Laws of Nature and their Supporting Casts

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What role do laws of nature play in the process of scientific inquiry? In answering this question, philosophers have tended to focus on a handful of predictive and explanatory roles with which laws have been traditionally associated. I argue that this traditional focus overlooks an important fact about scientific practice: before they can be of any predictive or explanatory use, laws must often be supplemented by a wide variety of modelling ingredients, such as material parameters, boundary conditions, auxiliary models, and so on – what I call their *supporting casts*. As a result, many accounts of laws have trouble conferring lawhood on the kinds of generalisations and principles worth calling laws without *also* conferring it on those features that play a merely supporting role. I argue that the key to avoiding this problem lies in recognising the important role that laws play in helping us to *coordinate* the various different kinds of information we must make use of in our attempts to model some system and thus explain or predict its behaviour. In addition to contributing to a more complete philosophical picture of scientific methodology surrounding laws, my account of this *coordinating role* also helps us to identify several *distinct* explanatory and predictive roles that laws play in scientific practice.

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Preface

It seems to me that there are others, besides myself, who deserve some share of the blame for the state of the document to come (and, indeed, for its very existence). Chief amongst the accused stand my advisors, Bob Batterman and Mark Wilson. Bob's graduate seminar on Models and Modelling in Science, which I took in my first year at Pitt, was largely responsible for my turning to the philosophy of science. Since then, he has been a constant source of insight and encouragement in the face of my various attempts to inflict my thoughts on an unsuspecting philosophical community. In addition to guiding me through the world of scientific models and applied mathematics, Bob also tirelessly guided me through the far more daunting world of the academic job market. It is in retrospect hard to imagine what it would have been like to run that gauntlet without his advice and support. In these respects and many others I owe him more than I could possibly repay, though I may begin by picking up a bar tab or two at Butterjoint in return.

For his part, Mark was responsible for my being at Pitt in the first place, primarily as a result of his tome *Wandering Significance*. It is not, I think, difficult to see how Mark's way of thinking about matters of scientific detail and philosophical abstraction have influenced my own. In the years since I arrived, Mark has consistently challenged me to capture as clearly and directly as possible the details of scientific practice that strike me as philosophically rich and important. Along the way, Mark was patient and consistent in helping me to develop my own outlook on the philosophical terrain surrounding science, metaphysics, and language. Without his efforts, the document to come would be in a very different state, and for that I am extremely grateful.

Next to Bob and Mark stand the rest of my committee – Erica Shumener, David Wallace, and Gordon Belot. It always struck me that the three of them effortlessly struck the right balance between understanding the Bob-and-Mark-shaped axes I was attempting to grind on the one hand and keeping me honest in matters of detail on the other. I will be forever grateful for Erica's suggestion that the literature on laws of nature might be a place where the kinds of thoughts I had about models and explanation in science could be put to good

use. I will also be grateful for the many hours that both David and Gordon spent with me working through, amongst other things, the claims that I made about various scientific endeavours – without their efforts I am certain that this dissertation would lean even more in the 'preaching to the choir' direction than it already does.

Finally, no small amount of blame ought to be apportioned to my friends in the philosophical community at Pitt. A whole bevy of graduate students in the department contributed in their own ways: pushing me on various points when I presented at Work-in-Progress talks, offering extremely insightful thoughts during our dissertation seminar, talking about my various philosophy-shaped brain bubbles, making grad school bearable through company and many a beer at The Cage, Old Man Breakfasts at Pamela's, and so on, and so on. It is difficult to name everyone, but in no particular order, the following people stand accused: Aaron Segal, Klara Andersson, Marco Maggiani, Rajiv Hurangahee, Taylor Koles, Stephen Mackereth, Patrick Chandler, Alexander Johnstone, James McCord.

It of course goes without saying that the lion's share of the blame for the shape of the document to come should fall squarely on my shoulders. Nonetheless, if anyone is looking for others to hold accountable when the reckoning comes, I'd start with those named above.

1.0 Introduction

Philosophers have traditionally agreed that *laws of nature*, whatever kind of metaphysical gizmo we should take them to be, bear *some kind* of very close connection to the assemblage of principles and generalisations that scientists make use of when they go about their business. That is, although the term *law of nature* may represent something of a philosopher's term of art, the kinds of discussions in which metaphysicians engage when they put forward or critique various 'accounts of laws' must eventually be answerable *in some way* to facts about what goes on in the course of scientific inquiry.

For example, although it might be difficult to write down an *exhaustive* list of the particular scientific laws that we should consider to be full-blown *laws of nature*, we might think that there are at least some paradigmatic candidates: Newton's laws, the laws of thermodynamics, the Einstein field equations, the Schrödinger equation, and so on. We may then suppose that any account of laws that can't *somehow* make sense of how such scientific exemplars might count as laws of nature is, all else being equal, worse off. Put differently, we might demand that a metaphysical account of laws ensure that at least some subset of privileged scientific principles be captured in the extension of the concept of a *law of nature*.

These kinds of *extensional* demands represent one way that philosophers working on laws of nature have tried to bring their metaphysical claims and accounts into contact with the details of scientific practice. Such demands also do not require that we look *too* closely at the messy details of scientific practice in order to follow their guidance. As a result, philosophers have thought it possible to satisfy such demands while largely treating laws as represented paradigmatically by generalisations of the form "*All Fs are Gs*." David Armstrong, for instance, declared that although any account of laws should *eventually* be capable of rendering the verdict that, say, Newton's law of universal gravitation counts as a law of nature, we need not concern ourselves with such details from the outset. In this vein, Armstrong writes that

> "It turns out, as a matter of fact, that the sort of fundamental investigation which we are undertaking can largely proceed with mere schemata of this sort [i.e. "it is a law that Fs

> are Gs"] [...] Our abstract formulae may actually exhibit the heart of many philosophical problems about laws of nature, disentangled from confusing empirical detail. To every subject, its appropriate level of abstraction." (Armstrong (1983, pp. 6–7))

Such extensional criteria, however, do not necessarily represent a tremendously difficult challenge to meet. If we content ourselves with describing things in schematic enough terms, it seems that we might construe, say, Newton's second law either as a necessary connection between universals à la Armstrong, or as an axiom of the best system à la David Lewis (1973, 1983, 1986, 1994), or as a statement exhibiting a certain kind of counterfactual stability à la Marc Lange (2000), and so on. That is, as long as we remain at a certain level of abstraction, such extensional criteria may not appear to provide us with a clear enough *point of difference* that we might leverage to decide between competing accounts of laws.

If such extensional criteria do not provide us with the point of different that we require, then we may perhaps turn to more detailed claims about *scientific practice*. If it is true that the way that scientists employ laws in the course of their investigation requires that they exhibit some particular characteristic or be fit for some particular kind of task, then we might think this presents us with an additional criterion for choosing between accounts of laws. That is, we may think that a metaphysical account of laws should not only get the *extension* of the concept of law right but should also help us to understand how the metaphysical gizmo it takes laws to be could exhibit the relevant characteristic or perform the relevant task.

In this vein, claims about scientific practice, and more specifically about the *role that laws play in scientific practice*, have come to occupy an important place in philosophical discussions of laws of nature. In a slogan, facts about what laws *do* are appealed to in order to constrain or drive our attempts to answer questions about what laws *are*. It is very common, then, to see arguments that proceed as follows. It is a fact that laws of nature perform XYZ role in the context of scientific practice. If metaphysical account ABC is correct, then it is mysterious how on earth laws could indeed be used by scientists to do XYZ. Metaphysical account ABC thus cannot be correct and should be rejected.

I do not think that there is *necessarily* anything wrong with this way of bringing questions about the metaphysics of laws into contact with facts about scientific practice. For this approach to work, however, two things need to be the case. First, it needs to be *true* that laws do, in general, perform XYZ role in scientific practice. Second, it needs to be possible to describe this XYZ role in reasonably abstract terms, otherwise it will be difficult to see how different accounts of laws may exhibit different abilities to account for the fact that scientists use laws to do XYZ. Suppose that our description of this XYZ role appeals to an array of quite complex and messy considerations regarding applied mathematics and the process of scientific modelling. It may be *true* that laws play XYZ role in scientific practice, and yet quite unclear how it is that this fact should guide philosophers in their attempt to answer metaphysical questions about laws of nature.

The papers that make up this dissertation all in their own way revolve around the observation that these two requirements are far harder to satisfy than many philosophers realise. Put differently, it strikes me that philosophers writing about laws systematically underestimate how difficult it is to extract general, schematic, and true 'facts about scientific practice' from the observation of scientific methodology. The central refrain of this dissertation, then, will be that scientists use laws in many interesting and complicated ways to perform a variety of tasks in the course of their inquiries, and so getting a philosophical handle on the law-related aspects of scientific methodology will involve digging deeper than slogans like 'laws explain their instances.'

As it happens, I am relatively uninterested in questions about the metaphysics of laws of nature. Although this dissertation discusses and criticises various metaphysical accounts of laws, my intent is not to put forward anything my own metaphysical view. Rather, I hope to begin the process of dragging philosophical questions about scientific methodology and laws out of the long shadow cast by the metaphysical literature on laws. Questions about the role that laws play in scientific practice are rich and important philosophical questions in their own right, and we are unlikely to make much progress on answering them if they are only addressed *en passant* within metaphysical discussions of laws. Moreover, tackling these questions in their own right may well involve accepting that the category of principles, statements, and generalisations we deem to be 'scientific laws' may exhibit more internal variation and heterogeneity than we can properly appreciate when we have one eye on abstract metaphysical questions about laws of nature.

Here, then, is the plan. In Chapter 1, 'Laws of Nature and their Supporting Casts,' I begin with the observation that before they can be of any predictive or explanatory use, laws must often be supplemented by a wide variety of modelling ingredients, such as material parameters, boundary conditions, auxiliary models, and so on – what I call their *supporting casts*. I argue that this observation spells trouble for a class of 'Pragmatic Humean' accounts that attempt to pick out laws by appealing to the predictive and/or explanatory roles that they play in scientific practice.

The issue is that although laws are surely *involved* in such predictive and explanatory contexts, taken by themselves they are almost always predictively and explanatorily inert. In most cases, it is a complicated *package* of laws and supporting constructions that allows us to predict and explain the behaviour of various systems, rather than simply the law *itself*. Thus if laws are just the principles and generalisations that play a certain predictive or explanatory role in scientific practice, then it seems that we are forced to confer lawhood not just on the starring lawlike generalisations and principles, but also on the *entire supporting cast*. If we only consider the explanatory and predictive roles of laws, we will not have the resources to distinguish between principles and generalisations worth calling laws and those that play a merely supporting role.

In Chapter 2, 'The Different Explanatory Roles of Laws,' I address the question of whether we should in the first place expect there to be a *single* explanatory role that laws play in scientific practice. In doing so, I argue that different kinds of laws can make *different* kinds of contributions to the explanatory and predictive projects of scientific inquiry. Where some laws might facilitate explanations and predictions by generating descriptions of the behaviour of systems in a relatively direct fashion, others might play a more indirect role in helping us to define and measure the features of some system that *themselves* carry explanatory or predictive import. Hooke's law does not *itself* explain why some system exhibits a linear elastic response to some applied stress, but it does allow us to define and measure the features of the material that *do*. If this is right, then it is not clear that it is at all true that 'laws explain their instances,' as is often claimed.

Finally, in Chapter 3, 'The Coordinating Role of Laws in Empirical Science,' I begin to put together part of a more nuanced and authentic picture of the role that laws play in scientific practice. Philosophers have typically attempted to answer the question of what role laws play in scientific practice by writing down something like a 'job description' for laws: a list of tasks that they perform more or less single-handedly in the course of scientific inquiry. I argue that this approach will not work, since as we saw in Chapter 1, laws are not equipped to do very much at all without their supporting cast. I put forward an alternative approach, on which we attempt to answer the question of the role that laws play in scientific practice by considering the contribution they make to the construction of scientific models.

Along these lines, I suggest that laws play *coordinating role* in scientific practice: they provide coordinating frameworks for the construction of scientific models. Here's an analogy: building a house requires a sort of central frame to hold together the relevant materials. This central frame supports the materials involved in different ways and mediates the interactions between them. The roof tiles don't have much to do with the foundation *directly*, but the weight of the tiles is distributed across the foundation because of the way it anchors the frame. Similarly, if we want to model some system we require not only different kinds of information about it but also principles that can provide the right kind of central frame. The boundary conditions for some fluid system might not have much to do with the material parameters that describe its viscosity, but they both interact with the Navier-Stokes equations in the right way to allow us to model the fluid's behaviour. I argue that it is the fact that they provide this central coordinating structure that distinguishes laws from the mere members of the supporting cast.

These three papers focus on slightly different aspects of scientific methodology surrounding laws and thus each shed a slightly different kind of light on the question of how claims about scientific practice might be brought to bear on metaphysical questions about laws. In the Epilogue, I tie some of these threads together and draw out what I take to be the lessons that emerge from these three papers. In particular, I think that the papers in this dissertation help to show two things. First, that there is a rich and interesting set of philosophical questions about the role that laws play in scientific question that we might ask and answer *independent of* any interest in questions about the metaphysics of laws. And second, that we may need to temper or adjust or reconsider the hope that the observation of scientific practice will furnish us with simple criteria for choosing between metaphysical theories of laws.

2.0 Laws of Nature and their Supporting Casts

2.1 Introduction

When I was a kid, I enjoyed watching some old Disney cartoons that featured Donald Duck and his nephews Huey, Dewey, and Louie. In a few episodes, the somewhat incompetent Donald Duck finds himself serving in the post of Sheriff of Bullet Valley.[1] Although he warns his nephews against joining him on this dangerous adventure, the incorrigible Huey, Dewey, and Louie sneak along nonetheless, intent on helping their bumbling uncle with his various challenges. Donald Duck remains blissfully unaware of the secret machinations of his nephews, despite the fact that they are by and large responsible for the success he encounters in performing his various sheriff-related duties. Although Donald believes he has single-handedly succeeded in restoring law and order to Bullet Valley and its surrounds, the viewer realises that his tenure as Sheriff of Bullet Valley would have proved disastrous without his cabal of helpful nephews.

It strikes me as an underappreciated fact that in the course of scientific inquiry many laws behave much like Donald Duck. On first glance, it might seem that such scientific laws are capable of performing a variety of tasks all on their own (for example, providing us with accurate descriptions of the evolution of various systems through time). Little do we realise, the law's ability to perform such a task owes much to its own supporting cast of helpers. In the case of laws, it is things like boundary conditions, material parameters, interfacial stipulations, rigidity constraints, and so on that serve as the analogues of the helpful Huey, Dewey, and Louie.

In this paper, I will argue that this fact about the way that laws feature in scientific practice spells trouble for some recent formulations of the Best System Account (BSA) of laws of nature. Where David Lewis originally insisted that the laws of nature are the true generalisations that feature in the deductive system that best balances strength and sim-

[1] I should thank Mark Wilson for reminding me of these old cartoons.

plicity with respect to an underlying Humean mosaic,[2] several philosophers have recently suggested that these criteria (of simplicity and strength) should be replaced with (or augmented by) others that are more sensitive to the role that laws play in scientific practice.[3] Although the criteria that these philosophers put forward differ in a variety of ways, they are primarily concerned with the ability of laws to furnish us with predictions and encode information. This, I suggest, is a problem. If it is true that many scientific laws do not *on their own* perform some of the roles with which they are traditionally associated, then they are unlikely in isolation to make meaningful contributions to the predictive strength of a system or encode information about particular systems. Such laws are thus unlikely to end up in the best system, and so these accounts will have trouble conferring lawhood upon them.

To be clear from the outset, I do not think that Humean views are *alone* in having difficulty accommodating the way in which laws rely on a variety of supporting cast members. Indeed, it strikes me that Humeans and non-Humeans alike in discussions of laws of nature *generally* overlook the fact that laws are typically only able to perform their familiar roles when embedded in the right kind of modelling environment. To this end, it seems to me that the problems I raise for various Humean accounts of laws in this paper are illustrative of a more *general* oversight in the literature on laws of nature. That being said, there are two good reasons to focus here on Humean accounts. The first is the fact that non-Humean accounts differ more widely from one another in character and structure than do Humean accounts, and so determining exactly *how* and *to what extent* this oversight affects such accounts is a delicate task. The second is that pragmatic Humeans tend to be more explicit than most about exactly how their account relates to the *role* played by laws in scientific practice, and so the problems that arise from considering the role played by constructions like boundary conditions can be seen most clearly in the context of these accounts. For these reasons, this paper will mainly focus on articulating the problems that this supporting cast dynamic raises for pragmatic Humean accounts. Once the shape of this problem becomes clearer, however, we will be in a position to consider how questions about the role and status

[2]See Lewis (1973, 1983, 1986, 1994).
[3]In particular, Dorst (2019b), Hicks (2017), Jaag and Loew (2020), and Wilhelm (2022).

of boundary conditions might impact our accounts of laws more broadly.

This paper, then, will proceed as follows. In §1, I briefly outline the traditional formulation of the BSA as well as the more pragmatically-inflected alternatives that have been proposed recently. In §2, I focus on a particular kind of construction on which scientific laws regularly rely: boundary conditions. Although in the philosophical literature it is common to see the term 'boundary conditions' employed as though it were more or less synonymous with 'initial conditions,' applied mathematicians and physicists often mean something far more substantial when they talk about boundary conditions.[4] §3 illustrates the essential role played by these more involved boundary conditions in allowing some scientific laws to perform the tasks traditionally associated with them by way of a case-study: the Navier-Stokes Equations. In §4, I outline the general problem that laws such as the Navier-Stokes equations present to the BSA with reference to Lewis's account. In particular, I argue that it is difficult to see how the BSA can render the verdict that the Navier-Stokes equations are, indeed, a law. In §5, I examine how this problem arises for the different attempts to reform the BSA along pragmatic lines by examining the details of the various proposals. In §6, I consider some of the differences between the kinds of boundary conditions required by the Navier-Stokes equations and explain why it is that the Humean cannot avoid the problem raised by simply accepting the verdict that the various boundary conditions turn out to be laws. Finally, in §7, I conclude by suggesting that addressing the problem outlined in this paper may require more radical reform to the BSA than simply providing new criteria for picking out the best system.

[4]It is worth mentioning here that boundary conditions are not the *only* example of the kind of dynamic between laws and supporting contructions that I am highlighting in this paper. For instance, *material parameters* (such as conductivity and viscosity) are constructions that allow us to capture the complex scale-dependent behaviours of some system (often a particular material) such that we may actually apply the relevant continuum-scale laws to that system. They do not simply report the initial values of variables that feature in certain continuum-scale equations. For more details on such material parameters, see Batterman (2013, 2021) and Batterman and Green (2020).

2.2 The BSA and its Pragmatic Variants

David Lewis originally formulated his Best System Account in terms of the 'Humean mosaic,' which is simply supposed to be the totality of all the particular matters of fact about the universe. The idea is that we might consider various axiomatised deductive systems as attempts to systematise as many of these particular matters of fact that make up the Humean mosaic as we can. Different systematisations may exhibit different virtues to different degrees. Some may be quite simple, perhaps in the sense that they contain relatively few axioms. Others may be quite strong, in that we can deduce many consequences from the axioms, or in that the axioms rule out many different possible worlds. In reality, Lewis suggests, we should want any systematisation of the mosaic to *balance* these competing virtues. As such, his account holds that a true generalisation is a law of nature if and only if it features as an axiom or theorem of the system that *best balances* the virtues of simplicity and strength. If there turn out to be several such systems, then the laws will be the true generalisations that feature in *all* of the best systems.

