Keeping track of Neurath's bill:
Abstract concepts, stock models and the unity of classical physics

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> *We do* not *arrive at* 'one' *system of science that could take the place of the 'real world' so to speak; everything remains ambiguous and in many ways uncertain.* [1]
>
> Otto Neurath

1 Introduction

In 1935 Otto Neurath penned these comments in his paper 'Unity of science as a task'. A passage introducing the paper remarks that *scientific* people aim at a common procedure of inquiry by which to better understand the ambiguities and uncertainties of our world. But he asks, "Is this uniformity the logical consequence of our program? It is not; I stress again and again; I see it as a *historical fact* in a sociological sense" (Neurath, 1935/1983, p.115). For Neurath unity of science was indeed a task, a goal. Unity in procedure of inquiry is crucial for understanding one another – and for making sense of the very uncertain world in which we live. But we do not arrive at it by discovering some small range of fundamental underlying principles of the world. It is this view of unity that resonates with the focus of this paper, a view that attempts to make sense of the domain of scientific inquiry while doing justice to the ambiguities and uncertainties that science necessarily leaves untreated.

As a means to this general end, this paper takes its cue from Nancy Cartwright's (1999) *The Dappled World*. It presents a defence of the view that the world in which we live – or rather the world given to us through scientific investigation – is probably at best described by a patchwork of laws with domains of limited range. Given this conception, the hope of uncovering some unifying set of characteristics of the world

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[1] Neurath (1935/1983).

could be dramatically misplaced: for all we know there is no simple set of underlying laws that describe these different domains. And we are better commissioned to examine the world as it is revealed to us – via a (monumental) set of often disjointed, sometimes converging, pockets of understanding. The world revealed to us through scientific investigation is a dappled one. We may elect to bring this world together under a unifying umbrella of scientific activity, but any such unity results from deliberative actions of scientists themselves. As *The Dappled World* suggests,

> What happens is more like an outcome of negotiation between domains than the logical consequence of a system of order. The dappled world is what, for the most part, comes naturally: regimented behaviour results from good engineering. (Cartwright, 1999, p. 1)

Many reject this point of view, claiming that the vast number of successful applications of scientific inquiry point in the direction not of some messy patchwork, but to a much more pristine and elegant set of fundamental laws applicable across the multifarious domains of scientific inquiry. These domains may not be unified in contemporary science, but we are warranted in holding out for the belief that they are in fact describable by a simple set of laws.

The present paper considers criticism from Sheldon Smith.[2] He locates his discussion in classical physics, forgoing the quantum domain. This restriction of scope suits our purposes just fine: if science provides a patchwork of local domains of analysis treatable locally then it seems we had best consider problems that arise within individual, narrowly defined domains one by one rather than assuming one sweeping philosophical account.

Smith argues that classical physics exhibits an element of unity which the *dappled view* – the view defended here – overlooks. Thus rather than challenge the dappled view in one global swipe, he attempts to ‘chip away’ at it by indicating where it fails to account for important unified relations among, what he takes to be, sub-theories of classical mechanics. Moreover, he draws attention to a potential problem for the role stock models play in Cartwright’s defence of dappling. In this paper we

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[2] Smith, S.R. 2001.

address the unity Smith claims is overlooked and take an opportunity to clarify certain features about stock models.

2 Warrant and the stretch of laws

The great virtue of our best physics theories lies in their claims to truth via the success of a huge number of precise empirical predictions. However, the cost of this virtue is the limitations on the domain we are warranted to assign them. Let us consider.

The no-miracles argument numbers among the most prevalent arguments in favour of the truth of scientific theories. The empirical success of our best scientific theories, the argument goes, would appear to be a miracle unless we assume that those theories are true. If we are to take this argument seriously, then we only have grounds for believing that our best theories are true within those domains in which they are empirically successful. Even the best of our scientific theories are successfully applied only to relatively rare situations in the real world. And many of these are situations that we devise within the walls of our laboratories or within the casings of our technological artefacts. The situations that occur spontaneously in nature and in which we have been able to make precise, successful predictions are few and far between. This gives rise to the view that the world could well be described at best by a patchwork of laws.

