# Structural Accounts of Mathematical Representation

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# Abstract

Attempts to solve the problems of the applicability of mathematics have generally originated from the acceptance of a particular mathematical ontology. In this thesis I argue that a proper approach to solving these problems comes from an 'application first' approach. If one attempts to form the problems and answer them from a position that is agnostic towards mathematical ontology, the difficulties surrounding these problems fall away. I argue that there are nine problems that require answering, and that the problems of representation are the most interesting questions to answer. The applied metaphysical problem can be answered by structural relations, which are adopted as the starting point for accounts of representation. The majority of the thesis concerns arguing in favour of structural accounts of representation, in particular deciding between the Inferential Conception of the Applicability of Mathematics and Pincock's Mapping Account. Through the case study of the rainbow, I argue that the Inferential Conception is the more viable account. It is capable of answering all of the problems of the applicability of mathematics, while the methodology adopted by Pincock trivialises the answer it can supply to the vital question of how the faithfulness and usefulness of representations are related.

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# 1 | Introduction

The relationship between mathematics and science is one of the most active areas of research in contemporary philosophy of science. The issues discussed range from the general, such as whether mathematics is indispensable to science, to the specific, such as whether prime numbers are explanatory. This thesis will look at the general problem of the applicability of mathematics. This problem can be initially characterised as the question "how is mathematics applicable to the world?" A little reflection, however, shows that this initial characterisation is seriously underdeveloped; what is meant by "applicable" is vague. One might be asking how a particular ontology of mathematics can be related to the world. Alternatively, one might be wondering whether mathematics is able to explain physical phenomena. Steiner (1998) noticed the ambiguity in this question and attempted to resolve it through philosophical analysis. He argued that one could distinguish several different problems that fall under the general problem of the applicability of mathematics, and that confusing and conflating these problems leads to difficulty in providing adequate answers to the issues.

I will argue (in Chapter 2) that part of the difficulty in answering these questions originates in the adoption of an 'ontology first' approach to the questions. It has typically been the case that philosophers have a particular ontology of mathematics in mind when they set out what they consider to be 'the' problem (or problems) of applicability. For example, a Platonist about mathematics is often challenged to provide an account of how abstract acausal objects are related to the world, such that they can be used to gain knowledge about the world. That is, to explain the role of mathematics in science. Alternatively, nominalists are challenged to provide an account of how false statements can be used to gain knowledge. I reject this approach and argue in favour of an 'applications first' approach: one should attempt to answer the problems of the applicability of mathematics in an ontologically neutral fashion.

My aim for this thesis is to identify a philosophical account of the applicability of mathematics that will answer the relevant problems. My approach to identifying such an account will be to properly distinguish the numerous problems that fall under the umbrella of 'the applicability of mathematics', argue that some of these problems depend on answers to others, and show how my favoured account can answer these problems. Specifically, I will argue that the 'applied metaphysical problem' should be answered first. This problem asks for a link between the mathematical and physical domains and little more. I will also argue that the more interesting and difficult question to answer is the problem of representation, the question of how mathematics is able to represent the world. Several of the problems of applicability can be understood as being (sub)problems of the problem of representation: issues related to misrepresentation, and the issue of how multiple (sometimes conflicting) interpretations of the same mathematics can each be partially successful. As I consider the problems of representation to be the most difficult of the problems to solve, the majority of this thesis will focus on issues related to scientific representation.

In Chapter 2 I will set out nine distinct problems that fall under the umbrella of 'the applicability of mathematics'. These nine problems are:

1. The pure metaphysical problem.

2. The applied metaphysical problem.

3. The problems of representation:

   3.a) The problem of representation.

   3.b) The problems due to misrepresentation.

   3.c) The multiple interpretations problem.

4. The novel predictions problem.

5. The problem of isolated mathematics.

6. The semantic problem.

7. The mathematical explanation problem.

I will follow Steiner's (1998) and Pincock's (2012) development and analysis of these problems. I will argue that the pure metaphysical problem should be answered by philosophers of mathematics, and that the semantic and mathematical explanation problems require far more work to be undertaken before they can be answered. As such, I will not answer these problems in this thesis. I will answer the applied metaphysical problem by appealing to structural relations. The main motivation for this answer is that the bar is set very low for a satisfactory answer to the applied metaphysical problem, but not for an account of representation. An adequate account of mathematical representation will have to fulfil certain criteria (discussed in Chapter 3). These criteria will dictate what will be required of the representation relation, which relates the mathematical and physical domains. As such, the representation relation is (in part) the answer to the applied metaphysical question.

I will conduct my investigation of representation in Chapter 3. There I will argue in favour of structural accounts of scientific representation. The chapter is structured around the question of whether there is a unique feature of scientific representation. I will adopt the position that representation is an activity that has a purpose, and that the purpose can set criteria for the representation relation. I will argue that scientific representation is most likely what Contessa defines as partially faithful epistemic representation. A partially faithful epistemic representation is one where our representational vehicle allows us to draw at least one sound surrogative inference about the representational target. An advantage of adopting structural accounts of representation is that we are able to use the (formal) notions of structural similarity to ground the notion of partial faithfulness. This, in turn, allows us to explain why representational vehicles which purposefully misrepresent their targets can be useful. In Chapter 4 I will set out two structural accounts of representation: the Inferential Conception of the Applicability of Mathematics,[1] and Pincock's most recent Mapping Account.[2] In addition to introducing these accounts in this chapter, I will also investigate how they fit into Contessa's approach towards representation, which involves distinguish epistemic and faithful epistemic representation.[3] I will argue that Pincock's Mapping Account can be understood as an account of epistemic representation, but the Inferential Conception cannot. However, I will sketch how both can be understood as accounts of faithful epistemic representation. I will only provide a sketch of this claim in Chapter 4, because establishing whether either account succeeds as an account of faithful epistemic representation takes a lot of careful, detailed argument centred around the relationship between the faithfulness and usefulness of representations. This argument will be undertaken in Chapters 6 and 7.

Before I conduct the investigation into the success of the accounts, I set out my central case study, the rainbow, in Chapter 5. In this chapter I will outline the core mathematical ideas behind three representations of the rainbow: the

[1] See Bueno & Colyvan (2011), Bueno & French (2011, 2012).

[2] Pincock (2012).

[3] Epistemic representation is characterised as a representation that is used to learn something about its target. It consists in the drawing of valid surrogative inferences. Faithful epistemic representations concerns the soundness of those surrogative inferences. See Contessa (2007, 2011).

ray theoretic representation; the Mie representation; and the Complex Angular Moment (CAM) approach. These representations all involve various types of idealisations, abstractions and mathematical techniques. Furthermore, they are related to each other in interesting ways. With an understanding of the rainbow in hand, I will turn to the arguments concerning the viability of the Inferential Conception and Pincock's Mapping Account being accounts of faithful epistemic representation. I will address each account individually. My approach to each account will be the same: I will establish whether the account can use the same structural resources to accommodate different types of idealisation; and whether the account provides an adequate explanation for how the faithfulness and usefulness of representations are related given the answer to the first issue.[4] During the analysis of the two accounts, I will employ the $\beta \to \infty$ singular limit, which is used to obtain a ray representation from wave theory, and the abstract mathematics involved in the CAM approach. These features of the representations of the rainbow, what role they play, how they are obtained, and so on will form crucial test cases for the conclusions I draw in answering the above two questions.

In Chapter 6 I will discuss the Inferential Conception. I will identify partial structures and the associated notion of partial truth as being capable of providing an explanation of how partially faithful representations can be useful, and that the same structural resources can be used for Galilean and singular limit idealisations. However, I will identify issues concerning how idealisations and the notion of 'surplus structure' should be accommodated on the Inferential Conception. As the Inferential Conception is part of the partial structures version of the Semantic View, other work on this topic can be appealed to. I will identify a dilemma generated by the attempt to accommodate surplus

[4] I will focus on two different types of idealisation: Galilean and singular limit idealisations. See McMullin (1985) for Galilean idealisations, Batterman (2001) for singular limit idealisations. See also Weisberg (2007, 2013) for a different way of distinguishing these types of idealisation.

structure on the Inferential Conception. I will employ the $\beta \to \infty$ limit to test the conclusions concerning idealisations and surplus structure. I will use the mathematics of the CAM approach to test the solutions to the dilemma. In doing so I generate a further problem due to the possible indispensability of ray theoretic concepts in the creation of the CAM approach models. I will argue that a possible solution to this problem is to regard the Inferential Conception as a third 'axis' of the Semantic View, to compliment the horizontal axis of inter-theory relations and the vertical axis of data, phenomena and abstract model relations. The Inferential Conception passes these tests, though further work is required to flesh out the solutions.

I will discuss Pincock's Mapping Account in Chapter 7. I will argue that it requires different structural resources to accommodate Galilean idealisations compared to singular limit idealisations. Pincock argues for understanding some instances of singular limit idealisations in terms of singular perturbations. As part of this argument, he advocates an interpretative position of 'metaphysical agnosticism' towards these perturbations, rejecting instrumentalist and metaphysical (i.e. emergentist or reductionist) positions. The Mapping Account appears to be capable of accommodating the relationship between faithfulness and usefulness for Galilean idealisations, through the use of the specification relation and structural relation that relate the vehicle to the target. I will find the case to be less clear for the singular limit (perturbation) idealisations. I will argue that Pincock's arguments in favour of his metaphysical agnosticism are lacking, and his exposition of the position is unclear. I will employ the $\beta \to \infty$ idealisation and CAM approach in an attempt to better understand metaphysical agnosticism, but I will conclude that it is either too undeveloped a position to currently hold, or that it collapses into a form of reductionism.

I will decide between the Inferential Conception and Pincock's Mapping

Account in Chapter 8. To choose between the two accounts I will draw on the conclusions of the previous chapters (6 and 7) to answer the relevant problems of applicability from Chapter 2. These are the problem of isolated mathematics, the novel predictions and the multiple interpretations problem. Answering the problem of representation and problems due to misrepresentation (specifically the issues raises by abstraction and idealisation) requires explaining how an account of representation can provide faithful epistemic representation; as such, the conclusion to Chapter 7 means that I will not able to immediately answer these problems for Pincock's Mapping Account. I will be able to answer them for the Inferential Conception. Before concluding in favour of the Inferential Conception, however, I will conduct a short investigation into the methodology adopted by Pincock to construct his account. I will draw on the analogous argument between French and Brading & Landry over the role of the philosopher of science. The root of their disagreement is a rejection by Brading & Landry of the 'meta-level' that French claims the philosopher of science operates at. I will argue that Pincock's position is similar to Brading & Landry's, such that he blurs the distinction between the meta-level and the object level. This results in a trivialisation of the relationship between the faithfulness and usefulness of representations. Due to this trivialisation, I will reject Pincock's Account, and adopt the Inferential Conception as the most plausible account of representation.

# 2 | The Philosophical Problems of the Applicability of Mathematics

## 2.1 Introduction: What is the Applicability of Mathematics?

In this chapter I will argue that the problem of the applicability of mathematics is really a collection of problems, such as the pure and applied metaphysical problems, the problems of representation and the mathematical explanation problem. I argue that the applied metaphysical problem, the problems of representation, the problem of novel predictions and the problem of isolated mathematics to be the problems we should aim to answer first. This chapter is concerned with explicating these problems, and justifying why I think these problems are the ones which should be answered first.

I will present two examples of how mathematics can be applied to the physical world: the use of the $SU(3)$ group to classify the heavy baryons and predict the quark model; and Dirac's relativistic wave equation and the prediction of the positron. I will use these examples to motivate some intuitive worries about how applications work. I will then turn to one of the most important works on the applicability of mathematics, Steiner's *The Applicability* *of Mathematics as a Philosophical Problem*[1] and contrast the intuitive worries generated by the examples to the problems identified by Steiner. Some agreement is found, though I will also argue against the way Steiner forms some of his problems (mostly in line with Pincock's criticisms (Pincock 2012, §8)). This is due in part to the ontology first approach Steiner has adopted: Steiner argues for a Fregean ontology of mathematics, which he accepts due to Frege's solution to the semantic problem that arises due to number terms acting as predicates in some contexts and singular terms in others (Steiner 1998, pg. 16-17).[2] I will end the chapter by arguing that the applied metaphysical problem is straightforwardly answered by structural relation accounts, and that the more interesting problems are those concerning how mathematics is capable of representing the world.

The general approach to the applicability of mathematics has been to assume a particular ontology of mathematics and then attempt to explain how that ontology either gets around problems that are due to that particular ontology[3] or does not actually suffer from them.[4] I take this approach to be a consequence of indispensability arguments for mathematics. These arguments claim that we should believe in those entities that are indispensable to our best scientific theories and that mathematical entities are such objects, therefore we should believe in the existence of mathematical entities.[5] Pincock (2004b) argues that indispensability arguments rest on a misunderstanding of how math-

[1]Steiner (1998).

[2]From this Fregean ontology, Steiner goes on to argue that the problems of applicability have consequences for the rationality of pursuing naturalistic approaches to science. I will not assess the arguments and claims concerning naturalistic approaches to science as there is already a body of literature which argues against the conclusions Steiner draws against naturalism. See Simons (2001), Liston (2000), Kattau (2001), Bangu (2006) and Pincock (2012). I will however introduce the arguments in passing, as what Steiner has to say about the "Pythagorisation" of prediction is tied to how he sets up problems related to novel prediction and "the unreasonable effectiveness of mathematics" Wigner (1960).

[3]e.g. How are the non-spatiotemporal, acausal abstract objects of mathematical Platonism related to the world in an informative way?

[4]e.g. That some anti-realist positions hold mathematical statements to be literally false is not a problem for accounting for the applicability of mathematics

[5]See Colyvan (2001).

ematics can be applied to the world and so draw unwarranted conclusions.[6] This disagreement over how applications work and what the indispensability argument shows, allows one to disagree with Colyvan over whether applications dictate mathematical ontology. I agree with Pincock's evaluation of these arguments and buy into his project of attempting to understand applications of mathematics before attempting to draw any ontological commitments.

This line of argument is similar to the one adopted by Yablo (2005). He argues that from a Fregean perspective towards mathematics (i.e. that it is a science of abstract, *sui generis* objects) applicability can be seen as either a *datum*, where "the question is, what *lessons* are to be drawn from it?", or "as a *puzzle*, and the question is, what explains it, how does it work?" (Yablo 2005, pg. 98). Yablo argues that the datum perspective is usually given priority, which leads to the following line of argument (Yablo 2005, pg. 89-90):

> [that] since applicability would be a miracle if the mathematics involved were not true, it is evidence that mathematics is true. The ... applicability [of mathematics] is [then] explained in part by truth. It is admitted, of course, that truth is not the full explanation. But the assumption appears to be that any further considerations will be specific to the mathematics involved and the application. The most that can be said in general about why mathematics applies is that it is true.

Attention then turns to questions of what makes mathematical claims true, that is, what the appropriate ontology of mathematics is (Platonic forms, structures, etc.). Yablo then proceeds to turn these perspectives around, and attempts to address the *puzzle* perspective of applicability first.

I can draw some support from other anti-realists for this approach, in that they provide accounts of how to understand applicability without reference to mathematical entities. Their motivations for constructing such arguments are

[6] Pincock argues that Colyvan's indispensability argument only establishes the semantic realism of applied mathematics - that such statements must have a truth value.

obvious, though different to mine. I am appealing to anti-realist arguments that proceed in two stages after putting forward the hypothesis that mathematical entities do not exist. The anti-realists first have to show how applications can be understood without reference to mathematical entities. Second, they argue from such an understanding of applications that mathematical entities do not exist. For example, Maddy's Arealist (similar to a Formalist), argues that the existence of abstract objects adds little to the utility of applied mathematics (Maddy 2007, pg. 380-381); and Bueno (2005) argues that mathematics can play a heuristic role and that it is the physics which is important in applications.[7] Further work has to be done from these positions to argue that mathematical entities themselves do not exist, as they are all compatible with mathematical entities either existing or not. I only require the first part of these arguments as they demonstrate that positing mathematical entities is *not necessary for understanding applications of mathematics*.[8]

I therefore conclude that the best approach to explaining how mathematics is applicable is to adopt accounts that only provide answers to what I will call the applied metaphysical problem, and remain neutral with respect to what I will call the pure metaphysical problem. Thus I will endorse answers to the applied metaphysical problem which are "independent of pure mathematics" (Pincock 2004a, pg. 130). I call this approach the 'application first' approach, to contrast with the previous approaches, which I call 'ontology first' approaches.

I also appeal to scientific practice as support for the 'applicability first'

[7]Bueno's position towards mathematics is variable. He offers a first step towards a constructive empiricist approach to mathematics in his (1999), which would consist in arguing for an agnostic attitude towards mathematical entities. In his (2009) he argues in favour of mathematical Fictionalism.

[8]I have not appealed to Field here as I follow Yablo's argument that just as the applicability of mathematics can be see as an argument or a problem, so too can indispensability, and that Field focuses on indispensability rather than applicability (Yablo 2005, pg. 92). The question that Yablo and I am concerned with is "how are actual applications to be understood, be the objects indispensable or not?"

approach. Applications of mathematics by scientists do not, in general, make any reference to the ontology of the mathematics used.[9] This is the case in the examples below. This suggests that applications of mathematics do not dictate that a side has to be taken on the realism/anti-realism debate in mathematics. As this claim is in direct opposition to Colyvan's position, remember that Pincock rejects Colyvan's indispensability argument, arguing that it only establishes semantic realism about mathematics. Pincock claims that Colyvan's argument should be reformulated with a weaker premise of "apparent reference to mathematical entities is indispensable to our best scientific theories" (Pincock 2004b, pg. 68). I therefore suggest (but will not argue) that applications, at most, restrict the range of possible (particular) mathematical ontologies. That is, while applications may rule out individual theories (Platonic forms or neo-Aristotelianism for example), they do not force one to be a realist (or anti-realist) about mathematics. Of course, all anti-realist strategies might be ruled out. However, such a situation would be a failure of anti-realist accounts individually as opposed to anti-realism being categorically ruled out. With regards to mathematical ontology, the mappings will require that the mathematical domain displays certain (structural) features. This restrictive move is similar to that taken by Bueno (2009), who argues that an ontology of mathematics must fulfil certain criteria to be viable, explicitly discussing the applicability of mathematics.

Now I will outline the two examples of applied mathematics: the application of the $SU(3)$ group to quarks; and the Dirac equation and the prediction of the positron.

[9]Colyvan challenges this point via the example of the imaginary number $i$. He argues that mathematicians did not accept it as a number until after Gauss made use of it in his proof of the fundamental theorems of algebra and physical applications for complex function theory were found (Colyvan 2002, pg. 104-105). I tentatively suggest that this shows that scientists are unconcerned about the ontological status, given that applications had to be found *before* $i$ was accepted by mathematicians.

## 2.2 Examples of Applied Mathematics

### 2.2.1 $SU(3)$ and Sub-Atomic Physics

In the 1950s, there were a large number of new, supposedly fundamental, particles being discovered. Besides the proton, neutron and electron, particle physicists were aware of the pions ($\pi^+, \pi^0, \pi-$), kaons ($K^+, K^0, K^-$) and eta ($\eta$) mesons, and the lambda ($\Lambda$), delta ($\Delta^{++}, \Delta^+, \Delta^0, \Delta^-$), sigma ($\Sigma^+, \Sigma^0, \Sigma^-$) and xi ($\Xi^+, \Xi^-$) baryons. Establishing which decay modes were allowable and which were not was a major problem caused by this wide range of particles. A similar problem had arisen in the 1930s with the proton, when the question was asked as to why it did not decay. A solution was found by requiring a new property to be conserved, the baryon number. These new particles were proposed to display a further new property, that of 'strangeness'.[10]

An attempt to categorise these new particles was made by Gell-Mann. He started by looking at isospin, an approximate symmetry that can be found between protons and neutrons.[11] The idealisation used to construct isospin allowed for various calculations and predictions to be simplified. Gell-Mann attempted to generalise the isospin strategy to the new particles by searching for symmetries for the strong, weak and electromagnetic forces (Gell-Mann 1987, pg. 483-489).

Isospin demonstrates the roles that idealisation and abstraction play in science and hint at how mathematics facilitates these processes. Following the distinction drawn by Jones, an abstraction is the omission of detail by means

[10] The particles were considered 'strange' as they had a short creation time and a (relatively) slow decay time, indicating a different process was involved in their creation to their decay. In fact, strong interactions (the interaction that holds the nucleus together) conserve strangeness and are responsible for the creation of strange particles, whereas weak interactions (the interaction responsible for beta decay) are responsible for the decay of the particles and do not converse strangeness.

[11] It was observed that these two particles are almost identical if their charges are ignored. It was therefore proposed to consider them as two different states of the same particle, a 'nucleon'.

of saying nothing about it, while idealisations are the inclusion of a falsehood about a system (Jones 2005, pg. 175). In the isospin case, the equating of the masses of the proton and neutron is an idealisation, while the lack of detail on the charge of the neutron or proton is an abstraction. We need to account for how mathematics is able to provide the tools for these moves and for how making the moves can be justified.

It was pointed out to Gell-Mann that the algebra he was using is actually the Lie algebra of the group $SU(3)$. A group is a set of elements together with an operator, such that the operator combines any two elements into a third element.[12] Groups can be represented by matrices. The matrices used to represent a group can be generated from the Lie algebra. Arranging the nine known heavy baryons ($\Delta, \Sigma^*, \Xi^*$, where an excited state is notified by a * superscript) according to their strangeness and charge as in Figure 2.1 produces a geometry which is almost identical to one found in a particular representation of the $SU(3)$ group. This representation gives a decuplet, an arrangement of 10 points. Due to experimental results which allowed this representation to be made, Gell-Mann was able to predict the existence of a $10^{th}$ heavy baryon, the $\Omega^-$, and its mass (Gell-Mann 1987, pg. 492). The light baryons form an octet when plotted in the same way, while the mesons produce a hexagon, each of which is identical to the geometry of other representations of $SU(3)$.

The prediction of the $\Omega^-$ is a case of novel prediction, a key test for a new theory. There are two features of this prediction that are of particular interest and involve the use of mathematics. First, the prediction is not made in the usual way as being the result of anomalous interactions that require explanation.[13] Rather, the $\Omega^-$ was "read off" from the mathematics. Second,

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[12] See Georgi (1999) Chapter 1 for details of what constitutes a group and Chapters 7 and 9 for information on $SU(3)$.

[13] See Bangu (2006) and §2.3.3 below for more on this claim.

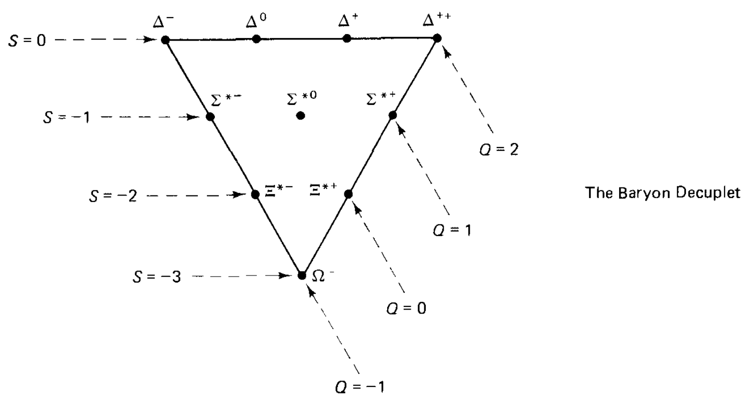


Figure 2.1: Charge verses Strangeness plot of the heavy Baryons, including $\Omega^-$. Reproduced from Griffiths (1987).

the mathematics that is used to produce the prediction is highly abstract. $SU(3)$ does not describe physical rotations as is the case with other groups. For example $SU(2)$ can be used to describe angular momentum symmetry. All symmetries that are found in a $SU(n)$ group occur in some $m$-dimensional space, the dimension of which is determined by the following equation: $m = n^2 - 1$. Therefore, $SU(2)$ rotations occur in a 3-dimensional space, whereas $SU(3)$ rotations occur in an 8-dimensional space. This raises the question as to why a piece of mathematics that describes rotations in an 8-dimensional space is appropriate for describing a collection of particles. How are we able to generate novel predictions by operations on such abstract mathematics?

The fundamental representation of the $SU(3)$ group produces the geometry of a equilateral triangle. This representation allowed Gell-Mann to draw the inference that hadrons could be understood as being composed of a combination of three fundamental parts. He did not originally consider these particles to be 'real', only 'mathematical', in that he did not think it was possible to observe the parts in isolation and so would be permanently confined within hadrons (Gell-Mann 1987, pg. 493). These parts are the foundation of the quark theory; specifically, they are the light quarks: up, down, and strange. Quarks have fractional charge, fractional baryon number and the strange quark carries a strange quantum number of −1. Baryons are composed of a combination of three quarks or antiquarks, while mesons are composed of a quark-antiquark pair.

$SU(3)$'s contribution towards quark theory highlights a further role that mathematics appears to be able to play, that of explanation. It seems that answers to the questions raised above can be found in the mathematics itself; that the symmetries of the physical system being described are completely reproduced in the symmetries of $SU(3)$ (given some abstractions and idealisations). The mathematics is therefore able to "explain" why we observe particular hadrons (only certain combinations of quarks are allowable, or only certain symmetries can be obtained with 3 fundamental constituents, for example).

### 2.2.2 Dirac and the Positron

Dirac's prediction of the positron was not a straightforward affair. A negative energy solution was obtained from his relativistic wave equation. This solution required interpretation within Quantum Mechanics. It was interpreted several times, as several of the interpretations failed tests of empirical adequacy. I will summarise the derivation of the Dirac equation and the relevant result. Then I will briefly discuss the inadequate interpretations and the one that predicted the positron as we now understand it.

The starting point for Dirac's attempts to find a relativistic quantum theory was the time dependent Schrödinger wave equation, (2.1). The Schrödinger equation needs to be manipulated to work with relativistic energies, masses and momentums. I will now present the derivation of the Dirac equation for a free particle of spin $\frac{1}{2}$.[14]

[14] The derivation is taken from Rae (2008).

$$-i\hbar\frac{\partial\Psi}{\partial t} = \hat{H}\Psi \tag{2.1}$$

$$E^2 = p^2c^2 + m^2c^4 \tag{2.2}$$

Assume that the energy operator, $\hat{H}$, can be expressed in terms of the momentum operator, $\hat{P}$, in the same way as the energy, $E$, and momentum, $p$, are in the classical limit, i.e. as in (2.2), to give (2.3). This gives an operator that is the square root of another operator. There is no clear way of how to handle this situation, but to preserve Lorentz invariance, we need the position and time coordinates to be of the same order in a relativistic equation. The left hand side is linear in time, so the right hand side must have linear position and time coordinates, as in (2.4).

$$-i\hbar\frac{\partial\Psi}{\partial t} = \sqrt{[\hat{P}^2c^2 + m^2c^4]}\Psi \tag{2.3}$$

$$-i\hbar\frac{\partial\Psi}{\partial t} = [c\gamma_1\hat{P}_x + c\gamma_2\hat{P}_y + c\gamma_3\hat{P}_z + \gamma_0 mc^2]\Psi \tag{2.4}$$

In order to resolve the equality and the relations required of the $\gamma_i$ coefficients, the coefficients cannot be scalar numbers; the most simple expressions for them are a set of 4 by 4 matrices. The final Dirac equation is given by (2.5). The use of matrices in the Dirac equation requires that the two component wave function, $\Psi$, of the Schrödinger equation is replaced with a four element “spinor", or column matrix, $\psi$, as in (2.6).

$$i\hbar\gamma_\mu\partial_\mu\psi - mc\psi = 0 \tag{2.5}$$

$$\psi = \begin{pmatrix} \psi_1 \\ \psi_2 \\ \psi_3 \\ \psi_4 \end{pmatrix} = \begin{pmatrix} \psi_A \\ \psi_B \end{pmatrix} \tag{2.6}$$

The classical relativistic equation (2.2), has solutions for negative energy values, less than $-mc^2$. These are rejected as being unphysical (as are positive values over $mc^2$) in classical mechanics. Such a move cannot be made in Quantum Mechanics however, as transitions to negative energy states are in principle achievable (Rae 2008, pg. 255). $\psi_B$ is the negative energy solution for the Dirac equation and is the result from which the positron is ultimately predicted.

The first interpretation of this solution by Dirac was an attempt to resist a negative energy particle interpretation. He proposed that they were states already filled by electrons. This is the famous Dirac sea of negative energy electrons. Transitions to these states would then be prevented by the Pauli exclusion principle.[15] When one of these sea electrons is excited by a photon with enough energy to move it to one of the positive energy states, a negative energy state is left vacant - described as a 'hole'. The excited electron behaves as a normal electron would. The electron sea and hole, however, behave differently. The total energy of the sea has increased (by the value of the photon minus the mass of the electron), making their net momentum $-p$, where $p$ is the momentum of the excited electron. This momentum behaves as an electron with a positive charge would in an electric field.

[15]The Pauli exclusion principle states that no two fermions may occupy the same quantum state simultaneously.

Despite several disagreements with experimental results, Dirac's equation was well supported (Kragh 1990, pg. 65). This was due in part to agreement with other formal results (Kragh 1990, pg. 63). The main complaints came from the interpretation of the negative energy states. While formal scattering results showed that the negative energy states had to be taken seriously (Kragh 1990, pg. 89), the interpretation of them as an unobservable sea of electrons was resisted. Pauli objected to the "zero charge" of the sea for example (Pais et al. 1998, pg. 52-53). Dirac attempted to interpret the hole as a particle, while maintaining the sea of negative states: first as a proton, then, due to objections by Heisenberg that this would require the mass of the proton and electron to be equated, as the positron (Kragh 1990, pg. 94, 103). Empirical confirmation came first from Anderson, who discovered a particle of similar mass to an electron but positively charged from cosmic ray interactions in a cloud chamber. Blackett and Occhialini confirmed Anderson's findings and explicitly identified the particle with that of the positively charged electron from Dirac's equation (Kragh 1990, pg. 109). Attempts at interpreting the Dirac equation as an empty vacuum state were led by Pauli and Weisskopf, and Oppenheimer and Furry, though their own theories were also considered inadequate (Kragh 1990, pg. 114). These theories were attempts to produce Quantum Field Theories with an empty vacuum state. We are now able to provide such an interpretation with modern Quantum Field Theories, such as Quantum Electron Dynamics. The Dirac equation is taken to describe the Dirac field, which has the (empty) vacuum as its ground state. The field is interpreted as being excited or de-excited which leads to the creation of electrons and positrons respectively.

The derivation of the relativistic wave equation is a normal application of mathematics. The prediction of the positron, however, is unusual. It raises questions over the nature of interpretation: how can one piece of mathematics have two very different interpretations yet both have some predictive success? This seems to imply that the mathematics refers to something in the world that the first interpretation did not recognise but the second did. The question of how mathematics can provide novel predictions is again raised.

I will use these two case studies to develop and distinguish some of the problems of the applicability of mathematics. Steiner also made use of these two case studies when he developed his set of problems, so they provide a common ground from which begin. The $SU(3)$ example is used by Steiner to generate his descriptive problem, which is motivated by the idea that mathematics has to be appropriate in some way for its application. I will argue, however, that the abstractions and idealisations involved in the development of isospin show that Steiner has missed several features of representation. I will subsequently argue that we should reject Steiner's descriptive problem in favour of the more nuanced problems of representation. I will also use the $SU(3)$ example, and other uses that $SU(3)$ has been put to, to clarify the problem of isolated mathematics, which concerns whether mathematics can be applicable if it was created or discovered in isolation from considerations of applicability. I will use the prediction of the positron to argue that Steiner has missed another problem of representation, that of multiple interpretations. Specifically, that the negative energy solutions could have useful but inconsistent multiple interpretations needs to be accommodated. Both case studies involve prediction; Steiner argues that the prediction of the $\Omega^-$ is a Pythagorean prediction, where the prediction involves reifying the mathematics. I will use these case studies to develop the problem of novel predictions, and later compare them to cases of typical prediction when answering these problems.

I will now move on to setting out what problems I believe an account of applicability must answer. I first argue that there is a separate metaphysical problem for applied mathematics than for pure mathematics. This is a problem initially identified by Steiner, and expanded by Pincock. I then argue for five more problems and contrast how I establish the problems with similar problems identified by Steiner.

## 2.3 The Development of the Philosophical Problems

In this section I will look at the problems posed by a statement of applied mathematics. The problems that I will outline are similar to those found by Steiner. I will follow Pincock's approach towards these questions, outlining how I will aim answer some of the problems and reasons for dismissing others. First, I will distinguish two types of metaphysical problem and argue that the applied metaphysical problem is the one which requires an answer in this thesis. Second, I will draw on the $SU(3)$ example in a discussion over Steiner's descriptive problem and conclude that the idealisations and abstractions involved in the example motivate an account of scientific representation.[16] Steiner's descriptive problem only touches on representation, however. Third, I will discuss novel predictions and Steiner's epistemological problem. This discussion will involve criticisms drawn from Bangu's arguments, the outlining of the novel predictions problem, and the forming of the problem of isolated mathematics from Steiner's comments on the historical origins of $SU(3)$. Finally, I set out the semantic and mathematical explanation problems, and argue that they

[16] In this chapter I only outline the problem of representation and the problem of misrepresentation due to abstractions and idealisations. In §3.3.1 - *The Argument from Misrepresentation* I will outline three further problems. These are: mistargeting due to mistaken target; mistargeting due to non-existent targets; and misrepresentation due to empirically inadequate results. In §3.3.1 - *The Argument from Misrepresentation* I solve the problem of mistargeting due to mistaken target and argue that empirically inadequate results are a general problem for philosophy of science. In §3.5 I argue for a solution to the problem of mistargeting due to non-existent targets. The problem of misrepresentation due to abstractions and idealisations is discussed throughout this thesis, in the context of establishing an account of faithful epistemic representation.

require too much work to be answered in this thesis.

### 2.3.1 The Metaphysical Problems

There are two types of metaphysical problem involved in the applicability of mathematics that need to be distinguished. The first is the problem of what mathematics refers to, asking if there are mathematical entities. I shall refer to it as the *pure metaphysical problem.* This is a question for philosophers of mathematics. I shall call the second problem the *applied metaphysical problem.* This problem arises because for "almost any interpretation of mathematics" there is a "gap between the subject matter ... and its applications" (Pincock 2012, pg. 172). For example, on Lewis' part-whole relation interpretation of mathematics, "the theory of hyperbolic differential equations is about some dispersed collection of mereological wholes and this intricately structured entity seems just as extrinsic to fluid systems as any platonic entity". This problem was originally formulated by Steiner as being specific to Platonism about mathematics. He held that there was a 'metaphysical gap' that "blocks any nontrivial relation between mathematical and physical objects, contradicting physics which presupposes such relations" (Steiner 1998, pg. 20). His complaint rests upon an argument that because science admits only causal and spatiotemporal relations, and abstract entities do not enter into such relationships, on the Platonic view, physical theories are false (Steiner 1998, pg. 21). I follow Pincock, however, in taking the applied metaphysical problem to be a problem for (almost) all interpretations of mathematics.[17] As it is such a general problem, I take it to be independent of the ontology of mathematics.

These problems can be characterised as asking different questions about the mathematical domain. The pure metaphysical problem asks what *inhabits*

[17] The one interpretation that might avoid this problem would be a nominalist one that identifies equations with the physical systems they are describing. In such a situation hyperbolic differential equations *are* about fluid systems, if they're about anything at all.

the domain, while the applied metaphysical problem asks how the mathematical domain is *related* to the physical world, *irrespective of what inhabits the mathematical domain.* Solutions to the applied metaphysical problem should therefore not dictate what mathematical entities are. That is not to say that the solutions will be completely silent on what inhabits to the domain, however. Depending on how the relations are understood, they will set certain limits on what can inhabit the mathematical domain. For example, if one insisted that for a relation to exist, that relation's relata must exist then Fictionalist accounts of mathematics would be *prima facíe* ruled out.

Above, I endorsed an 'applications first' approach to the problems of applicability, rather than the 'ontology first' approach most commonly found in the literature. This 'applications first' approach means that I should attempt to answer the applied metaphysical problem prior to settling the pure metaphysical problem. In this thesis I will only be answering the applied metaphysical question; as I said above, the pure metaphysical problem is one for philosophers of mathematics, and my answer to the applied metaphysical problem should be as silent as possible on the pure metaphysical problem.

### 2.3.2 Representation & The Descriptive Problem

Next, I turn to Steiner's descriptive problem. I will argue that Steiner has been lead astray by his Fregean ontology, and that he has posed a badly formed question that, understood properly, concerns mathematics' ability to represent the world. I then set out some of the subproblems of the problems of representation: issues related to misrepresentation (due to abstraction and idealisation), and multiple interpretations. More specific concerns with representation are set out in the next chapter, where I argue for adopting structural relation accounts of representation. The discussion here leads into the next problem Steiner discusses, the problem of novel prediction.

**Steiner's Descriptive Problem**

Steiner's descriptive problem can be loosely phrased as asking for an explanation as to why certain mathematics is capable of describing certain physical phenomena. Initially this seems very similar to asking how a certain piece of mathematics is able to represent a physical situation. However, as will become clear, Steiner is not working with a notion of 'description' that can be understood as representation, due to the way his Fregean ontology causes him to phrase his descriptive problem. Steiner's solution to his descriptive problem is to attempt to "[explain] in nominalistic language" why a piece of mathematics is appropriate to describe a situation. The explanation Steiner provides consists in attempting to pair the mathematical concept with a physical concept. Steiner's descriptive problem originates in his adoption of the Fregean ontology; as such, the applications first approach allows one to dismiss the problem as being badly formed. However, I will set out his problem, and Pincock's response, as it is a good example of how the ontology first approach causes unnecessary complications that the application first approach can avoid.

The $SU(3)$ example established an intuitive worry that is similar to Steiner's descriptive problem. The question was asked why the 8 dimensional rotations described by $SU(3)$ were appropriate for describing the hadrons. Steiner argues for a way of assuaging this worry in his account of how the application of $SU(3)$ should be understood. The solution comes from $SU(3)$ providing an account of how the hadrons came to be built out of quarks. The quarks are 'found' from $SU(3)$ as follows (stated in general terms): "The group $SU(n)$ has two representations of $n$ dimensions. The eigenvectors of the first can be taken to be the state vectors of the $n$ quarks. The second is conjugate to the first, and its eigenvectors represent the antiquarks" (Hendry & Lichtenberg 1978, pg. 1716). This means that, as $SU(3)$ has a representation of 3 dimensions, the number of eigenvectors of the first representation, 3, gives the number of quarks, 3. These eigenvectors are written thus: up: $\begin{pmatrix}1\\0\\0\end{pmatrix}$; down: $\begin{pmatrix}0\\1\\0\end{pmatrix}$; strange: $\begin{pmatrix}0\\0\\1\end{pmatrix}$. The matching is between these eigenvectors, mathematical concepts, and the concept of the quarks, which is linked to the physical objects themselves.

$SU(3)$'s success in answering the descriptive question at first appears to be uncontroversial. An objection can be raised which, although it can be answered in this case, can be generalised to call into question Steiner's framing of the descriptive problem. The objection runs as follows: $SU(3)$ has only one application, and that is to the light quarks. What if $SU(3)$ is found to have another application far removed from quarks? The matching then cannot be between the eigenvectors and the quark concept. The reply to this is that $SU(3)$'s success is not due to the quarks *themselves*, but that there are 3 objects which display the symmetries that are found in $SU(3)$. Any subsequent application of $SU(3)$ would have to be to 3 objects which display these same symmetries.

The above objection to $SU(3)$ can be generalised thus: "why is it that a single piece of mathematics must be associated to the same physical concept in every application?" Steiner has not argued for this being the case other than requiring it to be a feature of a successful answer. Pincock takes up this issue with Steiner: "he is assuming that when a given mathematical concept functions descriptively in two representations, this concept must be describing the same physical property in both cases" (Pincock 2012, pg. 177). Pincock shows that applications of the same piece of mathematics do not always involve the same features of the mathematics, never mind involving a consistent, single, physical concept across all applications.[18]

Pincock has shown, at the very least, that Steiner requires an argument for his presupposition that each applied piece of mathematics requires a *single*

[18] He makes use of examples that show that superposition does not require a superposition of causes and that different features of analytic functions are used in different applications of these functions (Pincock 2012, pg. 177).

physical property consistent to all its applications. I agree with Pincock that "the way Steiner frames his descriptive question ... makes it harder to answer than it should be and suggests a mystery where none exists". The crux of this objection is a disagreement with Steiner's understanding of 'description'. He seems to confuse application with description, in that any application of mathematics is 'descriptive' of some physical feature, whereas this is just not the case (Pincock 2012, pg. 177). Rather, description should be understood as representation. Representation is, loosely, taken to be the use of mathematics to 'describe' and gain information, about the world.[19] There are several key features of representation that Steiner does not address. Chief among these are idealisation and abstraction. As I will now argue, accounting for idealisation and abstraction is another problem that should fall under the umbrella of the problems of the applicability of mathematics: it is one of the problems of mathematical representation.

### Idealisation & Abstraction

Gell-Mann's starting point for classifying the light hadrons was isospin. As summarised above, the origin of isospin involved both idealisation and abstraction. How mathematics allows one to perform these actions needs to be accounted for. I claim that any account of applicability (i.e. the answer to the applied metaphysical problem) is too simple to handle idealisation and abstraction, and that such accounts will need to be expanded to take account of scientific representation.[20]

Abstraction is the omission of detail. This is demonstrated by isospin not including details of the charge of the neutrons or protons. Idealisation involves including known falsehoods in one's description[21] of a situation. In the isospin

[19] I hold out against using the jargon of representation until the next chapter.

[20] This is in part because idealisation and abstraction are examples of misrepresentation. Other instances of misrepresentation are discussed in §3.3.1.

[21] I use 'description' here without intending to commit myself to any particular view

case this occurs when treating the mass of the neutrons and protons as being of equal value.

The way that mathematics can facilitate an act of abstraction is easy to understand: where an equation with 10 variables accurately describes every feature of a situation, including less variables constitutes abstraction. There is no obvious way in which one can provide a mathematical explanation for why, or which, variables are dropped. These reasons are external to the mathematics, such as what is considered relevant, or not relevant, to the representation, and so can or cannot be omitted. They depend upon what we might call heuristic, pragmatic, and contextual considerations. These are things like the aims and goals of the scientist, what they consider acceptable accuracy, and so on.

Similarly, the reasons for introducing falsehoods into one's description of a situation depend upon heuristic, pragmatic and contextual considerations. For instance, establishing how to simplify a model or deciding what is a relevant feature of a system that requires idealisation (Jones 2005, pg. 187). There is a further question over idealisation, however: how is one able to gain useful information from the use of false descriptions?[22] In the case of isospin, the false equating of the neutron and proton masses allowed an approximate symmetry to be found. Isopsin, however, is now not recognised to be physically real. Any attempt to explain an application of mathematics that involves abstraction or idealisation will necessitate an account of scientific representation.

of how mathematics provides information about the world. i.e. I am not endorsing that mathematics literally describes the world, or a structure of the world, or that it represents in a particular way, etc.

[22]This is a question that proves central to my later analysis of accounts of representation. See chapters 6 and 7.

**Multiple Interpretations**

Dirac's prediction of the positron provides an example of where a single piece of mathematics, the negative energy states, was given multiple interpretations. They required some interpretation to either justify throwing them away (treating them as unphysical 'waste products' of obtaining a relativistic account of how electrons behave) or to keep them. In classical mechanics, negative energy solutions can be rejected as being non-physical and so are redundant. This cannot be done in Quantum Mechanics, so some sort of physical interpretation is required. Dirac originally interpreted them as the sea of negative energy particles. However, only the 'hole' received any sort of physical interpretation. We now understand the negative energy solutions as antiparticles, in this case the positron.[23]

When introducing this example in §2.2.2, I posed the question "how can one piece of mathematics have two very different interpretations yet both have some predictive success?" This question can be rephrased within the context of representation. It asks of an account of representation for an explanation as to how we can interpret a single piece of mathematics in physically very different ways, yet have predictive success with each interpretation. This falls to an account of representation as interpretation of the representation vehicle in terms of the target is a key part of an account of representation.

### 2.3.3 Novel Predictions

A feature of Dirac's prediction of the positron that was not discussed above is that the prediction was *novel*. That is, he was able to predict something new that previous versions of Quantum Mechanics did not. In this case, the positron and its behaviour. This problem has already been outlined as an

[23]Steiner only focuses on Dirac's prediction of the positron in the context of his argument that prediction has undergone a change in definition, so has nothing to say on this particular problem. Steiner's arguments concerning prediction are discussed in the next section, §2.3.3.

intuitive worry concerning the prediction of the $\Omega^-$ hadron via the use of $SU(3)$ and the prediction of the positron. Accounts of the applicability of mathematics need to explain how these novel predictions can be legitimately made. A challenge to the prediction of $\Omega^-$ comes from Steiner, who argues that it was achieved by 'Pythagorean' analogies, which are irrational for scientists who advocate a naturalistic methodology. I will follow Bangu (2008) in arguing that there is a difference between the prediction of the $\Omega^-$ and the positron,[24] outline Steiner's objection to the way that $\Omega^-$ was predicted, and then form the problem of novel predictions in a similar way to Bangu.

The standard philosophical framework for novel predictions is provided by Hempel's (1962; 1965) Deductive-Nomological (D-N) account, which states that predictions are derived from laws of nature and a set of initial conditions. Bangu argues that this framework of prediction can account for "existential" predictions (the prediction of some existing entity, as opposed to some physical phenomena that occurs) quite easily (Bangu 2008, pg. 245). The prediction of novel entities often arises as the result of some anomalous behaviour, as an explanation of that behaviour. In the case of the positron, for example, the negative energy solutions indicated some entity (first interpreted as the vacant state in the electron sea) would behave as an electron with positive charge. Once this behaviour was observed, further anomalous behaviour was noted and then "explained away" by the positron (Bangu 2008, pg. 247). This situation is different to the one surrounding the prediction of the $\Omega^-$. While one can construct anomalies that require explanation,[25] there is no mention of interaction in the prediction of $\Omega^-$. The argument proceeds along apparently Formalist lines that because the other spin-$\frac{3}{2}$ baryons behave in a way consis-

[24] French also highlights the difference by arguing that the D-N model is "too simplistic" to account for the prediction of the $\Omega^-$ (French 1999, pg. 202).

[25] Bangu points to the nine baryons that already fitted the decuplet requiring some explanation. The finding of a tenth spin-$\frac{3}{2}$ baryon would allow for such an explanation, the "law-like generalization H: 'Spin-$\frac{3}{2}$ baryons fit the symmetry scheme"' (Bangu 2008, pg. 247).

tent with $SU(3)$, there should be a tenth, the $\Omega^-$. The physicists "read off" the characteristics of $\Omega^-$ from the formalism, as opposed to having some empirical evidence of them due to interactions they had participated in (Bangu 2008, pg. 249).

This type of "reading off" prediction is described by Steiner as a 'Pythagorean' approach. He defines two such analogies (Steiner 1998, pg. 54):

> **Pythagorean:** a mathematical analogy between physical laws "not paraphrasable at $t$ into non-mathematical language" - that is, an analogy between two mathematically described laws.
>
> **Formalist:** "one based on the *syntax* or even orthography of the *language* or *notation* of physical theories, rather than what (if anything) it expresses" - that is, by the mathematical formalism used, irrespective of what it represents.

