems, for almost any function, subject to a small set of constraints. And since almost any force can be modeled by such a function, or so Smith's story goes, Lindsay's function provides principled models for such forces. Smith then wishes to run an analogous argument in the case of CM, where the analog of the models generated by Lindsay's function is *S,* i.e. the set of constitutive equations satisfying the Truesdell constraints (Smith (2001): Sec. 4.1).

The problem with this strategy for generating an infinite—and infinitely diverse—set of principled models that will account for any real material is that it severely vitiates the plausibility of Smith's understanding of "principled". If we allow almost any function to fulfill the theoretical constraints and to define a principled model (as Smith suggests is the case in his example regarding Lindsay's function), the relationship between empirically successful principled models and the merely potential principled models turns out to be very loose indeed. Why should we think that we can transfer the epistemic warrant from empirically successful models in the set *S* to all the other models of *S* if we allow almost any function to define a principled model in the set? It seems that the reasons Smith gives to believe that (F5) is justified also severely undermine the justification for (F4), thereby threatening the ability of Smith's own definition of "principled" to set any substantive constraints on what counts as a principled model.

As we have just seen in detail, the disagreement between fundamentalists and anti-fundamentalists boils down to a disagreement about how to understand principled models. The Neo-Aristotelian philosopher may acknowledge that CM can generate a set of models potentially capable of describing phenomena involving fluids and inter-body gravitational forces; however, the Neo-Aristotelian would take issue with understanding the relationship between the theory, the model, and the model's application to material phenomena in the highly formal terms that Smith relies on. Rather, philosophers of a Neo-Aristotelian inclination tend to think that experimental and material knowledge play a far more substantive role in the generation and justification of principled models. We now turn to a way of spelling out these Neo-Aristotelian thoughts that has been championed by Nancy Cartwright.

Nancy Cartwright has developed an in-depth analysis of scientific practice defending what we call a "concretist" epistemology of models. The immediate consequence of her epistemology for the case at hand is that in order for a model to be principled, it is not enough that it accords to some general formal principles; we also need to have the relevant material and experimental knowledge that allows us to determine and justify whether some specific mathematical model fits the phenomenon at hand.

Cartwright does not offer an explicit definition of what it is for a model to be principled; however, she does offer heuristics concerning how principled models are generated in scientific practice. As we understand Cartwright, a model *M* of a phenomenon *X* in experimental context *Y* is principled only if it has been generated from theory *T* by means of a process that satisfies the following conditions:

(i) The construction of the model takes into account information regarding the material phenomenon X and experimental context Y (e.g. what causal factors of the phenomenon X are relevant, how to properly shield the experimental context Y in order to isolate X, etc.),

(ii) The construction of the model takes into account information regarding how to construct a computationally tractable model (e.g. how to generate a mathematical model that can be used for calculations, simulations, etc.), and

(iii) The construction of the model takes into account knowledge about how to bridge the gap between theoretical and experimental knowledge (e.g. what can be simplified or idealized, what parameters should be defined, etc.).

Cartwright captures these conditions for the derivation of principled models in scientific practice by referring to a set of rules that she calls "bridge principles." Bridge principles are the primary guidelines for determining how we can generate principled models from theory; indeed they also determine the scope of the theory's application by constraining the range of models that can be derived from a theory in a principled way. Note, however, that Cartwright emphasizes that bridge principles do not generate models solely on the basis of theoretical input; one must also have in mind the target of the model and its intended experimental context. In this sense, bridge principles should provide a way of determining whether the model matches the target phenomenon that is to some degree independent of the theory.

In section 4 we will elaborate on how to more precisely understand a concretist epistemology of models more generally. For now, let us just emphasize once more that Smith's argument requires us to accept an abstractionist epistemology of models in order for premises (F4) and (F5) to be justified. As we have noted, this abstractionist epistemology relies on allowing very permissive constraints on what counts as a principled model that strongly contrasts with Neo-Aristotelian methodological inclinations regarding the epistemic relevance of material and experimental knowledge. Fortunately for the Neo-Aristotelian, an abstractionist epistemology is not the only epistemology of models available. As we have seen, Cartwright presents a concretist epistemology of models that undermines key premises of Smith's argument. Furthermore, as we show in section 3.2 below, Cartwright's concretist epistemology can be put into work in order to construct a counter-argument against Fundamentalist Unification.