To illustrate this point, *The Dappled World* borrows an example from Neurath—a thousand-dollar bill swept by the wind in Saint Stephen's Square.[3] Can we model the trajectory of the bill dropped from the cathedral? Not likely. Could we model it *in principle*? The argument defended here is that empirical successes in classical mechanics at predicting the motion of an object cannot warrant the belief that these same laws of classical mechanics – or more precisely, point particle mechanics – govern the behaviour of the bill. If the theory does not provide the resources to construct a predictively successful model of this particular situation then we are not warranted in claiming that it falls under the laws as they are specified. No model. No

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[3] Cartwright, 1999, p.27. Originally from Neurath, O. (1933/1987).

laws.[4] It is blind faith to say that we can model it in principle based on the scope of confirmed assignable force functions in classical mechanics.

According to Newton’s second law the total force on the bill is equal to its mass times its acceleration. But to say merely that there are forces acting on the bill is too abstract to tell us about the situation. Any such description will apply only with more concrete statements about the particular forces at work. And these descriptions are provided by the stock models of the theory. For example, one of the basic stock models of point-particle mechanics tells us that an unsupported object of mass m in the vicinity of the earth will experience a force of magnitude mg towards the centre of the earth. So these models provide us with bridge principles to apply abstract theoretical concepts like ‘force’ to concrete situations. Since the bill is an unsupported object in the vicinity of the earth, the bridge principle tells us that there is a gravitational force on it.

However, point particle mechanics does not seem to have a bridge principle that allows us to associate a force with another crucial cause acting upon the bill: the wind. What point particle mechanics provides us then is a partial model requiring supplementary information describing the effect of the wind as a force. This information could perhaps be provided by fluid dynamics, given answers to the right sorts of questions about the conditions within which the bill was dropped, what shape or state the bill was in, etc. We should however be wary of assuming that we can always describe the conditions of the bill in the right way – a way that allows us to assign a force function to the wind via a bridge principle. That’s because the floppiness of the bill and the irregularities of its surface would make the distribution of lift around its surface at each moment practically impossible to describe with the set of stock models given in fluid dynamics. Alternatively we could construct a model in which the force that the wind exerts on the bill at each instant is set equal to the total force on the bill minus the force due to gravity so that the direction and magnitude of the total force on the bill at t is deduced, via Newton’s second law, from the direction and magnitude of acceleration of the bill at t. This model might describe the motion of the bill quite accurately. Its descriptive success, however, cannot be

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[4] Of there are laws functioning, but we have neither specified them, nor therefore, located the theory that gives the form of their application.

evidence for Newtonian mechanics because the assignment of the force function to the wind is purely ad hoc.

What is true of the bill is true in physics in general. It is usually only in very special circumstances that predictively successful models can be constructed in a principled way from the resources of a single theory. These are cases where all the factors relevant to the targeted features of the phenomenon fall under the concepts of the theory, and of the same theory. In general no one theory contains enough stock models to go beyond simplified situations. Models may of course be fine-tuned to represent particular circumstances, but such corrections are generally ad hoc, often not given by any theory let alone one single theory.

Smith's paper argues that Neurath's bill can, in principle, be treated in a single theory. That is because it can be properly treated with the combined resources of point-particle physics and fluid dynamics, and these two are not really separate theories. They are actually instances of a more general super theory: classical continuum mechanics (CCM). Moreover, CCM utilizes not a limited number of stock models with a limited scope but a huge (perhaps infinite) number of principled models that are not ad hoc. This is because it provides models only in a trivial sense.

