ENLARGING THE POSSBILITY SPACE FOR SCIENTIFIC MODEL-BASED EXPLANATION

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The undersigned, appointed by the dean of the Graduate School, have examined the dissertation entitled

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and hereby certify that, in their opinion, it is worthy of acceptance.

Professor Andre Ariew

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Professor Collin Rice

Professor Randall Westgren

For my family: Tim, Diane and Cheryl.

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ABSTRACT

Two prominent views in the scientific explanation literature are: (1) that scientific explanations should be ontic or track causal or constitutive relations between the explanans and explanandum; (2) Idealizations in scientific models can be either epistemically dispensable or indispensable in principle. (1) manifests in the requirements which proponents of that view hold for scientific models to be deemed explanatory. Per these advocates, scientific models must not only track causal or constitutive relations but must include some mapping from the model components to the target system. (2) represents something like the current state of play for understanding the place of idealizations in scientific models and involves the longstanding issue of intertheoretic reduction. Idealizations can either be epistemically indispensable (that is not derivable from or reducible to) the relevant micro-level theory or epistemically dispensable in principle.

The following project aims to rebut both of these views, thereby seeking to enlarge the possibility space for scientific explanation. For this reason, this project gestures towards and develops new dimensions for scientific model-based explanation. Pace (1), there are many scientific models which do not track ontic or causal relations but are nevertheless explanatory. The first chapter considers a cognitive dynamical model –the HKB model of bimanual coordination– which fails these requirements for explanation but is one which I claim can still be shown to be explanatory. This represents a promising bit of evidence which can be marshalled and directed against this commitment. Along the lines of (1), proponents of this requirement claim that scientific models must be ontic or risk facing a problematic “directionality problem.” The second chapter provides a route of response

for the advocate of non-ontic scientific explanations, demonstrating how this problem can be resolved along pragmatic lines. Finally, the partition of the possibility space for understanding the role of idealizations in scientific models encapsulated in (2) is challenged in the third chapter. Therein, a certain species of idealization –continuum idealizations– are discussed and a pragmatic and deflationary approach to the issue of intertheoretic reduction is argued for. These chapters all serve to demonstrate countervailing considerations which, if successful, act as important challenges for the veracity of both (1) and (2). Rather than achieving a mere refutation of these commitments, the success of this project calls for a re-imagining and enlargement of the possibility space for scientific model-based explanations.

INTRODUCTION

The introduction of this project consists of two parts. First, I provide some background to the relevant issues: scientific explanation, causal versus non-causal/non-ontic explanations and mathematical explanation. Second, I provide a brief summary of each chapter in the form of a roadmap.

1. Historical background of scientific explanation

The issue of scientific explanation is a perennial one, stretching back to at least Aristotle. The enormity of erecting an exegesis of the topic over the course of over 2,000 years is daunting and is a task unnecessary to my purposes. For the sake of brevity and focus, our survey of the topic is more contemporary, beginning with the work of Carl Hempel.

1.1 Hempel's D-N view of scientific explanation

From the late forties to the early sixties, Carl Hempel attempted to provide an adequate account of scientific explanation which would correctly demarcate between explanations which were scientific and those which were not by providing an account of independently necessary and jointly sufficient conditions for something to count as a scientific explanation (Hempel and Opponheim, 1948). Hempel's model of scientific explanation is known as the deductive-nomological (D-N) model of explanation or the covering law model of explanation. On the D-N model, something qualifies as an explanation if and only if: 1. It consists of an explanandum result which is deductively entailed by the explanans and 2. The explanans includes at least one law of nature and this law is *essential* to the derivation of the explanandum (Hempel, 1965). The deductive quality of the view is located in the first, entailment condition and the second condition provides the nomic component, hence the "D-N view."

The D-N view represented a promising attempt at constructing an adequate account of scientific explanation but it was quickly beset by serious objections. I will mention two:

(A) The Symmetry Objection: As was famously demonstrated in an example involving a flagpole which casts a shadow, the D-N view problematically permits symmetrical explanations and this violates a desirable norm for scientific explanation (Bromberger, 1966; Salmon, 1989). Imagine a flagpole which is casting a shadow. Given the D-N model, one could non-problematically explain the length of the shadow by constructing a deductively sound argument including natural laws and initial conditions having to do with the height of the flagpole, the propagation of light in straight lines and the angle of the sun's elevation. But problematically, one could also produce a deductively sound argument that runs in the other direction, using the length of the shadow and the same laws and initial conditions to explain the height of the flagpole. However, this reversed case is *clearly non-explanatory*; it is a desirable norm of explanation that natural explanations should not be symmetrical, running in both directions. Hempel's D-N view does nothing to rule out this reversed case and so the view seems to be an inadequate account of scientific explanation since it licenses these symmetrical explanations of natural phenomena thus violating a desirable norm of explanation.

(B) The Hexed Salt Objection: The D-N view appears to be an inadequate account of scientific explanation because it does not sufficiently limit the inclusion of irrelevant premises in the explanans. Imagine a case in which one gave an explanation for the explanandum question "why does all hexed salt dissolve in water?" One could construct a

deductively sound argument that includes a natural law about the chemical properties which account for the salt's solubility as well as some initial conditions including the hexing of the lump of salt. However, this seems obviously wrong. The salt's solubility is explainable solely by citing its chemical properties which account for its solubility. The hexing seems straightforwardly irrelevant to its dissolution. But Hempel's view does nothing to rule out the irrelevant information about hexing the salt. For example, that the addition of the irrelevant information about the salt's hexed properties does not disrupt deductive entailment. Once again, the D-N view appears inadequate since at best it permits gratuitous explanations and at worst it permits pseudo explanations.

1.2 The turn towards causal reductionism

To avoid these problems, many philosophers adopted a kind of causal reductionist view about scientific explanations, namely, that all genuine explanations are *causal* (Salmon, 1989). This was meant to both face up to and eliminate the kinds of aforementioned problems which troubled the D-N account.[1] Seemingly, restricting all genuine explanations of natural phenomena to causal explanations solves for the flagpole objection. If one holds that all genuine explanations must be causal, the flagpole case is blocked since, unlike the flagpole's height which helps to produce the length of the shadow, there is *no causal relationship* between the shadow's length helping to produce the height of the flagpole. Simply, an effect cannot help to causally produce its cause. The causal view eliminates the symmetry problem since all causal explanations are

[1] Another significant development was to give a pragmatic view of explanation which was sensitive to contextual facts about the explanation. An example of this kind of view is Bas Van Fraasen's pragmatic view of explanations (Van Fraasen, 1980) which holds that an explanation is a three term relation between theory, fact and context.

properly asymmetrical, running only in one direction. Similarly with the hexed salt objection. If one holds that all genuine explanations are causal, then this represents a way to restrict premises from appearing in the explanans which are irrelevant. In order for an explanans to explain some explanandum event, *E*, one may require that the premises which comprise the explanans must all be *causally relevant* to producing *E* where causal relevance may be defined as some fact or law's being causally efficacious in producing *E*. In the hexed salt case, the salt's having a hexed property fails to be causally relevant since it is causally inefficacious in the salt's dissolution in water. And so the causal reductionist is well equipped to solve for both the flagpole and hexed salt objection.

1.3 Tasks and challenges for causal reductionism

Although the causal reductionist view attracted considerable support for its ability to resolve the problems which hampered the D-N view, the causal reductionists also faced some considerable challenges in further developing their view. Three are worth mentioning here:

1.3.1 Providing an account of causation

One of the first tasks for the causal reductionists was to provide an adequate account of causation. This was necessary to discriminate between causal processes and pseudo-processes in nature and thereby avoid admitting both cases of spurious causation and pseudo processes as genuinely causal. To cite a well-known example, there is a strong correlation between a barometer's falling and the occurrence of storms. Low pressure systems typically presage storms and also cause the reading of barometric pressure to plummet. One would want to avoid citing the barometer's falling as causing the storm since no causal relationship obtains. To do otherwise would be to mistake spurious causation for the genuine article. Next, consider a shadow moving across a wall and someone's training a laser pointer on the shadow, following the shadow's movement. Is there a genuine causal process between the laser and the shadow? The answer appears to be a definitive "no," since this is only a pseudo process. It does not involve a genuine causal relationship. By contrast, there does seem to be a genuine causal process between the object which casts the shadow and the movement of the shadow. Working out these differences was an area of concern for the causal reductionist (Salmon, 1989). To resolve this problem, Wesley Salmon provided a view called the "mark transmission theory" of causation. For Salmon, a process is a causal rather than a pseudo process only if that process is capable of transmitting a mark where by mark he means a signal or information. This provides a necessary condition for something to count as a causal process and so many problematic pseudo processes are ruled out in virtue of failing this condition. Others turned to David Lewis' counterfactual account of causation (Lewis, 1973; 1986) for inspiration, arguing that some event, E1, is a cause of some further event, E2, just in case E1 *counterfactually depends* on E2 in something like the following way: E2 depends on E1 just in case if E1 were not to occur, then E2 would not occur. However, Lewis' account faced its own host of serious objections involving issues like preemption, both early and late, as well as problems with causal overdetermination. None of these accounts were widely accepted as constituting adequate accounts of causation and the search for such an account continues currently.

1.3.2 The issue of causal reductionism and causal efficacy

Recall that a way out of the hexed salt case for the causal reductionist was to limit the explanans of a suitable explanation solely to the inclusion of information that was causally efficacious with respect to the explanandum event. But the nexus between causal relevance and causal efficacy was not without its critics.

In a well-known objection, Jackson and Petit (1990) argue that an event can be causally relevant to the production of another event but yet not causally efficacious to the production of that event. Consider a case of boiling water cracking a flask. One might pose the explanandum question "what caused the glass to break?" One answer is to cite the temperature of the water whereas another is to cite the molecular energy of the individual molecules which broke the flask. The latter is a micro-level causal explanation which could be multiply realized; several different combinations of individual molecules could have caused the glass to crack. However, it is plausible that citing the water's temperature, a macro-level property, is explanatory. The temperature is said to *program* for the effect of the glass cracking. This is meant to show that something can be causally relevant to an effect and hence explanatorily adequate, even if it is not causally efficacious in producing that effect. This seems to push back on the causal reductionist's drawing an equivalency between causal relevance and causal efficacy.

1.3.3 Non-causal explanations

Another challenge levied against the causal reductionist is that some explanations of natural phenomena are non-causal. An early example comes from Hilary Putnam. Consider the simple problem of trying to fit a square peg through a round hole (Putnam, 1975). Suppose one has a cubical peg which is 15/16 of an inch and is attempting to insert it through a circular hole which is 1 inch in diameter. One quickly finds that the peg will not fit neatly into the hole and this may give rise to the explanadum or "why" question: "why won't the square peg fit through the round hole?" Putnam thinks the obvious answer has to do with citing the geometrical properties of the peg and the hole. But notice that if one holds, as the early causal reductionists did, that only the micro-level causal story or the story including the entities which are causally efficacious in producing the failure of fit is relevant, then the geometrical explanation, which occurs at a higher level, is non-causal. So, *pace* the causal reductionist, some explanations may be non-causal. This same kind of problem would receive further attention and clarity in the work of Elliot Sober who argued that equilibrium explanations cannot be given a causal gloss for the same kind of reason (Sober, 1983). These are explanations which treat higher order or macro phenomena rather than individual or micro-level phenomena. And only the latter seem to be causally efficacious in producing the explanandum event. If causal efficacy is a necessary condition for explanation, then it seems equilibrium explanations pose a difficult counterexample to the causal reductionist.

A second argument against the claim that all genuine explanations are causal is that some scientific explanations are non-causal in virtue of being mathematical (Steiner, 1978). And if an explanation is mathematical, it is non-causal since mathematics are a-causal. Consider a simple case involving Mother, who has 23 strawberries and 3 children. One might pose the following explanandum question "Why couldn't Mother divide her strawberries evenly (without cutting or splitting any) among her three children?" The following kind of explanation seems to naturally suggest itself. Begin with the premises that both "Mother has 23 strawberries" and "23 is not evenly divisible by 3." These two premises entail the explanandum event, viz. "Mother can't divide her strawberries evenly among her three children." Although this example is not a scientific explanation, it inspired the search for genuine scientific explanations that are mathematical. Some early examples of scientific mathematical explanations are provided in Baker (2005) and include the life cycles of cicadas and the famed honeycomb conjecture.

Examples of both mathematical scientific explanations and other kinds of non-causal explanations soon began to receive great interest and attention in the scholarship. And this pushes back on a core assumption in the literature, namely, that in order for something to count as a scientific explanation, it must track causal or constiuitive relations in the world. The bevy of non-causal or non-ontic explanatory cases serve as an important body of evidence against this claim. A second widely held view concerns the role of idealizations in scientific explanations. Idealizations can be thought of as purposeful distortions of the target system. Certain classes of idealizations distort or falsify details pertaining to the micro-level of the target system in order to capture macro-level patterns or properties. The widely held view is that the status of these idealizations involves their epistemic (in)dispensability to the explanation in question. Either an idealization is epistemically indispensable in principle —that is, cannot be reduced to or derived from the relevant micro-level theory— or rather episetmically dispensable in principle. This is another unforunate divide that this project shall seek to rebut and grows out of a misplaced concern with the topic of the inter-theoretic reduction and explanatory reducibility more generally.

2. Chapter Summaries and Preview

In what follows, I provide a brief summary of each chapter to follow, returning to how each chapter aims, in its own way, to overturn the incorrect assumptions mentioned above.

2.1) Chapter 1: Cognitive dynamical models as minimal models

The debate over the explanatory nature of cognitive models has been waged mostly between two factions: the mechanists and the dynamical systems theorists. The former hold that cognitive models are explanatory only if they satisfy a set of mapping criteria, particularly the 3M/3M* requirement. The latter have argued, pace the mechanists, that some cognitive models are both dynamical and constitute covering-law explanations. In this paper, I provide a minimal model interpretation of dynamical cognitive models, arguing that this both provides needed clarity to the mechanist versus dynamicist divide in cognitive science and also paves the way towards further insights about scientific explanation generally. As such, the aim of this paper is to firmly dispute the assumption that scientific models must track causal or constitutive relations and satisfy mapping requirements in order to be explanatory.

2.2) Chapter 2: Distinctively mathematical explanation and the problem of directionality: A quasi-erotetic solution

The increasing preponderance of opinion that some natural phenomena can be explained mathematically has inspired a search for a viable account of distinctively mathematical explanation. Among the desiderata for an adequate account is that it should solve the problem of directionality —the reversals of distinctively mathematical explanations should not count as members among the explanatory fold but any solution must also avoid the exclusion of genuine explanations. In what follows, I introduce and defend what I refer to as a quasi-erotetic solution which provides a remedy to the problem in the form of an additional necessary condition on explanation.

The aim of this chapter is to resolve the directionality problem which is said to confront explanations which are non-ontic/non-causal. This chapter demonstrates how a solution can be purchased without the cost of giving up on non-causal/non-ontic scientific explanations.

2.3) Chapter 3: Reckoning with Continuum Idealizations

Scientific models often range over length-scales, spanning several orders of magnitude. Continuum idealizations permit scales to be bridged but at the cost of fundamentally misrepresenting the microstructure of the system. This engenders a mystery: If continuum idealizations are dispensable in principle, this de-problematizes their representational inaccuracy –since continuum properties reduce to lower-scale properties— but the mystery of how this reduction could be carried out endures. Alternatively, if continuum idealizations are indispensable in principle, this is consistent with their explanatory and predictive success but renders their representational inaccuracy mysterious. I shall argue for a deflationary solution to this mystery, enlisting the applied scientific method of upscaling as demonstrated in a case from soil hydrology.

In this chapter, the usefulness of the assumption that idealizations must either be epistemically indispensable or dispensable in principle is challenged. A deflationary and pragmatic stance is adopted and argued for here.

CHAPTER ONE: COGNTIVE DYNAMIAL MODELS AS MINIMAL MODELS

1. Introduction

The debate over the explanatory status of cognitive models has been principally waged by two camps: the mechanists and the dynamical systems theorists (hereafter DS). The mechanists have argued that while some cognitive models are dynamical, they ultimately fail to constitute explanations since they do not satisfy the 3M model to mapping requirement, amounting to little more than phenomenal models (Machamer et al, 2000; Craver, 2006; Kaplan and Craver, 2011). DS proponents have countered that, *pace* the mechanists, some cognitive models are both dynamical and explanatory since these models are formable into covering-law explanations (van Gelder, 1995; Clark, 1997; Bechtel, 1998; Walmsley, 2008). Further, this fact about formability is indicative that dynamical, cognitive explanations are capable of bearing counterfactual support which is a mark in their explanatory favor (Woodward, 2003). An impasse was thus formed between these two groups about models and explanations in cognitive science although more recently there have been investigations into whether there is room for complementarity between the two approaches (Chemero, 2000; Zednick, 2011; Chirimuuta, 2014)[2].

As a way of breaking out of this entrenchment, I shall argue that some cognitive models are both dynamical and explanatory not because they can be construed as

[2] Chirimuuta (2014) gives a very similar but importantly different treatment of dynamical models. Although she also argues dynamical models can be assigned a minimal model interpretation, her objective is to reconcile computationalism with dynamicism and so constructs a variant of the minimal model which contains computational elements. My claim is far broader and does not explicitly aim for complementarity but rather for a more robust defense of cognitive dynamical models as explanatory models.

covering-law explanations but rather since some of these models represent minimal model explanations (Batterman, 2002). The argument can be represented as follows:

1. Some cognitive models are dynamical models.
2. Some of these cognitive dynamical models are minimal models
3. Minimal models are explanatory when they include a demonstration that various lower-level details of a range of systems are irrelevant to the phenomenon of interest.

Therefore,

4. Some cognitive models are explanatory since they demonstrate certain lower-level details of a range of systems to be explanatorily irrelevant to the phenomenon of interest.