Section 3.2: Cartwright's Anti-fundamentalist Argument

The general underlying principle behind the above discussion is that, in order to understand what are the standards for a good theory-model relationship (i.e. how to produce and recognize principled models) we need to go beyond the theory. In addition to the theory we also need to take into account theoretical know-how regarding what is computationally tractable, experimental know-how regarding the material conditions to implement the model, and practical know-how regarding how to bridge from theory to experiment.

On the basis of her analysis of scientific practice, Cartwright famously argues that science does not offer us a fundamentalist picture of the world. Instead, it offers us an image of the world as "dappled", i.e. the world is not governed by a limited set of universal laws that apply to all domains, but is instead divided into small pockets of restricted regularities (Cartwright (1994)). Here is the structure of her argument, as applied to the classical domain:

Cartwight's Anti-Fundamentalist Argument:

(F1) Theories only apply to a domain insofar as there is a principled way of generating a set of models that are jointly able to describe all the phenomena in that domain.

(AF2) Classical mechanics has a limited set principled models, so it only applies to a limited number of sub-domains.

(AF3) The limited sub-domains of AF2 do not exhaust the entire classical domain.

(AF4) From (F1), (AF2) and (AF3), the domain of classical mechanics is not universal, but dappled.

As we mentioned above, (F1) is a shared premise between fundamentalists and anti-fundamentalists. The differences arise in how they respectively understand what it takes for a model to be principled; Cartwright's view on this is captured by premises (AF2) and (AF3) of the anti-fundamentalist argument.

Cartwright justifies (AF2) by pointing out that we only have the know-how to reliably apply a handful of bridge principles that cover only a limited set of sub-domains. This is because most of our material and experimental knowledge is limited to situations we can control by shielding the target system from external interferences. Given these limitations on our material and experimental knowledge, we cannot generate principled models that describe phenomena outside these shielded sub-domains. Accordingly, Cartwright's strategy to justify (AF3) is just to point out that the classical domain is much larger that the conjunction of all the sub-domains defined by those shielded situations we have principled models that describe them.

The main insight driving Cartwright's argument for the disunity of classical mechanics is precisely that for many phenomena within the domain of classical mechanics, we lack the material and experimental knowledge necessary to generate and apply models in a principled way. We may well have a formal mechanism that allows us to show the existence of a model, as suggested by Smith's analysis, but this is insufficient to provide the right kind of theoretical unification.

Cartwright takes her concretist epistemology as a starting point for her analysis of scientific practice, which leads to the conclusion that the domain of classical mechanics is not universal, but dappled (i.e. AF4). Accordingly, Cartwright's argument is an attempt to undermine CFU (and thereby Fundamentalist Unification) by providing an analysis of the relation between theories, models and phenomena that highlights the limitations of classical mechanics to unify its whole domain.

Section 4: Towards a Concretist Epistemology of Models

One of the limitations of Cartwright's concretist epistemology is that, as presented, the normative principles that she recommends are rather vague, and it is difficult to see how they generalize to cover different kinds of cases. The root cause of these limitations is what we might call Cartwright's extreme "particularism" about the philosophy of science. Her writing sometimes suggests that she does not think one can give any general guidelines and/or constraints for how to identify principled models and bridge principles, as well as the role that they play; all we can do is investigate the details of how they are used in scientific practice. For instance, she remarks the following with respect to interpretative models:

> "And what kinds of interpretative models do we have? In answering this, I urge, we must adopt the scientific attitude: we must look to see what kinds of models our theories have and how they function, particularly how they function when our theories are most successful and we have most reason to believe in them. In this book I look at a number of cases which are exemplary of what I see when I study this question. It is primarily on the basis of studies like these that I conclude that even our best theories are severely limited in their scope." (Cartwright (1999): 9)

So, for Cartwright, the amount of details and precision that a general account of principled models (or most scientific concepts) is very limited precisely because of the richness and variety that we find in scientific practice. At the end of the day, if we want to know the scope of a particular theory, or whether a specific model is principled, we have to do the hard work of looking into the details of that theory and that model and how are they used in scientific practice.

In this section we attempt to depart from Cartwright's specific strain of particularism in order to attempt to define some general guidelines for a concretist epistemology of models. Section 4.1 presents Cartwright's main case study in favor of her concretist epistemology of models, what she calls the "Neurath's Bill" case. We use this case as an illustration of some of the central desiderata for a general concretist epistemology. The next sub-section, section 4.2, will use the lessons learned in 4.1 to define the main desiderata for a concretist epistemology of models emphasizing the differences with Smith's abstractionist epistemology. Finally, Section 4.3 addresses the potential worry of whether and how a concretist epistemology of models can accommodate the role of theory and theoretical unification in scientific practice.