3 Unification: the success story

As Smith describes it, CCM contains two main kinds of equations. First, there are general principles, which describe abstract physical laws and apply to all materials. Second, there are constitutive equations, which describe the mechanical behaviour of specific classes of materials. As an example of a general principle of CCM, Smith mentions Cauchy's first law of motion:

$$\partial T_{ij} I \partial x_j + \rho b_i = \rho\left(dv_i / dt\right)$$

Smith notes that T_{ij} represents the stress tensor, while b_i represents body forces acting on the bill. This is crucial because locating both the contact forces and the gravitational forces in one equation precludes the notion that one must draw on models from different theories. Smith takes it that both the stress tensor and gravitational forces are utilized, but he argues, "it does not thereby follow that we are bringing to bear two different theories. All of this takes place within the framework of CCM centred around Cauchy’s laws” (Smith, 2001, p.463).

This unification is nice. As Smith claims, CCM provides an abstract framework in which to bring together causes of motion otherwise separately treated in point-particle mechanics and in fluid dynamics. And it provides a way to show how to calculate what happens when both kinds of causes act at once. Moreover it is well confirmed across a variety of cases where both kinds of causes act simultaneously. This last feature is important because it provides warrant for the formula ‘as a whole’ and hence for deductive predictions that follow from the formula when both kinds of causes are at work at once.

Perhaps it is well to explain this last virtue in more detail. In thinking about the travel of warrant from past predictive successes to hypotheses about new applications, we might face a version of an old problem that frequently besets theories of confirmation. Cauchy’s formula reduces essentially to $f = ma$ in point-particle mechanics whenever T_{ij} is zero. It hence makes a huge number of accurate deductive predictions for these cases. Similarly it reduces to an analogous formula in fluid dynamics when b_i is zero and hence can count a huge number of these kinds of cases as predictive successes. Nevertheless we would not want to allow warrant to travel from the union of these two sets of successes to new cases where neither quantity was zero without a large number of additional successes for cases where both kinds of causes are at work at once. Happily these are available and where available we have warrant for ascribing successful application of the unified theory.

4 The open question

The question then remains: can CCM, as characterized by Smith, treat Neurath’s bill? CCM, as Smith portrays it, provides a theory that describes the accelerations of bodies that are subject both to stresses – the causes studied in fluid dynamics – and to bodily forces – the causes studied in classical particle mechanics. Does that mean that (pace relativistic and quantum considerations) at least as far as accelerations are concerned we have a single theory that will cover every case, including Neurath’s bill? That depends.

What is important for this question is that, as Smith points out, Cauchy’s laws themselves “are not sufficient for tracking the motion of any system because one does not know anything about what the stress-tensor needs to be or what body forces might be acting on the medium” (Smith, 2001, p.462). That is, it is not until the constitutive assumptions are added to describe how to assign functional forms to the relevant stress tensor or body forces that the laws give a particular domain of application. These principles in CCM are in our vocabulary the ‘bridge principles’ of the theory, connecting specific descriptions of situations and materials (the stock interpretive modes of CCM) with the functional forms of T_{ij} and b_i that are supposed to apply when the descriptions obtain.

The constitutive principles matter: the strong warrant that can be accorded the predictions of CCM depends on them. They fix the domain in which Cauchy’s laws have the kind of strong warrant that is demanded in physics and they thus fix the domain of application for these laws. Can the stock models associated with these constitutive principles describe all the causes of acceleration that occur?

Phenomenologically the causes of acceleration are indefinitely various. Prima facie it seems unlikely they can all be properly described by the very restricted set of stock models available. The past history of successes and failures at bringing real situations under the purview of these models can hardly decide: there are notable successes and there are hosts of failures. The problem is that even when the resources of those two theories are successfully pulled together, we may not be able to construct a predictively successful model of the situation and, therefore, we would have no evidence to believe that the motion of the dollar bill in Saint Stephen’s Square falls under the jurisdiction of CCM.