This argument has a couple of notable implications for the explanation debate over cognitive models. The first is that it provides a promising alternative to the covering-law approach for disputing the mechanist's claim of explanatory hegemony in the explanation debate in cognitive science. Minimal models fail the 3M mapping requirement for reasons suggested by Batterman and Rice (2014); whereas the mechanist view of explanation which motivates and grounds the 3 M requirement is a "common features account" minimal models are not. A second implication is that understanding dynamical systems as minimal models is more fruitful than viewing them as covering law explanations because this approach captures what is explanatory in dynamical models. Additionally, minimal model explanations sidestep several important challenges which bedevil the covering law account. Thus, the minimal models based approach establishes that some cognitive dynamical models are explanatory without inheriting the costs of the covering-law model.

This paper will unfold as follows. In the second section, I broadly outline both the mechanist and DS positions, spelling out two important objections on behalf of the mechanists against the covering-law defense for dynamical models. In the third section, I introduce the HKB model of bimanual coordination, a paradigm instance of a cognitive dynamical model, and also sketch Batterman's account of minimal model explanation. In the fourth section, I demonstrate how the HKB model can be assigned a minimal model interpretation and then revisit the mechanist's charges against the DS position, showing how adopting a minimal models interpretation provides needed clarity to the issue of model-based explanation in cognitive science. I conclude by considering what further insights these developments can bestow on the issue of scientific explanation more generally.

2. Cognitive models: The mechanistic and dynamical systems approaches

A fuller appreciation of the conceptual disagreement between the mechanists and dynamical systems proponents concerning both explanations and models in cognitive science is obtainable by taking an inventory of each of these approaches to cognitive modeling. In this section, I provide a brief sketch of each position, eventually distilling these differences down into two criticisms made on behalf of the mechanists against the dynamical systems approach, particularly the covering-law defense.

2.1 Mechanistic cognitive models and the 3M requirement

The mechanist approach, broadly construed, holds that scientific explanation involves the search for and discovery of mechanisms. “A mechanism is a structure performing a function in virtue of its component parts, component operations, and their organization” (Bechtel and Abrahamson, 2005, 43; Machamer et al., 2000). From this gloss, two key features of mechanisms are extractable: 1. Mechanisms are counterfactual supporting; 2. Mechanisms play important roles in the production of regularities. The discovery of mechanisms relies upon the idea of decomposition or the obtaining of functional information about a mechanism by breaking it into its component parts, thus attempting to glean understanding about a mechanism via the “reverse engineering” of it (Simon, 1969; Cummins, 1975). This decomposition can be partitioned into two types: structural and functional (Bechtel and Richardson, 2003). In cases of structural decomposition, a system is sub-divided into a set of the subsystems and component parts. However, structural decomposition is not sufficient for the discovery of a mechanism. It needs to also be the case that the sub-components contribute towards the production of the macro-behavior of the system. In instances of functional decomposition, the behavior of the system is characterized as the upshot of the organized behavior of the system’s sub-components. Once again, mere characterization couched in terms of functionally decomposability is not sufficient for the unearthing of a mechanism A mechanism is discovered when these behaviors can actually be localized within the sub-components of the system (Povich and Craver, 2018).

The mechanistic view of explanation dovetails well with computationalism, which represented the prevailing approach within cognitive science for decades[3] (Newell and Simon, 1976; Fodor, 1981). Computational models involve modeling cognitive phenomena via a reconstruction of algorithmic operations on symbols. A nexus between computationalism and the mechanist approach naturally arises in the notion of functional decomposition: if the cognitive phenomenon can be modelled as an organized set of algorithmic operations on symbolic representations, this suggests a route for the modelling of the macro-behavior of the system in terms of the organized activities of its sub-components (Cummins, 1983). More recently, the mechanistic approach has found application in connectionist neural models. These connectionist or artificial neural network models "provide abstract descriptions of the neurobiological systems in which cognitive mechanisms are realized" swapping out symbolic representations for nodes (Zednick, 2011, p. 241).

A well-worn example of a mechanistic explanation in cognitive modeling is the HH model of the action potential, particularly the model's maturation from its initial phenomenal form containing "black boxes" into a full-blooded explanation with these boxes filled in (Hodgkin and Huxley, 1952). Although not initially deemed an explanation by mechanistic standards (Bogen, 2005), Hodgkin and Huxley's primary accomplishment was to formalize the time course of the action potential (I) into a "current" equation which included an inventory of the relevant variables —the capacitative current [$C_M dV/dt$)], the potassium current [$G_K n4$ (V – VK)], the sodium

[3] An additional virtue of the mechanist view is that it squares well with the de-idealizing character of major models in cognitive science —e.g. the HH model of the action potential and the Zipser-Andersen gain field model of motor control (Zipser-Andersen, 1988).

current [$G_{Na}m^3h$ (V – V_{Na})] and the leakage current [G_1(V-V_1)](Craver, 2007). This yielded:

$$I = C_M dV/dt + G_K n4\ (V - VK) + G_{Na}m^3h\ (V - V_{Na}) + G_1(V\text{-}V_1)$$

Where GK, GNa and G are the maximum conductance values for the set of ionic currents. VK, VNa and V1 stand for the differences in equilibrium potentials for the various ions in terms of voltages (hence, V). An ion is in equilibrium when the voltage's diffusion and the driving force of the voltage are balanced such that no net current flow exists. CM or the capacitance of the membrane stands for the membrane's capacity to hold opposite charges on the intra-and extra-cellular sides. Finally, there are three co-efficients (h, m and n) which vary with voltage and time. Notably, Hodgkin and Huxley were not only able to construct an equation for these variables but also to calculate the values these variables would take on in the total current equation for I above. This permitted a description of, inter alia, the form, amplitude and threshold of an action potential, the propagated action potential and resistance changes during an action potential.

In its initial incarnation, this model left open details regarding the mechanisms which enable the action potential.[4] Only by tracing intra-membrane conductance changes to conformation changes in ion specific channels in the cell membrane —i.e. filling in the black boxes of the original model or substituting in mechanistic detail for filler terms— could the HH model be made genuinely explanatory, graduating from a mere how-possibly to a genuine how-actually explanation (Craver, 2006).

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[4] As Craver points out (2006, 2007) the HH model was not explanatory by Hodgkin's own lights since the model was compatible with a wide range of possible mechanisms.

Per the mechanists, a model explains *only if* it highlights the causal structure of the underlying mechanism (Kaplan and Craver, 2011). Thus, explanation will entail both a structural and functional decomposition with respect to the system producing the phenomenon as outlined above. This credo is formulated into the following 3M requirement which acts as model-to-mechanism mapping constraint on model-based explanations:

> The 3M requirement: A model of a target phenomenon explains that phenomenon to the extent that (a) variables in the model correspond to identifiable components, activities, and organizational features of the target mechanism that produces, maintains, or underlies the phenomenon and (b) the (perhaps mathematical) dependencies positive among these (perhaps mathematical) variables in the model correspond to causal relations among the components of the target mechanism (Kaplan and Craver, p. 611).

The 3M view implies that the more detail a model includes about its target phenomenon, the better the model will be (Kaplan, 2011). So, detail and the goodness of the model are positively related as stated in (a). The second condition maintains the mechanist's tendency towards causal exclusionism, ruling out mathematical components which fail to map onto and thereby capture causal relations among the mechanism's subcomponents. This last point, however, requires caution. According to Kaplan, 3M does not set the bar for explanatory adequacy such that only "completely, non-idealized" models are acceptable (2011, p. 347). This would place the 3M requirement at odds with much of actual science. The view is permissive of idealizations. But, following from (a), the mechanist view implies that a model which linked up more of its variables with components, activities and organizational features of the target mechanism would be superior to a model of that same target which included more idealizations.

2.2 The dynamical systems approach to cognitive models

Dynamical systems (DS) proponents radically diverge from the mechanists on the nature of cognitive models and scientific explanation more generally. The case for modeling cognitive phenomena as dynamical systems is made from necessity since neither computationalism nor connectionism are capable of modeling phenomena where time is a continuous variable; the former exclusively deal in discretized models (van Gelder and Port, 1995)[5]. The impotency of computational or connectionist models in adequately describing cognitive phenomena subject to continuous temporal evolution parallels the shortcomings of algebra as compared to calculus when physicists attempt to calculate instantaneous changes in an object's velocity. Clearly, algebra fails to mechanically measure up to the task and this necessitates the deployment of more advanced machinery; in this case, derivatives or integrals. This lacuna in cognitive modeling is one that DS models are uniquely well-situated to occupy since these models are meant to capture systems which are state-dependent and "evolve continuously over time according to some rule" via the incorporation of differential equations into the models where these differentials encode for the dynamical behavior of interest (van Gelder and Port, 1995, p. 5). Applications of dynamical models in cognitive science are by now relatively pervasive, ranging from models about agential decision making (Busmeyer and

[5] This claim by DS advocates is one that can and has been challenged (Chemero, 2000). Upon closer inspection, the claim that discretization is *ipso facto* inadequate for the modelling of dynamical phenomena is dubious. Moreover, the question of how to model a phenomenon (discretely or continuously) may reflect more about the pragmatic decisions of the modelers rather than something about the nature of the phenomenon itself. I thank Colin Allen for drawing my attention to this point.

Townsend, 1993) to developmental psychology (Thelen and Smith, 1994; Thelen et al., 2001) to a description of minimal cognitive agents (Beer, 1995; Beer and Williams, 2015).

A paradigm instance of a DS model-based explanation which demonstrates the need for a dynamical toolkit is the Watt governor example (van Gelder, 1995). The "governor" was a device designed by James Watt which was meant to solve the pressing 19th century problem of regulating the speed of steam engines. *Pace* Watt's governor, which was a self-regulating device, van Gelder first considers what a computational solution to the problem of steam engine regulation would look like. This is laid out in a series of discrete steps, six in total, describing the calculations and input adjustments an engineer would need to perform for speed maintenance; i.e. measuring the speed of the flywheel, comparing the actual versus desired speed and adjusting the throttle to either increase or decrease steam pressure (van Gelder, 1995, p. 348). In place of this manually intensive solution, Watt's governor is a self-regulating device consisting of a vertical spindle connected to the engine's flywheel. Attached to the spindle were two arms tipped with metal balls geared to the throttle valve. As the spindle rotated faster signaling an increase in speed, the metal balls moved out and rose up due to centrifugal forces, closing the valve and thus slowing the release of steam. As the spindle began to rotate more slowly, the arms lowered, which opened the valve, permitting more steam to be released. The Watt governor thus enabled the engine, in dynamical terms, to self-regulate its own speed in response to perturbations about an equilibrium point through a feedback system.

The behavior of the Watt governor can be mathematically represented as the following second-order, non-linear differential equation:

$$\frac{d^2\,\theta}{dt^2} = (n\omega)^2 \cos(\theta)\sin(\theta) - \frac{g}{l}\sin(\theta) - r\frac{d\theta}{dt}$$

Where θ is the angle of the arms, n is a gearing constant, ω is the speed of the engine, g is a constant for gravity, l is the length of the arms, and r is a frictional constant for the hinges. The differential equation represents the behavior of the dynamical system by telling us how arm angle position is "changing, depending on the current arm angle, the way it is changing already and engine speed" (van Gelder, 1995, p. 356). van Gelder notes that the self-regulating and dynamical Watt governor contrasts sharply with the computational solution which was considered initially in at least four ways. The Watt governor is non-representational. Owing to this difference, the Watt governor has no symbolic representations to manipulate and so is not computational. Further, the governor is not sequential or cyclic since time is represented continuously not discretely. Nor is it homuncular. This comparative analysis funds the conclusion, per van Gelder, that DS explanations are both conceptually distinct from and mechanically superior to computational or connectionist frameworks.

2.3 Flashpoints between the mechanist and DS approaches to cognitive modelling

Given the vast differences in both of these approaches to model-based explanation in cognitive science, the potential points of conflict are legion. However, I present two criticisms levied against the DS view which are particularly troubling.

The first charge against the DS view is that the models which are its stock and trade do little more than save the phenomena (Kaplan and Craver, 2011). Thus, the DS approach produces models which amount to exercises in curve-fitting and despite their predictive success, they nonetheless fail to be explanatory. To supplement this point, it should be acknowledged that predictive success no more entails explanatory power than the predictive accuracy of the barometer's falling for the arrival of rain-storms explains the occurrence of the rain-storms. Indeed, the two are notionally separable. This can be formulated into "Problem 1" or P1 below.

P1: DS models are phenomenal models which are at best predictively successful of the phenomena but fail to be genuinely explanatory of the phenomena.

This criticism has not gone unanswered by DS advocates. Most notably, it has been argued that DS models are explanatory since they can be formulated into covering law explanations (van Gelder, 1995; Clark, 1997; Bechtel, 1998; Walmsley, 2008)[6]. Recall that covering law explanations demonstrate that the phenomenon of interest is deductively entailed by a set of premises consisting of both natural laws (hence the title), initial conditions and auxiliary assumptions (Hempel and Oppenheim, 1948). Since DS models can be given this kind of gloss, they are capable of bearing counterfactual support which is a mark in their explanatory favor (Woodward, 2003). Thus, "a good dynamical explanation will enable us to say how the dynamical system in question would have behaved in various non-actual circumstances, for example if it suffered specific perturbations, or if its control parameters were altered" (Walmsley, 2008). Although this kind of response may provide reason for the mechanists to slightly revise their claim, it fails to repudiate the spirit of it. The mechanists can invoke the how-possibly versus how-actually distinction of explanation, which is a readily available move to reformulate their criticism (Craver, 2006). The DS models may be explanatory in a how-possibly sense in

[6] Another response has been to embrace the charge of predictivism, maintaining that predictive success is in fact evidence for explanatory goodness (Chemero and Silberstein, 2008).

that they provide hypothetical or how-possibly accounts for how a phenomenon could occur, but nevertheless they fail to be of the how-actually variety or explanatory in a deep sense. Resorting to the covering-law defense doesn't appear to rebut P1 convincingly on terms that the DS proponents would accept.

A second criticism is that while phenomenological laws like the kind DS advocates employ in their explanations may enjoy predictive success in counterfactual circumstances, "it often remains unclear why the law applies in the first place. Put differently, phenomenological laws by themselves provide no means of determining when they can or cannot be used in deductive inferences about the target phenomenon" (Zednick, 2011, p. 246). This can be summarized in P2 below:

P2: The covering-law defense of DS explanations fails to provide a justification for the applicability of phenomenological laws in making explanatory inferences about a target phenomenon.

Zednick's worry is that while a phenomenal model might permit counterfactual inferences to be made about some target by applying phenomenological laws to that system, it provides no principled way of demarcating between a merely hypothetical or how-possibly description of that phenomenon and a more robust, how-actually explanation of it. For instance, for a DS covering-law explanation, it just has to be the case that the system's behavior can be understood as an instance of a phenomenological law. But merely applying that law to the system is too minimal a criterion for understanding why that system can or cannot be understood in that way. Notice that, contrastively, the mechanists are able to demarcate between how-possibly and how-actually distinctions; for them, an explanation is how-actually when the 3M requirement is satisfied. Both P1 and P2 should be borne in mind as important challenges to the DS view and I shall revisit them throughout.

3. The Haken-Kelso-Bunz model and minimal model explanations

In the following section, an itemization of the components in the HKB model of bimanual coordination is given. This model is then evaluated against the charges of 2.3 and shown to appear guilty on both counts. Finally, the basis of a response to these problems is begun by outlining Batterman's notion of a minimal model.

3.1 The HKB model of bimanual coordination

The HKB model drew inspiration from Herman Haken's early work on what he termed "synergetics." Synergetics was an interdisciplinary approach to the patterned dynamical behavior of phenomena which treated complex systems far from equilibrium (Haken, 1983). The core initiative of this project was the study of patterns of dynamic stability and instability in both equilibrium and non-equilibrium complex systems. Concrete examples of the synergetic approach ranged from fluid mechanics to chemical reactions; e.g. Raleigh-Bernard convection. The advantage of this approach was that it permitted patterns to be isolated in dynamical systems via the exploration of dynamic instabilities in these systems. This enabled predictions to be formed about non-linear dynamical systems specifically the behavior of certain variables around so-called points of criticality.

Synergetics found application in cognitive science, particularly in the area of motor control. The Haken-Kelso-Bunz (HKB) model of bimanual coordination represents a dynamical model for non-equilibrium phase transitions in the rhythmic motion of human hands (Haken et al., 1985). Central to the model is the characterization of two kinds of oscillatory movement of the left and right index fingers. *In phase* mode corresponds to the coordinated oscillation of the left and right index fingers such that both fingers point in, towards one another or point outwards, away from one another. *Anti-phase mode* involved the coordinated oscillation of left and right index fingers parallel to one another such that both fingers point leftward or both fingers both rightward. Subjects were then asked to oscillate their fingers at a frequency set by clicks of a metronome. The crucial result is that as frequency increased or is scaled up, a critical point is reached where subjects spontaneously shift from the anti-phase mode to the in-phase mode. Only one pattern of coordination, namely the in-phase mode, continues to be stable beyond a certain point of criticality as regards frequency (Kelso, 1995).

This dynamic instability/stability result and the relationship between the oscillatory frequency and phase relation is expressed in the following differential equation:

$$\dot{\varphi} = -a \sin \varphi - 2b \sin 2\varphi$$

Where alternations in the phase relation between fingers (φ) is represented as a *sin* function of a and b, the angle of the fingers. This equation is meant to capture a “coordination law” which tells us that when a certain critical point is reached, anti-phase motion becomes unstable, giving way to stable, in-phase motion. Finally, this model exposes a *hysteresis effect* where a phase transition is predicted when frequencies rise from low to higher values but not in the reverse case where frequencies fall from higher to low values. Thus, the in-phase mode is asymptotically stable since perturbations (increases or decreases in frequency) do not displace the phase relation or primary value

(φ) from its basin of attraction; more technically, it fails to contain a bifurcation[7] (Norton, 1995). By contrast, the anti-phase mode is dynamically unstable since perturbations (increases in frequency beyond the critical point) do displace the phase relation value (φ) from its attractor basin —a bifurcation occurs— as it collapses into in-phase motion.[8]

The HKB model of bimanual coordination enjoys at least two virtues. First, the model meets with high predictive success. For initial conditions, input any oscillation frequency (the a and b values) as well as any phase relation (φ) and the model outputs the phase relation that will eventually obtain: $\dot{\varphi}$. Second, the model captures a law-like regularity in expressing a coordination law, allowing many helpful counterfactual inferences to be drawn. So, the model is capable of transmitting valuable "what-if-things-had-been-different" information about how the phase relation will be altered by perturbations.