Dorst (2019b) helpfully points out that we might distinguish two components here. The first is the thought that the laws are those statements that feature in our best systematisation of something like a Humean mosaic. The second is an actual specification of what makes for the best system (and thus the laws). The idea here is that in evaluating the merits of various candidate systematisations of the Humean mosaic, we attempt to balance certain principles. Dorst calls this second component the 'nomic formula.' Thus Lewis's original nomic formula involves finding the best balance between strength and simplicity.

In recent years, a series of philosophers have suggested that these two components of Lewis's BSA can and should be separated from one another. According to such proposals, we ought to retain a broad commitment to the idea that the laws are those generalisations that feature in the best sytem while replacing Lewis's nomic formula with one that better reflects the role played by laws in scientific practice.[5] Thus Dorst (2019b) suggests that the

[5]That is not necessarily to say that the nomic formula proposed by Lewis has *nothing* to do with the epistemic practice of science. Indeed, he suggests that the system that best balances strength and simplicity "has the virtues we aspire to in our own theory building" (Lewis (1983, p. 41)). Nonetheless, Lewis restricts himself to a more schematic and abstract characterisation of the epistemic practice of science than would appeal to recent pragmatically-inclined Humeans.

best system is the one with the highest predictive utility, Jaag and Loew (2020) argue that the best system encodes information in a way that is most cognitively useful for creatures like us, Hicks (2017) focusses on the fact that laws must facilitate predictions and explanations and be inferred from repeated experiment, and Wilhelm (2022) adds computational tractability to the list of principles that should appear in our nomic formula.

The general thrust of these recent attempts to reform the BSA is the thought that in developing an account of laws of nature we should pay more attention to the pragmatic role that laws play in scientific practice. As such, many of these recent proposals for alternatives to Lewis's BSA begin by asking a question like: what role do laws *actually play* in scientific practice? Once we have determined the salient role or roles, the thought is that we can adjust our nomic formula to ensure that whatever it is that our account declares the laws to be is capable of playing the role that laws play in scientific practice.

Althought these more pragmatically-inflected versions of the BSA strike me as clear improvements on Lewis's original formulation, the tale of Scrooge McDuck with which we began might indicate that there is a problem lurking here. If it is true that the tasks typically assigned to be performed by laws in scientific practice are actually performed by laws along with a substantial supporting cast, how much of an improvement can we make on the BSA by focussing on 'the role that laws play in scientific practice'? Answering this question will be the focus of the rest of this paper. In the meantime, however, it will be important to meet at least one member of the supporting cast and to see exactly how certain laws rely on them to furnish us with predictions, descriptions, and so on.

2.3 Boundary Conditions

Amongst the various kinds of constructions and modelling ingredients that feature in the supporting casts upon which many laws rely, *boundary conditions* stand out as particularly important and ubiquitous in scientific practice. As a result, it will be helpful to examine in at least some detail the way in which boundary conditions support scientific laws in their traditional tasks. Although boundary conditions are often associated with initial conditions, they play a distinct role in scientific practice and it will be especially important to understand exactly how the two differ from one another.

Indeed, philosophers in the literature on laws of nature (and, indeed, beyond) tend to assume that 'boundary conditions' are more or less *the same* as 'initial conditions.' The following passage from Bhogal and Perry (forthcoming, p. 17) illustrates this tendency:

> However, the best system is not a *purely* nomic entity. It contains non-nomic boundary conditions as well as laws. The best system is, roughly, the deductive closure of statements which best systematize the facts about the mosaic, balancing simplicity and informativeness. Nothing about that systematization requires that it only include *laws*; it may include contingent things, like the precise boundary conditions. In fact, such intuitively contingent boundary conditions seem like they will be required for the system to be informative. A system where the axioms are only the laws of Newtonian mechanics, for example, would not be particularly informative on its own – it needs the addition of boundary conditions specifying what objects there are, their mass, their velocity, and so on.

As another example, Hicks (2017, p. 1002) writes that the orthodox BSA "cannot differentiate laws from boundary conditions" before explaining how by contrast his own account delivers a "distinction between initial conditions and laws." That is, the task of differentiating laws from boundary conditions is seen as the same as that of differentiating laws from initial conditions. Jaag and Loew (2020, p. 2542) consider the question of why scientists distinguish laws proper from "mere boundary conditions," by which they mean information about the coordinates, masses and charges of various particles. By and large, one sees the term 'boundary condition' used either as though it were synonymous with 'initial condition' or as though boundary conditions were a particular *kind* of initial condition.

In reality, initial conditions and boundary conditions are two very different kinds of things. Granted, there may be some specific fields, such as point particle mechanics, within which boundary conditions tend to look very much like initial conditions. However by and large what physicists and applied mathematicians mean by 'boundary condition' is something above and beyond merely fixing the value of some parameter or parameters at some specified time. Boundary conditions in this more substantial sense are constraints on the values that a differential equation must take on the boundary region of the solution space of the relevant problem. They typically arise in the contexts of *boundary value problems* in which a core differential equation must be augmented by additional constraints before it admits of a unique or appropriate solution. These constraints, moreover, must typically apply *at all times* t and not merely at some specified initial or specific time. Indeed, they are often differential equations themselves.

Bursten (2021) helpfully distinguishes between the 'variable fixing' and 'structure specifying' role that such modelling ingredients can play. When philosophers, such as in the passages above, write about 'boundary conditions,' they are typically referring to something like what Hempel (1942, p. 36) calls 'determining conditions.' These determining conditions are statements that provide the information about the specifics of an event required for some universal hypothesis to properly apply to it. We might know, for instance, that some differential equation captures the way in which certain classes of populations grow, but before we can use that equation to model some *particular* population we need to know the initial population size, relevant growth rate, and so on. These kinds of conditions, then, are contingent facts that specify the value of certain parameters or variable that appear in the general equations for some kind of system.

Distinct from this variable fixing task, however, is the task of specifying the mathematical structure of the boundary of the space on which some differential equation is defined. Performing this *structure-specifying* task requires more than simply plugging in the right kind of values for the parameters of the system at hand (such as "the initial population consisted of n individuals," or specifying masses and velocities of particles, and so on). In many cases, solving the differential equation requires that we impose constraints on how the function that will emerge as a solution to the differential equation can evolve over time in the region of the boundary. For example, we might need to specify how the normal vector or derivative of some velocity field changes over time in certain directions. We will see how this works in more detail shortly, but for now it is simply important to note that without such boundary conditions our original equations often may not possess a 'solution' in any cogent sense.

The general point here is that laws of nature in their differential equation form rely on boundary conditions in a far more substantial fashion than is typically recognised.[6] The

[6]There are, of course, exceptions. In addition to Bursten (2021) and Sykora (2019), Mark Wilson (2006, 2017) has repeatedly emphasised the way that conceptual and mathematical differences between boundary conditions and initial conditions are often overlooked. In a similar vein, Wolf and Read (2023) note that

ability of some scientific laws to perform their central descriptive and predictive tasks depends on modelling ingredients, such as boundary conditions, which involve more than simply (as Bhogal and Perry write), "specifying what objects there are, their masses, their velocity, and so on." Before we consider whether the BSA is able to handle this fact about the way that laws operate in scientific practice, it will help to see exactly how it is that such boundary conditions play this more expansive structure-specifying role. To this end, we shall in the next section meet the Navier-Stokes equations and the boundary conditions with which they are typically augmented.

2.4 The Navier-Stokes Equations and Slip Conditions

The *Navier-Stokes equations*[7] are employed in a wide variety of scientific contexts involving fluid flow, such as ocean currents, weather patterns, the motion of water in pipes, blood flow, air flow over the wing of a plane, and so on. They have for quite some time been considered the correct formulation of the laws governing fluid motion. As Hermann von Helmholtz (1873) wrote:

> "As far as I can see, there is today no reason not to regard the hydrodynamic equations [of Navier and Stokes] as the exact expressions of the laws that rule the motion of real fluids."

In particular, the Navier-Stokes equations improved on the previously known Euler equations by correctly formulating the influence of fluid viscosity on fluid motion.[8]

Yet the Navier-Stokes equations on their own do not tell us how individual systems featuring fluid motion will behave. For that, they must be augmented with a variety of boundary conditions, some quite general and others more specific. Most prominently, we require a *slip condition*, which specifies the tangential component of the velocity of a fluid

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boundary conditions play an important structural role in our attempts to evaluate claims of empirical equivalence between dynamical theories.

[7] One will occasionally see historical references to the singular *Navier-Stokes equation*, but modern terminology has settled on referring to the equations in the plural. Since they are vector equations they can, if necessary, be written as a series of equations in each of the component spatial directions.

[8] For more detail on the equations, what they look like, and what the various terms in them mean, see Moffatt (2015) and Batchelor (2000).

at the surface of flow along the stationary boundary. For instance, how does the contact between the walls of a pipe and flowing water impact the velocity of the fluid along the walls? Without such additional constraints, we are typically unable to solve the equations or find ourselves provided with incorrect values, depending on the system.

Typically, though not always, we must augment the Navier-Stokes equations with a *no-slip* condition, which sets this tangential component of the velocity to zero. In physical terms, this captures the fact that at the fluid-solid interface, the force of attraction between the fluid and solid particles is greater than that between the fluid particles themselves, owing to the fact that the effect of viscosity predominates at the boundary (see Rapp (2017, pp. 244–245) and Schobeiri (2010, p. 234)). This specification of boundary structure allows the equations to apply in some concrete fashion to real systems. (Something like the no-slip condition is required to explain why dust accumulates on a stationary ceiling fan, for instance.)

It is important to note again that a boundary condition such as the no-slip condition is not simply a mere contingent fact that we plug into the equation expressing the relevant law. Indeed, Sykora (2019) has shown that the no-slip condition in particular is invariant under certain classes of interventions and enjoys quite broad empirical and theoretical support. The no-slip condition does not tell us what the velocity of any particular fluid particle is at any particular time, but rather provides a constraint on the way the velocity of the fluid particles in the boundary region must evolve for all times t. It is this added structure that ensures that the task of solving the Navier-Stokes equations amounts to what Jacques Hadamard (1923) famously termed a "well-posed problem." This simply means that the model admits of a unique solution that changes continuously with the initial conditions. Without the inclusion of some kind of slip condition, we would be unable to find a unique solution (or sometimes any solution at all) to the Navier-Stokes equations for fluid systems.

In specific cases, other boundary conditions may be required. For instance, if we are interested in the way that fluid behaves after being poured out of a pipe, we require so-called *inlet/outlet conditions* before the Navier-Stokes equations can be properly applied to our system. The form of these conditions depends on the kind of inlet or outlet we have, though often something along the lines of $\frac{\partial u}{\partial t} + \bar{u}\frac{\partial u}{\partial x} = 0$ is required, where $\bar{u}$ denotes an averaged value of the velocity in a particular area. If our fluid flows along a surface that

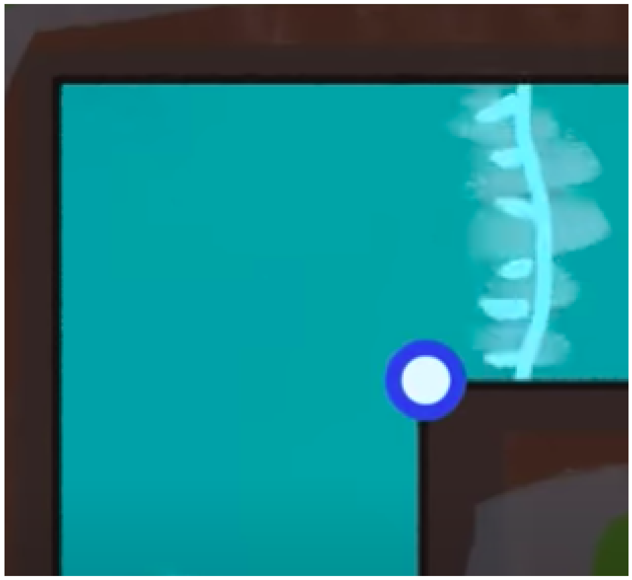

Figure 1: Without the appropriate boundary condition (in addition to the obligatory slip condition), the Navier-Stokes equations will predict that the velocity field diverges to infinity in the indicated inside corner.

forms a right angle, on the other hand, the Navier-Stokes equations (plus the appropriate slip condition, of course) will predict that the fluid velocity along the inside corner is infinite (see Figure 1). This is a result not of some defect in the equations but rather of a lack of certain pieces of information required by the geometry of the problem. To accommodate such systems, we must also augment the equations with a *Neumann boundary condition* which specifies the derivative of the velocity at that point of the boundary.[9]

In many circumstances we may be able to *apply* some set of equations very comfortably to some system without needing to *solve* them in their entirety; that is, without possessing an explicit *solution.* We may do this because it is extremely difficult to get our hands on

[9]More precisely, such additional boundary conditions serve as a kind of prerequisite for the numerical techniques we use to tame the singularities that the Navier-Stokes equations contain in cases involving sharp corners. Applied mathematicians and physicists commonly deal with singularities in the core differential equations of their model by way of a variety of numerical and 'semi-analytic' methods. In cases involving sharp corners, we must imposed further boundary conditions on our flow before we can employ such methods to extract information from the Navier-Stokes equations regarding our system. For examples of this approach, see Gupta, Manohar, and Noble (1981) and Deliceoğlu, Çelik, and Gürcan (2019).

explicit solutions, and so we might approximate a solution or treat the equations numerically or something along those lines. The sense in which the Navier-Stokes equations without the boundary conditions do not admit of a solution is different than this. Without the boundary conditions the application of the Navier-Stokes equations to a particular system will likely not amount to a *well-posed problem*. In such cases there is no sensible solution to be approximated or treated numerically in the first place. So when I say that certain boundary conditions are integral to our ability to solve the Navier-Stokes equations, I mean this in the sense that without the boundary conditions we do not even have a well-posed problem to solve, rather than the sense that the boundary conditions help us to apply the Navier-Stokes equations by allowing us to get our hands on *actual solutions*.

One final point is important here. In this context, whether the Navier-Stokes equations can be 'solved' in some case or another is not merely a matter of computational tractability. Even in more ideal cases the equations are often extremely intractable and must be handled using a complex toolkit of numerical methods and approximations developed by applied mathematicians. In the above cases, the Navier-Stokes equations themselves do not, without the appropriate boundary conditions, possess the right kind of structure to ensure that sensible solutions exist. This cannot be rectified simply by finding the correct information to add to the equations themselves. The Navier-Stokes equations *are* the correct laws for describing the motion of viscous fluid, and they can be verified as such by both empirical and theoretical considerations. The morale here is this: simply because some statement is a physical law of nature does not necessarily guarantee that it can be applied to any system at all without the addition of the boundary structure appropriate to that system.

2.5 Boundary Conditions and the BSA

Why, then, might the way in which laws like the Navier-Stokes equations rely on boundary conditions present a problem for the BSA? The rough idea is that unless the required boundary conditions are included in our candidate system, the Navier-Stokes equations are unlikely to make any meaningful contribution to the strength of our system. In such a case the Navier-Stokes equations would be unlikely to end up in the best system and thus unlikely to come out as laws. On the other hand if we *do* include the boundary conditions in our system, then we run the risk of conferring lawhood on the entire supporting cast. It will perhaps help to see how this problem plays out for Lewis's 'strength and simplicity' formulation of the BSA and then think about the more pragmatic formulations that have appeared in recent years.

Recall that according to Lewis, a generalisation is a law of nature if it appears as an axiom or theorem in the deductive system that best balances simplicity and strength with respect to the Humean mosaic. Yet as we have seen, the Navier-Stokes equations on their own are unlikely to make any contribution to the strength of some candidate system. Unless they are coupled with the appropriate boundary conditions, which may vary depending on the system at hand, there is very little that we will be able to *deduce* about any particular system (or very few possible worlds we can rule out) as a result of the Navier-Stokes equations. Indeed, as we saw, they may in fact provide *incorrect* results in such a case. Given that the inclusion of the Navier-Stokes equations in our system would result in at least a marginal decrease in simplicity with no real gain in strength, it would seem unlikely that the best system would include the Navier-Stokes equations on their own. In other words, if the boundary conditions are not included in our candidate systematisation, it seems unliklely that Lewis's BSA will declare the Navier-Stokes equations to be a law.

It might seem as though there is a simple solution here: we can simply add the boundary conditions to our system in order to ensure that the Navier-Stokes equations is in a position to contribute to its overall strength. However there are two problems with this move. The first is that there is an extraordinarily large (possibly infinite) number of boundary conditions that we would need to add to our system in order to accomplish this, appropriate to the various physical systems that we might encounter. This would seem to represent a pretty dramatic loss with respect to the simplicity of our system. Of course, much has been written about exactly how the trade-off between simplicity and strength is supposed to work in Lewis's BSA,[10] but it would seem that whichever way you slice it a system which includes both the Navier-Stokes equations and the full litany of boundary conditions they employ

[10]For instance, objections have been raised by Hall (2015), Roberts (2008), and Woodward (2014).

would likely be one of the least simple candidates on offer.[11]

Suppose, however, that we overcome this problem. That is, suppose that some system which includes the Navier-Stokes equations and the relevant boundary conditions turns out to be the one that best balances strength and simplicity. In that case, we have succeeded in conferring lawhood on the Navier-Stokes equations. Unfortunately, we may have gone too far. If any true generalisation that features in the best system comes out as a law of nature, then it would seem that our entire supporting cast of boundary conditions will turn out to be laws of nature. Granted, some of these, such as the no-slip condition, display a limited range of lawlike characteristics. Many others, however, such as the variety of inlet/outlet conditions, do not (these differences will be discussed further in §6). It would seeem to be a real problem for the BSA if the only way it could render the verdict that the Navier-Stokes equations were laws of nature was at the cost of declaring that *all* the boundary conditions also turn out to be laws of nature.

Of course Humeans, such as Bhogal and Perry as we saw earlier, tend to recognise that the best system will need to contain a variety of initial condition statements in order to ensure that the laws contained therein are informative. The mere fact that $f = ma$ features in our system, for instance, does not allow us to derive or deduce correct statements about the Humean mosaic unless we also include some information about "what objects there are, their mass, their velocity, and so on." Yet simply because such initial conditions feature in our best system does not on its own seem to mean that we run the risk of conferring lawhood on them. The laws, after all, are the *generalisations* that feature in the best system, and initial conditions statements about the masses and velocities of particular objects clearly do not seem to be generalisations.[12]

We might then ask: why are boundary conditions any different? The answer is that

[11] There is a related problem here worth mentioning. Given that different classes of systems will require different, incompatible boundary conditions, there is a risk that including all the requisite boundary conditions will render the system *inconsistent*. Perhaps the Humean might avoid this by suggesting that each boundary condition should be included in the system with a specification of the kinds of systems it is to be applied to and the kinds of laws it should combine with, but this seems once again to place a pretty heavy toll on the system's simplicity. Not only must our system include an enormous quantity of boundary conditions, but these boundary conditions are quite complicated specifications in and of themselves. It seems then even more implausible that such a system would count as simple enough to win the title of 'best system.' Thanks to an anonymous reviewer for suggesting this point.