The reliance on Pythagorean analogies leads Steiner to argue that the meaning of 'prediction' has in fact changed: "Prediction today, particularly in fundamental physics, refers to the assumption that a phenomenon which is mathematically possible exists in reality - or can be realized physically", that the "concept of 'prediction' has itself become thoroughly Pythagoreanized" (Steiner 1998, pg. 161, 162). The idea is that the $\Omega^-$ was predicted due to a Formalist approach, where it is "read off" from the mathematical formalism. It is from this understanding of prediction that Steiner argues against the rationality of pursuing naturalistic approaches to science.

Bangu reconstructs Steiner's argument and identifies an additional premise in the prediction of the $\Omega^-$. He calls the additional premise the "Reification Principle" (RP) and claims that this is the Pythagorean basis of the prediction (Bangu 2008, pg. 248):

> If $\Gamma$ and $\Gamma'$ are elements of the mathematical formalism describing a physical context, and $\Gamma'$ is formally similar to $\Gamma$, then, if $\Gamma$ has a physical

> referent, $\Gamma'$ has a physical referent as well.

The reconstruction focuses on whether the prediction of the $\Omega^-$, proceeding via the RP as opposed to interactions, can be accommodated by a "physical", i.e. naturalistic, methodology (Bangu 2008, pg. 250). One way that one might attempt to do so is to adopt a pluralism about methodology, where the RP is incorporated as a novel part of the methodology. This is the approach Bangu takes Steiner to be adopting. There is a difficulty in doing this, however, as the RP is held to be counter to a naturalistic approach to science for the reason that "there is no naturalistic explanation why RP could work, while the very idea of a higher-level correspondence between mathematical objects and reality reminds us of numerology, astrology, and other dubious practices involving reifying mathematics" (Bangu 2008, pg. 250). Bangu's characterisation of the naturalist has them rejecting the RP as false, insisting that "there are countless situations in physics when the urged 'principle' just does not work. In addition, no one can explain (without appealing to some additional mystical verbiage) why such a principle works, even when it does." Further suspicion is cast on Steiner's approach and redefining of prediction by noting his project to argue against naturalism more generally (Bangu 2008, pg. 251). Given these problems, it appears that one should reject the Pythagoreanisation of prediction, at least in terms of "a systematic and all-encompassing methodological move in (particle) physics". However, doing so causes serious problems, as the D-N model of prediction cannot accommodate (as argued above) the $\Omega^-$ prediction. Bangu finishes his argument as follows:

> while the D-N model just does not work, the idea to go beyond it and adjust scientific methodology along Pythagorean lines (by taking the RP principle on board) is marred by serious conceptual problems. The acceptance of RP as a credible element of physical methodology implies not merely an innocuous re-definition of the concept of 'prediction', but

> the abandonment of the very idea of a naturalistic viewpoint about the world.

Bangu offers three possible responses to this worry, in an attempt to maintain a standard naturalist view point. These are (Bangu 2008, pg. 251-255):

i) The RP played a heuristic role in the prediction of the $\Omega^-$, by generating a hypothesis rather than proving the existence of the $\Omega^-$;

ii) The RP played a justificatory role in the prediction of the $\Omega^-$;

iii) Though the $\Omega^-$ was predicted due to the mathematics, it was done so through *interpreting* the mathematics. It is this interpretation that is significant.

Bangu rejects each of these in turn, then asks rhetorically whether the failure of these responses means that we should embrace the strict naturalistic methodology and reject the practice of physicists who use the RP as being unscientific. He holds that such a response would be unrealistic. He also refers to French's claims about the D-N model being too simplistic here,[26] suggesting that if this is the case, then we should reject it. Bangu then argues that the real problem is not the predictive practice itself, but rather (Bangu 2008, pg. 255):

> about understanding this practice. The problem presented here is thus not a direct challenge for the working physicists, but for the physicist-qua-methodologist; that is, a challenge for her ability to propose a systematic philosophical/methodological framework able to accommodate this episode. Consequently, our problem is an instance of a broader kind of concern: what is the most appropriate way to construe the relation between philosophical/methodological standards and scientific practice.

[26] French (1999), pg. 202.

Bangu's suggestion is to reject the strict naturalistic methodology in favour of what he calls "methodological opportunism" (Bangu 2008, pg. 256):

> By embracing this form of opportunism, the scientist can agree that the D-N model cannot account for these predictions, and she can also accept that the central role played by the mystical-sounding (RP) in this reasoning is at odds with other naturalist convictions scientists often endorse. Yet she believes that this form of opportunism offers her resources necessary to get over this tension; that is, she believes she is entitled to retain the freedom to ignore this kind of conceptual deadlocks and profit, opportunistically, from whatever might lead (even if unlikely) to scientific progress.

Given the above arguments of Bangu, the novel prediction problem can be seen to have several strands to it. First it asks for an explanation as to why Formalist/Pythagorean prediction is successful (if one is available)? (This could be understood as asking for whether an explanation of why the RP succeeds is available. Bangu's naturalist claims that such an explanation is not available.) Second, it asks whether we can provide a consistent methodological understanding of how novel predictions are made? (Whether we can have a consistent methodological treatment of the positron and $\Omega^-$ predictions, of D-N type and Formalist/Pythagorean type predictions.) Third, if we can have such a consistent methodological treatment, can it be one that conforms with the strict naturalistic view? Or must it be Bangu's methodological opportunism, or similar? Fourth, if we cannot have such a consistent methodological treatment, must we accept Steiner's pluralism, or similar?

### 2.3.4 Isolated Mathematics and Steiner's Epistemological Problem

The Pythagorean analogies introduced in the section above play a key role in Steiner's epistemological problem. He takes this problem to be the "unreasonable effectiveness of mathematics in the natural sciences" highlighted by Wigner (1960). In discussing the success of $SU(3)$ in predicting the quarks, given the use of Pythagorean analogies, Steiner states that:

> Mathematicians, not physicists, developed the $SU(3)$ concept, for reasons unconnected to particle physics. They were attempting to classify continuous groups, for their own sake.
>
> Because the $SU(3)$ story is not isolated, there are physicists who maintain that mathematical concepts *as a group*, considering their origin, are appropriate in physics far beyond expectation. ... It concerns the applicability of mathematics as such, not of this or that concept. ... It is the question raised by Eugene Wigner about the 'unreasonable effectiveness of mathematics in the natural sciences' Wigner (1960).

He then draws the following argument from Wigner (Steiner 1998, pg. 47):

(1) Mathematical concepts arise from the aesthetic impulse in humans.

(2) It is unreasonable to expect that what arises from the aesthetic impulse in humans should be significantly effective in physics.

(3) Nevertheless, a *significant* number of these concepts are *significantly* effective in physics.

(4) Hence, mathematical concepts are unreasonably effective in physics.

There are two threads the problem that I wish to draw out. First is the issue of what I call 'isolated' mathematics - mathematics that originated in isolation from considerations of applications. Steiner claims that $SU(3)$ is a piece of isolated mathematics. Second, the main force of Steiner's epistemological problem, and the anti-naturalist conclusions he draws from it, is the claim that what decides whether something is 'mathematical' or not - the *mathematical criteria* - is based on human aesthetic (and tractability) considerations. I will argue below that isolated mathematics, the first thread, is a problem for accounts of applicability. First I will survey Bangu's arguments that the problem of mathematical criteria, the second thread, is not an issue that needs to be answered by accounts of applicability, but by philosophers of mathematics. Consequently I will restrict the epistemological problem, and hence rename it, to the problem of isolated mathematics.

Steiner argues that the use of Pythagorean analogies should lead to a rejection of naturalism. To explain the apparent 'mystery' of mathematics being applicable at all, he concludes that there is some special connection between the human mind and the world. Bangu argues that Steiner's conclusion (cosmological anthropocentrism) rests upon two conditions (Bangu 2006, pg. 36):

> First, if there is a record of systematic employment of mathematically guided successful strategies in the invention of new theories, and no non-mathematical strategy is available. Second, if these successful, mathematically guided, strategies are indeed anthropocentric, but this holds only if mathematics is an anthropocentric concept.

Where mathematical anthropocentrism is defined as follows (Bangu 2006, pg. 35):

> Mathematical anthropocentrism is a view arrived at by examining how mathematics develops as a discipline and not by analyzing specific mathematical concepts applied in physics. In this context, 'anthropocentric' is a predicate that applies not to particular mathematical concepts, but to the nature of the criteria employed when concepts are added to the corpus of the discipline.

Bangu provides two examples[27] that show it is not the case in the history of mathematics that the mathematical criteria are exclusively anthropocentric,[28] thus Steiner's argument is unsound. I agree with Bangu that the problem of mathematica criteria is a problem for philosophers of mathematics; it does not fall under the umbrella of problems of the applicability of mathematics. As I am following the applications first approach, issues concerning what is or isn't mathematics are of no relevance to answers of the problems of the applicability of mathematics.

### Isolated Mathematics

The history of how mathematical concepts are discovered/created does not depend upon one's metaphysics. It is the fact that the mathematics originated for purely mathematical reasons, as opposed to being discovered or created for a specific physical situation that causes an apparent mystery. The reason that isolated mathematics is claimed to be problematic is that there appears to be no reason why it should allow any information to be found about the world on previous accounts of applicability. Compare such a piece of mathematics with the development of the calculus. This was developed in response to four problems according to Kline: establishing the velocity and acceleration of a particle at an instance; finding the tangent of a curve (and even establishing a global definition of tangent); finding the maximum and minimum value of functions; and the length of curves (Kline 1972, pg. 342-343). Solutions were found independently to these problems and some links had even been found between the solutions before the calculus, but the solutions lacked the generality provided by the work of Newton and Leibniz (Kline 1972, pg. 356). Given

[27] The debate between Euler and d'Alembert over what a function is (Bangu 2006, pg. 37-38), and over the Axiom of Choice (Bangu 2006, pg. 38-39).

[28] Some instances of establishing whether a concept is mathematical are explicitly anthropocentric. He quotes Zermelo, who "insists that the [Axiom of Choice] is '*necessary for science*' (Zermelo 1967, pg. 187)" (Bangu 2006, pg. 40).

the history of the calculus, one can see the difference between isolated mathematics and applied mathematics is that the applied mathematics is explicitly discovered or constructed in connection with a problem and so is formulated in a way conducive to being applied.

Whether a mathematical concept falls under the banner of 'isolated mathematics' is a matter of historical fact. Steiner claims that $SU(3)$ is a piece of isolated mathematics, but it is not as clear a case as Steiner presents it as being. There is evidence that group theory in *general* was developed before any applications: Maddy (2007) argues this, heavily relying on Kline (1972). However, Wigner adopted group theory to work on symmetry in crystals and French (2000) argues that the application of $SU(3)$ to the light hadrons was not a result of isolated mathematics. This is due to the work of Weyl and Wigner in the early $20^{th}$ century. They further developed group theory so that it could be applied to both the fundamental symmetry principles of Quantum Mechanics and various problems to provide simple (or even new) methods of calculation. There are other cases of isolated mathematics, which are of a less controversial nature. For example, the mathematics used to establish the tightest packing possible for spheres has been used to model modem signals and their associated noise as eight dimensional spheres.[29]

The problem of isolated mathematics is the requirement for an explanation of how mathematics that originated in isolation from any considerations of application can nevertheless be found to be applicable to physical situations.

This concludes the discussion of Steiner's problems that I will answer in this thesis. I will now turn to the semantic and mathematical explanation problems, and explain why they will not be answered. The semantic problem asks

[29]This is the most clear cut cases of isolated mathematics from seven examples found in Rowlett (2011). Some of the examples given by Rowlett come close to a related problem described by Maddy as one of 'transfer' (Maddy 2007, pg. 333). A solution to the isolated mathematics problem should also solves this 'transfer' problem.

for a consistent logical interpretation of statements that involve mathematics. The mathematical explanation problem asks whether mathematics contributes explanatorily to science, and if so, how it is capable of doing so.

### 2.3.5 The Semantic Problem

Accounts of applied mathematics have to make sense of arguments that involve both statements of pure mathematics and 'mixed statements' - statements that have both mathematical and physical terms in them. One such mixed statement is 'the satellite has a mass of 100kg'. This statement might occur in an argument over the force required to launch the satellite into orbit. The semantic problem can be immediately recognised from such arguments. The problem is the requirement for "a constant interpretation for all contexts - mixed and pure - in which numerical vocabulary appears" (Steiner 1998, pg. 16). The problem arises as in pure mathematical statements numerical terms appear to be singular terms that refer to numbers, while in mixed statements they appear to be predicates (in this case, about the mass of the satellite). Steiner's solution to the semantic problem is to follow Frege in taking all numerical terms to be singular terms (Steiner 1998, pg. 16-17).

Pincock holds that the mixed statement 'the satellite has a mass of 100kg' should be understood as follows: "the statement seems to be about the satellite, the real number 100, the standard gram (assuming that there is such a thing), and some complicated relation between them" (Pincock 2004a, pg. 139). While it seems that Pincock wishes to follow Steiner in taking all numerical terms to be singular terms, Pincock argues that the truth of mixed statements depends on the relation between the world and the mathematical domain, rather than the existence of numbers (Pincock 2004a, pg. 145). This implies that the interpretation of the numerical term is secondary to the relation between the physical world and the mathematical domain.

In his (2012) Pincock explicitly shies away from attempting to answer the semantic problem, due to difficulties related to interpreting natural language sentences and their relationship with scientific representation (Pincock 2012, pg. 171-172). This is partly because the semantic problem goes further than questions of how to interpret numerical terms. Applied mathematical arguments also include functions, algebra and other mathematical terms in their mixed statements. Establishing how to interpret all mathematical terms in a consistent way goes beyond what Pincock wishes to achieve in his (2012), and goes beyond what I intend to argue for in this thesis. I will therefore say no more on the semantic problem.

### 2.3.6 The Mathematical Explanation Problem

I have left the problem of mathematical explanation to last for two reasons. First, there is no single philosophical theory for all cases of explanation. Second, it is not settled whether mathematics can explain physical phenomena. This immediately leaves one unclear on how to frame the problem, and complicates what form an answer might take. I will briefly outline the key ideas and players in the debate over this issue, mostly by following Lange (2013). Due to the amount of work required to settle the two debates, however, I will not attempt to answer this problem in this thesis.

The typical starting point for a discussion concerning scientific explanation is the Deductive-Nomological (D-N) model of Hempel.[30] Here explanation is understood to be a deductive argument, where the explanandum-event is deduced (and therefore explained) from general laws and particular facts (the premises of the argument being the explanans). A D-N explanation has the following, syllogistic form (Hempel 1962, pg. 686):

[30]See Hempel (1962), Hempel & Oppenheim (1948)

$$\frac{\begin{array}{l} C_1, C_2, \ldots, C_k \\ L_1, L_2, \ldots, L_r \end{array}}{E}$$

Unfortunately, the D-N model counts various non-explanations as explanatory, e.g. a flag pole's shadow counting as an explanation of its height. It has been argued that the solution to this problem is to include causal relations in one's explanations. Lange points to several philosophers who hold the stronger view that all scientific explanations are causal explanations (Lange 2013, pg. 486-487).[31]

This view faces serious difficulty with accounting for mathematical explanations. It is commonly held that mathematical explanations, whatever they might consist in, make some appeal to a mathematical fact, feature or result.[32] The key consequence of this is that mathematical explanations are held to be non-causal. Any account of scientific explanation that is exclusively causal will thus rule out mathematical explanations in principle. Other possible accounts of explanation that might be capable of accommodating mathematical explanation include the unification account,[33] and abstract or structural explanations[34]

The debate concerning whether mathematics can explain physical phenom-

[31]Lange quotes Salmon (1977, 1984), Lewis (1986) and Sober (1984) amongst others as being in support of this claim. He also points to the more recent accounts of Strevens (2008) and Woodward & Hitchcock (2003) due to their emphasis on causal connections.

[32]Baker's (2005) appeal to the number theoretic theorem that prime periods minimise intersection compared to non-prime periods is an example of this. Lange's particular conception of mathematical explanations rejects this, arguing that distinctive mathematical explanations appeal to facts that are "modally stronger than ordinary causal laws" (Lange 2013, pg. 491). This disagreement between Baker and Lange is relevant to the idea that there is no clear idea of what constitutes a mathematical explanation, rather than the point I am making here.

[33]Kitcher (1981, 1989)

[34]Pincock's (2007) abstract explanations are called structural explanations by McMullin (1978). In his (2012), Pincock refers to the explanation of the bridges of Königsberg as mathematical explanation, due to being an abstract (acausal) representation. Batterman (2001, 2010) can also be understood as offering up a structural account of explanation.

ena has proceeded in a series of papers focusing on the indispensability argument.[35] The idea at the heart of the debate is that for mathematics to be indispensable such that we can conclude that numbers exist, it has to be indispensable *in the right way*, namely, explanatorily indispensable. The debate then proceeds by the identification of putative mathematical explanations of physical phenomena, such as the prime number life cycles of periodic cicada and the hexagonal shape of honeycombs. Arguments are then put forward in an attempt to determine whether the explanation really does depend on some piece of mathematics, a mathematical fact, etc. Recent work, such as Lange's, has attempted to gain a firmer grip on the notion of mathematical explanation, and identify what the putative explanations actually depend on to do their explanatory work (e.g. causal facts, laws, or in Lange's case, modally stronger facts).

In this section I have distinguished nine problems that fall under the banner of the problems of the applicability of mathematics. I've argued that three of these problems do not need to be answered in this thesis, and that six need to be. I will now answer the applied metaphysical problem and explain why the answer to this problem should serve as a basis for an answer to the other problems I will be answering.

## 2.4 Solving the Applied Metaphysical Problem

I have argued above that previous attempts to solve the problems of the applicability of mathematics have approached the issues from the wrong direction. Applications of mathematics by physicists do not generally involve considerations of ontology. These questions are secondary to the application; they

[35] These papers include: Melia (2000, 2002), Colyvan (2002), Baker (2005, 2009) and Saatsi (2007, 2011).

ask what the mathematical statements refer to, rather than how the applications work. I have argued for the problems above to be framed in ways which make as few assumptions about the ontology of mathematics as possible. Any solutions to these problems should be equally minimal in their ontological commitments, preferably making no commitments at all. The first step to solving these problems is to answer the applied metaphysical problem. An ideal answer should be one which forms the basis for an account of mathematical scientific representation, which is capable of solving the problems of representation and hopefully be extended to solve the other (semantic, mathematical explanation) problems. I believe that Structural Relation Accounts (SRAs) of applicability can do this. These accounts do not commit one to be a realist or anti-realist about mathematics, as, in general, these accounts are compatible with numerous realist and anti-realist positions.

Before arguing for the SRAs, I wish to make note of Pincock's analysis of Steiner's answer to the applied metaphysical problem. As Steiner endorses a Fregean ontology of mathematics, his answer to the applied metaphysical problem for counting is a one-to-one correspondence relation. This is extended to the rest of mathematics via adopting a set-theoretic approach where we can have impure sets, i.e. sets that contain physical objects as individuals (Steiner 1998, pg. 28). Pincock is rather unsatisfied with this as an answer to the applied metaphysical problem (Pincock 2012, pg. 173, my emphasis):

> If this set-theoretic solution is deemed adequate, then it shows how low the bar is set to solve the [applied] metaphysical problem. Steiner raises the issue by alluding to a gap between mathematics and the physical world. *A bridge across this gap need only show that mathematics is related in some way to the physical world.* But as Steiner's own set-theoretic response indicates, *there is no requirement that the bridge do anything else.* It need not illuminate what, as [Pincock has] continually put it, mathematics contributes to the success of science.

I agree with Pincock's analysis here: an answer to the applied metaphysical problem is straightforward. All it requires is a relation be provided between the world and a mathematical domain. The requirement that the "bridge" do anything else, i.e. restrictions on what type of relation might adequately relate the world and the mathematical domain, comes from the *other* problems of applicability. This is why the only restriction I place on an answer to the applied metaphysical problem be that it is capable of being extended to answer the other problems.

SRAs hold that the applicability of mathematics is due to structural relations between physical structures and mathematical structures. These structural relations are structure preserving: they take the structure of physical entities or properties to areas of mathematics that display a given level of structural similarity.[36] For example, when numbers are applied to state the length of some object, a structural relation will be involved. This relation will have to take the structure of the length property of the object to the relevant part of the real number structure. It will also have to include information on the length property in terms of its unit. e.g. "that the mapping takes all meter sticks to 1, all objects that are half as long as a meter stick to $\frac{1}{2}$, etc." (Pincock 2004a, pg. 147). There are various types of structural relations that may fulfil the role required by applications of mathematics; isomorphisms and homomorphisms are the most commonly appealed to structural relations.[37] Which structural relations are taken to constitute the representation relation

[36]The simple explanation given here is naïve. As rightly pointed out by Bueno and Colyvan, the parts of the world involved in our scientific theories are not always actual constituents of the world. They adopt a standard understanding of a structure as "set of objects (or nodes and positions) and a set of relations on these)" and remind us that "the world does not come equipped with a set of objects (or nodes or positions) and sets of relations on those. [(The view of structure above originates from Resnik (1997) and Shapiro (1997).)] These are either constructions of our theories of the world or identified by our theories of the world" (Bueno & Colyvan 2011, pg. 347). How theories are capable of carving up the world in successful ways will fall under an account of representation and so will be addressed in the next chapter.

[37]Loosely, an isomorphism maps all of the structure from the domain, $D$, to the codomain, $D'$. A homomorphism maps some of the structure of $D$ to $D'$.

is one of the main distinguishing features of the different SRAs.

The structural relations involved in these accounts are taken to be external relations. These are relations that do not "involve the criteria of identity of the mathematical objects" (Pincock 2004a, pg. 145). Admitting only external relations in applications of mathematics has the advantage of limiting the ontological commitments one is required to make in understanding mathematical applications. An internal relation is defined thus: "$a$ stands in an internal relation $R$ to $b$ just in case $aRb$'s obtaining is involved in $a$'s criteria of identity" (Pincock 2004a, pg. 140). Pincock argues that using internal relations in the hypothetical reasoning involved in science forces one to adopt modal realism (Pincock 2004a, pg. 143-144). Given that I am attempting to make as few ontological commitments about mathematics as possible I cannot justify making commitments about modality. Field's approach is described by Pincock as a 'no relation' account. As Field denies that there are mathematical entities there can be no relations between the physical world and mathematical entities. Pincock argues directly against Field's account, rather than no relation accounts in general. This begs the question as to whether no relation accounts are in principle compatible with structural relation accounts. Bueno and Colyvan hold this view, as they claim that Field *would* be able to make use of the Inferential Conception (Bueno & Colyvan 2011, pg. 368). I will not settle this debate here. I take it to be part of the work required to answer the pure metaphysical question to establish which mathematical ontologies are consistent with the solutions to the problems I argue for throughout this thesis. However I will commit to the claim that the structural relations are be external relations, as this maintains my goal of making minimal ontological commitments.

SRAs answer the applied metaphysical question as follows: the mathematical domain is related to the physical domain in virtue of a structural relation.

The world can be understood as conforming to a particular structure which can be found in the mathematical domain. Any piece of applied mathematics is applicable in virtue of this sharing of structure, via the structural relation. This is a very straightforward, and general answer. Notice that it does not explain how the structural relation is capable of identifying the structures, nor how any *particular* piece of mathematics is applicable to any *particular* piece of the world. The answer to these questions needs to be provided by an account of representation, as questions over a particular piece of the world having a particular piece of mathematics applied to it are actually questions over how a particular piece of mathematics is capable of representing that piece of the world.

## 2.5 Conclusion

In this chapter I have argued for nine individual problems that fall under the applicability of mathematics. I have employed structural relations to answer the applied metaphysical problem, with the promise that accounts that make use of such relations can be developed that can answer the problems of representation, the novel predictions problem and the isolated mathematics problem. Specifically, these accounts need to be developed into accounts of representation. But what are the requirements for a successful account of representation? Are these requirements the same for all forms of representation, or is there something unique about scientific representation? Can structural relations satisfy these conditions? These are some of the questions I introduce and begin to answer in the next chapter.

# 3 | Representation

## 3.1 Introduction

Representation is a very wide ranging concept. It appears not only in science, but also in art, psychology, cognitive science and linguistics. As such there are a huge number of issues involved with its philosophical analysis. The first, and perhaps most serious, of these issues is whether there is a fundamental representation relation, consistent across all types of representation. Possible answers to this raise further questions: if there is a fundamental relation, what distinguishes the various types of representation, if they can be distinguished at all? If there is not a fundamental relation, then how are we to understand the various activities that fall under the term 'representation'?

In this chapter I will highlight only a few of these issues, those that I take to be most pressing on scientific, and mathematical scientific, representation. I will follow the lines of argument set out in the literature in this regard. I will begin my investigation by focusing on the question of whether there is a "special problem of scientific representation", a question posed by Callender & Cohen (2006). By concentrating on this question I will be able to explore whether there is something unique to scientific representation that distinguishes it from putatively distinct types of representation, such as aesthetic representation, or whether there is a unified notion of representation. I will argue in line with Contessa (2007; 2011) that one can identify epistemic representation as a distinct form of representation, rejecting a unified notion of representation. One can establish this distinction by noting that one uses epistemic representation to gain information about a representational target (and so the presence of surrogative inferences are a distinguishing feature of epistemic representation), whereas aesthetic representation is used to convey aesthetic values (e.g. beauty, emotions, etc.).

In following Contessa's arguments, I reach a conclusion that is in part in agreement with Callender and Cohen: there is nothing special about scientific representation. However, it is in part in disagreement with Callender and Cohen, as I reject a uniform notion of representation; there might not be anything special about scientific representation, but this is compared to other instances of epistemic representation. Scientific representation is simply epistemic representation undertaken by scientists or where the target is the world. There is still a distinction between epistemic and other forms of representation. Contessa's arguments in favour of epistemic representation do not complete his project, however. He identifies the need for an account of *faithful* epistemic representation: representation where at least one of the surrogative inferences from the representational vehicle to the target is sound. I will argue that, in general, structural relation accounts of representation are the accounts that are best placed to provide such accounts. Outlining and explaining how Pincock's Mapping Account ((2012)) and the Inferential Conception (Bueno & Colyvan (2011), Bueno & French (2012)) can be understood as accounts of faithful epistemic representation will be started in the next chapter. Evaluating the ability of these two accounts to sufficiently explicate the relationship between how faithful a representational vehicle is and its usefulness constitutes the rest of the thesis.

## 3.2 What is Representation?

A first pass at analysing representation is "$X$ represents $Y$". This, however, is rather uninformative. We would like to know *how* $X$ represents $Y$. In virtue of what does $X$ represent $Y$? The first pass ignores a crucial element of representation, that it involves agents. It is not so much that "$X$ represents $Y$", but that an agent *uses* $X$ to represent $Y$. This is noted in the literature in a couple of slogans. van Fraassen points to the analogous example of Mead's[1] reflections on teacups: "If there were no people there would be no teacups, even if there were teacup-shaped objects. For 'there are teacups' implies that 'there are things used to drink tea from' which in turn implies 'there are tea-drinkers'." (van Fraassen 2010, pg. 25). Giere makes use of the slogan "no representation without representers" (Giere 2006, pg. 64, endnote 13). The idea behind these slogans is that representation is an intentional activity: an agent *uses* a entity to represent a target. This notion of use needs spelling out.

van Fraassen argues that 'use' encompasses many contextual factors, which prompts us to look at the practice of representation to properly analyse 'representation' (van Fraassen 2010, pg. 23). He argues that in some cases distortion is crucial to the success of representation, yet in others is a cause of misrepresentation (van Fraassen 2010, pg. 13-14). The important conclusion from this discussion is that representation is representation *as*. Our representations involve using a vehicle to represent the target *as* something, where that something is contextually dependent upon our use, the aims we are seeking to fulfil in adopting the representation. We can therefore expand our initial analysis of representation to: "$A$ uses $X$ to represent $Y$ as $F$";[2] or "$S$ uses $X$ to represent

[1] van Fraassen references McCarthy (1984) for this observation.

[2] van Fraassen initially presents a minimum schema of "$X$ represents $Y$ as $F$" and argues that this is the minimum form the schema takes (van Fraassen 2010, pg. 20). His idea is that it gets modified according to the use at hand.

$W$ for purpose $P$".[3]

I will argue below for the view that representation is an activity, where the success of this activity is dictated by the purpose involved. That is, the purpose to which someone adopts a representation dictates how successful that representation is. 'Purpose' and 'use' are cashed out contextually. I will make use of this to argue that the type of representation fills in some of the 'purpose' and 'use' criteria, and that this dictates what sort of representation relation should be adopted for particular types of representation.

While agents are necessarily involved in representation, the above schema do not make it clear how an agent should be involved in the representation relation. Should they be related via the representation relation to both vehicle and target? Should they be related by another relation to the only the vehicle, or only the target? The answer to this question varies from account to account, and so will be provided in the next chapter rather than here.

## 3.3 A Special Problem for Scientific Representation?

Whether the above schema of "$A$ uses $X$ to represent $Y$ as $F$" should be expanded or refined, and how, will depend on the answer to two questions. First, are there any special features or purposes of representation in general? Second, is there is some special feature or purpose of scientific representation specifically, that sets it apart from other forms of representation? Fortunately, attempting to answer the second of these questions will provide an answer to the first.

The title of this subsection comes from the title of a paper by Callender and Cohen (CC), who pose the problem specifically for scientific models. They

[3] Giere argues for this formulation. $S$ is an individual or group of scientists, or a scientific community, while $W$ is an "aspect of the real world" (Giere 2006, pg. 60).

ask how can models be about the world given that they can have features the world lacks, or lack features the world has. This is further complicated if it is right that models are not truth-apt (or approximately-truth-apt). Their paper distinguishes four questions that they take to be addressed by accounts of scientific representation, though they do not think that any particular account has addressed all four questions. These questions are (Callender & Cohen 2006, pg. 68-69):

**(CQ)** **Constitution Question**: What constitutes the representational relation between a model and the world?

**(DQ)** **Demarcation Question**: What is distinct about scientific representation?

**(NI)** **Normative Issue**: What is it for a representation to be correct?

**(EQ)** **Explanatory Question**: What makes some models explanatory?

CC introduce the NI in response to what Morrison calls the "heart of the problem of representation", namely the question "in virtue of *what* do models represent and how do we identify what constitutes a correct representation?" (Morrison 2008, pg. 70). They argue that there are two problems in this quotation: the 'in virtue of' question being the constitution question; and the 'correct representation' question broaching the normative issue. They further argue that proponents of the 'models as mediators' view[4] push too strongly on the idea that the "representational and explanatory capacities of a model are interconnected".[5] They agree with this claim if one understands the 'interconnection' to be due to the NI and EQ presupposing answers to the constitutive ones, the CQ and DQ, but no more than that (Callender & Cohen 2006, pg. 69).

[4]See Morrison (1999).

[5]Callender and Cohen point the reader to page 40 of Morrison's (1999) in support of this claim.

CC favour what I consider to be a unified account of representation. They argue that "the varied representational vehicles used in scientific settings ... represent their targets ... by virtue of the mental states of their makers/users" (Callender & Cohen 2006, pg. 75). All representation is to be reduced to mental representation. Thus there is no special problem of scientific representation as it is merely reduced to mental representation where the agents doing the representation are "scientists and their audiences are either fellow scientists or the world at large" (Callender & Cohen 2006, pg. 83).

Due to their adoption of this unified account of representation, CC argue that issues related to the misrepresentation involved in scientific models are not as important as the literature makes them out to be. They do this by appealing to examples from other types of representation (Callender & Cohen 2006, pg. 70). For example, they rhetorically ask whether linguists are concerned that "the marks 'cat' aren't furry or that cats lack constituents that are parts of an alphabet" in order to make the point that such questions are "bad". If one is making use of a unified account of representation, they argue, these questions should also be bad questions to ask of scientific representation. Their argument against taking misrepresentation due to abstraction, idealisation, and so on, seriously is that all forms of representation have something in common that make such questions inappropriate (perhaps even due to category mistakes - representations are not the sort of thing one should ask such questions of). Obviously a rejection of their unified account of representation, and the adoption of a non-unified notion of representation, leads to these issues having greater significance than CC take them to have, in accordance with the other literature on scientific representation. These issues are most relevant to the NI, though also play a role in establishing an adequate answer to the CQ (as the representation relation has to be able to account for misrepresentation in a satisfactory manner).

I agree with CC's position on the relationship between the questions: answers to the NI and EQ will depend upon the answers provided for the CQ and the DQ. I will therefore first provide answers to the CQ and DQ in this chapter, arguing that the answer to the CQ depends upon an answer to the DQ and against a unified account of representation in the process of this. Although I agree with CC's conclusion that there is nothing special about scientific representation, this is for different reasons, namely that scientific representation is epistemic representation performed by scientists. Thus the more interesting DQ is not what counts as scientific representation, but what counts as epistemic representation. The answer to the CQ then must be capable of providing epistemic representation. I will, in the later chapters, argue that the Inferential Conception and Pincock's Mapping Account are plausible accounts of faithful epistemic representation. An answer to the NI will be given in the process of establishing which, if either, of the accounts succeeds as an account of faithful epistemic representation. I will not address the EQ. In the context of mathematical scientific representation, I see this question to be asking the same as the mathematical explanation question of the previous chapter and so will leave it for the same reasons provided there.[6]

### 3.3.1 The Constitution Question

I take the Constitution Question to be the most important question to answer if one is attempting to establish a unified account of representation across all forms of representation. If one is able to show that all types of representation occur due to one type of relation then one has gone most of the way to providing a unified account of representation. Thus, if I wish to show that a unified account of representation is not possible, I merely have to find an example of representation that must proceed via a different type of relation to scientific

[6] See §2.3.6.

representation. One way of doing this is to pay attention to the purpose representations are put to. If a particular type of representation has a distinct purpose, this purpose supplies the answer to the DQ. Further, in order for the representation to be able to fulfil that purpose, the representation relation for different types of representation may have to be constituted by a different type of relation. That is, an answer to the CQ will depend on an answer to the DQ. The answer to the DQ will establish certain requirements that the representation has to fulfil to be a representation of that type. The representation relation must be capable of allowing the vehicle to fulfil these requirements. Thus if I am able to identify a unique feature of scientific representation, I hold that a unified account of representation is not possible.

My argument that scientific representation has a unique feature will proceed by first surveying arguments put forward against the similarity and structural relation accounts by Suárez and Frigg. I will begin with these arguments due to the conclusion of the previous chapter, that accounts of representation based on structural relations could be developed to answer the problems of representation, novel predictions and isolated mathematics. I will find these arguments to be successful against similarity accounts, but unsuccessful against structural relation accounts. This survey will also indicate that inferences are important to scientific representation. This will prompt me to switch focus to the DQ, in order to establish the role inferences play in scientific representation, namely whether they are a distinguishing feature of scientific representation.

There are three broad categories of accounts of scientific representation which supply different answers to the CQ: similarity; structural relation; and the Inferential Conception Approach. Giere (1988, 2004) argues for the representation relation to be one of "similarity" or "fit" between a model and the world. The structural relation approach includes those who propose the relation is some form of partial-morphism[7], Pincock who takes there to be other structural relations involved[8] and various other authors who pick a particular form of morphism.[9] Finally, there is Suárez' Inferential Conception Approach[10] which involves refraining from specifying any particular type of relation, but rather explicating conditions a particular type of relation would have to meet in under to be a representational relation.

In the literature the similarity and structural relation accounts are often held to fail for the same reasons. Suárez (1999, 2003) and Frigg (2002, 2006) provide the same sorts of arguments along these lines. In Suárez' case, these are clearly meant to show that superiority of his Inferential Conception Approach. I will follow the arguments of Suárez (2003) here.[11] Five arguments against the structural relation and similarity accounts are put forward:

**The Argument From Variety:** similarity & isomorphism do not apply to all representational devices;

**The Logical Argument:** similarity & isomorphism do not posses the logical properties of representation;

[7]Such as French (2003), Bueno & Colyvan (2011), Bueno & French (2011, 2012)

[8]Pincock (2012).

[9]For example Bartels (2006), who argues for using homomorphic mappings. I will not discuss Bartels' account, as I take it to collapse into a partial structures account and therefore favour the Inferential Conception. The major problem with Bartels' account is that the notion of homomorphism he employes is too rigid to be able to accommodate all cases of misrepresentation. Bartels is aware of this, and in attempting to fully accommodate misrepresentation he argues for a weakening in the conditions that hold for a homomorphism is occur (Bartels 2006, pg. 10). He then points to the partial structures framework as an example of a structural account which has adopted this kind of weakening (Bartels 2006, pg. 10, footnote 3). As Bartels' account is not developed further along the lines of the partial structures framework, it should be rejected in favour of the account that is, the Inferential Conception.

[10]Suárez (2003).

[11]Suárez offers his arguments against a specific version of the structural relation accounts, namely a simple isomorphism account: "$A$ represents $B$ if and only if the structure exemplified by $A$ is isomorphic to the structure exemplified by $B$" (Suárez 2003, pg. 227). He also offers them against a simple similarity account: "$A$ represents $B$ if and only if $A$ is similar to $B$". He does go on to argue that these arguments can be extended to the "weaker" and "amended" versions of the isomorphism account (Suárez 2003, §5-6). I respond briefly to Suárez' claims in §3.3.1 - *Further Arguments, and Amended & Weakened Versions.* These weaker and amended versions are still less sophisticated than the accounts I will endorse in the next chapter.

**The Argument From Misrepresentation:** similarity & isomorphism do not make room for the ubiquitous phenomena of mistargeting and/or inaccuracy;

**The Non-necessity Argument:** similarity & isomorphism are not necessary for representation - the relation of representation may obtain even if similarity & isomorphism fail.

**The Non-sufficiency Argument:** similarity & isomorphism are not sufficient for representation - the relation of representation may fail to obtain even if similarity & isomorphism hold.

The argument from variety can be avoided by proponents of the similarity and structural relation accounts in a legitimate way, according to Suárez, by leveraging a distinction he draws between the *means* of representation, and the *constituents* (Suárez 2003, pg. 230):

> **Means of representation:** At any time, the relation $R$ between $A$ and $B$ is the means of representation of $B$ by $A$ if and only if, at that time, $R$ is actively considered in an inquiry into the properties of $B$ by reasoning about $A$.
>
> **Constituents of representation:** The relation $R$ between $A$ and $B$ is the constituents of the representation of $B$ by $A$ if and only if $R$'s obtaining is necessary and sufficient for $A$ to represent $B$.

What Suárez is identifying by the 'means of representation' is the relation between $A$ and $B$ that is used in the drawing of a particular surrogative inference about the target (Suárez 2003, pg. 229).[12] There are many relations that exist between a representational vehicle and its target; one of these, say an isomorphism, might be useful for obtaining one set of surrogative inferences, while

[12] Suárez follows Swoyer (1991) in taking "the main purpose of representation [to be] surrogative reasoning". I will assume that this is the case while discussing Suárez' arguments, and will argue in favour of this being the case for scientific representations in §3.4.4.

another, say similarity (the sharing of properties), might be useful for obtaining some other set of inferences. While addressing the CQ, only arguments aimed at showing that similarity and isomorphism fail to be the constituents of representation need to be answered. The means of representation is relevant to the DQ; if structural relations cannot provide the means of scientific representation, then they will not satisfy any particular conditions of scientific representation that distinguish it from other forms of representation.

I will briefly outline these arguments below, and respond to them in accordance with the structural relation accounts in general (where possible). I take these arguments to be damaging to the similarity account of representation.[13] I will not outline the argument from variety due to my response to the DQ. Below, I will argue that the answer to the DQ sets limits on the CQ, specifically that the relation that provides the answer to the CQ has to be capable of providing surrogative inferences. Thus I reject Suárez' distinction that one can draw surrogative inferences from a different relation to the one that constitutes the representation relation. According to my arguments, for structural relations to constitute the representation relation, they have to be capable of providing the surrogative inferences in all cases of scientific representation, hence the distinction collapses, and the variety argument is implicitly answered.

### The Logical Argument

A representation relation is held to have a particular set of logical properties, specifically that it is asymmetric, non-reflexive and non-transitive. Similarity is reflexive and symmetric, while isomorphism is reflexive, symmetric and

[13] I do not mean to deny that one is capable of responding to these arguments from the position of a similarity account. Rather, due to the focus of this thesis, I take these arguments and the responses from the structural relation accounts to be sufficient for me to adopt the structural accounts from this point on. As a result, I will not provide any responses in favour of similarity accounts.

transitive.

Structural relation accounts have various options open to them. Suárez' argument is aimed at isomorphisms, so any structural relation account that can make use of either partial isomorphisms or other morphisms than isomorphisms (such as homomorphisms) should be able to provide the asymmetric and non-transitive properties. The non-reflexive property is more challenging to provide for the partial isomorphism account, as partial structures can be partially isomorphic to themselves. However, if one considers the domain as a whole, there are other factors at play that break the symmetry (that is, explain why partial isomorphisms are not symmetrical) (Bueno & French 2012, pg. 887).

### The Argument from Misrepresentation

This argument comes in two kinds, each of which involves two further types of misrepresentation. The first is mistargeting. This occurs when there is a problem with the relationship between the target and vehicle. The first type, the form of mistargeting I will call 'mistaken target', occurs when we "mistakenly suppose the target of a representation to be something that it actually does not represent" (Suárez 2003, pg. 233). The second type occurs when the target of the representation does not exist. The second sort of misrepresentation is the phenomenon of inaccuracy. This can occur either due to representations leading to empirically inadequate results, or when a representation includes abstractions and idealisations. Misrepresentation due to empirically inadequate results is not a specific problem for accounts of representation. Rather it is a general problem for philosophy of science. It is related to issues concerning measurement, accuracy, and other practical aspects of science. The claim is that the source of these inaccuracies is not the representation, but the information fed into the representation: the data, the experimental results, etc. Any source of inaccuracy due to the representation itself will fall under the banner of abstraction and idealisation.

The idea behind the abstraction and idealisation type of inaccuracy is that the representation vehicle does not accurately represent the target due to omitting details (abstraction) or due to claiming something that is not true of the target (idealisation). Suárez argues that similarity accounts can account for abstraction and idealisation as such accounts only require the sharing of some properties between target and vehicle. He also argues that the simple isomorphic version of structural accounts cannot account for this kind of misrepresentation, as any difference between target and vehicle would result in there being no isomorphism between the target and vehicle. Fortunately, there are structural relation accounts that make use of other structural relations than isomorphisms and so can potentially avoid this problem. I will address this type of misrepresentation in more detail in chapters 6 and 7, focusing on how these accounts can explain the relationship between the faithfulness and usefulness of representations, which is a crucial part of understanding how these accounts fit into Contessa's approach.

As an example of mistaken target misrepresentation, Suárez asks us to imagine a friend dressed up as the subject of a painting, such as a painting of Pope Innocent X. Here, we might mistake the painting to be representing our friend, rather than the actual Pope Innocent X. Suárez states that the main reason for the misrepresentation is the ignorance of "the history and the *true target* of the representation" (my emphasis) (Suárez 2003, pg. 234). But this isn't simply a statement that the right causal history has to be in place for a vehicle to represent its target, as, for Suárez, the existence of a relation between vehicle and target does not produce a representation. Rather the representation has to be *used* by an agent, and the agent must be capable of using it: "the skill and activity required to bring about the experience of seeing-in (the appreciation by an agent of the 'representational' quality of a source), is not a consequence of the relation of representation but a condition for it". The point Suárez is attempting to make is that due to this essential involvement of the agent in establishing a representation, the mere existence of a relation (whether one of similarity or isomorphism) is not sufficient for a representation, nor is it sufficient for explaining why a representation *fails*. The explanation for the failure here is that the agent has only used extant relations of similarity (or isomorphisms) between the painting and their friend to establish the target of the painting, when the agent should have also paid attention to other relations between the painting and potential targets to establish what the painting is a representation of.

The above example is for artistic representation; Suárez also provides one for scientific representation. He points to the specific case of a mathematician providing a solution to a equation, which the mathematician is unaware is the quantum state diffusion equation. The general point Suárez is attempting to make is that on the isomorphism account, when "a [mathematician discovers] a certain new mathematical structure", if this new mathematical structure is "isomorphic to a particular phenomenon [this] would amount to the discovery of a representation of the phenomenon - independently of whether the mathematical structure is ever actually applied by anyone to the phenomenon" (Suárez 2003, pg. 243). This should not be the case: our intuition is that the mathematical structure only becomes a representation when it is applied to the quantum phenomenon.

There are two problems with this example as a case of mistaken target. The first problem concerns the way Suárez has introduced the isomorphism and similarity accounts as attempts to 'naturalise' representation. The second is due to a disanalogy between the artistic and scientific examples. The point that a structural relation is insufficient for representation is admitted by proponents of actual structural relation accounts.[14] Yet critics of these accounts often make the mistake of claiming that structural accounts *do* hold the existence of structural relations between targets and putative vehicles to be sufficient. This mistake is based on the belief that structural relation accounts attempt to 'naturalise' representation. Suárez states this belief as follows (Suárez 2003, pg. 226):

> One sense in which we may naturalize a concept is by reducing it to facts, and thus showing how it does not in any essential way depend upon agent's purposes or value judgements (Putnam (2002), van Fraassen (2002)). The two theories that [Suárez criticises] are naturalistic in this sense, since whether or not representation obtains depends on facts about the world and does not in any way answer to the personal purposes, views or interests of enquirers.

Given the number of times that proponents of the structural relations accounts discuss pragmatic, context and heuristic concerns and the role of agents' intentions in representation this claim can be seen to not only be false, but also destructive to the debate. By claiming that the isomorphic type accounts hold this view, Suárez is arguing against a straw man position.

The disanalogy between the painting example and the quantum state diffusion equation originates in a rejection of the straw man version of the isomorphism account. In the state diffusion case, Suárez argues (incorrectly) that structural relation accounts hold there to be a representation due to the isomorphism between the solution the mathematician produces and the phenomenon, irrespective of agents using the solution as a representation. The 'mistargeting' here is that the solution is not targeted at anything, yet it appears as though it should be on the (straw man) isomorphism account. In the painting case, the mistargeting is due to incorrectly identifying the target

[14] e.g. Bueno & French (2012).

of an actual representing vehicle. The painting already has a target, but the agent has mistakenly identified the target. By rejecting the straw man version of the isomorphism account, we see that there is no target for the new solution to be related to. There is no targeting occurring, never mind a case of mistargeting. A better example would be a case where we had a solution to the state diffusion equation obtained by a physicist for one particle, but we mistakenly use the solution to describe a second particle. Here the solution is being used as a representation, and so there is a target and another putative target to mistakenly identify. What these two examples, spelt out in this way, shows, however, is that the cause of the mistargeting in the mistaken target case is the agent using the representation. They choose the wrong target. The structural or similarity relations might contribute to this, but as these relations are not held to be sufficient for representation, the error lies in another part of the account (i.e. the intention of the agent who is using the representation). This argument shows that the cause is not the structural relation, and so the problem of mistaken target is not a special problem for structural accounts of representation. The argument also shows that the fault in such cases lies with the agents and their use of the representation vehicles. This solves the problem.