However, per the foregoing discussion of 2.3, a model being predictively successful and lawlike is insufficient for explanatory goodness according to the 3M mechanistic perspective. Viewed through that lens, the HKB model appears to stumble into the mechanist's crosshairs, succumbing to both P1 and P2. With respect to P1, notice that HKB, in spite of its predictiveness, does not map any of its inputs onto any neural

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[7] A bifurcation occurs when "a parameter value is reached at which a sudden change in the qualitative type of the attractor occurs" (Norton, 1995). Notice that a bifurcation occurs only in the anti-phase motion as frequency is scaled up but not in the in-phase motion which remains markedly stable in response to perturbations.

[8] For a helpful illustration of these relations, see Kelso (1995, p. 57) where the HKB model is expressed pictorially as a vector field. The vector field highlights the instability of the anti-phase attractor in the state space by letting (φ) be the state variable and *b/a* be the control parameter where *b/a* is the inverse of the finger's oscillation frequency. The vector field presents a nice visualization of both the "fixed points" of (φ) as unstable as well as repelling and the "pitch-forked shape" of the bifurcation that occurs.

substrates. To wit, all of the model's inputs (a, b and φ) merely permit the question of underlying structure to remain a series of unfilled black boxes. HKB is thus vulnerable to the mechanist's complaint expressed in P1.[9] Further, recall that a model's being lawlike will not appease its detractors either. As Zednick points out, the HKB model just shows that "the target system can be described as a system of coupled oscillators" (2011, p. 246). What is conspicuously absent from the model is some kind of warrant for understanding the system in this way in the first place. For now, much like other DS explanations, both P1 and P2 befall the HKB model. In the interest of rehabilitating the model, we shall begin developing the basis of a promising route of response with a description of minimal model explanations.

3.2 Minimal model explanations

It is worth prefacing the sketch of minimal models by underscoring a crucial distinction from Robert Batterman. Batterman distinguishes between two kinds of explananda or "why-questions" in scientific explanation: A type (i) why-question "asks for an explanation of why a given instance of a pattern obtained" whereas a type (ii) question "asks why, in general, patterns of a given type can be expected to obtain" (Batterman, 2000, p. 23). Indeed, failure to appreciate this distinction has engendered a great deal of confusion about the concept of a minimal model. A type (i) question inquires about a token instantiation of some pattern or regularity. To give an example from economics, one may ask "how did perturbing supply values away from the equilibrium point with demand values affect the price variable in this particular market?" By contrast, a type (ii)

[9] Additional evidence for this claim can be found in Kaplan and Craver (2011) who explicitly single out the HKB model as running afoul of their "predictive but not explanatory" complaint.

question asks what is necessary for the recurrence and maintenance of the pattern or regularity itself. Thus, one might ask "why do supply and demand form an equilibrium point which recurs over a plurality of markets?"

Minimal model explanations traffic exclusively in answering type (ii) questions. Minimal models proceed by describing why a particular higher-scale regularity or pattern obtains in a diverse or heterogenous group of phenomena (Batterman and Rice, 2014). One of their examples is the similarity in fluid flow during phase transitions between a microscopically heterogenous group of fluids, better known as a renormalization group explanation in physics. Although these systems each differ at the micro-scale (their respective molecular composition; e.g. H_2O and CO_2), they exhibit similar behavior at the macro-scale (their phase transition) near their respective point of criticality. This is, in effect, the inter-systems phenomenon known as *universality* where systems which are heterogenous at the micro-scale share a similar pattern of macro-level behavior (Strevens, 2019). And construction of a minimal model involves sorting systems into a universality class. These systems can be placed in the same "universality class" when they are shown to possess a feature or property which is necessary for generating the shared macro-scale behavior[10] (Kadanoff, 2013). In addition, the universality class is delimited by showing certain features of the systems occupying the class to be irrelevant or non-necessary for the phenomenon of interest to occur; a process of extracting the explanatory information from the noise. Thus, the explanatory value of minimal model explanation lies in demonstrating the truly minimal character of what is necessary for the occurrence of the

[10] Macro-scale is technically not quite correct. A minimal model explanation may also address meso-scale regularities. Thus, macro-scale should be taken to mean "non-micro-scale."

macro-scale behavior; i.e. the pattern or regularity under consideration. Minimal models explain, in large measure, by showing much of the details surrounding the target phenomenon to be explanatorily irrelevant to it (Rice, 2019).

The differences between minimal models and other more common forms of model-based explanation(e.g. common features accounts such as the mechanistic view) are apparent in the following three questions which Batterman and Rice claim minimal model explanations are uniquely suited to answer (2014, p. 361):

Q1. Why are the common features among systems necessary for the phenomenon to occur?

Q2. Why are the remaining heterogenous details (those left out of or misrepresented by the model) irrelevant for the occurrence of the phenomenon?

Q3. Why do [systems] which are very different [at lower scales] have features in common?

To see the view in application, it is worth rehashing Batterman and Rice's example of R.A.Fisher's equilibrium model for the 1:1 sex ratio in biological populations where sex is bi-valued (Fisher, 1930). The use of population models to capture higher-level patterns within diverse populations is common in biology. Batterman and Rice claim that these models represent another instance of universality that "can be exploited to find minimal models that can be used to investigate, explain, and understand real biological systems" (2014, p. 365). The occurrence and recurrence of the 1:1equilibriumin sex ratio can be accounted for by observing that when it is perturbed, a fitness advantage is enjoyed by parental organisms producing offspring in the sexual minority (Sober, 1997). For example, in a population featuring a 2:1 sex ratio of females to males, organisms producing male offspring will enjoy greater reproductive value for their investment than organisms producing females since male offspring will have more reproductive opportunities than females(Rice, 2019). This advantage diminishes as the population equilibrates and returns to the 1:1 stable state.

Fisher's model encodes for this information in terms of resource cost parameters. The equilibrium in sex ratio is then demonstrated to be the result of a tradeoff between the resource costs of sons and daughters or, in economic terms, the "linear substitution cost" between sons and daughters (Rice, 2015). The linearity assumption holds that the cost in resources of producing a son is equal to the cost of producing a daughter and that an additional son implies one less daughter. Charnov (1982) generalizes Fisher's model as follows:

$$r = CM/CF + CM$$

Where r stands for the sex ratio of a population, CM represents the average resource cost of one son and CF the average resource cost of one daughter. The "linear substitution cost" expressed in the equation is intended to capture the dynamical character of the 1:1 ratio as a point of stability for the population (Batterman and Rice, 2014, p. 360). To see how this constitutes a minimal model explanation, Batterman and Rice return to their initial list of three questions. The three questions are reformulated to reflect the specific character of the sex ratio example as follows:

Q'1. Why is the common feature (the linear substitution cost) necessary for the phenomenon to occur?

Q’2. Why are the remaining heterogenous details (those left out of or misrepresented by the model) irrelevant for the occurrence of the phenomenon?

Q’3. Why do very different biological populations have this feature in common?

Attempting to answer these questions begins with Q’2. This question can be answered by demonstrating the robustness or invariance of the sex ratio across populations which differ widely and are heterogenous at lower scales. Batterman and Rice provide examples demonstrating the equilibrium’s invariance despite shifting details such as population size, the optimization process and the underlying physical mechanisms. This procedure, which is part of delimiting a universality class, ultimately permits an answer to Q’2: the heterogenous details are irrelevant since the phenomenon is shown to be robust across a spectrum of cases where these details are altered or eliminated(2014, p. 361). The irrelevance of the lower-scale details thus enables an answer to Q’3. Biological populations share the equilibrium in sex ratio due to this higher-scale phenomenon’s lack of dependence on the lower-scale heterogenous details(2014, p. 362). Finally, this allows an answer to Q’1. The necessity of the common feature, linear substitution cost in this case, is accounted for by the fact that the set of systems which share this feature can all be placed in the same universality class and the realization that all such systems which occupy this class share this feature.

4. The HKB model as a minimal model explanation

In what follows, I demonstrate how the HKB model is both formable and interpretable as a kind of minimal model explanation. I then revisit the earlier criticisms of 2.3, arguing that the minimal model interpretation of dynamical models cuts significant ice in resolving the clash between the mechanists and DS proponents in cognitive modeling. Further, I claim that this interpretation also paves the road to deeper insights about scientific explanation generally.

4.1 Constructing a minimal model from HKB

The HKB model describes a non-equilibrium phase transition for bimanual coordination where the coordination of human index fingers is represented as a system of couple oscillators. At first glance, the specificity of this characterization would seem to contrast sharply with the universality of Batterman's fluid example. However, non-equilibrium phase transitions are expectable in any system that could be represented as a set of coupled oscillators and which satisfied some additional criteria. As such, non-equilibrium phase transitions in systems comprised of coupled oscillators have received attention across the natural sciences. Examples include a suspended, unforced and undamped cable line, genetic control networks or the quadri-pedal movement of elephants (Watts and Strogatz, 1998) as well as speech production (Port, 2003) and behavioral coordination between individuals (Oullier, et al, 2008). In fact, the kind of phase transition observed in the bimanual coordination case of HKB could be generated by a system as disparate as one containing two, undamped pendulums whose wired bobs were set at certain angles and programmed to swing with a certain, rising frequency[11]. Both the heterogeneity and range of the set of actual or potential systems which exhibit the phenomena is suggestive of universality. Kelso himself describes the HKB as having wide scope, claiming that the general dynamical model of HKB "describes a general principle of pattern formation"

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[11] An additional example is Kugler and Turvey (1987) recreating quadri-pedal locomotive, dynamical motion with an inverted pendulum and spring.

and that "variations of this model can be used to describe and predict several different kinds of coupled oscillatory motion" (Kelso, 1995, p. 2). And so the following explanandum question naturally arises: Why do a number of systems of such diverse physical composition (micro-scale detail) manifest similar dynamical stability/instability around a critical point?

An answer to this question is not far to tread. Any system whose initial conditions include contained twin oscillators set to certain angles (a and b in our equation respectively), which included the phase relation (φ) (or the between oscillator phase relation) as an order parameter and finally included b/a (or the inverse of the oscillation frequency) as the control parameter would reproduce the dynamical stability/instability results. Let that be our minimal model. Next, let the universality class consist of the set of coupled oscillators catalogued above —quadri-pedal movements of elephants, two undamped pendulums, two human index fingers— which generate the shared macro-level behavior; in this case, the dynamical stability/instability results in the non-equilibrium phase transitions these systems undergo. What explains both our expecting the phenomena to occur and the fact that it recurs over such a diversity of systems? Answer: the fact that the conditions for the phenomena's occurring are so *minimal*.[12] Any system instantiating these conditions would exhibit the dynamical, macro-scale behavior, i.e. the stability and instability results during phase transitions. In Batterman's phrasing, the macro-scale phenomena, in this case the dynamical stability/instability results, include many lower level realizers. This analysis may prove nettlesome to some but notice that

[12] Ariew et. al (2017) provide a similar but importantly novel line of explanation as regards minimality for the observation of statistical phenomenon "reversion towards the mean" in biological populations across nature.

the result is paralleled in other instances of minimal models. Why is it the case that a suite of biological populations whose sex is bi-valued all tend towards a 1:1 sex ratio? (Batterman and Rice, 2014). Because the conditions for this tendency, namely, the optimization benefits and costs of producing offspring in the sexual majority or minority, are so minimal and widely spread throughout biological populations. Thus, the 1:1 sexual ratio is multiply realized by organisms as diverse from one another as flies are to humans.

The minimal model interpretation of HKB agrees well with the inter-disciplinary recurrence of non-equilibrium phase transitions; in short, the minimality accounts for the plurality. However, it is unclear from the mere fact that the HKB model can be dressed up as a minimal model explanation that this does any real work in resolving the two earlier objections of the mechanists. In the following section, those objections will be reevaluated with the HKB model now understood as a kind of minimal model explanation.

4.2 Revisiting the mechanist vs. DS debate and further insights

Recall that the mechanists' attack against the covering-law defense of cognitive dynamical explanations contained two prongs: P1 and P2. P2 stated that covering-law explanations failed to include any justification for the application of phenomenological laws in particular cases. More simply, covering-law explanations apply laws to systems yet fail to tell us *why* that law applies to a particular system. The minimal models interpretation provides a means of response. The minimal model isolates the feature(s) which are necessary for the occurrence of the phenomenon of interest and tell us why much else, namely, the micro-level detail, is irrelevant. Thus, a justification for the application of law to system is generated via the construction of the minimal model itself.

The fact that the system under consideration can be placed in a universality class with a minimal model which exhibits the explanandum phenomenon represents the justification for applying the law to the system in question.

Further recall P1 held that dynamical, covering-law type explanations do little more than save the phenomena and consequently are merely how-possibly explanations at best. This problem, however, begins to dissipate once Batterman's type (i) versus type (ii) explanatory distinction is acknowledged. Cognitive dynamical models cast as minimal models answer type (ii) questions: they tell us why higher-level regularities can be expected to occur and recur in certain systems. The mechanist's demand for lower-level nuance is thus orthogonal to the intended purpose of minimal models.

Here, the mechanist may feel inclined to interject, claiming that minimal models are how-possibly explanations since they ignore lower-level causal details. And this makes them compatible with a wide range of possible mechanisms, similar to Hodgkins' diagnosing his own HH model as initially just a how-possibly model (see 2.1). A reply on behalf of the minimal models proponent is two-fold. The first is to reiterate that minimal models capture higher-level regularities and so they often are compatible with many lower-level mechanisms, hence the multiple realizability claim. However, this is no strike against their status as genuine, how-actually explanations. They are how-actually explanations about the regularities they capture. If they are only how-possibly with respect to lower-level mechanisms, so be it. That is not the intended target of these models and so can hardly count against them.

A second and important aspect of the reply is to reject the mechanist's demand for explanations that go all the way down which is implied in the (a) condition of the 3M requirement which posits that the amount of detail within a model is positively related to its explanatory quality. Minimal models as well as cognitive dynamical models I have argued, operate at a higher level. Here, the mechanist is free to contest the status of level-specific scientific explanations as genuine ones but this would place them at odds with scientific practice. Two evidentiary examples are helpful: one from atmospheric dynamics the other from engineering. When meteorologists calculate vorticity in attempting to predict frontal activity, they relegate themselves exclusively to the meso-scale.[13] From this vantage, the atmosphere is viewed as a set of “air parcels” or units of one cubic foot (Martin, 2006). This enables them to use the Navier-Stokes equations by treating the atmosphere as a continuous fluid which is sufficient for calculating vorticity. They neither need to nor do pay much attention to the molecular level since that is quixotic for atmospheric dynamical analysis. Similarly, when engineers want to build bridges from materials like steel, their primary focus is to measure the strength and resiliency of the material. Since their interest is in things like material shear and stress, they employ the Navier-Cauchy equations where some lower level detail —also referred to as “representative volume elements”— is encoded for in the “Young’s modulus” parameter but more tellingly *much* is ignored. Steel is thus treated as a “continuous blob” which is ontologically incorrect but enables engineers and material physicists to assess the materials more tractably (Batterman, 2013; Wilson, 2017)[14]. Level-specific explanations which are constrained and guided by concern for scale are very much the

[13] Vorticity can be defined as circulation per unit area.
[14] For an insightful and informed treatment of the use of scales in engineering explanations as well as the “tyranny of the scales” issue in material physics, see chapter 5 of Wilson (2017).

order of the day in scientific practice. Hence, the mechanist's demand for explanation all the way down appears inappropriate.

5. Conclusion

From the following, I conclude that understanding cognitive dynamical models as minimal models represents a significant upgrade over making their explanatory status hinge on the fact that they can be given covering-law explanations. The minimal model interpretation can answer to the criticisms of the mechanists directly, offering a robust justification for the explanatory status of DS explanations in cognitive science. And this constitutes an appreciable improvement over merely eschewing these concerns.

This all points towards two further insights: one about the scientific explanation debate generally and the second about the status of cognitive models. The scientific explanation debate is often subject to many of the pitfalls of the cognitive models debate —participants in the exchange sort themselves into two camps where one demands explanations satisfy some causal criteria and the other maintains that some explanations neither do nor need to satisfy this criteria. (Reutlinger, 2017)[15] These groups often proceed to talk past one another which results in an "explanation is in the eye of the beholder" problem, creating an impasse similar to the one in cognitive modeling. This is a quandary that we are best advised to avoid. And a promising means of avoidance is available in directly addressing the criticisms of the opposing crowd. I hold that the minimal models defense succeeds in this strategy in the cognitive science debate whereas

[15] This provides an extensive overview of the recent state of play in the scientific explanation debate.

the covering-law defense does not. And this failure is made evident by the lack of an answer to the criticisms of the mechanists considered in this paper.

A second consideration is what the proposal offered herein portends for the relationship between mechanistic and dynamical explanation in cognitive science. Recently, there has been some activity on the possibility of dynamical models being given mechanistic interpretations (Bechtel, 1998; Chemero, 2000; Zednick, 2011) and even for dynamical explanations to be given a computational minimal models interpretation (Chirimuuta, 2014). I consider the argument presented here to echo this call for complementarity between mechanistic and dynamical models. What I have adamantly disputed is the requirement that cognitive explanations be exclusively mechanistic. Nevertheless, the spirit of this argument has been an endorsement of opening the door to further investigation of cognitive model-based explanation.

CHAPTER TWO: DISTINCTIVELY MATHEMATICAL EXPLANATION AND THE PROBLEM OF DIRECTIONALITY: A QUASI-EROTETIC SOLUTION

1. Introduction

Historical surveys on the issue of scientific explanation, which are no less instructive for their whiggishness, often return to Hempel's covering-law view of explanation (Hempel and Oppenheim, 1948). The instructiveness follows from the fact that in scientific explanation, past is preface. Adherents of the ontic conception claim as credit for their view that it successfully dissolves many of the early problems bedeviling Hempel's approach, problems involving explanatory symmetry, relevance and genuineness. Indeed, these problems are almost too familiar to warrant rehearsal. The moral to be extracted from these problems, when viewed through the ontic prism, is that explanations capture mind-independent 'relations in the world,' and that the traditional problem of sorting the explanatory wheat from the chaff 'cannot be accomplished without taking the ontic aspect of explanation seriously' (Craver, 2014, p. 41).