Smith knows this. He suggests the initial conditions would make it unlikely that it would, in fact, be possible to model the event. Nevertheless he wants to argue, along with others, that this would still be possible in principle because Cauchy’s law with the relevant constitutive equations contains terms accounting for both gravitational and contact forces. Smith wants to suggest that by subsuming fluid dynamics and point particle mechanics under CCM there is a unity there. We do not wish to deny this. Our worry is that this unity is not warranted for application beyond the domain in which its stock models have proven themselves. Locating individual theories under a more abstract notion can be fruitful in unifying distinct efforts in scientific inquiry, but it does not warrant the application of that abstract beyond the stretch of its bridge principles, or in the case of CCM, beyond the stretch of its constitutive principles. We are consequently left with nothing like the unity suggested by Smith, but rather with a unified theory that is warranted only over a much smaller domain than he supposes.

5 Stock models and the *dappled view*

Smith's second concern is over the distinction between ad hoc models and principled ones. According to Smith, there is no uncontroversial way to single out some of the models of CCM as its stock models. If science gives a patchwork of developed theories each with a limited number of stock models, then we need an account of why certain models get to count as 'stock'. Smith suggests that Cartwright (1999) leaves this account unclear, but that it potentially relies on three bases. First, one can look to something like a canonical list, for instance a list constructed from a text such as R.B. Lindsay's, which Cartwright herself cites as a source of stock models.[5] Second, one could consider scientific use, wherein past modelling successes give warrant for adopting specific bridge principles. Third, one can consider non-phenomenological models, which suggests that all legitimate scientific models are given directly by theory. He finds all three of these potential bases problematic. We consider the first two proposals in this section and the third in the next section.

Smith argues that looking to a textbook is clearly not a principled way to single out some models as stock models. We agree. We go to a textbook to learn what the principles of a theory are, but that does not show us why those *are* the principles of the theory. A model is not a stock model because it is included in Lindsay's textbook. Rather, a model is included in Lindsay's textbook because it is taken to be a stock model.

Smith also notes that, in the Section "Forced Oscillations of a Dissipative System", Lindsay introduces as an external force the force $F_0e^{i\omega_0 t}$, which Smith calls the 'Lindsay equation'. If all models included in Lindsay's text were stock models, then, Smith argues, since F_0 and ω_0 are arbitrary constants, we would allow: "[…] any (odd) piecewise-continuous function of time (on the interval from $-\pi$ to π) to count as

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[5] Lindsay, R.B. (1933 [1950]). *Physical Mechanics*, 3rd ed., London: D. Van Nostrand Co.

a principled force function derived in a principled way from this stock model” (Smith 2001, p.467).

It may look, at first sight, as if a bridge principle is presented here: ‘when a dissipative system is subject to a forced oscillation it is subject to a force $F_0e^{i\omega_0 t}$’, but this is not correct. The Lindsay equation represents a generic force that drives the oscillatory system, not any specific force. In other words, the Lindsay equation represents a wide range of forces that might be driving the oscillations. We are not told *what* the force is. We are simply told its abstract form and, as Smith stresses, this is not to be told very much at all, since almost any concrete function can be cast into this form. So, in fact, no bridge principle is given here for assigning a concrete force function to a dissipative system subject to a forced oscillation. We are hardly even given a constraint on what any such force function must look like.

Second, Smith argues that if one relies on scientific use to fix what stock models are, then since Lindsay’s text provides a canonical list of usage, use as a basis for selecting stock models falls to the same objection as the first. Scientific use does not limit the range of stock models because those models allow an unlimited range of force functions, and thus any cause that contributes to acceleration can be modelled within the theory. As we noted above, in classical particle mechanics, for example, if x represents the total force function that can be assigned to causes using other bridge principles of the theory, the remaining causes can be represented by the function $f=ma-x$.

But the lessons of successful prediction – the kinds of predictions that speak for the truth of the theory – point in the opposite direction. In order to develop a new model for classical mechanics, it is not sufficient to put forward one of the infinitely many possible force functions that are compatible with Newton’s laws. We also need to associate with that force function a more concrete description of the circumstances under which a body is subjected to that kind of force. The role of stock models in classical mechanics is exactly that of providing us with a more concrete description of the circumstances under which a body is subjected to a force given by a specific function.