The final type of mistargeting occurs when the target does not actually exist. This has occurred frequently in the history of science. Examples include phlogiston and the mechanical ether. Initially this seems like a large problem. It asks the question of how we can hold there to be a representation of a target, when that target does not exist? I will answer this question below, in §3.5, in response to Chakravartty's discussion of the issue.

### The Non-necessity & Non-sufficiency Arguments

As we have seen above, the straw man version of the isomorphism account claims that the existence of an isomorphism between vehicle and target establishes a representation. Thus any extant isomorphism is sufficient for representation. What is missing in this case, according to Suárez, is an intention for the vehicle to represent the target. Non-sufficiency is clearly accepted by structural relation proponents: for example Bueno and French admit that this is the case, and claim that other factors combine with the structural relation to be jointly sufficient, where these factors are "broadly pragmatic having to do with the use to which we put the relevant models [representations]" (Bueno & French 2012, pg. 887).[15]

With respect to the non-necessity argument, anyone who admits a non-uniform account of representation (in general) will be able to accept the non-necessity of any type of relation to establishing representation. Perhaps Suárez' point could be restricted to just scientific representations. This might be what Suárez intends given that he advocates abandoning the search for "universal necessary and sufficient conditions that are met in each and every concrete real instance of scientific representations" (Suárez 2004, pg. 771). In response to this charge, advocates of structural relation accounts and similarity accounts admit the necessity of their preferred relation. The argument is straightforward: without such relations, the success of scientific representations would be a miracle.[16]

[15] A similar move is available to the similarity account. As I explained above, Giere argues for representation being impossible without agents, which implies that he takes similarity relations to be insufficient for representation (Giere 2006, pg. 60, 64 endnote 13).

[16] Bueno & French (2012) endorse Chakravartty's (2009) argument along these lines (though they endorse structural relations while Chakravartty endorses a similarity relation). They go on to discuss the need for such a relation to ground the inferences that scientific representations facilitate, a position I adopt and explore after discussing Contessa's position.

### Further Arguments, and Amended & Weakened Versions

To be fair to Suárez, he does recognise that there are versions of the similarity and isomorphism/structural relation accounts that do not attempt to naturalise representation. He describes these as amended versions, accounts that employ a notion of 'representational force', which he defines as (Suárez 2003, pg. 237):

> $A$'s capacity to lead a competent and informed enquirer to consider $B$ as the *representational force* of $A$ ... [Representational forces] are determined at least in part by correct intended uses, which in turn are typically conditioned and maintained by socially enforced conventions and practices: $A$ can have no representational force unless it stands in a representing relation to $B$; and it cannot stand in such a relation unless it is intended as a representation of $B$ by some suitably competent and informed inquirer.

These amended versions avoid the non-sufficiency argument. The logical argument might also be avoided. Suárez' formulation of the amended versions of the structural relation and similarities accounts involves the additional condition of $A$'s representational force pointing at $B$. The avoidance of the logical argument depends upon how one explicates intended use. The other arguments, however, are claimed to continue to apply. While the accounts I will look at do not adopt Suárez' notion of representational force, their authors do discuss the intentions of representing agents, and the relationship between representations and intentions on their accounts.

Suárez also recognises that there are structural relation accounts that employ other structural relations than isomorphisms. He discusses the use of homomorphisms and partial isomorphisms as possible representation relations, though he finds them wanting. Homomorphisms are claimed to be able to cope with "partially accurate models", and so avoid the abstraction and idealisation version of the argument from misrepresentation. It is also held to weaken the non-necessity argument (though I do not take this argument to have any force), but suffers from the other arguments. In particular, homomorphism does not avoid the logical argument due to being reflexive. Fortunately, one might leverage the notion of representational force (or similar) to avoid this problem.

### Possible Solutions to the CQ

I take the above discussion to show that the main arguments against the structural relations account are not successful against suitably refined versions of such accounts, such as those I will outline in the next chapter. I do take the arguments to tell against an account of similarity, however. Thus in order to answer the constitution question we are left with a choice between Suárez' account of representation and a structural relation account. Suárez' account of representation is explicit in not attempting to establish necessary conditions, in particular in claiming that a particular type of relation is necessary for representation (Suárez 2004, pg. 771). He formulates his Inferential Conception Approach (ICA) as:

> **[Inf]** *A* represents *B* only if:
>
> (i) the representational force of *A* points towards *B*, and
>
> (ii) *A* allows competent and informed agents to draw specific inferences regarding *B*.

As such, it does not make any sense to ask whether the ICA provides *an* answer to the CQ; it is compatible with any putative representation relation, provided that the relation can, for the representation in question, allow for the "specific inferences" to be drawn.

Thus in order to argue for the structural relation accounts, one would have to either: a) show that structural relations are the best candidates for the representation relation in all instances of scientific representation (i.e. it is the relation that provides the inferences in the most accessible or efficient way); or b) that structural relations are the *only* relation that provide the inferences. a) is obviously weaker than b), and b) requires a response to the non-necessity argument (which may be restricted to scientific representation). Suárez' position and my counter argument rest on the assumption that inferences play an essential role in scientific representation. Such an assumption might form part of, if not the, answer to the DQ. Thus I will now turn to the DQ and argue that the establishment of surrogative reasoning is a distinctive feature of scientific representation.[17]

### 3.3.2 The Demarcation Question

The DQ, as set out above by CC, asks "what is distinct about scientific representation?" CC's answer is as follows (Callender & Cohen 2006, pg. 83):

> Plausibly scientific representation is just representation that takes place when the agents are scientists and the audience are either fellow scientists or the world at large.

What this means for CC is that the demarcation problem is transformed from concerning scientific representation to science itself: it is " the demarcation" problem of science. CC reach this conclusion because they believe that all representation can be reduced to a form of mental representation. Thus there is nothing special about scientific representation other than the fact that it occurs exclusively within the domain of science, or that it is a form of representation performed by scientists.

The issue then, is whether there is anything unique about scientific representation that prevents it from being reduced to some fundamental represen-

[17]At least this is a distinctive feature of epistemic representation, of which scientific representation is a subspecies.

tation (as CC presume there is not). Above I claimed that this unique feature would turn out to be the ability to draw inferences. Further, to prevent reduction to another type of representation, it appears that these unique features must be provided by the representation relation required by the particular type of representation. i.e. that scientific representation has the unique feature of allowing inferences to be drawn about the target by the representing vehicle, and these inferences are dependent upon a structural relation from the target to the vehicle.

A challenge to taking such surrogative inferences to be distinctive of scientific representation can be found in Contessa (2011), where Contessa argues for a demarcation between epistemic and non-epistemic representations based on surrogative inferences. While this distinction is complimentary to my project, it actually cuts across the demarcation that CC discuss and forces me to agree with their conclusion but for different reasons. I will outline Contessa's arguments for epistemic representations in the next section and argue that one should answer the DQ in terms of epistemic representation performed by scientists.

## 3.4 Contessa's Approach

Contessa is initially concerned with the demarcation between epistemic and non-epistemic representations, and later between merely epistemic and faithful epistemic representation. Epistemic representation is characterised by Contessa as follows (Contessa 2007, pg. 52-53):

> A vehicle is an *epistemic representation* of a certain target for a certain user if and only if the user is able to perform valid (though not necessarily sound) surrogative inferences from the vehicle to the target.

and as follows (Contessa 2011, pg. 123, endnote 7):

> To say that a representation is an epistemic representation is just to say that it is a representation that is used for epistemic purposes (i.e. a representation that is used to learn something about its target)

An example of non-epistemic representation is aesthetic representation, i.e. how a painting represents its subject. These categories of representation are not mutually exclusive, in that an epistemic representation can also be understood to be an aesthetic representation. Contessa's concern is the main purpose of the representing vehicle here, so talks in broad terms about epistemic or aesthetic representation. Thus establishing something as an epistemic representation does not prevent it from being an aesthetic representation - one can clearly gain information from a painting - yet the main purpose of the painting is aesthetic, so in broad terms the painting is taken to be an aesthetic representation.

Contessa takes the main symptom of epistemic representation to be the ability of agents to perform surrogative reasoning about the target of the representation by the representational vehicle. A key feature of epistemic representations is that the surrogative reasoning does not need to provide sound inferences about targets in order for the vehicles to be successful epistemic representations. The difference between the soundness and unsoundness of the inferences can be cashed out in terms of the faithfulness of the representations (Contessa 2007, pg. 54). A completely faithful representation is one where all the surrogative inferences we can draw from the vehicle to the target are sound. A partially faithful representation is one that provides some sound inferences from the vehicle to the target, but not all inferences. A partially faithful representation can still be a successful representation.[18] Contessa uses this distinction between merely epistemic and faithful epistemic representation to pose two questions that he thinks an account of representation should be

[18] These are the concepts I will adopt and use to answer the NI.

able to answer:

I) What makes a vehicle an epistemic representation of a certain target?

II) What makes a vehicle a more or less faithful epistemic representation of a certain target?

He also claims that these questions have been conflated into a single question concerning the 'problem of scientific representation'.

It is here that Contessa's distinction between epistemic and non-epistemic representation comes into direct conflict with the DQ. This is made most explicitwhen he states: "another way in which the label 'scientific representation' can be misleading is that it seems to imply that something sets aside scientific representations from epistemic representations that are not scientific" (Contessa 2011, pg. 124, footnote 10). The task at hand, then, is to look at Contessa's arguments for his preferred account of epistemic representation and faithful epistemic representation, and see whether he establishes epistemic representation as distinct from, or encompassing, scientific representation.

Contessa claims that there are three rival accounts epistemic representation: the denotation account; Suárez' ICA; and the Interpretational Account. He also holds there to be two ("some related") accounts of faithful epistemic representation: the similarity account and the structural account.

Contessa's project can understood as the attempt to answer the following questions:

1. What are, if any, the necessary and sufficient conditions for epistemic representation?
2. Do the necessary and sufficient conditions for epistemic representation have any necessary and sufficient conditions themselves?
3. What are, if any, the necessary and sufficient conditions for faithful epistemic representation?

Contessa answers 1 and 2 in his (2007) and claims that structural relations will play a role in answering 3 in his (2011). He is clear that the first two questions can be separated from the third, in that the questions of how to establish epistemic representation and how to establish faithful epistemic representation are two distinct questions and should be addressed separately (Contessa 2007, pg. 67-68), (Contessa 2011, pg. 124). This is partly due to their different nature: epistemic representation is binary, either we have it or we do not, whereas faithful epistemic representation is gradable (Contessa 2007, pg. 55).

### 3.4.1 Necessary & Sufficient Conditions for Epistemic Representation

Contessa argues for denotation to be a necessary condition for epistemic representation but not a sufficient condition (Contessa 2011, pg. 125). An example Contessa uses is the use of an elephant to represent the London Underground. While one can denote the London Underground via the elephant, it is not clear how one could use the elephant to perform surrogative inferences about the network. This example indicates that there are further conditions to satisfy in order to have a case of epistemic representation.

The ICA can be understood to be providing the second necessary condition for an epistemic representation. For example in Suárez (2004) the necessary condition is given as an agent being able to perform surrogative influences from the vehicle to the target. Contessa believes that this approach is "ultimately unsatisfactory" and in fact involves an *ad hoc* move: the ICA "seems to turn the relation between epistemic representation and surrogative reasoning upside down" (Contessa 2011, pg. 125). Take the example of the map of the London Underground. The ICA suggests that the map represents the Underground network *in virtue of* the fact that you *can* perform surrogative inferences *from it to* the network. However, Contessa argues, the reverse seems to be the case:

one can perform surrogative inferences to the network from the map *in virtue of the fact that the map is an epistemic representation of the network.* i.e. the ability to draw surrogative inferences *depends on a map being epistemic representation*, not the other way around. "If you do not take [the map] to be an epistemic representation of the London Underground network in the first place, you would never try to use it to perform surrogative inferences about the network."

Contessa's position on the ICA can be summarised as the view that an account of epistemic representation should explain what makes a certain vehicle into an epistemic representation of a certain target (for a certain user) and how in doing so it enables the agent to perform surrogative inferences about the target. The ICA, however, takes the ability to draw surrogative inferences to be basic and so lacks this explanation.

Given the initial characterisation of an epistemic representation[19] and the above, it is clear that a necessary and sufficient condition for epistemic representation is that a vehicle allows an agent to perform valid surrogative inferences about the target. This allows question 2 to be rephrased as follows:

2′ What are the necessary and sufficient conditions for an agent to be able to use a vehicle to draw valid surrogative inferences?

I will outline Contessa's answer to 2′ in the next section.

### 3.4.2 Necessary & Sufficient Conditions for Surrogative Inferences

In order to provide an account of epistemic representation, Contessa proposes his Interpretational Account, where a vehicle is an epistemic representation

[19] "A vehicle is an *epistemic representation* of a certain target for a certain user if and only if the user is able to perform valid (though not necessarily sound) surrogative inferences from the vehicle to the target" (Contessa 2007, pg. 52-53).

of a certain target for a certain user (i.e. a user can use the vehicle to draw valid surrogative inferences) if and only if: the user takes the vehicle to denote the target; and the user adopts an interpretation of the vehicle in terms of the target.[20] The DDI account of Hughes (1997)[21] is selected as a possible reference for an account along similar lines. The key feature of the Interpretational Account for the present discussion is that "the adoption of an interpretation of the vehicle in terms of the target [is] what turns a case of mere denotation into one of epistemic representation" (Contessa 2011, pg. 126). The interpretation of the vehicle in terms of the target supposedly provides the user with a set of "systematic rules to 'translate' facts about the vehicle into (putative) facts about the target". This set of systematic rules is the Interpretational Account's explanation of the relation between epistemic representation and surrogative reasoning. I will now outline the Interpretation Account and draw out the necessary and sufficient conditions it fulfils for surrogative inferences.

Contessa argues for the following (Contessa 2007, pg. 57):

> A vehicle is an epistemic representation of a certain target (for a certain user) if and only if the user adopts an interpretation of the vehicle in terms of the target.

He takes the adoption of an interpretation to be what grounds the ability to perform surrogative inferences from vehicle to target and so is the necessary and sufficient condition for obtaining an epistemic representation. His argu-

[20] The story is slightly more complicated however. To avoid accusations of 'naturalising' representation, Contessa points out that a vehicle is an epistemic representation *for a user*, rather than an epistemic representation "in and of itself" (Contessa 2007, pg. 53). Contessa thus claims that the representation relation in the case of epistemic representation is a *triadic* relation between the vehicle, a target and a (group of) user(s). Above, in §3.3.1 - *The Argument from Misrepresentation*, I mentioned this way of avoiding a naturalisation of representation and responding to the non-sufficiency argument. I have yet to provide details for handling intentions - I will do this when I discuss the accounts in detail in the next chapter. Contessa opts for one of the possible ways to accommodate the intentions of agents: he includes the agent as a relata of the (epistemic) representation relation. Not all of the accounts follow this approach.

[21] This account is a major influence on the structural relation account of Bueno & Colyvan (2011) and Bueno & French (2012).

ments for this position consist in cashing out the notion of interpretation, and showing how adopting an interpretation of the vehicle in terms of the target is necessary and sufficient for the performance of surrogative inferences from vehicle to target.

A loose first pass of cashing out the notion of interpretation, what I will call the 'general notion', is that a user "interprets a vehicle in terms of a target if she takes facts about the vehicle to stand for (putative) facts about the target". This can be considered a good starting point, which we can tighten up in certain respects to obtain quite specific and detailed characterisations of interpretation. One such characterisation is an *analytic interpretation.* This is a set-theoretic understanding of interpretation. It proceeds by the user first identifying the following (Contessa 2007, pg. 57-58):

- A (nonempty) set of relevant objects in the vehicle: $\Omega^V = \{o_1^V, \ldots, o_n^V\}$
- A (nonempty) set of relevant objects in the target: $\Omega^T = \{o_1^T, \ldots, o_n^T\}$
- A (possibly empty) set of relevant properties of and relations among objects in the vehicle: $P^V = \{{}^nR_1^V, \ldots, {}^nR_m^V\}$, where ${}^nR$ denotes an $n$-ary relation; properties are construed as 1-ary relations.
- A set of relevant properties and relations among objects in the target: $P^T = \{{}^nR_1^T, \ldots, {}^nR_m^T\}$
- A set of relevant functions from $(\Omega^V)^n$ to $\Omega^V(\Psi^V = \{{}^nF_1^V, \ldots, {}^nF_m^V)$, where ${}^nF$ denotes an $n$-ary function, and $(\Omega^V)^n$ is the Cartesian product of $\Omega^V$ with itself $n$ times.
- A set of relevant functions from $(\Omega^T)^n$ to $\Omega^T(\Psi^T = \{{}^nF_1^T, \ldots, {}^nF_m^T)$

A user must then satisfy the following conditions:

1. The user takes the vehicle to denote the target;

2. The user takes every object in $\Omega^V$ to denote one and only one object in $\Omega^T$ and every object in $\Omega^T$ to be denoted by one and only one object in $\Omega^V$;

3. The user takes every $n$-ary relation in $P^V$ to denote one and only one relevant $n$-ary in $P^T$ and every $n$-ary relation in $P^T$ to be denoted by one and only one $n$-ary relation in $P^V$;

4. They take every $n$-ary function in $\Psi^V$ to denote one and only one $n$-ary function in $\Psi^T$ and every $n$-ary function in $\Psi^T$ to be denoted by one and only one $n$-ary function in $\Psi^V$.

His in (2011), Contessa opts for a slightly different description of the situation. He states that the interpretational account has two necessary and sufficient conditions: the user takes the vehicle to denote the target; and the user adopts an interpretation of the vehicle in terms of the target (Contessa 2011, pg. 126). This might seem a different set of conditions, as he takes a denotation relation to a separate condition to interpretation. The difference, however, is only one of bookkeeping; the same relations will still be present, just that in his (2007) version of the Interpretation Account the denotation relation will be included under a composite interpretation relation, whereas in the (2011) version it is separated from the composite interpretation relation. Both relations are part of the composite representation relation in either case. As evidence of this being an insignificant difference, I point to Contessa's explanation of how one might understand the Rutherford model of the atom to be an epistemic representation. He states that on the interpretation account, the Rutherford model is an epistemic representation of an atom if and only if (Contessa 2007, pg. 59):

> (1) they [the user] take the model as a whole to stand for the atom in question . . . , (2) they take some of the components of the model to stand

> for some of the components of the system, and (3) they take some of the properties of and relations among the objects in the model to stand for the properties of and relations among the corresponding objects in the system (if those objects stand for anything in the atom).

It appears that Contessa separates out the relation of denotation between vehicle and target (call this the 'global denotation relation') in order to emphasise that the vehicle has to be aimed the a particular target we use to use the vehicle for in order to obtain valid inferences. He explains that, without the global denotation relation, one might still be able to draw inferences from a vehicle to a target due to some general features of the vehicle. For example, subway maps are designed with a general interpretation in mind, and so can be epistemic representations for subway networks other than the ones they were explicitly designed for (Contessa 2011, pg. 126, endnote 14).

So far, so good, but in order to properly explain how adopting such an interpretation allows one to draw *valid* surrogative inferences from a vehicle to a target something implicit in this characterisation of interpretation needs to be made explicit. That is, adopting an analytic interpretation results in the adoption of a set of inference rules (Contessa 2007, pg. 61):

> **Rule 1:** If $o_i^V$ denotes $o_i^T$ according to the interpretation adopted by the user, it is valid for the user to infer that $o_i^T$ is in the target if and only if $o_i^V$ is in the vehicle;

> **Rule 2:** If $o_1^V$ denotes $o_1^T$, …, $o_n^V$ denotes $o_n^T$, and ${}^nR_k^V$ denotes ${}^nR_k^T$ according to the interpretation adopted by the user, it is valid for the user to infer that the relation ${}^nR_k^T$ holds among $o_1^T, \ldots, o_n^T$ if and only if ${}^nR_k^V$ holds among $o_1^V, \ldots, o_n^V$

> **Rule 3:** If, according to the interpretation adopted by the user, $o_i^V$ denotes $o_i^T$, $o_1^V$ denotes $o_1^T$, …, $o_i^V$ denotes $o_i^T$, and ${}^nF_k^V$ denotes ${}^nF_k^T$, it is valid for the user to infer that the value of the function ${}^nF_k^T$ for the

arguments $o_1^T, \ldots, o_n^T$ is $o_i^T$ if and only if the value of the function ${}^nF_k^V$ if $o_i^V$ for the arguments $o_1^V, \ldots, o_n^V$.

An inference is then valid if it is in accordance with Rule 1, 2 or 3.

So much for cashing out of the notion of interpretation and providing an explanation for why it provides *valid* surrogative inferences from vehicle to target. Contessa still has to provide arguments for why adopting an interpretation is necessary and sufficient. As Contessa explicitly states that he does not want to give the impression that all interpretations are necessarily analytic, he only provides arguments for why adopting an analytic interpretation is sufficient for epistemic representation (Contessa 2007, pg. 58, 63). I therefore take his argument for adopting an interpretation to be necessary for surrogative inferences to be the claim that one must take facts about the vehicle to stand for (putative) facts about the target in order to infer anything from the vehicle to the target (i.e. the general notion).

Contessa argues for the sufficiency of the analytic interpretation by looking for a case where a user adopts an analytic interpretation of the vehicle yet cannot draw valid inferences about the target, i.e. the vehicle fails to represent (*qua* epistemically represent) the target. Key to understanding his arguments that such situations do not exist is the recognition of the distinction between mere epistemic representation and faithful epistemic representation. Most importantly, that epistemic representation requires *only valid* inferences, while faithful epistemic representation requires *sound* inferences, where soundness is cashed out in terms of the inferences being valid and their conclusions true of the target (Contessa 2007, pg. 51).[22]

[22]Contessa defends his account by showing how the Rutherford model of the atom can be understood as an epistemic representation of a hockey puck sliding on the frozen surface of a pond (Contessa 2007, pg. 64-65). These arguments are informative, but do not need to be summarised here.

### 3.4.3 Faithful Epistemic Representation

The account of faithful epistemic representation that Contessa favours is a structural (relation) account. Contessa presents a characterisation of such an account as follows: *if* a vehicle is an epistemic representation of a target for a certain user, *then*, *if* some specific morphism holds between the structure of the vehicle and the structure of the target, *then* the model is a faithful representation of that target. This seems to imply that Contessa takes epistemic representation to involve a denotation relation and an interpretation relation, and faithful epistemic representation to involve a relation of denotation, of interpretation, and a structural relation.

After this review of Contessa's arguments concerning epistemic and faithful epistemic representation, one may ask the following questions. Can a clear distinction be drawn between scientific and epistemic representation? If not, is scientific representation a subspecies of epistemic representation? And if it is, is scientific representation identical with faithful epistemic representation? If not, what distinguishes scientific representation from faithful epistemic representation?

### 3.4.4 Scientific and Epistemic Representation

In the discussion above concerning the CQ I noted that Suárez' position and my (future) arguments in favour of the structural relations accounts of scientific representation rested (and would rest) on the assumption that establishing surrogative reasoning was a unique feature of scientific representation. As I also noted above, this assumption is a solution to the DQ. However, the preceding review of Contessa shows this assumption to be unjustified; surrogative reasoning appears to be a unique feature of the broader category epistemic representation. Contessa frequently makes use of the example of a map of the London Underground. There can be no denying that a map is a representational vehicle that allows one to gain information about a target, and to gain information through surrogative reasoning. Yet any attempt at calling a map a scientific representation would undermine the term 'scientific representation' if it is to have any special meaning at all. I therefore take Contessa to have shown that surrogative reasoning is not a unique feature of scientific representation, but of epistemic representation.

Surrogative reasoning might be a necessary but insufficient condition for scientific representation. I therefore take scientific representation to be a subspecies of epistemic representation. Thus there is no clear distinction to be drawn between epistemic and scientific representation.

The example of the Underground map can be used to answer the third and fourth questions. An old map of the Underground that does not have modern stations or lines on it is a less faithful representation of the modern Underground than a modern map. However, one can still draw some true conclusions about the Underground from the older map. Similarly, scientific representations of varying faithfulness can be used to draw true conclusions about their targets (Contessa discusses this with the example of the model of the toboggan with differing gravitational sources included in the model). From just this brief description of different examples of varyingly faithful epistemic representations it should be clear that simply because a representation is partially faithful has nothing to do with whether it is classed as scientific or not. Thus the answer to the third question (is scientific representation identical with faithful epistemic representation) is negative because we can have partially faithful, non-scientific representations.

A definite answer to the fourth question (what distinguishes scientific representation from faithful epistemic representation) is challenging to give. It is not clear that all cases of scientific representation are cases of faithful epistemic representation. It might be the case that all scientific representations are partially faithful, in that they facilitate at least one sound inference. It is not immediately clear how to settle this issue. For any representation to be useful in any way it must facilitate at least one sound inference about its target. The issue is how to evaluate whether scientific representations that are typically taken to be completely inaccurate representations give any sound inferences. Examples of such representations are those that suffer from non-existent targets, such as the mechanical ether. It is reasonable to take such representations as providing some sound inferences, namely empirically adequate predictions at a certain time, even though any inferences concerning the ontology of the target are completely unsound. I will discuss this issue in more detail in the next section.

It is therefore the case that we cannot answer the DQ by only pointing to surrogative reasoning. This is a distinguishing feature of epistemic representation, and while all scientific representation is at least epistemic representation, not all epistemic representations are scientific ones. I suggest that the DQ should be answered in a similar way to CC: what distinguishes scientific representation from other forms of representation is that it is epistemic representation performed by agents who are scientists, whose audience is other scientists or the world. The above discussion has gone a good way to showing that this is a suitable answer.

Before turning to answer the CQ with this answer to the DQ in mind, I will survey Chakravartty's discussion of what he calls informational and functional accounts of representation.[23] His discussion at first appears supportive of Contessa's approach and the answer to the DQ I wish to endorse. However, he raises an issue that he claims requires further investigation than he is able to provide in his paper. I attempt this further investigation below, and argue

[23]Chakravartty (2009).

that Contessa's approach fits in with his earlier arguments, and that the issues can be settled via Contessa's approach.

## 3.5 Chakravartty: Informational vs. Functional Accounts of Representation

Chakravartty reviews a different way of answering the DQ, where the DQ is phrased as "what ... are the 'essential' properties of a scientific representation?" (Chakravartty 2009, pg. 198). He considers there to be two, supposedly conflicting, categories into which one can place the various attempts to answer this question that can be found in the literature. He names these categories *informational* and *functional*. Accounts that can be classed as informational are those that claim a "scientific representation is something that bears an objective relation to the thing it represents, on the basis of which it contains information regarding that aspect of the world". The most general version of the informational approach will appeal to relations of similarity. Accounts to be classed as functional emphasise the functions of representations: "their [the representations'] uses in cognitive activities performed by human agents in connection with their targets" (Chakravartty 2009, pg. 199). These functions can be divided into further categories: the demonstrations and interpretations of target systems the representations allow; and the inferences they permit concerning aspects of the world.

In his discussion of this distinction, Chakravartty highlights three arguments, "charges", against the informational accounts that are intended to show the superiority of the functional accounts. These are a (familiar) charge of non-necessity, a (familiar) charge of non-sufficiency, and the charge that there are essential functions to scientific representation that informational accounts remain silent about and are therefore "defective".[24] Chakravartty deals with the third charge by arguing that this worry is predicated on the confusion between the means and ends of scientific representation. To summarise his argument: informational and functional theories focus on different aspects of the question 'what are scientific representations?' Informational theories consider representations as knowledge bearing entities, whereas functional theories consider representations to be a set of knowledge exercising practices. These different conceptions of representation are not contradictory; in fact they are complementary, thus questions asked of these two conceptions require answers that must be taken together to form a general understanding of scientific representation.

I agree with Chakravartty's responses and accept his arguments to all of the charges. I also agree that the dichotomy between informational and functional accounts of scientific representation is a false one. The natural next step, given the rejection of the dichotomy, would be to endorse an account of scientific representation that had features of both the informational and functional accounts, namely intentionality, a similarity or structural relation, and surrogative reasoning. One such approach is Contessa's. While Chakravartty is willing to entertain this idea, he cautions against it being straightforward. There is an issue that requires further investigation before this kind of account can be adopted without difficulty.

The issue concerns cases of mistargeting, where the target does not exist such as in the case of phlogiston or the mechanical ether, and accounts of representation that make use of a gradable notion of accuracy or success.

[24]I have briefly covered Chakravartty's response to the non-necessity charge above in §3.3.1 - *The Non-necessity & Non-sufficiency Arguments*. He appeal's to Goodman's claim that similarity is "no sufficient condition representation" (Goodman 1976, pg. 3-4) and points out that it does not appear to be part of any informational account that similarity or isomorphism (or any other such relation) is sufficient for representation (Chakravartty 2009, pg. 205). He also appeals to the role of agents' intentions in supplying the non-symmetry and non-reflexive properties to the representation relation.

Chakravartty argues that it is unclear how to treat such cases. Take the model of the mechanical ether. Chakravartty claims that further work has shown it to be about as inaccurate as it is possible to be. He offers the reader two responses. One might conclude that the model was an unsuccessful/inaccurate representation of its intended system (due to the system not existing), yet is still a scientific representation. Alternatively one might conclude that such a model is undoubtably scientific (due to its presence in previous scientific investigations), but due to how unsuccessful/inaccurate it was, it is *not* a representation, merely a model (we were mistaken in taking it to be a representation) (Chakravartty 2009, pg. 209). Chakravartty takes this problem of mistargeting due to the non-existence of the target to be significant, as whether we count models which mistarget in this way will determine the stance one takes with respect to the necessary and sufficient conditions for scientific representation. For example, if one accepts a distinction between mere representation and accurate representation, then one can exclude the information relation from the necessary conditions for mere representation and include in the necessary conditions for accurate representations. However, if one rejects this distinction, then one might hold that there is some threshold of accuracy for a representation to count as scientific, and hence maintain that the information relation is a necessary condition for scientific representation.

I take Contessa's distinction between epistemic and faithful epistemic representation to fit the distinction between mere and accurate representation. As outlined above, Contessa's approach does entail different necessary and sufficient conditions for epistemic (mere) representation from faithful epistemic (accurate) representation. We can now decide on whether scientific representation should be a subspecies of epistemic or faithful epistemic representation: I have already argued that we should consider scientific representation to be a subspecies of epistemic representation. The issue is whether all scientific representations can be considered partially faithful representations. Remember that even one sound surrogative inference allows us to call a representation a partially faithful representation.

Accepting that all scientific representations are epistemic representations allows us to call cases like phlogiston and the mechanical ether scientific representations: they are epistemic representations performed by scientists. Chakravartty anticipates such a response to his worry, in that he describes 'scientific representation' as a "term of art", such that "we may define it as best serves the various philosophical uses to which it is put", defending this conventional approach against a definitive approach by pointing to conflicting intuitions (Chakravartty 2009, pg. 210). I personally hold the intuitions that misrepresentations are still representations, and that the term 'scientific representation' does not connote anything distinctive. I believe that Contessa's approach provides a justification for these positions, contra the ideas that there is there is a minimum level of accuracy for a representation to count as scientific and that a putative representation's target has to exist for it to be an representation.[25]

Part of how Contessa's approach provides this justification is the separation of faithfulness and success (or usefulness) (Contessa 2011, pg. 130). By grounding faithfulness in terms of the soundness of the surrogative inferences, we can understand faithfulness as involving the information relation. Indeed, Contessa points to this himself, arguing for a structural relation account of faithful epistemic representation. By grounding success in heuristic, contextual and pragmatic terms, we can have successful representations that are wildly unfaithful and unsuccessful representations which are very faithful. A

[25]In the next chapter, I will be arguing that Pincock's Mapping Account from his (2012) fits into Contessa's approach. Here I wish to note what Pincock says concerning phlogiston as an early indication that his account will fit: he claims that such representations have content, such that had phlogiston existed their representations would have been correct. Due to phlogiston not existing, however, their representations all turned out to be incorrect (Pincock 2012, pg. 26).

further advantage can be found by looking at two different ways in which we might evaluate the success of the mechanical ether representation. We might evaluate the representation along instrumentalist lines or considering whether it manages to 'save the phenomena',[26] or along ontological lines. The mechanical ether representation had some success in providing accurate predictions, and so might be considered successful according to the instrumentalist/saving the phenomena line. However, it failed completely in terms of the ontology it posited. Contessa's approach lets us explain why this is the case. The mechanical ether, as a partially faithful epistemic representation allowed for some sound surrogative inferences to be drawn concerning the behaviour of light. However, all surrogative inferences that we could draw from it concerning the ontology of light were found to be unsound. Evaluation of the success of a representation, then, involves restricting ourselves to certain sets of the surrogative inferences we can draw from the vehicle, and establishing the number of sound inferences in that set. The *total* number of sound and unsound inferences is not a guide to the success of the representation; we need a context within which to weigh the number of sound and unsound inferences, and in this sense success is contextual. The context often comes from the aims of the user of the representation.

In not recognising that accuracy and success as separate, Chakravartty is unable to make use of this response to the mistargeting worry. I think that part of the reason he does not recognise this separation is that he holds an intuition that scientific representation should be ontologically committing. That is, any scientific representation should always be evaluated in terms of the ontology it prescribes. This is made clear in his definition of the informational accounts in terms of there being an "objective relation" between the vehicle and the target.

[26]I have in mind something like the distinction van Fraassen draws between the notions of appearance and phenomena when he briefly discusses the idea of 'saving the phenomena' (van Fraassen 2010, pg. 8).

I want to reject this assumption. I think that we can evaluate a scientific representation in terms of its ability to save the phenomena separately from its ontology. Developing this position further, however, will require further work on what the target of the representation is. A simple reading of scientific representations would be that the target is the world (indeed, this seems to be the root of Chakravartty's complaints that the mechanical ether representation is inaccurate). A more sophisticated reading will take into account phenomena and data models, particularly if one adopts the Semantic View of theories. There is clearly more work to be done here. I will leave this discussion at this point as it is straying into issues of realism, which are unrelated to my main argument.

Above I have surveyed Chakravartty's discussion of the DQ and informational & functional accounts of representation. I have argued that Contessa's approach solves the issue of mistargeting *qua* non-existent targets Chakravartty raises for accounts with gradable notions of accuracy. In doing so, I also argued that Contessa's approach justifies a rejection of the intuition that there is anything significant to the term 'scientific representation', in that it allows for one to hold onto the intuition that a misrepresentation is still a representation. This leads me make my answer to the DQ more precise: scientific representation is *epistemic* representation performed by scientists, when their target is other scientists or the world. However, any *successful* scientific representation will require some sound inferences to be drawn. Given the arguments I put forward concerning misrepresentation and the context dependence of the notion of 'success', it seems plausible that all scientific representations can be considered partially faithful. Therefore, while the answer to the DQ requires only that we can draw valid surrogative inferences, our answer to the CQ will require that we can draw *sound* surrogative inferences.

## 3.6 Conclusion: Answering Callender and Cohen's Questions

### 3.6.1 Answering the Constitutive Question

In this chapter I have argued that structural accounts of representation survive the counter arguments of Suárez, that scientific representation should be considered epistemic representation performed by scientists and it is highly likely that all scientific representations should facilitate at least one sound inference. I have also argued that the answer to the DQ will set out the conditions that the representation relation must fulfil. In the case of scientific representation, the relation must be capable of providing sound surrogative inferences. In order to do this, the relation must be capable of providing an interpretation of the representational vehicle in terms of the target, and there must be some kind of denotation involved. There is no requirement that all of this is achieved by a single relation: the representation relation can be a compound relation. For example, Contessa argues for the relation responsible for epistemic representation to consist in a denotation relation and the relation(s) responsible for his analytic interpretation. He then also claims that structural mappings will constitute the faithful epistemic representation relation.[27]

I think that Contessa has the right of it: structural relations are the best candidates for constituting the relation responsible for faithful epistemic representation. I also agree that there needs to be some kind of denotation involved, and that the agents' intentions need to be accommodated. There is of course the advantage that adopting a structural relational account of representation fits with the answer to the applied metaphysical question I provided in the previous chapter. There I argued that a structural relation could account for

[27]He also includes the intention of the agents.

how mathematics can be applied to the world. But I also argued that the relation would need to be able to account for mathematical representation as these issues are the more interesting and difficult issues that need answering. I have shown in this chapter that structural relations are up to this job as well. How we are able to accommodate the gradable nature of faithfulness and usefulness will be investigated in the next chapter. Issues that are still outstanding relating to the problem of misrepresentation due to abstraction and idealisation will also be addressed in subsequent chapters.

### 3.6.2 Answering the Normative Issue

The final question to address is the NI, which asks "what is it for a representation to be correct"? CC introduced this problem while discussing the idea that models are not truth-apt. This causes a problem for answering this question. This problem is avoided due to the notion of adopting an interpretation of the vehicle in terms of the target, and by evaluating the soundness of the surrogative inferences we draw via the representation. This means that we don't discuss the representation being 'true', but rather of it being faithful, partially faithful or entirely unfaithful. We can judge the conclusions of our inferences as being true or false of the target, as they will be fully interpreted statements about the target. For example, a numerical prediction from a mathematical representation will actually be a statement about the physical value of a measurement, not a statement about the representation. Again, however, the specifics of how to answer this question will depend on the account to hand. A full answer to this question will therefore have to wait until later chapters.[28]

I will now turn to setting out two accounts of scientific representation that employ structural relations as their representation relations: the Inferential

[28]§6.2 for the Inferential Conception, and §4.2.2 and §7.2.1 for the PMA.

Conception of Bueno, Colyvan and French, and Pincock's Mapping Account. In the next chapter I will outline the accounts and explain how they can be considered accounts of faithful epistemic representation.

# 4 | Structural Relation Accounts of Representation

## 4.1 Introduction

In the previous chapter, I argued that scientific representation should be understood to be a subspecies of epistemic representation, and suggested that it is a subspecies of faithful epistemic representation. I accepted Contessa's arguments against Suárez, that surrogative inferences have to be *grounded* in something, that we have to explain *in virtue of what* we are capable of using a representing vehicle to draw surrogative inferences about a target. Contessa argued that interpreting a representational vehicle in terms of a target grounded the drawing of surrogative inferences, and that the ability to draw valid surrogative inferences with a vehicle made that vehicle an epistemic representation for that user. He also argued that a partially faithful representation is an epistemic representation that allows for the drawing of some sound inferences from the vehicle to the target. In this chapter I will be arguing for how to understand two accounts of structural representation as providing accounts of epistemic and faithful epistemic representation.

Contessa talks of requiring an account of faithful epistemic representation *in addition* to an account of epistemic representation. We need an account of faithful epistemic representation in order to explain how models represent phenomena, when those models contain various types of misrepresentations. It is not clear whether Contessa is claiming that an account of faithful epistemic representation is a separable account that provides *only* the faithful part of such representation, or if it is capable of providing the epistemic part as well. This is an issue which will be briefly explored in the course of the wider argument.

The structure of this chapter is as follows. I will outline the details of the Inferential Conception (IC) and Pincock's Mapping Account (abbreviated to 'the PMA' for 'the Pincock Mapping Account') in §4.2. I will then attempt to fit these two accounts into Contessa's approach. In §4.3.1 will argue that while the IC cannot provide an account of epistemic representation, it can provide an account of faithful epistemic representation. This is because the IC only involves structural relations and the intentions of agents. The interpretation of the vehicle is provided by the structural relations, which also provide the gradable notion of faithfulness. In §4.4.1 I will outline that the partial structures framework used by the IC allows it to provide a measure on the faithfulness of a representation, and promise to account for the relationship between the faithfulness and soundness of a representation in Chapter 6. In §4.3.2 I will argue that the PMA fulfils the criteria set out by Contessa for analytic interpretations. In this section I also discuss how Pincock's views on representational inferences can be accommodated by Contessa's approach. In §4.4.2 I will argue that despite initial problems, the PMA can be considered an account of faithful epistemic representation, providing the gradability of faithfulness through the use of various types of structural relations, in a manner akin to that recommended by Contessa. I am only able to sketch how both accounts can provide faithful epistemic representation in this chapter. A full investigation is pursued in the subsequent chapters, where the question of how faithfulness and usefulness are related is explored in detail. A choice between the accounts rests on the result of this investigation.

## 4.2 Structural Mapping Accounts of Representation

### 4.2.1 The Inferential Conception of the Applicability of Mathematics

The IC draws inspiration from Suárez'[1] and Swoyer's[2] claims concerning surrogative inferences, Hughes' DDI account of representation[3] and the partial structures programme.[4] One can see these three influences playing key roles in the account: representation occurs due to the existence of multiple (partial) mappings between (partial) structures, for the purposes of facilitating surrogative inferences. The account itself closely resembles the structure of Hughes' DDI account, with three stages involved in the representation of a vehicle by a target.[5]

In this section I'll provide an outline of the partial structures framework and how it is employed by the account at each of its three stages. As I am concerned with the mechanics of representation, ontological considerations will not be pursued. A defence of the majority of the ideas behind this account has already been presented in response to the various arguments against structural

[1]Suárez (2003, 2004)

[2]Swoyer (1991)

[3]Hughes (1997)

[4]Bueno (1997, 1999), Bueno et al. (2002), da Costa & French (2003), French (1999, 2003), French & Ladyman (1998) amongst others.

[5]The account is initially presented as being a way of understanding how mathematics can be applied to the world in Bueno and Colyvan's (2011). It was quickly developed to accommodate wider issues involved in mathematical representation and scientific representation more generally by Bueno and French in their (2011) and Bueno & French (2012). These papers also helped to locate the IC within the partial structure version of the Semantic View advocated by Bueno and French. This development does not alter the core principles of the account, so where the papers talk of an "application" of mathematics, "applying" or "applied" mathematics, I will talk of representation via mathematics, a mathematical representation or (representational) vehicle.

accounts in the previous chapter (see §3.1), though it might be useful to remember that the IC is not a purely structural account. Explicit (and frequent) use is made of its allowance for "additional pragmatic and context-dependent features in the process of applying mathematics" (Bueno & Colyvan 2011, pg. 352).[6]

### The Partial Structures Framework[7]

A partial structure is typically represented as a set-theoretic construct, $\mathcal{A} = \langle \mathcal{D}, R_i \rangle_{i \in I}$, where $\mathcal{D}$ is a non-empty set, $R_i$ is a partial relation and $i \in I$ is an appropriate index set. A partial relation $R_i$ is a relation over $\mathcal{D}$ which is not necessarily defined for all $n$-tuples of elements of $\mathcal{D}$. We can understand each partial relation $R$ as an ordered triple, $\langle R_1, R_2, R_3 \rangle$. $R_1$, $R_2$ and $R_3$ are mutually disjoint sets, with $R_1 \cup R_2 \cup R_3 = \mathcal{D}^n$, such that: $R_1$ is the set of $n$-tuples that belong to $R$, $R_2$ the set of $n$-tuples that do not belong to $R$, and $R_3$ is the set of $n$-tuples for which it is not defined whether or not they belong to $R$. When $R_3$ is empty, $R$ is a normal $n$-place relation that can be identified with $R_1$.

For partial truth, we can understand a partial structure as the set-theoretic construct: $\mathcal{X} = \langle \mathcal{Z}, R_j, \mathcal{P} \rangle_{j \in J}$, where $\mathcal{Z}$ is a non-empty set, $R_j$ is a partial relation, and $j \in J$ is an appropriate index set. $\mathcal{P}$ is a set of sentences of a language $\mathbb{L}$ which is interpreted in $\mathcal{X}$.[8] As before, for some $j$, $R_j$ might be empty (as it was for $i = 3$, $R_3$, above). $\mathcal{P}$ may also be empty.

The set that the partial relations are defined over, $\mathcal{D}$ and $\mathcal{Z}$ respectively, denote the set of individuals in the domain of knowledge[9] modelled or repre-

[6]Bueno and French go through the arguments presented by Suárez and defend the IC against them in §9 of their (2011).

[7]For this section I will reproduce the introduction to partial structures from Bueno et al. (2002) and da Costa & French (2003).

[8]As we are dealing with partial structures in terms of truth, we have to employ model theoretic machinery. See Hodges (1997) for an introduction to model theory.

[9]With respect to the domain of knowledge, $\Delta$, rather than understanding $\mathcal{D}$ as simply denoting the set of individuals, we might also understand this in terms of of a 'data structure',

sented in the particular case at hand, and the family of partial relations, $R_i$ or $R_j$, model or represent the various relationships that hold between the individuals. $\mathcal{P}$ can then be regarded as a set of distinguished sentences of $\mathbb{L}$, which might include, for example, observation statements concerning the domain.

A partial structure, $\mathcal{A}$, can be extended into a total structure $\mathcal{B}$ (or $\mathcal{X}$ to $\mathcal{Y}$). This total structure can be described as an $\mathcal{A}$-normal structure, where $\mathcal{B} = \langle \mathcal{D}', R'_i \rangle_{i \in I}$, if:

(i) $\mathcal{D} = \mathcal{D}'$;

(ii) every constant of the language is question in interpreted by the same object both in $\mathcal{A}$ and in $\mathcal{B}$; and

(iii) $R'_i$ extends the corresponding relation $R_i$, in the sense that each $R'_i$ is defined for every $n$-tuple of objects of its domain.

For the partial structures used for partial truth, there is a fourth condition:

(iv) If $S \in \mathcal{P}$, then $\mathcal{B} \vDash S$

where $S$ is a sentence in $\mathbb{L}$. This can be understood more loosely as follows: a total structure $\mathcal{Y}$ is called $\mathcal{X}$-normal if it has the same similarity type as $\mathcal{X}$, its relations extend the corresponding partial relations of $\mathcal{X}$, and the sentences $\mathcal{P}$ are true (in the Tarskian sense, as the notion of partial truth is supposed to be an extension of Tarskian truth) in $\mathcal{Y}$. This allows us to say that $S$ is partially true in $\mathcal{X}$, or in the domain that $\mathcal{X}$ partially modelled or represents, if there is an interpretation $\mathcal{I}$ of $\mathbb{L}$ in an $\mathcal{X}$-normal structure $\mathcal{Y}$ and $\mathcal{S}$ is true in the Tarskian sense in $\mathcal{Y}$. This can also be phrased as the claim that $S$ is partially true in the structure $\mathcal{X}$ if there exists an $\mathcal{X}$-normal $\mathcal{Y}$ in which $S$ is true in the correspondence sense. If $S$ is not partially true in $\mathcal{X}$ according to $\mathcal{Y}$, then $S$ is said to be partially false in $\mathcal{X}$ according to $\mathcal{Y}$. A major advantage of this

as in (da Costa & French 2003, pg. 17).

notion of partial truth is that it "captures the gist of the idea of a proposition being such that everything occurs in a given domain *as if* it were true (in the correspondence sense of truth)" (da Costa & French 2003, pg. 19).

The notion of partial structures also includes the notions of partial isomorphisms and homomorphisms. Let $\mathcal{A} = \langle \mathcal{D}, R_i \rangle_{i\in I}$ and $\mathcal{B} = \langle \mathcal{E}, R'_i \rangle_{i\in I}$ be two partial structures (with $R_i = \langle R_{i1}, R_{i2}, R_{i3} \rangle$ and $R'_i = \langle R'_{i1}, R'_{i2}, R'_{i3} \rangle$ as above). We can define a function $f$ from $\mathcal{D}$ to $\mathcal{E}$ as a *partial isomorphism* between $\mathcal{A}$ and $\mathcal{B}$ if:

1 $f$ is bijective; and

2 for $x$ and $y$ in $\mathcal{D}$:

(2.i) $R_{k1}xy \rightarrow R'_{k1}f(x)f(y)$; and

(2.ii) $R_{k2}xy \rightarrow R'_{k2}f(x)f(y)$.