Fittingly, the ontic conception has enjoyed no shortage of adherents but recent trends tell against the view's hegemony. The past twenty years has featured a proliferation of examples of non-ontic, genuine scientific explanation. Exemplars have spanned the gamut from optimality explanations in biology (Rice, 2015), renormalization group explanations in physics (Batterman, 2002), portfolio theory in finance (Walsh, 2015), to topological explanations across many disciplines (Huneman, 2010). A notable subspecies of the non-ontic are distinctively mathematical explanations or DMEs [16]

[16] Here DMEs are taken to be a kind of non-ontic explanation but there is room for the possibility that they are ontic explanations of one kind or another. One exemplar of this latter view is Mark Povich's NOCA

(Baker, 2005; Lange, 2013). DMEs generate their explanatory power not by citing causes in the world but rather mathematically. What evinces the explanatory status of these seemingly non-ontic cases? The line that has sometimes been adopted leads us back to Hempel: these non-ontic explanations are genuinely explanatory by dint of being formable into covering-law explanations. Non-ontic explanations are then very much newer wine in older bottles. However, to base the explanatory status of non-ontic explanations on the covering-law approach is to inherit the costs of that view. Many of the same problems resurface and so we are swept back into the fray of male birth-control and flagpoles. Non-ontic explanations and DMEs by extension are accordingly beset with problems involving genuineness and relevance (Baron, 2016).

A further problem facing DMEs is what I shall call the problem of directionality (Craver and Povich, 2017). Just as Hempel's covering-law view problematically permitted explanatory reversals, allowing causes to explain their effects —e.g. the length of the flagpole's shadow to explain the height of the flagpole— DMEs also license explanatory symmetry. The problem demands resolution if DMEs are going to remain a viable form of explanation, however, the issue encircles the entire domain of non-ontic explanation. Since non-ontic explanations cannot avail themselves of the usual ontic mode of defense, using causes to determine relevance and constrain the direction of explanations, all non-ontic explanations prove susceptible to this form of attack. In what follows, I will offer what I shall call a quasi-erotetic solution which amounts to a kind of

view which takes DMEs to be ontic explanations (2019). Moreover, there may be ontic explanations which are non-causal and so this paper adopts an agnostic attitude toward the causal/non-causal nature of ontic/non-ontic explanations. Finally, the use of the term 'DME' differs from Marc Lange's definition of the term (Lange, 2013, 2017). For Lange, DMEs enlist only mathematical facts in the explanada whereas the usage of the term DME herein is more ecumenical, permitting DMEs to range over explanations which involve both physical and mathematical facts in the explanada.

pragmatic solution. This takes the form of proposing an additional necessary condition on explanation, specifically the presuppositional contextual appropriateness condition or *PCAC*. Adoption of the *PCAC* remedies the affliction, dissolving the directionality problem for non-ontic explanations. Note that the proposal on offer here does not constitute a full-fledged view of DMEs and is thus confined to resolution of the directionality problem.

This paper shall proceed in four main stages. In section 2, I canvass the set of objections which stand as an obstacle for DMEs, allotting focus to the problem of directionality. In section 3, I consider two recent views which represent the current state of play in the rehabilitation of DMEs: Baron's REDC view (2019) and Povich's NOCA view (2019), as well as assemble a list of adequacy conditions for resolving the problem of directionality. In section 4, I introduce the quasi-erotetic solution and the *PCAC* is delineated. In section 5, I consider some objections for the quasi-erotetic solution. Section 6 concludes.

2. Distinctively mathematical explanations and their discontents: The problem of directionality

2.1 Distinctively mathematical explanations: Issues of relevance and genuineness

The rise and fall of Hempel's covering-law account ushered in the predominance of the ontic conception of explanation which stood as an alternative to Hempel's epistemic conception (Salmon, 1989). Whereas Hempel's view was commonly understood as positing that explanations were representations, where representations are mind-dependent descriptions of some phenomenon which permitted epistemically valuable activity to be conducted, the ontic conception notably broke ranks (Wright, 2018). The ontic conception which began with Wesley Salmon has matured into something like the following view in recent years consisting of the following two commitments (Craver, 2007; Illari, 2013; Craver, 2014):

1. Ontic explanations involve identification of the relations or ontic structures –e.g. causal mechanisms, attractors, statistical relations– among features in the world. These relations are understood to be either causal or constitutive.

2. Following from 1, ontic explanations are not entirely epistemic or representational. Rather, they track objective or explanatory relations which are mind-independent and out in the world.

The ethos of this view enabled the elimination of early problems which dogged the covering-law view. Relevance was constrained by causal relevance, scientific explanations were demarcated inasmuch as they trafficked in causes and finally, explanatory asymmetry was underwritten by causal asymmetry. A place for everything and everything in its place.

For many years, the ontic conception held serve but pushback has increasingly materialized in the last twenty years. This has mostly taken the form of a proliferation of counterexamples which constitute a class of exemplars of genuine scientific explanations of a non-ontic variety (Rice, 2015; Bokulich, 2011; Saatsi and Pexton, 2012; Walsh, 2015). Prominent among these counterexamples are mathematical explanations (Baker, 2005; Pincock, 2007; Lyon, 2012). These explanations explain primarily not by citing causes but rather mathematically. A familiar toy example is Steiner's strawberries case

(Steiner, 1978). The question ‘why can’t Mother evenly divide her 23 strawberries among her children?’ is demonstrated to be answerable by an explanans consisting of the empirical premise that Mother has three children and the mathematical premise that 23 is not evenly divisible by 3. The simplicity is what allows a homing in on the operative source of explanatory power in the case. Clearly, the explanatory power is principally generated not by empirical facts or ontic structures but rather by the simple mathematical fact about arithmetic.[17] Additionally, the case provides insights into the important modal dissimilarities between garden variety ontic explanations and mathematical ones. According to Lange, distinctively mathematical explanations trade in a stronger form of modality than ontic explanations since ‘mathematical necessity is a stronger variety of necessity than natural necessity’ where ontic explanations are exclusively the province of the latter (Lange, 2016, p. 31). This modal difference thus accounts for the uniqueness of DMEs and so a powerful set of counterexamples to the ontic conception is unlocked.

The rosy picture for DMEs begins to darken, however, once problems in the foreground are acknowledged. Specifically, the old issues of genuineness and relevance resurface for the view.

Among the set of desiderata for an adequate account of DMEs is the genuineness constraint or the demand that an account of DMEs be sensitive to the distinction between explanatory and non-explanatory applications of mathematics in science (Baron, 2016). The desirability of the constraint is straightforward: many if not most scientific explanations include mathematical facts or reference mathematical structures in the

[17] For an alternative diagnosis of the this case as well as a refutation of DMEs as explanations, see (Skow, 2016, Ch.5.)

explanans. Delimiting the domain of mathematical explanation too loosely threatens an overgeneralization problem whereby many if not most scientific explanations would wrongly be miscast as DMEs (Pincock, 2015).

To precisify this problem, Baron introduces a train case where the explanandum question is 'Why does train T arrive at stations S* at 3:00 pm?' An explanans is formable which includes the empirical facts that T left station S at 2:00 pm, that S is a distance of 10 kilometers from S*, and that T is traveling at 10 kmph. A mathematical premise which performs a simple calculation for time which is equal to distance traveled (10 kilometers) over rate of travel (10 kmph), yields the conclusion that T pulls into S* at 3:00 pm. Although the explanation in this case involves mathematics, clearly mathematics does not do the lion's share of the explanatory work but is rather only instrumental in the explanation. An adequate account of DMEs should resist admitting an explanation like this into the domain of mathematical explanation or face implausibly overgeneralizing to such cases.

A second and familiar problem is the issue of relevance. In classical logic, it is a fact that for any non-empty set of propositions Γ, if some proposition or set of propositions {A} entails B and {A} and B ∈ Γ, then adding some irrelevant proposition C to {A} does not disrupt the entailment. This fact turned out to be unfortunate for Hempel's view since classical logic affords no barrier for entry against unwanted, irrelevant premises creeping into the explanation. Similarly, DMEs but especially those which assume a Hempelian covering-law framework, must also face this problem.

2.2 The Problem of Directionality

Similar to genuineness and relevance, the problem of directionality represents another Hempelian bird which comes home to roost for DMEs. Bromberger (1966) and others charged Hempel's covering-law view of permitting explanatory symmetry, allowing reversals of putative explanations to count as the genuine article. Recall that covering law or deductive-nomological explanations demonstrate that the phenomenon of interest is deductively entailed by a set of premises consisting of both natural laws (hence the title), initial conditions and auxiliary assumptions (Hempel and Oppenheim, 1948). On this view, explanations are arguments.

The problem of directionality is demonstrable in the famed flagpole case. In explaining why a flagpole of some height *h*, produces a shadow of length *l*, one could join the fact about *h* with further facts about light's rectilinear propagation and the angle of the sun's elevation to form the explanans. This is fine as far as it goes. However, nothing in the covering-law account prevents this explanation from being reversed, drawing on *l* and these same facts about light's rectilinear propagation and the angle of the sun to 'explain' *h*. Clearly, this is not an explanation despite featuring the required Hempelian deductive nomic entailment. For the ontic conception, this problem is readily eliminated: the putative explanation tracks a causal relationship between the sun, light, and the flagpole which produces the shadow. The reversal does not.

Non-ontic proponents of explanation bear the sting of this problem since, similarly to the covering-law view, one cannot avail themselves of the ontic resources enlisted to solve the problem initially. Craver and Povich (2017) charge contemporary views of non-ontic explanation, including DMEs, with a similar kind of directionality problem. To demonstrate the problem's force, Craver and Povich (p. 32-34) give several well-known examples of DMEs accompanied by their reversed counterpart, (I mention three), where *E* stands for the explanandum result, (EP) stands for empirical premise, (MP) stands for mathematical premise, *EQ* stands for the explanandum question and the premises constitute the explanans:

Strawberries (SB):

EQ: Why can't Mother divide her strawberries evenly among her 3 children?

1. Mother has 23 strawberries (EP).
2. 23 is not evenly divisible by 3 (MP).

E: Mother cannot divide her strawberries evenly among her 3 children.

Reversed Strawberries (R-SB):

EQ: Why doesn't Mother have 23 strawberries?

1. Mother evenly distributed her strawberries among her 3 children. (EP)
2. 23 is not evenly divisible by 3. (MP)

E: Mother doesn't have 23 strawberries.

Königsberg (K):

EQ: Why can't Marta walk an Euler path around Königsberg's bridges in 1735?

1. That year, Königsberg bridges formed a connected network with four nodes (landmasses); three nodes had three edges; one had five. (EP)

2. Only networks that contain either zero or two nodes with an odd number of edges contain an Eulerian path. (MP)

E: Marta cannot walk an Euler path through Königsberg.

Reversed Königsberg (R-K):

EQ: Why did either zero or two of Königsberg's landmasses have an odd number of bridges in 1756?

1. Marta walked through town, hitting each bridge exactly once; she walked an Eulerian path. (EP)
2. Only networks containing zero or two nodes with an odd number of edges (bridges) contain an Euler path. (MP)

E: Either zero or two of Königsberg landmasses had an odd number of bridges.

Trefoil Knot (TK):

EQ: Why can't Terry untie his shoes?

1. Because Terry has a trefoil knot in his shoelace. (EP)
2. The trefoil knot is not isotopic to the unknot in three dimensions. (EP)
3. Only knots isotopic to the unknot in three dimensions can be untied. (MP)

E: Terry can't untie his shoes.

Reversed Trefoil (R-TK):

EQ: Why doesn't Terry have a trefoil knot in his shoelaces?

1. Terry untied the knot in his shoelaces. (EP)

2. The trefoil knot is not isotopic to the unknot in three dimensions, and only knots isotopic to the unknot in three dimensions can be untied. (MP)

E: Terry doesn't have a trefoil knot in his shoelaces.

Craver and Povich point out that in each of these cases, the first set of forward-facing cases or *S*-cases represent a putative explanation but the reversed or *R*-cases clearly fail to be explanatory. The reversals merely show the explananda *must* follow, failing to show *why* the explananda followed. Yet there seems to be no principled way to block the *R*-cases if one grants there are DMEs.

The three foregoing problems considered for DMEs can be distilled down to the following non-exhaustive list of desiderata or adequacy conditions for an account of mathematical explanation.

An adequate account of DMEs should:

(A1. Respect the genuineness constraint, resisting the absurd result via overgeneralization that any scientific explanation including mathematics in the explanans is a DME.

(A2. Block irrelevant premises (or components) from appearing in the explanans.

(A3. Successfully avoid the problem of directionality.

3. The Search for an Adequate Account of Distinctively Mathematical Explanation

The current stage of the search for an adequate account of DMEs can be informed via the appraisal of two views: Baron's REDC view and Povich's NOCA view. Each shall be considered in turn.

Baron's task is to provide a full-blooded account of DMEs. His REDC account (2019) consists of the following 5 conditions:

1. DMEs are sound arguments (where validity is understood in terms of relevance logic, not classical logic. On the assumption that soundness just is validity plus the truth of the premises.)

2. DMEs feature a conclusion which is a physical proposition describing some natural phenomenon.

3. DMEs must include at least one mathematical premise.

4. Removal of the mathematical premise would invalidate the argument.

5. DMEs must satisfy the Razor-sharp Essential Deducibility Constraint (REDC).

The first three conditions serve to importantly narrow the boundary conditions for DMEs. The first condition acts as a kind of veridicality constraint, ruling out explanations which involve at least one false premise. Additionally, the first condition does double duty in satisfying A2, blocking irrelevant premises from being free riders in the explanatory argument. Unlike Hempel's covering-law view, insertion of an irrelevant premise disrupts the soundness of the argument since, according to relevance logic, the consequence relation is non-monotonic: In relevance logic, if A proves B, it does not follow that A and C prove B (Baron, 2019, p. 699). Validity, as expressed in condition 1, requires a premise to not only be true but to also contribute information towards the conclusion. Condition 2 importantly restricts the explanatory domain, preventing thoroughgoing mathematical explanations —e.g. proof based explanations in number theory modulo any empirical propositions— from counting as DMEs. Condition 3, however, answers to the opposite concern, precluding wholly empirical explanations from qualifying as mathematical. The fourth condition forecloses the possibility of the mathematical premise playing a dispensable or trivial role in the argument.

The real work of resolving the genuineness problem is done at Condition 5. Baron describes the REDC condition as follows (2019, p. 693):

> Razor-Sharp Essential Deducibility Constraint (REDC): A non-mathematical claim, *P*, is essentially deducible from a premise set, *S*, that includes at least one mathematical sentence, *M*, just when for an appropriate choice of expressive resources there is a sound derivation of *P* from *S* and either for the same choice of expressive resources there is no sound derivation of *P* from a premise set, *S**,that includes only physical sentences or all sound derivations of *P* from premise sets $S_1 ... S_n$, each of which includes only physical sentences are worse than the mathematical derivation or for all appropriate choices of expressive resources the best derivations use *M*.

The upshot of REDC is to require that for some choice of expressive resources, a sound derivation of *P* from a premise set consisting of at least one mathematical claim is superior to a sound derivation of *P* from an alternative premise set which excludes the mathematical claim. The superiority is determined by rank-ordering derivations on the basis of two criteria: simplicity and strength (2019, p. 690). Simplicity is understood as involving less premises in the explanans whereas strength is understood as featuring a conclusion or premises with wider generality. If the derivation featuring the mathematical claim bests the wholly non-mathematical derivation for *P*, then *P* admits of a DME.

Other issues aside, our question of interest is to investigate whether the REDC view rules out reversals or satisfies A3. Unfortunately, side-by-side comparison of the *R*-cases with imagined rival explanations which feature no mathematical premises tell against this conclusion.[18] Consider the *R-SB* variant of the strawberries case. The argument features two premises: an EP to the effect that Mother successfully divided her strawberries evenly among her three children and an MP that 23 is not evenly divisible by 3. Indeed, it is easy to imagine rival explanantia for 'why Mother does not have 23 strawberries' which involve no mathematical premises. However, it is difficult to imagine there being rival explanantia which include less premises.[19] Similarly, strength appears of little use in this case. Any strategy of generalizing the premises, thus widening their scope, would also seem to be available *mutatis mutandis* in reformulating the pseudo explanation given in *R-SB*. Moreover, generalizing in certain cases will rule out standard cases of explanation which feature a more specific set of propositions and that is clearly the wrong result. Baron's REDC view makes considerable strides in answering to A1 and A2 but is found wanting in answering A3. And so the problem of directionality persists.

Mark Povich introduces his NOCA view which purports to resolve the problem of directionality. Povich acknowledges that one of the adequacy conditions for an account of DMEs is that the account should rule out problematic [explanatory] reversals, thereby answering to the problem of directionality. Per NOCA, an explanation is a DME if and

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[18] Povich (2019) raises this kind of criticism of Baron's view in failing to answer to A3.
[19] This claim can be strengthened further if we gerrymander the explanandum in *R-SB* by inserting the empirical premise into the explanandum as a presupposition. This yields an *EQ* like 'Given that Mother successfully divided her strawberries among her 3 children, why doesn't Mother have 23 strawberries?' This reduces the premise set of the explanans to one premise, namely, '23 is not evenly divisible by 3.' This makes the gerrymandered *R-SB* maximally simple since there are obviously no rival non-mathematical explanantia that include less than one premise. However, this is the wrong result since the object is to rule out the reversals or the *R-SB* case.

only if: '(a) it shows a natural fact to (weakly necessarily) depend counterfactually only on a mathematical fact or (b) it is necessitated by a natural fact that weakly necessarily counterfactually depends on a mathematical fact' (2019).

To isolate this kind of dependence, Povich enlists a two-step process: first, the explananda are narrowed, bringing the empirical premise into the explanandum as a presupposition and second, events are converted to states of affairs, yielding a counterpossible which satisfies the weak necessity dependency relation enunciated in either (a) or (b). The emphasis on dependency evinces the view's commitment to counterfactualism or the view that explanations involve the demonstration of the explanandum's counterfactual dependence on the explanans. As such, the view is consistent with and amenable to other views of explanation of a counterfactualist stripe such as Woodward's interventionist account (2003), Streven's difference-making account (2008) or more recently Reutlinger's explanatory monist view (2018).