Stock models are well established when they have been successfully applied to concrete situations via bridge principles time and time again. The current set of the stock models of classical mechanics may not (and probably does not) exhaust the set of all possible stock models. In principle, we might be able to develop new bridge principles associated with new ‘stock’ models that tell us what the force on a certain body is when the new stock models apply, but until we do so we have to resort to the set of well-established stock models. On our view then the only way to warrant the claim that it is possible to construct a principled model for the force of the wind on the bill is to show how such model can be constructed in a principled way and showing it to be successful. Anything short of this would seem to be nothing more than a promissory note that for all we know is likely to be void.

Smith, however, follows a different strategy. Rather than try to show that CCM provides us with the resources to construct a principled model of the wind-swept bill, Smith argues that no model of CCM is principled in our sense. This, according to Smith, is for two reasons. The first is that CCM does not operate with stock models. There is only a set of general principles with which any constitutive equations must comply. Even though most continuum mechanics texts treat a few tractable examples in detail, the standard practice in CCM is that any equation that adheres to these principles is an acceptable constitutive equation for use in modelling. But there will be infinitely many such equations. So, there are bound to be the ones needed for Neurath’s bill (Smith, 2001, p.471).

This fits very nicely with his treatment of the Lindsay equation in classical particle mechanics. We have argued that the Lindsay equation does not figure a proper bridge principle since it does not specify a concrete functional form for the force causing the oscillation. But it does constrain the form of this function to some extent, and hence fits Smith’s conception of a general constraining principle. The second objection that Smith makes to taking models in CCM as principled in our sense is that, according to him, in CCM there are no guides for applying constitutive equations, except for completely trivial ones such as ‘if the material is a Hookean elastic solid, then apply Hooke’s law’.

Smith’s first claim seems mistaken. Classical continuum mechanics has a set of favourite constitutive equations that regularly can be counted as confirmed bridge principles. Textbooks in continuum mechanics usually provide their readers with constitutive equations for a variety of classes of materials, which typically include non-viscous fluids, Newtonian viscous fluids and Hookean elastic solids (cf. Fung

1969, Ch. 7 and Spencer Chs. 8 and 10). And each of these has been repeatedly used with successful prediction and hence has claim to be included among the principles of the theory.

In fact, the development of what we call stock models for specific classes of materials seems to be one of the main aims of continuum mechanics. As one textbook puts it:

> The problems of continuum mechanics are […] of two main kinds. The first is the formulation of constitutive equations which are adequate to describe the behaviour of various particular materials or classes of materials. […] The second problem is to solve the constitutive equations, in conjunction with the general equations of continuum mechanics, and subject to appropriate boundary conditions, to confirm the validity of the constitutive equations and to predict and describe the behaviour of materials in situations which are of engineering, physical or mathematical interest. (Spencer, 1980, pp. 2–3)

Admittedly the first of these two tasks is not easily accomplished, so bridge principles are hard to come by. If there are few real bridge principles in CCM this is due to the fact that finding the right constitutive equation for a certain kind of material in certain kinds of circumstance is a formidable task, not to the fact that the theory can do without them and still have within it the resources to provide principled predictions for new cases.

To get a sense of the difficulties in devising bridge principles in CCM, consider the case of Newtonian fluids. The stress-strain relationship of a Newtonian fluid is specified by the equation: $\sigma_{ij} = -p\delta_{ij} + D_{ijkl} V_{kl}$, where D_{ijkl} is a tensor of the viscosity coefficient of the fluid and V_{kl} is the rate-of-deformation tensor. (Note that when $V_{kl} = 0$ the constitutive equation reduces to the one for a non-viscous fluid considered above.) As another textbook of continuum mechanics notes:

> For Newtonian fluids we assume that the elements of the tensor D_{ijkl} may depend on the temperature and density of the fluid, but not on the stress or the rate of deformation. The tensor D_{ijkl} […] has […] 81 elements. Not all these constants are

> independent. A study of the theoretically possible number of independent elements can be made by examining the symmetry properties of the tensors σ_{ij}, V_{kl}, and the symmetry that may exist in the atomic constitution of the fluid. We shall not pursue it here because we know of no fluid that has been examined in such details as to have all the constants in the tensor D_{ijkl} determined. (Fung, 1969, p. 129)

In most cases, a highly simplified version of the above constitutive equation is actually used. The rationale for using this equation is the assumption that most fluids are isotropic. For a fluid that is not isotropic the theory does not have the resources within itself to predict its behaviour. The shortage of bridge principles is a severe handicap in applying the theory.