If $R_{k3}$ and $R'_{k3}$ are empty, i.e. we no longer have partial structures but 'total' structures, we recover the standard notion of isomorphism.

We can define $f$ from $\mathcal{D}$ to $\mathcal{E}$ as a *partial homomorphism* between $\mathcal{A}$ and $\mathcal{B}$ if for every $x$ and every $y$ in $\mathcal{D}$:

(1) $R_{k1}xy \rightarrow R'_{k1}f(x)f(y)$; and

(2) $R_{k2}xy \rightarrow R'_{k2}f(x)f(y)$

Again, if $R_{k3}$ and $R'_{k3}$ are empty we recover the standard notion of homomorphism.

### Outline of the Inferential Conception

The IC is predicated upon the claim that "the fundamental role of applied [i.e. representational] mathematics is inferential" (Bueno & Colyvan 2011, pg. 352):

> by embedding certain features of the empirical world into a mathematical structure, it is possible to obtain inferences that would otherwise be extraordinarily hard (if not impossible) to obtain.

In order to accommodate this inferential role, the proponents of the IC claim that establishing mappings between the target system and the mathematical structure is "crucial". One of the problems with such a claim that is frequently raised is that the target system does not possess a structure, in that it is not a set-theoretic object, nor a mathematical object.[10] The solution to this problem is to adopt the attitude that the world has an assumed structure: "that there is some natural structure of the [target system] or that an appropriate structure can be imposed upon the [target system]", and that this is not a trivial matter, nor that there is a unique structure (Bueno & Colyvan 2011, pg. 353, endnote 17).[11]

Adopting the position that there is an assumed structure in the target system is only part of what Bueno and Colyvan mean by "empirical set up". They claim that the IC is idealised in two respects, one of which is that there is a "sharp distinction" between the empirical set up and the mathematical structures.[12] This is because "very often the only description of the set up available will invoke a great deal of mathematics", which leads to the situation where it is "hard to even talk about the empirical set up in question without leaning heavily on the mathematical structure, prior to the immersion step" (Bueno & Colyvan 2011, pg. 354). One should therefore understand the empirical set up to be "the relevant bits of the empirical world, not a mathematics-free description of it".

The core of the IC is a three step scheme, which can be understood as an

[10]Bueno and Colyvan recognise this problem, noting that "the world does not come equipped with a set of objects ... and sets of relations on those" objects (Bueno & Colyvan 2011, pg. 347).

[11]For a criticism of the SMA along the lines of non-unique structures, see Baker (2012).

[12]The second is dealt with below.

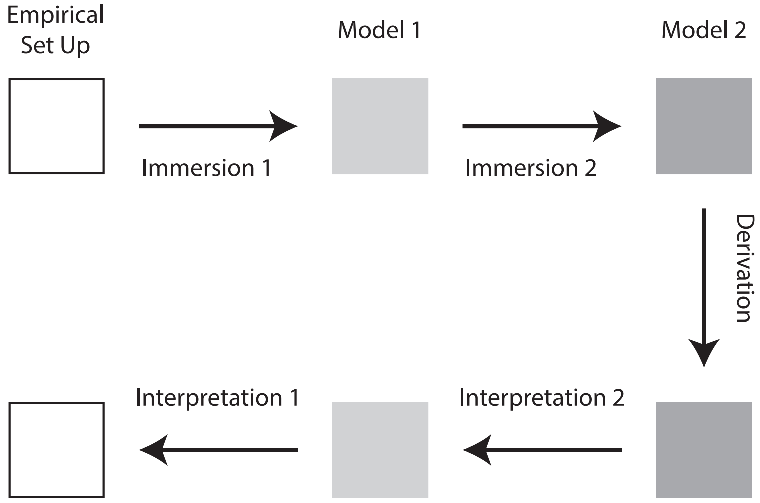


Figure 4.1: A schematic diagram representing the three steps of the Inferential Conception, with the immersion and interpretation steps iterated.

extension to Hughes' DDI account (Bueno & Colyvan 2011, pg. 353-354):

> **Immersion:** establish a mapping from the empirical set up to a convenient mathematical structure. This step aims to relate the relevant aspects of the target system to the appropriate mathematical context. The choice of mapping is a contextual matter, dependent upon the particular details of the representation.
>
> **Derivation:** consequences are drawn from the mathematical formalism, using the structure obtained in the immersion step. Bueno and Colyvan consider this the key point of the application process.
>
> **Interpretation:** the mathematical consequences (obtained in the derivation step) are 'interpreted' in terms of the initial empirical set up. Here, an interpretation is understood to be a mapping from the mathematical structure to the initial empirical set up. The mapping does not need to be the inverse of the one used in the immersion step. No problems emerge so long as the mappings are defined for suitable domains.

The immersion step does not have to be immediately followed by the derivation step. One can iterate the immersion mapping, to embed the first mathematical structure into another (Bueno & French 2012, pg. 91-92).[13] The immersion mapping is what gives representations their content on the IC. As I will explain, this content is uninterpreted mathematics. The content of representations is what constitutes the models manipulated in the derivation step. The general idea behind structural relation accounts of representation is that the content of representations is itself structural. Thus the models are understood in terms of their structural properties.

The derivation step initially seems simple: one performs some mathematical manipulations on the structured obtained from the immersion step and obtains a result. I will refer to this result as the 'resultant structure'. It is important to get clear about what is being manipulated in this step. In mapping from the initial empirical set up to the model used in the derivation step, via the immersion mapping, we shift from interpreted mathematics in the empirical set up to uninterpreted mathematics in the derivation model. The derivation step therefore involves the manipulation of an uninterpreted mathematical structure and so the resultant structure is uninterpreted. This is why the interpretation step is required: we have to map the resultant structure to an empirical set up which provides physical interpretation for the mathematics.[14] However, there might be a problem for this understanding of the IC when one takes into account the second respect in which the IC is idealised. This is the realisation that "the mathematical formalism often comes accompanied by certain physical 'interpretations"' (Bueno & Colyvan 2011, pg. 354). This threatens both the definition of interpretation as a mapping from a mathematical structure to an empirical set up, and the notion that the

[13] As can be seen in Fig 4.1, this requires the interpretation mapping to be iterated as well. This is because the first model is mapped to the second model by the second immersion mapping. The resultant structure will therefore be 'of' the second model. In order to gain any physical insights from this resultant structure, we have to interpret in terms of the first model.

[14] A problem arises if one does not provide such a mapping: "without an inverse mapping the mathematics remains uninterpreted and says nothing about the empirical system it is supposed to be representing" (Bueno & Colyvan 2011, pg. 349, endnote 8).

content of a representation on the IC is a pure mathematical structure. I will return to this possible problem in Chapter 6 when I outline what I call the *epistemic problem* for the IC.[15] For now, I propose that these 'interpretations' are not strictly interpretations, but rather heuristic, pragmatic and contextual considerations that will help guide the choice of manipulations that will be performed on the mathematical structure obtained from the immersion step.

The interpretation step is straight forward. However when discussing how the IC is able to provide a framework within which one can conceptualise the selection of appropriate mathematical structures, Bueno and Colyvan claim that "we need not think of the immersion step as being logically prior to the interpretation step" (Bueno & Colyvan 2011, pg. 356-357). This appears to be because the selection of an appropriate mathematical structure arises from going back and forth between the immersion and interpretation steps. This claim threatens the idea that the content of representations on the IC is a pure mathematical structure. The threat comes from the possibility that we end up with interpreted mathematics in the derivation step due to the interpretation step coming before the immersion step. In response to this, I propose that for the *initial* application of mathematics to a target system, the first time such a system is represented with mathematics, the immersion step *must* come first. This is because there is no mathematical structure to map to the target system via an interpretation mapping. Once the initial immersion and interpretation mappings have been performed, a chain of iterations of the three steps is established, leading to the situation where any *non-initial* immersion or interpretation mapping is not logically prior to any other (granting that the structures involved allow this). The reason for adopting this line of argument is that I see maintaining a pure, uninterpreted, mathematical structure as the content of representation on the IC as vital to its success when addressing

[15]See §6.2.2, page 168.

idealisations, as well as the only viable response to the epistemic problem.

Finally, I turn to the how he IC accounts for the relationship between the agent and the representational vehicle and target. The challenge is to identify what the intention relates in a non-problematic way. Does the intention relate the agent to the vehicle, to the target, to both, or to the structural relation itself? Is the intention part of the representation relation? One potential problem that can arise from including the intention that a vehicle represents a particular target in the representation relation is that this may prevent the vehicle from representing other targets (Bueno & French 2011, pg. 886-887). The idea is that by including the intention of the scientist that a vehicle represents a particular target would fix that target as the only target for the vehicle. For example, if we privileged the intention of Einstein that special relativity represents the behaviour of rods and clocks, then Minkowski would not have been able to use the theory to represent four dimensional spacetime. Bueno and French argue that in order to avoid such a situation not only should we not include the intention of agents in the representation relation, but that "we must allow for pragmatic or broadly contextual factors to play a role in selecting which of these relationships to focus on", and that the intentions should be separated from the underlying mechanism (i.e. the partial morphisms) so that the original representing agent's intentions can be "overridden" by other agents who wish to use the vehicle to represent some other target.

The approach favoured by Bueno and French is for the intention to pick out the structural relation between the target and vehicle. i.e. to pick out the relevant partial morphisms responsible for the immersion and interpretation steps. One might worry that this approach to intentions does not solve the non-sufficiency argument. After all, merely pointing to an isomorphism was the cause of the insufficiency claim. The response to this challenge is to highlight the role the intentions of the agent, along with contextual and pragmatic factors, play in identifying the structure of the final empirical set up.

On the IC multiple interpretations of a resultant structure correspond to different partial mappings from that resultant structure to different partial structures in the empirical set up. The intentions of the scientist and other pragmatic and contextual factors will pick out a particular partial morphism and therefore a particular empirical set up, i.e. a particular target. Remember that an empirical set up is a particular part of the world, suitably described in terms of interpreted structures. The interpretation associated with the empirical set up, along with contextual factors such as what use the agent is putting the representation to, pick out that empirical set up from the other structures that the resultant structure is partially -morphic to. Thus on the IC the representational relation consists in the immersion and interpretation mappings, and the intentions of the agents relate the agents to these mappings. Both these mappings and the intentions are necessary for a representation to occur, along with the pragmatic and contextual considerations.

This concludes the introduction of the IC. I now turn to outlining Pincock's Mapping Account.

### 4.2.2 Pincock's Mapping Account

In his (2012), Pincock is interested in establishing what the contribution of mathematics is to scientific representations, how it can be involved in the confirmation of scientific claims, its role in scientific inferences, how it can contribute to scientific explanation and what conclusions can be drawn for the realist/anti-realist debate in light of the contributions he identifies. Because of these wide ranging goals, Pincock's account of mathematical representation is not schematically set out, but rather outlined abstractly, then expanded upon through numerous examples. This is both useful and problematic: it allows one to see how certain parts of his account reveal the contribution of mathematics in the various areas he is interested in, yet it causes problems in identifying a clear, concise statement of what his account consists of. In light of this, I shall provide as clear an outline of his account as I can in this chapter. Further details will be provided through applying the PMA to my case study of the rainbow in Chapter 7.

Pincock takes the content of mathematical scientific representations to be "exclusively" structural, in that the "conditions of correctness ... [imposed] on a system can be explained in terms of a formal network of relations that obtains in the system along with a specification of which physical properties are correlated with which parts of the mathematics" (Pincock 2012, pg. 25). A representation is obtained by denoting a target and a structure to act as a representational vehicle. A structural relation is then identified between the target and the structure, and the "specification" relation is identified, which helps inform the content of the vehicle, including some form of interpretation of the mathematics in terms of the target. The exact content of the representation will depend on the application at hand, in that various parts of the mathematical structure might become "decoupled" from its interpretation and what is determined to be 'intrinsic' or 'extrinsic' mathematics. For example, some predictions are considered to be extrinsic mathematics and so are not part of the content of the representation. Pincock's account is very close to agreeing with Contessa's idea of an analytic interpretation.[16] I will show that this is the case in §4.3.2. In this section I will expand upon Pincock's notions of content, introduce the distinction between intrinsic and extrinsic mathematics and the related notion of core concepts, and detail the structural and specification relations.

[16] At one point Pincock comes close to setting his account out in the same way as Contessa (Pincock 2012, pg. 257).

**Pincock's Notion of Content**

Pincock distinguishes between *theories, models and representations* (Pincock 2012, pg. 25-26):

> **Theory** A theory for some domain is a "collection of claims" that aim "to describe the basic constituents of the domain and how they interact".
>
> **Model** Any entity that is used to represent a target system [i.e. a representational vehicle]. We "use our knowledge of the model to draw conclusions about the target system". They may be concrete entities, or mathematical structures.
>
> **Representation** A model with content.

Pincock takes contents to "provide conditions under which the representation is accurate". Understood schematically, this agrees with the IC: offering a mathematical scientific representation can be summarised as the claim that "the concrete system $S$ stands in the structural relation $M$ to the mathematical system $S^*$" (Pincock 2012, pg. 28). A representation is correct if "both systems exist and the structural relation obtains", otherwise it is incorrect.

Before exploring the various notions of content Pincock works with, a quick comment should be made concerning two of the assumptions he works with. The first assumption is that scientists are capable of referring to the world in some weak sense of 'refer'. That is, they are able to refer to "the properties, quantities and relations that constitute their domain of investigation" (Pincock 2012, pg. 26). This weak sense of 'refer' is cashed out in a counter-factual way. Pincock explicates this notion through the example of phlogiston, and thereby addresses the mistargeting problem of non-existent targets:

> In specifying the contents of the contents of [the representations of phlogiston], I take the scientist's ability to talk about phlogiston for granted. But ... I do not want it to follow that the phlogiston representations

> were without content. Instead, I would describe the situation as one where the scientists were referring to phlogiston in the minimal sense that they were coherently discussing a substance that could have existed, and had it existed, some of their phlogiston representations would have been correct. As it stands, such representations all turned out to be incorrect.

The second assumption is mathematical Platonism, though this assumption is later weakened to semantic realism about mathematics (Pincock 2012, §9). Pincock argues that the anti-realist positions of Hellman and Lewis are consistent with his account of mathematical representation and of the Indispensability Argument. I will not engage with Pincock's views on the Indispensability Argument in this thesis. However, I think that the approach Pincock has taken to reach these conclusions is the right one. He has investigated the way in which mathematics represents the world and produced an account of representation that has consequences for which mathematical ontologies we are able to adopt. That is, his answer to the problems of representation and the applied metaphysical question have set limits on the available ways we have to answer the pure metaphysical question, as I argued they would in §2.3.1.

The importance of these two assumptions is that they grant the ability to agents to refer to both mathematical and physical entities when attempting to represent the world mathematically. This can be seen in two ways. First, if (schematically) representations are correct due to the existence of a concrete system $S$, a mathematical system, $S^*$, and a structural relation $M$ holding between them, then an agent performing a representation requires "referential access" to the mathematical system $S^*$ in order to adopt a belief with the structural content $S_c$, i.e. the content of the representation (Pincock 2012, pg. 28). Second, they allow Pincock to adopt semantic internalism for mathematical concepts and semantic externalism for physical concepts (Pincock 2012, pg. 26-27).[17]

Now for the various notions of content. Pincock distinguishes four types of content, which roughly correlate with differing levels of sophistication of representation, though he warns the reader that he does not "claim that every representation goes through these stages [of differing contents] or that scientists would always be happy making these distinctions". Rather, one is to think of these distinct types of content as a "tool to help explain how to find the content in a given case" (Pincock 2012, pg. 26). The order in which they are introduced correlates with this story of increasing sophistication in representation.

The first notion of content Pincock distinguishes is called **basic** content. This is the type of content that arises from simple structural mappings (such iso- and homo- morphisms) and the basic elements of the concrete and mathematical systems (Pincock 2012, pg. 29). To get a grip on what Pincock means by "basic elements" of concrete and mathematical systems, one can contrast the examples he provides and his statements concerning those examples in his §2.2 - *Basic Contents* and those he provides in his §2.3 - *Enrich Contents* in particular the statements concerning derived elements. Derived elements are a type of mathematical element Pincock distinguishes en route to his second type of content, **enriched** contents.

In his §2.2 Pincock provides the example of concrete system composed of a group of 5 people and the relation of 'order of age', which is held to be isomorphic to the natural numbers 1 through 5 and the less than relation (on the assumption no two individuals share the same age). In his §2.3, Pincock provides the example of the heat equation, a partial differential equation:

$$\alpha^2 u_{xx} = u_t \tag{4.1}$$

[17]These positions are again altered slightly later when Pincock considers Wilson's approach to concepts and the revision of concepts. See Pincock (2012) Chapter 13, and Wilson (2006).

When applying this equation, one might expect the accuracy conditions to be along the lines of requiring an isomorphism between the temperature at each point in time and the set of ordered pairs $(x, t)$ picked out by the solution to (4.1) (Pincock 2012, pg. 30). But this would cause two problems. First, we do not expect our representation to be as accurate as an isomorphism would demand. Second, temperatures are not defined on a single spatial point, but rather thought to be a property of spatial regions. One way of responding to these problems is to stick with the basic contents and conclude that all such representations are inaccurate. This response faces serious difficulties in accounting for our use of our representations if they are nearly all taken to be inaccurate. Pincock opts for a different approach, which I will outline after establishing what constitutes basic elements.

From the above comments I propose that the basic elements of concrete and mathematical systems are as follows. For concrete systems they are things like space time points and properties at space time points. For mathematical systems they are things like numbers, properties of numbers, and ordered pairs of numbers. Thus anything more sophisticated than these elements will fall under the banner of 'derived' elements. So things like functions, properties over regions and so on.[18] These derived elements might constitute all of the elements involved in a representation, and are especially prevalent in idealisations (Pincock 2012, pg. 29). I gather that the content of any representation that involves derived elements will not be exclusively basic. But this does not mean that all representation that involve derived elements will have content

[18]It should be noted that Pincock appears to only introduced derived elements in terms of the mathematical structures: "these derived elements *in the mathematics* will be used to represent physical entities beyond those [that] can be related directly to the mathematical entities that appear in the domain of the mathematical structure" (Pincock 2012, pg. 29, my emphasis). However, given Pincock's claims about temperature not being a fundamental property of an iron bar (the system he applies the heat equation to in his example), I believe it makes sense to extend this notion to elements of the target domain. I take this to be what Pincock refers to as "derived quantities" in the introduction to his §2.4 - *Schematic and Genuine Contents*. Further, nothing much rests on this. The role of derived elements in idealisations will be far less significant than that played by the idealising assumptions.

exclusively composed of enriched content; rather enriched, and the other two types of content will include derived elements.

Enriched contents are another way of responding to the two problems for the application of the heat equation. We move from basic contents to enriched contents, employing derived elements in our concrete and mathematical systems. An enriched content can be understood to be a mathematical structure where a representational intention and/or details of the derived elements of the mathematical structure alter the structural relation $M$ between $S$ and $S^*$, both by employing a more sophisticated relation than an iso- or homo- morphism,[19] and by allowing the relation/specification relation to have an influence on the content of the representation (Pincock 2012, pg. 31). Pincock explains which part of the representation of heat diffusing through an iron bar is constituted by enriched content (Pincock 2012, pg. 30-31):

> In the heat equation, we have to work with small regions in addition to the points $(x, t)$ picked out by our function. We should take these regions in the $(x, t)$ plane to represent genuine features of the temperature changes in the iron bar. This representational option is open to us even if the derivation and solution of the heat equation seem to make reference to real-valued quantities and positions. We simply add to the representation that we intend it to capture temperature changes at a more coarse-grained level using regions of a certain size which are centered on the points picked out by our function. The threshold here can be set using a variety of factors. These include our prior theory of temperature, the steps in the derivation of the heat equation itself or our contextually determined purposes in adopting this representation to represent this particular iron bar. My approach is to incorporate all of these various inputs into the specification of the enriched content.

[19]By 'more sophisticated' I am referring to Pincock's use of employing mathematical terms within the structural relations. I cover the introduction of these terms below, on page 111.

> The enriched content has a much better chance of being accurate as it will typically be specified in terms of the aspects of the mathematical structure which can be more realistically interpreted in terms of genuine features of the target system.

Enriched contents are inadequate for accounting for all of the features of representing the diffusion of heat through an iron bar (and all of the features of a large number of idealisations). Specifically, the practice of representing the iron bar as being infinitely long. This practice requires a different notion of content than can be supplied by enriched contents, as such a representation seems to be doing something different than claiming that the iron bar is *approximately* infinitely long, or that it is infinitely long given some error term. How one interprets these idealisations threatens to commit one (or certain realists at least) to inconsistent properties.[20] For example, when employing the heat equation we make the idealisation that the bar is infinitely long, yet we also know that the bar is only 1m long from our observations. Thus by adopting the heat equation representation, this representation in conjunction with our observations attribute inconsistent properties to the bar. Pincock's approach to these sorts of idealisations is motived by a rejection of these inconsistencies. The idea is that such representational practices "decouple" or "detach" the relevant part of the mathematical structure from its "apparent physical interpretation" (Pincock 2012, pg. 32). Thus, when one sets the length of the iron bar to infinity, one is not claiming that the bar is infinitely long. Rather, one detaches the mathematics from its "prior association with a physical quantity in the physical system" and so "these structural similarities between the mathematical system and the physical system become irrelevant to the correctness of the overall scientific representation." Most importantly, "*it is not*

[20]See Maddy (1992) and Colyvan (2008) for arguments that such idealised properties are indispensable to science and so commit us to inconsistent claims when they are employed. Pincock's rejection this analysis of these idealisations will be covered in §7.2.1. He gives his response in §5 of his (2012).

*part of the content* of the scientific representation that *the bar is infinitely long or, perhaps, that it is any length at all*" (my emphasis). In doing so, one shifts to **schematic** contents. One way of understanding schematic contents is that they are pieces of the mathematical structure which have no interpretation. Alternatively, due to the previous physical interpretation this content had we are able to track that the representation says nothing about a particular property of the system - in this case the length of the iron bar.

Schematic contents can be understood to be schematic in two senses. First, schematic contents are "impoverished" compared to enriched contents, in that they place less restrictions on the way the system has to be for the representation to be correct. Second, the schematic content, often a particular variable in the mathematical structure, is considered to be an "unspecified parameter" as it now has no physical interpretation. These parameters reflect the fact that the representation provides *no information* on the property they were previously interpreted as. This should be contrasted with a situation where a representation claims the iron bar has *no* length. I presume this would amount to setting the length variable to 0.

By attributing values to unspecified parameters, one moves from schematic contents to **genuine** contents (Pincock 2012, pg. 32). How the move from schematic contents to genuine contents occurs depends on various contextual considerations, such as the differences in the target systems, the purposes of the investigation and so on. However, these goals do not get included in the content of the representation, because "it is no part of the accuracy conditions of the representation that the representation serve the goals and purposes of the scientist" (Pincock 2012, pg. 33). I take this to mean that the specification is guided by the goals of scientists, but that it does not include these goals.[21]

[21]The above exposition can only serve as an introduction to the notions of content that Pincock works with. For the clearest, although rather simple, examples of how Pincock's notions of content can be applied to actual representations, see his example of how to representing traffic flow (both in dynamic and steady states) and the example of the bridges

### Core Concepts and Internal & External Mathematics

The different notions of content are not the whole story of what mathematics constitutes the content of representations. The notion of core concepts and the distinction between intrinsic and extrinsic mathematics play a role. While both of these ideas are influenced by Pincock's views on the inferential role of mathematics in scientific representations (which shall be addressed in §4.4.2), they can be introduced independently of those considerations.

Pincock distinguishes the intrinsic and extrinsic mathematics as follows (Pincock 2012, pg. 37):

> **Intrinsic Mathematics** "Mathematics that is used in directly specifying how a system has to be for the representation to be correct"; the mathematics that "appears in the mathematical structure involved in the content".
>
> **Extrinsic Mathematics** Mathematics that is *not* intrinsic, or provides *other* contributions using other mathematics to the intrinsic mathematics. e.g. The "mathematics used to derive and apply a set of equations might be different to the mathematics of the equations themselves".

What might be considered an odd consequence of this distinction is that particular predictions about a target system are not part of the content of a representation. Pincock explicitly states that "extrinsic mathematics [can be] used to derive a prediction from a mathematical scientific representation", the core example of this being the solving the partial differential equations involved in the heat equation, (4.1) (Pincock 2012, pg. 38).

Establishing what is the intrinsic mathematics, and therefore the content, of a representation is not always straight forward. For example, one might be representing a section of an iron bar via a series of real numbers. This raises

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of Könngisberg (Pincock 2012, pg. 48-58).

questions of the following kind. Are all real numbers part of the intrinsic mathematics? What about complex numbers, which are necessarily linked to the real numbers? Pincock employs the notion of 'core conception' to help settle this issue. In this case, complex numbers are not taken to be part of the core conception of real numbers, and so would constitute extrinsic mathematics. But how does one establish what a core conception is?

The main motivation for Pincock's adoption of the notion of core conceptions is a problem related to inference. For now, this can be summarised as an attempt to find a middle path between the two extreme responses to the question of whether metaphysical necessity is respected by logical inference. These two extremes being the claim that either *all* metaphysical necessities are respected, or *none* of them are (Pincock 2012, pg. 35). The idea is that there is some 'core' elements or features of mathematical terms that must be grasped in order to be said to have an understanding of the terms. For example, transitivity is a part of the core conception of 'greater than'. Thus, when one considers the mathematical techniques required to solve the partial differential equations involved in the heat equation, one should see that these techniques "go far beyond what could be reasonably required to understand the equations themselves". As part of explaining what constitutes "understanding" here, Pincock appeals to the restricted faculties agents possess. That is, there are limitations on the number of calculations, inferences, or steps of a derivation an agent can perform without the use of tools (such as pen and paper, or a computer), and that these limitations play a role in delineating the core conceptions. This can be summarised as the stipulation "that only a certain level of complexity in [a] derivation can be tolerated", where complexity is judged by, for example, the length (i.e. the number of steps) of the derivation (Pincock 2012, pg. 37).

Now that I have introduced the notions of content and core conceptions, and the intrinsic/extrinsic mathematics distinction, I will turn to explaining Pincock's position on what the structural and specification relations are, and their role in Pincock's account.

**Pincock's Structural Relations and Specification Relation**

Pincock defines a structural relation as follows (Pincock 2012, pg. 27):

> A *structural relation* is one that obtains between systems $S_1$ and $S_2$ solely in virtue of the formal network of the relations that obtains between the constituents of $S_1$ and the formal network of the relations that obtains between the constituents of $S_2$ [where a] formal network is a network that can be correctly described without mentioning the specific relations which make us the network.

This is a fairly general notion and Pincock recognises that it must be tightened up in terms of explicating what the range of acceptable structural relations are in order for his account to succeed. Pincock is willing to accept the usual set theoretic mapping relations of isomorphisms and homomorphisms (and presumably other such mappings). However, when introducing enriched contents, Pincock argued that iso- and homo- morphisms are insufficient. They are restricted to relating the basic elements of mathematical and physical systems and so caused problems, prompting the shift to enriched contents. This shift resulted in an alteration of the structural relation $M$ between the two systems, $S$ and $S^*$. What this alteration consists in can now be explained.

Rather than be restricted to (simple) set theoretic structural mappings, Pincock is open to incorporating mathematical notions in the structural relations: "[he will] allow more intricate sorts of structural relations, including those whose specification requires mathematics" (Pincock 2012, pg. 27). How these structural relations are formed will depend on the representation at hand. For example, when attempting to represent the temperature diffusion through an iron bar with the heat equation, (4.1), we must take into consideration the fact that temperature is defined over regions, rather than points, of space. One way of doing this proposed by Pincock is to consider an isomorphism between the temperature at times, and $u$ in the mathematical structure, subject to a spatial error term $\epsilon = 1mm$. The idea is that the structural relation includes the 'threshold' of the spatial regions over which the temperature is defined.

The specification relation is first introduced as Pincock attempts to sharpen up the notion of structural content (Pincock 2012, pg. 25, my emphasis):

> . . . the content of mathematical scientific representations . . . is exclusively structural [which means] that the conditions of correctness that such representations impose on a system can be explained in terms of a formal network of relations that obtains in the system *along with a specification of which physical properties are correlated with which parts of the mathematics.*

This initial statement is then refined by comparison to Suárez (2010), when Pincock explains how he bridges the apparent gap between concrete systems and the set theoretic descriptions of structures[22] (Pincock 2012, pg. 29, my emphasis):

> Suppose we have a concrete system *along with a specification of the relevant physical properties. This specification fixes an associated structure.* Following Suárez, we can say that the system instantiates that structure, relative to that specification, and allow that structural relations are preserved by this instantiation relation (Suárez 2010, pg. 9). This allows us to say that a structural relation obtains between a concrete system and an abstract structure.

I take these quotations to detail the two roles the specification plays:

[22] Pincock appeals to the distinction between systems and structures in Shapiro (1997).

i) Provide an interpretation of the mathematical structure.

ii) Fix the mathematical structure.

I also take these quotations to imply that the representation relation for Pincock is at least a two-part compound relation, composed of a structural relation and the specification relation. The agent is also involved in the representation, but exactly how is not clear. The agent might be related to either the vehicle, target and specification relation, or to the vehicle and target by virtue of the specification relation, or in some other way. This will be clarified in §4.3.2, when inference is discussed in more detail.

We can see how the specification plays its two roles by referring to how enriched and schematic contents are identified. Enriched contents involve the adoption of derived elements in the mathematical structure. Here, the specification can play the ii) role: we recognise that temperature is defined over a region of space, and so must alter our mathematical structure to accommodate this feature of the world in order to have a possibly correct representation. Pincock explains this in terms of "incorporating" the contextual features that influence the choice of $\epsilon$ into the specification (Pincock 2012, pg. 31). Schematic contents involve the decoupling of the (prior) interpretation of mathematical variables. Here the specification will play the i) role, by simply not relating the variable $x$ to the length of the iron bar any more.

It appears that the specification relation is more than simply intentions. Pincock states that the intention for the heat diffusion representation to capture regions of space is only one of the inputs to be incorporated into the specification. He also lists such things as a theory of temperature and the derivation of the heat equation as "inputs" to the threshold of regions to be incorporated into the specification.

This concludes my introduction to the Inferential Conception and Pincock's

Mapping Account. I will now turn to establishing how one might understand these accounts in terms of Epistemic Representation.

## 4.3 The Structural Mapping Accounts & Epistemic Representation

In this section and the next I will be exploring how the IC and PMA can be understood in terms of Contessa's approach. *prima facíe* Contessa requires separate accounts for epistemic representation and faithful epistemic representation. He offers his Interpretational Account of epistemic representation separately from his arguments in favour of a structural account of faithful epistemic representation (Contessa 2011, pg. 126-130). The Interpretational Account of epistemic representation consists in (what I have called) a global denotation relation between vehicle and target, and Contessa's analytic account of interpretation. While Contessa does not offer an account of faithful epistemic representation, he indicates that he would advocate an account that makes use of various types of structural relations in order to cash out the gradability of faithfulness in terms of similarity of structure: "the more structurally similar the vehicle and target are . . . the more faithful an epistemic representation of the target the vehicle is" (Contessa 2011, pg. 129-130). However, it does not appear necessary for one to supply separate accounts, and cannot be necessary if the IC and PMA are viable accounts of faithful epistemic representation. Rather the requirement seems to be that there are three separable parts of an account:

i) Global denotation: a relation that points the vehicle to the target.

ii) Interpretation of the vehicle in terms of the target: loosely characterised, this requires the agent to "take facts about the vehicle to stand for (putative) facts about the target" (Contessa 2007, pg. 57).

iii) Gradable faithfulness relation: some relation that provides a suitable answer to the question "in virtue of what does [a] model represent a certain system faithfully" and is a gradable relation (Contessa 2007, pg. 68), (Contessa 2011, pg. 129).

The first two requirements constitute Contessa's account of epistemic representation, while the third constitutes his (putative) account of faithful epistemic representation. Whether the first two requirements are genuinely distinct requirements is debatable. In §3.4.2 I highlighted that Contessa includes the global denotation in his account of analytic interpretation in his (2007), but separates it out in his (2011). All that is required is that the vehicle is aimed at the target. This could be through explicitly distinct denotation and interpretation relations, through an interpretation relation that includes the denotation relation, or some other compound interpretation relation.

To establish whether the IC and PMA satisfy these requirements and fit into Contessa's approach, I will ask the following questions of the accounts:

(1) Which part of the account provides the epistemic representation?

(2) How does the account establish faithful epistemic representation? including:

  (2.a) How does it supply a gradable notion of faithfulness?

  (2.b) How does it account for the relationship between faithfulness and usefulness?

Question (2.b) is motivated by Contessa's arguments that faithfulness and usefulness do not always coincide (Contessa 2011, pg. 130-131). He argues for the splitting of faithfulness and usefulness in response to Galilean idealisation, but this notion can be generalised to any case of misrepresentation. What is significant about this realisation is that what determines a useful representation is the purposes and the goals of the agent using the representation. A viable account of faithful epistemic representation has to account for how less faithful representations can be more useful than more faithful representations.

As part of answering these questions I will be concerned with the idea that the relations responsible for epistemic representation have to be completely separable from the relations responsible for faithful epistemic representation. I take this to be a fair reading of the distinction Contessa argues for, rather than a requirement that there are two separate accounts. Given the introduction of the accounts above, it should be clear that the PMA can provide separate relations that would account for epistemic representation and faithful epistemic representation. It does not seem possible to provide separate relations on the IC, due to the interpretation of the vehicle being provided by the interpretation mapping, a structural relation. I will argue below that the IC is evidence that Contessa is mistaken to think that epistemic and faithful representation should always be separable, both in terms of distinct accounts and in terms of there being distinct relations. This has no effect on adopting his approach and considering the IC and the PMA as accounts of faithful epistemic representation.

I will now turn to answering (1). I will then provide outlines of answers to question (2) in §4.4. Fully worked out answers to questions (2.a) and (2.b) will be provided in Chapters 6 for the IC and 7 for the PMA.

### 4.3.1 The Inferential Conception

Representation occurs on the IC when an agent adopts a partial mapping from some empirical set up to a mathematical model (the immersion step), performs some mathematical manipulations to obtain a resultant structure (the derivation step), and then interprets this resultant structure in terms of the empirical set up by adopting another partial mapping, from the resultant structure to the empirical set up (the interpretation step). As explained above, the target of the representation is picked out by the intention of the agent, and the contextual and pragmatic considerations such as the use the agent is intending to put the representation to. The intention picks out the immersion and interpretation mappings. We can therefore point to the intention of the agent and the pragmatic and contextual considerations as playing the role of the global denotation relation, and the interpretation mapping as providing the interpretation of the vehicle in terms of the target. These are the relations that constitute the epistemic representation part of the IC.

There are two problems with this analysis of the IC. First, the way Contessa set out his accounts of epistemic and faithful epistemic representation implied that the relations that constitute the epistemic representation relation should be distinct from those that constitute the faithful epistemic representation relation (whether these relations constitute two distinct accounts or not). This is clearly not the case for the IC, as the relation which provides the interpretation of the vehicle in terms of the target is a structural relation, which is supposed to responsible for the faithfulness of the representation. Second, we are only supposed to be able to draw inferences about our target after we have adopted an interpretation of the vehicle in terms of the target. However, it appears that we can draw inferences before we adopt such an interpretation: the derivation step appears to involve drawing surrogative inferences, and this step finishes before we have interpreted the vehicle in terms of the target (before the interpretation mapping is identified).

The first problem puts pressure on accepting the IC as an account of faithful epistemic representation. I argued above that although there need not be separate accounts of epistemic and faithful epistemic representation, Contessa could be interpreted as requiring that there be relations that separately provide epistemic and faithful epistemic representation. The IC clearly cannot provide these relations, which leaves us with the choice of either rejecting the IC as an account of faithful epistemic representation (as it does not include relations for epistemic representation), or rejecting this interpretation of Contessa's statements (and possibly the statements that were interpreted).

There is strong evidence that we should accept the IC as an account of faithful epistemic representation: the authors discuss the role it plays in providing a framework for accommodating surrogative inferences; it is inspired by Hughes' DDI account, which is an account of representation Contessa advocates as a possible account of epistemic representation; and it introduces the structural relations required to accommodate the faithfulness of representations.

I think that the correct course of action is to reject Contessa's statements concerning the distinctions between epistemic and faithful epistemic representation as endorsing any kind of necessary distinction of either accounts or relations. We can reject these statements while remaining consistent with Contessa's approach. Faithful epistemic representation is held to occur due to the existence of a structural relation. If this relation does not exist, it need not be the case that we have an epistemic representation, as this can be established through different relations. We can have accounts of faithful epistemic representation that use structural relations (and agents intentions) to establish the interpretation of the vehicle in terms of the target as well as providing the gradable notion of faithfulness.

A possible objection to this solution to the first problem is that we would be unable to have situations where we thought we had a (partially) faithful epistemic representation that then turned out to be a wholly unfaithful representation. Contessa thinks that such cases exist (Contessa 2011, pg. 63). We would be unable to have these situations on the IC because either there exists a structural relation between the target and the vehicle, and so the vehicle is a (partially) faithful epistemic representation, or the relation does not exist and so it is not a representation of any kind. If the structural relation does not exist, there is no relation that can play the role of the interpretation relation, and so the vehicle could not be an epistemic representation of the target.

I can provide two responses to this objection. Either we were always mistaken that the vehicle was a (partially) faithful representation, and so we should not have been employing the IC anyway; some other account would have to be provided to account for our taking of the vehicle to be a representation (e.g. Contessa's Interpretational Account of epistemic representation). Or the representation *is still* partially faithful, it is just not faithful in any way we now deem useful. I have in mind here the response I gave in §3.3.1 and §3.5 to the non-existent targets type of misrepresentation. If we ever thought that the representation was partially faithful, then there must have been some inferences that were accepted as sound at the time. Most likely, these would be measurement results or predictions of measurement results. Provided we leverage the contextual dependence of 'use', we will still be able to accept these inferences as sound (i.e. rather than saying the model is providing both measurement predictions and ontological statements, it is now only providing a way of saving the phenomena.). There is still a structural relation between the vehicle and the measurement results, even if we can now state it as being less strong than initially thought.

The second problem can be resolved by recognising a distinction between intra-vehicular inferences, and extra-vehicular inferences.[23] The derivation step involves inferences that are internal to the representing vehicle. As the structure is an uninterpreted mathematical structure, we cannot take any inferences we draw from it to be about anything *but* mathematics. This is the entire reason for requiring the interpretation mapping. Thus simply drawing consequences from the mathematical formalism is not a performance of surrog-

[23]The idea of intra-vehicular inferences I'm proposing here is similar to the notion that models have an "internal dynamic" (Hughes 1997, pg. S331-S332).

ative inference. To perform surrogative inferences, we have to infer something *from* the vehicle *to* the target, that is, we have to *interpret* something about the vehicle *in terms of* the target, which is what the interpretation mapping provides. I therefore call the inferences drawn in the derivation step intra-inferences: they are internal to the vehicle. Surrogative inferences are extra-inferences, inferences that are external to the vehicle, they result in claims that are external to the model. Thus, although we can perform inferences before interpreting the mathematics, they are not the sort of inferences that Contessa is referring to by surrogative inferences. On the IC, the interpretation mapping provides the surrogative inferences. But again, *which* mapping we choose will be dependent upon the pragmatic and contextual considerations. The IC will involve a pragmatic and contextual 'relation' for each mapping involved in the representation.

The above discussion shows that the IC and Contessa's programme are compatible. The broadly contextual and pragmatic considerations that are involved in representation include such things as representational and contextual intentions, and these considerations in conjunction with the immersion and interpretation mappings constitute the part of the IC which allow us to draw surrogative inferences with the representational vehicle. The gradability part of faithful epistemic representation will be provided, as mentioned above, by the partiality of the structures and mappings involved.

I now turn to outlining how the PMA deals with epistemic representation.

### 4.3.2 The Mapping Account

As indicated in §4.2.2, the PMA looks, *prima facíe* to be an account of mathematical scientific representation that exemplifies Contessa's notion of analytic interpretation. This can be seen most clearly when Pincock defends his approach from Fictionalist arguments. He summarises the process by which representations obtain content as follows (Pincock 2012, pg. 257):

> As I have presented things in this book a scientific representation gets its content in three steps. First, we must fix the abstract structure which we are calling the model. This is a purely mathematical entity. Then some parts of this abstract structure must be assigned physical properties or relations. At this second stage the parts of the purely mathematical entity are assigned denotation or reference relations. Finally, a structural relation must be given which indicates how the relevant parts map onto the target system or target systems of the representation. At the end of these three steps the representation has obtained its representational content. This means that it is determinate how a target system has to be for that representation be an accurate representation of that target system.

In this quotation one can identify the two roles of the specification: fixing the mathematical structure and providing an interpretation for the mathematical structure.[24] We can also see that the specification relation is distinct from the structural relation.

I take Pincock's account as conforming to Contessa's notion of an analytic interpretation. As such it does provide separate relations for epistemic and faithful epistemic representation. In order to have an analytic interpretation, the account needs to satisfy four conditions: taking the vehicle to denote the target (the 'global denotation relation'); taking every object in the vehicle to denote one and only one object in the target; similarly for every relation in the vehicle and target; and similarly for every function in the vehicle and target. I take the specification relation to be capable of providing the 'global denotation relation' as it includes representational intentions. These intentions

[24] These roles were outlined in §4.2.2 - *Pincock's Structural Relations and Specification Relation*, on pg. 111.

are involved in the specification's first role, of fixing the structure. This is the process of finding a mathematical structure that can be used to represent the physical system. This is a little more involved than simply denoting a piece of mathematics. The fixing of the structure does not just take into consideration the representation intentions, but also features of the mathematical structure that is being fixed (such as steps taken to derive the structure), features of the theory or previous theories that are used to represent the phenomena, and so on (Pincock 2012, pg. 31).

Further, this step only involves an uninterpreted mathematical structure at this point. I consider it to be similar to searching for the correct tool. The agent roots around their 'mathematical tool box' attempting to find the right mathematical structure. The intentions inform the search, rather than fixing any target. This reverses the direction of the intentions from how the problem was posed above, where the intention was going *from* the vehicle *to* the target. e.g. That $E = mc^2$ would always have to represent rods and clocks if Einstein's intentions were included in the representation relation. Pincock sets up the intention relations as going in the other direction: one has a target and must find a structure to use as a representation. This way of looking at the involvement of intentions, and how it avoids the problem, is reinforced by the distinction drawn between the specification and the structural relation. i.e. Intentions concern the identification of the vehicle, rather than the target or a structural mapping. A consequence of this approach is that one might obtain the structural mapping for 'free', due to choosing the structure of the vehicle. This is not the only way in which the PMA uses intentions, however. When outlining how the PMA will accommodate the gradable notion of faithfulness, I will argue that the intentions will pick out different structural relations in addition to their role here.

The other three conditions for an analytic interpretation can all be met by the second role of the specification, that of providing an interpretation. There is evidence that the specification does this through denotation. In the above quotation, Pincock describes the interpretation of the mathematical structure to occur due to “the parts of the purely mathematical entity [being] assigned *denotation* or reference relations” (Pincock 2012, pg. 257, my emphasis); when discussing the heat equation, he talks of the physical and mathematical basic contents being isomorphic to each other, where the “set of ordered pairs $(x, t)$ picked out by the solution to our equation, where the first coordinate *denotes* position and the second coordinate *denotes* time” (Pincock 2012, pg. 30). Further, I think it is possible to consider this denotation separate to the isomorphism, both from how Pincock explains the situation here, and that the previous quotation (from pg. 257) explicitly separates out both relations, talking of first the establishing the specification/interpretation relation and then the structural relation. Thus I take the way the specification relation provides an interpretation of the mathematical structure in terms of the target to be an instance of Contessa’s analytic interpretation.

### Inferences and Mathematical Necessity

There are two problems with the idea that Pincock’s account directly and simply fits into Contessa’s programme however. First, Pincock distinguishes his approach to representation from inferential approaches, including Contessa’s. Second, Pincock has a particular view on inference, which is motivated by considering the relationship between the metaphysical necessities that mathematics includes and logical consequence.

The first of these problems is not too significant. Pincock explains the difference as (Pincock 2012, pg. 28):

> Perhaps the main competitor to an approach based on accuracy conditions tends to put inferential connections at the heart of their picture of

> representation. Inferential approaches must explain the scientific practice of evaluating representations in terms of their accuracy. While there does not seem to be any barrier to doing this, I have found it more convenient to start with the accuracy conditions. On my approach, inferential claims about a given representation follow immediately from its accuracy conditions: a valid inference is accuracy-preserving.

I take the distinction between Contessa's and the IC's, and Pincock's approaches to be one of priority, which has no consequences for understanding Pincock's account in terms of epistemic representation. That is, Pincock constructs his account starting from accuracy conditions, and addresses inferences after he has made clear how representations provide accuracy conditions, whereas the inferential approaches construct their accounts by starting with a focus on how to guarantee inferences, and then establish how these lead to accurate representations. In other words, we can understand Pincock to be attempting to establish an account of faithful epistemic representation, which entails an account of epistemic representation. Inferential approaches might aim to establish an account of epistemic representation first and then extended it to an account of faithful epistemic representation.[25]

The second problem is far more threatening. It originates in the approach Pincock takes, focusing on accuracy conditions before inference. This results in a focus on how scientific representations are confirmed to a greater extent than the inferential approaches do.[26] Pincock's discussion of inference begins with his adoption of the "*prima facíe* assumption that representations are confirmed by entailing, perhaps in conjunction with auxiliary assumptions,

[25]The advocates of the IC start from the position that mathematics is capable of providing surrogative inferences. They construct the IC to show how mathematics can play this role. As such their starting position entails that the IC should be an account of epistemic representation. However, as they adopt structural relations to account for the surrogative inferences, they skip an account of epistemic representation and actually argue instead for an account of faithful epistemic representation.

[26]At least more than Contessa, who does not discuss confirmation, or the authors of the IC, who only discuss the consequences of establishing whether a representation is empirically adequate or not, rather than how the representations can be found to be confirmed or not.

some predictions that are then experimentally verified. *So we need to focus on this inferential link between our representations and these predictions to understand confirmation*" (Pincock 2012, pg. 33, my emphasis).