4.1 Some Lessons from the Neurath's Bill Example

In keeping with her belief that the philosophy of science cannot be seriously conducted except by attending to the details of scientific practice, Cartwright's arguments are generally framed within the context of a physical example. For instance, her "Neurath's Bill" example is the setting for trying to demonstrate "dappledness" within the domain of classical mechanics. In this example, a $1000 bill is dropped from the top of St. Stephen's Square, travels erratically over the air while being swept away by the wind, and finally lands somewhere in the square. Cartwright is skeptical that any theory will provide a "good description" for such cases, which has aspects best described by classical fluid mechanics, on the one hand, and aspects best modeled by point particle mechanics, on the other hand. It is worth quoting her conclusions from this example in full:

> "Let us set our problem of the 1000 dollar bill in St. Stephen's Square to an expert in fluid dynamics. The expert should immediately complain that the problem is ill defined. What exactly is the bill like: is it folded or flat? straight down the middle, or...? is it crisp or crumpled? how long versus wide? and so forth and so forth and so forth. I do not doubt that when answers can be supplied, fluid dynamics can provide a practicable model. But I do doubt that for every real case, or even for the majority, fluid dynamics has enough of the 'right questions' to ask to allow it to model the full set of causes, or even the dominant ones. I am equally sceptical that the models that work will do so by legitimately bringing Newton's laws (or Lagrange's for that matter) into play. How then do airplanes stay afloat? Two observations are important. First, we do not need to maintain that no laws obtain where mechanics runs out. Fluid dynamics may have loose overlaps and intertwinings with mechanics. But it is in no way a subdiscipline of basic physics; it is a discipline on its own. Its laws can direct the 1000 dollar bill as well as can those of Newton or Lagrange. Second, the 1000 dollar bill comes as it comes, and we have to hunt a model for it. Just the reverse is true of the plane.We build it to fit the models we know work. Indeed, that is how we manage to get so much into the domain of the laws we know.
>
> Many will continue to feel that the wind and other exogenous factors must produce a force. The wind after all is composed of millions of little particles which must exert all the usual forces on the bill, both at a distance and via collisions. That view begs the question. When we have a good-fitting molecular model for the wind, and we have in our theory (either by composition from old principles or by the admission of new principles) systematic rules that assign force functions to the models, and the force functions assigned predict exactly the right motions,then we will have good scientific reason to maintain that the wind operates via a force. Otherwise the assumption is another expression of fundamentalist faith." (Cartwright 1999, pp. 284-5)

Let us highlight some important points in the quoted passage that bear on Cartwright's concretist epistemology of models. First, it is important to note that Cartwright does not present this example as a challenge to the possibility of giving a scientific explanation of the trajectory of the bill, but as a challenge to the fundamentalist claim that all "classical phenomena" fall under the domain of the same theoretical principles or laws. The core disagreement between fundamentalists and anti-fundamentalists is not about whether there are actual or possible scientific explanations for all classical phenomena, but about to what extent all these (actual and possible) explanations that science provides fall under a unified set of theoretical principles or laws.

Second, the target of Cartwright's skepticism is not the possibility of finding a model for the 1000 dollar bill; rather, her skepticism is about the possibility of finding a principled model for that situation, that is, a model that is derived from the theory through systematic rules (i.e. bridge principles). Accordingly, the reason why the Neurath's Bill example is a challenge to fundamentalists is not only because it is a complex situation, but because it is a situation sufficiently different from any situation that we have good models for and we have no systematic rules that tell us how to generate models for situations like this one.

There is a rather delicate but important issue that arises in the context of Neurath's Bill (and other examples with a similar structure), and we believe that it is one about which Cartwright is not sufficiently careful. This can be seen from the fact that Cartwright slides from "skepticism about whether any theory will provide a good *description* for some phenomena" to "skepticism about whether any theory will provide a principled *predictive* model for Neurath's Bill". This move is only licensed if one identifies "description" with "prediction", which is not one of Cartwright's explicit commitments. Furthermore, notice that any system that displays extreme sensitivity to initial conditions (such as Neurath's Bill) will not admit of predictive models in practice, and so if "description" were to be identified with "prediction", principled models in such cases would be ruled out by *fiat*. This seems unfair to Cartwright's interlocutors and furthermore threatens to make Cartwright's philosophy of models collapse into some form of operationalism. We thus conclude that the relevant question about cases such as Neurath's Bill is not whether they admit of principled predictive models, but rather whether they admit of principled models that adequately *describe* the empirical scenario.