Consider now the second objection. Even if in CCM there is a constitutive equation for a Newtonian viscous fluid, Smith maintains, there seems to be no bridge principles except for trivial principles such as ‘use the constitutive equation $\sigma_{ij} = -p\delta_{ij} + D_{ijkl}\, V_{kl}$, for a Newtonian viscous fluid’. However, even this claim is not entirely correct. In introducing Newtonian viscous fluids, the Spencer textbook tells us:

> In experiments on water, air and many other fluids, it is observed that in a simple shearing flow […] the shearing stress on the shear planes is proportional to the shear rate s, to an extremely good approximation and over a very wide range of shear rates. This behaviour is characteristic of a Newtonian viscous fluid […]. This model of fluid behaviour describes the mechanical properties of many fluids, including the commonest fluids, air and water, very well indeed (Spencer, 1980, p.116).

Moreover the Fung textbook tells us:

> Air and water can be treated as nonviscous in many problems. For example, in the problems of tides around the earth, waves in the ocean, flight of an aeroplane flow in a jet, combustion in an automobile engine, etc., excellent results can be obtained by ignoring the viscosity of the media and treating them as a nonviscous fluid. On the other hand, there are important problems in which the viscosity of the media, though small must not be neglected. Such are the problems of determining the drag force acting on an airplane, whether a flow is turbulent or

> laminar, the heating of a re-entering spacecraft, the cooling of an automobile engine, etc. (Fung, 1969, p.129)

So if in CCM there are no strict bridge principles that associate a certain constitutive equation to a certain specific material, one reason is because in different situations we may use different equations for the same material. Air and water for example can be represented as Newtonian viscous fluids as well as non-viscous fluids depending on the problem at hand. But this is not to say that two completely different constitutive equations are assigned to the same material in different circumstances. As we have remarked above, the constitutive equation for a Newtonian viscous fluid reduces to the one for non-viscous fluid when the viscosity of the material in question is negligible.

The mechanical behaviour of real materials is diverse and complex and it would be impossible, even if it were desirable, to formulate equations which are capable of determining the stress in a body under all circumstances. Rather, we seek to establish equations which describe the most important feature of the behaviour of a material in a given situation. Such equations can be regarded as defining ideal materials. It is unlikely that any real material will conform exactly to any such mathematical model, but if the ideal material is well chosen its behaviour may give an excellent approximation to that of the real material which it models. The model should be selected with the application as well as the material in mind, and the same real material may be represented by different ideal materials in different circumstances. For example the theory of incompressible fluids gives an excellent description of the behaviour of water flowing through pipes, but it is useless for the study of the propagation of sound waves through water, because for the sound-wave propagation a model that takes into account the compressibility of water is essential (Spencer, 1980, pp.104–105).

Smith’s denial of the existence of stock models and constitutive equations in CCM may derive from the fact that the Lindsay text is an advanced and highly abstract textbook. It can thus assume the reader is already familiar with the stock models of continuum mechanics. It can also assume that the reader is familiar with the stock models of both particle and fluid mechanics, which it encompasses.

Let us be clear in closing this section exactly what we take to be the selection criteria for stock models. Bridge principles associate a stock model with a theoretical description, so the stock models are the ones that appear in bridge principles of the theory. Their admissibility of the bridge principles is determined in the same way as that of any theoretical principle. Different methodologists have different views about what makes a principle admissible and we would like to stay neutral about that for our purposes here. We do at least though want to stress that empirical confirmation is crucial (with all the usual caveats that no principle is confirmed in isolation, etc.).