Pincock approaches his analysis of the inferential link by comparing it to our standard account of logical inference. He focuses in particular on the translation of English into logic, which allows us to make the "distinction between logical and nonlogical terms ... explicit". Pincock goes on to argue, however, that extending this approach to our mathematical scientific representations "will distort their genuine inferential relations and obscure difficulties in confirming them". To motivate this view, Pincock translates the sentences 'the number of fish is greater than the number of cats', 'the number of cats is greater than the number of dogs', and 'so, the number of fish is greater than the number of dogs' into mathematical language:

$$\begin{aligned} & a > b; \\ & b > c; \\ \therefore \quad & a > c \end{aligned}$$

and logical language:

$$\begin{aligned} & Nx(x \text{ is a fish}) > Nx(x \text{ is a cat}); \\ & Nx(x \text{ is a cat}) > Nx(x \text{ is a dog}); \\ \therefore \quad & Nx(x \text{ is a fish}) > Nx(x \text{ is a dog}) \end{aligned}$$

or:

$$\begin{array}{rl} & G(ab); \\ & G(bc); \\ \therefore & G(ac) \end{array}$$

The problem with these two translations in to logical language is that there are no logical terms; that is the greater than relation, >, has been translated into a two-place predicate, which has no "fixed interpretation" (Pincock 2012, pg. 34). As the two place predicate $G(\cdot\cdot)$ does not have a fixed interpretation, when analysed in terms of possible worlds there will be worlds where this argument has true premises and a false conclusion. e.g. $G(\cdot\cdot)$ could be interpreted as 'greater than', or 'less than'. Thus, the above argument is invalid. For Pincock, this presents us with a dilemma:

i) inference must respect the metaphysical necessities, and therefore the above argument is valid; or

ii) we widen our notion of necessity and possibility by invoking a sense of logical possibility according to which the inference is valid.

Pincock argues that we should adopt ii). His argument can be summarised as the claim that if we adopt i), then all mathematical claims entail all other mathematical claims due to the metaphysical necessary relationships between them, which stops us from explaining how learning new mathematics can lead to new inferences from scientific representations (Pincock 2012, pg. 34). The idea is that, if the metaphysically necessary connections are respected, then all mathematical claims are tautologous, and so all inferences including mathematics will be trivially valid. Thus when we perform an inference from a mathematical scientific representation "all we wind up doing … is making explicit what was already implicit in the content of the original representation".

To see what sort of consequences this has for surrogative inferences, lets look at the heat equation, (4.1), and how we use it to represent heat diffusion. We specify all of the parameters of the equation's schematic content to get genuine content. This allows us to make predictions and test the accuracy of the representation, which involves finding out what the accuracy conditions actually are and looking to see if they occur in the target system. That is, we solve the partial differential equations where variables have been given values specific to the target system we are representing. Yet the solution to a partial differential equation (if the equation is well formed) is unique and it is (supposed) to take on unique values by metaphysical necessity. Pincock poses the understanding of this inference to be problematic: are we supposed to understand the metaphysical necessary connection between the partial differential equation and the values of its solution to be a metaphysically necessary statement about the iron bar and the way heat diffuses through it, for example?

Pincock's solution to this problem, and motivation for adopting ii), is the claim that "we can understand the content of the representation and yet not be able to work out what the solution is. This suggests that the solution is not part of the content of the representation". This is supposedly clear, as the solution is derived from metaphysically necessary truths, yet the necessity of these truths is not "relevant to the conception of inference [Pincock] is ... articulating". What *is* relevant, is the "nonlogical character of the mathematical terminology", as it is the nonlogical character of the terminology that "blocks the entailment relation between the systems of equations and its solution. To get genuine entailment, we need to add additional premises to the argument corresponding to the features of the mathematical entities that are sufficient to pin down the interpretation of the mathematical terms".

This is where Pincock's notions of intrinsic and extrinsic mathematics and core conceptions come into play with regards to inference. The idea is that the content of our representations includes just the core conceptions of the intrinsic mathematics, and it is these core conceptions which have a 'fixed' interpretation. As transitivity is part of the core conception of the 'greater than' relation, the number of fish argument is valid without additional premises. However, the solutions to partial differential equations are *not* part of the core conceptions of the partial differential equations themselves. Thus we need to employ extra premises, extrinsic mathematics, in order to construct a valid inference that the solution to the partial differential equations makes a prediction about the target system. As these inferences require the extrinsic mathematics, the triviality of the conclusion is removed, helping to alleviate the worry that our mathematical scientific representations make claims that the world is necessary in some way.

So why is this a problem for fitting the PMA into Contessa's approach? The way the approach was set out did not include any considerations of how mathematics can lead to valid inferences. The sense of validity Contessa seemed to be concerned with was that of *modus ponens*: if $A$ holds in the vehicle, then $B$ holds in the target; $A$ holds in the vehicle, $\therefore B$ holds in the target: $A \rightarrow B$; $A$; $\therefore$ $B$. Given Pincock's statements above, this appears to be too simple an approach to inference. This difference in apparent simplicity can be accounted for by looking at how the IC and the PMA deal with the heat equation in terms of extra- and intra-vehicular inferences.

We can understand the heat example in a *modus ponens* argument form as follows: we start with the claim that the solution of a PDE, $S$, represents the heat diffusion at a particular point in the iron bar as having the magnitude $\mathbf{S}$. We solve the PDE to get a particular value for $S$ (intra-vehicular inferences). We then conclude that the heat diffusion in the bar at a particular point will have the magnitude $\mathbf{S}$ (an extra-vehicular inference). This implies that the notion of inference Contessa's epistemic representation work with is only concerned with extra-vehicular inferences, such as predictions about magnitudes and properties of the target system. I take this to be the same for the IC. This understanding of inference is in contrast to Pincock's, which concerns not just predictions, but also how the predictions are reached, and so might threaten the intra-/extra-vehicular inference distinction drawn above.

On the Contessa/IC view, solving PDEs are inferences that are internal to the representing vehicle. One can solve PDEs with the content of representations on these accounts. But according to Pincock, solutions to PDEs are not part of the content and so cannot be considered to be an intra-inferences. We cannot consider them to be extra-inferences either as these are supposed to be inferences *from* the *vehicle* to the *target*. Solving a PDE is an inference from a piece of interpreted mathematics to another piece of interpreted mathematics, which is then inferred from again to give the physical magnitude.

One possible solution is to consider the derivation of the solution to be part of an extended extra-inference. One should consider the derivation of the solution, and the extra premises needed (according to Pincock) in order to turn this solution into a prediction, to be the extra-vehicular inference. This fits both Pincock's notion of inference and with the idea that what matters for epistemic representation is an inference *from* a representational vehicle *to* a target. There is nothing about this notion of extra-vehicular inference which restricts it to only arguments of the *modus tollens* form, or at least not of the $A \rightarrow B$; $A$; $\therefore B$ form where $A$ is atomic. e.g. $A$ could contain further conditional statements (e.g. $((A \wedge B) \rightarrow C) \rightarrow D$; $((A \wedge B) \rightarrow C)$; $\therefore D$). I will not explore this solution further here. I take this solution to be good enough to secure an understanding of Pincock's account in terms of epistemic representation.

I have shown above that Pincock's view of inference originates from his approach to representation. He starts from accuracy conditions, from which the inferential claims are supposed to follow, rather than concentrating on the inferential claims and then attempting to explain how these claims result in the evaluation of representations in terms of accuracy. This lead Pincock to find the necessity of mathematical claims to be problematic, and finally to his notions of intrinsic and extrinsic mathematics, and core conceptions.

The inferential approaches do not take the necessity of mathematical claims to be problematic, or at least there is nothing in the literature to show that they do. I think the reason for this is brought out in a footnote of Pincock's, where he rejects an idea put forward by a referee of his book, that "learning mathematical truths may serve only to make us aware of valid inferences that were already available from a given representation" (Pincock 2012, pg. 34, footnote 8). Roughly, I think the idea is this: mathematics allows us to perform surrogative inferences, where the mathematical inferences we make are analogous to inferences we could make in the target; the mathematical inferences 'mirror' the inferences we could make with the target. This is obviously an oversimplification, though this idea has some intuitive appeal, especially when the inferential approaches are explicated in terms of morphisms between structures and given what the proponents of the IC state.[27] A plausible response to the necessity of mathematics on these inferential lead accounts is that while the intra-vehicular mathematical inferences are necessary, the extra-vehicular inferences are not, i.e. the interpretation of the conclusions of surrogative inferences are not necessary.

[27]For example, that "by embedding certain features of the empirical world into a mathematical structure, it is possible to obtain inferences that would otherwise be extraordinarily hard (if not impossible) to obtain" (Bueno & Colyvan 2011, pg. 352).

## 4.4 The Structural Mapping Accounts & Faithful Epistemic Representation

Above I have argued that both the IC and the PMA can be accommodated within Contessa's approach, provided that we make some adjustments to the first understanding of Contessa I put forward in §3.4. The PMA fits nicely into the framework of Contessa's analytic interpretation. The IC does not have the separate relations required to supply epistemic representation, but does have the resources to provide an account of faithful epistemic representation. The adjustment required to accept accounts that provide faithful but not epistemic representation was rejecting that the interpretation and gradable faithfulness relations had to be distinct and accepting that one relation could play both roles. The case of the IC, this is the interpretation mapping.

In this section I will provide broad answers to the questions concerning faithful epistemic representation:

2. How does the account establish faithful epistemic representation? including:

   (2.a) How does it supply a gradable notion of faithfulness?

   (2.b) How does it account for the relationship between faithfulness and usefulness?

I have already given a broad answer to question (2): these accounts establish faithful epistemic representation through structural relations between the vehicle and the target. The details of what those structural relations are have already been given in the introduction to each account, but below I will briefly explain how these structural relations can answer question (2.a).

One of the arguments Suárez brought against the straw man isomorphism account in his (2003) was the argument from misrepresentation. I argued that there are four types of misrepresentation: two types of mistargeting, mistaken targets and non-existent targets; and two types of inaccuracy, empirically inadequate results, and due to abstraction and idealisation. Of these four types, I held only non-existent targets and inaccuracy due to abstraction and idealisation to be of particular relevance to accounts of scientific representation. I have already sketched a solution to the problem of non-existent targets. The problems caused by abstraction and idealisation require a lot of work to be dealt with adequately. They have a direct bearing on the issue of faithfulness, and on the relationship between faithfulness and usefulness. Contessa introduces the idea that faithfulness and usefulness do not always coincide and can in fact be opposed to each other due to considering Galilean idealisation (Contessa 2007, pg. 130-131). An adequate answer to (2.b) will therefore require a discussion of how each account deals with various types of idealisation. I pursue this line of investigation in Chapter 6 for the IC, and in Chapter 7 for the PMA. I provide an answer to (2.b) for the IC in Chapter 6. However an answer for the PMA has to wait until Chapter 8, as my investigation in Chapter 7 finds answering the question difficult for the PMA (despite its initial conformity with Contessa's analytic interpretation).

### 4.4.1 The Inferential Conception

The structural relations employed by the IC are those of the partial structures framework. These relations are often characterised as an ordered triple, $R = \langle R_1, R_2, R_3 \rangle$, where $R_1$ is the set of $n$-tuples that hold for $R$, $R_2$ is the set of $n$-tuples that do not hold for $R$ and $R_3$ is the set of $n$-tuples for which is it not known, or for which it is not defined, whether or not they belong to $R$.[28]

The first and strongest reason for adopting the partial structures framework when attempting to account for the gradability of faithfulness is that the partial

[28]See §6.2.1 for a more detailed explanation of the partial structures framework.

structures framework was introduced along with the notion of partial truth in order to account for the partial information we have in science. By adopting the view that scientific representations are (mostly) partially faithful epistemic representations, in that they lead to some sound and some unsound inferences to be drawn about their targets, one can see a direct parallel to how da Costa & French (2003) claim scientific models can be partially true.

The faithfulness of the idealised vehicle can be measured by comparing the $R_i$ of the vehicle to the $R_i$ of our target, in the same way the 'degree' of similarity or approximation of models can be measured (da Costa & French 2003, pg. 50-51), (da Costa & French 1990, pg. 261). For example, the greater the number of elements related by the $R_3$ component, the more 'partial' the structure is, and the less information we have, noting again that the $R_3$ component can be understood as containing those elements for which we do not know whether statements are satisfied or not.

There is a problem in understanding the partial structures' approach to gradability in terms of only the 'size' of the $R_3$ component. Once a partial structure has been 'filled out', or 'completed', by mapping all elements of $R_3$ to $R_1$ or $R_2$, we obtain the usual notion of structure. Yet this 'full' structure might still produce unsound inferences; that is, the 'full' structure might still be partially faithful. We have two options in response to this: either we are always using partial structures, as a full structure would be the complete structure of the situation under investigation; or we only obtain 'full' structures for restricted domains. I think that the first of these responses is the more likely, but do not have the space to argue for it here.[29]

The second benefit of adopting the partial structures framework is its (supposed) ability to deal with the abstractions and idealisations present in the ma-

[29] A sketch of why I favour the first response is that scientific (mathematical) representation seems to be one of the kinds of representation for which idealisation and abstraction, i.e. misrepresentation, is essential, and subsequently that restricted domains will involve idealisations and abstractions, i.e. that the second response will entail the first.

jority of scientific representations. A third, and related, benefit is the partial structures framework's capacity for dealing with inconsistencies in representational vehicles. Assuming that the world is not inconsistent (for the sake of argument), an inconsistency in a representational vehicle would be a serious case of unfaithfulness, a case that one might initially think results in a completely unfaithful representation. There are several instances of this not being the case,[30] most notably Bohr's model of the atom.

As I said above, an investigation into how idealisations are accommodated on the IC will wait until Chapter 6.[31] I will also briefly point to how the IC might deal with inconsistent representations, though I will also argue that cases such as Bohr's model of the atom should be dealt with by another part of the Semantic View.

### 4.4.2 The Mapping Account

On Contessa's approach, the partial faithfulness of a representation can be measured by the number of sound compared to unsound surrogative inferences a vehicle allows us to draw about a target. Unfortunately Pincock's approach to constructing his account of representation was to focus on accuracy conditions rather than surrogative inferences, making the evaluation of his account as an account of faithful epistemic representation difficult. Above I argued that despite this difference in approach to constructing his account and the problems it raised for understanding inference on the PMA, the account could be understood as an account of epistemic representation.[32] As the PMA includes a structural relation, which may take the form of any structural mapping or

[30]Or many cases if one adopts the position that idealisations can be understood as resulting in contradictions and therefore positing inconsistencies in the world Maddy (1992), Colyvan (2008).

[31]I will not address abstraction directly, as whatever is said for idealisation can be adapted to accommodate abstraction.

[32]In fact, the PMA actually appeared to fulfil the criteria of Contessa's analytic interpretation.

mappings that include mathematical terms, the account should *prima facíe* be capable of handling the gradability of faithfulness.

However, the focus on accuracy conditions appears to prevent this. This is for two reasons. First, although Pincock talks about representations setting accuracy conditions, he also talks about representations only being correct if there is a structural relation $M$ between the physical structure $S$ and the mathematical structure $S^*$, and incorrect if either of these structures do not exist, or the relation does not hold (Pincock 2012, pg. 28). Second, Pincock does not appear to follow a typical understanding of the notion of 'accuracy conditions' as gradable. He cashes out 'accuracy conditions' in terms of placing restrictions on the way the world must be for one to consider the representation to be accurate, i.e. a strong claim about the structure of the target systems. Pincock takes the descriptions of representations as "accurate" and "correct" to be synonymous. The most relevant consequence of this is that whether a representation is accurate/correct is a binary state of affairs: either it is correct or it isn't.

Fortunately we can understand the PMA as providing a gradable notion of faithfulness. This involves emphasising the various notions of content and the structural relation. These bring out two ways in which we can motivate a gradable notion of faithfulness on Pincock's account: one in line with the usual understanding of accuracy; another in line with the way in which Contessa aimed to provide an account of faithful epistemic representation by making use of different types of structural relations to accommodate the difference in the structure of target and vehicle (Contessa 2011, pg. 129-130). These two notions of faithfulness come from an ambiguity in how one can understand 'faithful'. As defined above, Contessa takes a partially faithful epistemic representation to be one which allows us to draw at least one sound inference from our representing vehicle to our target. What these inferences are is left unspecified. Thus we can understand the inference to be a prediction that a magnitude has a particular value, or that a target system has a particular property, or expresses a particular behaviour and so on. Only the first of these types of inferences can be understood in terms of the typical understanding of 'accuracy'. I will call this sort of inference *numeric faithfulness*. The other sort of faithfulness, the sort that concerns the structural similarity of vehicle and target, can be classed as *structural faithfulness*. Numerical faithfulness is just a consequence of misrepresentation due to empirically inadequate results and so does not need to be accommodated specifically by the PMA.

The structural faithfulness can be varied through the use of different structural relations and will involve considerations of the types of content involved. The idea here is that while the choice of a particular structural relation might produce an unfaithful representation, the choice of a different structural relation will produce a more faithful representation. For example, the choice between an isomorphism and an isomorphism with an error term in the heat equation representation (Pincock 2012, pg. 31). The intentions of the agent will play a role here, in selecting which structural relation to adopt.

Different structural relations and notions of content are involved in Pincock's approach to different types of idealisation. As such, a detailed account of how the PMA can answer question (2.b) will require an investigation into what Pincock has to say on idealisation. This will be pursued in Chapter 7.

## 4.5 Conclusion

In this chapter I have outlined the IC and the PMA, and argued that they fit into Contessa's approach of understanding scientific representation as a subspecies of (partially faithful) epistemic representation. The IC cannot supply an account of epistemic representation, but this is not a problem as we can adopt some other account of epistemic representation if required. As it seems plausible that all scientific representations are partially faithful epistemic representations (in that they at one time facilitated the drawing of at least one sound surrogative inference), we only need an account of faithful epistemic representation to account of mathematical scientific representation. I also argued that the IC had the resources to answer question (2.b). The PMA satisfies the conditions for analytic interpretations. There were some interpretative challenges to understanding the PMA as an account of faithful epistemic representation due to the approach to inferences Pincock advocated and his understanding of accuracy. These challenges could be met, though how the PMA will answer question (2.b) still needs to be explained.

I will now turn to setting out the salient points of my case study, the rainbow.

# 5 | The Rainbow: An Introduction

## 5.1 Introduction

In this chapter I will outline the history of the scientific investigation into the rainbow. The rainbow is an excellent case study for exploring issues related to representation, in particular the role of idealisation. It provides several different sorts of idealisations, as well as prompting interesting questions concerning the interpretations of the mathematics involved. It has been used by other philosophers to discuss issues such as reduction[1] and mathematical explanation.[2] It has also been used by Pincock[3] to discuss all of these issues and so grants a great insight into his account of representation.

I will focus on two parts of the mathematics involved. The idealisation of taking the 'size parameter'[4] to infinity, and the introduction of abstract mathematics in the Complex Angular Momentum (CAM) approach to finding an approximate solution to the Mie solution. The $\beta \to \infty$ idealisation is involved in a ray-theoretic explanation of the colour distribution of the rainbow, while the CAM approach is involved in an explanation of the supernumerary bows, an interference pattern that is sometimes seen inside the primary rainbow. These two pieces of mathematics provide useful examples for gaining further

[1] Batterman (2001)

[2] Batterman (2010) and Bueno & French (2012).

[3] Pincock (2012) Chapter 11.

[4] The size parameter is a dimensionless variable $\beta$ that describes the relationship between the wave number of the light incident on the water droplet and the radius of the water droplet.

insights into the Inferential Conception and Pincock's Mapping Account. I will return to these examples in subsequent chapters, but outline them here so that the reader gains a familiarity with them before I put them to philosophical work.

The history of the investigation into the rainbow can be roughly split into three. First, there was the ray theoretic approach that was able to provide apparent explanations for the angle of the rainbow and the distribution of colours within it. Here, light is represented as geometric rays and the rainbow is explained to be due to internal reflections of the these light rays within the water droplets. Next came the wave theoretic Mie solution. Mie was able to derive an exact solution of the scattering of an electromagnetic light wave by water droplets using Maxwell's equations. Unfortunately the Mie solution, expressed as a sum of partial wave terms, converged too slowly to provide practical results. This prompted a search for a way to transform the solution so that an approximate, but faster converging, form of the Mie solution could be found. As part of this search, an analogy was made with the scattering of quantum particles: light was represented via the mathematics used to represent the complex angular momentum of quantum particles. This move to the complex plane allowed for an approximate solution to be found, which now gives an accurate result for a very large range of physical situations. I will discuss the first and third of these parts of the investigation in much more detail below. I will only provide a sketch of the Mie solution. Full mathematical details can be found in Appendix A.

## 5.2 The Ray Theoretic Approach to The Rainbow Angle and Colours

The two most striking features of a rainbow, that it always appears at an angle of 42° to the observer and with a colour distribution of violet, blue through to orange and red from bottom to top, can be accounted for by the ray theory of light. Here light is considered to be a ray, rather than an electromagnetic wave. As such, it lacks various wave features: there is no wavelength, frequency and so on.

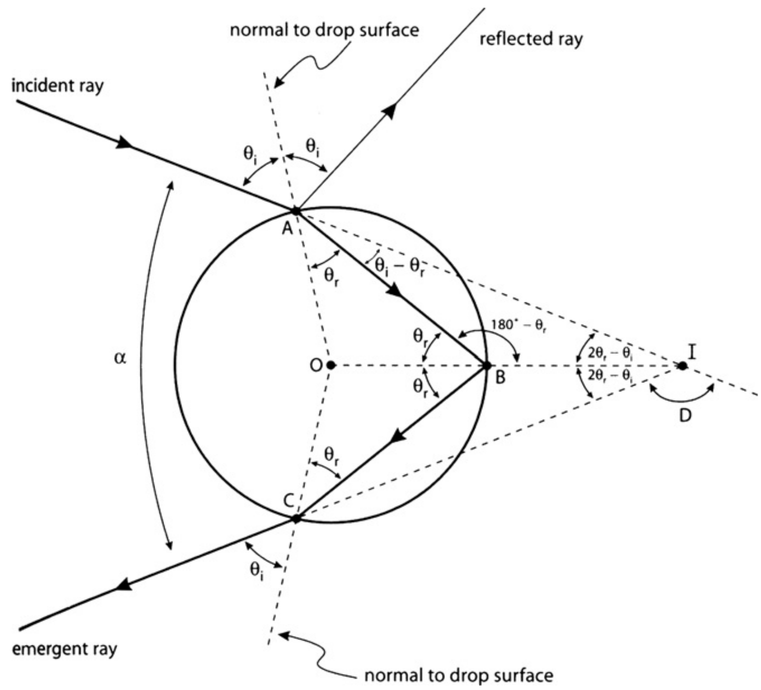


Figure 5.1: The basic geometry of a ray incident on a spherical water drop. The single internal reflection leads to the primary rainbow. $O$ is the centre of the water drop, $i$ the angle of incidence, $r$ the angle of reflection. Reproduced from Nahin (2004).

The angle of the rainbow can be explained through applying the claim that the angle of incidence equals the angle of reflection and Snell's Law to the path of a light beam that hits a water droplet. The light beam undergoes refraction, then internal reflection on the back surface of the water droplet, and finally refraction when exiting the raindrop. Snell's law and the geometry of the circular water droplet allows us to claim that the total angle of deflection of the light ray, $D$ is equal to:

$$D = 180^{\circ} + 2\theta_i - 4\sin^{1}\frac{1}{n}\sin\theta_i \tag{5.1}$$

where $\theta_i$ is the angle of incidence of the light ray on the water droplet. $D$, $\theta_i$, and $\alpha$, the observation angle, are those in Figure 5.1. The rainbow occurs at the angle that the majority of the light is refracted out of the water droplet (see Figure 5.2). That is, the rainbow occurs at the angle where $D$ is minimum and $\alpha$, as given in (5.2), obtains a maximum, for the light hitting the water droplet at angles $0^{\circ} < \theta_i < 90^{\circ}$.

$$\begin{aligned} \alpha &= 180^{\circ} - D \\ &= 2\theta_i - 4\sin^{1}\frac{1}{n}\sin\theta_i \end{aligned} \tag{5.2}$$

We find that the maximum value $\alpha$ may take is $42^{\circ}$, hence the angle of the rainbow. Further, as this is the maximum angle, no light will be seen at a greater angle than this: no light is refracted and reflected above the primary bow.

In order to account for the distribution of colours, we note that $n$, the refractive index, can vary depending upon the colour of the light incident on the water droplet. The above calculation for the maximum value of $\alpha$, at $42^{\circ}$, assumed that the light was white, giving a refractive index of $n = \frac{4}{3}$. But white light is composed of many colours of light, each of which has a different refractive index. Thus we can run the calculation for the maximum angle of reflection using the refractive index of red, orange, through to violet light and establish the maximum value $\alpha$ takes for each colour. Here we find that $\alpha$ for red light is $42.25^{\circ}$ and for violet light is $40.58^{\circ}$. As $\theta_{\alpha \text{ red}}$ is greater than $\theta_{\alpha \text{ violet}}$, the red band of light will be above the violet.[5]

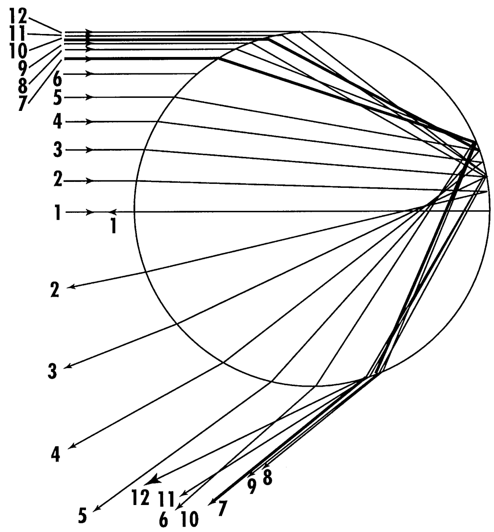


Figure 5.2: Diagram demonstrating that light groups around the minimum angle of deflection. The ray numbered 7 is the 'rainbow ray' and defines the minimum angle of deflection, i.e. the rainbow angle $\theta_R$. Reproduced from Adam (2002).

While the angle of the rainbow and the colour distribution can be explained by a purely ray theoretic account, as above, this explanation can be seen as lacking in certain respects. For example, the link between the colour of light and refractive index is one of stipulation, or being treated as a "brute fact" (Pincock 2012, pg. 226). Pincock argues that not only is the explanation for colour problematic in this way, so too is the explanation for the angle: the pure ray theoretic explanation leaves several parts of the explanation underdeveloped and unexplained itself. The solution is, roughly, that we can do better by adopting a wave theoretic position and obtaining the ray theoretic explanation through a transformation of the wave theoretic mathematics. More precisely, we should adopt a particular idealisation of the wave theory that results in a ray conception of light. This would then allow us to use the wave theoretic explanation for why refractive indices vary by the colour of light: refractive index

[5] The smaller angle for the violet light puts it on the inside of the reflected light cone, the red on the outside. Hence, for the top half of the cone from our line of sight, red is at the top of the rainbow, while violet is at the bottom.

depends upon the wavelength of the incident light, and wavelength determines the colour of light.

One way of performing this idealisation would be to take the wavelength of light to 0, which can also be understood as taking the wave number, $k = \frac{2\pi}{\lambda}$, to infinity. Unfortunately this idealisation leads to a representation that loses the most important piece of the wave theory for accounting for the varied refractive indices, the wavelengths of the differently coloured light. However, there are still some wave like behaviours at this limit. Batterman (making use of Berry (1981), and so providing a derivation of the ray representation that is amenable to catastrophe optics) claims that a ray representation obtained by defining rays to be normals to geometrical wavefronts displays such a feature. Here, a ray is defined as the gradient of the optical distance function, which is the Euclidean distance between the point of wavefront we take the ray to be normal to, and some point $P$, where we are attempting to approximate the wave via rays.[6] There will be many rays passing through this point due to the curvature of the wavefront, so it would be natural to consider the amplitude and phase of the resultant wave to be due to contributions from all of these waves. Using Fermat's principle, defining the phase of the $n^{th}$ ray to be $2\pi$ times the optical distance, $\phi$, from the wavefront to $P$ in units of wavelength, and approximating the wave intensity by considering the flux through an area, we obtain the shortwave approximation:

$$\psi(\mathbf{R}, z) \approx \sum_n \left| det \left\{ \frac{\partial r^n}{\partial \mathbf{R}} (\mathbf{R}, z) \right\} \right|^{\frac{1}{2}} e^{ik\phi_n(\mathbf{R},z)} \tag{5.3}$$

Berry[7] states that (Berry 1981, pg. 519-520):

> [a]s a shortwave (or, in quantum mechanics, semiclassical) approximation, [(5.3)] has considerable merit. Firstly, it describes the interference

[6]See Batterman (2001), Figure 6.4, pg. 85.
[7]As quoted by Batterman (2001), pg. 88.

> between the contributions of the rays through $\mathbf{R}, z$. Secondly, it shows that wave functions $\psi$ are non-analytic in $k$ as $k \to \infty$, so that shortwave approximations cannot be expressed as a series of powers of $\frac{1}{k}$, i.e. deviations from the shortwave limit cannot be obtained by perturbation theory. Thirdly, in the shortwave limit itself [$k = \infty$], $\psi$ oscillates infinitely fast as $\mathbf{R}$ varies and so can be said to average to zero if there is at least some imprecision in the measurement of $\mathbf{R}$ in the intensity $|\psi|^2$, the terms from [(5.3)] with different $n$ average to zero in this way, leaving the sum of squares of individual ray amplitudes, i.e.
>
> $$|(\mathbf{R}, z)|^2 = \sum n \left| det \left\{ \frac{\partial r^n}{\partial \mathbf{R}} (\mathbf{R}, z) \right\} \right| \tag{5.4}$$
>
> when $k = 8$ and this of course does not involve $k$

This case suggests that it is possible to find a ray representation that still has wave features present at the limit. However, this particular way of obtaining the ray representation fails, not only because we've taken $k \to \infty$, but also because the amplitude it describes becomes infinite on a caustic. Rainbows can be considered as caustics, so the shortwave approximation fails exactly where it is needed.[8]

We therefore need to find an idealisation that gives us a ray conception of light, yet allows us to refer to the wavelength of light in some way. The suggestion is that we use the size parameter, $\beta$. This is a dimensionless parameter, defined as the product of the radius of the water drop, $a$, with the wave number: $\beta = ka = \frac{2\pi}{\lambda}a$. Provided that the water droplet is large enough in comparison to the wavelength of the incident light, we can use this idealisation to describe the direction the light travels in with a ray. The idea is that the distance between the wave crests is sufficiently small compared to the radius of

[8]See Batterman (2001, §6.3 for this discussion. The shortwave approximation fails in terms of being unable to tell us how the intensity changes or the wave pattern (the fringe spacings) on or near a caustic as $k \to \infty$ (Batterman 2001, pg. 88).

the water droplet, such that they appear as if they form a continuous straight line. Thus we can still appeal to the wavelength of the light to account for the differences in refractive index.

Pincock claims that "when [Batterman] is being careful", he refers to this limit (as opposed to the $k \to \infty$ limit). Further, we can see that van de Hulst (1981) makes use of this limit. van de Hulst pioneered the use of the impact parameter for light scattering, which associates a partial wave with an incoming ray. This allows one to say that one has *localised* a partial wave to a ray. Thus I propose that any ray representation thats use of the notions of impact parameters or localised waves will make use of the $\beta \to \infty$ idealisation to obtain its ray representation. Finally, in comparing the accuracy of a ray representation to the Mie solution, Grandy Jr makes use of the $\beta \to \infty$ idealisation from the Mie solution (Grandy Jr 2005, §4.3). As the Mie solution for the rainbow is a wave theoretic solution, it is clear to me that we should adopt an idealisation that obtains the ray representation from the Mie solution.

How one justifies this idealisation, and how one accounts for the structural moves will depend upon which account of representation one adopts. In the next two chapters, this idealisation will be appealed to. In the case of the IC, it will be used as an example of the introduction of surplus structure due to a limit. For the PMA, it will be used to discuss Pincock's position of metaphysical agnosticism. This is a position Pincock offers as a third response to the predictive success of singular perturbations, after rejecting an instrumentalist position towards the mathematics and the metaphysical positions of reductionism and emergentism.

## 5.3 The Mie Solution

The Mie solution is arrived at by solving the Maxwell equations for the scattering of a plane wave of monochromatic light by a homogeneous sphere (the water droplet). First, the Maxwell equations are solved for waves in an arbitrary medium. These general results are then applied to the specific case of scattering of a plane wave by a homogeneous sphere. We find the potentials inside the sphere, and for the incident and scattered waves outside the sphere, and match coefficients. The scattered wave is then solved in the far field zone to give the Mie solution, (5.5).[9]

$$S_j(\beta,\theta) = \frac{1}{2}\sum_{n=1}^{\infty}\left\{\left[1 - S_n^{(j)}(\beta)\right]t_n(\cos\theta) + \left[1 - S_n^{(i)}(\beta)\right]p_n(\cos\theta)\right\} \tag{5.5}$$

$$(i, j = 1, 2 i \neq j)$$

The Mie solution gives the scattering amplitudes of the scattered wave in terms of the angular functions $p_n$ and $t_n$ and $S$-matrix elements $S_n^j$, where the $S$-matrix give the scattering of the partial waves in terms of phase shifts.

## 5.4 Supernumerary Bows & The CAM Approach

The explanation of the angle and colour distribution that depended on a ray conception of light, even the conception arrived at through the $\beta \to \infty$ idealisation, is incapable of providing an explanation of the supernumerary bows. These are bands of constructive and destructive interference sometimes seen inside the primary rainbow. The Mie solution is capable of providing an account of how these come to be, but the partial wave sum converges very slowly. It needs terms in the order of the size parameter, typically over 5000 terms, to produce a result which is sufficiently accurate, that allows for the later terms to be disregarded.

The solution to this problem is to transform the Mie solution in a form

[9] Full details of this derivation can be found in the Appendix, A, which draws on Nussenzveig (1992), Liou (2002), Adam (2002) and Grandy Jr (2005).

that converges much quicker, and thereby allowing us to 'extract the physics' from the solution. The approach taken is to apply the Poisson sum formula to individual terms of the Debye expansion. We apply it to each term individually as applying it directly to the Mie solution as a whole results in a series that converges at about the same rate as the Mie solution itself (as will be shown below). The Poisson sum formula, (5.6), allows for a infinite sum over a real function to be transformed into an integral over complex variables.

$$\sum_{l=0}^{\infty} \psi(\Lambda, \mathbf{r}) = \sum_{m=-\infty}^{\infty} (-1)^m \int_0^{\infty} \psi(\Lambda, \mathbf{r})\, e^{2im\pi\Lambda}\, \mathrm{d}\Lambda \tag{5.6}$$

This move facilitates the analogy to quantum scattering and the description of light as possessing a complex angular momentum. While a straightforward application of the Poisson sum formula does not require the move into the complex plane, this is required for generating the asymptotic approximations that are essential for the CAM approach. In summarising the history of this approach, Nussenzveig explains that Watson proposed a transform based on equation (5.7) (which can be shown to be equivalent to the Poisson sum formula).

$$\sum_{l=0}^{\infty} \phi\left(l + \frac{1}{2}, x\right) = \frac{1}{2} \int_C \phi(\lambda, x) \frac{e^{-i\pi\lambda}}{\cos \pi\lambda}\, \mathrm{d}\lambda \tag{5.7}$$

This includes a contour integral, the path for which was chosen to be about the real axis.[10] Once in the complex plane, one can deform the paths over which one is performing integrals, provided that one takes into account any possible singularities. The singularities met are the poles of the function. Poles are features of complex functions. The necessary concepts required to understand what they are outlined in §A.6. Briefly, poles are found when the $b_n$ coefficients of the Laurent series expansion of $f(z)$ are non-zero. The coefficient

[10] See Nussenzveig (1992), Figure 6.2, pg. 49.

$b_1$ is described as the residue of $f(z)$. When subjecting the scattering functions to the CAM approach, they are transformed into a form that include a residue *series*; they include a sum over all poles in the complex plane and the residue $r_n$ at pole $\lambda_n$. See, for example, Grandy's discussion of the application of the Watson transform to the partial wave series for quantum mechanical scattering:[11]

> [O]ne rewrites the partial-wave series as a contour integral:
>
> $$f\left(E, q^2\right) = \frac{1}{2i} \int_C \frac{(2l+1)\, f_l\,(E)\, P_l\,(-\cos\theta)}{\sin \pi l}\, \mathrm{d}l \tag{5.8}$$
>
> where the contour $C$ [surrounds the real axis, crossing the origin]. Analytic continuation is effected by deforming the contour to the vertical and closing it to the left in two quarter circles to avoid the bound-state poles. In doing so the contour 'sweeps across' a number of Regge poles, thereby picking up the corresponding residues. ... The result of this continuation is the *Watson transformation of the scattering amplitude*:
>
> $$f\left(E, q^2\right) = \frac{1}{2i} \int_{C'} (2l+1)\, f_l\,(E)\, \frac{P_l\,(-\cos\theta)}{\sin \pi l}\, \mathrm{d}l + \pi \sum_{i=1}^{n} \beta_i\,(E)\, P_{\eta_i(E)}\,(-\cos\theta) \tag{5.9}$$
>
> where the $\beta_i\,(E)$ are the residues of the integrand at the Regge poles with the Legendre function separated out. The first term on the right-hand side of (5.9) is referred to as the *background integral*, and the second is the *residue series.*

The poles of the scattering function in the complex plane are known as Regge poles. We concentrate on the Regge poles as the motivation behind the CAM approach, specifically the path deformations they allow us to perform. The idea is to (Nussenzveig 1992, pg. 62):

[11] Grandy Jr (2005), pg. 47-49.

> concentrate dominant contributions to the integrals into the neighborhood of a small number of points in the $\lambda$ plane, that will be referred to as *critical points*. The main types of critical points are *saddle points*, that can be real or complex, and *complex poles* such as the Regge poles

The saddle points (points where a curve in one direction has a maximum and a curve in another has a minimum) are the major contributions to the background integrals (when the integrand has saddle points) (Nussenzveig 1992, pg. 64). The contributions of these saddle points is found through the method of 'steepest descent'. This method will be outlined briefly below. It is found to be wanting in the initial application of the CAM approach, which leads to the development of the CFU method.

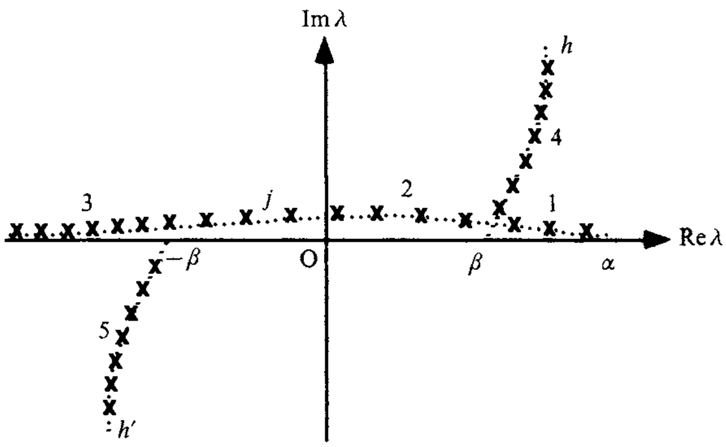


Figure 5.3: Regge poles for $N > 1$. The **x** represent the poles. Those in region 1 are those for narrow resonances, 2 for broad resonances, and 4 for surface waves. Reproduced from Nussenzveig (1992).

Applying the Poisson sum formula directly to the Mie solution and extending the result into the complex plane gives us the scattering function $S^{(j)}(\lambda, \beta)$, given in (5.10). The Regge poles are the roots of $\{1\beta\} - mv_j\{\alpha\} = 0$, the denominator of $S^{(j)}$. These Regge poles can be placed into two classes. The first of these are along the curves 4 and 5 in Figure 5.3. These are associated with surface waves. The first class of poles are of relevance below, once we have applied the Debye expansion, while the second class of poles are of immediate relevance. These are those found above the real axis along the curve $j$ in Figure 5.3. The separation of these poles is of the order 1, and their imaginary part is not large. As there are as many poles as $\mathcal{O}(\beta)$ in the relevant part of the complex plane for the residue series, the series over these poles will converge about as slowly as the partial wave series in the Mie solution. Clearly, we need a form of the Mie solution that produces a residues series that converges quicker than the partial wave series in the Mie solution, so applying the Poisson sum formula as we have, directly to the Mie solution, is not the way to go. Thus we turn to the Debye expansion of the Mie solution.

$$S^{(j)}(\lambda,\beta) = -\frac{\zeta_\lambda^{(2)}(\beta)}{\zeta_\lambda^{(1)}(\beta)}\left[\frac{\{2\beta\} - mv_j\{\alpha\}}{\{1\beta\} - mv_j\{\alpha\}}\right] \qquad j = 1,2 \tag{5.10}$$

$$[jz] = \ln'\zeta_\lambda^j(z) + \frac{1}{2z} \tag{5.11}$$

$$[z] = \ln'\psi_\lambda + \frac{1}{2z} \tag{5.12}$$

The Debye expansion involves representing the multipole of index $n$ in the partial wave sums (found in the scattering amplitude equations, (A.123) - (A.125)) in terms of incoming and outgoing spherical waves that are partially transmitted and partially reflected at the surface of the sphere (rather than the standing waves inside the sphere, and the scattered plane waves outside the sphere) (Grandy Jr 2005, pg. 144) (Adam 2002, pg. 283). As the Debye expansion involves modelling the waves as partially transmitted and reflected waves, we write the scattering function in terms of spherical reflection and transmission coefficients (themselves expressed in terms of Henkel and Bessel functions). For example, the external spherical reflection coefficient is given in (5.14).[12] The rest of the coefficients are given in equations (A.145) - (A.148) in the Appendix.

[12]Note the difference in the definitions of $[j]$ and $[jz]$. This difference is because these equations have not yet been extended to the $\lambda$ plane.

$$S_n^{(j)} = R_n^{22} + T_n^{21} \sum_{p=1}^{\infty} \left(R_n^{11}\right)^{p-1} T_n^{12} \tag{5.13}$$

$$R_n^{22}(\beta) \equiv -\frac{\zeta_n^{(2)}(\beta)}{\zeta_n^{(1)}(\beta)} \frac{[2\beta] - mv_j[2m\beta]}{[1\beta] - mv_j[2m\beta]} \tag{5.14}$$

$$S_{(j)}(\beta,\theta) = S_{j,0}(\beta,0) + \sum_{p=1}^{P} S_{j,p}(\beta,\theta) + \text{remainder} \tag{5.15}$$

$$[jz] = \ln' \zeta_\lambda^j(z) + \frac{1}{2}z^{-1} \tag{5.16}$$

$$[z] = \ln' \psi_\lambda + \frac{1}{2}z^{-1} \tag{5.17}$$

The Debye expansion of the total scattering amplitude, in terms of the spherical reflection and transmission coefficients in (5.13). It is also given in terms of the scattering amplitudes, is given in (5.15). $S_{j,0}$ is the direct reflection term, $S_{j1}$ is the direct transmission term, and $S_{jp}$, where $p \geq 2$ corresponds to transmission following $p-1$ internal reflections. We now apply the Poisson sum formula to each individual term. It is the $p = 2$ term that we concentrate on, as it is responsible for the primary rainbow (as it is the term for one internal reflection). We find a rapidly converging residue series for these terms. Here, rather than Regge poles, we talk of Regge-Debye poles (to reflect that we are dealing with the Debye expansion of the Mie solution). These poles are found from the roots of the equation $\{1\beta\} - mv_j\{2\alpha\} = 0$, which is the common denominator of all of the spherical transmission and reflection coefficients. This equation differs from the one used to find the Regge poles when we applied the Poisson sum formula to the Mie solution by the substitution $\{\alpha\} \to \{2\alpha\}$, which reflects the change from standing waves to ingoing waves within the sphere. When we look at the poles for the Debye expansion, we find only poles of the first type outlined above (Nussenzveig 1992, pg. 93-94). We also obtain a new set of poles, found in the second quadrant of the complex plane, which have a large imaginary part compared to the corresponding poles in the first quadrant.

One can see the Regge-Debye poles in Figure 5.4. Thus we expect that the residue series contributions from the Regge-Debye poles to become rapidly damped as $n$ increases, and so we can obtain rapidly convergent asymptotic expansions for each term of the Debye series through the CAM approach.

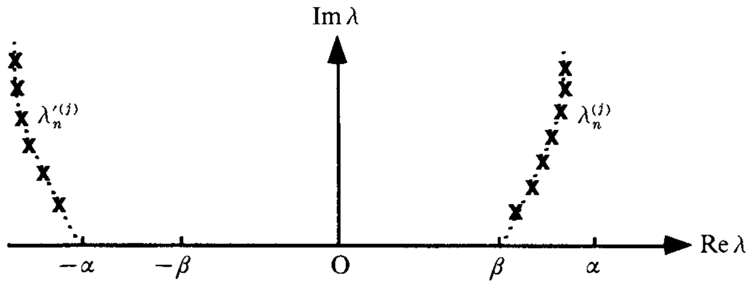


Figure 5.4: Regge-Debye poles for $N > 1$. Reproduced from Nussenzveig (1992).

When the CAM approach was first developed, it was non-uniform for all $m > 1$ and for the domain $0 \leq \theta \leq \pi$ (Nussenzveig 1992, pg. 101). The domain $\theta_R < \theta < \theta_L$ is covered twice (i.e. has incident light refracted at this angle from two separate incident rays/waves), where $\theta_R$ is the rainbow angle and $\theta_L$ is the maximum deflection angle (at $\theta_L = 4\cos^{-1}\frac{1}{m}$), leading to a subdivision at the geometrical-optic level into three angular regions: the 0-ray (geometrical shadow) region $0 \leq \theta < \theta_R$; the 2-ray region $\theta_R < \theta < \theta_L$; and the 1-ray region $\theta_L < \theta \leq \pi$. In each region, the Poisson transformed scattering amplitude is reduced to a background integral, which typically has contributions dominated by the saddle points and a Regge-Debye pole residue series (Nussenzveig 1992, pg. 102). We find that as $\theta$ increases from $\theta = \theta_L$ towards $\theta = \theta_R$, there are two saddle points which converge and meet at $\theta = \theta_R$. The rainbow is thus described in the $\lambda$ plane as the collision between two saddle points. A reason for the non-uniformity of the CAM approach is that the method for establishing the contributions due to the saddle points to the background integral breaks down when the ranges of the saddle points overlap. The notion of the range of a saddle point is introduced as, for asymptotic purposes, one does not always have to pass through the exact saddle point, but through an approximation to it. The notion of the range of the saddle point is supposed to clarify what deviations are allowable. de Bruijn offers the following informal definition: "if $\zeta$ is a saddle point of the function $\psi$, then the range of $\zeta$ is a circular neighbourhood of $\zeta$, consisting of all $z$-values which are such that $\left|\psi''(\zeta)\,(z-\zeta)^2\right|$ is not very large" (de Bruijn 1958, pg. 91).

The Chester-Friedmann-Ursell (CPU) method is an answer to the question "what happens in the saddle-point method when two saddle points approach one another?" (Nussenzveig 1992, pg. 105). The original approach to providing an answer to this question involves the method of steepest descent. One considers a general complex integral, such as in (5.18). Here the integral is over some path in the $w$ plane, where $\kappa$ is an asymptotic expansion parameter (like $\beta$) and is large and positive, and $\varepsilon$ is an independent parameter (like $\theta$). If the integral is dominated by a single saddle point $\bar{w} = \bar{w}(\varepsilon)$, around which $f$ and $g$ are regular, one can approximate in this neighbourhood (5.19) where $f''_w$ denotes the second derivative with respect to $w$.

$$F(\kappa,\varepsilon) = \int g(w)e^{[\kappa f(w,\varepsilon)]}\,\mathrm{d}w \tag{5.18}$$

$$f(w,\varepsilon) \approx f(\bar{w},\varepsilon) + \frac{1}{2}f''_w(\bar{w},\varepsilon)(w-\bar{w})^2 \tag{5.19}$$

$$f(w,\varepsilon) = \frac{1}{3}u^3 - \zeta(\varepsilon)u + A(\varepsilon) \tag{5.20}$$

Choosing the steepest decent through $\bar{w}$ alters the relevant part of the integral to a Gaussian type. An asymptotic expansion of the integral can be obtained by adopting a change of variables that makes the exponent into an exact Gaussian, followed by a power series expansion of $g$ around the saddle point and integration term by term (Nussenzveig 1992, pg. 106). If there are two saddle points, $\bar{w}'$ and $\bar{w}''$, as in the 2-ray region, then their contributions can be added independently *provided that their ranges do not overlap*, which is what occurs when the two saddle points collide near $\theta_R$. Thus, we need to adopt the CFU method to establish what happens at $\theta_R$.