A problem, however, emerges for NOCA's solution of the problem of directionality. This is a general issue having to do with the view's adoption of counterfactualism. Counterfactualism or the view that explanations involve identifying a form of counterfactual dependency between explanatia and explananda seems to imply explanatory exclusionism when coupled with claims about modal priority. Let the explanandum or *E* be a physical proposition or empirical claim. Suppose counterfactualism is true or that a necessary condition of something's explaining *E* is that *E* counterfactually depends (with some kind of modal necessity) on a fact or the facts which constitute the explanans. Suppose two explanans are offered for *E*: one is mathematical, dealing with mathematical necessity and the other is causal, dealing in natural necessity. If *E* genuinely depends on some mathematical fact with mathematical necessity, this implies that *E* will fail to depend on the causal facts owing to mathematical necessity operating at a stronger modal level than mere natural necessity. In counterfactual terms, the counterfactual which makes *E* dependent on causal facts is swamped out once the stronger counterfactual which makes *E* dependent on a mathematical fact is placed on the table.[20] Consider the *SB* case. Once Mother has 23 strawberries and 3 children, there just is no set of causal facts which her failure to divide the fruit can be shown to depend upon. Why? Because the mathematical fact about the uneven divisibility of 23 by 3 cancels out the dependency of her failure on causal facts. Even if the causal necessity relation failed to obtain in cases like this, the result of the explanandum would still follow by dint of the explanandum's being necessitated by mathematical facts. The problem magnifies when it is acknowledged that there seem to be a plethora of scientific explanations which feature explananda which admit of both causal and mathematical explanations (Bokulich 2011; Lange 2016; Andersen 2018). To cite an example from Lange, physicists do not seem dubious towards the genuineness of partial differentials causally explaining the set of equilibrium configurations for a double pendulum given the availability of a topological explanation of the very same phenomenon. Explanatory exclusionism places counterfactualism at odds with scientific practice.[21]

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[20] This problem is certainly resolvable for counterfactualism. However, some solution will need to be given to avoid the unwanted byproduct of explanatory exclusionism.

[21] A further issue is that by adopting counterfactualism, NOCA is less desirable for those who do not subscribe to this view about explanation but are committed to the existence of non-ontic explanations, a position Pincock refers to as explanatory pluralism (Pincock, 2018). For example, neither Lange (2016) nor Baron (2019) are counterfactualists about explanation. Additionally, Robert Batterman's minimal model explanations, an influential exemplar of non-ontic explanation (2002; 2010) are of a non-counterfactualist bent.

The motivation for a novel solution to the problem of directionality is now evident. Baron's REDC view makes considerable headway in providing an account of DMEs, satisfying both A1 and A2 but problematically remains vulnerable to the directionality problem. Povich's NOCA view resolves the directionality problem but only at the cost of problematically adopting counterfactualism.

4. The Quasi-Erotetic Solution

As is apparent in the following two cases, one denies the relevance of contextual sensitivity for interrogatives at one's own peril:

Case 1

Q1: Why do migratory birds fly south for Winter?

R1: It's faster than walking.

Case 2

Q1: Why can Whales swim?

R2: Because a Whale's ability to swim is perfectly consistent with the set of *a priori* truths.

While neither R1 nor R2 seems appropriate, the confusion in each case is instructive. In Case 1, there is a confusion about the contrast class of Q1 or the space of relevant alternatives. The inquirer clearly desires a causal answer to her causal question, intending a contrast class like 'Why do migratory birds fly south for Winter?' (rather than flying to some other location or not at all). R1 problematically assumes a contrast class along the lines of (fly rather than walk, run…take some other mode of transport). In Case 2, the confusion surrounds the relevant modal context at issue. The inquirer intends the modal verb ‘can’ in Q2 to be understood in terms of natural possibility whereas the interlocutor gives the verb a gloss of epistemic possibility. As such, neither R1 nor R2 are satisfactory. They represent instances of presupposition failure or a clash between the presuppositions of the inquirer and her interlocutor with the presuppositions of both parties thereby failing to be in the common ground (Stalnaker, 1968).

Why-questions which represent a sizable subset of scientific questions can be understood to have the following anatomy:

$$(Given\ clause\ (Presupposition)|Event\ or\ State\ of\ Affairs)$$

$$\left\{ Contrast\ Class \left| \begin{matrix} A1 \\ A2 \\ \ldots \\ Ak \end{matrix} \right) \right\} Indices[i \ldots j]$$

The ‘Given clauses’ in the form of explanatory presuppositions, which typically attach to some event or state of affairs as well as the contrast class or the set of relevant alternatives and indices for time, place, scale or modality all serve to delimit the boundary conditions of the question’s explanatory context (Garfinkel, 1981). The presuppositions include the set of facts that are assumed to hold or that are taken as given. For instance, the embedded propositional attitude in the statement ‘Barney wants to phone his sister,’ presupposes *inter alia* that ‘Barney has a sister’ (Karttunen, 1974). The contrast class is a set of propositions –[$A1 \ldots Ak$]– which represent a set of alternatives. Indices [$i \ldots j$] serve to further delimit the context of the why-question and may include temporal, spatial or modal indexicals.

Notably, the importance of so-called pragmatic factors for explanation came to the fore with Bas van Fraassen's 'erotetic' view of scientific explanation (1980)[22] although application of the erotetic view to the topic of mathematical explanations –e.g. proofs in number theory– has also been considered (Sandborg, 1998). Per van Frassen, an explanatory question can be formulated as an ordered triple, consisting of a topic (Pk), a contrast class (X) and a relevance relation (R) or <Pk, X, R>. The relevance relation specifies boundary conditions for a range of admissible answers. For example, when asked 'why does the heart pump blood throughout the human body?' an answer which cited the mechanical properties of the heart and the fluid dynamics of circulation might fail to constitute an explanation on grounds of irrelevance (Sandborg, 1998). If the why-question is being posed in an evolutionary sense and the explanatory request is for what benefits the heart's pumping blood confers upon the organism's functionality, the mechanical explanation is clearly an irrelevant one. And (R) is meant to block these kinds of responses. Alternatively, an answer which cited the role of circulation in the oxygenation and nourishment of tissue would fit the bill. Despite these developments, van Fraassen's view foundered mostly due to the fraught nature of the relevance relation (Kitcher and Salmon, 1987).[23] In what follows, I will not adopt a thoroughgoing erotetic approach *a la* van Fraassen, opting instead to cherry pick a particular pragmatic feature

[22] An additional pragmatic view of note was Achinstein's (1983) view which adopted a speech-acts oriented approach to scientific explanation, thus making the view more sensitive to speaker intentionality than van Fraassen's earlier view.

[23] The problem with the relevance relation, per Kitcher and Salmon, was it permitted vacuous explanations. This is demonstrated via showing a pseudo-explanation —which 'explains' the date of Kennedy's assassination by astral influences— satisfies the relevance relation. The problem is that the relevance relation is too broadly construed, permitting factors to count as relevant which clearly are not.

which is indispensable to our cases, or so I shall argue. As will soon become clear, this more surgical strategy is largely how the quasi-erotetic solution earns its qualifier.

Let us define distinctively mathematical explanations as those explanations which satisfy Baron's five criteria (2019). This runs us headlong into the problem of directionality since, as seen in 2.2, the *R*-cases grade as explanatory on these criteria but problematically the *R*-cases represent a set of pseudo-explanations, telling us why some explanandum, *E*, *must* follow rather than *why E* follows. The crucial step is to diagnose what has gone wrong in the *R*-cases.

In brief, my answer is as follows: the *S* and *R*-cases feature fundamentally different types of explananda but share the same type of explanantia, such that the *S*-cases qualify as explanatory whereas the *R*-cases feature a mismatch or a presupposition failure between explanans and explanantia. As such, the explanantia of the *R*-cases turn out to be irrelevant to the context in which their respective explananda are issued and thus are not genuine explanations.

The *S*-cases represent a request for an explanation about the modal status of their respective explananda *E*: e.g. why must *E* be the case or why must *not-E* be the case, why couldn't some individual do *E* etc. (Kratzer, 1977; Lewis, 1979). As such, given their explanantia, they are rightly regarded as explanatory. Alternatively, the explananda of the *R*-cases suffer from an underspecification problem. An underspecification occurs when both the explanandum fails to explicitly specify a particular modal index and when a mismatch exists between the implicit modal character of the respective explananda and their corresponding explanantia. The *R*-cases do not involve explanatory requests about the modal status of their explananda but rather a request for the contingent causal facts or intentional states which eventuated in *E*.

In the *R*-cases, unlike the *S*-cases, the explanatory request is decidedly of a non-modal kind; when one reads the *R*-case explananda, one naturally presupposes a non-modal request for information. In this respect, the *R*-cases are similar to traditional examples of hidden indexicals (Perry, 2000). When Sarah asks ‘Is there any beer left?’ without specifying whether she means in the fridge, at the grocery store or in the world, it is common to identify this as a case where the spatial indexical is hidden or left implicit (Kaplan, 1989). Sarah doesn’t need to explicitly state that she means the refrigerator in the kitchen of the location she currently occupies, rather it is implicit in her interrogative (Stanley and Szabo, 2000). Similarly, when the why-question in *R-SB* is posed ‘why doesn’t Mother have 23 strawberries?’ there is a clear presupposition left implicit in this context that the request is for the set of contingent facts that eventuated in Mother not having 23 strawberries. Not for the pseudo-explanation which shows that it was mathematically impossible for Mother to have 23 pieces of fruit given certain other contingent and mathematical facts about distributing the fruit to her children. When explanantia are provided which account for the modal status of the explananda in response to a non-modal informational request, a presupposition failure occurs. And this is precisely what the diagnosis of the *R*-cases as non-explanatory hangs on.

Consider the following case. Suppose Jones is due in Boston on Tuesday for an interview but fails to appear. A natural question arises: ‘Why wasn’t Jones in Boston on Tuesday?’ One explanans may consist of the empirical fact that Jones was in Pittsburgh which can be read as equivalent to ‘Jones was not in Boston’ accompanied by a second premise which is a version of the law of the excluded middle: ‘Jones was either in Boston or Jones was not in Boston (and Jones was not both in Boston and not in Boston)’, yielding the conclusion. A second reply may involve the empirical facts that Jones encountered heavy traffic on the way to the airport in Pittsburgh, causing her to miss her flight as well as the airline having no available seats on other flights to Boston for that day. The second response seems satisfactory whereas the first does not. Namely, the first case represents a presupposition failure whereas the second does not.

To motivate this a bit further, consider the language of the explananda in the *S*-cases.[24] All involve explananda which feature the modal verb ‘could’ or ‘can’: Why *couldn’t* Mother divide her strawberries…, Why *can’t* Marta walk an Euler path…, Why *can’t* Terry untie his shoelaces. The appearance of this modal verb in the explananda funds the presupposition that the question is geared towards the modal status of the event in the explananda. Alternatively, consider the explanandum of *R-TK*: ‘Why *doesn’t* Terry have a trefoil knot in his shoelaces?’ The natural presupposition to make here is that an acceptable response to this question will describe some causal process or reference Terry’s intentional states which explains this fact, foreclosing the availability of an explanans with a modal flavor, either mathematical or otherwise. When an explanans is offered which involves a mathematical modal character, like the one in *R-TK*, this creates the aforesaid clash in presuppositions, returning the diagnosis that *R-TK* is not explanatory. And so too with *R-K* and *R-SB*. This all motivates adoption of the following necessary condition on explanation:

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[24] In the assessment of the cases to follow, I take the empirical premises along the lines of Lange to be both explanatory prior to the explanatory question being asked and ‘fixed parameters for the cases at hand’ (Lange, 2016, p. 33).

Presuppositional Contextual Appropriateness Condition (*PCAC*). For some explanation consisting of both an explanandum, *E*, and explanans, *X*, the modal character of *X* should be consistent with the explicit or implicit presuppositions about the modal character of *E*.

To capture the modal or non-modal flavor of *E*, this variable can be joined to a modal index. The modal index can be understood as a kind of binary contextual parameter that indicates whether *E* involves a request for modal information; i.e. the necessity or possibility of *E* with some kind of modal strength, or rather whether the why-question is not a request for this kind of information.[25] Thus, when specified, *E* can take on one of at least the following two values: *E* [α] or *E* [Ø]. *E* 'alpha' represents *E* with the modal index filled in, signaling a request for the modal status of *E*: why can or must *E* obtain either logically, metaphysically, deontically or naturally and so on? Alternatively, *E* 'null' signifies an empty modal index which expresses a non-modal request for information pertaining to *E*.

The *PCAC* can be appended to Baron's five criteria and enlisted to rule out the *R*-cases while retaining the *S*-cases. Consider the *EQ* for *SB* where brackets represent the modal index placeholder:

SB explanandum: Why couldn't Mother divide her strawberries evenly among her children? [α]

The '*α*' symbol indicates that *SB* can be understood as being indexed to some modal context. Both the presence of a modal index as well as the modal verb 'couldn't' evince

[25] As we will see in 5.1, this parameter actually is a ternary one, admitting of three values rather than two.

that the forgoing is an explanatory request for the modal status of Mother's failure to divide her strawberries evenly. *SB* answers this question in a *PCAC* satisfying manner.

Next, consider *R-SB*:

R-SB explanandum: Why doesn't Mother have 23 strawberries?[Ø]

Formulating *R-SB* by leaving the modal index placeholder 'null' signals that the informational request is not being made for the modal status of the event 'Mother's not having 23 strawberries.' This move makes the hidden modal presupposition explicit that funded the diagnosis of the explanans of *R-SB* as non-explanatory. Notice that the explanans of the *R*-cases will fail to satisfy *PCAC* if the explanandum is understood in this way. This formulation can also be applied to *R-K* and *R-TK*.

The *PCAC* enables the *R*-cases to be unmasked as cases of presupposition failure and hence ruled out as contextually inappropriate explanations. These kinds of presupposition failures, however, signal a deeper issue with the explanatory candidates being given in the *R*-cases: namely, these explanatory candidates are *irrelevant* to the contexts in which these why-questions are being issued. Relevance is a putative explanatory norm. As mentioned earlier, the problem of irrelevant premises appearing in the explanans amounted to a difficult problem for Hempel's DN view. In those cases, the appearance of an irrelevant proposition in the explanantia was taken to worsen the explanatory goodness of the explanation being offered. In the *R*-cases considered herein, the 'explanation' being offered fails to respect the presuppositions which narrow the relevant context for admissible answers to the why-questions being posed. Just as the mechanical explanation of the heart's pumping failed to be an adequate explanation to the why-question which presupposed an evolutionary sense since it represented an irrelevant answer, the *R*-cases fail to be explanatory since they fail to respect the modal context delimited by the presuppositions which frame the why-question. And irrelevant explanations are hardly explanatory.

This all suggests a natural nexus between the *PCAC* and Baron's view. Recall that Baron aims to address the problem of irrelevant premises by deploying relevance logic to govern the validity of the DN type arguments his view addresses. The *PCAC* operates in a similar spirit, enshrining relevance into the set of adequacy conditions for explanation by requiring that the modal character of the explanantia be consistent with the modal index of the respective explananda which they are intended to answer. This permits the problematic *R*-cases to be blocked but in a manner which is born out of considerations of a putative explanatory norm, namely, relevance. Thus, the *PCAC* is responsive to the same issues that motivated and underpin Baron's REDC view. As such, this bodes well for the prospects for integrating the *PCAC* into Baron's view.

5. Objections Considered

The foregoing solution (*PCAC* condition) can be objected to on at least the following three grounds: *PCAC* is overly restrictive; *PCAC* is ad hoc; and *PCAC* problematically overpopulates the explanatory field. Each of these objections is considered and responded to in turn.

5.1 PCAC is overly restrictive

The *PCAC* which requires the modal character of the explanandum to be consistent with the modal character of the explanans is vulnerable to the following counterexample.

Consider the explanandum in *S-K*: 'Why *can't* Marta walk an Euler path…' This explanandum can be modally weakened such that it has a contingent character as follows: 'Why *didn't* Marta walk an Euler path around the bridges of Königsberg?' Then, one could run the same explanans for this explanandum as appeared in the original version of *S-K*; the empirical premise about the bridges' configuration as well as the mathematical premise about the properties of an Euler graph, yielding the explanandum. This case appears to be explanatory. And yet the *PCAC* seemingly casts the verdict that this modally weakened case is non-explanatory. Notice that in these cases, *pace PCAC*, a contingent explanandum is explained by modal necessity in the explanans. The problem generalizes to the other two *S*-cases. The objection follows that *PCAC* is overly restrictive since it seems to block these cases of genuine explanation.

What the modally weakened *S*-cases demonstrate is that in some cases, it is not clear merely from the modal character of the explananda what modal presuppositions are being made: one cannot always simply read off the modal presuppositions from the explanandum's modal character. The weakened *S*-cases are all task or action-oriented. They all involve questions about why some *event* did not occur or why some agent failed to successfully complete a task where the task has mathematical content embedded directly into it: e.g. diving strawberries evenly, walking an Euler path or untying a Trefoil knot. Despite the contingent character of the explanandum, the modal character of the explanandum is ambiguous; these cases would seem to admit of explanantia which demonstrate the mathematical impossibility of the agent completing their mathematical task. Notice that this seems presuppositionally valid. If one inquires about why some agent did not successfully accomplish some mathematical task, *M*, an explanation which demonstrates the mathematical impossibility of *M* appears relevant given this kind of why-question. Alternatively, the *R*-cases which feature contingent explananda do not inquire about why some agent failed to complete a mathematical task but rather why some *state of affairs* (which involve math) failed to obtain: e.g. Mother having 23 strawberries, the number of landmasses in Königsberg, or Terry not having a Trefoil knot in his shoelaces. To see this difference, consider the following two homely explananda:

Case 1: Why didn't Rachel draw a four-sided triangle?

Case 2: Why does New York City not contain at least 10 buildings with a height of over 1,000 feet?