Beyond admissibility, we can enquire about *usefulness*. Here it is important to consider both ends of the bridge principle. The principle will be of little use if we do not have some independent ways of deciding if the model in it fits a given situation. Think back to our discussion of air and water. If we have no idea when air or water can be described as a 'Newtonian viscous fluid' then having a bridge principle that tells us that $\sigma_{ij} = -p\delta_{ij} + D_{ijkl} V_{kl}$ is a Newtonian viscous fluid will not be of much use. At the other end, the principle will be of little use if the theoretical description is not specific enough to allow us to do calculations. This happens for instance if the theoretical description has system specific constants in it that we do not know how to evaluate, or if the functional form is not specified but only loosely constrained.

The two features, admissibility and usability, are not unrelated though. For if a principle is not very usable, either because we do not have good cues about where it applies in the world or because it does not give a specific enough theoretical description of the situations to which it applies, it will be equally difficult to confirm. And we reiterate: in our view theories can only be taken to stretch as far as their well-confirmed principles can take them.

6 Phenomenological models

Finally, we should like to address a possible misunderstanding – Smith's discussion suggests the demand that the domain of a theory be determined by the range of the stock models may put a stranglehold on development of theory.

Smith's construction of Cartwright's argument seems to be the following,

- All proper science appeals to a small set of stock models, which provides the pool of legitimate models that fix the domain of the theory.
- We can find these models in a canonical text like Lindsay's or look to scientific use.
- Phenomenological models cannot number among the stock pool, which means that all legitimate science is given by pre-articulated theories.

These features allow Smith to conclude,

- If no legitimate models come via phenomenological considerations, then none of science can count as legitimate since, "Every model used by classical mechanics is merely phenomenological" (Smith, 2001, p.469).
- The dappled view then is too restrictive.

Smith's criticisms are helpful in that they direct attention to the need to explain what makes a model legitimate. However Smith's supposition that phenomenological models cannot be – or cannot become – stock models for our arguments to succeed seems mistaken.

Few philosophers of science would be willing to suggest that all legitimate scientific modelling proceeds only from established theories. Certainly this is no part of the view defended here. Cartwright, Showmar and Suarez (1995), for example, argue explicitly that, "a theory-driven view of models can not account for common procedures used by scientists to model phenomena" (p.142). Using the example of the London brothers' pre-quantum model of superconductivity in the 1930s, they argue that phenomenological considerations generate important instances of scientific model construction. That model provided an equation defining the domain of superconductivity that, "greatly influenced the development of theoretical treatments of superconductivity for very many years afterwards" (ibid). Existing theory had not been able to account for the Meissner effect, which is "the sudden expulsion of magnetic flux from a superconductor when cooled below its transition temperature" (ibid, p.144). The London brothers made dramatic ad hoc corrections to the existing electromagnetic model to account for this phenomenon. The resulting model came to be a stock model for a superconducting material in electromagnetic theory at the time (defining the domain of superconductivity), and it was generated by phenomenological considerations.

The distinction between phenomenological and theoretical models is intended to indicate that stock models do not simply derive from existing scientific theory: they are also legitimated by phenomenological considerations. However presenting the distinction this way is too strong to account for the development of actual model construction in science. As Margaret Morrison (1999) has suggested, there are never strictly theoretical or phenomenological models, but all have elements of both. This seems to be in line with the intent of Cartwright et al. (2005), who write,

> Our scientific understanding and its corresponding image of the world is encoded as much in our instruments, our mathematical techniques, our methods of approximation, the shape of our laboratories, and the pattern of industrial developments as in our scientific theories. (p.138)

Cartwright et al. certainly do not advocate a view of science that holds all legitimate modelling derives from established theory. Successful models often incorporate both theoretical and phenomenological considerations. We must be careful, though, about what ‘success’ means in these claims and what follows from it. The London model was successful in that it accounted for the phenomena – and for a while it was the standard model. But it involved the use of functional forms for the electromagnetic field that were not at the time licensed by bridge principles from a description of a superconducting material that could be assigned by independent means. The description of superconductors as ferramagnets, as the functional forms suggested, were ad hoc. So in this case the theory was shown at best to accommodate superconductivity, not to ‘predict’ it. Correlatively, the ‘success’ of the model did not count for much in defending the truth of the theory. So models involving phenomenological elements can be very successful at accommodating phenomena, as well as at a variety of other tasks. But these successes are no indication of the extent of the warranted claims of the theory.