The basic idea of the CFU method is to transform the exponent of (5.19) into an exact cubic through a change of variables, e.g. (5.20). There the two saddle points have been transformed into $\pm\zeta^{\frac{1}{2}}(\varepsilon)$. The crucial point is to introduce "a mapping that preserves the saddle point structure". Substituting into (5.18) and suitable expansion of the integrand allows one to obtain an equation for $F(\kappa,\varepsilon)$ in terms of the Airy function, $Ai$.[13] Making the relevant substitutions to apply the general equation (5.18) to the rainbow case allows us to write the dominant contribution to the third Debye term in the rainbow region in the form of (6.1):

$$S_{j2}(\beta,\theta) \approx \beta^{\frac{7}{6}} e^{[2\beta A(\varepsilon)]} \Big\{ \big[c_{j0}(\varepsilon) + \beta^{-1} c_{j1}(\varepsilon) + \dots\big] Ai\Big[(2\beta)^{\frac{2}{3}}\zeta(\varepsilon)\Big] + \big[d_{j0}(\varepsilon) + \beta^{-1} d_{j1}(\varepsilon) + \dots\big] \beta^{-\frac{1}{3}} Ai'\Big[(2\beta)^{\frac{2}{3}}\zeta(\varepsilon)\Big]\Big\} \tag{5.21}$$

where the CFU coefficients $c_{jn}(\varepsilon)$ and $d_{jn}$ can be expressed in terms of the Fresnel reflection and transmission coefficients and their derivatives of various orders with respect to the angle of incidence, evaluated at the exactly known saddle points $\theta_1', \theta_1''$ (the order of the derivatives increases with $n$) (Nussenzveig 1992, pg. 108). The CFU method allows for the CAM approach to be applied uniformly for the whole range of $\theta$.

## 5.5 Philosophical Responses

The two main contributions to the solutions found via the CAM approach are those provided by the critical points, the saddle points and the Regge-Debye poles. Throughout the derivations, we find references to the Regge-Debye poles

[13]The Airy function originates from Airy's treatment of the rainbow, which made use of a cubic wave front. For more details see (Adam 2002, §2.1), (Nussenzveig 1992, §3.2), Grandy Jr (2005, Appendix B), etc.

as being interpreted in terms of surface wave contributions.[14] This can either be understood wave-theoretically, as waves that travel along the surface of the drop rather than enter into the water droplet, or ray-theoretically as rays that take shortcuts by being critically refracted along the edge of the water droplet before being critically refracted back into the droplet. The saddle points are often said to represent rays,[15] though real saddle points can also be interpreted in terms of points of stationary phase.[16].

Exactly which interpretation one should adopt, and how that interpretation is to be described in terms of which piece of mathematics is related to something else (more mathematics, the world, etc.) will vary according to which account of representation one adopts. For the IC, the CAM approach provides another, though different, approach to surplus structure than the $\beta \to \infty$ idealisation appears to. The CAM approach is a clear case of moving to a different mathematical model which will have to be related to the first mathematical model before a physical interpretation can be found. The application of the Poisson sum transformation and the work with saddle points and Regge-Debye poles in the complex plane appears to be a use of surplus structure. The saddle points and Regge-Debye poles will have to be mapped back to something in the wave model before they can be given a physical interpretation. The CAM approach is therefore an example of the iterative version of the IC, outlined in Bueno & French (2012)

Pincock aims to distinguish his position from Batterman's. Pincock claims that interpreting the saddle points requires both ray and wave theoretic concepts, which commits us to the ray theoretic concepts, but not some metaphysical commitment to rays in a way he claims that Batterman's position requires, in terms of some kind of emergence. Thus the CAM approach, the

[14]Nussenzveig (1992, pg. 89-90); Adam (2002, pg. 282); Grandy Jr (2005, pg. 54).
[15]Nussenzveig (1992, pg. 94, 102-107); Adam (2002, pg. 242-243, 284, 287); Grandy Jr (2005, pg. 143, 159, 170-181).
[16]Nussenzveig (1992, pg. 50); Grandy Jr (2005, pg. 39)

move to the complex plane and the way saddle points are physical interpreted (either directly or by proxy) provides a point of contrast between the PMA and Batterman, which will hopefully allow for a greater understanding of Pincock's metaphysical agnosticism. I will argue that, although the rainbow does not involve a case of a singular perturbation, the moves involved are very similar and so can be used to gain information on metaphysical agnosticism.

I will now turn to establishing how the IC and PMA can accommodate faithful epistemic representation in detail.

# 6 | The Inferential Conception and the Rainbow

## 6.1 Introduction

In Chapter 4 I argued in favour of understanding the Inferential Conception and Pincock's Mapping Account as accounts of faithful epistemic representation. I also argued that a key to the success of these accounts would be how they accommodate idealisations. In this chapter and the next I will explore how the two accounts aim to do this, focusing on two issues. The first issue is the question (2.b), 'how do the putative accounts of faithful epistemic representation account for the relationship between faithfulness and usefulness?' Answering question (2.b) will involve answering the following type of questions: what do the accounts have to say about how these two features are related, if anything? Can they provide any explanation for how a less faithful representation can be more useful than a more faithful one? And if so, is this an explanation that can be generalised to all types of idealisations,[1] or do

[1]The issue of how many 'types' of idealisation is an open and active one in the literature. McMullin (1985) offers a defence of Galilean idealisation, while Jones (2005) argues that some of these sorts of idealisations would be better accommodated as abstractions under his scheme. Weisberg (2013) offers a different way of classifying idealisations, according to which he distinguishes three types of idealisation. Rohwer & Rice (2013) offer another type of idealisation consistent with Weisberg's scheme and raise the possibility of there being even more. They also, correctly, criticise Weisberg's classifying the idealisations focused on by Batterman as 'minimalist' idealisations (ones that isolate a single cause); they cannot be such idealisations as Batterman explicitly denies he is identifying a causal feature of the systems under investigation (Batterman 2001, §8.4). In this discussion I will be focusing

the differences between Galilean idealisations and singular limit idealisations prevent the explanation from being generalised? The second issue concerns the structural resources used by the accounts to accommodate the idealisations. Are the same structural resources used to accommodate different types of idealisations?

I will find problems with both accounts in my investigation of these issues. In this chapter I will be focusing on the IC, and I will look at the PMA in the next chapter. The IC's use of the partial structures framework is initially promising. I will argue that the partial relations between partial structures and the notion of partial truth are well suited to accommodating the variable notion of faithfulness, and its relationship to the usefulness of a representation. However, I will identify a lack of consistency of how the $R_3$ component is employed across examples that are supposedly explanatory of how the partial structures framework functions. This will lead me to identify a problem with how to understand surplus structure on the IC. In attempting to answer this question, I will identify four different types of surplus structure. I will argue that these can be restricted to different parts of the SV and IC. This restriction will result in a different way in which the partial structures framework is to be employed in scientific representation in general[2] and to how I believe it has to be accommodated when the partial structures framework is used in *mathematical* representation.[3] Although this proposal looks promising, I con-

on Galilean and singular limit idealisations as they pose the most interesting philosophical questions.

[2]By 'in general' I mean instances where the representational vehicles involve interpreted mathematics. This instance of scientific representation will be contrasted to the instances where we are concerned with how uninterpreted mathematics can represent the world. In the general scientific instances, the introduction of surplus structure is *prima facíe* understood as a move to uninterpreted mathematics. It is the lack of interpretation of the mathematics that will generate the problem for understanding surplus structure on the IC, though the situation is move complicated than a simple more to uninterpreted mathematics, as outlined in footnote 3.

[3]What constitutes surplus structure is more involved than the brief comment in the previous footnote conveys. It requires "'new' structure which is genuinely 'surplus' and which supplies new mathematical resources" (French 1999, pg. 188). French characterises this as the embedding of the original structure into a *family* of further structures, an insight at-

struct a dilemma for one type of surplus structure that the IC should be able to accommodate. This dilemma needs to be answered for the IC to be an acceptable account of mathematical representation; I outline several possible solutions.

To finish my discussion of the problems of the IC and the PMA I will return to the features of the rainbow I identified in the previous chapter: the singular limit of taking the size parameter to infinity, and the introduction of complex numbers in the CAM approach. In this chapter, I will discuss these features as possible examples of the introduction of surplus structure. They will provide a test case for the solutions I outline in response to problems raised in this chapter.

## 6.2 The Inferential Conception and Idealisation

The Inferential Conception's account of idealisation rests upon the partial structures framework. It employs both aspects of this framework: the notion of partial structures (and relations) and the notion of partial truth. I will address how the notion of partial truth should be employed by the IC before turning to the notion of partial structures. I argue that the notion of partial truth should only be employed at the end of an application of the IC, after the interpretation step. This raises the question of whether partial structures should be employed during the derivation step of the IC, which subsequently raises an issue for how to properly understand the notion of surplus structure.

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tributed to Octávio Bueno. This move from a structure to a family of further structures does not change the point, however, that the structures must still be uninterpreted mathematics. This is made clear through French's later discussion of how group theory provides us with more structure that is ruled out by physical laws (French 1999, pg. 195) and the discussion of the relationship between group theory and quantum mechanics. Further evidence is given in Bueno and French's discussion of surplus structure: "we typically have surplus structure at the mathematical level, so only *some* structure is brought *from mathematics to physics*" (Bueno & French 2012, pg. 88, second emphasis mine).

### 6.2.1 Partial Truth and Idealisation

Proponents of the Semantic View (SV) of theories who employ the partial structures framework support a view of idealisation that involves 'as if' description, where idealisations are taken as if they were true. This view of idealisation can be underpinned by the notion of partial truth.[4] The idea is that an idealisation, although strictly false, can be considered true within a restricted domain, and this is exactly what the notion of partial truth is supposed to capture (da Costa & French 2003, pg. 19):

> [W]e say that $S$ is pragmatically true in the structure $\mathcal{A}$ if there exists an $\mathcal{A}$-normal $\mathcal{B}$ in which $S$ is true, in the correspondence sense. If § is not pragmatically true in $\mathcal{A}$ according to $\mathcal{B}$, then § is said to be pragmatically false in $\mathcal{A}$ according to $\mathcal{B}$.

This view of idealisation is readily applicable to both idealised theories and terms, which can be thought of as "idealizing descriptions laid down within a theoretical context" (da Costa & French 2003, pg. 163). For example, to "describe an electron as if it were a point particle is to lay down a bundle of properties that have meaning only within a model or, more generally, a structure".

Thus, provided that all idealisations can be understood as 'as if' descriptions, then partial truth can be used to explain why an idealisation might be useful despite being partially faithful. Provided that the context within which the idealisation is being used allows the idealisation to be considered 'as if' it were true, i.e. as partially true, then we have an explanation for why the idealisation is useful: within the context the idealisation is true.

Further, the 'partiality' of the truth gives a measure of how faithful the representation is. However, this must be measured external to the theoretical

[4]See: French & Ladyman (1998), pg. 56-58; da Costa & French (2003), pg. 163; and §4.2.1 - *The Partial Structures Framework*.

context, in that the representation must be compared to other representations to gage how partially true it is. Thus we can see how faithfulness and usefulness are related, though come apart: one is external to the theoretical context of the idealisation (the faithfulness, how partially true the idealisation is compared to the target) and the other is internal to the context (the usefulness, that the idealisation *is* partially true within the context).[5] The threat to this view comes from idealisations which we cannot consider as 'as if' descriptions. Galilean idealisations can be straightforwardly considered as 'as if' descriptions. It is not clear that the same can be said for singular limit idealisations.

Galilean idealisations were first introduced by McMullin (1985).[6] They can be generally characterised as "a deliberate simplifying of something complicated (a situation, a concept, etc.) with a view to achieving at least a partial understanding of that thing" (McMullin 1985, pg. 248) and involves "assuming as true ... a proposition that is false" (McMullin 1985, pg. 255). McMullin goes on to identify two places in which these simplifications can occur, either the *conceptual representation* of the target (i.e. the representational vehicle, the model) or the "*problem situation*" itself, and two different ways in which these simplifications can occur in each place. Altering the *conceptual representation* results in *construct idealisation*. This alteration can either occur due to features known to be relevant to the situation being represented by the model being simplified or omitted, which would be a case of *formal idealisa-*

[5] This distinction is how partial truth is used when discussing theories. Partial truth is related to the 'intrinsic' characterisation of theories, whereas the 'extrinsic' perspective concerns the relationship between theories themselves, and between theories and the world. Strictly, the models cannot be considered partially true from the extrinsic perspective. When I said the partiality of the truth of the representations could be compared, I meant that the size of the $R_i$ could be compared. I will sketch this idea in the next section. See da Costa & French (2003) and French & Saatsi (2006) for more on the extrinsic/intrinsic perspective distinction.

[6] I will be using the general characterisation of Galilean idealisation below. Critical developments and other characterisations of Galilean idealisation can be found in the references listed in footnote 1.

*tion*, or through irrelevant features being left unspecified, which would be a case of *material idealisation.* Altering the *problem situation* results in *casual idealisation.* This involves the "move from the complexity of nature to the specially contrived order of the experiments" and can occur either literally or through thought experiments (McMullin 1985, pg. 365). These idealisations are justified pragmatically. This is borne out in the idea of deidealisation, the "adding back" of theoretical corrections which cannot be *ad hoc* if they are to be justified deidealisations (McMullin 1985, pg. 259). The idea is that Galilean idealisation should be capable of being deidealised given sufficient computational power and sophisticated mathematical techniques.[7]

Singular limits are limits that result in a mathematical discontinuity, an asymptote. They are the main focus of the work of Batterman.[8] The most important difference between idealisations due to singular limits and Galilean idealisations, at least as argued for by Batterman, is that the singular limit is essential to the idealisation. One cannot represent the situation properly if one attempts to deidealise by stepping back from the limit. There is much debate over the status one should grant to the models that result from taking a singular limit. Batterman holds them to play essential explanatory roles[9] whereas proponents of the partial structures approach often claim that the resulting structures are cases of surplus structure[10] (Bueno & French 2012, pg. 91). It is this claim that the structures which result from singular limits to be surplus structures that causes a problem for the IC. But to see this problem, we must take a step back.

In the above introduction of the 'as if' view of idealisations I mentioned

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[7] See Weisberg (2013).

[8] See Batterman (2001, 2005b, 2008, 2010) amongst others.

[9] More precisely, the asymptotic limits produce "structures and properties [which] play *essential explanatory roles*" (Batterman 2001, pg. 21).

[10] They also argue that in the absence of an appropriate account of explanation the claim that such structures do play an indispensable explanatory role cannot be sustained (Bueno & French 2012, pg. 101-103).

that theoretical terms could be understood as descriptions "laid down within a *theoretical context*" (da Costa & French 2003, pg. 163, my emphasis). When discussing idealisation in the context of scientific representation in general, as is typical for discussions of how the partial structures facilitates the SV, the theoretical context is provided by the interpretation of the model. For example, take the idealisation of a cannon ball as only being under the influence of gravity in a typical projectile problem in Newtonian Mechanics. The equations used are interpreted in terms of Newtonian Mechanics. $F$, $m$, $a$ and so on have the interpretation of Newtonian force, mass and acceleration. When we are mapping to an uninterpreted mathematical structure, however, there is no theoretical context.

In fact, this lack of theoretical context is one of the benefits of moving to surplus structures: it allows us to identify structural features, or relations within the lower model that we could not without the extra mathematical resources, as in the case of the relationship between quantum mechanics and group theory (French 1999, §3). Group theory is useful for describing the symmetry of systems. By understanding quantum states as displaying certain symmetries, group theory can be introduced in order to give a more fundamental understanding of the symmetries. "An atom or an ion, whose nucleus is considered as a fixed center of force $O$, possesses two kinds of symmetry properties: (1) the laws governing it are spherically symmetric, i.e., invariant under arbitrary rotation about $O$; (2) it is invariant under permutation of its $f$ electrons" (Weyl 1968, pg. 268), quoted in (French 1999, pg. 194-195). French explains that the symmetries of type (1) are described by the rotation group, while those of type (2) are described by the finite symmetric group of all $f!$ permutations of $f$ things.

With symmetries of type (2) we find that the appropriate system space for two indistinguishable individuals is reducible into two independent subspaces, which are symmetric or anti-symmetric (French 1999, pg. 197). We have surplus structure both from the 'mix-symmetry' state functions that describe the parastatistics, and if the state is one of bosons, then the fermion subspaces can be considered surplus structure and vice versa (French 1999, pg. 198). Whether the quantum state is of bosons or fermions, however, requires a map back to the empirical set up to establish which subspace is empirically adequate. i.e. we do not establish what structure is surplus until we have a theoretical context. Thus, a move to surplus structure cannot strictly be called an idealisation, at least while we are within the mathematical domain, as there is no theoretical context to provide the 'as if' description.[11]

Being unable to call a move to surplus structure an idealisation until we move back to an interpreted model does not seem to be problematic when partial structures are used for the Semantic View. However, it does seem problematic when using partial structures to account for the applicability of mathematics via the IC. According to the IC, there are only two places where the mathematics has an interpretation: the initial and the final empirical set ups. The immersion mapping takes us into the realm of uninterpreted mathematics, and the immersion step takes us back to an interpreted (in terms of the world, or scientific context) structure. This means that during the application of the IC, we cannot speak of there being an idealisation once we have performed the immersion mapping, until we perform the interpretation mapping to an empirical set up. So strictly on the IC, the only time we have an idealisation is once we have obtained the final empirical set up, and *this*, the interpreted (partial) structure of the empirical set up, is what we should refer to when talking of an idealisation or idealised model on the IC.

This raises two further problems. First, *strictly speaking*, we do not have

[11]In the case of (1), there are many more representations of the rotation group than physically occur; hence we have surplus structure from group theory (French 1999, pg. 195). However, identifying what structure is surplus and what is physically significant requires reinterpreting the mathematics, that is, providing a theoretical context for it.

idealised representations on the IC. Our representational vehicles are mathematical structures and the targets are the empirical set ups. The IC accounts for how mathematics can be used to obtain surrogative inferences about empirical set ups. How, then, do we have idealised representations of the world? The answer to this is to recognise that the empirical set ups are the representational vehicles, i.e. the models, of the SV. As such they can be considered idealised representations when they are acting as vehicles for the SV, when it provides an account of how scientific models represent the world.

The second problem is whether surplus structure makes sense within the IC. Bueno's and French's response to Batterman's position on singular limits is to iterate the immersion mapping and consider the second mathematical model to be surplus structure.[12] This response is not possible if surplus structure is the move from an interpreted mathematical model to uninterpreted mathematics, as the first immersion mapping has already taken us to an uninterpreted mathematical model. I will discuss this problem further below, after looking at different roles partial structures have in the SV and the IC.

### 6.2.2 Partial Structures and Idealisations

Partial truth requires partial structures, but partial structures can provide more than partial truth. They are employed in solutions to problems in major areas in the philosophy of science, such as the structure of theories, inter-theory relationships, the theory-data-phenomena relationship, and of course the applicability of mathematics. One can see that there are many more motivations for adopting the partial structure framework than the associated notion of partial truth.

One such motivation is to ground the notion of faithfulness. Above I argued that partial truth could be used to explain how the partial faithfulness and

[12] Bueno & French (2012) provide this response to Batterman (2010).

usefulness of representations could be related. This explanation was vague, talking of 'how partially true' an idealisation is as reflecting the faithfulness of the representation. This notion can be made more precise by looking at the (partial) relations between the (partial) structures that describe the idealised model and the empirical set up, world, or whatever we are judging the faithfulness of the model against (i.e. the target). The faithfulness of the idealised model can be measured by comparing the $R_i$ of the model to the $R_i$ of our target, in the same way the 'degree' of similarity or approximation of models can be measured (da Costa & French 2003, pg. 50-51), (da Costa & French 1990, pg. 261).

We can further supplement the explanation of why idealised models are useful with the partial structures framework. In addition to the theoretical context setting down what we are happy to consider 'as if' it were true, the relationship between the target and the idealised model (a relationship that might compose numerous partial structures and partial mappings) allows us to see how that idealised model has some link to the target, yet be strictly false when compared to the target directly. Thus the partial structures framework provides an explanation, and a formal grounding for the explanation, of why partially faithful models can be extremely useful.

As well as discussing the role that partial structures can play as a whole, we can identify the roles that the individual components can play:

- $R_1$ Similarities between models, or between the empirical set up and the mathematical structures.

- $R_2$ Dissimilarities between models, or between the empirical set up and the mathematical structures.

- $R_3$ Providing the 'openness' of structure which allows for the introduction of group theory as surplus structure (Bueno et al. 2002, pg. 514); "mis-

> matches" between the empirical set up and the mathematical structure when using the partial structures as a *framework* for accommodating idealisations (Bueno & Colyvan 2011, pg. 359); and housing the mathematical "technique" of the renormalization group (Bueno & French 2012, pg. 103).

The descriptions of what the $R_3$ component can be used for appear to be confused. The first two roles indicate that the 'openness' of a partial structure comes from the $R_3$ component, but this 'openness' is cashed out in two very different ways.

In the group theory case, this 'openness' involves the introduction of genuinely new structure, i.e. surplus structure. Here group theory is appealed to as a way of providing a greater understanding of the Bose-Einstein statistics initially used to describe superfluidity. In attempting to provide a model for the observed behaviour of liquid helium, London drew an analogy with a Bose-Einstein ideal gas. Bueno et al. argue that it is the structural similarity between the λ-point of helium (the phase transition that liquid helium undergoes at $2.19°K$) represented graphically and the discontinuity in the derivative of the specific heat associated with a Bose-Einstein condensation that provides this analogy, and the introduction (at a higher 'level') of the Bose-Einstein statistics (Bueno et al. 2002, pg. 512). What is important for the present discussion is that they claim the introduction of group theory allows for an understanding of the Bose-Einstein statistics via considerations of symmetry, the symmetry being represented group-theoretically (Bueno et al. 2002, pg. 514). The particular features of group theory that are appealed to are the existence of further "bridges" within the mathematics of group theory, specifically the reciprocity relationship between the permutation group and the group of all homogenous linear transformations (Bueno et al. 2002, pg. 508) (French 1999, pg. 199). The idea here, then, is that the 'openness' involves the use of mathematics to gain a greater understanding of the structural relationships within the original model by moving to a domain where we can explore the structural relationships between the uninterpreted mathematics more easily than we can between the interpreted mathematics, the moves being restricted or obscured by the physical interpretation.

However, in the framework case, the 'openness' is cashed out in terms of mismatches between the target and the ('idealised') mathematical structure acting as the representational vehicle (Bueno & Colyvan 2011, pg. 359):

> The $R_1$ and $R_2$ components of the partial relations in the empirical set up are mapped via the relevant partial isomorphism or partial homomorphism into the corresponding partial relations in the mathematical structure. However, the $R_3$ components are left open. These components correspond to the features of the idealization that bring mismatches between the actual empirical world and the mathematical model.

This quotation seems to indicate that all idealisations should be located in the $R_3$ component of the partial structure. Yet this component is supposed to include statements for which we do not know whether they hold in the structure.[13] This causes a problem, as we can see if we pay attention to the details of the economics example Bueno and Colyvan use to motivate their claims that we can use the partial structures approach as a framework for idealisations. This example is the attempt to model the rationality of agents involved in running a business, where the goal is to maximise profits. One of the idealisations involved is the 'quantity idealisation': the quantity produced, $q_s$, is assumed to equal the quantity demanded, $q_d$ (which is a function of price: $q_d = D(p)$): $q_s = q_d = q$. This allows us to calculate the profit as the difference between the gross receipt, $R$, and the cost of production, $C$.[14] i.e. Profit =

[13] See the introduction of the partial structures framework in §4.2.1 - *The Partial Structures Framework*, and da Costa & French (2003).

[14] The gross receipt is the product of the price and quantity demanded $R = pq_d$. The cost of production is a function of the quantity produced: $C = C(q_s)$.

$R - C = pq = C(q)$.[15] As the quantity idealisation is an explicit statement about the model, it appears to be a statement that we do know is satisfied, and so should be placed in $R_1$. This immediately contradicts the claim in the quotation that such idealisations should be placed in the $R_3$ component. However, we explicitly make this statement as an idealisation, and so know that it is false, or at least some kind of misrepresentation, and so should be placed in the $R_2$ component. But if it is placed in the $R_2$ component, then one cannot use the idealisation to say anything positive about the world. The same goes for a variety of other idealisations: frictionless planes, point particles, etc. It appears there is a fatal flaw in the partial structures' approach to idealisations, due to the contradictory epistemic attitudes one is supposed to take. We do not know which of the $R_i$ components should contain the idealisation statement. I shall call this problem the *partial structures' epistemic problem for idealisation*.

There is a possible solution to the epistemic problem.[16] One must remember that the content of representations on the IC is supposed to be uninterpreted mathematics. Thus, when one equates $q_d$ and $q_s$, one is not making any statements about the world. One is simply equating two mathematical variables to, for example, help ease the tractability of the problem one is attempting to solve. Thus one is not obliged to place the idealised statement into $R_2$. But where should one place it? This idealisation does not seem capable of being understood as being open; either $q_d = q_s$ or $q_d \neq q_s$. Thus I would argue that it should be placed into the $R_1$ component. So even with this possible, and I think correct, solution to the epistemic problem, the claim that one can use the $R_3$ component to capture the openness of idealisations looks to be false. Further, as the introduction to the partial structures shows, the matches and mismatches between the target system and an idealised model are found in

[15] The price and quantity at which profits are maximised can now be calculated through simple analysis: $p_{\max}$ when $\frac{dR-C}{dq} = 0$.

[16] Thanks must go to Octávio Bueno for providing this response to the problem in conversation.

the $R_1$ and $R_2$ components respectively in order for the 'as if' understanding of idealisation to succeed.

A charitable interpretation of the openness framework claim might be that features which are 'screened off' through idealisation come to be found in the $R_3$ component, such as in the case of the ideal gas. Here, an idealisation results in the omission of some information, namely the internal structure of the gas molecules (Jones 2005, pg. 189-190) (McMullin 1985, pg. 258). McMullin's explanation of these idealisations as 'material' idealisations in some Aristotelean sense is amenable to this charitable reading. He talks of the model as including unspecified parameters or parts, of them being "open for question in a different context", that such models are "necessarily incomplete; they do not explicitly specify more than they have to for the immediate purposes at hand" (McMullin 1985, pg. 262-263). The charitable reading, then, might be that the idea of 'openness' that Bueno and Colyvan are appealing to in the context of the economics example is that such classical economics models 'cover' certain features of the world, in terms of leaving them unspecified, which can be specified in a different context. The $R_3$ component seems suitable for this job, as it is supposed to accommodate statements for which we do not know, or for which it has not yet been established, whether they hold in the structure or not.

I do not think that this charitable reading is successful. It fails because this 'openness' in terms of mismatches between target and vehicle is given in the context of a framework that is supposed to be general and accommodate *all types* of idealisation. The charitable reading relies on one type of Galilean idealisation. This would tie the 'openness' brought about by mismatches between target and vehicle to this single type of Galilean idealisation for the partial structures framework. Thus the framework would not be able to handle any of the other numerous types of idealisation. The charitable reading should therefore be dispensed with as a way of saving this 'openness' claim in general. If all Bueno and Colyvan meant by this claim is that the economics example is a case of material Galilean idealisation, then this notion of 'openness' can survive.

The third role given above for the $R_3$ component is that it houses the 'technique' of the Renormalization Group (RG).[17] I will ignore any possible significance over the terminology used to describe the RG. I will instead focus on the fact that an application of the RG involves taking singular limits. This allows one to understand such an application as a singular limit idealisation, and therefore might involve surplus structure. This should mean that this role of the $R_3$ is the same as the role for group theory. However, it is not clear that this is the case as the group theory role is within the context of an SV use of the partial structures framework, whereas the RG role is discussed within the context of an IC use of the partial structures framework. The RG role implies the introduction of surplus structure due to a move from an uninterpreted mathematical model to another uninterpreted mathematical model. The London model group theory example introduces surplus structure due to a move from an interpreted mathematical model to an uninterpreted one, as well as implying surplus structure is related to the analogies drawn between the Bose-Einstein and group theoretic models. There is clearly a difference in how to accommodate surplus structures between the SV and the IC.

The above discussion shows that there is some motivation to adopting partial structures in the derivation step to accommodate surplus structure. But this motivation requires refinement, due to the difficulty in understanding exactly what surplus structure is on the IC. I now turn to clarifying the notion

[17]See Batterman (2005a, 2011) for some of Batterman's introductions and discussions of the renormalisation group, and Bueno & French (2011) for an explanation and response from the IC perspective. Kadanoff (2000) provides a textbook introduction to the topic.

of surplus structure, identifying what role the IC should accommodate.

### 6.2.3 Surplus Structure on the Inferential Conception

Thus far I have been working with a definition of surplus structure as the introduction of genuinely new structure, but I have also talked about it as the move from interpreted mathematics to uninterpreted mathematics. That is, the genuinely new structure is always *new uninterpreted mathematical* structure. However, there is an ambiguity in the literature over what constitutes surplus structure. The notion of surplus structure as the introduction of new structure and involving a move from interpreted to uninterpreted mathematics is based upon the way the term is used in the discussion of how group theory is related to Bose-Einstein statistics in the London-London case by Bueno et al in their (2002). This example is of the same kind as the case of analytic $S$-matrix theory in elementary particle physics (Redhead 1975, pg. 88) (Redhead 2001, pg. 80). Here scattering amplitudes are considered as functions of real-valued energy and momentum transfer. These functions were continued into the complex plane and axioms concerning the singularities of the functions in the complex plane were introduced to set up systems of equations which "controlled" the behaviour of the scattering amplitudes considered as functions of the real, physical, variables. The common feature of these two examples, group theory and $S$-matrix theory, is that "there was no question of identifying any physical correlate with the surplus structure" (Redhead 2001, pg. 80).[18] These two examples are contrasted to cases where "what starts as surplus structure may come to be seen as invested with physical reality", that is, what starts as surplus structure, uninterpreted mathematics, gains an interpretation. Redhead offers the examples of the kinetic theory of matter as viewed by positivists before the discovery of Brownian motion (Redhead 1975,

[18]The symmetries of type (1) in the group theory and quantum mechanics example from French (1999) is this kind of surplus structure.

pg. 88), energy (Redhead 2001, pg. 80-81) and the negative energy solutions to the Dirac equation (Redhead 2001, pg. 83).[19] These were first thought to be artefacts of the mathematics, then came to be interpreted as 'holes' in a sea of negative energy electrons before finally being reinterpreted as positrons.[20] Redhead also outlines two further roles for surplus structure: certain idealisations, such as representing physical magnitudes by the real numbers, can "[add] (surplus) structure to the physics rather than stripping it away, as in alternative senses of idealization", i.e. formal Galilean idealisations (Redhead 2001, pg. 83); and as a way of accommodating Hesse's account of how theories develop through analogous models.[21][22]

The last of these roles for surplus structure, that of accommodating Hesse's account of theory development, is clearly not a role that the IC should accommodate: this is something that the SV handles as it involves inter-theory relations (da Costa & French 2003, pg. 47-52). The second and third roles can be accommodated by the IC. The notion of there being more mathematical structure compared to physical structure, what underlies the particular type of idealisation that Redhead highlights as the third role, is noted by Bueno and Colyvan in their criticism of Pincock's original mapping account (Bueno &

[19]The symmetries of type (2) in the group theory and quantum mechanics example from French (1999) is this kind of surplus structure.

[20]I outlined Dirac's attempts at interpreting the negative energy solutions in §2.2.2.

[21]Redhead, summarising Hesse (1963) states that "theories can develop by providing physical interpretation for [the] surplus structure and [she] argues persuasively that the justification for obtaining a successfully strongly predictive theory by this manoeuvre depends on pre-theoretic material analogies with an analogue model which already provides an interpretation for the surplus structure" (Redhead 1980, pg. 149).

[22]Teller provides an alternative distinction between two types of surplus structure: *strongly surplus structure* where the mathematical formalism has interpreted elements that never have actual physical correlates, and *weakly surplus structure* where the mathematical formalism contains some uninterpreted elements (Teller 1997, pg. 25-26). He also describes the example of the negative energy solutions of the Dirac equation as being *apparent surplus structure*. Teller is clearly working with a conception of surplus structure similar to the one I have been thus far, that surplus structure has to be uninterpreted mathematics. However, Teller also goes on to argue that genuine surplus structure should be "shunned", both in the weak and strong cases, which leads me to think that his conception is slightly different to mine. I will not discuss Teller further, as I will argue that the Redhead conceptions of surplus structure can be accommodated by the IC and the SV.

Colyvan 2011, pg. 348). However, given what I have argued above about idealisation, this 'surplus structure' should only present itself in the final empirical set up, after mapping from the resultant structure (as within the derivation step, before the interpretation mapping, we haven't finished representing anything yet). The second case can be accommodated by the IC in a similar way. Bueno and Colyvan explain that the multiple interpretations of the Dirac negative energy solutions consist in adopting different partial morphisms from the resultant structure of the Dirac equation, to different empirical set ups (Bueno & Colyvan 2011, pg. 364-366). Here the surplus structure would be mapped to the $R_i$ component of the empirical set up depending on the interpretation, e.g. the $R_2$ component when they are dismissed as being unphysical.

The crucial distinction between these two cases and the first is at which stage the surplus structure is identified within the IC. The second and third cases concern structure that is surplus *after* the derivation step, and involve the relationship between the resultant structure and the empirical set up. As the first role involves mathematics that will not be physically interpreted, this suggests that the surplus structure will be structure obtained after the iteration of the immersion mapping, i.e. it will be in the Model 2 of the iterated IC. The problem I intend raise concerning the plausibility of surplus structure on the IC is targeted at the first type of surplus structure. 'Surplus structure' will refer exclusively to this first role from this point on.

While there is a clear division between interpreted mathematics (the scientific models) and the uninterpreted mathematics that constitutes the surplus structure when discussing the SV, this division completely breaks down when one applies the IC. The immersion and interpretation step can be iterated; Bueno and French argue that this iteration is the way to account for cases of surplus structure, such as an application of the RG (Bueno & French 2012, pg. 91-92):

> One of the features of the account we advocate is that it can accommodate the role of surplus mathematical structure, whereby a given physical structure can be related via partial homomorphisms (or some other partial morphism) to a suitable mathematical structure, which in turn is related to further mathematical structure, some of which can then in turn be interpreted physically (see Bueno (1997), Bueno et al. (2002) and Bueno & Colyvan (2011)). We can represent such a surplus structure within the inferential conception by straightforward iteration: the initial mathematical model (Model 1) that is used to represent the original empirical set up is itself immersed into another model (Model 2), which gives us the surplus structure, and the results are then interpreted back into Model 1, which only then is interpreted into the physical set up.

I have challenged that such an iteration can lead to surplus structure. Model 1 is already uninterpreted mathematics, and so 'surplus' in the sense of being uninterpreted. An iterated immersion mapping to Model 2 does not alter this. There must be more to the notion of surplus structure. When discussing the application of group theory (both to quantum mechanics and superfluidity) it is the shift from interpreted to uninterpreted mathematics *in addition* to that uninterpreted mathematics providing *genuinely new structure* which does the novel work (Bueno et al. 2002, pg. 508-509). This still does not solve the problem; there must still be yet more to the notion of surplus structure.

Another feature of surplus structure can be identified by looking to how the IC can accommodate the application of mathematics to other mathematics. The situation is the same as the iteration case: one maps from uninterpreted mathematics to other uninterpreted mathematics. Bueno and Colyvan leverage the IC to explain how the unification of the two different domains of the complex numbers and the theory of differential equations can be understood in terms of importing structure into different domains (Bueno & Colyvan 2011, pg. 364). What is emphasised is the introduction of "new inferential relations [where] [u]nification emerges ... as the result of establishing such inferential relations among apparently unrelated things". So the reason that surplus structure is so useful is three fold: there are surplus structural resources; uninterpreted mathematics provides such surplus structural resources; and these surplus uninterpreted mathematical resources provide new inferential relations to bring to bear on our original structures. Described as such, there seems to be less problem in moving from one mathematical structure to another, and describing the second as surplus structure.

There is a problem, however, in how this move is supposed to be accommodated if one takes surplus structure to involve partial structures. In the group theory case, it is argued by Bueno et al. that group theory comes to enter the discussion through the openness of London's 'rough and preliminary' model, that is, through the $R_3$ component: "Retaining the component associated with Bose-Einstein statistics, understood group-theoretically, new elements were introduced ... corresponding to a move from our $R_3$" (Bueno et al. 2002, pg. 514). From this discussion I think one has to infer that group theory is some how 'in' the $R_3$ component already, for it be capable of being introduced. Or perhaps more coherently, there are structures in the $R_3$ that are (partially) iso-, homo-, or otherwise, -morphic to the structures of group theory. But this begs the question of what limits the $R_3$? If the $R_3$ is to truly represent 'openness', it has to be capable of introducing more than one type of mathematics, more than group theory. It has to be open to the agent to introduce various different types of mathematics; the physical interpretation is what picks out group theory (and only certain parts of group theory) as being appropriate to the application at hand, not the content of $R_3$.[23] Given this, one must be

[23]As we are dealing with the first type of surplus structure here, the physical interpretation comes from mapping the group theoretic results to the Bose-Einstein model, and this being mapped to an empirical set up. The group theoretic resultant structure does not get a direct physical interpretation.

capable of extending the original structure to any type of mathematics. This problem should be recognised as being a serious problem, as it appears *any time* we apply mathematics. There is no way to predict whether we will need to turn to surplus structure in any given application of mathematics (or at least any novel application of mathematics). Thus, any application of mathematics will require all mathematical structures in the $R_3$ (or again, structures that are (partially) -morphic to all mathematical structures).

It appears that if we understand surplus structure in terms of partial structures, then our partial structure must contain all mathematical structures in the $R_3$ component. Call this the *Very Large $R_3$ Problem*. This problem creates a dilemma for the proponents of the partial structure framework. Either they must admit the Very Large $R_3$, or they must introduce some way of constraining the $R_3$. However, as has been argued, the derivation step takes part in the domain of uninterpreted mathematics, where physical considerations do not play any role (to avoid the epistemic problem and to maintain the 'as if' view of idealisation). Thus there is a problem concerning what we can appeal to in order to constrain the mathematical moves we make (and hence $R_3$). Call this the *Mathematical Restriction Problem*. The Mathematical Restriction Problem can be understood as an attempt to cut off the Very Large $R_3$, by providing a reason to only include structure that is (partially) morphic to the relevant mathematics (e.g. group theory) in the $R_3$. If this problem cannot be solved, then we have to deal with the Very Large $R_3$ Problem. These two problems thus compose the two horns of a dilemma. Let us start by attempting to deal with the Mathematical Restriction Problem.

There are two ways in which we can attempt to restrict the mathematics that might be required for the $R_3$: reasons that are either *internal* or *external* to the mathematics. Looking first at the internal reasons: there might be some intrinsic feature of the mathematics that leads us to prefer to the adoption of a certain transformation than other. For example, we might decide that a Fourier transform is more appropriate than a Taylor expansion due to the original function being periodic. This sort of restriction, however, is not the kind of restriction we require. This only restricts which moves we *take*, not which moves are available. This is a 'use' restriction, in that it restricts what mathematics we use in the model. What we are looking for is a restriction that prevents certain mathematics from being in the $R_3$ in the first place, i.e. a restriction in principle rather than in practice. A possible solution might be to opt for a kind of negative solution. Some extensions can be seen to be very 'natural', in that there are strong internal mathematical reasons for the extensions. Take as an example the extension of real functions into the complex plane. It is reasonable to claim that to properly understand any real function, one has to extend it into the complex plane. Hence, this extension is 'natural' in that it is required for a proper understanding of the function at hand. We can then put forward the claim that any non-'natural' extension should not be included in the $R_3$ component. While promising, this requires more work to see whether it is a viable option. It is plausible that this non-'natural' restriction might be too restrictive in some instances.

I think that there are four external reasons to restrict the $R_3$:

(a) physical interpretations;

(b) tractability concerns;

(c) intentions of the scientists/representing agents;

(d) pragmatic, contextual and heuristic reasons.

(a) can be rejected immediately. We are dealing with the first type of surplus structure, which has no chance of being physically interpreted. Physical interpretations therefore have no bearing on what we need to place in the $R_3$ for this type of surplus structure. (b) can be dismissed as tractability can be considered a 'use' restriction rather than an in principle restriction. Tractability will prevent us from pursuing certain lines of mathematical enquiry, but it will not prevent them from being possible avenues and hence having to be included in the $R_3$.[24]

(c) and (d) come together and are initially the most promising solutions. The agents performing such representations have intuitions about what sort of mathematics would be best to find the sort of structural relations or features that might have a bearing on the models they require the surplus structure to enlighten. This seems promising: the solution can be incorporated into the agents intentions, provided that the earlier comments concerning the agents intentions[25] and the idealisation of the IC[26] hold. This is only the sketch of a solution, however. A lot more needs to be said by the proponents of the IC on exactly what constitutes the heuristic, pragmatic and contextual considerations, given that they are relied on so heavily throughout the discussion of the IC and other applications of the partial structures framework.

So much for the first horn of the dilemma. Let us assume for the sake of argument that the above possible solutions fail. What of the second horn of the dilemma, the Very Large $R_3$ Problem? If we cannot restrict the mathematics

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[24]This dismissal might be too quick. Tractability can be considered both internal and external to the mathematics. For example the Mie solution provides an exact solution for the scattering of light by a water droplet for the rainbow. It converges slowly, but solutions can be found with modern computers. The mathematics is therefore tractable internally in the sense that we can solve the equations. One can consider it to be intractable in an external sense, however, as physicists claim that it is hard to 'see the physics' within the Mie solution.

[25]See §4.2.1, where I argue that the intentions should not be included in the representation relation, on pain of fixing the target of the representation, and relate to the particular structural relation involved in a particular representation. I also discussed the role pragmatic and contextual factors played in identifying the empirical set ups and structural relations. The discussion there was focused more on the interpretation mapping. If similar arguments hold for the interpretation mapping, then this response has some plausibility.

[26]I'm referring here to the second way in which the IC might be idealised, that "the mathematical formalism often comes accompanied by certain physical interpretations" (Bueno & Colyvan 2011, pg. 354). This was also discussed in §4.2.1. I rejected any interpretations of the mathematics in the derivation step, claiming that the role played by these 'interpretations' was accommodated by the pragmatic, heuristic, and contextual considerations.

we have to include in the $R_3$, it looks like the Very Large $R_3$ Problem is a bullet the proponents of the partial structures approach will have to bite. But there are at least two ways in which the proponents might bite it. They might either reify the partial structures, which would lead to a problem, or insist that while they are biting the bullet, it is not a problem as the partial structures are simply a representational device and so a large $R_3$ has no consequence.

The argument for reifying the partial structures is as follows. The size of and the content of the partial structures is significant, e.g. the size of the $R_i$ grounds the notion of how 'faithful' a representation is. Hence the size of the $R_3$ cannot be dismissed. As we are using mathematical structures as our representation vehicle, we are not dealing with a representation of the structures, but the mathematical structures themselves. We reify the partial structures when in the mathematical domain. Therefore the argument that a very large $R_3$ being required to introduce surplus structure is an argument that we need a mathematical structure that contains all of mathematics in its $R_3$ component. This, however, raises the problems of how we are to make sense of and deal with such a large mathematical structure. There is serious work to be done here to justify such a position, especially given the strong support the next response has, that the partial structures are representational devices.

Insisting that the partial structures are representational devices has strong textual evidence.[27] This response, while simple, raises some immediate problems. First, what does it mean to have a representation that suffers from the Very Large $R_3$ Problem? What is the very large $R_3$ *representing*? If it is representing the situation that we require all mathematics in order to understand surplus structure, the Very Large $R_3$ Problem is not solved, we've merely reformed it at the level of the mathematics we are representing. Alternatively, one might claim that the Very Large $R_3$ isn't representing anything at all, as a

[27] See the discussion of this in §8.3. See also French (2012) and (Bueno & French 2011, §9.7).

way to avoid the problem. But this seems to render the whole notion of partial structures and surplus structure useless. What is the justification for adopting the partial structures in some areas and not others (such as the Very Large $R_3$)? This move risks being *ad hoc*, and so does not seem like a viable option. There is clearly work to be done here to explain exactly what consequences the position of the partial structures being simply representational devices has on the Very Large $R_3$ Problem. The consequences are not obvious, and the proponents of the partial structures program owe us more.

A possible way out of this problem is to avoid biting the bullet altogether, by not requiring partial structures in the derivation step. In order to establish whether we do, we need to look at whether we can move from one uninterpreted mathematical structure to another in a way which introduces the necessary surplus mathematical resources with full structures. It appears that the usual model-theoretic notion of structure extension is incapable of doing this (French 1999, pg. 188):

> However, the mathematical surplus which, it is claimed, drives these heuristic developments cannot be captured by the usual model-theoretic notion of a structure extension since this simply involves the addition of new elements to the relevant domain with the concomitant new relations. What is required is 'new' structure which is genuinely 'surplus' and which supplies new mathematical resources. Thus an appropriate characterisation would be one in which $T'$ is itself embedded in an entire family of further structures - $T''$, $T'''$ and so on - and this may then capture the heuristic role of mathematics in theory construction.:'

Understanding a move to surplus structure as involving a move to a family of structures does not require the notion of partial structures or partial -morphisms. They are introduced by French to accommodate the "incompleteness and openness of the heuristic situation" of applying mathematics to the world (French 1999, pg. 192). However, we do need to use partial structures in the derivation step in order to make use of the partial mappings in the immersion and interpretation steps.

We can motivate the idea of surplus structure involving an embedding to a family of structures to solve the problem of whether surplus structure makes sense on the IC. It seemed that since the defining features of surplus structure were the uninterpreted mathematics allowing new inferential relations to be drawn, that there was no difference between the the Model 1 and Model 2 of the iterative application of the IC that one would expect in order to call Model 2 a case of surplus structure. Now, we can argue that Model 2 isn't a 'model', but rather a family of models. That is, an iterative application of the IC that leads to surplus structure moves one from a singular mathematical model, Model 1, to a family of mathematical models (structures), which is labeled Model 2.

The question now is whether these solutions to the problems above actually work. To test this, I will turn to my case study of the rainbow, to the two instances of what could be considered surplus structure: the taking of the size parameter, $\beta$, to infinity; and the introduction of the complex plane in the CAM approach. These are two different ways of introducing surplus structure; for the solutions above to be useful, they have to accommodate both of these ways of introducing surplus structure.