In Case 1, similarly to the modally weakened *S*-cases, we have a contingent explanandum concerning why some event did not occur (the failure to accomplish a mathematical task) that admits of an obvious explanans which shows, given some basic Euclidean postulates, that drawing a four-sided triangle is mathematically impossible. In this case, the modal index in the explanandum is ambiguous. In Case 2, we can easily imagine a kind of *R*-case style explanans which shows that the actual number of buildings in New York City which have a height greater than 1,000 feet (a state of affairs) is some number which is not identical to and less than 10 and so there being at least 10 buildings over 1,000 feet is mathematically impossible. But similarly to the *R*-cases, this would fail to be explanatory given the lack of consistency between the modal character of the explanandum and explanans, failing due to irrelevance.

To accommodate the ambiguous cases, the binary contextual parameter which served as the modal index in *PCAC* can be revised thus:

The contextual parameter which expresses the modal character for some explanandum E [] is a ternary operator and can admit of at least three values: [α] signifying a request for modal information; [Ø] signifying a request for non-modal information and [?] signifying ambiguity.

This allows for *PCAC* to be retained. Recall that *PCAC* requires the modal character of the explanandum and of the explanans to be consistent. In cases where the modal character of the explanandum is ambiguous –E[?]– such as the modally weakened *S*-cases which involve explananda which feature a failed mathematical task or why some event did not occur, a modal explanans joined to a contingent explanandum is contextually permissible and thus consistent. This matches our intuitions and as such the presuppositions about the case that the mathematical impossibility of some mathematical task is relevant to why an agent failed to accomplish this mathematical task.

5.2 PCAC is Ad Hoc

Ontic critics may object against *PCAC* for missing the important underlying point of the directionality problem. What the problem of directionality demonstrates is that there is a fundamental difference in the legitimacy of ontic and non-ontic explanations: ontic explanations are not subject to reversals since the condition for ruling them out is directly encoded into the ontic conception of explanation itself whereas non-ontic explanations are both subject to reversals and do not admit of an endogenous solution. Papering over this difference by plugging an additional necessary feature into the set of jointly sufficient conditions for explanation fails to appreciate the problem. As such, *PCAC* seems an ad hoc solution, only serving to obfuscate the real nature of the issue.

While it is certainly true that ontic explanations include built-in safeguards against reversals such as causal asymmetry, it appears false that ontic explanations are immune to the more general underspecification problem which underpinned the directionality problem. The *R*-cases represent a set of cases which involve underspecified non-modal presuppositions in the explananda and explanantia that are decidedly modal in character. There is no reason to think this issue uniquely afflicts DMEs.

Consider the following explanandum question: Why did Suzy break the window at time T′? As alluded to in section 4, this explanandum admits of at least two modal interpretations:

E1: Why did (must) Suzy break the window at T′? [α]

E2: Why did Suzy break the window at T′? [Ø]

Clearly, *E2* would appear to be the more plausible reading here given the non-modal character of the explanans. However, prior to modally specifying the explanandum, notice that two explanantia are available. Explanans 1 includes the contingent fact that Suzy threw a rock at T, which was temporally prior to T′, along with facts about the rock's mass, trajectory, acceleration, the window's material strength and so on. These facts are joined to Newtonian laws of force and motion as well as perhaps a law from material physics pertaining to the fragility of the glass. Explanans 2 consists of Suzy's intentional states —i.e. that she wanted to break the window to pester an annoying neighbor— as well as some contingent facts about the rock and the window's material constitution. Explanans 1 has a modal character and accounts for why the window must have broken by showing that this event was inevitable via natural necessity. If Explanans

1 is offered in response to *E2*, the same inconsistency which motivated *PCAC* is encountered although this case is clearly ontic in kind. Similar to the *R*-cases, the inconsistency of *E2* and Explanans 1 reflects a presupposition failure, particularly an inconsistency between the non-modal character presupposed in the explanandum and the modal character of the explanans. This demonstrates that insisting that all explanations should be ontic is not sufficient to avoid the problem of underspecification which arose in the reversed DME cases. Thus, modal underspecification is not uniquely the province of non-ontic explanation.

5.3 PCAC faces an overpopulation problem

The quasi-erotetic solution has maintained that the explananda in the *R*-cases are underspecified. The most natural presuppositional filling in of the modal index leads to a presupposition failure. But what if one goes the other way, indexing the *R*-cases not to modal nullity but such that their explananda are read as an inquiry about the modal status of the event contained therein as follows:

R-SB EQ: 'Why doesn't Mother have 23 strawberries?' [α]

Does the *R-SB* explanans then explain this indexed explanandum? It is hard to see why not. As Povich has pointed out, Baron's set of criteria affords no protection against counting *R-SB* as a genuine explanation. Furthermore, this seemingly satisfies *PCAC* since underspecification is avoided and no presupposition failure ensues. Notice that admitting these properly specified *R*-cases into the explanatory fold generates negative consequences. The permissiveness of the *PCAC* threatens to overpopulate the explanatory field. If each known DME admits of at least one reversal, simple arithmetic implies that the number *n* of known DMEs then becomes n^2.[26] Additionally, the *R*-cases should plausibly not be deemed explanatory in any guise. The quasi-erotetic solution will thus problematically license pseudo-explanation on the cheap.

The degree of concern one should have for the overpopulation objection undoubtedly depends on how problematic the explanations responsible for the 'overpopulation' really are. Returning to *R-SB EQ*, it is apparent that the wording of the explanandum is misaligned with the modal placeholder. The modal placeholder signals that the explanatory request is for the modal status of the event 'Mother's not having 23 strawberries', i.e. the necessity of her not or impossibility of her having 23 strawberries. *R-SB EQ* is thus better formulated as follows:

*R-SB EQ**: 'Why *couldn't* Mother have 23 strawberries?' [*α*]

The explanans of *R-SB* can then be deployed. Mother's successfully dividing the *n* number of strawberries she has evenly among her children plus the mathematical fact that 23 is not evenly divisible by 3 seem to be a plausible explanation of *R-SB EQ**. The explanation no longer seems pseudo-explanatory. Concerns about overpopulation dissipate once the modally indexed *R*-case explananda are suitably brought into alignment with the language of their respective explananda.

6. Conclusion

The set of adequacy conditions for a suitable view of DMEs was shown to include A3 — viz. the view should demonstrate that DMEs can avoid the problem of directionality. This

[26] I say 'known' DMEs since the set of potential DMEs is likely infinite and perhaps uncountable: theoretically, every member of R may have a potential DME.

problem remains outstanding for Baron's REDC view but receives an ontic treatment on Povich's NOCA view. The quasi-erotetic solution presented herein represents a less ontic, pragmatic alternative which is more parsimonious. Any view which deals with interrogatives that admit of multiple modal as well as non-modal contexts but lacks the resources for specification is almost certain to run aground on the shoals of contextual underspecification. And scientific explanations which range over both natural and mathematical necessity are no exception. The *PCAC* offers a conceptual resource to right the ship. In the final analysis, solving the problem of directionality remains an important item on the agenda for those who endorse not just DMEs but other forms of non-ontic scientific explanations. Resolution of this problem is necessary to prevent the book from being closed on an important and ever-increasing area of scientific explanatory interest.

CHAPTER THREE: RECKONING WITH CONTINUUM IDEALIZATIONS

1. Introduction

A cursory overview of multi-scalar scientific models is enough to set one adrift in perplexity. Modeling a particular phenomenon at one scale of the target system often requires coding for the phenomenon at a lower one but these two scales usually differ in several orders of magnitude; they are separated by a vast gulf. The mystery deepens given the way this gulf is negotiated in multi-scalar models: typically, the target system's microstructure is shoehorned into parameters at an intermediate or continuum scale, amounting to a drastic misrepresentation of the microscale. The discrete and heterogenous nature of the microscale is thus papered over by parameterization, smoothing out the microstructure so that it is both continuous and homogenized. The end-product is a representation of the microscale at the continuum level that is something far less than a facsimile of the genuine article. And yet these continuum models enjoy considerable explanatory and predictive success. The mystery endures.

This process is nowhere more evident than in the case of soil hydrology. Hydrological analysis of soil is crucial for understanding the porosity of different materials for the behavior of fluid flows, calculating run-off and transport, as well as the formation of necessary parametric values for short and long-range climate models —e.g. land surface as a diabatic or adiabatic heat source (Zhang and Schaap, 2019). Models in soil hydrology range over spatial scales which are as small as 10^{-6} microns at the pore scale to "soil fields" which are 10^{4} meters at the watershed and climate model macroscales (Or, 2019). The disparity in temporal scales is similarly exponential since soil hydrologists treat microscale processes like chemical reactions which may only be a few milliseconds in length to soil and rock formation, processes which may range over millennia. Straddling these valleys in scale is very much the task at hand in hydrological multi-scalar models.

An exemplar of how this inter-scalar scaffolding is erected in soil hydrology is the calculation of hydraulic conductivity or the rate at which water passes through porous media like soil. At the micro or pore-scale, soil can be represented as a medium consisting of a collection of pores, differing in both their radii and fluid retention properties. To calculate hydraulic conductivity at the pore-scale, soil hydrologists employ Stokes' equation for an incompressible, creeping fluid from fluid dynamics:

$$\mu \nabla^2 u_i - \partial_i \rho = 0 \quad (1)$$

Where μ is the dynamic viscosity of the fluid, u is the fluid's velocity and ρ is the gradient of pressure. This method is fine as far as it goes at the pore-scale but a cleaner equation is desirable for calculating water's passage through larger areas. Moreover, Stokes' equation codes for the particular "phases" or size of the particular pores. The idea is to slough off this unwanted geometrical complexity or eliminate these degrees of freedom at the lower scale by coding for these details on an "as needed" basis at the continuum scale. As such, soil hydrologists can parametrize this detail for hydraulic conductivity by employing a permeability tensor or *K*, using Darcy's law to calculate a fluid's flow or q at a larger, macro-scale:

$$q = -\frac{K}{\mu} \nabla \rho \quad (2)$$

Where μ is the viscosity of the fluid, the del product of ρ represents the pressure drop over a given distance and *K* is the permeability tensor which represents the medium's hydraulic conductivity. This equation encapsulates a proportionality relationship between flow rate as it relates to the fluid's viscosity, permeability and drop in pressure. Thus, *K* codes more sparingly for lower, pore-scale level detail, permitting greater computational tractability at the macroscale. This feat is enabled in part by a continuum idealization. The pore-scale, *contra* fact, is represented as continuous rather than discrete and homogenous, not inhomogeneous. This permits "representative volume elements" or RVEs to be obtained such that the pores can be averaged over in terms of their volume and radii which furnishes us with the *K* parameter in Darcy's law, a continuum scale equation.

Continuum idealizations, which permit inter-scale bridging, are ubiquitous throughout scientific modeling:

- Material physicists treat hunks of steel as continuous blobs, thus enabling a more surgical assessment of the material's deformation (Batterman, 2013; Wilson, 2017). Additionally, material physicists model the microstructure of silicon as a continuum at higher scales in order to simulate and observe the propagation of nanoscale cracks in this material (Winsberg, 2006, 2010; Bursten, 2018).
- Meteorologists characterize the mid-level atmosphere as a continuous fluid, paving the way for usage of the Navier-Stokes equations to calculate vorticity and cyclonic activity (Martin, 2006).
- Biologists employ models which bridge the cellular and tissue level to account for biomechanical processes occurring in skin tissue (Green and Batterman, 2017, Batterman and Green, 2020).

This rosy picture begins to darken with acknowledgement of the problems lurking in the foreground which encircle these fraught inter-scale relationships. The first is a problem well-known to engineers as the “tyranny of the scales” which arises from the scale-dependency of phenomena within a multi-scalar system. Phenomena which reside at different scales within the same system require different boundary conditions for solving the system of differential equations necessary for modeling them and so no one mathematical model can account for all of these phenomena at multiple scales. This yields the rather baffling result that two scientists examining the same behavior in the same target system at different scales will often draw inconsistent conclusions about that target system’s behavior. The issue of how to unify these oft-conflicting, scale-dependent models accordingly besets the applied mathematician (Oden, 2006).

A further troubling issue, which will be our primary focus, is how to regard continuum idealizations in multi-scalar models. Specifically, whichever position one takes with respect to the in principle dispensability of these idealizations, one runs headlong into a puzzle. If these continuum idealizations are deemed to be dispensable in principle, this in part de-problematizes their representational inaccuracy since continuum properties would turn out to reduce to lower-scale properties but this leaves the reduction mysterious.[27] On the other hand, if the continuum idealizations are taken to be indispensable in principle, this is consistent with their explanatory and predictive success but their representational inaccuracy remains a mystery. Robert Batterman concisely captures this tension in wondering about cases in physics: “How can theories of continuum scale physics (continuum mechanics, hydrodynamics, thermodynamics, etc.)

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[27] The point here is that it is unclear how this reduction would actually be carried out. Maddy (2008) raises a similar kind of dilemma for continuum models and reductionism whereas this dilemma centers around continuum idealizations.

work so well and be so robust when they essentially make no reference to the fundamental/structures that our foundational physical theories are about?” (Batterman, 2018, p. 863)

I shall argue that the manner in which extant views about the (in)dispensability of continuum idealizations answer to this problem illuminates the impasse which has formed on this issue. In section 2, I consider how several of these responses address both the explanatory/ predictive success and representational inaccuracy of continuum idealizations. In section 3, I describe a kind of deflationary solution for this problem which involves both further examination of our example in soil hydrology as well as drawing on the upscaling procedures which are frequently enlisted as applied methods in the construction of multi-scalar scientific models. In section 4, I consider how the applied methods solution informs a reimagining of the place of idealizations in scientific modeling, arguing that their unique pragmatic justification resides in the ineliminable role continuum idealizations play in the application of upscaling to the target system of interest. Section 5 concludes.

2. The (in)dispensability of continuum idealizations

The confounding nature of continuum idealizations can be illustrated by a well-known example from fluid mechanics: the lattice gas automaton or LGA model. This inter-systems model demonstrates that a set of fluids which are widely heterogenous at the micro or molecular scale all share certain properties —locality, conservation and symmetry— at the macro scale. In the study of fluids, there are two common exploratory points of origin: either one starts at the micro-scale, with a discrete, molecular model or one begins by describing a particular macro-scale as a smooth and varying continuum

(Rothman and Zaleski, 1997). Construction of the LGA model enlists the second strategy, using renormalization group techniques –i.e. lattice block spins– to capture the relevant properties of interest (Batterman and Rice, 2014). The LGA model enjoys much explanatory and predictive success in describing the behavior of the fluids near their respective point of criticality (e.g. their parabolic momentum profile when flowing through a pipe) at the continuum scale (Frisch et al., 1986). Yet the fluids subsumed under the model are as diverse from one another at the micro or molecular scale as H_2O is to gasoline. This tension generalizes to all such cases involving continuum idealizations: for some multi-scalar model, there is great explanatory and predictive success at the continuum scale but these models involve a radical distortion or misrepresentation of the target system's microscale at the continuum level. How can the predictive and explanatory success of the continuum model be reconciled with the model's lack of representational accuracy?

This problem about continuum idealizations is importantly linked to another problem involving infinite idealizations and reductionism. Infinite idealizations scale some model parameter, N, to an infinite value ($N \rightarrow \infty$) thereby capturing some phenomenon or property in the limit; e.g. the use of thermodynamic limits in models of phase transitions. In these cases, the limiting relations span the gap between discrete, finitely valued models of the target system at the molecular level to continuous, infinitely valued models of the target system at the continuum level. Problematically, however, this constitutes a misrepresentation of the target system since the systems under consideration do not include infinite molecules at the micro-scale (Fletcher et al., 2019). Responses to this problem have spawned two positions: the indispensabilists who argue that the infinite idealizations are necessary for capturing real, emergent phenomena, thus adopting a top-down approach to the issue of inter-theoretic reduction and the dispensabilists who have argued that infinite idealizations do not substantively feature in mature scientific theory and are explanatorily dispensable in principle, adopting a bottom-up approach[28] (Shech, 2018). Both of these positions can be considered and evaluated as responses to the mystery of continuum idealizations.

2.1 Continuum idealizations are indispensable in principle

The indispensabilist position to continuum idealizations can be glossed as follows: For some property or phenomena, there exists a lower scale theory or model which accurately represents this property or phenomena as occurring at the lower scale. However, the prediction, explanation or understanding of this property or phenomena requires a continuum idealization and the corresponding continuum scale model or theory is indispensable; that is, the continuum scale model or theory is not reducible (derivable, deducible, or explainable from the lower scale model or theory) (Fletcher et al., 2019). The indispensabilists or essentialists claim that the indispensability or irreducible nature of the phenomena or property at the continuum scale to the lower scale can be accounted for in at least two ways: first, by that property or phenomena's being emergent at the continuum scale or second, by the ineliminable role that continuum idealizations play in isolating the counterfactual dependencies requisite for explanation (Woodward, 2003).

[28] Strictly speaking, this debate is not completely binary as some authors have staked out a middle ground, arguing for a kind of compatibilist approach between these two views (Ruetsche, 2011; Shech, 2013, 2015).

Regarding the former tack, the indispensabilists claim that continuum idealizations are necessary for capturing phenomena which are emergent at continuum scales. Rueger (2006) Batterman (2002, 2013), Maddy (2008), Morrison (2012), and Batterman and Rice (2014) have all argued for the indispensability of infinite or continuum idealizations along these lines. As Rueger observes: "Different scales allow us to 'see' different patterns in the distribution of microphysical behaviors; a behavioral pattern may be pertinent in a description at the macro level, but may be lost in a micro level description of the same system." He concludes: "This indicates that the macro-level description, within [some forms] of scientific explanation, has a certain inevitable autonomy —you cannot get rid of it in favor of the micro description alone." (Rueger, 2006, 342).

This ineliminability claim is underpinned by the further claim that while other modes of explanation (e.g. covering-law and causal-mechanistic explanations) are capable of accounting for why an instance of a phenomenon obtains, continuum idealizations are required to explain why types of patterns can be expected to obtain generally. Construction of a minimal model, for example, which may involve misrepresenting the target system at the micro-scale explicitly demonstrates why these micro-scale details are irrelevant to the explanation of some phenomenon of interest (Batterman and Rice, 2014). In the same spirit, Morrison avers that these kinds of continuum models are essential for isolating the higher-level, emergent, dynamical properties of the target system (Morrison, 2012).