[12] Some pragmatic Humeans, such as Dorst (2019b), talk in terms of *principles* rather than generalisations.

where initial conditions are particular, discrete pieces of information about the state of a system at a certain time, boundary conditions are differential equations themselves which impose ongoing restrictions on the evolution of our system. Unlike initial conditions, then, it does not seem that there is any clear reason not to regard these boundary conditions as *generalisations* in our system. The no slip condition, for instance, is a generalisation that relates the velocity of the fluid at the boundary to the shear rate at the boundary. As such, it seems that as long as such boundary conditions feature in the best system the defender of the BSA is committed to declaring that they are laws.

By talking about boundary conditions as *generalisations* here I do not mean to point to a mere *syntactic* difference. After all, if this distinction amounts simply to the difference between, for instance, statements that are universally quantified and those that are not, then it seems that we could simply stretch initial conditions into the right shape to count as a generalisation.[13] Such a distinction would not be able to robustly capture the difference between initial conditions and boundary conditions. When I say that boundary conditions (and laws) are generalisations where initial conditions are not, I mean that boundary conditions are general statements *about* the Humean mosaic and not the kind of thing we can think of as being *in* the mosaic.

There are, of course, a variety of ways that one might understand the facts that make up this mosaic, the predicates featured therein, and so on. Nonetheless, it seems right to say that whether or not we stretch them into the logical shape of a generalisation, statements roughly of the form "system $\mathcal{S}$ exhibits properly $\mathcal{P}$ at time t" are the kind of thing we should imagine as making up the mosaic. On the other hand, statements like "system $\mathcal{S}$ exhibits properly $\mathcal{P}$ *at all times* t" or "systems of class $\mathcal{C}$ exhibit properly $\mathcal{P}$ *at all times* t" seem clearly to be general statements *about* the mosaic rather than the kind of discrete fact we should expect to find *in* the mosaic. The suggestion then is that where initial conditions by and large are discrete facts that might feature *in* the mosaic, boundary conditions pose a special kind of problem because they are of the latter kind of more general statement *about* the mosaic.

That, at least, is the shape of the problem. There are two reasons, however, that it might

[13]Thanks to an anonymous reviewer for pushing me on this point.

not help to dwell on the implications of the role played by boundary conditions for Lewis's BSA in particular. The first is that there seems to be relatively broad consensus amongst Humeans that, for a variety of reasons, Lewis's particular nomic formula stands in need of revision.[14] In particular, many philosophers have argued on grounds totally unrelated to those that concern us here that Lewis's BSA does not sufficiently reflect facts about scientific practice.

The second reason is that Lewis insists on a far sharper distinction between fundamental and non-fundamental laws than do some of the BSA's recent reformers. On his view, laws must only make reference to an elite class of 'perfectly natural' properties, and so it is less clear that his account is intended to capture all of the laws of fluid dynamics at all. That is to say that since the Navier-Stokes equations make reference to macroscale material properties such as viscosity which are not on his view 'perfectly natural,' Lewis may have rejected them as not sufficiently fundamental and thus beyond the scope of his account.[15]

More recently, Humeans of various stripes have attempted to dispense with this aspect of Lewis's view.[16] In the absence of some strict naturalness constraint, however, it might seem difficult to insist on a sharp distinction between the 'fundamental laws' one's account is intended to cover and the 'non-fundamental' laws it is not.[17] The main point here is that where Lewis's machinery of perfectly natural properties might allow him to dismiss the Navier-Stokes equations as beyond the intended domain of adequacy of his account, this move does not quite seem available to the more pragmatically-inclined Humeans that are

[14]Of course, pragmatically-inclined Humeans such as Hicks (2017), Dorst (2019b), Wilhelm (2022), and Jaag and Loew (2020) represent a big part of this consensus. But in addition, more orthodox Humeans such as Loewer (2007, 2020) and Cohen and Callender (2009) have suggested a variety of modifications to Lewis's nomic formula.

[15]It also seems to me that some distinction between fundamental and non-fundamental laws is likely to play a role in how some non-Humeans would respond to the problems raised by the relationship boundary conditions and laws. For instance, if one is a primitivist (such as Maudlin (2007)) and thus thinks that laws are primitive entities who perform the role of carrying the universe from prior states to subsequent states, then it would seem to be a real problem if the laws must rely on boundary conditions to accomplish this task. Presumably, then, such a primitivist would want to deny that laws like the Navier-Stokes equations are fundamental in some relevant sense. A similar line of thought would seem to apply to those who, like Emery (forthcoming), think that laws play some kind of metaphysical *governing role*. As I mentioned in §1, however, assessing the way in which the relationship between laws and boundary conditions impacts the viability of non-Humean views is beyond the scope of this paper.

[16]Most notably Cohen and Callender (2009) and Loewer (2007, 2020).

[17]Jaag and Loew (2020, 2526fn3), for instance, simply distinguish the "fundamental laws of physics" from the "so-called laws of the special sciences."

wary of positing such properties.[18]

2.6 Alternative Nomic Formulas

Now that we have seen at least the broad shape of the problem posed by boundary conditions for the BSA, we can look in detail at how this might apply to some recent reformulations of Lewis's nomic formula.

2.6.1 Computational Tractability

Wilhelm (2022) argues that along with strength and simplicity we should consider *computational tractability* to be one of the theoretical virtues our best system should balance. A system X is more computationally tractable than another Y if X is "overall, more computationally useful than Y when it comes to performing numerical integrations, estimating infinite series expansions, constructing idealized models of phenomena, approximating exact solutions to equations of motion, and so on" (Wilhelm (2022, p. 3)). The idea is that laws of nature ought in practice to do more than simply rule out a large array of possible worlds. A system that is computationally tractable as well as strong not only rules out plenty of possibilities but also "gives us the tools to determine which worlds are eliminated" (Wilhelm (2022, p. 5)).

Does adding computational tractability to the list of theoretical virtues help with the problem of conferring lawhood on the Navier-Stokes equations? It does not seem to me that it does. Recall that the Navier-Stokes equations are computationally intractable not simply

[18]This point is borne out, I would suggest, by some of examples that these more pragmatic Humeans appeal to. Jaag and Loew (2020, p. 2530) mention the Wiedemann–Franz law which deals with macroscale thermodynamic properties such as thermal conductivity. Dorst (2019b, p. 887) considers the ideal gas law, which again relates a variety of macroscopic properties of ideal gasses. Hicks (2017) discusses at various points different theories of planetary motion. It does not seem to me that any of these laws have a strong claim to be 'more fundamental' than the Navier-Stokes equations in such a way that would relieve these pragmatic Humeans from the burden of accounting for the details presented earlier. Indeed, given that we do not currently possess a truly fundamental physical theory, it would be quite philosophically awkward to appeal to a substantive characterisation of the role played by laws in contemporary scientific practice to motivate an account of laws that was only intended to capture some restricted subset of 'fundamental' laws.

in the sense that they are difficult or computationally expensive to solve (though they are) but rather in that without the boundary conditions they do not present us with a well-posed problem to solve in the first place. Put differently, a system that contains the Navier-Stokes equations (but not the relevant boundary conditions) is no more useful when it comes to constructing idealised models of phenomena or approximating exact solutions to equations of motion than the same system with the Navier-Stokes equations removed. Such a system would then presumably be unlikely to be the one that strikes the best balance between the relevant theoretical virtues and thus the Navier-Stokes equations would be unlikely to come out as laws.

If we attempt to remedy this by adding the boundary conditions as axioms to our system, then we run into the same problems as we saw in the previous section. First, any increase in computational tractability and/or strength will come at a significant cost to simplicity given the sheer number of boundary conditions we will require. Suppose that this can be overcome, we nonetheless risk conferring lawhood on the entire supporting cast of boundary conditions, since Wilhelm's account has it that any theorem of the best system comes out as a law of nature.[19] The underlying problem is that computational tractability is not a feature that laws in general exhibit in isolation. Many laws on an abstract level capture how certain systems behave but require the right kind of support before they can accomplish the tasks that Wilhelm collects together under the banner of computational tractability.

2.6.2 The Epistemic Role Account

Hicks (2017) argues that we should depart even more substantially from the criteria laid down by Lewis. The BSA, he suggests, focusses too much on the outputs of scientific inquiry and not enough on the inputs, such as experimentation. He rightly points out that the methodology of science is concerned with more than simply the organisation and unification

[19]Given that Wilhelm's BSA holds that the laws are all of the *theorems* of the best system and not necessarily only the *generalisations*, one might wonder whether this means he faces some problem of unintentionally conferring lawhood on all the initial conditions as well. However Wilhelm does not include initial conditions *themselves* in any of his various candidate systems, instead considering what one could derive from his various candidate systems when they are 'supplemented' with sentences about initial conditions. Such an approach would seem strange in the case of boundary conditions, since they are not particular, discrete sentences about the state of some part of the mosaic at some time but generalisations of a kind with those that feature in the deductive systems under consideration.

of as many truths as possible. In particular, science "aims both at discovering truths that can be employed in a wide range of situations much smaller than the universe as a whole, and at marshalling empirical evidence to provide epistemic support for believing those truth" (Hicks (2017, p. 993)).

With this in mind, Hicks presents the *epistemic role account* (ERA), according to which

> "The laws of nature are those true statements that, as a group, are best suited to produce predictions and explanations and to be inferred from repeated observation and controlled experiments." (Hicks (2017, p. 995))

The 'output role' that the ERA identifies for laws is similar to the one that features in the BSA in that "science should output a set of generalizations that will enable us to easily deduce predictions and provide explanations" (Hicks (2017, p. 995)). Where the ERA differes from the orthodox BSA is in the importance it places on the 'input role' of laws, in that they must be the kind of thing we can infer from observation and experimentation. Hicks thus adds two extra requirements: the laws must be able to be observed in isolated subsystems of the universe, and the laws must be observable in isolation.[20]

In the next section, we will consider how the focus on the predictive role played by laws fares in light of the boundary conditions-related difficulties we have discussed so far. In the meantime, it is worth thinking about Hicks's requirement that the laws of nature must be observable in isolation. In 1828, Antoine Cournot wrote of Claude-Louis Navier's (correct) formulation of what would come to be known as the Navier-Stokes equations that

> "M. Navier himself only gives his starting principle as a hypothesis that can be verified solely by experiment. If, however, the ordinary formulas of hydrodynamics resist analysis so strongly, what should we expect from new, far more complicated formulas?"

In essence, Cournot was complaining about the fact that it was at the time extremely difficult to subject the Navier-Stokes equations to empirical testing. The viability of some of the premises employed in Navier's derivation was difficult to ascertain, since it was unclear how the resulting equations could be applied to even simple systems (Darrigol (2005, pp. 116–8)). Indeed Navier himself, although quite confident in the theoretical underpinnings of

[20] I take it that by 'observing a law' in this context Hicks means observing *particular instances* of the generalisation captured by the law.

his equation, nonetheless conceeded that the formula "cannot suit the ordinary cases of application" (Darrigol (2005, p. 115)).

So what changed between this point and 1873 such that Helmholtz could as we saw earlier triumphantly declare that the Navier-Stokes equations were "the exact expressions of the laws that rule the motion of real fluids"? The answer is that physicists succeeded in determining the correct boundary conditions for several systems of central importance. As Darrigol (2005, p. 144) writes, the key reason that "as late as the 1860s the Navier-Stokes equation did not yet belong to the physicist's standard toolbox" was that a consensus had yet to emerge with regard to "the boundary condition, which is crucial in judging consequences for fluid resistance and flow retardation."

Indeed it was (unsurprisingly) George Stokes who realised that considerations of boundary conditions were key to the applicability of the Navier-Stokes equations to real fluid systems. In 1850 he employed the no-slip condition, which we met in §3, in order to extract from Navier's equation an array of correct predictions regarding the motion of fluid through a cylindrical pipe (Darrigol (2005, pp. 142–3)). Despite the fact that a variety of molecular and non-molecular derivations of the Navier-Stokes equations had already been given, it was not until the work of Stokes that physicists were able to subject them to thorough empirical testing. Once the correct boundary conditions were found for certain central cases, physicists were able to understand more generally how to determine the boundary conditions appropriate to a wider class of systems.[21] It was exactly *this* development that inspired Helmholtz's optimistic declaration of 1873.

The morale of this historical interlude is that the reliance of some laws on the appropriate supporting cast can run all the way to questions of confirmation and testing. Some laws cannot be properly subject to experimental testing until the right boundary conditons (or material parameters, or rigidity constraints, and so on) are produced. Although Hicks (2017, p. 1000) is right to point out that scientific investigation is characterised by a "divide and conquer methodology of evidence gathering," it is too much to demand that "each part of the lawbook must be independently tested." Our ability to subject certain laws to empirical

[21]Indeed, developing techniques for producing boundary conditions for the Navier-Stokes equations and understanding their behaviour remains a very active area of modern mathematical research. See, for instance, Kučera and Skalák (1998), Nordström and Svärd (2006), and Raymond (2007).

testing is contingent upon our ability to formulate the correct boundary conditions.

Hicks articulates the thought that we must be able to subject laws to isolated experimental testing in terms of the virtue of *modularity.* Roughly, a set of laws L is more *modular* than a set of laws L^* if various subsets of the laws in L apply to more subsystems of the universe than do the subsets of the laws in L^*. We can put the problem, then, as follows. Let L be some set of laws and L^* be the set of laws that we get by adding the Navier-Stokes equations to L. Then since L^* does not contain the boundary conditions, the addition of the Navier-Stokes equations will not allow L^* to apply to more subsystems than our original L. That is, the Navier-Stokes equations in isolation do not contribute to the modularity of our set of laws – L^* is just as modular as L. Of course, we could always *include* the boundary conditions, but in such a case we would run into the by now familiar problem of conferring lawhood on all the relevant supporting ingredients.

2.6.3 Prediction

One common thread that runs through several of the proposed alternative BSAs is the idea that *prediction* is one of the most important roles that laws play in scientific practice. As such, several of the alternative nomic formulas that feature in these BSAs hold that something like predictive utility is the key to determining the best systematisation of the Humean mosaic. Jaag and Loew (2020, p. 2534) propose that the best system is the one that is maximally cognitively useful to creatures like us, but insist that "the main cognitive function of the laws is facilitating predictions." Cognitive usefulness, then, is simply something like predictive utility. Dorst (2019b, p. 886) suggests that "the primary pragmatic use of laws is predictive" and so his BSA centres around several desiderata such that "the system with the 'best balance' is the one with the highest predictive utility." Hicks (2017) also intends for his nomic formula to ensure that the best system is one suited to the predictive needs of agents operating in the world.

Of course, it is undoubtedly right to say that prediction lies at the heart of the overall role that laws of nature play in scientific investigation. Moreover, in articulating alternative nomic formulas framed around this predictive role, the pragmatic reformers of the BSA have shed light on the kinds of features that might allow laws to play this role. The question, however, is whether we can expect laws to play this predictive role in isolation, without the help of their supporting cast. If, as I have urged, we cannot in general maintain such an expectation, then we must ask whether these proposals too are faced with a problem similar to those we have seen in previous sections. Does a nomic formula centred on prediction allow us to confer lawhood on the Navier-Stokes equations without also conferring lawhood on the collection of sundry boundary conditions on which they rely?

Unfortunately, I do not think so. Suppose that you have some fluid system, the various parameters of which you are able to measure to an arbitrary degree of accuracy. Now take the Navier-Stokes equations and plug in the values produced by your measurements. Do the resulting equations tell you what the velocity field will look like a minute or two from now? No. The resulting equations will not have a solution unless you provide the right kind of boundary constraints (and perhaps also inlet/outlet conditions, depending on your system). In the terminology we introduced earlier, the problem will not be well-posed and so we will not even be able to *approximate* our way to a reliable solution. This is exactly the problem we saw in the historical interlude of the previous section: it was so difficult to subject the Navier-Stokes equations on their own to empirical testing because without a procedure for generating the appropriate boundary conditions one is not in a position to say what it is they predict of any particular system.

In articulating his Best Predictive System Account, Dorst (2019b) outlines several desiderata, the best balance of which should ensure the highest predictive utility. One way of putting the point above is to say that in isolation the Navier-Stokes equations fail almost entirely to meet the first two (and arguably most important) desiderata: informative dynamics and wide applicability. Dorst requires that "the actual putative laws of nature joinly imply a dynamics for various systems" Dorst (2019b, p. 887). But any set of principles featuring the Navier-Stokes equations and not the boundary conditions will imply no such dynamics for fluid systems. Similarly, Dorst also includes as a desideratum that our dynamical principles apply to a wide variety of systems so that we need not gather additional information about particular subclasses of systems we might meet in different circumstances. Of course, the Navier-Stokes equations tolerate a wide variety of initial conditions relevant to all the possible fluid systems we might encounter. Yet as we have seen, this does not guarantee that the Navier-Stokes equations may be *applied* to that same wide variety of systems in the absence of the more specific boundary conditions required by the problem at hand.

At any rate, it seems that our familiar problem rears its head again. Without the boundary conditions the system (or set of principles) containing the Navier-Stokes equations will be no more predictively useful than the system without them. Yet if we include the boundary conditions in our set of principles then we must thereby admit them into the pantheon of laws.

2.6.4 A Difference in Roles

One might at this stage worry that the pragmatic Humean can avail themselves of a rather simple reply. Even if boundary conditions play an important and indispensible supporting role for some scientific laws, they nonetheless are *not* the kind of thing playing the central pragmatic or epistemic role associated with laws. We might think that there is a stark difference between playing the appropriate 'law role' in science and assisting some *other* generalisation as it goes about playing that role. Although they rely on boundary conditions in all kinds of complex ways, it is the Navier-Stokes equations *themselves*, and not those boundary conditions, which are responsible for the predictions and explanations that scientists are able to produce. On this line of thinking, then, the pragmatic Humean can admit that constructions like the no-slip condition play an integral role in *supporting* the Navier-Stokes equations and even admit that they will need to be included in any eventual best system without being forced thereby to *confer lawhood* upon the boundary conditions.[22]

There is something very intuitive about this suggestion. Indeed, the thought that there is an important distinction to be drawn between the supporting cast and the laws that play the starring role is *precisely* the source of our intuition that accounts of laws should be expected to confer lawhood on the Navier-Stokes equations and *not* on things like the no-slip condition. The problem, however, is that this intuitive distinction is exactly the kind of thing that we would want an account of laws to explain in the first place, and so is not the

[22]Thanks to an anonymous reviewer for suggesting this reply.

kind of distinction to which an account of laws should appeal. Given a set of regularities, the job of an account of laws is in part to tell us which are the laws and which are not. If the criteria offered by the account label some things laws that we intuitively recognise play a slightly different role, then it is no response at all to say: those things are not laws on my account because they only play a supporting role to the *real laws*. After all, it is the job of the account in the first place to tell us *what the real laws are*. From the perspective of an account of laws, the many regularities that obtain in the world do not come, as it were, pre-labelled.