Nor does the view here imply that theory sets its stock models as static. Sometimes we learn that the same model works repeatedly for the same kind of situation. In that case a new bridge principle can be added to the theory, thus expanding the set of ‘stock models’. The London model seems a good example in that it became included among the stock models of condensed matter physics. Thus through successful application a phenomenological model can come to be principled. There is nothing about the view here suggesting that theories are not revisable through scientific practice and the success of a phenomenological model can be a good source of suggestion for changes to the theory.

7 Conclusion

Smith proposes three bases for stock models. Considering his third suggestion first, one need not hold that stock models must be derived from existing theory.[6] This contradicts Smith’s claim that according to the view defended here legitimate science proceeds exclusively from a set of well-articulated theories. Second, Smith is right to suggest that the proper place to look is to scientific use: there is no guide to principle except successful practice. Stock models are those that appear in the bridge (or ‘constitutive’) principles of the theory. So the question of what legitimates a stock model is really a question of what legitimates a bridge principle. And we have stressed one sine qua non: bridge principles (constitutive principles) are principles of theory. They at least must be empirically well confirmed by the repeated success of predictions from the theory that use the principle in an essential way. Successful modelling provides a pool of stock models. These can be generated through a mix of phenomenological and theoretical considerations, but once a model becomes stock, it gives principled applications of relevant functions of a specified scope. It can affect, moreover, the structure of the theoretical backdrop itself, which is not unrevisable. Third, any reference to a source like Lindsay’s text merely indicates one place we might look to find what our stock models are.

Our argument does not deny that CCM can encompass point-particle mechanics and fluid dynamics. Rather, we wish to point out that in moving to the super-theory,

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[6] Though they of course become part of the theory since the constitutive and bridge principles are essential to it.

the situation does not change substantially. In both cases, we have laws that involve abstract concepts. In the case of Newtonian mechanics the concept in question is force; in the case of CCM, the concepts are those of stress and contact force. The situation with respect to these abstract concepts is not substantially different in CCM than in Newtonian mechanics. In CCM, as in Newtonian mechanics, we apply the abstract concepts of the theory (stress tensor, contact force) by means of stock models (Newtonian viscous fluid, non-viscous fluid) and the associated bridge principles. The range of its stock models used in proper successful prediction fixes the domain over which the theory describes. And so far as we can see there is little positive evidence that the stock models of any one theory can cover all the causes that make an object move.

Smith takes himself to be showing that classical physics has a much larger domain than our view suggests. But his worry is misplaced: the domain of physics is as wide as the successful application of its models demonstrates. This will remain the same in the picture we defend here as it does in Smith's unity picture. An important difference however is that the *dappled view* avoids any pious hope that the successes of the scientific enterprise can somehow warrant claims beyond the scope determined by those successes.

We began this paper noting that Neurath proposed a conception of science that resonates with the view defended here. Of course, Neurath dealt with questions that were pressing in his own time. His target, in general, was metaphysics, understood by him to be unexplicated notions that misleadingly were taken to provide a scientific understanding of our world. Our focus is distinct though connected. The unity Smith argues for indicates a successful unification of scientific endeavours under the abstract principle of a single theory, CCM. However, that practical unification does not warrant application beyond the domains in which we have had empirical success. Like Neurath, we argue that unity does not give us a nice single set of principles from which to interpret the ambiguity of our world. Unification comes from the empirical successes of science. And the warrant we are accorded does not go beyond the domains of that success. Neurath's bill blows in the wind and it may or may not be governed by CCM or by any future theory that we have strong empirical reason to hold true.

8 Sources

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