Returning to the mystery with which we began, the manner in which indispensabilists account for the explanatory and predictive success of continuum idealizations is clear: continuum idealizations enjoy high predictive and explanatory success because they play an indispensable role in isolating emergent phenomena at continuum scales. Concerning their representational inaccuracy, the indispensabilists opt for a top-down, emergentist approach. From this vantage, it is hardly surprising that the microscale is distorted at the continuum level since there is no clean bottom-up reducibility of one to the other. The minimal model form of explanation in fact demonstrates why most micro-scale details are irrelevant to phenomena at higher, continuum scales (Holmes, 2020).

2.2 Continuum idealizations are dispensable in principle

Pace the indispensabilists, the dispensabilists claim that continuum idealizations do not substantively feature in mature scientific theory and as such are dispensable in principle (Earman, 2004; Butterfield, 2011; Norton, 2012). Continuum idealizations enable more mathematically convenient ways of explaining and predicting the phenomena at higher scales and so are pragmatically useful but not indispensable to the explanations in which they feature.

The dispensabilists insist on clear conditions for determining the (in)dispensability of idealizations to scientific theory. In the case of continuum idealizations, the concern is that these idealizations may not prove necessary to the existence of the higher-scale effect which they are meant to predict or explain. And one should not reify these limits or continuum models (Earman, 2004). To Earman’s point, John Norton has observed that the infinite idealizations –e.g. the thermodynamic limit– can be replaced by approximations in the renormalization group case (Norton, 2012).

Thus, idealizations in this case are deemed to be non-essential for predicting or explaining phase transitions once they are properly unmasked as approximations.

In addition to these kinds of constraints, another variant of the dispensability approach is given by Jeremy Butterfield (2011) who argues that continuum idealizations (particularly the thermodynamic limiting idealizations operative in renormalization group explanations of phase transitions) are in principle eliminable moves which permit greater computational tractability. Thus, these idealizations prove to be fundamentally unreal and their use can be justified as follows:

> *Straightforward Justification*: This justification consists of two obvious, very general, broadly instrumentalistic, reasons for using a model that adopts the limit $N = \infty$: mathematical convenience, and empirical adequacy (up to a required accuracy). So it also applies to many other models that are almost never cited in philosophical discussions of emergence and reduction. In particular, it applies to the many classical continuum models of fluids and solids, that are obtained by taking a limit of a classic atomistic model as the number of atoms N tends to infinity…(Butterfield, 2011, p. 1080).

The justification for the use of both infinite and continuum idealizations is indeed straightforward by Butterfield’s lights: these idealizations are pragmatically justified since they represent much more mathematically convenient techniques for modeling the phenomenon of interest. However, this does not imply that these infinitely valued parameters or continuum models are real or explanatorily essential in any meaningful sense.

Revisiting the continuum mystery, the dispensabilists partially account for the representational inaccuracy of continuum idealizations by noting their in principle dispensability in a mature scientific explanation. This disavowal of their playing an essential explanatory role permits them to retain a bottom-up view of reducibility as regards the target systems in which continuum idealizations are featured. Moreover, the pragmatic worth of continuum idealizations is taken to reside in their predictive success. Continuum idealizations are thus a kind of computational half-way house for scientific purposes until they can be swapped out for better, more representationally accurate ones.

After surveying the (in)dispensability debate which surrounds the issue of inter-theoretic reduction for continuum idealizations, we are left at an impasse. The indispensabilists and dispensabilists purport to account, at least in part, for both the representational inaccuracy as well as the explanatory and predictive success of continuum idealizations but fundamentally differ on both the proper view of reduction and whether these idealizations are merely standing in for better methods or capturing real, emergent phenomena at higher scales. The latter point leaves the prospects for settling this debate rather bleak since resolution is made to depend on whether future science will reveal infinite and/or continuum idealizations to be makeshift tools or rather a set of essential explanatory moves (Ruetsche, 2011). This all motivates taking a closer look at how scientists apply these methods in practice with the hope of making headway on this mystery.

3. Continuum Idealizations: An Applied Scientific Methods Approach in Soil Hydrology

Soil hydrologists confront a difficult kind of puzzle which recurs in multi-scale modeling throughout science. There are very good models for modeling fluid flow at the pore scale or at the spatial scale of 10^{-6} microns but there is a practical need to model these flows at the higher, soil field level which resides at a spatial scale of 10^4 meters. A further complicating factor is that at the microscale (pore-scale) for some phenomenon (fluid flows) there exists considerable geometric and physical complexity (heterogenous and discretized porosity) in addition to other computational obstacles (anisotropic flows, multiple dimensions, etc.). How can these factors be mitigated for the purpose of measuring hydraulic conductivity at the level of the soil field?

Table 1. The hierarchy of scales in soil hydrology

Observation Scale	**Scale Length**	**Model Techniques/Equations**	**Research Themes**
Pores	10^{-6} microns	Stokes' Equation	Multi-phase dynamics
Soil Fields	10^4 meters	Darcy's Law; Richards Equation	Plant-soil interactions, water run-off and transport
Landscape	$<10^4$ meters	Short-term and Long-term climate models	Water evaporation and transpiration; atmospheric heat transfer

Table 1. While our example concerns the bridging between the pore and soil field scales, the hierarchy extends even further to short and long range climate models where moisture retention in land surface is especially relevant for atmospheric heating and cooling.

Attempting to funnel this micron-for-micron complexity into modeling equations which are operative at the meters length-scale outpaces our powers of computability. The task at hand is to negotiate this gap by formation of some inter-scale scaffolding between the microscale, measured in microns, and the macroscale, measured in meters. As has been alluded to throughout, filling out this gap is achieved via the production of representative volume elements (RVEs). The formation of RVEs amounts to a kind of optimization process, the object of which is to furnish elements which remain somewhat representative of the target system's microstructure but given the least amount of possible detail.

Since the problem of how to bridge scales is pervasive in scientific modeling, it is worth initially considering this issue in a more general, step-wise sense. Inter-scalar bridging is an iterative process which commences with the setting of boundary conditions and side constraints. Boundary conditions are "specified sets of values that a differential equation must take at the boundary region of the problem's solution space" (Bursten, forthcoming). Less technically, the boundary conditions serve to delimit the spatio-temporal domain of inquiry by stipulating an appropriate interval of values which the differential equations must assume and thus prove invaluable in the bridging process: what may prove an essential detail at one scale often turns out to be unwanted, byzantine complexity at a higher scale. Boundary conditions both constrain and permit the derivation of boundary value problems which represent the solution to the relevant set of differential equations requisite for modeling the phenomenon at some scale (Green and

Batterman, 2017). The setting of boundary conditions is thus necessary for generating a well-posed problem or a problem in which there is exactly one solution and for which minor alternations in the initial and boundary conditions do not create major alterations in the behavior of the solution (Cain and Reynolds, 2010, p. 232).

Additionally, the formation of boundary conditions is abetted in cases of upscaling by what are known as homogenization techniques. Homogenization, which is also referred to as “upscaling” or “coarse graining”, involves description of some material “that is inhomogeneous at some lower length scale in terms of a (fictitious) energetically equivalent, homogenous reference material at some higher length scale” (Bohm, 2016, p. 4). Homogenization techniques allow for a homing in on the behavior of interest while peeling away the husk of lower-scale complexity. These techniques serve to spatially smooth the target system at lower scales, enabling parameters to be constructed which encode for the relevant lower scale detail while omitting the rest. After setting the boundary conditions, this generates a well-posed problem and the boundary value problem can be solved.

Returning once again to the soil hydrology case, the formation of RVEs and the process of winnowing down the microscale detail initiates with the setting of boundary conditions and other assumptions which act as a set of simplifying constraints. In the case of fluid flow, a “no-slip” boundary condition is assumed: for some viscous fluid *F*, at a solid boundary *S* (a fluid-solid interface), *F* will have zero velocity relative to *S* (Todd and Mays, 2004). The no-slip boundary condition’s function is to enable a simplification of the governing equations –i.e. the Navier-Stokes equations— which were famously unable to render exact solutions for the behavior of viscous fluids involving many layers.

The no-slip condition permits the problem to be simplified by relegating attention exclusively to the boundary layer (Morrison, 2018). In addition to the no-slip condition, it is assumed that the porous material which comprises the medium is non-fractal. Finally, the flow is assumed to have a uni-phase character or to run in only one direction (i.e. the flow is isotropic rather than anisotropic), constituting a dimensionality assumption.

Satisfaction of the no-slip boundary condition requires a continuum idealization whereby the pores are assumed to be both homogenous and continuous (Whitaker, 1999). This is clear when considering the meaning of the no-slip boundary condition since if the microstructure was inhomogeneous and discontinuous, slipping would occur since the fluid would not run perpendicular to the boundary but would "slip," complicating the fluid-solid interface. Absent the satisfaction of this condition, the boundary condition necessary for a well-posed problem in this case would disappear (Todd and Mays, 2004). Similarly, the flow is assumed to be saturated or uni-phase rather than multi-phase since a multi-phase or unsaturated flow would require a derivative for unsteadiness.[29] In other words, Darcy's Law would prove insufficient for the task of modeling these non-linear multi-phase flows and so the necessity of these boundary conditions and side constraints is made vivid. Similarly, the structure of the pores is assumed to be non-fractal since this enables homogenization which is explained below.

[29] Unsaturated or multi-phase flows are accounted for by Richards' equation which includes such a derivative for unsteadiness. Another similar instance is the problem of modeling turbulent flows which requires a more sophisticated, non-linear, multi-phase metric (Bokulich, 2018b).

Only with these boundary conditions or simplifying constraints in hand can the RVEs can be constructed. In our case of fluid flow through a porous medium, specifically, the strategy is to quantify K or the permeability tensor in “terms of a few geo-spatial characteristics” (Icardi et al., 2019). The formation of RVEs typically involves representing the microscale as a stochastic ensemble where the number of characters are given by the number of relevant pores and the relevant properties are their volume and radii. Volume elements are then obtained by a process of averaging over the characters of the ensemble. An additional and important methodological tool is homogenization. More specifically, homogenization is an upscaling technique which demonstrates the asymptotic convergence of the numerical values of the measurements towards some further value (Batterman, 2013). This can involve infinite limits ($\varepsilon \to \infty$) as witnessed in the (in)dispensability debate in section 2 or convergence on some finite number ($\varepsilon \to 1$). In our case, homogenization is used in in rounding off the stochastic properties of the pore structure —i.e. their radii and volume— and is enabled by the second constraint above. Since the microstructure is assumed to be nonfractal, the relevant RVEs are taken to converge towards some finite number (x) such that ($x\varepsilon \to x$).

With the homogenization technique, no-slip boundary condition and dimensionality assumption —all of which are either permitted by treatment of the microstructure as a continuum or necessary for forming the relevant boundary conditions— the RVEs about pore-structure are then obtainable from a spatial stochastic averaging process (Bear, 1972). This involves representing the microstructure or the pore shape and radii as a set of porosity functions ($F(x)$) which are expressed as a multi-Gaussian random field (Icardi et al., 2019). By averaging over the pore functions, $F(x)$, stochastic features such as the mean and variance of these functions (μ and σ) are extracted and our RVEs derived. This enables the transmission of the relevant information about the microstructure to be inputted into the macroscale parameter or $F(x) \rightarrow K(x)$. Soil hydrologists are now in position to calculate fluid flows through a porous medium at the soil field scale with use of Darcy's Law or equation 2 below:

$$q = -\frac{K}{\mu}\nabla p \qquad (2)$$

The upscaling process, however, does not terminate here. The suitability of the RVEs and the simplifying constraints which enabled them are then run through a gauntlet of tests and experiments which serve to safeguard against certain microscale perturbations which may problematize the model. A fuller picture of upscaling is provided below in Figure 1:

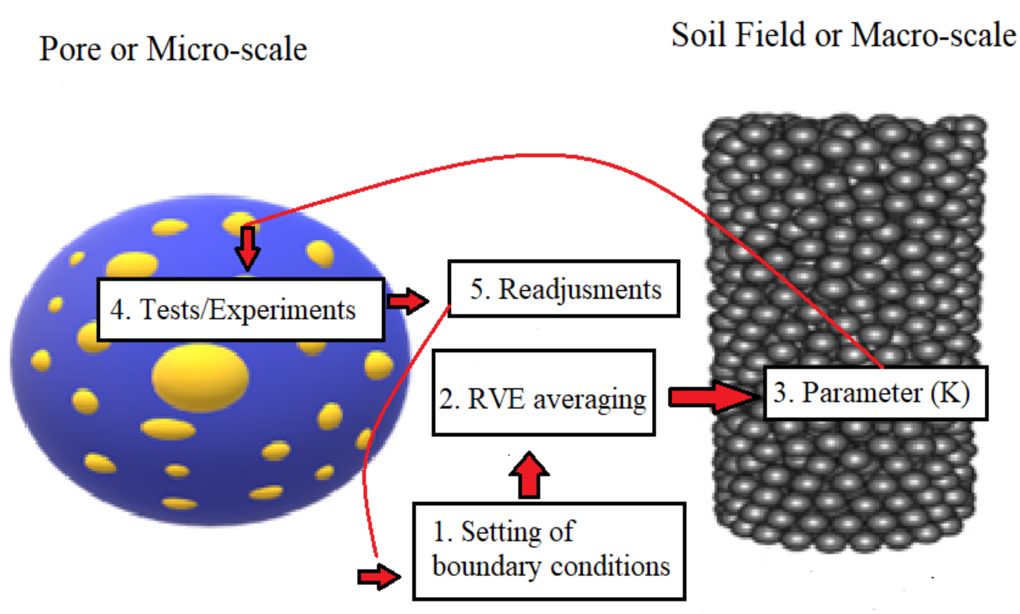

Figure 1. Upscaling process from micro to macroscale as a feedback process. The sphere on the left represents a particular pore at the micro or pore scale which includes geometric structural

heterogeneity prior to upscaling whereas the figure on the left represents a soil field at the macro-scale where each sphere represents a spatially smoothed set of pores. The upscaling process is represented here as an iterative process which involves feedback.

In brief, this case demonstrates the importance of continuum idealizations in constructing the desired continuum model of fluid flow or hydraulic conductivity at the soil field level. This process begins with a microscopic model and coding for specified features about pore structure (their volume and radii). Construction of this microscale model also necessitates certain boundary assumptions; e.g. the no-slip condition. Homogenization techniques are then deployed in order to construct RVEs which involve optimizing the trade-off in effective size of the volume element between calculability and simplification of details and to effectively upscale the microscale model in order to determine an effective value for our continuum parameter K, the permeability tensor. The setting of boundary conditions, the application of homogenization techniques and the generation of a well-posed problem are enabled by the requisite continuum idealization.

As is clear in Figure 1, assessing the viability of the RVEs involves further inter-scalar communication. An example of the potential failure of the macroscale equations due to microscale perturbations are “hysteresis effects” whereby small changes at the microscale can aggregate up to significant changes at the mesoscale.[30] Hysteresis effects

[30] An illustrative example of hysteresis effects is given by Mark Wilson which involves the hysteresis effect caused by subjecting a 1meter steel rail to repeated compression and decompression (e.g. banging on it with a hammer). These forces cause tiny fractures at the microscale which can eventually result in the macroscale effect of the rail cracking (Wilson, 2017). Accordingly, material physicists must be vigilant about certain microscale details in assessing the model for adequacy.

are important in that they constitute a range of conditions in which application of the model to the target system will fail due to physical changes in the target system. Thus, RVE assessment involves monitoring for hysteresis effects in order to ensure the continued applicability of the model to the physically changing target system and this underscores the importance of inter-scalar communication even after RVEs have been constructed.

An informative heuristic for envisioning the concept of hysteresis is the old saw about the stalk of straw which broke the camel's back —minor alterations in the camel's load eventually spill over spectacularly at the macroscale. In soil hydrology, a common hysteresis effect involves gradual changes in the moisture content of the media or processes that are commonly referred to as "wetting" and "drying" (Vogel, 2019). As water travels through a porous media, the porosity of the media and the uniformity of the pore structure determines how much water changes the moisture content of the material: media which experiences more wetting or material with lower permeability due to lower porosity and structural uniformity, will tend to accumulate a higher moisture content as water flows through it over time whereas media which is prone to less wetting or more drying, or material with higher permeability due to higher porosity and uniformity, will accumulate less. Over time, gradual and minor changes in moisture content at the micro or pore scale can often aggregate up to drastic changes at the macroscale: namely, a saturated flow can become an unsaturated one, complicating the applicability of Darcy's Law via the failure of the dimensionality assumption (Whitaker, 1999). When a flow becomes unsaturated, it is no longer isotropic but anisotropic and so the equations will fail due to a violation of the dimensionality assumption side constraint. To guard against this and other hysteresis effects, hydrologists are often crosschecking, looking back to the pore-scale to monitor these kinds of alterations.

Upscaling in soil hydrology thus informs a more nuanced understanding of both the negotiation of inter-scalar relationships in scientific models and the place of continuum idealizations. The process of upscaling draws on optimization procedures and feedback loops between scales in order to satisfy some goal-directed aim. Soil hydrologists do not approach the inter-scalar chasm with nothing more than open-hearted intellectual curiosity but are rather guided by practical modeling concerns from the jump. Their aim is to parametrize microscale detail such that macroscale equations of greater spatial range can be carried out. This goal-directedness is alloyed directly into the set of optimization instructions for constructing RVEs: code for as little microscale detail as possible without loss of representation where representation is taken to mean something like “make sure the volume is small enough to ensure that it does not include large scale changes in the value of the effective property.”[31] And this injunction to optimize is often satisfied by portraying the microstructure as a continuum via the enlistment of boundary conditions and simplifying side constraints. This optimization process which involves trading off between representationalism (the minimum effective volume of the RVE) and simplification (the maximum effective size of the volume element) is glossed by a soil

[31] The notion of representation used as a constraint in this RVE optimization process is cashed out more fully below in the excerpt from Koestel et al., 2020. It should not be confused with what is typically meant by “representational accuracy” in the modeling literature since it has more to do with the minimum size or the minima of the RVE element and less with a correspondence between model and target system.

hydrologist who describes the process of forming RVEs and taming the geometric complexity of the pores for parameterization as follows:

> The [RVE] range includes scales for which the effective property of interest (e.g. porosity) is constant with the change of considered volume. To qualify as an RVE, the volume needs to be i) large enough so that the effective property does not change when the volume is slightly increased and ii) small enough so that it does not include larger scale changes in the effective property, e.g. a drift in porosity due to macroscopic heterogeneities such as a transition between different horizons in a soil profile (Koestel et al., 2020, p. 2).