Finally, note that Neurath's Bill is supposed to be just one out of a cornucopia of scenarios within this domain which are not described by a unified set of laws and theoretical principles. Cartwright's more general point is that classical mechanics is not unified because there are no laws or theoretical principles that apply in all the domain of classical mechanics. There are some domains of classical mechanics that we have a very good grasp of such as point particle mechanics and fluid dynamics; however, there are many other domains where we do not have a good grasp of how to understand (explain and predict) the phenomena occurring there. So, according to Cartwright, classical mechanics is not unified because its domain is divided in different sub-domains, not all of which can be accounted by a single set of theoretical principles or laws.

How can we build on the above morals to sketch some general desiderata for a concretist epistemology of models? To be clear, what we are trying to do here is sketch guidelines for an approach that accepts the primacy of studying how particular theories/models work in order to make philosophical progress, while still avoiding the particularist extreme that Cartwright sometimes inclines towards (on which no general rules can be articulated). Based on the previous discussion, it seems to us that on the concretist's view, a principled model needs to at least possess the following features:

(i) It is predictively accurate when applied to the sub-domain D' of the relevant domain D
(ii) It is descriptively accurate when applied to the relevant domain D
(iii) It is formally adequate with respect to the relevant theory
(iv) It is materially adequate with respect to the relevant experimental context

(i) is an uncontroversial criterion that both concretist and abstractionist epistemologies will agree on. On the other hand, disagreements will begin to crop up with respect to (ii): For abstractionists such as Smith, the fact that *some* elements in a set *S* of models (where *S* is perhaps picked out by the requirement that its models satisfy some generic physical assumptions) satisfy (i) can be used to secure (ii) for other, nonpredictive, elements of *S*—in other words, (i) is used to bootstrap to (ii) for D much larger than D'. On the other hand, Cartwright does not seem particularly interested to consider cases in which D differs from D', i.e. she is generally only interested in models which are predictively accurate. We recommend that the concretist explore a *via media* between the Smith and Cartwright positions: in particular, it seems plausible that some models can be descriptively accurate without being predictively accurate, perhaps in virtue of the specific (not merely formal) relationship that they bear to predictively accurate models.

(iii) is again an area in which one might expect some common ground between concretists and abstractionists (although there is quite a bit of room for disagreement about how tight these "formal relationships" have to be). And finally, (iv) will be another point of contention between concretists and abstractionists: as we saw in Section 2, Smith is not particularly concerned with the material adequacy of models, thus leading to his highly permissive conception of principled models.

4.2 Accommodating Abstraction into Scientific Practice

Neo-Aristotelians emphasize the inherent complexity of the natural world, and show a healthy skepticism regarding the possibility of capturing such complexity with overly general and abstract methods. However, this skepticism should not translate into a rejection of formalization and abstraction as theoretical tools which play a role (albeit a limited role) in understanding the natural world. Fortunately, the advocate of a concretist epistemology does not have to reject abstraction altogether as a theoretical tool; rather, concretism grants abstraction a legitimate theoretical role in scientific practice while keeping in check the philosophical excesses that stem from a blind faith in the power of abstraction.

In particular, abstraction plays a crucial role in a kind of unification that we call **Schematic Unification (SU)**. We have a case of SU when one tries to unify sub-theories by formulating a more abstract theory such that some laws/concepts/quantities of the sub-theories turn out to be instances of some laws/concepts/quantities of the abstract theory. Notice that, in itself, SU implies nothing about the prospects for theoretical unification, in the sense that finding a mathematical abstraction of a successful set of models (which in turn allows us to generate many new models) in no way guarantees that such an extension will describe a real domain. Indeed, radically generalizing a theory so that its range of models becomes infinite might in fact make it more difficult for the theory to have an empirical grip on reality.