It is also worth registering that scientifically speaking the question of the different roles played by central laws and boundary conditions can be quite subtle. If one were to merely write down the Navier-Stokes equations and the relevant boundary conditions as a set of equations, it would not be right to say that one could somehow immediately discern that the Navier-Stokes equations are the real laws and the boundary conditions merely supporting actors. The intuitive distinction that we draw between the Navier-Stokes equations and their boundary conditions is rooted in relatively complex and subtle facts about the way that these respective components come to be *used*. But recall that for pragmatic Humeans (and proponents of the BSA in general) it is the *system as a whole*, and not individual regularities, that we evaluate according to some list of pragmatic criteria. We do not ask whether the Navier-Stokes equations play some lawlike role in scientific practice but whether the *system containing them* best fulfils some criteria inspired by the role that laws play in scientific practice. If the only way to get the Navier-Stokes equations into the best system is to include some regularities that seem otherwise to play a different *individual* role in scientific practice, then perhaps this an indication that the general framework of the BSA is too coarse-grained to capture important distinctions between the roles played by the different components of the set of equations we must use to make predictions about fluid systems.

In short, this kind of reply puts the philosophical cart before the horse. There is almost certainly an important distinction to be drawn between the individual roles played by the Navier-Stokes equations and their attendant boundary conditions, but this is the kind of thing that ought to *emerge from* an account of laws rather than be *appealed to* by an account of laws. Moreover, the fact that pragmatic Humean accounts confer lawhood on *all* of the regularities that make it into the best system makes it hard to see how we would be able to recover such a distinction by imposing further conditions at the level of the *system*. We will return to this point again in §7.

2.6.5 Predictive Contexts

In §4, we asked why boundary conditions pose a problem for the traditional BSA that is distinct from the one posed by initial conditions. The answer was that where initial conditions may be construed as pieces of information *in* the Humean mosaic, boundary conditions (like laws) are instead general statements *about* the mosaic. As such, where the proponent of the BSA could plausibly admit initial conditions into their best system without conferring lawhood on them, this would not work in the case of boundary conditions.

In the context of the pragmatic Humean reformulations of the BSA, we might consider a different form of the suggestion that we can handle boundary conditions in the same way we handle initial conditions. Rather than thinking about the informativeness of a system in terms of how much it tells us about the mosaic, as Lewis did, some pragmatic Humeans may conceive of informativeness as the extent to which a system allows us to input relatively small amounts of information about the mosaic and get back larger amounts of information about the mosaic.[23] We may then imagine that we already possess the information about the mosaic that is relevant to our given predictive context, and that the best system will be the one that allows us to get the most out of this information. On this conception, there is no need to include initial conditions in our system at all, and thus no risk that they may end up counting as laws against our wishes. Rather, initial conditions are pieces of information about the mosaic that we *input into* a system of generalisations (or *to which we apply* a system of generalisations), and the laws will be the members of that system which allows us to maximise some list of pragmatic criteria. The question then is: why can't we simply think of boundary conditions in the same way?

There are two answers worth outlining, here. The first is similar to the reply offered in

[23]This conception of the best system as in some sense 'amplifying' our knowledge of the mosaic is one that comes out most explicitly in Dorst (forthcoming) and Callender (2017). Thanks to an anonymous reviewer for pointing this out.

the case of the traditional BSA: boundary conditions are not the kind of thing we can easily construe as discrete pieces of information that we *input into* our set of laws. Distinguishing beteween the generalisations in our candidate system and the information we have on hand in some predictive context may make sense when we imagine that information to take the form "system $\mathcal{S}$ exhibits properly $\mathcal{P}$ at time t," but this distinction gets a bit murkier if the information we are 'inputting into' our system is of the more general form "system $\mathcal{S}$ exhibits properly $\mathcal{P}$ *at all times* t" or "systems of class $\mathcal{C}$ exhibit properly $\mathcal{P}$ *at all times* t." If we want to treat some statement S as the kind of thing that we merely 'plug into' some system of laws rather than needing to consider as part of the system itself, it would seem to me that S should be at least in the neighbourhood of a discrete piece of information about the Humean mosaic. Boundary conditions, I suggest, are not quite in this neighbourhood.

The second (and perhaps more interesting) reason that we cannot treat boundary conditions as we would initial conditions relates to the difference between the 'variable fixing' and 'structure-specifying' roles mentioned in §2. There is a difference between a statement specifying the *condition at the boundary* of some system in terms of the value of some variables and a *boundary condition* in a more involved mathematical sense. Whereas conditions at the boundary, like initial conditions, help us to specify the system to which we would like to apply our laws, boundary conditions help to provide the mathematical structure required to apply the laws at all. More specifically, they help to ensure that our attempts to apply certain laws to some system (or class of systems) amount to a *well-posed problem*. If you change the initial conditions (or conditions at the boundary), you change the system you are working with. If you change the *boundary conditions*, on the other hand, you change the nature of the predictive problem you are trying to solve. In this sense, boundary conditions, alongside laws, form part of the theoretical machinery that we use to turn particular bits of information into predictions, rather than simply being 'inputs' into that theoretical machinery. Unlike initial conditions, then, they should not be treated as information that we 'input into' some predictive system but rather as part of the predictive system itself.

2.7 Boundary Conditions as Laws?

The pragmatic Humean might at this stage wonder what is so bad about the possibility that their account delivers the result that the boundary conditions required by the Navier-Stokes equations are laws.[24] After all, I did mention that some of these boundary conditions, such as the no-slip condition, exhibit a limited range of lawlike characteristics. On the other hand, many of the boundary conditions on which the Navier-Stokes equations rely do not exhibit these characteristics. In particular, it will help to look at the differences between the no-slip condition and some of the inlet-outlet conditions required for certain systems.

It is worth noting to begin with that the no-slip condition applies to a relatively wide variety of systems, from fluid in pipes to air flowing around a ceiling fan. In these contexts, it serves as a generalisation that relates the velocity of the fluid at the boundary to the shear rate at the boundary. Moreover, such slip conditions remain invariant under quite a wide variety of interventions we might perform on our system.[25] For instance, if a slip condition is the appropriate one for a fluid-solid pair, then changing the size of the shape of the boundary in most ways will not affect the boundary conditions at all. Indeed, as long as the amount of slip is independent of the amount of shear, as it is in most cases, then physicists treat the amount of slip as a robust properly of a given fluid-solid pair (Lauga, Brenner, and Stone (2007, p. 1232)). That is to say that the slip condition for water flowing along a lead surface will apply whether the surface is a closed pipe, a container wall, an obstacle in a stream, and so on. The slip conditions for various fluid-solid pairs are thus invariant under considerable changes in boundary shape. Finally, as the discussion in §5.2 of Stoke's derivation shows, these slip conditions are not merely empirically measured but indeed enjoy a sort of theoretical support.

The above is not to suggest that the various slip conditions for the Navier-Stokes equations *certainly are* laws. Rather, it is simply to point out that *some* boundary conditions for the Navier-Stokes equations display some of the characteristics that we intuitively associate with laws: they have wide scope, they are invariant over a wide array of changes, and they

[24] Thanks to Erica Shumener, as well as an anonymous reviewer, for raising this point.

[25] For a more detailed overview of the experimental data, see Sykora (2019, pp. 15–21).

enjoy some form of theoretical support (i.e. they are not brute empirical generalisations).

Inlet-outlet conditions, on the other hand, lack these features entirely. If there is additional liquid flowing *into* (or out of) our system, then working with the Navier-Stokes equations requires that we characterise via boundary conditions how this inflow (or out flow) behaves. By contrast with slip conditions, such inlet conditions are often very specific. The right inlet condition depends in sensitive ways on the specific geometry of the physical system and so our inlet conditions often have a very limited scope.[26] For this reason, they do not display particularly notable amounts of invariance under manipulations: small changes to the shape of the boundary can radically impact the suitability of a given inlet condition. Finally, we do not often possess good 'theoretical' methods for determining these inlet-outlet conditions and in such cases must employ heavily computational empirical methods to produce them.

If the slip conditions were the only boundary conditions required by the Navier-Stokes equations, then the pragmatic Humean may simply want to bite the bullet and accept that on their account slip conditions will turn out to be laws. The fact that these conditions exhibit some of the characteristics that we intuitively associate with lawhood may make this an acceptable price to pay. However conferring lawhood on the entire set of boundary conditions involves conferring lawhood on the inlet conditions as well, even though they display almost no intuitively lawlike behaviour. Indeed, in spite of their formal structure these conditions seem far more like particular, contingent facts than the kind of thing that any scientist would recognise as a law.

Part of the difficulty here, as we saw in §5.4, is that the pragmatic criteria with which the BSA operates are applied at the level of the *system as a whole*. This means that pragmatic Humeans who are happy to confer lawhood on slip conditions but want to avoid conferring lawhood on inlet-outlet conditions must outline some criteria for picking out the best system on which the slip conditions appear in the best system but the inlet-outlet conditions do not. The problem is that despite the fact that they may exhibit very different degrees of intuitively lawlike behaviour, they are *equally integral* to the ability of our system as a whole

[26] Relatedly, we must often resort to highly computational methods appropriate to very specific circmunstances in order to determine these inlet-outlet conditions in the first place. For examples, see Galusinski et al. (2017) and Bruneau and Fabrie (1994).

to play the pragmatic role we want it to play. For instance, both kinds of conditions are vital to the question of whether the task of solving the Navier-Stokes equations amounts to a well-posed problem or not. Removing the inlet-outlet conditions from our system would have just as negative an impact on the pragmatic capacity of our system as would removing the more 'intuitively lawlike' slip conditions.

In short, it does not seem promising for the pragmatically-inclined Humean to respond to the problem we have posed by simply embracing one horn of the dilemma and accepting the verdict that the boundary conditions for the Navier-Stokes equations are laws. Although this might seem an acceptable price to pay in some cases, it is clearly too high a price to pay in others. Moreover, both the more intuitively lawlike and less intuitively lawlike boundary conditions are equally vital to the ability of any system containing the Navier-Stokes equations to perform certain pragmatic tasks. As such, it is difficult to see how the pragmatic Humean could outline criteria for picking out the best system that would ensure that things like the slip conditions found their way into the best system (and thus were counted as laws), while things like inlet-outlet conditions did not.

2.8 Conclusion: A Pragmatic Tension

So where does all of this leave these recent attempts to reform the BSA? By and large these alternative BSAs proceed by identifying features that laws of nature must possess in order to play the role that they do in scientific practice and then use these features to generate a new nomic formula while leaving in place the broader framework of the BSA. I have argued that there is a problem with this strategy, since many scientific laws require the assistance of (often quite complex) additional modelling ingredients before they are in a position to perform their central role in scientific inquiry. If this is right, then it is difficult to see how such alternative BSAs will be able to render the verdict that such laws are indeed laws. The Navier-Stokes equations, along with many others, will be left out in the cold.

Perhaps there are other strategies for generating nomic formulas that avoid this problem, but it does seem to me that the fact that laws do not always operate as lone wolves poses a broader challenge for the framework of the BSA. Recall that the BSA is primarily phrased in terms of the Humean mosaic, made up of particular matters of fact, and generalisations over that mosaic. Not every such generalisation is a law, however, and so the task becomes that of cleaving the laws proper from the accidental generalisations. In practice, however, scientists make constant use of modelling ingredients that occupy a somewhat messy continuum between full-blown laws and simple initial conditions-style matters of fact. The BSA faces the challenge of reconciling the fact that these ingredients do not seem to be (in most cases) appropriate candidates for lawhood with the fact that they play an integral role in scientific practice (and indeed in allowing laws to do the job that they do). It is not easy to see how a different set of criteria for picking out the best system, however motivated by an examination of the role laws play in scientific practice, will help us to handle the delicate interplay between scientific laws and their supporting casts.

Perhaps what the BSA needs here is some independent handle on the distinction between laws proper and their supporting casts. If the Humean were able to differentiate in some robust way between the generalisations eligible to be laws and those merely eligible to play supporting roles, then they could avoid the problems we encountered above by ensuring that their nomic formula applies only to the former kind of generalisation. Armed with a distinction between law-eligible and supporting-eligible generalisations, the BSA may then proceed along some of the pragmatic lines we have seeen in order to distinguish the accidental generalisations from the laws proper. On this line of thinking, even if we are forced to include some of the supporting cast members in our best system, they will not thereby turn out to be *laws* because we have in hand some independent distinction between the members of the best system eligible to be *laws* and those merely eligible to play supporting roles.

Maybe it is possible to draw such a distinction, but this would be no simple task. The slip conditions required by the Navier-Stokes equations are differential equations in their own right that apply at all times t to the velocity field describing the motion of fluid particles in the system. At the very least this seems to suggest that a mere syntactic criterion will not be enough to maintain such a distinction. Perhaps we can draw the distinction required by attending more closely to the roles played by laws proper *within* the broader modelling environments consisting of laws and their supporting casts, though I am not sure exactly how this might look. At the very least, drawing the distinction in such a way would require closer examination of the details of the role played by boundary conditions (and material parameters, and so on) in scientific practice than has been characteristic of the literature on laws of nature thus far.

There would also be something quite strange about this way of defending the BSA. In some sense the central claim of the BSA is that it is precisely the notion of membership in the best system that captures what it is to be a law and thus what it is to play a lawlike role in scientific practice. Perhaps it is true that the pragmatic Humean could respond to the difficulties surrounding boundary conditions by saying something like: although members of the supporting cast might find their way into the best system, there are finer distinctions between the role that various components of the system play in scientific practice to which we will need to attend in order to separate the laws from the non-laws. In some sense, this is probably right. But in another sense, we might ask: how much work is the notion of membership in the best system now doing in separating the laws from the non-laws? If we are denying lawhood to general statements about the Humean mosaic that find their way into the best system on the basis of more fine-grained considerations of the role played by different kinds of general statements in scientific practice, then why should we continue to work within the framework of the BSA? In such an event it would seem like it was these more fine-grained considerations that were doing the real work of separating the laws from the non-laws.

Of course, these issues must be worked through carefully, and doing so is beyond the scope of what I hope to achieve here. The point of this paper is to argue that the integral role played by supporting cast members such as boundary conditions in the scientific employment of laws presents a considerable obstacle to recent attempts to reform the BSA. The point of these concluding remarks is to tentatively suggest that getting around this obstacle might require more radical reform than simply switching out Lewis's old nomic formula for a more pragmatically-inspired one.

There is a way in which this, if true, is unsurprising. I think that Woodward (2014, p. 92) was right to say that

> "The appeal of the BSA does not, I believe, mainly derive from its demonstrated descriptive

adequacy as a treatment of detailed aspects of scientific practice involving laws. It rather has to do with its overall fit with other ideas to which many philosophers are committed: two of these are a picture of scientific reasoning as involving a trade-off between simplicity and strength, and a 'Humean' programme of reduction of the nomic to the non-nomic. [...] This makes many philosophers think that something along the lines of the overall package must be right and perhaps that they ought to pay less attention than they should to the details of exactly how the account is supposed to work."

More recent defenders of the BSA have done an admirable job in attempting to refine the BSA so that the kinds of generalisations that feature in the best systems picked out by their nomic formula more closely reflect the laws that feature in scientific practice. But then again, one might wonder, as Woodward does, whether the ability to capture the methodology of modern science in all its complex, gory detail was ever part of the BSA's core appeal. Driven by commendable naturalistic scruples to demand more of the BSA in terms of descriptive adequacy to the methodology of modern science, we may find that the framework begins to collapse (or at least creak unpleasantly) under a kind of pressure it was never intended to withstand. Adding a kind of pragmatic inflection to the nomic formula is one thing, but reckoning with the fact that the scientific use of laws involves a far wider array of constructions than simply laws and pieces of the Humean mosaic may turn out to be another thing entirely.

Perhaps my pessimism will turn out to be misplaced. Either way, if we want to amend the BSA so that it provides us with a more descriptively adequate picture of scientific methodology, we must do more than simply ask ourselves what role laws (on their own) play in scientific practice. We must ask ourselves *how* they go about playing that role and whether, in fact, they require any help in doing so. If, as I have argued, they do, then the question is: does the BSA have the resources to recognise the supporting cast without inadvertently giving them all a star billing? It seems to me that the viability of the program of pragmatic reform of the BSA depends on the answer to this question.

3.0 The Different Explanatory Roles of Laws

3.1 Introduction

Philosophers writing on laws of nature have long been convinced that laws play a central role in many of the explanations that emerge in the course of scientific inquiry. We use Newton's laws to explain why celestial bodies behave the way that they do, we use the Ideal Gas Law to explain why changing the pressure exerted on some sample of gas leads to a change in its temperature, and so on. Such observations have served as the starting point for a number of arguments from scientific practice that have come to occupy a central place in the literature on laws of nature. These arguments often insist that any self-respecting account of laws should be able to make sense of the central explanatory role that they play in scientific practice.

But what *is* "the central explanatory role that laws play in scientific practice"? A common suggestion is that laws, in scientific practice, *explain their instances*. It is not always clear what this suggestion amounts to, however. If it is simply to suggest that laws help us to explain the particular facts and occurrences that fall under the general patterns they describe, then the question remains: *how* do they go about doing this? Or rather: what *role* do they play in helping us to explain the particular in terms of the general?

There is another question lurking here: is there a *single* explanatory role that laws play in scientific practice? There seems to be a common presumption that there is. Talk of *the* explanatory role of laws is ubiquitous in the philosophical literature. Moreover, when philosophers disagree about the way that laws contribute to scientific explanations, they seem to nonetheless agree that the philosophical task is to correctly articulate the single role that laws, in general, play in scientific practice. Yet as a few philosophers have noted, the connection between laws and explanation can be quite amorphous and flexible.[1] We might

[1] Tim Maudlin (2007, p. 8), for instance, writes that "an amorphous connection is generally acknowledged to hold between laws and explanation," which he takes to motivate the claim that "a law ought to be capable of playing some role in explaining the phenomena that are governed by it or are manifestations of it." Additionally, David Armstrong (1983, p. 40) suggests that laws in general are used as "principles of explanation" and that any account of laws should be able to make sense of the fact that we often explain

then wonder: should we really think that there is a *single* explanatory role that *all* laws play in scientific practice?

I do not think that we should. In this chapter, I identify multiple *different* ways that laws may contribute to scientific explanations. Some of these may strike us as relatively familiar, and look something like laws "explaining their instances." Others, I will argue, do not look anything like this, except in a very fuzzy sense. The upshot is that although laws certainly play *some* kind of central role in the explanations that emerge from scientific practice, this may amount to quite different things on different occasions. Perhaps all laws "explain their instances" in the quite broad sense that they help us to explain particular facts in terms of general patterns, but there is not a *single* way that they uniformly go about doing this. Developing a more detailed picture of these different contributions that laws may make to scientific explanations allows philosophers to clarify what exactly they have in mind when they appeal to the fact that laws play an explanatory role in scientific practice in their discussions of various metaphysical accounts.

Here's the plan. I begin in §3.2 by outlining the *generative explanatory role* that laws might play, on which they help us to explain the behaviour of certain systems roughly by combining with certain kinds of specifying information to produce *descriptions* of that system's behaviour. This, I suggest, is one way of understanding the idea that "laws explain their instances." This *generative explanatory role* is most paradigmatically played by a class of scientific laws that I will call *general relational principles.*. In §3.3, I distinguish these general relational principles, from *constitutive laws*, and I argue in §3.4 that the latter are in general not capable of playing the kind of generative explanatory role played by the former. This is not all bad news, however, because as I suggest in §3.5, there is an important, yet distinct, *supporting explanatory role* that these constitutive laws play in scientific inquiry. In §3.6, I suggest that this diversity of explanatory roles pushes us towards a new perspective on philosophical discussions of *the* explanatory role that laws play in scientific practice. I conclude in §3.7 by reflecting on what all of this means for metaphysical accounts of laws more generally.

facts by "appealing to" laws of various kinds, but he does not insist on any particular uniform picture of how this must unfold.