Further, the construction process requires continual checking-in with the microscale level even after macroscale parametrization has occurred. Testing and experiments are conducted for the assessment of the RVEs and the microscale is revisited in order to verify that certain hysteresis effects are guarded against. The picture drawn by the applied methods approach from soil hydrology is decidedly a busier one than many extant views about idealization and modeling have led us to expect. However, this schema illuminates several important qualities without which the mystery of these relationships would certainly persist.

4. The Place of Continuum Idealizations in Multi-scalar Scientific Models

The tools outlined by the case of soil hydrology can now be applied in returning to the mystery about both the predictive/explanatory success and intertheoretic reduction as it pertains to the case of continuum idealizations. My response to this mystery unfolds in two parts. In 4.1, I consider how the process of RVE construction fortifies the case for the unique, pragmatic indispensability of continuum idealizations. In 4.2, RVEs are shown to imply the need for a more holistic conception of model adequacy.

4.1 Fortifying the case for the unique, pragmatic indispensability of continuum idealizations

Recall that our canvassing of the dilemma concerning continuum idealizations concluded with acknowledgement that neither the indispensabilist nor the dispensabilist position was determinate and that the prospects for settling the debate appeared dim owing to the fact that resolution about the (in)dispensability of continuum idealizations depends upon a verdict that only future science can render. I shall argue that the morals of the process of RVE construction lend needed specificity to the case for the unique and pragmatic indispensability of continuum idealizations, which recommends assuming a deflationary position towards the (in)dispensability debate.

In the RVE process, continuum idealizations are deployed to eliminate the considerable and unwanted microscale complexity —physical and geometrical heterogeneity. In the soil hydrology case, the use of continuum idealizations was necessary for implementing the no-slip boundary condition which acted as an important simplifying assumption for streamlining the case as well as the application of homogenization or upscaling techniques. This and other side constraints —the dimensionality and homogenization techniques the usage of which was unlocked by continuum idealizations— enabled the microscale to be averaged over and the RVEs to be obtained for upper scale parameterization; i.e. for our K parameter to be constructed at the continuum scale.

While one justification of these upscaling maneuvers would seem to be computational tractability, this drastically understates their crucial role in multi-scalar modeling. Beyond simplification, the continuum idealizations also permit the crucial inter-scalar interfacing or feedback cycles to be realized as well as the optimization to occur which is vital to the construction of RVEs. Absent idealizing about the microscale as a continuum, communication between micro and macroscale would have been precluded —the lower scale complexity would have acted as a barrier for accessing the macroscale in our soil hydrology case. In enabling the construction of RVEs which act as a kind of intermediate or pidgin language between scales, the pragmatic value of continuum idealizations runs beyond mere mathematical convenience. In this respect, continuum idealizations are importantly different from garden variety idealizations which involve eliminating or falsifying irrelevant details for the sake of mere tractability (Cartwright, 1983; Weisberg, 2007). Moreover, the operative continuum idealizations in the soil hydrology case but in other scientific cases as well were necessary for the setting of boundary conditions and the construction of a well-posed boundary value problem, the solution of which permitted the macroscale behavior to be isolated. Additionally, the continuum idealizations allowed application of the relevant homogenization techniques at a continuum scale –or RVE construction in our case. Without continuum idealizations, the ascent to the macroscale vantage which was a necessary step in capturing the behavior of the target system at that scale would've be foreclosed. The inter-theoretic reduction debate which surrounds the (in)dispensability of continuum idealizations elides the crucial roles played by these idealizations in multi-scalar models, as Mark Wilson observes: "Nagelian reductionists wrongly view these scale relationships as inter-theoretic reductions between the levels of some theory. But these kinds of reductions often fail, leaving us in the waters of mystery" (Wilson, 2017, p. 220).

The road to demystification is paved by taking a deflationary attitude towards this problem. The role of continuum idealizations in multi-scalar modeling contexts is at least three-fold: to permit the necessary spatio-temporal delimiting of the problem space (i.e. setting boundary conditions and construction of the relevant, well-posed boundary value problem); allowing for the inter-scalar communication which is requisite for ensuring against hysteresis effects and the application of homogenization techniques; and finally for the optima to be discovered between the competing interests of minimal representationalism (effective minimum size of the volume element) and predictive/explanatory success regarding the phenomenon of interest. This far outstrips the story about mathematical convenience which issues from the dispensabilist camp. Only by transcending the reduction debate and understanding these methods in application can the genuinely unique role of continuum idealizations be appreciated. A deflationary view about the inter-theoretic reduction enables the core argument to be grasped: continuum idealizations turn out to be pragmatically ineliminable to the multi-scalar models in which they inhere.

A dispensabilist may reply that pragmatic ineliminability aside, a genuine ontological problem remains on the table. This problem, roughly, can be stated as follows: "how can continuum idealizations which so perversely distort the ontology of the system's microstructure provide a model which tells us anything genuine about the system?" An answer is that these continuum idealizations enable a homogenization of the system's microstructure —RVEs to be formed— which is necessary for capturing that system's behavior at higher scales.[32] Yet this naturally raises a question about how this technique is even possible: why doesn't a pervasive distortion of the target system's microstructure outright preclude predictive and explanatory success? As Tao notes, when a system includes too many interacting components to permit feasible computation, the system is said to suffer from the "curse of dimensionality" (Tao 2012). Surprisingly, however, these higher scale phenomena can be captured at the macro-level by often ignoring this lower-level complexity. As Tao observes:

> Even more surprising, these macroscopic laws for the overall system are largely independent of their microscopic counterparts that govern the individual components of the system. One could replace the microscopic components by completely different types of objects and obtain the same governing law at the macroscopic level. When this occurs, we say the macroscopic law is *universal.*" (2012, p. 24).

Universality is indeed a revelatory property but it also aids in accounting for why the kind of optimization procedure used in RVE construction can succeed despite eliminating ostensibly relevant details (Batterman, 2018). Much of the target system's lower scale detail simply proves irrelevant to an analysis of said system at the continuum scale.

Once again, the initial dispensability objection to the pragmatic approach can be revived and redirected at the foregoing. The narrative about minimal representation

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[32] The concept of minimal representation under discussion here is similar to what Batterman calls "universality" or the inter-systems phenomenon whereby systems of varying heterogenous microstructures exhibit shared higher-scale behavior (Batterman, 2002). The concept of minimal representation I discuss here dovetails with what Batterman and Rice refer to as "minimal models" (Batterman and Rice, 2014).

recited here may mitigate the ontological mysteriousness of continuum idealizations in application but, the objection might run, this is no bar to their dispensability *in principle*. This narrative merely demonstrates strong pragmatic grounds for their indispensability but this is hardly reason to regard them as in principle indispensable.

A response can begin with the acknowledgement that in scenarios like the ideal gas case from statistical mechanics, a kind of bottom-up analysis is achievable whereby the molecules are homogenized at a lower scale and this enables higher scale properties such as their pressure to be adduced. This coheres well with the “in principle dispensability” claim and is undoubtedly guilty of breathing life into the dispensabilist position. However, this proves to be more exception than rule. In the case of renormalization group explanations –which involves thermodynamic limit taking about systems going through phase transition where these systems’ microstructures are represented as lattice systems– the lattice systems appear homogenous away from the point of criticality but appear heterogenous around the point of criticality (Batterman, 2010). Deciding whether to treat them as homogenous or heterogenous, i.e. to apply a continuum idealization, ineliminably involves the higher scale vantage (at the point of criticality) wherein this difference in character with respect to the relevant system phases is accessible. This is a crucial point which dispensabilists such as Butterfield simply miss (Batterman, 2013). From the bottom-up perspective, there simply is no way to construct the relevant boundary value problem or decide on the adoption of the relevant boundary conditions. The bottom-up vantage proves insufficient for making this decision about how to represent the target systems’ microstructures (Batterman, 2013). Often in multi-scalar models the kinds of cross-scalar dependencies which are locatable through feedback will require multi-scalar vantages (Green and Batterman, 2017). And these vantages necessarily require inter-scalar feedback and communication, processes which are afforded by continuum idealizations. The claim that the continuum idealizations which are operative in continuum cases like these are in principle dispensable turns out to be either dubious or largely orthogonal to how these methods are applied in practice. The Horatian caveat that there is more under heaven and on Earth than is dreamt up in one's philosophy is especially apt here.

4.2 Towards a holistic conception of model adequacy

Multi-scalar models which depend upon upscaling or construction of RVEs to bridge scales recommend both a more nuanced and holistic conception of model adequacy. The optimization process for RVE construction whereby explanatory power is traded off against minimal representationalism as well as the more complicated feedback cycles which range over multiple scales tells against the simplicity of the inter-theoretic reduction debate. Recall that both for the dispensabilists and indispensabilists, (in)dispensability turned on the reduction of the continuum to microscale. RVE construction subverts this claim. The feedback cycles crucially rely upon model holism or levels of scale not being modular in character —which microscale details are relevant is determined from upper scale vantages and RVE formation depends upon interfacing between scales.

Moreover, the seeming tension between representational accuracy, reduction and predictive/explanatory success in the problem which comprised the initial mystery about multi-scalar models is in part dispelled by the process of upscaling. Viewing these features as discrete and unrelated is omissive of the trading off which is negotiated between representational accuracy and practical modeling concerns in RVE construction. Lower level detail is purposely minimized where this minimization is constrained by representation requirements. However, this does not eo ipso cast the model as inadequate from an explanatory perspective nor does it provide grounds for the existence of emergent phenomena at higher scales per se. And so the correct attitude towards making the indispensability of continuum idealizations dependent upon inter-theoretic reduction would seem to be a deflationary one. This proposal echoes the similar deflationary tone adopted more recently about the indispensability of continuum models (Green and Batterman, 2017; Wilson, 2017; Bursten, 2018; Batterman and Green, 2020).

This all motivates a view of model adequacy which can account for the richness and complexity of continuum models. Recent proposals for a more holistic view of models which understand the criteria for model adequacy in a more contextual manner have proliferated (Potochnik, 2010; Bokulich, 2018a, 2018b; Rice, 2019). One such view is Wendy Parker's "adequacy-for-purpose" view (Parker, 2020). On this view, evaluation of a model is indexed to a context of use. The model is then evaluated for a particular context of use along the lines of four variables including: a model's adequacy for some user, methodology, target and circumstances. On this view, even representational accuracy is understood within some context of use. This makes representational accuracy less a discrete variable or feature and more an appropriately versatile standard of model evaluation.[33] This coheres well with the optimization process which is blended directly into the construction of RVEs. The adequacy-for-purpose view also incorporates the

[33] This model holism approach and the optimization proposal on offer here also echoes Potochnik's concept of groups of models which are epistemically interdependent but explanatorily independent. This provides one way of responding to the tyranny of the scales problem mentioned earlier and has great purchase in the arena of multi-scalar modeling.

needed holism which is the stock and trade of multi-scalar scientific models, thus avoiding the crude oversimplifications which ran rampant in the (in)dispensability debate.

5. Conclusion

In most standard accounts of idealizations, the generality of their description belies the breadth of their functional complexity. Continuum idealizations represent a subset of intricate tools in model construction which are much more functionally sophisticated than most caricatures of idealizations would suggest. Consideration of their deployment, particularly how they prove pragmatically ineliminable to the process of RVE construction in multi-scalar scientific models, informs and thus necessitates a richer conception of both scientific idealizations and model adequacy. This conception should make room for accommodation of the kinds of optimization procedures prevalent in upscaling as well as integrate the kind of feedback processes which facilitate interfacing between scales. These revisions in the treatment of idealizations are requisite for crafting an evaluative framework of model adequacy which better comports with scientific practice and does not purchase evaluative simplicity at the cost of trivializing important aspects of scientific modeling.

CONCLUSION

Recall that this project set the refutation of the following two core assumptions in the area of scientific model-based explanation as its primary aim:

1.) Ontic Assumption: Scientific explanations ought to track causal or constitutive relations between the explanans and explanandum event.

2.) Inter-theoretic Reduction Assumption: The idealizations in scientific models can be understood as either epistemically dispensable or indispensable to the model-based explanations in which they feature.

The first two chapters of this dissertation push back against (1) or the ontic assumption as follows.

The first chapter investigated the area of scientific model-based explanations in cognitive science. Debates in this area have been included a significant amount of discussion as regards (1) with some proponents arguing that some cognitive models fail to provide explanations in virtue of failing to satisfy (1). Alternatively, countervailing advocates deny that (1) represents a necessary condition on scientific model-based explanation. Their strategy has been to both introduce counterexamples to (1) in the form of cognitive models that appear explanatory or to introduce alternative options to the mechanist or ontic conception of scientific explanation which their opponents embrace. The first chapter made inroads on this debate by providing a non-ontic or non-causal explanatory interpretation of an influential model in cognitive science; namely, the Haken-Kelso-Bunz model of bimanual coordination. This model clearly fails (1) insofar as it makes no attempt to map its model components onto the underlying neural substrate of its target system nor does it isolate any causal relationships within the target system it treats. However, I argue that we ought to understand the macro-level pattern which the HKB model isolates as explanatory, the failure of (1) notwithstanding. To make this case, I demonstrate how a minimal models explanation can be cobbled out of the pattern which HKB isolates. The upshot of the first chapter is that the plausibility of (1) as a necessary restriction of scientific model-based explanation is cast into doubt and the domain of potential explanatory possibilities for models in cognitive science is properly broadened past the narrow set of possibilities which proponents of (1) restrict this possibility space to. More directly, if there are models in cognitive science which proffer model-based explanations without satisfying (1), then it is dubious that the ontic assumption really does represent anything like a necessary condition on scientific model-based explanation.

The second chapter confronts a problem for non-ontic or non-causal scientific explanations which is known as the "problem of directionality." This problem is a variant of the famed "flagpole objection" to Hempel's D-N view which problematically demonstrated that D-N explanations were symmetrical or could be made to run both forwards and backwards. According to defenders of (1), non-ontic explanations succumb to this problem by denying (1) since, unlike ontic explanations which require that explanations be asymmetrical, there is nothing on the non-ontic view which precludes explanations from being bi-directional. This is a formidable challenge since any view of scientific explanation which counts symmetrical explanations as genuine ones is clearly too permissive to be taken seriously. The second chapter poses a solution to this problem in the form of introducing an additional necessary condition on scientific explanation: the presuppositional contextual appropriateness condition or PCAC. I diagnose the reversed or symmetrical explanations as instance of presuppositional failure of a modal sort. Additionally, I show, pace the proponents of (1), that the problem of presuppositional failure is not one which uniquely afflicts non-ontic or non-causal explanations; indeed, the problem can arise for ontic explanations as well. Once the PCAC is adopted as an additional necessary condition of scientific explanation, the reversed or symmetrical cases are ruled out as non-explanatory. Thus, a solution is achieved for non-ontic explanations which does not involve surrendering the core claim that there are scientific explanations which are non-ontic. The upshot is that a significant challenge to the viability of non-ontic scientific explanations is resolved.

The third chapter attempts to refute (2) where this assumption can be understood as giving priority to the issue of inter-theoretic reduction. Many philosophers of science have assumed that in idealizations in scientific models ought to be understood as either epistemically dispensable or indispensable in principle. Once this status is sorted out, we will then have some greater understanding about the nature of these idealizations as either beacons of emergentism or simply workarounds until we arrive at some ideally completed physics. I take a deflationary line towards this debate, arguing that the inter-theoretic reduction question has wrongly received too much attention. Both stances on this issue are shown to be fraught. A more promising approach is to examine how scientists employ idealizations in practice in order to gain insight into their role in model-based explanation. I examine continuum idealizations, focusing on soil hydrological models. An in-depth examination of these idealizations is then used to provide an interpretation of continuum idealizations as uniquely pragmatically indispensable to the models in which they feature. Thus, a story can be told about the place of idealizations in scientific models which mostly ignores (2) as a rabbit hole we are best advised to avoid tumbling into.

I conclude this summary of the project with a few brief comments about future work and some global remarks on the project generally. First, since completion of this project, I have returned to the topic of chapter 1, investigating whether there are additional examples of cognitive model-based explanation which do not satisfy (1). In a recent paper entitled "Cognitive Extra-Mathematical Explanations," I provide an additional example of a cognitive model-based explanation (i.e. efficient coding models) which can best be understood as providing a mathematical or non-ontic explanation. This goes some way towards confirming the insight in the first chapter that there are indeed many more examples of non-ontic scientific explanatory models in cognitive science than the proponent of (1) would have us believe. Second, much of the debate about the viability of (1) ignores the issue of complex systems. Complexity is increasingly the focus of scientific investigation and target systems which manifest this feature often can best be approached via the pattern-level. This stands in stark contrast to the requirements of (1). In future work, I will continue to investigate the nature of complexity not solely as a refutation of (1) but rather as a way to understand the full possibility space for scientific model-based explanation.

In evaluating the novelty of one's insights, one is reminded of that platitudinous caveat "there is nothing new under the sun." In the course of this project the newness of the material is relative. Presumably, there is little that is new here to scientific praxis but perhaps much that appears as such to philosophers. While the two core assumptions that this project has aimed to refute —namely, that tracking causal or constitutive relations are requisite for something to be an explanation and that the status of idealizations importantly hinges on their epistemic (in)dispensability— are plausible assumptions, this project has aimed to show that the possibility space for scientific explanation is considerably richer than many philosophers have traditionally envisioned it to be. Accordingly, there appears to be more under heaven and on Earth than has been in our philosophy but rather than to leave one feeling stymied, this realization should be greeted with optimism. There is more work for philosophers to do and this project will have contributed something if it has but briefly illuminated the avenues towards further, fruitful inquiry.

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