Accordingly, one way of re-formulating Cartwright's criticism of Smith's account of theoretical unification is to say that Smith overemphasizes the role that SU has in theoretical unification. However, this is not to deny that SU has no legitimate role in theoretical unification. SU is a crucial step in setting up a common formal framework that allow us to understand and manipulate different theories and models under the same formalism. Physics provides various cases of SU forming part of a such an strategy to secure theoretical unification. For instance, take the case of Newtonian point-particle mechanics (PM). Let our sub-theories be (i) PM equipped with Universal Gravitation as a special force law (Gravitational PM); and (ii) PM equipped with Coulomb's Law as a special force law (Coulomb PM). We then introduce a unifying theory "Super PM", which is PM equipped with a purely schematic force function F. Evidently, SU is achieved by subsuming the different concrete forces under a single abstract force concept. Notice, however, that there is also a non-schematic, supplemental element that accompanies SU in this case: we are told how to combine different concrete forces, i.e. by vector addition. In other words, the schematic character of force is not only used to accomplish SU, but also provides a theoretical framework for describing interactions between Gravitational PM and Coulomb PM.

Accordingly, *under appropriate conditions*, SU can yield a genuine strategy for obtaining a larger theory that can unify two domains of phenomena. From a concretist perspective, these "appropriate conditions" are those in which we have enough information about the material systems and the experimental context that we can supplement the schematic character of the concepts of the unifying theory with specific principles that bridge the gap between the mathematical abstraction and the concrete phenomena at hand.

Smith's emphasis on SU exemplifies many of the general worries that motivate neo-Aristotelians' skepticism with respect to the use (and abuse) of formal methods in the sciences. By distinguishing SU from theoretical unification and analyzing their relation and epistemic differences, a concretist epistemology of models allows for a more careful and empirically-grounded understanding of SU in theoretical unification. This thus provides an example of how a Neo-Aristotelian framework can incorporate the use of formal methods as legitimate without compromising the relevance of materiality and concreteness.

Section 5: Conclusion

Our main aim in this paper was to provide the Neo-Aristotelian philosopher with the conceptual tools to resist the fundamentalist challenge. We have argued that the best argument in favor of fundamentalism (Smith's Fundamentalist Argument) is based on an implausible abstractionist epistemology of models that begs the question against the anti-fundamentalist and violates the Neo-Aristotelian ethos of emphasizing materiality and concreteness over formalization and abstraction.

Fortunately for the Neo-Aristotelian philosopher, Smith's abstractionist epistemology of models is not the only viable option. Cartwright's concretist approach to the epistemology of models allows the Neo-Aristotelian philosopher to, first, resist Smith's arguments for the theoretical unification of the classical domain; and, second, to implement the conceptual resources of Cartwright's concretist epistemology to construct a positive argument for anti-fundamentalism, thereby shifting the burden of the proof in the debate.

Cartwright's concretist epistemology is limited by her particularism, so it is difficult to generalize from her specific, detailed examples, to a more general concretist account of the epistemology of models. In order to overcome this difficulty, we analyzed Cartwright's concretist approach and tried to extend it by sketching the form of a set of constraints that need to be satisfied in order for a model to be "principled". These constraints include both criteria of formal and material adequacy. The resulting framework provides an excellent example to Neo-Aristotelian philosophers of how one can be critical of the excessive use of formalization and abstraction while remaining faithful to actual scientific practice.

We started the paper by emphasizing the threat that fundamentalism may pose to Neo-Aristotelian ontologies. Let us conclude by drawing some general morals that may give hope to ontological projects of a Neo-Aristotelian flavor. First, the arguments against fundamentalism presented in this paper give Neo-Aristotelians ammunition to resist the main threat from fundamentalism: the accusation that Neo-Aristotelian ontologies do not square well with contemporary scientific theories or the scientific consensus.

Second, it provides a prima facie reason to explore alternative ontologies to traditional fundamentalist ones. To the extent that fundamentalism is one of the central motivations for proposing fundamentalist ontologies of the world, the undermining of fundamentalism—through embracing a concretist epistemology of models—also undermines the motivation for exclusively focusing on fundamentalist ontologies. Thus, anti-fundamentalist creates room to entertain a plethora of alternative ontologies.

Finally, the dappled view of science resulting from this concretist epistemology fits better with power ontologies than with traditional law ontologies (as noted by Cartwright (1994)). Furthermore, a dappled view of the world leaves much more space for non-reductive accounts of causation and causal powers, thereby opening many more ontological possibilities to the Neo-Aristotelian, including different varieties of substance ontologies and hylomorphism.

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