3.2 A Generative Explanatory Role

The suggestion that laws play an explanatory role in scientific practice by explaining their instance is often parsed in a more specific way. That is, it is commonly suggested that laws explain their instances in the more specific sense that the explanatory role played by laws in scientific practice can generally be patterned on the relationship between schematic laws like *All Fs are Gs* and instances like *a is F and a is G*.[2] Even Hicks (2021), who rejects the claim that laws explain their instances in favour of the claim that laws play a meta-explanatory role in scientific practice, articulates this role in terms of such schemata, suggesting that the fact that it is a law that *All Fs are Gs* itself *explains why* 'a is F' *in turn explains why* 'a is G.'

We should take care when putting things in such abstract terms. The laws that appear in scientific practice do not often look much like these schemata and the process by which they are applied to real world systems does not often look much like universal instantiation. Very often this process involves the construction of quite complex models. At the very least, we should look closely at the way that laws come to be involved in scientific explanations, and ask whether, in general, such cases involve laws explaining their instances *in this more specific sense*. Doing so, I suggest, requires that we phrase our suggestion about the explanatory role of laws in terms more endemic to the scientific cases we are interested in examining.

A more general way of articulating the thought that laws explain their instances, then, might be to say that laws, in general, play a *generative role* in scientific explanation.[3] According to this picture, the explanatory role played by laws in scientific practice is intimately connected to their ability, once combined with more specific kinds of information, to generate *direct descriptions* of the behaviour of systems in which we are interested. We can use laws to explain how certain systems behave because when we provide them with the values of certain parameters relevant to that system, they provide us with a description that matches the behaviour we see (at the appropriate level of granularity, at least).

The role carved out for laws by the historically prominent Deductive-Nomological model

[2]As a brief sampling, see Dretske (1977), Earman (1986), Hicks and Elswyk (2015), Lange (2013), Loewer (2012), Marshall (2015), Roski (2018), Shumener (2019), and Ward (2007).

[3]Something like this terminology is also used by Ward (2007).

of explanation fits this more general characterisation, since the model takes laws to be universal generalisations that, when combined with "determining conditions" or "antecedent conditions," allow us to deduce the behaviour we want to explain.[4] In a slogan: $\forall x Fx \rightarrow Gx, Fa \vdash Ga$. But we can also think about this generative role in less schematic terms, as Dorst (2019a, p. 2659) does when he writes:

> "consider the Lorentz force law, which states that a charged particle traversing a magnetic field will experience a 'Lorentz force' perpendicular to its direction of motion. Now suppose that we observe an electron traversing a magnetic field, and as it does so, it curves off of its original trajectory, indicating that it is experiencing a force. Call this event 'e'. We can explain why e occurred by appealing to the Lorentz force law: the electron is a charged particle, and as it traverses the magnetic field, the Lorentz force law states that it will experience a Lorentz force. The event e is thus both an instance of, and explained by, the Lorentz force law."

As Dorst sees it, the reason that the Lorentz force law helps us to explain e is that it can be combined with particular statements about the charge of an electron in order to generate descriptions of a system which exhibits the behaviour relevant to e. In our terms, he suggests that the Lorentz force law plays a *generative role* in scientific explanations of e.

We might then tentatively characterise this *generative explanatory role* as follows

> THE GENERATIVE ROLE OF LAWS IN SCIENTIFIC EXPLANATION
> A set of laws $\mathcal{L}_1, \mathcal{L}_2 ..., \mathcal{L}_n$ can help us explain why system $\mathcal{S}$ exhibits behaviour $\mathcal{B}$ insofar as we can combine $\mathcal{L}_1, \mathcal{L}_2 ..., \mathcal{L}_n$ with particular statements $\mathcal{P}_1, \mathcal{P}_2, ..., \mathcal{P}_n$ in order to generate a description of $\mathcal{S}$ in which it exhibits $\mathcal{B}$.

We may then ask: do some kinds of scientific laws seem particularly well-placed to play this kind of generative explanatory role? There are a variety of intuitive distinctions one might draw regarding the different principles and generalisations that scientists refer to on different occasions as 'laws.' One especially important one for our purposes concerns the *parameters* in terms of which a law might be phrased. Some laws feature parameters that we take ourselves to have some prior conceptual grip on. Before Kepler wrote down his laws of planetary motion, scientists understood what was meant by the distance between a planet and the sun, and they knew how to calculate it. Similarly, before Clapeyron derived the Ideal Gas Law in 1834, scientists knew what was meant by the pressure and temperature of

[4] See Hempel (1945), Hempel and Oppenheim (1948), and Nagel (1961)

some gas, and they knew how to calculate the value of these parameters. Such laws relate the value of previously understood and measurable quantities. Call these laws *general relational principles*.

What I am calling the generative explanatory role strikes me as exactly the kind of role that such general relational principles are well-suited to play. The fact that general relational principles line up with the explanatory role traditionally allocated to laws is unsurprising, since many of the examples of scientific laws that have occupied a central place in the philosophical literature have been a specific kind of general relational principle: namely, general *dynamical* principles. In fact, there is a sense in which such dynamical principles appear to have struck many philosophers as the paradigmatic case of a scientific law. In articulating his *primitivist* view of laws, for instance, Maudlin (2007) focuses his attention on what he calls "Fundamental Laws of Temporal Evolution" or FLOTEs, which describe the temporal evolution of systems in terms of certain basic parameters. Similarly, although Emery (2019) begins by considering the question of "the relation between a law and its instances," she almost immediately restricts her attention to what she perceives to be the central case of dynamical laws. Such examples might be multiplied at length – the main point here is that for many philosophers, there is an intuitive connection between dynamical principles as the paradigmatic examples of scientific laws and 'explaining their instances' as the paradigmatic explanatory role that laws play in scientific practice.

In any case, the reason that such general relational principles are well-suited to play this generative explanatory role is connected to the fact that they are phrased in terms of parameters of which we take ourselves to have prior understanding and which we can measure independently of our knowledge of the law in question. This independent handle that we have on the parameters that feature in our general dynamical principles apparently allows us to generate descriptions of the behaviour of certain systems in a straightforward sense. If some law $\mathcal{L}$ tells us that for any system the value of $\mathcal{P}_1$ will be related in a certain way to other parameters $\mathcal{P}_2, ..., \mathcal{P}_n$, then it seems that if we combine $\mathcal{L}$ with statements of the values of $\mathcal{P}_2, ..., \mathcal{P}_n$ relevant to some system $\mathcal{S}$, we can generate a description to the effect that the value of $\mathcal{P}_1$ in $\mathcal{S}$ will be such and such.

Many of the garden variety scientific explanations we might see in textbooks involve such general relational principles being employed in this direct fashion. Suppose we want to explain why the electrostatic force we measure between two charged bodies takes some value F_1. Coulomb's law tells us in general that

$$|F| = k_e \frac{|q_1||q_2|}{r^2}$$

where q_1 and q_2 are the magnitudes of the charges, r is the distance between them, and k_e is the Coulomb constant. The force F is along the straight line joining the two charges. We know what all of these parameters mean and can measure them without reference to Coulomb's law. In this sense, Coulomb's law is an example of a *general relational principle.* When we combine Coloumb's law with statements about the magnitudes of the charges of both bodies respectively, we can quite easily extract a description of our two bodies according to which the electrostatic force between them is the measured F_1 that we were looking to explain.

3.3 Constitutive Laws

Not all laws enjoy the kind of straightforward relationship with their parameters that general relational principles do. Unlike general relational principles, some laws play an important role in *defining* some of the parameters in terms of which they are phrased. *Fourier's law for heat conduction*, for instance, features a parameter for the thermal conductivity (k) of the material involved. This parameter is *not* a quantity that we take ourselves to understand in isolation from Fourier's law. In their *Fundamentals of Heat and Mass Transfer*, Bergman et al. (2011, p. 70) write of Fourier's law that it

> "is an expression that *defines* an important material property, the thermal conductivity. In addition, Fourier's Law is a vector expression indicating that the heat flux is normal to an isotherm and in the direction of decreasing temperature. Finally, note that Fourier's Law applies for all matter, regardless of its state (solid, liquid, or gas)."

Although what Fourier's Law tells us about the world around us is phrased in terms of this thermal conductivity, it also plays a role in defining what thermal conductivity *is.* Moreover, the techniques we have for measuring this thermal conductivity are not independent of Fourier's law in the way that one could, for instance, measure the volume filled by some sample of gas without having any awareness of the Ideal Gas Law. As another example, the generalised stress-strain formulation of *Hooke's law* defines two important parameters for the materials involved: Young's modulus (E) and yield strength (σ_Y). Outside of their appearance in Hooke's law, we do not have an independent handle on what these parameters might mean or how exactly we might measure them.[5] Call laws that are phrased in terms of parameters that they themselves play a role in defining *constitutive laws*. We will look at some of the details of how these laws and material parameters work in practice in the next section.

It is important to note that such constitutive laws play an *ongoing* role in our attempts to measure and understand the parameters that they define. Suppose that some law $\mathcal{L}$ defines some parameter $\mathcal{P}$ in the way that constitutive laws typically do. Subsequently, however, we learn that there is an alternative way to understand and measure this parameter $\mathcal{P}$. Indeed, it may well have been that the regularity captured by $\mathcal{L}$ was integral to the development of this alternative technique. It may even still prove useful to introduce $\mathcal{P}$, say in undergraduate textbooks, primarily by citing its appearance in $\mathcal{L}$. Nonetheless, $\mathcal{P}$ has in truth flown the coop. We no longer require the information that $\mathcal{L}$ provides about the behaviour of $\mathcal{P}$ in order to calculate its value. In such a case, we should say that $\mathcal{L}$ is no longer a constitutive law, though it once was. Indeed, it is very likely at this stage to be a general dynamical principle.

That is to say that the kinds of laws I am calling constitutive laws play an ongoing role in our attempts to understand the behaviour of these material parameters. They are not, like our $\mathcal{L}$ above, the ladder that we simply climb and kick away. In most cases, this means that we possess only *experimental* methods for reliably and accurately calculating the value of the given material parameter, as opposed to theoretical methods that might proceed from the 'bottom up' or 'first principles' (e.g. from information about the material's molecular makeup). This is often because the parameter involved tracks features of the material that are sensitive to its mesoscale structure and not simply its molecular makeup (though more

[5] Martin (2015) provides a good overview of these features of Hooke's law.

on this later).

For example, we discover a material's Young's modulus and yield strength by subjecting it to carefully chosen stresses and observing how it behaves relative to the linear response captured in Hooke's law. Indeed, these parameters are so sensitive to structural features of the material involved that engineers typically make do with *ranges* of values for certain materials. The Young's modulus of float glass can vary between 47.7 and 83.6! Moreover, attempts to calculate Young's modulus on the basis of information about the molecular structure of a material have proved largely ineffective (Courtney (2005)). Measuring the thermal conductivity of a material is even more complicated from an experimental standpoint. In rough outline there are two kinds of technique: *steady state methods*, which infer the thermal conductivity of a material from the measurements made when the material reaches a steady-state temperature profile; and *transient methods* which do so by operating on the instantaneous state of a system during its approach to steady state.[6] Either way, Fourier's law serves as the backbone around which such techniques are developed.

There are a few things worth clarifying, at this point. The first is that general relational principles may *also* feature parameters that we can only determine by some sort of empirical measurement. For example, *Newton's law of universal gravitation* tells us that the magnitude of the attractive force between two bodies is directly proportional to the product of their masses (m_1 and m_2), and inversely proportional to the square of the distance between them (r). That is,

$$F = G\frac{m_1 m_2}{r^2}$$

The constant G here is the *universal gravitational constant.* We do not possess any independent, theoretical method of determining the value of this constant, and must rely on a variety of clever and careful experimental methods to do so.[7] Nonetheless, we do not take Newton's law of universal gravitation to *define* this gravitational constant. Rather, we typically understand G as expressing an independent fact about the strength of gravitational interactions between massive bodies in our universe. Newton's law then tells us, in terms of

[6]A helpful survey of these techniques can be found in Zhao et al. (2016).

[7]Indeed, since the gravitational force is extremely weak compared to other fundamental forces, this process of measurement can be very difficult and delicate.

this constant and other parameters we understand independently (such as the mass of the bodies), what we should expect the gravitational forces to be.

The point here is that Newton's law of universal gravitation is *no less* a general relational principle just because it features a parameter we can only measure empirically. The distinction between constitutive laws and general relational principles concerns whether or not we understand what the parameters involved mean *independently of the law itself.* We don't take Newton's law to tell us what the universal gravitational constant *means* – after all, the constant shows up in the Einstein field equations of general relatively, albeit wearing slightly different theoretical clothing. In short: just because some law features parameters that we can only determine empirically does not mean that it is automatically a constitutive law (though it *is* true that constitutive laws will almost always feature parameters that we can only determine empirically).[8]

It will also help to clarify what I mean by our having an 'independent grasp' on a parameter. We might think, for instance, that our understanding of mass changes when we move from a Newtonian theoretical setting to that of general relativity. We now take mass to be the kind of thing that it is understood to be in Einstein's theory, and consequently think that Newton was not quite right about what mass really is. Does this mean that the laws of general relativity, such as the Einstein field equations, play at least some role in defining what mass means for us? And also does this mean then that Newton's laws played a role in defining what physicists took mass to mean before the formulation of general relativity? And in this case, does this mean that neither Newton nor Einstein had what I am calling an 'independent grasp' on the notion of mass?[9]

The thought that our understanding of the various quantities and properties that scientists study is very sensitive to the underlying theoretical context in which we are working has been expressed in different ways by a variety of different philosophers. Einstein himself famously remarked to Heisenberg that "it is the theory which decides what we can observe."[10] Nomic essentialists think that properties are essentially connected to the role they play in

[8]Thanks to Gordon Belot for suggesting I be clearer on this point.
[9]Thanks to Tyler Hildebrand for pushing me on this point.
[10]See Heisenberg (1971)

various laws of nature.[11] Thomas Kuhn (1962) at different times suggested that paradigm shifts in science could induce radical changes in what we understand properties like mass or charge to be. Philosophers have emphasised the way that scientific observation is importantly 'theory-laden' – what we take ourselves to observe may depend on the kinds of theoretical commitments we hold.[12]

When I speak of our having an 'independent grasp' of a parameter, I do not mean that we need to be able to understand the meaning of the parameter *independent of any theoretical context or commitment whatsoever.* Rather, I mean that we understand what the parameter means *independent of the regularity expressed by some particular law.* We can speak of our having a grasp of what some parameter means independent of its appearance in some particular law $\mathcal{L}$ without that grasp being *totally* theory independent. We can thus say at the same time that (a) Newton had a grasp on the notion of mass that was independent from his three laws, (b) Einstein (and those following) have a grasp on the notion of mass that is independent of the core equations of general relativity, even though they understand this notion differently to Newton, and also that (c) no one has a grasp on the notion of the Young's Modulus and yield strength of a material independent of Hooke's law.

3.4 An Explanatory Mismatch

The reason that these differences between constitutive laws and general relational principles matter is that the former are not very well-suited to playing the generative explanatory role characteristic of the latter. A rough way of putting the point is that since constitutive laws define some of the parameters that appear within them, and since we rely on those constitutive laws in order to measure the value of such parameters, such laws will be far less capable of providing us with some kind of independent dynamic model of the behaviour of some system. Rather, constitutive laws define properties of materials that themselves are responsible for certain kinds of regular (mostly dynamical) behaviour.

[11] As a brief sampling, see Bhogal (2020), Bird (2007), Chakravartty (2003), and Swoyer (1982).
[12] As another brief sampling, see Azzouni (2004), Chang (2005), and Hanson (1958).

To see this, it will help to look at an example in detail. Let's think about *Hooke's law*. We typically first encounter Hooke's law when dealing with springs, in which case it tells us that the restoring force exerted by some wire spring is proportional to the displacement of the spring from its equilibrium position,

$$F = -kx,$$

where k is known as the *spring constant*. As it happens, Hooke's law can describe the behaviour of linear elastic solids more generally. In such cases, Hooke's law relates the *strain* that the material exhibits to the *stress* to which it is subject. Here, strain measures the deformation of the material, while stress measures the internal forces that the parts of the material exert on each other. In this more general context, we write

$$\sigma = E\epsilon,$$

which states that the stress, σ, is proportional to the strain, ϵ. The constant of proportionality (or material parameter) E is known as *Young's Modulus*.

So far, so good. We may then think that Hooke's law is perfectly capable of playing some kind of generative role in scientific explanations after all. Suppose we subject a steel beam to some kind of stress, say by pulling on both ends, and we see that the deformation is proportional to the measured stress to which the beam is subject. In other words, the material exhibits what we call a *linear elastic* response. Well then by augmenting the general statement of Hooke's law with the particular values of the parameters relevant to our steel beam, couldn't we generate a description of this linear elastic response? If this is true, then it seems as though Hooke's law can contribute to an explanation of the behaviour of our steel beam in exactly the generative fashion that I described in the previous section.

Things are a little more complicated than that, however. When our steal beam is subject to greater amounts of stress, it may well no longer obey Hooke's law. That is, it may deform in a way that is no longer simply proportional to the stress. Materials scientists employ *stress-strain curves* to represent this relationship between stress and strain.

FIGURE 1 shows a typical stress-strain curve. We can see that up until a critical point, the material obeys Hooke's law. That is, the stress-strain curve is a straight line with the

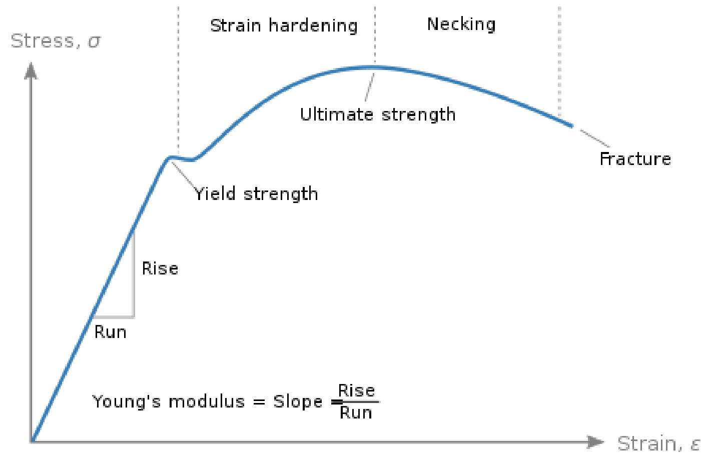

Figure 2: A typical stress-strain curve.

slope given by the material's Young's Modulus, E. After that point, however, the material no longer responds in a linear fashion to the stress imposed upon it. Material scientists call the region of the curve in which the material obeys Hooke's law the *elastic region* and the region where it does not the *plastic region*. Once in the plastic region, the material will not return to its original state once the stress is removed. That is, it will in some way be permanently deformed. We call the stress value after which the material's deformation begins to be plastic rather than elastic (i.e. according to Hooke's law) the *yield strength* or *elastic limit*, denoted by σ_Y. This value is by and large unique to each material.