## 6.3 The IC Applied to the Rainbow

### 6.3.1 $\beta \to \infty$

Taking the size parameter to infinity is proposed as a way of obtaining a ray theoretic representation from wave theory. This is to allow an explanation of the colour distribution of the rainbow via the wave theoretic concepts that underpin this ray theoretic representation. Specifically it allows one to appeal to various values of the wave number, $k$, to give a different value of the refractive index, $n$, for each colour of the rainbow. This is in contrast to obtaining a ray representation from wave theory by taking the wave number to infinity. This approach fails if we wish to investigate the rainbow, however, as at the rainbow angle we have infinite intensity and amplitude. We lose the information we need to discuss the distinct colours and their particular refractive index, $n$. Thus we turn to a ray theoretic model obtained form the Mie solution.

The question that needs to be answered in this section is whether the $\beta$ limit is a *singular* limit, and if it is a singular limit, does the model that results from it fulfil the definition of being surplus structure, i.e. a family of uninterpreted mathematical structures that provide additional inferential relations? Proponents of the IC hold the view that the models that result from singular limits are ones of surplus structure (Bueno & French 2012, pg. 91). Thus, if the $\beta$ limit is singular and does not result in surplus structure, an adjustment will have to be made to the IC.

Grandy Jr states explicitly that we need to take the limit in order to introduce a cut off and convert the infinite sum of the scattering functions into an integral (Grandy Jr 2005, pg. 120). Here an attempted separation of the effects of diffraction and reflection is made. The partial wave coefficients, $a_n$ and $b_n$ (given in (A.121) and (A.122) in the Appendix, and (3.140) in Grandy Jr (2005, pg. 101), describe the effects of diffraction and the scattering functions $S_n^E$ and $S_n^M$ (given in (A.123) and (A.124) in the appendix and (3.141) in Grandy Jr) describe the reflection. A cut off is introduced by letting $a_n = b_n = \frac{1}{2}$ for $n + \frac{1}{2} < \beta$, and 0 otherwise. The conversion of the sum in the scattering functions to an integral depends on fixing $\cos(\theta) \equiv \left(n + \frac{1}{2}\right)\theta$ as $n \to \infty$. This allows us to adopt approximations for $\pi_n$ and $\tau_n$ to put the upper limit of the integral as $\beta$:

$$S_1(\theta) = S_2(\theta) \simeq \sum_{n=1}^{[\beta]} \left(n + \frac{1}{2}\right) J_0\left[\left(n + \frac{1}{2}\right)\theta\right] \to \int_0^{\beta} \lambda J_0(\lambda\theta)\, \mathrm{d}\lambda = \beta^2 \frac{J_1(\beta\theta)}{\beta\theta} \tag{6.1}$$

where $\lambda$ is the continuum extrapolation of $n + \frac{1}{2}$ when we replace the sum by an integral over all relevant impact parameters.[28] Grandy Jr emphasises that "introduction of a cutoff and direct conversion of the sum to an integral are gross approximations that can be valid *only in the limit* $\beta \to \infty$" (Grandy Jr 2005, pg. 120, my emphasis). The rest of the analysis requires taking further asymptotic expansions, such as the Debye asymptotic expansions given in Grandy Jr (2005, Appendix C). Eventually we obtain the equation for the rainbow angle:

$$y_R = \sin\frac{\theta_R}{2} = \frac{(8 + n^2 c)}{3n^2}\cos\frac{\theta}{2} = \frac{s^3}{n^2} \tag{6.2}$$

It is clear from Grandy Jr's analysis that we have to be at the limit in order to make use of these approximations. It is also clear that the mathematics involved is not surplus structure. There are no obvious mathematical moves we can make from this equation to recover a wave theoretical model. While it might seem that we can map from the angle here to the angle in the Mie solution in some way, I don't think that such a mapping would be appropriate. This would have the effect of providing an interpretation of the angle as an angle between normals to wave fronts, rather than between rays. Given that the motivations for finding a ray theoretic model from the Mie solution was to provide a ray theoretic explanation of the colour distribution, adopting a mapping that results in a wave theoretic interpretation of the angle would not

[28]With the additional approximation of $\sin\theta$ by $\theta$ in the the Airy diffraction pattern.

only be unmotivated, but would actually be providing the wrong interpretation of the angle. Thus we must conclude that the singular limit of $\beta \to \infty$ does not result in a model that is surplus structure, and so is a counter example to the claim that singular limits result in surplus structure.

So how should we make sense of the model we obtain by the singular limit of $\beta \to \infty$ on the IC? I think that we have to recognise it as a normal Model 1 obtained via a mapping from an empirical set up. This would require us to adopt the Mie solution as our empirical set up. Such an empirical set up is worrying, however. The Mie solution appears to be far too mathematical to be the starting point for an application of mathematics. While Bueno and Colyvan claim that the empirical set up need not be entirely free of mathematics, describing it as “the relevant bits of the empirical world, not a mathematics free description of it”, and claim that “very often the only description of the set up available will invoke a great deal of mathematics”, adopting something like the Mie solution as the empirical set up seems a step too far (Bueno & Colyvan 2011, pg. 354). I can offer a two part response to this worry. First, there is no need to require the empirical set up to be a completely novel description of the world. We have already had to apply mathematics to the world in order to obtain the Mie solution. When we did, we would have mapped the mathematics of those solutions to a final empirical set up, providing the interpretation of them. We can adopt this empirical set up as our starting point, and any complaint that it involves too much mathematics can be answered by pointing to that previous application (or those previous applications) of mathematics and how the structures mapped to and from in those respective empirical set ups were identified. Second, the Mie solution are derived from the Maxwell equations and are exact solutions. They are as accurate a description of the world as we can expect from classical mechanical laws. If there is any empirical set up that could be considered the “only description of the set up [that] invoke[s] a great deal of mathematics", then one involving the Mie solution is an extremely good candidate.

The above considerations prompt an adjustment to when we can claim surplus structure has been obtained via a singular limit in the context of the IC. When a singular limit is used to move from Model 1 to Model 2 (in an iteration of the IC), Model 2 is surplus structure. When a singular limit is used to move from the initial empirical set up to Model 1 (whether there is a Model 2 or not), Model 1 is not surplus structure, but a typical Model 1. This adjustment removes any special features from the singular limits. i.e. A singular limit no longer entails surplus structure on the IC.

A potential criticism of this solution is that we should not be moving from the empirical set up to the mathematical domain via a singular limit. This is motivated by some intuitions: first that singular limits only make sense between mathematical structures (i.e. they should only take place between Model 1 and Model 2); and second, and slightly differently, that moving from the empirical set up to a mathematical model via a singular limit is 'too quick'. The idea here is that singular limits require some setting up of the situation mathematically, before we can perform them. i.e. There are moves we to take in the derivation before we perform the singular limit of $\beta \to \infty$. The first of these intuitions can be rejected by either pointing to the mathematics involved the empirical set up, or positing that we move to a Model 1 that is isomorphic to the empirical set up before taking the limit (and do not always do so explicitly). The second intuition can be assuaged, though not rejected, by claiming that the empirical set up we use in such a situation comes from another application of the IC. The reason this is not a conclusive response is that it is not *prima facíe* clear whether we could have a suitable application of the IC where we 'finish' with an empirical set up that would satisfy the requirements of the IC and contain the required structure, i.e. the structure of the mathematics at the point in the derivation before we apply the limit.

### 6.3.2 CAM Approach

The CAM approach is a way of dealing with the slow convergence of the Mie solution. The approach consists in transforming the Debye expansion of the Mie solution from an infinite sum of real functions to an integral of complex functions. The saddle points and Regge poles of these functions are then used to gain information on how the scattered wave behaves: the saddle points are related to reflected and refracted light and the Regge poles to surface waves, waves of light that travel along the surface of the water droplet. Thus the application of the CAM approach appears to be a standard introduction of surplus structure at the immersion stage: we introduce the genuinely new features of the complex plane in order to gain greater insight into the behaviour of the real functions found in the Mie solution. This occurs due to the shift from the infinite sum of real functions to the integral over complex functions, i.e. due to the application of the Poisson sum formula:

$$\sum_{l=0}^{\infty} \psi(\Lambda, \mathbf{r}) = \sum_{m=-\infty}^{\infty} (-1)^m \int_0^{\infty} \psi(\Lambda, \mathbf{r}) \, e^{2im\pi\Lambda} \, \mathrm{d}\Lambda \tag{6.3}$$

The Poisson sum formula is a Fourier transform. These are transformations that allow us to represent a series (i.e. a sum) of discrete values as a continuum (i.e. via an integral) and in doing so moves us to the complex plane. Fourier transforms result in periodic functions, that is they include transformations to functions that involve sine and cosine functions. The transformations are due, in part, to the Euler formula, the relationship between the exponential function and the trigonometric functions:

$$e^{i\theta} = \cos\theta + i\sin\theta \tag{6.4}$$

This provides a chance to see whether any of the possible solutions might be capable of solving the dilemma between the Very Large $R_3$ Problem and the Mathematical Restriction Problem. The situation above looks like it would be suited to appealing to the 'natural extension' solution to the Mathematical Restriction problem. First, there is the move to the complex plane, which is the example I used to introduce the notion of natural extension. Second, recognising that the Poisson sum formula is a Fourier transform and so in part relies on the Euler formula, suggests that we need the Euler formula in the $R_3$ component of the partial structure of the Debye expansion of the Mie solution. This does not seem like an unlikely thing to include in the $R_3$ component. Much as one might argue that the structure of the complex plane should be included in the $R_3$ for any real function, Euler's formula should be included in the $R_3$ for any trigonometric or exponential function; Euler's formula displays a relationship that is fundamental to a proper understanding of these functions. The question remains over what *else* one would need to include in the $R_3$. For example, do we need further mathematical techniques to perform Fourier transforms? A second question is also raised: how much mathematics should be included in the derivation step? As I will outline below, solving the resulting equations is not a straight forward matter. Due to the coalescing of the saddle points, further mathematical techniques (the CFU method) are required to provide a solution to the equations that can provide suitable physics for the rainbow. These two questions can be summarised under the question of 'what are the family of structures we map into?' Before answering this question, I will detail how the surplus structure of the CAM approach is to be related back to the Mie solution. At best this should involve identifying what interpretation mapping we should adopt. At minimum it should identify which parts of the mathematics of the CAM approach should be mapped back to the mathematics of the Mie solution.

**Interpreting the CAM Approach's Solutions**

The saddle points and Regge poles of the Debye expansion, extended into the complex plane, are used to obtain the physics of the rainbow. The mathematical details of how we obtain these critical points was set out in the last chapter, in §5.4, and in the Appendix, in §A.5. The reason behind looking at the transformations and the targeting of the critical points can be summarised as follows. The transformation was employed to obtain a series that converged quicker than the original Mie solution. This required that we adopt the Debye expansion of the scattering amplitudes, from which we obtain the asymptotic representation of each term by applying the Poisson sum formula to each term and then making use of the ability to deform paths in the complex plane. This results in a background integral, and allows us to "concentrate dominant contributions to the integrals into the neighborhood of a small number of points in the $\lambda$ plane, [the] *critical points*. The main types of critical points are *saddle points*, that can be real or complex, and *complex poles* such as the Regge poles" (Nussenzveig 1992, pg. 62). As stated in §5.5, the interpretation of the Regge-Debye poles and saddle points varies across the physics text books. The Regge-Debye poles are consistently referred to as representing surface waves, but the texts vary as to whether the surface waves are cashed out in terms of actual waves or as rays taking short cuts along the surface of the droplets. The saddle points are variously interpreted as points of stationary phase or as rays. Points of stationary phase are also interpreted as rays (Nussenzveig 1992, pg. 62):

> A real saddle point is also a stationary-phase point. Since the phase of the integrand in most cases can be approximated by the WKB phase, stationarity implies that the action principle (or Fermat's principle) is satisfied, so that such points will be associated with contributions from *classical paths* (or *geometric-optic rays*)

As I understand the mathematics, and due to conversations with physicists,[29] I think that whether the IC is capable of interpreting the Regge-Debye poles and saddle points in a way that is consistent with all that I have said above depends on whether the models are under construction or being used. This is because there appears to be different concepts required during the development of the model compared to its use. Ray theoretic concepts seem essential to the generation of the uniform CAM approach. Once we have constructed the model, some of its uses (explanation, prediction, etc.) can be performed through interpreting the saddle points as points of stationary phase and the Regge-Debye poles as actual surface waves. There is at least one use that requires ray theoretic concepts, however: the process of teaching and learning the model needs the ray theoretic concepts, or at least relies on them heavily, as heuristic and pedagogical tools.

If it is the case that ray theoretic concepts are required to interpret the mathematics of the CAM approach during model construction, this is problematic for the IC and the partial structures framework. One of the main draws for adopting the partial structures version of the Semantic View is that the partial structures framework is supposed to provide a unified way of handling both model construction and model use. Showing that the construction of the CAM model requires ray theoretic concepts but the use of the representation does not require them threatens this unified approach. Bueno and French outline the partial structures approach to model construction and the way it relates to the IC as follows (Bueno & French 2012, pg. 88-89):

> Using [the partial structures approach], we can provide a framework for accommodating the application of mathematics to theory construction in science. The main idea is that mathematics is applied by bringing

[29] I exchanged a series of emails with John Adams and James Lock, wherein I discussed the appropriate interpretations of the Regge-Debye poles and saddle points, and the indispensability of ray theoretic concepts to these interpretations.

> structure from a mathematical domain (say, functional analysis) into a physical, but mathematized, domain (such as quantum mechanics). What we have, thus, is a structural perspective, which involves the establishment of relations between structures in different domains. Crucially, we typically have surplus structure at the mathematical level, so only some structure is brought from mathematics to physics.
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> ...
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> It is straightforward to accommodate this situation using partial structures. The partial homomorphism represents the situation in which only some structure is brought from mathematics to physics (via the $R_1$- and $R_2$-components, which represent our current information about the relevant domain), although 'more structure' could be found at the mathematical domain (via the $R_3$-component, which is left open). Moreover, given the partiality of information, just part of the mathematical structures is preserved, namely that part about which we have enough information to match the empirical domain. These formal details can then be deployed to underpin the [IC].

However, one can find comments else where in the literature on partial structures which threatens this straightforward understanding of how model construction should be understood. In their (2012), Bueno and French claim that the London-London example of superconductivity[30] is a case of "model construction" (Bueno & French 2011, pg. 862). In his (2000), French distinguishes between horizontal and vertical relationships between theoretical models and structures (French 2000, pg. 106):

> [Partial isomorphisms capture] (i) the 'horizontal' inter-relationships between theories, thus providing a convenient framework for understanding theory change and construction; and also (ii) the 'vertical' relationships

[30] This is a different example to the London account of superfluidity, though very closely related.

> between theoretical structures and data models, accommodating, in particular, the role of idealisations

In their (2011) and (2012) Bueno and French indicate that model construction occurs along the normal IC lines. We construct models by representing empirical set ups mathematically, perform surrogative inferences in the mathematics to obtain new (partial) structures (and partial structural relations) in the empirical set up, and so have a new model. Neither of these papers refers to the distinction between the vertical and horizontal relationships between models that French outlines in his (2000). I think it is ignoring this distinction which has caused the problem. When discussing the four types of surplus structure in §6.2.3 I argued that the fourth type, that of accommodating Hesse's account of analogies, belonged to the SV rather than the IC. I also argued in §6.2.1 that the IC does not strictly produce idealised representations of the world, as the targets of the IC are the empirical set ups, which only become representational vehicles themselves when used in scientific representations in the context of the SV. I propose that we understand model creation as part of the SV, along the horizontal axis (the accommodation of Hesse's analogies should also be placed here). One might be tempted to place the IC on the vertical axis. I think, however, that it should in fact form its own third axis, due to mathematics being involved in models on both axes. This allows the IC to account for the mathematical representation that occurs in all parts of the SV, while not falling prey to problems such as how to relate the empirical set ups to the world at any one time (as this is done by the vertical axis).

If model construction occurs along the horizontal axis, then we are not constrained by the strict requirements of the IC. The requirement to iterate the IC during the CAM approach meant that the saddle points, in Model 2, could only be related to the wave theoretic Model 1, which itself was mapped to the empirical set up to gain a physical interpretation. i.e. The mathematics in Model 1 was also uninterpreted mathematics (though we can describe this as wave theoretic as it gains a wave interpretation through the mapping to the empirical set up). As there was no ray theoretic model in this iteration, we could not map the saddle points to rays, as we appear to need to do in order to construct the model. If model creation occurs within the scope of the SV's use of partial structures on the horizontal axis, however, then we are dealing exclusively with models, i.e. interpreted mathematics. In this case, the requirement to map back through uninterpreted mathematical models is dropped. This raises the possibility of having more freedom in our choice of mappings between models and which models we use. For example, we could map from a model with a wave theoretic interpretation, to a model that has a ray theoretic interpretation, or from both to a third model which contains both ray and wave theoretic interpreted components.

The obvious objection to this is that we will result in models that claim inconsistent things about the world. There are numerous responses available to inconsistent models, which Bueno and French survey in §5 of their (2011). They advocate accepting that inconsistent models exist, but reject that they are false because the inconsistent objects they putatively refer to do not exist. Rather, they accept the representations to be partially-true. For example, the object the Bohr model putatively refers to is held to not exist, though the representation is regarded as partially-true, reflecting its success. The "central inconsistency" in the model is claimed to be the stable stationary states (Bueno & French 2011, pg. 867). One can consider two parts of the model that are inconsistent, the dynamics of the electron in the stationary state and the transitions between these states, to be considered parts of "strong sub-theories". At least in the Bohr case, the inconsistent claim, the stable stationary states, can be located in the $R_3$ component, to represent statements which have yet to be established whether they hold in the domain of investigation. If this approach works for handling the use of inconsistent representations, hopefully it can be adopted to handle the inconsistencies that arise during model creation.

There is a lot more work to pursue here: the notion of the IC being a third axis requires further exploration, while the ability of the partial structures framework to accommodate inconsistent models is now a more significant feature of the framework.

**Which Families of Structures?**

I suggested above that the only additional mathematics required for the CAM approach when moving from the Mie solution was the mathematics required to solve Fourier transformations, i.e. Euler's equation and mathematics involved in the extension into the complex plane. The mathematical steps involved in the application of the CAM approach fall into those conducted within the wave theoretic model and those conducted within the CAM model. If new mathematical resources are required than are required to derive the Mie solution, then further mathematical structure will have to be introduced, and we might be dealing with more than one case of surplus structure and hence with more than one iteration of the IC. The most significant steps are: the Debye expansion, the Poisson sum formula, the deformation of the contour integral to obtain the Regge-Debye poles and saddle points, and finally the CFU method to find the saddle point contributions around $\theta = \theta_R$. I will look at each of these steps and detail whether any additional resources are required.

The Mie solution could be interpreted as involving standing waves within the water droplet and transmitted plane waves outside of the water droplet. The Debye expansion involves the transformation of the Mie solution which can then be interpreted as involving spherically partially transmitted and reflected waves. This involves expressing the scattering function in terms of spherical transmission and reflection coefficients. These are expressed in terms of the

Henkel and Bessel functions. All of this work can be done within the Mie model, which is also expressed in terms of Henkel and Bessel functions. No extra resources are needed. The application of the Poisson sum formula and its extension into the complex plane constitutes the (partial) mapping from the wave model to the CAM model, and the introduction of the surplus structure of the complex plane. The deformation of the contour integral, finding poles and saddle points are all stand procedures in complex analysis. The resources needed to conduct these moves should come with the move into the complex plane.

The CFU method should be regarded as a part of the derivation step. Although there is discussion of introducing "a mapping that preserves the saddle point structure", the move is a rather typical one: a change of variables can hardly be considered to be introducing genuinely new mathematical resources, and certainly not the introduction of a family of structures. Indeed, the CFU method only transforms one equation that contains an inexact cubic to one that contains an exact cubic.

This suggests that although the mathematics involved in the CAM approach is quite sophisticated, in terms of the families of structures required it is a rather straight forward approach. We only need to move into the complex plane and bring the standard tools required for solving and manipulating complex functions: finding poles, constructing residue series and solving integrals whose contributions come from critical points. This suggests that the Mathematical Restriction Problem is likely to be solved in this instance through the 'natural extension' solution, as we need nothing beyond the typical mathematical structures that come with a move into the complex plane.

## 6.4 Conclusion

In this chapter, I have argued that the IC can accommodate the relationship between faithfulness and usefulness by leveraging the notions of partial truth and partial structure, adopting an analogous view to the extrinsic and intrinsic conception of theories offered by the proponents of the partial structures version of the SV. I also argued that the structural resources used were the same for the Galilean and singular limit idealisations, but that the notion of surplus structure required some work to be accommodated within the IC. I rejected the idea that singular limits necessarily lead to the adoption of surplus structure. This was shown with the $\beta \to \infty$ idealisation used to obtain a ray theoretic model of the rainbow. I argued that surplus structure could be accommodated on the partial structure framework, however, doing so opened up the account to a dilemma from the Very Large $R_3$ Problem and the Mathematical Restriction Problem. I proposed several possible solutions to each horn of this dilemma, the most promising of which is the 'natural' restriction solution. This solution aims to restrict the $R_3$ through including only the mathematics in the $R_3$ that could be considered a 'natural' extension in the way that complex numbers could be considered a 'natural' extension of the real numbers. The plausible, general success of this solution was outlined with its success in restricting the $R_3$ for the CAM approach. The most significant further work that needs to be carried out is developing the idea of the IC as being the third axis of the SV approach, where inter-theory relationships are along the horizontal axis and the relationships between data, phenomena and theoretical models are along the vertical axis.

I will now investigate what answers the PMA can provide to the questions concerning the relationship between the faithfulness and usefulness of representations.

# 7 | Pincock's Mapping Account and the Rainbow

## 7.1 Introduction

In this chapter I will turn my attention to Pincock's Mapping Account of representation, subjecting it to the same questions I posed to the IC: what is the relationship between the faithfulness of an idealised representation and the usefulness of such a representation?; and are the structural resources used by the PMA to accommodate different types of idealisations are the same? I will argue that a major problem with the PMA is an inability to establish a general explanation for how usefulness and faithfulness are related. This is in part due to Pincock's approach to the content of representations and what he believes we can infer from the content, and in part due to the position of 'metaphysical agnosticism'. This is a position he adopts in response to arguments concerning emergentist and reductionist interpretations of singular perturbation representations. The 'metaphysical agnosticism' position raises questions over the generality of Pincock's account and whether Pincock's methodology is compatible with Contessa's approach towards faithful epistemic representation.

I will again make use of the two idealisation examples from the rainbow, the $\beta \to \infty$ singular limit and the move to the complex plane involved in the

CAM approach. Pincock discusses this example himself. He makes comparisons between the $\beta \to \infty$ idealisation, the $k \to \infty$ and his attitude towards singular perturbations. I will argue that this comparison is not informative with respect to metaphysical agnosticism. Pincock also contrasts his views on the CAM approach to Belot's and Batterman's position on the rainbow. I explore Pincock's metaphysical attitude towards the CAM approach and conclude that he does not hold a metaphysical agnosticism position towards it. Rather, I suggest that he has adopted a kind of reductionist position towards the critical points involved in the CAM approach. Pincock's discussion of Belot's and Batterman's positions is also unhelpful for understanding his metaphysical agnosticism position. I conclude the chapter by arguing that metaphysical agnosticism is either too undeveloped to hold as a serious position or collapses into a form of reductionism.

## 7.2 Pincock's Mapping Account and Idealisation

Idealisation is, in general, dealt with very straightforwardly on Pincock's account by his various notions of content, the flexibility in what structural relations he admits, and the specification relation. Despite this ease of dealing with idealisations in general, I will argue that Pincock's approach threatens a unified account of how faithfulness and usefulness are related on the PMA.

### 7.2.1 Account of Idealisation

The majority of idealisations will be accounted for by enriched contents, structural relations that involve mathematical terms and the specification relation. How these are handled is best explained by Pincock himself, when he introduces the enriched contents (Pincock 2012, pg. 31):

> In the heat equation, we have to work with small regions in addition to the points $(x, t)$ picked out by our function. We should take these regions in the $(x, t)$ plane to represent genuine features of the temperature changes in the iron bar. This representational option is open to use even if the derivation and solution of the heat equation seem to make reference to real-valued quantities and positions. We simply add to our representation that we intend it to capture temperature changes at a more coarse-grained level using regions of a certain size that are centered on the points picked out by our function. The threshold can be set using a variety of factors. These include our prior theory of temperature, the steps in the derivation of the heat equation itself, or our contextually determined purposes in adopting this representation to represent this particular iron bar. [Pincock's] approach is to *incorporate all of these various inputs into the specification of the enriched content.* The enriched content has a much better chance of being accurate as it will typically be specified in terms of the aspects of the mathematical structure, which can be more realistically interpreted in terms of genuine features of the target system.
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> ...
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> We arrive at the enriched content of a representation, then, by allowing the content to be specified in terms of a structural relation with features of the mathematical structure beyond the entities in the domain of the mathematical structure. The resulting structural relations need to be more complicated than just simple isomorphisms and homomorphisms. In particular, *we allow the specification of the structural relation to include mathematical terminology.* For example, we may posit an isomorphism between the temperatures at times and $u$ in the mathematical structure subject to a spatial error term $\epsilon = 1mm$.

More sophisticated idealisations will involve schematic contents. The example of the deep water waves shows that not all cases of taking singular limits involves a case of singular limit idealisation.[1] Remember that a singular limit idealisation has to necessarily involve the limit. The deep water wave case is justified along Galilean lines, and as such the limit can be removed. Yet Pincock argues that the deep water wave idealisation should not be understood as the taking of a limit at all, but rather in terms of taking a set of scales. I shall call this sort of idealisation *perturbation idealisation*. This itself can be split into two types: regular and singular perturbation idealisation. Pincock argues that at least some cases of singular limit idealisation should be understood in terms of singular perturbation idealisation. In doing so, one can solve some of the problems introduced by Batterman, namely those of interpreting the limit. I will introduce the deep water wave example, first focusing on how it is justified in terms of a Galilean idealisation, then on how Pincock argues for it to be understood as a regular perturbation idealisation. I will then summarise how the notions of content and the specification relation are involved in a singular perturbation idealisation. This will allow me to move onto a discussion of how faithfulness and usefulness are related on Pincock's account.

The deep water wave representation aims to find the speed of a wave crest. This involves starting with the Navier-Stokes equation, applying the 'small amplitude' idealisation, then the deep water wave idealisation to find a simple representation in terms of the wavelength of the wave. When introduced as a Galilean idealisation, the deep water wave idealisation involves taking the variable for the depth of the ocean, $H$, to infinity. We take $H$ to infinity because we obtain a $\tanh x$ term, where $x = \frac{2\pi H}{\lambda}$, and as $x \to \infty$, $\tanh x \to 1$. Thus, when the depth of the ocean is much greater than the wavelength of the waves, such as when $H > 0.28\lambda$, we can replace the $\tanh x$ term with 1 and obtain a maximum error of 3%. Remember that on the PMA, the mathematics has an interpretation. This would seem to imply that taking $H \to \infty$ involves

[1]See Pincock (2012) pg. 96-104 for the full details of Pincock's analysis of this example.

the claim that the ocean is now infinitely deep. This is not the case, however as taking the limit involves decoupling the interpretation from $H$, leaving us with an unspecified parameter. Thus the representation can be described as having nothing to say about the depth of the ocean after taking the limit.[2] Decoupling interpretations also involves the specification relation changing (as this supplies the interpretation). Justifying an idealisation through appealing to a maximum error also implies the adoption of a structural relation which includes such an error term. Thus the only difference between the simple cases of idealisation mentioned above that involved the enriched content is the move to schematic content and the lack of interpretation for the mathematics that constitutes the schematic content.

The main motivation Pincock offers for understanding such an idealisation (and singular limit idealisations) in terms of perturbation idealisations is that we can replace the "vague notion of an approximately true claim with the claim that for phenomena $p$ some set of scales $s$ is adequate" (Pincock 2012, pg. 103). This is promising for getting a grip on the relationship between faithfulness and usefulness, given that the IC cashes out approximate truth in terms of partial truth. As I argued above, partial truth and the associated partial structures are capable of providing an explanation of how faithfulness and usefulness are related. To understand the deep water wave example in terms of a regular perturbation, Pincock first introduces us to the notion of scales. Here a variable is replaced by a 'scaled' version of it. For example, the $x^*$ variable ($^*$ denoting an original variable) is replaced by $x = \frac{x^*}{\lambda}$. There is often a choice in the set of scales, that is, which variables we choose to replace and which variable we divide the original by. Our choice of $x$ and $\lambda$ constitutes the claim that the "relevant processes operate in the $x$ spatial direction only on the order of $\lambda$". The idea is that we are attempting to show that the contribution of certain

[2] The other interpretation of schematic content is that these are pieces of the mathematics which have no interpretation. See §4.2.2 - *Pincock's Notion of Content*.

terms are negligible relative to other terms (preferably that non-linear terms are negligible relative to linear terms, for ease of calculation). Notice that our chosen replacement also results in $x$ becoming dimensionless (it involves a length variable divided by a length variable). The set of scales chosen for the deep water wave example result in the claim that (Pincock 2012, pg. 103):

> terms preceded by $h$ become orders of magnitude more important, or equivalently, that terms preceded by $\frac{1}{h} = \frac{\lambda}{H}$ become orders of magnitude less important. When $h$ appears in the boundary conditions, this has the effect of moving the boundary as far from the system as possible. Thus for this set of scales to be adequate, boundary effects must be irrelevant to what we aim to represent

We establish whether a set of scales is adequate by seeing whether the set of scales "reveals anything of relevance to $p$", and then conducting "additional tests ... to see if the representation does indeed accurately capture" $p$. If it does, then we have an adequate set of scales.

In summarising his discussion of why sets of scales (and by implication, perturbations) are important, Pincock gives the clearest description of how he thinks faithfulness and usefulness are related (Pincock 2012, pg. 104):

> What we see, then, is a kind of trade-off between the completeness of a representation and its ability to accurately represent some phenomena of interest. ... The problem is basically that a complete representation of a complex system will include countless details, and these details typically obscure what we have selected as the important features of the system. By giving up completeness, and opting for a set of scales, we shift to a partial representation of the system that aims to capture features that are manifest in that scale. This partiality gains us a perspicuous and accurate representation as well as an understanding of features that would otherwise elude us.

The idea here seems to be that by adopting a particular set of scales we are introducing some mathematical structure that partially represents the target system and in doing so reveals certain features at that scale. Thus we will be relating part of the target system to our mathematical structure. This will involve the structural relation mapping from the mathematics to only part of the target system, with the specification relation helping us to interpret the mathematical structure appropriately for the scale we are using. i.e. That parts of the mathematics should not be interpreted at *this* scale (are schematic contents) in the way they would in a mathematical structure that is a complete representation.

In order to make the notion of "adequate set of scales" more precise, Pincock turns to perturbation theory.[3] Our original problem involved finding an unknown function $f(x)$ that satisfies some differential equations and boundary conditions. We change it by recasting $f(x)$ as a function of $f(x, \epsilon)$, where $\epsilon$ is a value that is small for the domain in question. We then aim to find an asymptotic expansion of $f(x, \epsilon)$ to $N$ terms, and use the magnitude of terms over a certain order of the asymptotic expansion of the function to establish which terms of the asymptotic expansion are to be kept and which are to be rejected. This allows us to find an answer to any degree of accuracy. i.e. the $\epsilon^2$ term might be smaller than experimental accuracy, so all $\epsilon$ terms of the order greater than 2 can be rejected.

A regular perturbation is one where we can start with $\epsilon = 0$, and add correction terms from the asymptotic expansion (i.e. where the order of $\epsilon > 0$). A singular perturbation, however, occurs when there is a difference in qualitative character between the $\epsilon = 0$ and $\epsilon \neq 0$ cases. Pincock uses the example of the equation $\epsilon m^2 + 2m + 1 = 0$. When $\epsilon = 0$, this equation has roots $m = -\frac{1}{2}$, but when $\epsilon \neq 0$, the equation has two roots which cannot be recovered

[3]The details can be found in his (2012), pg. 104-105.

from the $\epsilon = 0$ case (they are in fact undefined when $\epsilon = 0$). In such a case, we shift to singular perturbation theory and have to employ multiple scales. What is nice about Pincock's moves here is that he has given us a formal framework within which to understand the difference between the limits in Galilean idealisations, which can be removed, and those which produce singular limits, such as in Batterman's examples. If we can use regular perturbations, then we have a Galilean limit, whereas if we have to singular perturbations, we have singular idealisation type limits.

Singular perturbations result in a different story of how the mathematical structure, specification relation and structural relations meet. Whereas in the regular perturbation case, one merely introduces some schematic content (and hence that part of the mathematics is decoupled from its interpretation), singular limits result in the need to give the resulting representations "a different physical interpretation than the original representations ... the singular character of the limit can be linked to the need to offer an interpretation in terms of different physical concepts" (Pincock 2012, pg. 223). I take this to mean that a singular limit causes all of the content to become (momentarily) schematic, before being given a new interpretation (i.e. a new specification relation), though some schematic content will obviously stay schematic (such as the term(s) involved in the singular limit/parameter that produces the singular perturbations).

### 7.2.2 PMA, Faithfulness and Usefulness

The above discussion has made it clear that Galilean idealisation requires different structural moves to singular (perturbation) idealisation on Pincock's Mapping Account. This is the answer to the second question posed at the start of this chapter.[4] Due to these differences in structural moves, there might be

[4] Are the structural resources used by the PMA to accommodate different types of idealisations the same?

differences in how faithfulness and usefulness are related in each case. I will look first at Galilean idealisation, and then at what Pincock says about interpreting three examples of singular perturbations: the boundary layer theory, a damped harmonic oscillator, and Bénard cells. Pincock advocates adopting a position of metaphysical agnosticism towards the damped harmonic oscillator and Bénard cells. I will explore what this position entails for the relationship between faithfulness and usefulness on the PMA.

The matter is straightforward for Galilean idealisation. Such an idealisation is partially faithful due to the structural similarity between the enriched (and genuine) contents and the target, cashed out by the (mathematically infused) structural relation. The usefulness is then accounted for by the specification relation and how this alters the structural relation. Remember that Pincock wants to include such factors as what we intend our representation to capture (i.e. temperature at a coarse-grained level), contextual determined purposes of adopting a particular representation, and so on, into the specification of the enriched content, i.e. the specification relation (Pincock 2012, pg. 30-31).[5] Being reductive, we can describe Galilean idealisations as being useful on the PMA because we design them to be: either we adopt the appropriate contents for our theoretical concepts we are trying to represent (e.g. enriched contents and temperature), or due to the mathematics we wish to use, and adopt a suitable structural relation for these purposes (a structural relation that contains something similar to an error term that allows for more useful representations to be given). The general idea is to obtain a structure that highlights relevant features.

The relationship between faithfulness and usefulness is much more complicated in the case of singular perturbation idealisations.[6] The most obvious

[5]See my discussion of this in §4.2.2 - *Pincock's Notion of Content* and of the specification relation in §4.2.2 - *Pincock's Structural Relations and Specification Relation*.

[6]While Pincock claims that the "vague" notion of approximate truth can be made more precise by the notion of an adequate set of scales, I reject this as being useful for the notion

complicating factor is that these representations are likely to have a high quantity of schematic content. While this might seem like it would make things easier (as there would be less interpreted content that could be described as faithful), this is not the case as we also have the recoupling of some of this content in terms of different physical concepts than those involved in the initial representation.

### Boundary Layer Theory

One of the key techniques involved in fluid dynamics is the boundary layer theory.[7] Here, the flow of a fluid around an object is analysed. One wishes to find out how the pressure and velocity values for the fluid are arranged around the object. To do so one can either adopt the Navier-Stokes equations (though these turn out to be intractable for this problem) or the Euler equations and adopt a set of scales. Unfortunately, the second option results in the prediction of zero drag on the object, which can be shown to be false through simple experiment (Pincock 2012, pg. 108-109). The cause of this problem is the assumption that the set of scales is adequate for the whole domain. By splitting the domain into two and adopting a sets of scales for each region of the domain, we can find a solution by matching the sets of scales at the boundaries of the regions. The region which is closest to the object is called the boundary layer, and its set of scales is picked using a new quantity $\delta$, which is described as the width of the boundary layer (Pincock 2012, pg. 110). This representation is very successful. Yet, although "the edge between the boundary layer and the outer region is fundamental to the representation, ... we do not take it to represent any genuine edge in the system itself", and there are in fact several inconsistent ways of establishing the value of $\delta$ (Pincock

of faithfulness. The way in which Pincock cashes out the notion of adequacy is binary, rather than gradable.

[7] See §5.6 of Pincock's (2012) for his presentation and discussion of the boundary layer theory. He relies on Kundu & Cohen (2008), §10 for his presentation.

2012, pg. 112), (Kundu & Cohen 2008, pg. 346-348). This prompts Pincock to claim that there is no warrant for a metaphysical interpretation of the edge between regions (i.e. that there are two regions in any physical sense). He also promises that he will go on to argue that "we have no general reason to conclude that the accuracy of [singular perturbation idealisations reveal] new underlying metaphysical structure" (Pincock 2012, pg. 113). Thus the lesson we should take from the boundary layer example is that singular perturbations can produce mathematics of purely instrumental benefit: they allow us to obtain an accurate result but with no new physical structure being uncovered. This would suggest that the link between faithfulness and success is broken here, in that the content of the representation is wholly schematic.

One might wonder how we are to get accurate results out of a representation which contains wholly schematic content. The answer to this is to recall Pincock's distinction between intrinsic and extrinsic mathematics, core concepts, and that we move from schematic content to genuine content in the process of producing a prediction. In order to obtain a prediction from a representation with schematic content, we must specify parameters according to the target system and in doing so we move from schematic content to genuine content (Pincock 2012, pg. 32). The solutions to equations are not always part of the content of our representations (Pincock 2012, pg. 38). In this instance I must conclude that there is no link between the faithfulness of a representation and its usefulness. It is tempting to call such representations entirely unfaithful: if the content of the representation is wholly schematic, then it can be understood as saying nothing about the target system. In such an instance, the mathematics could be argued to not even constitute an epistemic representation as it would no longer be interpreted in terms of the target. The mathematics would still be taken to be denoting the target. The adoption of genuine content might solve this problem, providing an interpretation of the mathematics in terms of the target again. However, there does not appear to be any link between the representation with genuine content and the target that could satisfy the requirements of answering how the partially faithfulness of the representation and its usefulness is related. The representation is useful due to the schematic content being made into genuine content through the filling in of specific parameters.[8]

The next two examples, the damped harmonic oscillator and the Bénard cells, are two examples of a different type of singular perturbation. The boundary layer theory involves splitting the domain into two parts, with a set of scales for each region. In the damped harmonic oscillator and Bénard cells examples the domain remains the same, but a different set of scales is adopted for different time periods.

**Damped Harmonic Oscillators**

For the damped harmonic oscillator, we start with the following equation:

$$my'' + cy' + ky = 0 \tag{7.1}$$

If we assume that the damping effects are small compared to the dominant process, we might adopt a set of scales such as:

$$t = \frac{t^*}{\sqrt{\frac{m}{k}}} \tag{7.2}$$

However, as time increases, this set of scales fails (Pincock 2012, pg. 115). Thus we should conclude that "we cannot assume there is a single process operating on a single time scale", and think of the $y$ as a function of two variables $t_F = t$ and $t_S = \epsilon t$. When $\epsilon << 1$, $t_S << t_F$ so $t_S$ "corresponds to a 'slow' time scale".

The damped harmonic oscillator example is used by McGivern (2008), who

[8]The worries here not unique to Pincock's account: similar concerns will arise on any account for any instrumental representation.

argues in favour of a physical notion of emergence against 'micro-based physicalism'.[9] In general, McGivern argues that the multi-scale analysis present in the damped harmonic oscillator involves properties that do not fit into Kim's[10] conception of reduction (McGivern 2008, pg. 54). Kim's conception involves identifying all higher-level properties with distinct micro-based properties, understood as mereological configurations of lower-level micro-constituents.[11] McGivern's position can be summarised as the claim that multiscale analysis "often involves decomposing a system's behavior into components operating on different scales" as opposed to "explaining a system's behavior by relating it to the behavior of its micro-constituents" i.e. physical, mereological decomposition.

The multi-scale structure outlined above in the damped harmonic oscillator is a high-level structure. McGivern sets out the approach a micro-physical reductionist should take towards this property (McGivern 2008, pg. 64):

> To accommodate the kind of multi-scale structure found in multi-scale analysis within the standard framework of 'micro-based' levels, we would need to show how multi-scale structural properties can be identified with distinct micro-based properties and then cashed out in terms of 'specific

[9]McGivern does not actually argue that reductionism is false, but that arguments for it must be given "in terms specific to the properties, explanations, and theories involved, rather than in the broad terms characteristic of arguments about causal competition" (McGivern 2008, pg. 55). One might read the position McGivern adopts here as similar to the one Pincock advocates, that we "should use multiscale representations to help reform and sharpen the metaphysical positions, rather than using them to champion one or the other side". I will look at this briefly below, when considering if Pincock adopts a "wait and see" agnostic position.

[10]See Kim (1998, 2003).

[11]As McGivern explains it, Kim's account starts by characterising the property to be reduced "in terms of its 'functional' or causal role and then *identifying* that property with whatever property fills that role on a given occasion. Importantly, these properties are assumed in general to be 'micro-based' properties, where a micro-based property is the property of having particular parts which themselves have particular properties and stand in certain relations" (McGivern 2008, pg. 58). For example, 'being a water molecule' is 'the property of having two hydrogen atoms and one oxygen atom in such-and-such bonding relationship'. Causation need not only occur at the micro-level however. 'Being a water molecule' is a higher-level property of a higher-level entity, i.e. a molecule. Thus we can have causation at higher-levels, between higher-level entities. Kim also carries out this reduction for structural properties (Kim 1998, pg. 117-118).

mereological configurations' of micro-level entities and their properties.

If one were to explain the structure of an organism in this way, one would identify the structure of the organism with its cellular structure, then this structure with its component parts and so on until one reaches the genuinely micro-based properties. And now McGivern comes to the crux of the issue (McGivern 2008, pg. 64-65, my emphasis):

> In "structural" terms, we can informally characterize this as "the property of having parts $S$ and $F$" where $S$ is the slow scale component and $F$ is the fast scale component. However, unlike in more familiar cases, such as those involving cellular structure, *the decomposition into fast and slow components doesn't give us spatial parts-instead, the decomposition is of a different "non-spatial" sort. Hence it's difficult to see how these could be properties of "non-overlapping" parts, as in the case of micro-based properties.* ... But in the case of decomposition into fast and slow components, *it is the same particles at the micro-level that realize both the fast and the slow components: there is no division of the system into "fast" and "slow" particles.* Hence we can't identify the slow component with one configuration of micro-level entities and the fast component with another, even if "configuration" is conceived of dynamically to allow for the fact that we are dealing the behavior of a system over time. So we appear to have a kind of structural property that doesn't fit with the standard micro-based account of reductive levels, understood in terms of entities related by ("spatial") parthood and characterized exclusively by micro-based properties.

And again later in the paper (McGivern 2008, pg. 70):

> But multi-scale structural properties aren't [spatial]: they don't involve a move from the relatively large to the relatively small. Hence, the kind of thinking that usually motivates the belief that macro-level properties

> can be identified with micro-based ones - the repeated explanation of phenomena in terms of smaller and smaller entities - doesn't get off the ground in the case of multi-scale structure.

Thus we can see how one might understand multiscale representations as not being capable of being reduced to micro-level properties, at least according to Kim's account. McGivern's arguments are not a complete rejection of reduction however. He emphasises that he hasn't tried to argue against reductionism in philosophy of mind (McGivern 2008, pg. 73). Rather his point is that "it seems wrong to begin with an assumed structure of 'levels' and then try to fit our theories and explanations to that structure: instead, we need to begin with our theories and explanations and see what sort of structure they imply", that is, we should start with our physical theories and attempt to read our metaphysical views on structures and levels from those theories (McGivern 2008, pg. 74).

### Bénard Cells

Bénard cells are used as an example by Bishop (2008) to argue in favour of "downward causation", against Kim's physical reductionism. Bénard cells are a stable configuration of a heated liquid. A liquid is sandwiched between two plates. The bottom plate is heated while the top plate is kept at a constant temperature. If the temperature gradient between the top and bottom plates is large enough, then the relevant dynamics of the fluid system shifts from the diffusion process, which occurs over a short time scale, to the faster convective process (Bishop 2008, pg. 236) (Pincock 2012, pg. 116). The diffusion process results in small scale effects, while the convective process results in large scale effects. The stability of the diffusion process is broken during the heating process, though stability is regained with the Bénard cells. The cells are "large-scale features of the fluid system that influence how the small-scale fluid elements move". The idea here is that we need two scales, one for the diffusion process and one for the Bénard cells. The size of the parameter we choose in establishing these scales informs us as to which set of scales dominates. The Reynolds number is adopted as the parameter; when it is small the diffusion process dominates, when it is large the cells dominate.

Bishop adopts a view of emergence that includes a notion of downward causation put forward by Thompson and Varela (Bishop 2008, pg. 230), quoting (Thompson & Varela 2001, pg. 420):

> **(TV)** A network, $N$, of interrelated components exhibits an emergent process, $E$, with emergent properties, $P$, if and only if:
>
> (a) $E$ is a global process that instantiates $P$ and arises from the non-linear dynamics, $D$, of the local interactions of $N$'s components
>
> (b) $E$ and $P$ have global-to-local ('downward') determinative influence on the dynamics $D$ of the components of $N$
>
> And (possibly):
>
> (c) $E$ and $P$ are not exhaustively determined by the intrinsic properties of the components of $N$, that is, they exhibit 'relational holism'

This proposal includes the claim that a property emerges not just *qua* property, but is instantiated in a process or some other "dynamical 'entity' unfolding in time".

Bishop goes on to argue that Bénard cells display features indicative of emergent behaviour. More generally, he argues that nonlinear systems require some kind of "particular global or nonlocal description", as the "individual constituents cannot be fully characterized without reference to larger-scale structures of the system", due to the principle of linear composition failing (Bishop 2008, pg. 231). In particular, he claims that Bénard cells exhibit the behaviours of control hierarchies and constraints (Bishop 2008, pg. 237):

> The Rayleigh-Bénard system clearly exhibits the features listed in Sect. 2.2 [(Bishop 2008, pg. 232)]. As $\Delta T$ exceeds $\Delta T_c$, the homogeneity of the distribution of the fluid elements and some of the spatial symmetries of the container are broken and the fluid elements self-organize into distinguishable Bénard cells. There is a hierarchy distinguished by dynamical time scales (molecules, fluid elements, Bénard cells) with complex interactions taking place among the different levels. Fluid elements are situated in that they participate in particular Bénard cells within the confines of the container walls. The system as a whole displays integrity as the constituents of various hierarchic levels exhibit highly coordinated, cohesive behavior. Additionally, the organizational unity of the system is stable to small perturbations in temperature and adapts to larger changes within a particular range.
>
> Furthermore, Bénard cells act as a control hierarchy, constraining the motion of fluid elements. Bénard cells emerge out of the motion of fluid elements as $\Delta T$ exceeds $\Delta T_c$, but these large-scale structures determine modifications of the configurational degrees of freedom of fluid elements such that some motions possible in the equilibrium state are no longer available.