We may not think that the importance of a material's yield strength poses any particular problem to the ability of Hooke's law to contribute to explanation in the typical, generative fashion. After all, we might note, many laws only apply within certain ranges of circumstances. Perhaps we should accordingly construe Hooke's law as telling us that a material will exhibit a certain kind of linear elastic response to stresses below a certain threshold. In such a case we will simply need to add the statement that the applied stress is less than a certain critical value to the other statements specific to our steel beam, at which point we will get a description of our beam's linear elastic response. Once again, it would seem that Hooke's law is more than capable of playing the direct, generative role that dynamical principles play in scientific explanation.

One more thorn lies in wait, however. How is it that we calculate this elastic limit or yield stress? In suggesting above that Hooke's law can play a direct, generative role in scientific explanations after all, we presumed that the value σ_Y is the kind of thing we can calculate independently of our observation of whether or not the material obeys Hooke's law. That is, as long as the forces are of the right kind and do not exceed some separately determinable quantity, the material will display Hookean behaviour. We imagine, then, that once we verify these conditions, Hooke's law is in a position to play a familiar generative role in explanations of the material's behaviour.

The problem is this: it is for the most part not possible to determine this yield strength *other* than simply by observing when the material stops obeying Hooke's law. In almost all cases, material scientists determine this elastic limit by subjecting the material to a variety of carefully calibrated workbench tests designed to determine when the material will begin to deform plastically rather than elastically. Indeed, attempts to determine the yield strength of certain materials based on characteristics they display on the atomic level have by and large not been able to replicate the values of σ_Y that we observe when we subject materials to tensile experiment (Courtney (2005)). We are left then to determine the yield strength by more or less playing around with the material and observing when it stops behaving according to Hooke's law.

This fact about the way that Hooke's law both defines and relies on a material's yield strength makes it difficult to see how Hooke's law could play a direct, generative role in scientific explanation. In general, we might not think there is anything wrong with using laws that only applied in restricted ranges in a direct, generative fashion. The following may seem like a perfectly fine explanation:

> Restricted Range Explanation
> Some law $\mathcal{L}$ only applies as long as the value of some parameter μ remains within a certain range. When combined with statements specifying the value of parameters $\mathcal{P}_1, \mathcal{P}_1, ..., \mathcal{P}_n$, $\mathcal{L}$ tells us that system $\mathcal{S}$ will exhibit behaviour $\mathcal{B}$. This fact, along with the fact that the value of μ was below the threshold value, explains why $\mathcal{S}$ exhibited $\mathcal{B}$.

Suppose we want to explain why some steel beam exhibited linear elastic behaviour when subject to some stress. We may simply try to adapt the above form of explanation to the case of Hooke's law.

> Potential Hookean Explanation
> Hooke's law only applies as long as the applied stress stays below the yield strength. When combined with statements specifying the parameters relevant to our steel beam, Hooke's law tells us that the steel beam will exhibit a linear elastic response. This fact, along with the fact that the applied stress was below the yield strength, explains why the steel beam exhibited a linear elastic response.

But given that the yield strength is defined with reference to Hooke's law, this way of putting the explanation is not quite the full story. The yield strength that our applied stress must stay below is defined and measure *with reference to* Hooke's law. So the above explanation is hiding something, in that it talks as though the yield strength is some independently calculable limit on the applicability of Hooke's law. If we re-phrase the above explanation to make this clearer, we might start to see the problem

> Complete Potential Hookean Explanation
> Hooke's law only applies as long as the applied stress stays below the point which a material no longer exhibits a linear elastic response. When combined with statements specifying the parameters relevant to our steel beam, Hooke's law tells us that the steel beam will exhibit a linear elastic response. This fact, along with the fact that the applied stress was below the point which a material no longer exhibits a linear elastic response, explains why the steel beam exhibited a linear elastic response.

It would perhaps be too much to say that the above explanation is *circular*, but it certainly seems defective in an important way. Part of what is strange about this explanation is that Hooke's law itself does not seem to contribute to it at all. If we know that there is a value of applied stress below which the material will exhibit a linear elastic response, and we know that the applied stress is below this point, then surely *this* is the explanatorily relevant information. Given that in order to know that Hooke's law applies in some circumstance we must already possess information that itself seems sufficient to explain the behaviour of our steel beam, it would seem that Hooke's law is *superfluous*. Where the descriptions that general relational principles provide of the behaviour of various systems seem quite directly explanatory, the descriptions that constitutive laws facilitate do not.

3.5 A Supporting Explanatory Role

So if constitutive laws like Hooke's law do not seem able to play a *generative* role in scientific explanations, is there some other contribution they might make to our explanatory endeavours? In answering this question, it might help to think about what exactly such laws tell us about the world. Part of the reason that constitutive laws like Hooke's law are poorly suited to playing a generative explanatory role in scientific explanations is that when we construe them in '*All Fs are Gs*' terms, they seem to say something trivial about the world. Understanding what such laws have to tell us about the behaviour of certain classes of physical systems may require that we adopt a more subtle point of view.

So how is it, then, that constitutive laws like Hooke's law or Fourier's law can tell us about the world, expressed as they are in terms of material parameters that they themselves play a role in defining? The short answer is, as Batterman (2021) explains, that the material parameters that feature in these continuum mechanics equations code for structures that exist *between* the atomic and continuum scales. These *mesoscale* structures, such as voids, cracks, grain boundaries, and so on, play an important role in determining how it is that the material will behave on the continuum scale. The fact that these parameters are sensitive to structures that emerge at this mesoscale level helps to explain why it is in general very difficult to reliably calculate them by upscaling from atomic considerations (recall, for example, that calculations proceeding from the atomic lattice drastically misestimate the yield strength of most materials).[13]

In short: the reason that these parameters can help to tell us how particular systems will behave is that they code for these kinds of mesoscale structures. Material scientists have developed a wide array of approaches involving transport functions and order parameters in order to understand exactly what aspects of a material's mesoscale structure such parameters are registering.[14] These material parameters, then, capture properties and threshold values that are integral to our understanding of what such laws tell us about the behaviour of certain systems. Nonetheless, they for the most part cannot be understood or calculated

[13]For a good introduction to the difficulties involved in modelling many of these scale dependent behaviours in physics, see Batterman (2013).

[14]See Batterman (2021) for more detail on how these order parameters and correlational variables work.

separately from the laws in which they feature. Although in most cases we must determine yield strength simply by subjecting a material to workbench tests and seeing when it stops behaving in a Hookean fashion, mesoscale considerations can provide us with confidence that this is not *all* that a parameter like yield strength captures. In this sense, it is really the law *alongside the appropriate material parameters* that allows us to understand how systems will behave.

The above may point to a kind of *supporting* explanatory role that constitutive laws might play in our scientific endeavours. Insofar as they code for this complicated mesoscale information, it is really the *material parameters* that carry much of the information relevant to explaining the behaviour of certain systems. But insofar as they capture some restricted regularities in terms that allow us to *define* these material parameters, constitutive laws do make an important contribution to our explanatory efforts. Hooke's law does not itself *explain* the linear elastic response exhibited by our steel beam, but it *does* allow us to define, measure, understand, and otherwise get at the properties of our steel beam that *do* explain its linear elastic behaviour. In other words, constitutive laws like Hooke's law play a kind of *definitional supporting role* in the explanation that eventually emerges from the consideration of the behaviour of our steel beam.

We can think about this definitional supporting role in something like the following way. Suppose you are interested in the behaviour of some particular system, $\mathcal{S}$. A savvy passerby gives you the following piece of information: the behaviour of a wide range of systems, including your $\mathcal{S}$, conforms to a relatively simple pattern (call it $\mathcal{L}$) as long as we express that pattern using a parameter, $\mathcal{N}$, that stands in for a number that is by and large unique to each system we might encounter. Moreover, the passerby adds, this number is not some sort of gerrymandered constant of proportionality but rather reflects robust structural features of the material of which $\mathcal{S}$ and things like it are made. What we *cannot* do, however, is calculate the value of this number from scratch or anything like that. Once the passerby leaves, you decide to use the information they have given you to calculate the value of $\mathcal{N}$ for your system by poking and prodding $\mathcal{S}$ and comparing its behaviour with the pattern expressed in $\mathcal{L}$. You find that by using $\mathcal{L}$ in this way, you can indeed arrive at a stable value for $\mathcal{N}$ appropriate to your $\mathcal{S}$. If we come to understand what $\mathcal{N}$ is telling us about $\mathcal{S}$ and other systems like it, then we may be able to explain why it behaves the way it does. The pattern $\mathcal{L}$ contributes to this explanation not by generating descriptions of the behaviour of $\mathcal{S}$, but rather by serving as the vehicle by way of which we can define and calculate the value of our parameter $\mathcal{N}$, which captures the mesoscale goings-on that are *really* responsible for our system's behaviour.

The suggestion, then, is that although they do not play the kind of generative explanatory role that general relational principles tend to (as we saw in §1), constitutive laws play *this* kind of supporting explanatory role in scientific practice. Constitutive laws tell us that otherwise disparate systems may be seen as exhibiting a common pattern when they are described in terms of the right material parameters. Since these material parameters are more or less unique to each system and not the kind of thing we can calculate from first principles, however, the value of what constitutive laws tell us does not lie in the dynamical descriptions they provide. Rather, the explanatory value of our constitutive laws lies in the fact that they help us to define and measure the material parameters that encode important and robust information about the material structure of our system.

3.6 Laws and Explanation

We began by asking: what *is* "the central explanatory role that laws play in scientific practice"? What we have learned is that the answer to this question is going to be less neat and uniform that philosophers have typically expected. We have identified at least two distinct contributions that laws may make to the explanations that emerge from scientific inquiry, but there are likely to be others. For instance, the kinds of explanation that broad conservation laws facilitate may not fit neatly into the paradigmatic generative role that general relational principles play.[15] Laws that are equilibrium in character and do not attempt to support unfolding dynamical descriptions at all may provide their own kind of support to our scientific explanations. Once we attempt in earnest to identify the different kinds of explanatory roles that laws play in scientific practice, we will likely realise that scientific laws

[15] See Lange (2016), Adlam (2022a,b).

form a relatively broad menagerie of principles and generalisations which may contribute to our explanatory projects in a *variety* of distinct ways beyond the two we have canvassed above.

Given this, it is important to be clear about what we mean when we say that laws explain their instances. There is no problem if all we mean by this is that the explanatory role that laws play in scientific practice is deeply connected to the way they allow us to explain particular behaviours in terms of more general facts and principles. In this sense we might say that all laws explain their instances, and there are a variety of ways they might go about doing this. We should reject, however, the idea that the explanatory role of laws consists in laws explaining their instances in the specific sense in which *All Fs are Gs* is supposed to explain *its* instances. Perhaps the generative explanatory role, if we squint quite a bit, might seem close enough to laws explaining their instances in this sense. The supporting explanatory role that constitutive laws play, however, very clearly does not fit this pattern. As the case of Hooke's law shows, laws can play a central role in our broad attempts to explain the particular in terms of the more general without *directly explaining* the particular facts in question.

Recognising this variety in explanatory contribution allows us to take an interesting perspective on certain philosophical disputes regarding the role that laws play in scientific explanation. Recently Hicks (2021), drawing on Ruben (1990) and Skow (2016), has argued that laws in general *do not* explain their instances. Rather, the suggestion goes, they feature in meta-explanations: laws explain why certain causes explain certain effects. Roughly, if it is a law that *All Fs are Gs*, then this law explains not why *a is F and a is G* but rather why *a's being F* explains *a's being G*. Although on such a view laws do not *directly* explain why some system behaves the way it does, they do "in an important sense, back the explanation" we might offer, since "in order for the explanans to explain the explanandum, the two must be connected by at least one law" (Hicks, 2021, p. 539). This dispute turns on the assumption that there is a *single* explanatory role that laws play in scientific practice. After all, if there is a single such role then we must presumably think that one of these characterisations (or perhaps another!) is the *right* way to capture it. The task then might fall to each camp to somehow explain away the kinds of cases on which the other relies.

If, on the other hand, we recognise that there are a variety of explanatory roles that different laws might play in scientific practice, this disagreement fizzles out. What has happened, we might think, is that Hicks and company have correctly identified, *pace* the traditional consensus, that laws on occasion seem to provide a kind of support to the explanations that feature in science that does not look all that much like explaining their instances. If laws play a variety of explanatory roles in scientific practice, there is no pressure to explain away or reduce the generative role in terms of the supporting role or vice versa, nor should we feel compelled to try and capture these two roles in some common, schematic fashion. Rather, as philosophical students of scientific methodology, we may simply note that scientific laws come in a variety of shapes and sizes and are entwined with our understanding of the physical world in different ways, and this means that in different explanatory contexts they may come to be used in different ways. Indeed, important and fruitful philosophy of science likely lies in the task of characterising these different roles and understanding what it is that allows certain laws to play certain roles.

3.7 Methodological Upshots

Once we recognise that there are several distinct explanatory roles that laws might play in scientific practice, we are pushed to think differently about our approach to the "central explanatory role" with which we began. Rather than a winner-takes-all challenge to provide *the* correct account of the explanatory role of laws, we might see the philosophical task as that of identifying the various distinct explanatory patterns in which laws feature in scientific inquiry and attempting to understand why it is that certain laws feature in the patterns they do. This kind of philosophical task will almost certainly involve paying attention to many of the more subtle mathematical and physical distinctions that scientists appeal to in their use of laws – for instance between ordinary and partial differential equations, or between elliptic, parabolic, or hyperbolic PDEs, between the kinds of modelling strategies that certain laws can support, and so on.[16]

[16]For a recent effort to draw attention to the importance of some of these distinctions, see Wilson (2022).

Of course, philosophical questions about the role that laws play in scientific practice are of interest in their own right, and perhaps the long shadow cast by debates about the *metaphysics* of laws has meant that such questions have not always received the dedicated treatment that they merit. But it is also true that if we want scientific practice to guide or constrain our metaphysical accounts of laws, then we will need to address these questions more carefully. As I mentioned at the outset, the observation that laws play a central role in scientific explanation serves as the starting point for a variety of arguments in the literature on laws. Such arguments tend not to be very specific about exactly what aspect of this central explanatory role they mean to appeal to, often gesturing quite generally to the fact that laws explain their instances or explain the particular facts we observe, or something along those lines. The goal of such arguments is often to show that some metaphysical account of laws or other tells either a particularly compelling or particularly implausible story about how it is that laws play this explanatory role in scientific practice.

The fact that laws play a variety of distinct explanatory roles in scientific practice does not *necessarily* undermine such arguments, but it does mean that in many cases something more specific must be said before we know what to make of them. Perhaps some arguments that appeal to the fact that laws "explain their instances" may be able to make do with the quite general, fuzzy way of understanding this claim. Or perhaps some arguments require that laws explain their instances in some more specific sense, but only require that laws *at least sometimes* explain their instances in *something like* this sense. The proponent of such an argument may find that what I have called the generative explanatory role comes close enough to what they have in mind, and may content themselves with the fact that although some laws do not play this role in scientific practice, at least some *do*.

It is entirely plausible, on the other hand, that some such arguments rely on a more specific understanding of what it is for a law to explain its instances, or perhaps on a claim of more general scope to the effect that *all* laws play this more specific role in scientific practice. Here, the details certainly matter. It may matter, for instance, that there is still quite some distance between the generative explanatory role that laws play in scientific practice and the schematic claim that laws explain their instances in more or less the same way that *All Fs are Gs* explains *its* instances. It may not. It may matter that only *some* laws explain their instances in anything remotely like this sense, but it also may not. Whether the fact that laws in reality play a variety of distinct explanatory roles in scientific practice threatens such arguments probably depends on the details of the argument. The point is simply that these details must be examined before we know what to make of arguments that, for example, proceed by stating that "*All* laws explain their instances," where this is intended in the *All Fs are Gs* sense.

This raises another question: to what extent should we expect metaphysical accounts of laws to make sense of the complexity and messiness of scientific practice? If it were true that laws played a single, uniform explanatory role in scientific practice, and that this role could be captured in a relatively abstract slogan, then it would seem perfectly reasonable to insist that any metaphysical account of laws should be able to make sense of this explanatory role. But this is not true. Given that the explanatory use of laws in scientific practice is in fact quite heterogeneous, it is less clear that we should expect our metaphysical accounts of laws to make sense of the many and varied explanatory uses to which they are put. Metaphysical accounts of laws aim primarily at answering abstract questions about the *nature* of laws and their relation to notions like necessity and possibility. If different laws in different contexts can be used to support explanations in different ways, then perhaps making sense of this behaviour requires that we grapple with the messier details of model construction, the mathematical techniques involved and the experimental contexts addressed, rather than by reaching down from the abstract vantage point of our metaphysical accounts. In short, we might ask: how relevant are the messy details regarding the various explanatory uses to which laws are put in scientific practice to the abstract questions about the *nature* of laws with which metaphysical accounts are concerned?

I don't have any particularly clear or sharp answers to offer to any of these methodological questions. The point is simply that if claims about the central explanatory role that laws play are to continue to play an important role in philosophical discussions of laws of nature, then we must be clear about how such claims are intended to make contact with the (often quite complex) details of scientific practice. If, as I have argued, laws play a far more heterogeneous explanatory role in scientific practice than has typically been appreciated, asking these kinds of methodological questions becomes even more important. Some of the arguments that appeal to the explanatory role of laws may, perhaps with some mild adjustment or rephrasing, strike us as perfectly legitimate even *after* we take account of such heterogeneity. I also suspect that quite a few will not. The devil, as we so often find, is in the details.

4.0 The Coordinating Role of Laws in Empirical Science

4.1 Introduction

The question of the role that laws play in the process of scientific inquiry has long occupied a place of central importance in philosophical discussions of laws of nature. The reason for this pride of place is by and large the natural thought that facts about what it is that laws *do* will surely help us in some way or another to work out what it is that laws *are*. So it is that it has become quite common for philosophers to criticise one or another metaphysical account of laws by first citing the fact that laws perform some particular task in scientific practice and then by arguing that the proposed account of what laws *are* does not seem to allow laws to perform that particular task. Indeed, under the auspices of *pragmatic Humeanism* some philosophers have explicitly relied on claims about what laws *do* in articulating their accounts of what laws *are*.

So: what role *do* laws play in the process of scientific inquiry? Call this our KEY QUESTION. Philosophers have typically approached the task of answering this key question by attempting to write down a sort of *job description* for laws in scientific practice. That is, they have attempted to answer our key question by writing down a list of tasks that laws perform more or less on their own in the context of scientific practice. It is commonly supposed, for instance, that lawlike generalisations provide explanations, facilitate explanations, and underwrite counterfactuals, where accidental generalisations do not. As a result of this, a popular answer to our key question has been to simply say: in scientific practice, laws provide explanations, facilitate explanations, underwrite counterfactuals, and so on.

There is a serious problem with this kind of *job description approach* to our key question: it falls foul of the fact that in scientific practice, laws on their own are not often *capable* of performing many of the tasks with which they are traditionally associated. Before they can be of much use, laws must often be supplemented by a wide variety of modelling ingredients, such as material parameters, boundary conditions, modelling constraints, and so on – what I call their *supporting casts*. Properly speaking, in most cases it is a complicated *package* of laws and supporting constructions, rather than the law itself, that allows us to do things like predict and explain the behaviour of various real world systems. It is thus quite difficult to write down a list of tasks that laws on their lonesome accomplish in the context of scientific inquiry.

In this chapter, I suggest that we thus require a fresh approach to our KEY QUESTION. In particular, I argue that we should approach the question of the role that laws play in scientific practice by focussing on the way that they support the construction of *scientific models*. After all, it is *models* of various kinds that serve as the central workhorses of scientific inquiry, and thus *models* that tend to perform many of the predictive and explanatory tasks that have traditionally been associated with laws. Adopting this approach, I put forward an answer to our key question: laws play a special role in providing a *coordinating framework* for the various different pieces of information we must employ in constructing models of real life systems.

Here's the plan. In §4.2, I outline what I take to be the core characteristics of the traditional *job description* approach to the question of the role that laws play in scientific practice. In §4.3, I illustrate how laws often require the help of a menagerie of supporting constructions before they can be of much use at all in scientific practice. In §4.4, I argue that this fact spells trouble for the job description approach, and motivate a new approach to our key question which emphasises the role that laws play in the construction of scientific models. In §4.5, put forward a particular answer to our key question along these lines: laws provide us with the coordinating frameworks we require for the construction of models. In §4.6, I work through the details of some examples in order to make this suggestion more concrete. In §4.7, I consider some of the philosophical upshots of the kind of picture I am developing, specifically with respect to special science laws and the connection between scientific theories and scientific models. In §4.8, I conclude with some comments on what this all might mean for the role that facts about scientific practice might play in abstract metaphysical debates.

4.2 The Job Description Approach

Philosophers writing on laws have typically approached the task of answering our KEY QUESTION in roughly the same way: they have tried to write down a list of tasks that laws perform in the course of scientific inquiry. Moreover, these tasks are supposed to be the kind of thing that mere accidental generalisations, by contrast, cannot perform. *"All copper conducts electricity"*, for instance, explains why *this* piece of copper conducts electricity, whereas "All the coins in my pocket are silver" does not explain why *this* coin is silver. The idea, then, is that there will be a series of such tasks, and we can answer our KEY QUESTION by more or less *listing* them. Call this the *job description approach* to our KEY QUESTION.

Indeed, it strikes me that there is some consensus amongst philosophers regarding the tasks that will feature in this job description. In particular, laws are supposed to explain their instances, provide predictions, support inductive inferences, underwrite counterfactual claims, or some combination thereof. In this vein, for instance, Swoyer (1982, p. 203) writes

> "That laws of nature play a vital role in explanation, prediction, and inductive inference is far clearer than the nature of the laws themselves."

Amongst others, Earman (1986, p. 101) produces a similar job description for laws, writing that they earn their keep by "supporting subjunctive and counterfactual conditionals, providing explanations, and grounding induction." The same tasks appear in the list of central characteristics attributed to laws by Fred Dretske (1977). Less recently, Braithwaite (1953), Goodman (1955), and Ayer (1956) put forward views according to which performing certain tasks were *constitutive* of lawhood – for Braithwaite it was explanation, for Goodman and Ayer, prediction.

More recently, a similar approach can be seen in the flowering of various *Pragmatic Humean* accounts of laws.[1] Such accounts work within the framework of David Lewis's 'Best Systems Account' (BSA), in which laws are simply the generalisations that feature in the best systematisation of the Humean mosaic of categorical properties.[2] What makes these accounts 'pragmatic' is that they propose that we pick out the best systematisation with

[1] See, for instance, Dorst (2019b), Hicks (2017), Jaag and Loew (2020), and Wilhelm (2022).
[2] See Lewis (1973, 1983, 1994).

explicit reference to the role that laws play within scientific practice. As Michael Hicks (2017, p. 993) puts it, such views "take the role of laws to be the primary metaphysical determiner of lawhood."

It would seem then that Pragmatic Humeans have more reason than most to think about the role that laws play in scientific practice. It is striking, then, that they too have largely adhered to this *job description approach* to the question of what role laws play in scientific practice. The idea remains that we might look at scientific practice and determine a list of tasks that laws singlehandedly perform in that context. The difference is that these accounts use this job description to evaluate laws as a group or system via candidate systematisations. So it is that the 'Epistemic Role Account' developed by Hicks (2017, p. 995) holds that laws are "those true statements that, as a group, are best suited to produce predictions and explanations and be inferred from repeated observation and controlled experiments." The 'Best Predictive System Account' put forward by Dorst (2019b) takes the central role of laws to be providing predictions, while Jaag and Loew (2020, p. 2534) proceed from the claim that "the main cognitive function of laws is facilitating predictions."

The important point here is that philosophers writing on laws have by and large understood our KEY QUESTION as calling for an inventory of tasks that perform more or less on their own in scientific practice – a *job description*. But what does this "more or less" amount to? It is of course sometimes conceded that laws must be providing with some kind of initial information before they can provide us with explanations and prediction – Hempel (1945, p. 36) called these "determining conditions." If we know that it is a law that "*All Fs are Gs*," we must still know that "*a is F*" before we can predict or explain the fact that "*a is G*." Bhogal and Perry (forthcoming, p. 17) put this point differently when they write that the best system must contain "non-nomic boundary conditions as well as laws," since

> "A system where the axiom are only the laws of Newtonian mechanics, for example, would not be particularly informative on its own – it needs the addition of boundary conditions specifying what objects there are, their mass, their velocity, and so on."

The idea here is that laws must of course be provided with certain initial pieces of information before they can be of much use in scientific practice. Nonetheless, once this information is on the table, the laws are fully capable of turning it into the kinds of things demanded by our job descriptions all on their very own. The job description approach then might be characterised as the idea that answering our KEY QUESTION involves writing down a list of tasks that laws, once provided with initial non-nomic information, are capable of performing all on their own in the context of scientific practice.

4.3 Supporting Casts

The job description approach falls foul of the following fact about scientific practice: before they can be of any explanatory or predictive use, laws must often be supplemented by a wide variety of modelling ingredients, such as material parameters, boundary conditions, auxiliary models, and so on – what I call their *supporting casts*. Properly speaking, it is a complicated *package* of laws and supporting constructions that allows us to predict and explain the behaviour of various systems, rather than simply the law itself (plus, perhaps, initial conditions). Although laws are surely *involved* in such contexts, taken by themselves they are almost always explanatorily and predictively *inert*. In this section, I'll say a little more about how this all unfolds, and in the next I'll explain why this fact should push us to look beyond the job description approach.

Let's start with an example. The *Navier-Stokes equation* are the laws that tell us how fluid behaves in a wide variety of circumstances. Nonetheless, we cannot simply plug some initial conditions into the Navier-Stokes equations and expect them to single-handedly produce a description of how our fluid is going to behave. Before they can do that they must be augmented with, amongst other things, a variety of *boundary conditions*. Although philosophers will often speak as though boundary conditions are more or less the same kind of thing as initial conditions, they are in fact both conceptually and mathematically quite different kinds of gizmo. Boundary conditions are constraints on the values that the solution to some set of differential equations must take on the boundary region of our space. They typically arise in the context of *boundary value problems*, in which a core differential equation must be augmented by additional constraints before it admits of a unique or appropriate solution. These constraints tend to be differential equations themselves, and moreover must typically apply *at all times* t and not merely at some specified initial time. Without these boundary conditions, the Navier-Stokes equations do not in many cases possess 'solutions' in any cogent sense.

A very important class of boundary condition on which the Navier-Stokes equations rely are called 'slip conditions.' These tell us what will happen at the boundary between the fluid and its solid container.[3] Typically, though not always, we require a *no-slip condition*, which sets the tangential component of the velocity to zero. Without this information, the Navier-Stokes equations cannot be solved in any concrete fashion for real life systems in which viscosity is important (i.e. most of them). In physical terms, the no-slip condition captures the fact that at the fluid-solid interface, the force of attraction between the fluid and solid particles is stronger than that between the fluid particles themselves, since the effect of viscosity predominates at the boundary (see Rapp (2017, pp. 244–245) and Schobeiri (2010, p. 234)). Something like the no-slip condition is required to explain why dust accumulates on a stationary ceiling fan, for instance.

It is important to note that they are by no means simple pieces of discrete information that we should think of as being 'plugged into' the relevant law. Indeed, Sykora (2019) has shown that the no-slip condition in particular is invariant under certain classes of interventions and enjoys quite broad empirical and theoretical support. Bursten (2021) outlines how they play a "structure-specifying" rather than "variable fixing" role, and for this reason suggests that they are "not not-laws." The no-slip condition does not tell us what the velocity of any particular fluid particle is at any particular time, but rather provides a constraint on the way the velocity of the fluid particles in the boundary region must evolve for all times t. It is this added structure that ensures that the task of solving the Navier-Stokes equations amounts to what Jacques Hadamard (1923) famously termed a "well-posed problem." This simply means that the model admits of a unique solution that changes continuously with the initial conditions. Without the inclusion of some kind of slip condition, we would be unable to find a unique solution (or sometimes any solution at all) to the Navier-Stokes equations for fluid systems.

[3] More precisely, they specify the tangential component of the velocity of the fluid at the surface of flow along a stationary boundary.

4.4 A New Approach

What the case of the Navier-Stokes equations illustrates is that at least sometimes laws are not capable of *single-handedly* turning initial condition-style information into the explanation and predictions that they are commonly supposed to provide. In short, *laws need help*, and they must turn for that help to modelling ingredients that look nothing like 'mere initial conditions.' Put differently, the Navier-Stokes equations play a role in helping us to predict and explain the behaviour of various systems not as heroic lone warriors but as part of a more developed *package* of modelling ingredients.

This poses a problem for what I've been calling the *job description approach* to our KEY QUESTION: laws simply do not on their own *do* many of the things that typically appear on the job descriptions that philosophers have written down. Moreover, we also cannot construe them as single-handedly 'taking in' initial condition-style information and turning it into the kind of thing that such job descriptions dictate. Perhaps this is no cause for alarm. We may think that philosophers have simply been *wrong* about the tasks that should appear on the job description for laws. There would be something very strange about a conception of the role played by laws in scientific practice that does not bear *any* connection to things like explanations and predictions, but perhaps the right job description may nonetheless be out there, waiting to be found.

Cases like that of the Navier-Stokes equations do suggest, however, that there is a deeper problem with the job description approach. The things that *do* perform many of the central tasks in scientific practice tend to be *models*. Models are, as Healey (MS, p. 4) puts it, "the work-horses of contemporary science." In most cases, arriving at a prediction or explanation of the behaviour of some system of interest requires that we construct a model. It is only when we speak loosely, or when certain modelling approaches strikes as second nature, that we attribute the predictions and explanations to the Herculean labour of the *laws* that lay at the heart of the model. Granted, laws are important ingredients in many such models, but they are by no means the only ones. If we try to answer our KEY QUESTION by thinking about the things that laws accomplish single-handedly, we are likely to miss the important work that they perform *within* such broader modelling projects.

This suggests a new way of approaching our KEY QUESTION. Thinking about what laws somehow single-handedly or directly *do* in the context of scientific practice gets in at the wrong level. If it is true that multi-component *models* are responsible for the lion's share of the predictions and explanations that emerge from scientific practice, we should instead ask: do laws play a special role in the process of *model construction*? Rather than trying to draw some direct connection between laws and the products of scientific inquiry with which they are typically associated, we should think about the contribution that laws make to scientific practice in the context of the models that *do* produce explanations and predictions.

4.5 The Coordinating Role

The model-based approach I am advocating provides us with a *recipe* for answering our KEY QUESTION. Rather than attempting to write down a series of tasks that laws perform directly and single-handedly in the process of scientific inquiry, we should think about the way that scientists go about constructing models and ask ourselves whether laws play any special central role in *that* process. On this approach, actually *answering* our KEY QUESTION involves detailing what exactly this special central role might be that laws play in the process of model construction. In the rest of this paper, I will attempt to do just that. My suggestion will be that laws play a special role in providing a *coordinating framework* for the various pieces of information that we need to make use of in order to model real world systems.

Here's an analogy, to start. If we want to build a house, there are a variety of different materials that we need: some kind of foundation, moving tiles, bricks, insulation, windows, and so on. However we can't simply stack these materials together on their own and expect them to form a stable domicile. In addition to the various raw materials, we require a *central frame* to hold everything together. This central frame, often made of wood as it happens, connects to the various building materials in different ways and serves to *mediate* the interactions between them. For instance, the roof tiles do not have all that much to do with the foundation *directly*, but the weight of the tiles is distributed sustainably across the foundation because of the way that the frame is anchored into the foundation and supports the tiles.

These kinds of construction frames exhibit a kind of *generality*: the same frame can be reliably used with a variety of different combinations of materials. If we change the shape of the roof tiles or the colour of the bricks, we don't need to go out and find ourselves a totally different central frame. Indeed, part of the importance of such a central frame lies in the fact that it saves us from having to determine on each occasion we want to build a house exactly how the particular materials we are using might be made to work with one another. This generality, however, is not without limit. We must still be careful: perhaps if we use a less dense foundation, we might find that our frame no longer allows us to combine certain kinds of heavy roof tiles with certain kinds of brick exterior walls and still expect our structure to stand. Although we can use such frames in a wide variety of circumstances, small changes in the combination of materials employed may affect whether certain frames are suitable after all. Although the frame provides much of the structure required for the construction of our house, the combination of central frame and assorted building materials must be *calibrated to each other*.

The idea, then, is that within our scientific modelling efforts, laws play a role that is broadly analogous to the central frame of a constructed house. In order to reliably model some real world system, we need to have on hand a variety of different pieces of information about it. These pieces of information may have different mathematical forms, be determined by different kinds of experiments, describe the behaviour of our system at different scales, and so on.[4] Because of this, we are rarely able to simply slap all of the information we have about a system together and expect a workable model to emerge. What we need, in such cases, is a kind of *coordinating framework* – we need the modelling equivalent of a central frame for our house.

My suggestion that in the course of scientific inquiry, laws provide exactly this kind of central coordinating framework for our modelling efforts. That is, they provide the central frame that can interact with the relevant different modelling ingredients in such a way that what emerges is a workable and reliable model of the system of interest. Like the central

[4]Moreover, as we saw in §3, the kinds of information we require are often not the kind of thing we can think of as simple 'initial conditions' in any way.

frames of our houses, this requires that laws must exhibit at least some degree of *generality*, in that our model must not simply fall apart if we change some initial conditions or swap out the values of certain material parameters. But also like the central frames of our houses, we must nonetheless remain vigilant. Otherwise innocuous-seeming changes in the character of these additional modelling ingredients may mean that a certain law no longer provides a workable coordinating framework. That is, the combination of laws and their supporting casts must be *calibrated to each other.*

Now, I am not suggesting that *all* of the models employed in the course of scientific practice are built around coordinating frameworks provided by laws. Just as with the right materials and know-how we can build a house without a central wooden frame, scientists make profitable use of many models that are not at their heart arranged around some statement of law – these are often called *phenomenological models.* The point of the model-based approach I put forward in §4 is that if we want to think about the role that laws play in the explanatory and predictive endeavours of contemporary science, we need to think about how they facilitate the process of model construction *when they so appear.* This by no means is to say that *all* of the models that scientists employ in explanatory and predictive contexts must make use of laws.

The answer I have put forward to our KEY QUESTION, then, is that the role that laws play in scientific practice involves providing central coordinating frameworks for the construction of models, and that these models *in turn* perform many of the tasks with which laws have traditionally been associated. This is, admittedly, a rather abstract way to characterise the role that laws play in scientific practice, motivated by way of analogy. To make things more concrete, it will help to see how this coordinating role plays out in a particular, detailed case.

4.6 Navier-Stokes Revisited

Before Claude-Louis Navier (or, indeed, George Stokes) wrote down what we know as the Navier-Stokes equations, physicists studying hydrodynamics were well aware that modelling the flow of fluid required lots of information. In particular, we needed amongst other things to know at least a little bit about: (1) the velocity of the fluid flow at certain key points, (2) the interaction between the fluid and any solid boundaries, (3) the *viscosity* of the fluid involved, (4) the *pressure gradient* to which the fluid is subject, and (5) the *thermal conductivity* of the fluid involved.

This assemblage of modelling ingredients contains a variety of very different kinds of information. The interaction between the fluid and its solid boundary must be captured by a differential equation like the no-slip condition that we met in §3. The viscosity and thermal conductivity of a fluid are *material parameters*, which encode quite complex information about the mesoscale structure of a fluid in a numerical value that is *unique to that fluid*.[5] The pressure gradient is a vector which points in the direction in which the pressure increases most rapidly. We get this pressure gradient by taking the gradient of a vector valued field characterising the pressure at various interior points of our space.

Even though physicists of the late 18th and early 19th centuries knew that these pieces of information capture the processes that are responsible for *some aspect or other* of fluid behaviour, it was not immediately clear how they could all be combined in one model.[6] The simpler *Euler equations* were able to coordinate some of these pieces of information, but not others – they do not allow us to take account of either the viscosity of the thermal conductivity of a fluid. The Navier-Stokes equations were such an important theoretical breakthrough, then, *precisely because* they finally allowed physicists to construct models that took account of almost all of the processes they antecedently knew to be relevant to fluid behaviour.

Even when combined with the right supporting cast of boundary conditions and material parameters, the Navier-Stokes equations are notoriously difficult to solve. Indeed, developing techniques for producing boundary conditions for the Navier-Stokes equations and understanding their behaviour remains a very active area of modern mathematical research.[7] Navier himself, although quite confident in the theoretical underpinnings of the equations, nonetheless conceded that the formula "cannot suit the ordinary cases of applica-

[5]On these mesoscale parameters, see Batterman (2021).

[6]For a helpful historical presentation of these developments, see Darrigol (2005).

[7]See, for instance, Kučera and Skalák (1998), Nordström and Svärd (2006), and Raymond (2007).

tion" (Darrigol (2005, p. 115)). Even as physicists became more and more confident that the Navier-Stokes equations were the correct way to combine the various modelling ingredients they knew to be relevant to fluid behaviour, it was not thereby *automatically* clear how we might thereby develop a model of the behaviour of real fluid systems.

This problem was addressed by Ludwig Prandtl, who used the Navier-Stokes equations as the core framework for his *boundary layer model* of fluid flow. Prandtl noticed that when we combine the Navier-Stokes equations with the no-slip condition and an understanding of what the viscosity of a fluid captures, we see that there are a wide range of cases in which frictional effects are only important in a very thin layer close to the boundary of the flow. Away from this *boundary layer*, we can model the flow using far simpler equations that ignore the frictional effects of viscosity. The resulting model, then, is a stitched together mix of the full Navier-Stokes equations near the boundary of the flow, and the simpler equations for inviscid flow away from the boundary.

In Prandtl's model, it is the Navier-Stokes equations that provide the *structure* that allows us to determine when frictional effects are important in the first place. With that framework in hand, we know where in our fluid flow we need to capture the gritty details of viscosity and where we can get away with a more smoothed over description. They tell us, to stretch the analogy somewhat, which walls in our house are load-bearing and which ones we can knock down if it makes our life easier. In providing just such a framework, the Navier-Stokes equations not only allow us to combine the various modelling ingredients we need to combine in order to model real-world systems, but provide us with ongoing guidance as to how to do this in a way that allows for tractable predictions to emerge with regularity.

4.7 Upshots: Special Science Laws and Models

At its core, the answer I have offered to our KEY QUESTION should be judged by whether it provides an authentic picture of the role that laws play in scientific practice. That being said, it strikes me that the picture of the role that laws that I have put forward may shed light on some issues both within the literature on laws of nature and in philosophy of science and metaphysics more broadly. In particular, the idea that laws provide coordinating frameworks for the construction of scientific models may suggest more profitable ways of thinking about (a) the question of special science laws and (b) the relationship between models and the more abstract, general machinery of a scientific theory. In this section, I'll say a little bit about both of these in turn.

4.7.1 Special Science Laws

Accounts of laws have traditionally struggled to make sense of the notion of law as it employed outside of the (often amorphously delineated) realm of 'fundamental physics.' Where 'fundamental laws' are supposed to be universal and exceptionless, the so-called laws of the 'special sciences' appear to admit of exceptions, and perhaps to require a wide variety of *ceteris paribus condition* for their application. Or where 'fundamental laws' are taken to refer to certain kinds of privileged predicates or properties, 'special science laws' seem less picky in their referential inclinations. Lewis's BSA, for instance, requires that laws make use of a special class of 'perfectly natural properties,' and thus precludes generalisations that talk about things like organisms and markets from being admitted to the pantheon of laws proper.[8] The result is that philosophers working on laws have tended to understand the things that are referred to as laws in sciences like biology and economics as importantly different from the laws of physics (or at least, the laws of the suitably 'fundamental' corners of physics).[9]

The gap between the laws of fundamental physics and those of the special sciences may appear to widen further when we turn our attention to the role that they play in various scientific contexts. If we are wedded to the *job description approach*, it might seem to use that such 'special science laws' are not going to be fit to play the role that 'fundamental laws' play in scientific practice. After all, if laws are supposed to single-handedly generate predictions and explanations and so on, then generalisations that admit of exceptions and require a lot of specific conditions to obtain before they can be applied reliably will perhaps seem at best fit to play a pale imitation of the role that laws are typically supposed to play

[8]See Lewis (1983).

[9]As a brief sampling, see Fodor (1974), Lange (1993), Rupert (2008), and Wilhelm (2022).

in scientific practice.

This seems to me a strange place to end up for two reasons. The first is that it would *prima facie* be quite odd if scientific methodology exhibited these kinds of sharp methodological discontinuities. It is of course true that scientists of various stripes study the world using a wide variety of experimental methods, conceptual tools, and mathematical techniques, and they are interested in different aspects or scales of the world. Nonetheless, it strikes me as strange to think that only the laws operating within a certain corner of physics are capable of properly performing the role that laws play in scientific practice, and that once we leave this setting we find that the things referred to as laws are only in some lesser sense capable of playing this role. The second reason is that the kinds of things scientists call laws both within the broader world of physics and in other sciences *do* seem to play a broadly similar role to those that feature in 'fundamental physics,' despite their differences.

In recent years, some philosophers have attempted to stretch various metaphysical accounts of laws with the aim of capturing special sciences laws in their net.[10] These attempts typically involve loosening or amending the machinery of the theory of laws in question so that the perceived differences between special sciences laws and fundamental laws are no longer relevant to their counting as laws proper. Whatever the merits of these attempts, it does not seem to me that they do much to remedy the problems I raise in the paragraph above. If we want to vindicate the idea that there is an important kind of methodological continuity that is exhibited in the role played by the things that scientists call laws in different domains, then it will not help much to simply amend our account of what a law might be. Rather than papering over the differences between the laws that appear in various sciences by amending our account of laws, we should attempt insofar as we can to find the common methodological thread that runs through the laws of various sciences *despite* the differences they might otherwise exhibit.

The picture of the coordinating role of laws that I have developed here, it seems to me, might provide precisely such a common methodological thread. If we think about the role of laws in terms of the way that they contribute to the construction of *scientific models*, then we are in a position to make far more sense of this methodological continuity. After

[10]See, for instance, Cohen and Callender (2009), Sartenaer (2019), and Schrenk (2014).

all, scientists of almost all stripes are in the business of constructing models, and so it might at first glance seem that an approach that emphasises the role that laws play in supporting the construction of scientific models is better placed to explain how the laws appealed to by material scientists and population geneticists might play the same role as those employed by those working in 'fundamental physics.'

I think it is quite plausible that the things that biologists and chemists and those working in other 'special sciences' refer to as laws can be understood as providing coordinating frameworks for the construction of models. Defending that suggestion in detail is beyond the scope of this particular paper. The point simply is that if we think, as the *job description approach* would have us do, of the role of laws in terms of some series of tasks that laws, because they are in some way special, are able to single-handedly perform in the context of scientific practice, then it is going to be very difficult to make sense of the methodological continuity (or at least the methodological gradation) that is exhibited in scientific practice. An approach that emphasises the importance of laws in the process of model construction, however, seems to offer a far more promising hope of thinking about the role of laws in a way that does not somehow silo off 'fundamental physics' from the rest of science.

4.7.2 Models and Theory

Philosophers of science of different stripes have long disagreed about the right way to think about the relationship between scientific models and scientific theories. On both the *syntactic* and *semantic* views of the structure of scientific theories, models play a kind of subsidiary role to theories. On the syntactic view, theories are sets of sentences in an axiomatised logical system, and as such models are simply particular interpretation of this calculus.[11] There is thus a clear sense in which the models are downstream of, or issue from, the more abstract theory. On the semantic view, scientific theories simply *are* collections of models.[12] Most proponents of the semantic view follow Suppes (1960) in taking models to be some kind or other of set-theoretic structure. The kinds of models that scientists employ in their practice, then, emerge when we take these set-theoretic structures and endow them

[11] A classic exhibition of this kind of view can be seen in Nagel (1961).

[12] For an overview, see French and Saatsi (2006), Suppe (2000), and van Fraassen (1989).

with some kind of physical interpretation.

This view of models as somehow subordinate to theory has been challenged on a number of fronts. These challenges emphasise the variety of ways in which models either operate independently of scientific theories or indeed the ways in which scientific theories *rely* on models of various kinds for their construction.[13] One prominent thread in this series of challenges focusses on the fact that models employed in scientific practice are in some important way *autonomous* from more abstract theory. Morgan and Morrison (1999) flesh this out as the claim that models both (a) *function* independently of theory and (b) are *constructed* with minimal reliance on theory.

It seems to me that there is something surely right about this way of seeing things. It also seems to me that the picture I develop of the way that laws serve to *coordinate* the construction of scientific models highlights the way in which the calibration between abstract theoretical structure and concrete models is a *two way street*. While model construction can sometimes proceed independently of theoretical generalisations like laws, it is also sometimes the case that the guiding laws *dictate what the models need to look like*. We saw this in the case of the Prandtl's boundary layer model, in which certain characteristics of the Navier-Stokes equations were exploited as constraints in order to construct workable models of certain classes of fluid flow.

In the terms of our analogy, it is certainly true – as Morgan and Morrison and company emphasise – that we sometimes independently know what building materials (models) would be best to use, and our decision of what frame (laws and/or theory) to employ follows from that fact. It is also true that sometimes we know that a certain frame is the right one to use for our landscape, and that seriously constraints the kind of foundation or flooring we can use. Indeed absent such constraints, it may have been very difficult to otherwise determine what the right foundation or flooring for our landscape might have been! That is to say: in providing the kind of coordinating frameworks they do, laws can provide important *constraints* on the process of model construction.

I take it that the kind of perspective we get by considering the coordinating role of laws

[13]As a brief sampling, see Cartwright (1983), Hartmann (1995), and Weisberg (2013), as well as the papers collected in Morgan and Morrison (1999).

in the context of such debates is quite friendly to the perspective advocated by Morgan, Morrison, and company. The thought is not that cases like the Prandtl boundary layer model show us that models are in fact *not* generally autonomous from more abstract theory. Rather, the thought is that if we are to (rightly) recognise that the traditional picture of models as emerging almost trivially from abstract theoretical structures is a non-starter, and thus emphasise the ways in which models can operate autonomously of said abstract theoretical structure, we need also to develop a more authentic story about how and when bits of abstract theory like laws can *constrain* and *guide* the process of model construction. My suggestion here is that the picture I have developed above can put us on a path to developing just such a story.

4.8 Conclusion: Metaphysics and Scientific Practice

Much of the philosophical discussion of the role that laws play in scientific practice has occurred more or less *en passant* within debates about one or another metaphysical theory of laws. But there is no reason that this need be so. Nothing about our KEY QUESTION prevents us from tackling it as a question in good standing with the philosophy of science more broadly. Indeed, if claims about the methodology of science are to continue to play a central role in metaphysical debates about laws of nature, then it is crucial that we develop a more detailed and less simplistic answer to our KEY QUESTION.

I have argued that providing such a more detailed and less simplistic answer to our KEY QUESTION requires that we abandon the traditional job description approach and adopt an approach that emphasises the way that laws contribute to the construction of scientific models. Moreover, I have suggested that this contribution is best thought of as laws providing just the kind of *central coordinating structure* that we often require in order to construct models of various systems. It is these models that, in turn, tend to do the explanatory and predictive work that is often otherwise attributed to laws.

What is there to like about this way of answering our KEY QUESTION? If nothing else, adopting this kind of approach represents a step towards remedying the conspicuous absence of any talk of *scientific models* within discussions of the role that laws play in scientific practice. Given the centrality of models of all kinds to the unfolding of scientific inquiry, it seems like remedying this absence is an essential precursor the development of a more authentic picture of how laws are used in scientific practice.

It is worth noting by way of concluding that part of the reason that our KEY QUESTION has come to occupy a place of central importance in the literature on laws seems to be that many contemporary metaphysicians adhere to some form or another of *naturalism*, which demands that our answers to abstract metaphysical questions 'fit with' or 'be informed by' scientific practice in the right way. One way of implementing this may be to hope that an examination of scientific practice might provide us with a helpfully naturalistic set of criteria that we can then employ when choosing between metaphysical accounts of laws.

If the picture of the role that laws play in scientific practice that I have developed is even roughly correct, then there is good news and bad news for the prospects of such an approach. If one's picture of scientific practice is filled out with claims like 'laws explain their instances,' then it is easy to see how scientific practice can guide and constrain our metaphysical theories of laws. The bad news, then, is that if the road to a characterisation of the role that laws play in scientific practice must travel through the messy terrain of scientific models, such schematic claims are unlikely to do a particularly good job of capturing scientific practice in any detail. If one's picture gets more complex and begins to make reference to the messy details of model construction in science, it is less clear what the immediate metaphysical import of our picture is supposed to be.

The good news, I think, is that this does not mean that the philosophical examination of scientific practice cannot generate *any* such additional criteria. It just means that this process might not quite be so *automatic*. That is, we may need to think a little bit more carefully about the relevance of scientific practice to the metaphysical question at hand, rather than simply lifting schematic claims from the philosophy of science and adding them to our criteria of adequacy for a metaphysical account of laws. As it stands, many of the appeals to scientific practice that appear in the literature on laws seem to proceed as though any self-respecting metaphysical account of laws should be capable of making sense of more or less *any* fact we might unearth about the way that scientists use laws in their inquiries. This has always struck me as a quite odd state of affairs. Insofar as the approach I am advocating to our KEY QUESTION may cause us to re-think the demand that any metaphysical account of laws must somehow *also* provide a detailed and authentic facsimile of the messiness of scientific practice, it strikes me as something that metaphysicians should regard as a Good Thing.

5.0 Conclusions

There are, I think, two broad morales that emerge from this set of papers, considered together. The first relates to the status of philosophical questions about laws and scientific practice, *independent* of questions about the metaphysics of laws, and the second relates to the way that facts about scientific practice might be expected to play a role in metaphysical discussions of laws. By way of concluding, I'll say a little bit about each of these in turn.

Given that questions about the role that laws play in scientific practice are so central to many debates in the literature on laws, it is striking that they tend only to be addressed in a somewhat *en passant* fashion, within metaphysical discussions of laws of nature. As we have seen getting a handle on the place that laws occupy in the methodology of contemporary science is a difficult task unto itself. In going about the business of providing explanations, predictions, and so on, scientists make use of a wide variety of pieces of information about the world in order to construct models of various real-life systems. These pieces of information can be quite general, quite specific, or somewhere in between. They can take different mathematical forms, or make different contributions to the structure or content of our model. They can relate differently to the experimental methods we employ, exhibit different kinds of counterfactual stability, relate to microphysical descriptions in different ways, and so on.

In the face of this complexity, we would do well to remember that we can think of the task of understanding the role that laws play in scientific investigation as *totally independent* from the task of providing and defending a metaphysical account of laws of nature. If we only pursue the former within the context of the latter, then we are to some extent shackled to the terms in which the latter is phrased. The metaphysical questions that philosophers ask about laws are, by nature, abstract, and bear close connection to a host of questions about determinism, free will, modality, and so on. As a result, they are best phrased in quite schematic and abstract terms – recall Armstrong's declaration that we may think of laws as variants on "*All Fs are Gs*." The more that we reckon properly with the details of scientific practice surrounding laws, the less purchase that we get by thinking in such schematic terms. That is to say that there is a kind of *mismatch* between the abstract and schematic terms in which metaphysical questions are best posed and the concrete and detailed notions involved in understanding complex scientific practice.

If questions about the role that laws play in scientific practice are valuable and interesting in their own right and independent from metaphysical questions about laws, then there is no reason that we need to tackle them using the abstract vernacular appropriate to metaphysical discussions of laws. It is striking, for instance, that despite the prominence of discussion of scientific practice in the literature on laws, *models* are hardly mentioned. In part, I think, this is because it is difficult to see how the terms in which philosophers of science think about models can be translated effectively into the abstract terms in which metaphysicians talk about laws. But if we are thinking of questions about laws and the methodology of science for their own sake, there we need not translate at all. The answers we provide can be as complicated and messy as they need to be, and can be phrased in whatever terms seem most appropriate to capturing philosophically important aspects of scientific methodology.

Of course, what emerges from this process may not bear in any obvious way on the task of providing a metaphysical account of laws, but that is perfectly okay. In a slogan, what we are doing is the *philosophy of science of laws* rather than the *metaphysics of laws*. There will, no doubt, be a variety of interesting and deep connections between these two projects. But the important thing here is that once we separate the two tasks, we may begin to realise just how many important and interesting distinctions, nuances, and subtleties in scientific methodology surrounding laws have escaped philosophical notice owing to the fact that the metaphysical project has historically cast such a long shadow over the philosophy of science one. And this, I suggest, is a cause for optimism: there is much rich and interesting work to be done!

The second morale that emerges from these three papers has to do with exactly how we can expect facts about scientific practice to bear on debates about the metaphysics of laws. Much of the focus on the role that laws play in the course of scientific inquiry that has characterised the philosophical discussions of laws of nature in recent years seems to be driven by some form or another of *naturalism*, which demands that our answers to abstract metaphysical questions 'fit with' or be 'informed by' scientific practice in the right way. Perhaps the hope here is that an examination of the way that scientists use laws in the course of their investigations might somehow furnish us with additional criteria for choosing between various metaphysical theories or accounts of laws. So, for instance, if one thinks that the traditional criteria that metaphysicians employ when they adjudicate between competing accounts (whatever those might be) do not quite settle the debate between, say, Humean and non-Humeans, one might hope that we can appeal to 'facts about scientific practice' to break the deadlock.

If one's picture of scientific practice is by and large populated by a series of abstract and schematic claims, then it is reasonably straightforward to see how this might work. In observing scientific practice, we notice, say, that 'laws explain their instances.' Then, although perhaps it is the case that the traditional criteria of metaphysical theory choice do not conclusively point either in the direction of Humean or non-Humean accounts, we are able to augment this list of criteria with an additional one: a metaphysical account of laws should, all else being equal, be able to makes sense of the *fact* that, in scientific practice, laws explain their instances.

If the things I say in the preceding papers are right, then things are unlikely to work this way very often at all. I do not think that this means that metaphysicians ought to abandon *all* hope that facts about scientific practice might *in some way* guide their attempts to answer abstract metaphysical questions about laws of nature. Rather, they should simply for the most part abandon the hope that this guidance is going to be as *automatic* or *straightforward* as they might have wished.

Once we see questions about laws and the methodology of science as independent of metaphysical questions about laws, we open the door to the possibility that the answers that philosophers of science provide to these questions are going to be quite unwieldy or messy or complex from the perspective of metaphysical debates about laws. That is, the answers that emerge are unlikely to *immediately* furnish us with anything like neat criteria for choosing between competing metaphysical accounts. Rather, extracting criteria for theory choice from a detailed and nuanced picture of the role of laws in scientific practice is itself a *philosophically substantive* task.

Things might then work something like this. Hoping for some kind of guidance from the practice of science, we might think carefully about the aims and goals of our metaphysical questions and develop a sense for *exactly what* aspects of scientific practice surrounding laws are likely to be relevant to those goals and aims. Having done so, we might then examine the details that emerge from a more detailed and nuanced picture of the role of laws in scientific practice, and think about exactly how we can extract some of the lessons learned therein and apply them to our metaphysical questions. It may turn out that no such clear criteria for theory choice can be distilled from the picture of scientific practice that emerges, but I don't see any reason to think that this will always be the case.

Something like the above picture strikes me as an improvement on the current state of play, in which it seems as though metaphysical accounts of laws are expected to be answerable to any old fact about scientific practice their rivals can conjure. Such appeals to scientific practice are very rarely accompanied by an explanation of exactly why *this* aspect of scientific practice is the kind of thing with which a metaphysical account of laws need concern itself. Leaving aside the question of whether or not these facts indeed hold water as such, it has always struck me as odd that metaphysical accounts of laws are expected to moonlight as authentic facsimiles of the messiness of scientific practice. It seems far more natural to me to think of the two tasks as distinct from one another, although capable of informing each other in different ways.

Thinking of things this way provides metaphysicians with a slightly different way of approaching their abstract questions about laws in a *naturalistic* spirit – that is, as somehow 'guided by' or 'fitting with' scientific practice. Rather than thinking that the mere observation of scientific practice will provide the kind of neat, general facts that they can plug into our existing list of criteria for theory choice, we might recognise that scientific practice is often complex and heterogeneous and that developing an authentic picture of this messiness is a tall philosophical order in itself. Once we have developed such an authentic picture, we may then ask ourselves: what, if anything, does this picture tell us about how we should go about answering our metaphysical questions about laws?

That is to say that it is to a *detailed philosophical picture of scientific practice* that our metaphysical accounts should be responsive, and not merely to any old individual fact about scientific practice that one might be able to produce. Moreover, what it might mean for our accounts to be 'responsive to' or 'guided by' this picture of scientific practice may depend both on the metaphysical questions we are asking and the details of the picture that emerges. It strikes me that this way of bringing facts about scientific practice into contact with metaphysical questions about laws may provide a more promising broader recipe for executing the kind of 'scientifically-informed metaphysics' that has become quite popular in recent years. Defending that claim, however, would require a dissertation unto itself.

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