Bishop argues that the fluid elements are necessary but insufficient to account for the existence and dynamics of the Bénard cells, or even their own motions. The local dynamics are constrained by the large-scale structures. Together the fluid elements, Bénard cells and the system wide forces form the necessary and sufficient conditions for the behaviour of the system (Bishop 2008, pg. 239). Thus he claims thats that Bénard cells satisfy (a), (b) and (c) of the **TV** proposal of downward causation (Bishop 2008, pg. 240):

> Bénard cells arise from the dynamics of fluid elements in the Rayleigh-Bénard system as $\Delta T$ exceeds $\Delta T_c$, where each fluid element becomes

> coupled with every other fluid element (element (a) of **TV**). More importantly, Bénard cells act as a control hierarchy, constraining and modifying the trajectories of fluid elements; that is, Bénard cells have a "determinative influence" on the dynamics of the fluid elements as lower-level system components (element (b) of **TV**). Moreover, although the fluid elements are necessary for the existence of Bénard cells, the former are insufficient to totally determine the behavior of the latter. This relationship between necessary and sufficient conditions seems implicit in (b), but due to its importance in issues surrounding reduction and emergence, this relationship should be brought out explicitly (e.g., Bishop (2006); Bishop & Atmanspacher (2006)).
>
> This relationship is a nonquantum kind of "relational holism""understood in Teller's (1986) sense, where, the relations among constituents are not determined solely by the constituents' intrinsic properties. The properties of integrity, integration and stability exhibited by Bénard cells are relationally dynamic properties involving the nonlocal relation of all fluid elements to each other (element (c) of **TV**). . . . However, the behavior of Bénard cells as units differs from holistic entanglement in quantum mechanics in the sense that fluid elements may be distinguished from each other while they are simultaneously identified as members of particular Bénard cells and participate in interaction with fluid elements throughout the system.

The key difference between the boundary layer example and the damped harmonic oscillator and Bénard cells examples is that we are not *spatially* dividing the domain, but claiming that there are two processes operating at different time scales.

### Metaphysical Agnosticism

Bishop and McGivern appear to have presented good arguments which indicate that we should reject reductionism (at least the sort offered by Kim's account) for systems that can be represented through temporal multiscale representations. Bishop takes a rejection of reductionism for the Bénard cells to justify a rejection of reductionism completely. McGivern thinks that we are only justified in rejecting reductionism for temporal multiscale representations and is open to adopting reductionism in other systems. One's response to these arguments will depend on whether one considers there be other options available. Pincock argues that we do have another option, namely his 'metaphysical agnosticism' (Pincock 2012, pg. 119).

Pincock claims that his metaphysical agnosticism is a third way between adopting a "metaphysical interpretation", i.e. reductionism or emergentism, and what he calls "instrumentalism". That is, he offers a third table entirely with reductionism and emergentism the options on the metaphysical table, instrumentalism a second table and his "metaphysical agnosticism" the third table. Metaphysical agnosticism involves (Pincock 2012, pg. 119-120 (my emphasis throughout)):

> emphasiz[ing] the epistemic benefits that multiscale representation affords the scientist. On this picture, *a successful multiscale representation depends on genuine features of the system. We come to know about these features when a multiscale representation yields a successful experimental prediction.* In this respect, it goes beyond the instrumentalist position. But it does not end up with a reductionist or emergentist metaphysical interpretation because the epistemic approach is consistent with a rejection of both reductivism and emergentism. To see why, consider the Bénard cells. ... [The] epistemic point about what we know based on our limited access to the details of the fluid dynamics need not entail

> any metaphysical conclusions about the existence of the Bénard cells in some more robust sense. Clearly, we can only understand the system by appeal to these larger-scale structures. But this may be a product of our ignorance and in the end may not correspond to what a fuller understanding of the system would reveal. ... More generally [Pincock] suggest[s] that *we cannot move from the success of a multiscale representation to the conclusion that it reveal novel metaphysical features of the physical system*. This metaphysical agnosticism is consistent with delineating a crucial epistemic role for the multiscale techniques in producing scientific knowledge.

I take Pincock to be arguing that we can conclude some *novel physical* features of a system from successful multiscale representations (first emphasis), but that we cannot (in general) use such representations to make *novel metaphysical* claims (second emphasis). Such a position would require some of the content of the representation to be given a new physical interpretation. This would be a limited interpretation as we would be unable to infer anything metaphysically novel about the target system from this interpretation, e.g. whether this feature is due to emergence. Thus we have a case where singular perturbation theory results in a representation that is not purely schematic content, as both of our sets of scales have physical interpretations, though they are limited in the sense just described. This would involve a limited specification relation and an appropriately restricted structural relation.

There are some major differences between the boundary layer theory, and the damped harmonic oscillator and Bénard cells, the most important of which is whether the multiple sets of scales represent genuine features of the target system. In the boundary layer theory, the reasons for doubting the reality of the layer are that there are inconsistent methods for setting its width and (Pincock implies) that the scales are due to a physical, i.e. spatial, split of the domain. Whereas in the other two examples, the two sets of scales are thought to represent two genuine processes due to the accurate predictions they give (and possibly as they involve a difference in *time*, rather than *spatial* scale). As quoted above, Pincock's metaphysical agnosticism commits us to a genuine feature of the target system "when a multiscale representation yields a successful experimental prediction". The boundary layer theory, however, gave very good predictions, yet Pincock states that "we have no general reason to conclude that the accuracy of these [multiscale] representations reveals any new underlying metaphysical structure" (Pincock 2012, pg. 113). If we accept that one can maintain a distinction between a genuine feature of the system and an "underlying metaphysical structure", then I think that Pincock's metaphysical agnosticism involves an explanatory gap that threatens his account's ability to explain how faithfulness and usefulness are related. Accepting for the sake of argument that we can admit a genuine feature of the system while having its metaphysical *qua* reductionist or emergentist origins being unknown, there is still the question of how the structure of this genuine feature identified by the representation is related to the structure of the target system (the metaphysical origin being one way of establishing this relationship). Establishing how the structure of the genuine feature identified by the representation is related to the target system will provide the explanation of how the representation can be as partially faithful as it is, and yet still be useful.

The problem here is that I do not think Pincock's arguments against either what he calls the "instrumentalism" view of the mathematics or against the metaphysical line are at all convincing. Further, I do not see how, *prima facíe*, his position is clearly distinguished from Batterman's. I will now outline the problems I have with Pincock's arguments against the instrumentalist and metaphysical positions. I will discuss the relationship between Pincock's and Batterman's positions in §7.3.2 - *Pincock, Batterman & Belot.*

Pincock describes instrumentalism about the mathematics as the following attitude (Pincock 2012, pg. 119):

> [R]escaling with multiple scales is just a tool that allows us to handle otherwise intractable mathematical equations. We have learned how to work with this tool, but it need not reveal anything about the underlying features of the systems represented. Multiscale modelling, then, is just a piece of brute force mathematics whose larger significance can be ignored.

This view is similar to one way in which surplus structure can be used: we adopt some further mathematics to shed light on some intractable problem from our initial representation of the target system. However it is not entirely the same, as in some cases the use of surplus structure can indirectly reveal underlying features of the systems being represented, such as group theory highlighting the role of symmetry in quantum mechanics.[12] The instrumentalist position justifies the use of the mathematics pragmatically, by providing a way to solve otherwise intractable mathematics. There is no question over the faithfulness of the mathematics, as it is not representing anything on the instrumentalist view. The surplus structure view has already been accounted for, during the discussion of the IC.

Pincock rejects the instrumentalist position by claiming that "successful multiscale representation depends on genuine features of the system", and that "we come to know about these features when a multiscale representation yields a successful experimental prediction". His rejection of instrumentalism is tied to his commitment to his metaphysical agnosticism, as he rejects the dichotomy between the instrumentalist and metaphysical (reductionism and emergentism) positions. But merely pointing to accurate prediction is insufficient here, as instrumental uses of mathematics can produce accurate predictions. In order

[12] I argue below in §7.3.2 - *Pincock, Batterman & Belot* that Pincock has conflated the instrumentalist and surplus structure positions, given what he says about Belot's and Redhead's response to the rainbow.

to reject the instrumentalist position and the dichotomy between it and the metaphysical positions, there must be more work done to establish how multi-scale representations can depend upon genuine features in a way which these two broad positions cannot account for.

Perhaps we are to consider the metaphysical agnosticism as a 'wait and see' type of agnosticism. This understanding of the agnosticism has some textual support.[13] But this would not constitute a real rejection of both the instrumentalist and metaphysical positions. McGivern's position is *prima facíe* the same as Pincock's. McGivern claims that arguments for reduction should properly be given in terms that are "specific to the properties, explanations, and theories involved, rather than in the broad terms characteristic of arguments about causal competition" (McGivern 2008, pg. 55). He also claims that "it seems wrong to begin with an assumed structure of 'levels' and then try to fit our theories and explanations to that structure: instead, *we need to begin with our theories and explanations and see what sort of structure they imply*" (McGivern 2008, pg. 74, my emphasis). He doesn't reject reductionism *in toto*, just a version of reductionism that requires spatial decomposition for systems that require (time) multiscale representation. One can therefore understand McGivern as being open to the possibility of reductionism in other systems, and Pincock recognises this (Pincock 2012, pg. 117). McGivern's position towards reductionism therefore is a "wait and see" position, asking for further developments in the reductionist position to occur. If they do not occur, or are shown to be incapable of occurring, then we are entitled to rule it out as a viable option. Pincock rejects McGivern's position, however, taking McGivern to draw metaphysical conclusions when he endorses emergentism, apparently due

[13]Pincock talks of the understanding of the system being dependent upon the large-scale features (of the Bénard cells) being a possible result of our ignorance "and in the end may not correspond to what a fuller understanding of the system would reveal". He also claims that "[e]ither metaphysical position seems premature, even if we can reform these views to accommodate this kind of case" (Pincock 2012, pg. 119).

to understanding McGivern as arguing for the existence of emergent properties due to the genuine causal and explanatory power they are attributed[14] (Pincock 2012, pg. 117-119). The distinction between Pincock and McGivern, then, is that Pincock rejects reductionism and emergentism *in toto*, whereas McGivern thinks that they are still viable positions. Thus Pincock's agnosticism cannot include any sort of "wait and see", contra McGivern's position. Further, any inclusion of a "wait and see" attitude in his agnosticism would contradict his more general point that we cannot move from the success of these multiscale representations to novel metaphysical features.[15]

Given the above arguments, I do not see how Pincock can provide can provide an explanation of how a multiscale representation is partially faithful and useful. His metaphysical agnosticism position appears to prevent any suitable structural relationship being established between the target system and the content of the representation. This is a major problem. It appears that, for the damped harmonic oscillator and Bénard cell examples at least, Pincock's account reduces to the claim that representations are useful because they are successful, and because they are successful they are partially (in a limited sense) faithful. But the metaphysical agnosticism blocks (metaphysical) explanations for why they are faithful, so the only explanation for why they are useful is because they are successful (which is no explanation at all as this is clearly circular). A possible solution to this problem is that the general point Pincock is trying to make, that the success of multiscale representations does not ensure we can gain novel metaphysical features from them, is more of a guide than a hard and fast rule. That is, in some cases we can gain novel metaphysical features, in some cases we have to wait, and in some cases they are explicitly denied. This would mean that the relationship between faithfulness

[14]Pincock refers to McGivern's mentioning of fast scale gravity waves and slow-scale Rossby waves (McGivern 2008, pg. 70).

[15]Including a "wait and see" attitude would imply that we could move to novel metaphysical features, we would just have to wait.

and usefulness will depend on the example at hand; that there is no general relationship between these two features when multiscale representations are involved. If this is the case, and this point generalises to any representation, then Pincock's account is not so much a unified framework but rather a set of tools to be adopted and adapted to each case study. He would in fact have adopted a different methodological approach to Contessa and the proponents of the IC, where one has a unified framework and attempt to show how examples fit in with it. This will shift the debate from whether the IC or the PMA provides the best account of faithful epistemic representation, to whether the methodological approach of the IC or the PMA is best for accommodating the ideas behind faithful epistemic representation.

Before switching to this discussion of methodologies, there is one final option for understanding Pincock in a way that might be compatible with the unified framework view. This is to understand Pincock as adopting a similar position as Batterman, that such representations establish a new level of description of the system. When Pincock discusses the rainbow, he offers up his account as an alternative, though, similar view to Batterman (Pincock 2012, §11.6). Thus the rainbow can be used to analyse Pincock's views. In particular, the $\beta \to \infty$ idealisation is described to have similar interpretative challenges as the boundary layer theory, and the CAM approach is at the heart of the discussion of how similar Pincock's view is to Batterman.

## 7.3 The PMA Applied to the Rainbow

### 7.3.1 $\beta \to \infty$

Pincock advocates the taking of the $\beta \to \infty$ idealisation over the $k \to \infty$ (or $\lambda \to 0$) idealisation due to a desire to provide what he considers the "best" explanation of the colour distribution of the rainbow available from a ray representation (Pincock 2012, pg. 226):

> [T]he best explanation of [the colour distribution] takes light to be made up of electromagnetic waves. This is the best way to make sense of the colors of light and how the colors come to arrange themselves in the characteristic pattern displayed by the rainbow ... Without some relationship to our wave representation of light, [the ray theoretic] account of [the rainbow angle] seems to float free of anything we should take seriously. This point raises an instance of the central question of this chapter: how can we combine the resources of the clearly incorrect ray representation with the correct wave representation to provide our best explanations of features of the rainbow?

The $\beta \to \infty$ and $k \to \infty$ idealisations are possible ways of relating the ray and wave representations. I have already established in §6.3.1 that the $\beta \to \infty$ idealisation involves a singular limit. I will now look at whether this limit can be understood in terms of singular perturbation. I will outline claims in favour of and against understanding the $\beta \to \infty$ idealisation as involving singular perturbation and conclude that the $\beta \to \infty$ idealisation does not involve singular perturbation. I will then argue that this idealisation, and comments Pincock makes in comparison to the $k \to \infty$ idealisation and boundary layer theory, call into question Pincock's general approach to interpreting idealisations. At this stage, it is only a worry. The CAM approach will then be addressed to see whether it can provide more information on Pincock's metaphysical agnosticism and its relationship to Batterman's position.

The most obvious reason that one might think the $\beta \to \infty$ idealisation makes use of multiple sets of scales or singular perturbation theory is that the approach is justified in the same way as scales are. Pincock claims that the ray representation that results from the $\beta \to \infty$ idealisation "need not require the assumption that light is a ray. It may assume only that in certain circumstances *some features* of light can be accurately represented using the ray representation" and that it "can be reliably used if there is a size of raindrop above which the wave-theoretic aspects of light are not relevant to the path of the light through the drop" (Pincock 2012, pg. 226, 227). Compare this to the first discussion of scales for the deep water waves example: there the claim is that "terms preceded by $h$ become orders of magnitude more important" or "terms preceded by $\frac{1}{h} = \frac{\lambda}{H}$ become orders of magnitude less important" (Pincock 2012, pg. 103). In both examples, the the justification of taking $\beta \to \infty$ idealisation appears to be the same as in the deep water case: a dimensionless parameter is taken to such a size that other terms do not contribute to the solution compared to those preceded by the parameter or vice versa.

Secondly, Pincock compares the $k \to \infty$ idealisation with the application of singular perturbation in the boundary layer theory example, and draws a lesson from the comparison (Pincock 2012, pg. 227):

> we need to pay attention to both the mathematical links between the two representations and their proper physical interpretation. Some mathematical links preclude any viable interpretation. This fits with the way singular perturbation theory was used to develop the boundary layer theory representation in chapter 5 [see §7.2.2 - *PMA, Faithfulness and Usefulness* above]. There we saw that the width of the boundary layer posed an interpretive challenge based on its central place in the representation.

I think a fair inference to draw here is that the limit involved in the $\beta \to \infty$ idealisation is one of these particular mathematical links that requires us to pay attention to the mathematics and the interpretations, possibly preventing a viable interpretation. That is, the limit is one that involves singular perturbation, and due to being a physical scale, as in the boundary layer case, it might require careful physical interpretation or even preclude physical interpretation. However, precluding a physical interpretation would be odd, as that is clearly the problem with the $k \to \infty$ idealisation. Thus, one might expect the $\beta \to \infty$ idealisation to provide a challenge to being interpreted physical, but a challenge that should be able to be met.

The claims against the $\beta \to \infty$ idealisation involving singular perturbation are far stronger than the two claims above. First, the size parameter, $\beta$, is not introduced as a scale parameter, but rather as a standard abbreviating substitution (replacing several terms with one term). For example, look to the derivation of the Mie solution in Grandy Jr (2005). The Mie solution is derived for a homogeneous dielectric sphere, during which equations represent internal and external electric fields, such as the following equation, (3.83a) in (Grandy Jr 2005, pg. 86):

$$\Pi_1^{ext} = i\frac{\cos(\phi)}{kN_0}\sum_{l=1}^{\infty}\epsilon_l a_l h_l^{(1)}\left(N_0 kr\right) P_l^1(\cos\theta) \tag{7.3}$$

$\beta$ is then defined as follows:

$$\beta \equiv N_0 ka = N_0\frac{2\pi a}{\lambda_0} \tag{7.4}$$

where $r = a$ for the boundary of the water drop. In order to be introduced as a scale parameter, a suitable variable is supposed to be multiplied or divided by either $k$ or $a$, and this clearly has not happened. Second, if the size parameter were to involve singular perturbation then one would expect there to be some discussion of the use of perturbation theory in the physics texts. This is not the case; in fact there is no mention of perturbations in any of the relevant sections of the physics texts I have used.[16]

[16] Perturbation theory is mentioned in the following books in the following contexts, all of which are not relevant to obtaining the ray representation from the Mie solution via the $\beta \to \infty$ idealisation: Grandy Jr (2005): inhomogenity (pg. 274) or distorted (i.e. non-spherical) spheres (pg. 297); Liou (2002) contains nothing in the relevant chapter (chapter 5); Adam (2002) refers to the topology of caustics being stable under perturbations being relevant to rainbows; and Nussenzveig (1992) also discusses perturbations in the context of

Third, the $\beta \to \infty$ idealisation does not result in any features indicative of singular perturbations. Above I argued that Pincock claimed singular perturbations grant us access to novel physical features of systems, but not novel metaphysical features. The only thing we get out of the $\beta \to \infty$ idealisation is the claim that we can treat the direction of propagation of the waves as acting like rays. This is hardly a novel feature, we have used ray optics to describe the direction light travels for centuries before the Mie solution was obtained.

I therefore conclude that the $\beta \to \infty$ idealisation does not involve a limit that can be dealt with by singular perturbation. This conclusion calls into question Pincock's comparison to the boundary layer theory and the correctness of the lesson he claims we should learn from the $k \to \infty$ idealisation. The boundary layer theory introduced a difficulty in interpreting the width of the boundary layer for two reasons: the decomposition of the domain involved a spatial decomposition; and that there were multiple contradictory ways of defining the width of the boundary layer. Neither of these features play a role in the $\beta \to \infty$ idealisation: not only are there no sets of scales involved, meaning that there is no actual decomposition of the domain in spatial terms, there is only one way to define the size parameter. So the problems that arose with the boundary layer theory are not relevant to the $\beta \to \infty$ idealisation. But the comparison seems even more odd when we have a look at what Berry has to say about the $k \to \infty$ idealisation in the passage quoted by Batterman (Batterman 2001, pg. 88). Berry claims that "[the] shortwave (or, in quantum mechanics, semiclassical) approximation ... shows that wave functions $\psi$ are non-analytic in $k$ as $k \to \infty$, so that shortwave approximations cannot be expressed as a series of powers of $\frac{1}{k}$, i.e. *deviations from the shortwave limit cannot be obtained by perturbation theory*" (Berry 1981, pg. 519-520, emphasis mine). So neither the $\beta \to \infty$ nor $k \to \infty$ idealisations involve perturbations.

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rainbows as being diffraction caustics in §10.6, as well as the CAM theory of the ripple in §14.4.

This muddies the waters over what sort of mathematics Pincock thinks precludes viable interpretation. It initially appeared that singular perturbations where the mathematics appeared to be used in an instrumental sense (as in the boundary layer theory) precluded viable interpretation, and that the $k \to \infty$ idealisation was an idealisation of this type. According to Berry, however, the $k \to \infty$ idealisation is not of this type. So either Pincock's point is general, in that it is a warning that *any* mathematical link might preclude viable interpretation, in which case it is rather vacuous: of course that might happen, and warning that it might is of no help whatsoever. Or Pincock's point is supposed to be about certain mathematical links and that Pincock thought the $k \to \infty$ and $\beta \to \infty$ idealisations are examples of these links and he is mistaken. In either case, the $\beta \to \infty$ idealisation does not provide us with any insight into Pincock's metaphysical agnosticism as it is not a case of singular perturbation, and so metaphysical agnosticism is not a viable interpretative position to adopt in response to the idealisation. Whether this dilemma for Pincock's lesson is a result of the $k \to \infty$ and $\beta \to \infty$ idealisations will now be tested by turning to the CAM approach and looking at whether it can shed any light on Pincock's metaphysical agnosticism.

### 7.3.2 CAM Approach

The CAM approach promises to be a useful example. Pincock's interpretation of the CAM approach can be analysed to learn more about metaphysical agnosticism by asking the following type of questions. Does he adopt the metaphysical agnosticism position towards the CAM approach? What are the reasons for adopting the position he does towards the CAM approach? What do these reasons tell us about metaphysical agnosticism? It will also allow the distinction between Pincock's metaphysical agnosticism and Batterman's position to become clear (as this is the use Pincock puts it to himself, as well as contrasting his view with that of Belot (2005)). Further, the soundness of Pincock's claims of how the CAM approach has to be interpreted can be questioned by comparing his interpretation with the surplus structure approach adopted by the IC. Specifically, that the ray theoretic concepts are essential for the *use* of the model. In the previous chapter, I argued that the ray theoretic concepts were heavily involved in the construction of the model, but are not required for any serious use of the model (except perhaps for teaching it). Accepting these arguments would make Pincock's claims unsound with respect to using the model to, for example, explain the existence of the supernumerary bows. Given that Pincock employes the CAM approach within a discussion of explanation, such a result threatens the soundness of Pincock's account. This problem will be developed a little further, while a possible escape is provided by, again, the possibility that Pincock is following a different methodology, such that this is not the problem it seems to be.

**Pincock's Interpretation of the CAM Approach**

Pincock interprets the saddle points in a ray theoretic way (Pincock 2012, pg. 234). He justifies this interpretation by appealing to scale-like reasoning, that the saddle points describe a situation where ...

> the dominant contributions ... arise from electromagnetic waves which approach the behavior of rays of light. In particular, we have assumed throughout that we are operating in a context where the wavelength of light is much smaller than the radius of the drop, the only other relevant length parameter. So in terms of our size parameter $\beta >> 1$.

As the major contribution to the integral comes from the critical points, the saddle points are conjectured to be associated to rays. Pincock does emphasise the "experimental nature of this association", pointing out that the Mie representation does not include light rays "in its scope", i.e. it does not contain ray theoretic concepts or make reference to rays. Similarly, the association between rays and saddle points is not dependent upon the ray theory. Pincock puts this down to our rejection of the ray theory and that ray theory has nothing to say about saddle points. Pincock has a similar line towards the other critical points, the Regge-Debye poles (Pincock 2012, pg. 235). He again points to the association being conjectural, though rather than linking the poles immediately to surface rays or waves, he initially associates it with light that has "traveled along the surface of the drop for some distance". He argues that we are aware that the ray theory cannot be capturing all of the light that contributes to the rainbow, in particular the way in which light is diffracted by spheres, and so we conjecture that the poles are associated with surface waves. It is unclear whether Pincock intends for the poles to be associated *only* with surface waves, or whether he thinks that they should be associated with surface rays, rays that hit the drop at the critical angle and travel along the surface of the drop.[17]

While these associations might be conjectural and yet provide accurate predictions, Pincock offers other reasons for why the CAM approach can be considered a good explanation of the rainbow, though I will only discuss the first of these.[18] He argues that each step in the derivation is susceptible to a physical interpretation (Pincock 2012, pg. 236). This a very different position to the one that the IC adopts, where the CAM approach is considered surplus structure, and that (roughly) most steps in the derivation can be considered to be part of the derivation step. The IC might be capable of adopting a view similar to this if 'intermediary' moves back to empirical set ups are viable. I do not think that such moves are viable however.[19]

[17]See Grandy Jr (2005), pg. 175, and Figure 5.8 on pg. 176. These rays are described in terms of surface waves here, though given Pincock does not go into details, there is an ambiguity as to what he is referring to in the passage on pg. 235.

[18]The second concerns understanding the CAM approach as an abstract varying representation.

[19]For example, if we consider the CAM approach to be surplus structure we might have

Pincock asks what the explanations based on the $\beta \to \infty$ idealisation and CAM approach mandate for our beliefs about the rainbow. He appeals to inference to the best explanation pointed towards the mathematical links involved in the explanation, rather than it's typical use of deducing the existence of entities (Pincock 2012, pg. 237). With respect to the CAM approach and the explanation of the supernumerary bows, he argues that (Pincock 2012, pg. 238):

> it is not possible to explain the existence and spacing of the supernumeraries using just concepts deployed in the ray theory. This is because interference and diffraction are central to what is being explained. At the same time, it is not possible to explain this phenomenon by appealing only to what we find in the wave theory. Important links were made between the critical points and aspects of the rainbow. These links were given in terms of rays and surface waves. They involved aspects of the light scattering that are not available from the perspective of the wave theory alone. Instead, a scientist must ascend from the wave theory to the ray representation before she is able to get the 'physical insight' into the supernumeraries that CAM provides. This does not mean that she must believe that the ray theory is correct. Instead, she must use the results of one idealization to inform the proper interpretation of another.
>
> The techniques deployed in the explanation of [the rainbow angle] and

the following arrangement: the Mie solution is the empirical set up; the Debye expansion is Model 1; the shift to the complex plane via the Poisson sum formula gives us Model 2; and the result of the CAM approach is the resultant structure of Model 2. This would have to be mapped via an interpretation mapping to Model 1, and the appropriate structure of Model 1 to an empirical set up to obtain an interpretation.

To obtain a physical interpretation of each step, we might start with the Mie solution as our empirical set up and consider the Debye expansion to be Model 1. The manipulation of the expansion to a form where it can be transformed by the Poisson sum formula would then be the resultant structure, which we map to an empirical set up. We would then have to start again with this empirical set up to obtain the CAM approach as a new Model 1.

There is a major problem with adopting a physical interpretation of each step in the above way, namely that it would effectively result in the rejection of the first type of surplus structure. The statements made throughout the literature on partial structures literature and the SV concerning this type of surplus structure indicate that it is not a type that one should give up. Therefore a version of the IC which includes interpretations for each step of a derivation should be rejected.

> [the colour distribution] and their proper interpretation let us explain [the supernumerary bows].

As I argued in §6.3.2, however, Pincock is mistaken about the impossibility of explaining the rainbow through the use of wave theoretic concepts alone. Both types of critical points can be associated with wave behaviour: the saddle points with points of stationary phase and the Regge-Debye poles with surface waves. The problem for the IC came from the apparent requirement of ray theoretic concepts in the construction of the model. Pincock's exposition of the rainbow and the relevance of the ray theoretic concepts to this, is within this model creation context. He is concerned with relating each stage of the derivation to a physical interpretation, and discusses the motivations behind each move, such as the adoption of the Debye expansion (Pincock 2012, pg. 231, 236). One might judge Pincock's account to give a better explanation of the way in which we construct models and how mathematics represents in such cases, i.e. it has an interpretation during model construction which the IC does not allow. This might be the case, but would require a comparison between Pincock's account and a further developed partial structures version of the SV, with the IC as a third axis. In terms of model use, I maintain that Pincock has misunderstood the physics.

I have outlined how Pincock interprets the various aspects of the CAM approach above. Now I will go on to explore how this relates to his metaphysical agnosticism and whether it can bring any clarity to this position.

### The Rainbow and Metaphysical Agnosticism

In order to gain a proper understanding of metaphysical agnosticism, three issues need to be settled:

(a) Does the CAM approach involve singular perturbation?

(b) Given the answer to (a), what does the CAM approach tell us about metaphysical agnosticism?

(c) How different from Batterman's position is metaphysical agnosticism?

Issue (a) is obviously the most important. Metaphysical agnosticism is a possible response to representations that involve singular perturbations, where it is unclear whether we should adopt an emergentist or reductionist line (presuming that we are rejecting an instrumentalist line).

Singular perturbations involve domains being split either physically, as in the boundary layer case, or temporally, as in the Bénard cells and damped harmonic motion cases. The temporal split can be interpreted as being due to the presence of two processes, one working at a short time scale and one at a long time scale. Such a split would be inappropriate for the CAM approach; there are no time variables in any of the relevant equations (e.g. the Mie solution or the third Debye expansion). Further, both Pincock and Batterman talk about the rainbow in terms of it providing explanations of the stability of the rainbow, a use of the representation that would be impossible unless the representation was timeless. The spatial split is more promising, given that the use of the ray theory explanation of the rainbow angle and colour distribution is accepted by Pincock (when arrived at due to the $\beta \to \infty$ idealisation) when the "wave theoretic aspects of light are not relevant to the path of the light through the drop" (Pincock 2012, pg. 227). A spatial split of the domain is not required, however. In the boundary layer theory case, we required singular perturbation theory as assuming the initial set of scales for the Euler equation resulted in an empirically false result, namely that the object would experience no drag. None of the problems with the various rainbow representations were of this kind. The ray theoretic representation was rejected for not being able to provide an explanation of the supernumerary bows because it lacked the necessary wave theoretic features. The Mie solution converged too slowly, which was remedied by the adoption of the CAM approach. There is no motivation here for a spatial split of the domain. There is clearly no use of singular perturbation in the CAM approach.

Despite the negative answer to (a), the CAM approach is still a strong candidate for being interpreted in terms of metaphysical agnosticism. This can be demonstrated by showing that what Pincock says about instrumentalism and the metaphysical positions with respect to the CAM approach leaves logical room for adopting metaphysical agnosticism. Pincock explicitly rejects an instrumentalist understanding of the mathematics involved in the CAM approach (or at least explicitly rejects an instrumentalist interpretation of similar asymptotical moves and therefore by implication those of the CAM approach). He agrees with Batterman's claim that (Batterman 1997, pg. 396):

> these 'methods' of approximation [the asymptotic analysis of the Airy function] are in effect more than just an instrument for solving an equation. Rather, they, themselves, make explicit relevant structural features of the physical situation which are so deeply encoded in the exact equation as to be hidden from view.

That is, he agrees that the mathematics "can contribute explanatory power" (Pincock 2012, pg. 239).

Given the agreement between Batterman and Pincock in the explanatory role these features can play, one can ask whether he also agrees with Batterman's position towards the relevant structures of the CAM approach (the Regge-Debye poles and the saddle points). Batterman can often be regarded as arguing for emergence, as in his (2001). With respect to these emergent properties and explanations, Batterman makes claims such as the following. When discussing the caustic-theoretic representation of the rainbow he claims (Batterman 2001, pg. 96):

> It seems reasonable to consider these asymptotically emergent struc-

> tures to constitute the ontology of an explanatory 'theory', the characterization of which depends essentially on asymptotic analysis and the interpretation of the results.

When discussing emergent properties more generally he claims (Batterman 2001, pg. 127):

> *The explanatory role of the emergents*: Emergent properties figure in novel explanatory stories. These stories involve novel asymptotic theories irreducible to the fundamental theories of the phenomena.

Other than quoting Batterman's position on emergent properties, Pincock makes no mention of understanding the rainbow in reductionist or emergentist terms. He does, however, signal a rejection of a reductionist approach by reminding the reader of the scaling techniques and how they prevent the adoption of a reductionist position. This signal, with his attempt at clarifying the idea behind Batterman's position, indicates to me that there is room to reject a metaphysical stance towards the rainbow, and adopt the metaphysical agnostic position (that is not to say that Pincock does this).

What, then, can the CAM approach tell us about metaphysical agnosticism? The structures we are investigating here are the Regge-Debye poles and the saddle points (the critical points) and more generally the ray theoretic structures. We need to get clear on what position Pincock adopts towards these structures, and how similar these positions are to the metaphysical agnosticism position. Lets start with the rays. When Pincock discusses the rays in the context of the CAM approach and the explanation of the supernumerary bows, he again uses talk of a scale like nature: "all we have come to accept as a result of our explanations is that in certain contexts there are aspects of light that are accurately captured by the ray representation" (Pincock 2012, pg. 241). He argues that the explanatory power of the ray representation comes from how it is grounded in the wave theory, i.e. the more fundamental theory.

In order to understand this point, we need to remember the distinction Pincock draws between theories, representations and models (Pincock 2012, pg. 25-26):

> **Theory** A theory for some domain is a "collection of claims" that aim "to describe the basic constituents of the domain and how they interact".
>
> **Model** Any entity that is used to represent a target system [i.e. a representational vehicle]. We "use our knowledge of the model to draw conclusions about the target system". They may be concrete entities, or mathematical structures.
>
> **Representation** A model with content.

Pincock is attempting to argue that we are only ontologically committed to the wave theoretic concepts we use, as this is the only theory we are entertaining. We can entertain and make use of various representations without being committed to what they would posit as the constituents of the domain if they were taken to be theories. This distinction also allows us to take representations to be explanatorily useful. In this sense, we can use rays to explain the rainbow, by relating rays to saddle points and Regge-Debye poles, without being ontologically committed to the rays. We can draw a parallel here to the way in which the Bénard cells are explained using multiple scales. i.e. The Bénard cells are considered to be physical features of the system, but we are not committed to them in any kind of metaphysical way (regarding them as emergent structures or that they can be reduced). In this situation, the Bénard cells are analogous to the critical points, and the multiple scales (representing two different processes) are analogous to the ray theoretic and wave theoretic contributions to the CAM approach.

One last position needs to be ruled out before we can claim that we can adopt a metaphysical agnosticism position towards the CAM approach, namely the "wait and see" kind of agnosticism (i.e. McGivern's position). Pincock argued against a "wait and see" type of agnosticism in the case of the Bénard cells by rejecting McGivern's position. I also argued that one could not adopt a "wait and see" position while maintaining that one was truly rejecting the reductionist and emergentist positions. Another part of Pincock's rejection of the metaphysical positions was that our knowledge of the Bénard cells and the system through them "may be a product of our ignorance and in the end may not correspond to what a fuller understanding of the system would reveal" (Pincock 2012, pg. 119). This is, in part, due to "our limited access to the details of the fluid dynamics". This suggests that we can avoid the metaphysical agnostic position (and possibly embrace one of the metaphysical positions) if we have a proper understanding of the fundamental theory. This is the case with the fundamental theory for the CAM approach. Pincock's argument is not that we have to use ray theoretic concepts to relate the critical points to the world due to an ignorance of the wave theory, but that the ray theoretic concepts are required for relating the critical points to the world. That is, the only way of connecting the critical points is through ray theoretic concepts. This suggests that Pincock does not hold there to be any metaphysical difficulties with understanding the rainbow due to ignorance. If there is any difficulty, it is for other reasons. This further suggests that Pincock does not adopt metaphysical agnosticism towards the rainbow. The conclusion of the above argument is that Pincock will adopt some kind of emergentist or reductionist position towards the critical points. Which position Pincock adopts will be established in the next section, §7.3.2 - *Pincock, Batterman & Belot.* Before I move on to this discussion, a complication with the above argument needs to be dealt with, which concerns the analogy between the CAM approach and the Bénard cells.

The idea that our knowledge of the fundamental theory should dictate which metaphysical position we adopt leads to a disanalogy between the CAM approach and the Bénard cells. We have a fundamental theory in each case: the wave theory for the CAM approach, and the Navier-Stokes theory for fluids. This raises the question as to why there appears to be no metaphysical interpretive issues in the case of the CAM approach due to ignorance, but there are for the Bénard cells. The difference seems to be that we have an explanation for how the critical points, ray theoretic concepts and wave theoretic concepts are related in the case of the CAM approach through the use of the theory, representation and model distinction. In the case of the Bénard cells we do not appear to have a similar explanation available. The explanation in terms of the distinction allows us bridge the explanatory gap between the faithfulness and usefulness in the CAM case. Our ability to judge the (partial) faithfulness of the CAM representation seems to come from the relationship between the ray representation and the wave theory, i.e. the taking of the $\beta \to \infty$ limit.[20] The usefulness of the representation is down to our ability to 'identify hidden structure' through the representation, i.e. structure that was already present in the wave representation but was otherwise inaccessible (Pincock 2012, pg. 221).

An alternative approach to this issue is to ask whether we can consider the Bénard cells to be 'hidden structure' in the way the critical points are. There is room for this position. Batterman uses the term 'hidden structure' to describe the relevant structures of the rainbow he holds to be emergent. If this position can be maintained for any structures that might be classed as emergent, then it is a suitable description for the Bénard cells. If we can class the cells as structures of this kind, then we can ask the question why we cannot employ

[20]There is a problem if this is indeed Pincock's position, as we do not in fact use the this limit when using the CAM approach; rather we only need $\beta$ to be greater than 50. Pincock needs to clarify how essential the actual taking of the limit is to the relationship between the ray representation and concepts to the critical points, or whether it is only the possibility of the limit is which is important.

the theory, representation and model distinction in the Bénard cells case, i.e. why we could not consider the Bénard cells to be a representation with no ontological commitment in the way the critical points are. A possible reason why the distinction is not relevant is the move to schematic content required by the use of the singular perturbations. The CAM approach involved shifts to schematic content when moving into the complex plane, however, so the mere presence of schematic content cannot be the reason for the difference. The only viable option seems to be the the supposed ignorance of the fundamental theory, but it is not clear how we are ignorant of the Navier-Stokes theory.

Pincock's metaphysical agnosticism is starting to look unjustified. The above comparison of the rainbow and the Bénard cells suggests that there is little difference between the two cases, at least insufficient differences to justify the adoption of a 'third table' between the instrumentalist table and the metaphysical table (reductionism or emergentism). All that remains is to establish whether there is any difference between the position Pincock does hold towards the rainbow and Batterman's, and whether this has any influence on how we should evaluate metaphysical agnosticism.

### Pincock, Batterman & Belot

As identified above, Pincock agrees with Batterman's claims that the CAM approach reveals hidden structure and that such structures can be explanatory (Pincock 2012, pg. 221). The first difference between their positions comes from Pincock's rejection of Batterman's claim that we should adopt the emergent structures as a new 'theory' (Batterman 2001, pg. 96):

> It seems reasonable to consider these asymptotically emergent structures to constitute the ontology of an explanatory 'theory', the characterization of which depends essentially on asymptotic analysis and the interpretation of the results.

To which Pincock responds that "both sympathizers and critics have not agreed on what Batterman takes the interpretative significance of this 'theory' to be" and subsequently puts forward his distinction between theory, representation and model (Pincock 2012, pg. 222). It this is novel distinction and attitude towards representations derived from theories that sets Pincock apart from Batterman and Belot. Pincock blames the "traditional assumption" that using a representation derived from a theory requires one to accept the ontology of that theory to be behind Batterman's stronger, emergentist position and Belot's "deflationary" view.

For instance, Batterman is quoted by Pincock as endorsing a ray theoretic ontology due to ray theoretic boundary conditions (Batterman 2005b, pg. 159):

> those initial and boundary conditions are not devoid of physical content. They are 'theory laden'. And, the theory required to characterize them as appropriate for the rainbow problem in the first place is the theory of geometrical optics. The so-called 'pure' mathematical theory of partial differential equations is not only motivated by physical interpretation, but even more, one cannot begin to suggest the appropriate boundary conditions in a given problem without appeal to a physical interpretation. In this case, and in others, such suggestions come from an idealized limiting older (or emeritus) theory.

Pincock's objection to this is that in using the ray representation (distinguished from the ray theory as he does), we have not been required to adopt any believes about light that the ray theory incorrectly offers, whereas Batterman is endorsing the adoption of such beliefs.

Pincock understands Belot's position to be a "deflationary" one. Pincock quotes Belot's discussion of the relationship between the wave (the more fundamental) theory and the ray (the less fundamental) theory (Belot 2005, pg. 151):

> The mathematics of the less fundamental theory is definable in terms of that of the more fundamental theory; so the requisite mathematical results can be proved by someone whose repertoire of interpreted physical theories includes only the latter; and it is far from obvious that the physical interpretation of such results requires that the mathematics of the less fundamental theory be given a physical interpretation.

Pincock argues that a correct interpretation of Belot would be to claim that the success of the CAM approach and catastrophe theory does not entail commitment to the ray theory's ontology. Pincock also argues, however, that this "deflationary" reading does not do justice to the explanatory power of the ray theoretic concepts, in particular the fact that the significance of the critical points could only be accessed through the use of ray theoretic concepts. Pincock also points to Redhead's (2004) response to Batterman here as being deflationary. Redhead agrees with Batterman over the new explanatory power that can be obtained through asymptotic reasoning, but asks why we cannot accept these structures as being cases of surplus structure, rather than reifying them and claiming them to be emergent properties or structures (Redhead 2004, pg. 529-530). This indicates that Pincock understands surplus structure to be an instrumental use of mathematics, a position I rejected in §7.2.2 - *Metaphysical Agnosticism* above.

The rejection of the traditional assumption in favour of Pincock's distinction between theories and representations does not result in a large difference between his and Batterman's views, at least according to Pincock himself. He believes his …

> conclusion is completely in the spirit of Batterman's many remarks on the interpretive implications of asymptotic reasoning. So I do not intend my discussion here as a criticism of his views. At the most, what I argue is that Batterman's views are not as clear as they should be, and this has

hampered the appreciation of the significance of cases like the rainbow.

In support of Pincock's claim that his position is in the spirit of Batterman's, Pincock points to a quotation which he claims outline's Batterman's "considered view": "asymptotic explanation essentially involves reference to idealized structures such as rays and families of rays, but does not require that we take such structures to exist" (Batterman 2005a, pg. 162).

It is not my job to argue for whether Batterman's strong position or what Pincock considers to be his considered view is correct. I am concerned with attempting to understanding Pincock's metaphysical agnosticism, what his attitude towards the rainbow is, and whether these positions are distinct from Batterman's position. The above discussion makes it clear that what does all of the work for Pincock is his distinction between theories, representations and models. This distinction allows Pincock's attitude towards the rainbow to be clearly distinguished from Batterman's strong position: there is no endorsement of any emergent properties, nor any ray ontology, whereas Batterman at times endorses both of these positions. I take a consequence of Pincock's distinction to be a rejection of emergentism, and an endorsement of reduction. He has effectively argued that, although we need to represent the rainbow with ray representations, we are not committed to any ray theoretic or emergentist (as with Batterman) ontologies, which constitutes a rejection of emergentism and endorsement of reductionism.

The above discussion and model, representation and theory distinction do little for helping to clear up the mystery around metaphysical agnosticism. The question can still be raised why we cannot take advantage of the theory, representation and model distinction towards the Bénard cells. What use is adopting the metaphysical agnosticism in this situation? Why could we not describe the situation as requiring a higher level representation, which does not commit us to any higher level ontology than that of the fundamental theory, the Navier-Stokes theory? Although Pincock does talk of the discovery of novel metaphysical features, I do not see the difference between this and the supernumerary bows. I do not think that Pincock can produce an answer to these questions, and therefore reject his metaphysical agnosticism as being justified.

At best, metaphysical agnosticism is too underdeveloped a position to be able to hold until more work is done; or at worst, it is untenable, collapsing into a rough form of reductionism.

## 7.4 Conclusion

In this chapter I have argued that different structural resources are required on the PMA for Galilean idealisation and for singular perturbation idealisation. For Galilean idealisation, the PMA can employ the notions of the structural relation, which include mathematics, the specification relation and schematic content. Singular perturbation idealisations are held to have wholly schematic content at a time, with some representations obtaining an interpretation in terms of new physical concepts, which requires a new specification relation and limited structural relation. The issue of how faithfulness and usefulness are related for each idealisation was not definitively settled in either case. For the Galilean idealisations, the faithfulness was accounted for through the structural similarity between target and vehicle, demonstrated through the structural relation. These idealisations were held to be useful due to the specification relation, providing interpretations and altering the structural relation to the specific requirements of the representation at hand. I urged some caution, here, as these idealisations could be described as being useful 'because we design them to be'.

Establishing how faithfulness and usefulness are related for singular perturbation idealisations turned out to be far more difficult. For the singular limits that resulted in instrumental type representations, such as the boundary layer theory, the relationship appeared to be broken. While there were mathematical links between the target and the vehicle, these were not strictly part of the representational content for the PMA, and the mathematics that was part of the content was schematic, i.e. had no interpretation. This schematic content threatened the possibility of calling the mathematics an epistemic representation. The introduction of the genuine content provided a possible solution here, but this move does not provide much help in establishing the link between the faithfulness and usefulness of the representation with genuine content and the target. Pincock advocated adopting the position of 'metaphysical agnosticism' towards some singular perturbation idealisations, such as the Bénard cells and damped harmonic oscillators. I argued that this could be understood as a 'third table' to the instrumentalist and metaphysical (reductionism or emergentism) tables. Pincock's original exposition of this position was unclear. I attempted to gain a clearer understanding of this position through the $\beta \to \infty$ idealisation and the CAM approach. This investigation resulted in a rejection of the metaphysical agnosticism as a viable position, with the argument that Pincock should adopt the reductionist position that results from his theory, representation and model distinction.

I posited that a reason for the difficulties in relating the faithfulness and usefulness for both types of idealisation might be the difference in methodology Pincock has in comparison to Contessa and the proponents of the IC. I will survey this difference in methodology in the next chapter. The next chapter also involves subjecting the IC and the PMA to the questions set out in Chapter 2, with the aim of choosing between the two accounts.

# 8 | Choosing An Account

## 8.1 Introduction

In Chapter 4 I argued in favour of understanding the IC and the PMA as accounts of faithful epistemic representation. At the end of the chapter, one question remained unanswered: question (2.b), which asked "how does it [the putative account of faithful epistemic representation] account for the relationship between faithfulness and usefulness?" The aim of the last two chapters was to attempt to answer this question according to each account. I approached the question by recasting it as the following two questions:

1. What is the relationship between faithfulness and usefulness?

2. What structural resources are used to account for idealisations, and are they the same across different types of idealisations?

The rainbow was chosen as a case study, to test the answers to the above two questions. The $\beta \to \infty$ idealisation and the CAM approach were leveraged to this end. In this section I will summarise how both accounts were found to answer the above two questions, and the success or otherwise, of those answers after being subjected to the $\beta \to \infty$ idealisation and CAM approach. I will be attempting to draw out more general points than were made in the previous chapters, so that the lessons learnt there can be brought into the broader discussion over what a successful account of mathematical scientific representation should look like. In the next section, §8.2, I will reintroduce the problems of the applicability of mathematics, and attempt to answer them on the IC and the PMA. The questions can be mostly answered, apart from the problem of misrepresentation due idealisation and abstraction on the PMA. The solution to this problem was supposed to come out of the discussion in the previous two chapters, as it did for the IC. In §7.2.2 - *Metaphysical Agnosticism* and §7.4 I suggested that the cause of this issue was the methodology Pincock adopts to construct his account. I will investigate the methodologies and argue against Pincock's in §8.3.

### 8.1.1 The Inferential Conception

The IC provided a good account of the relationship between the faithfulness of a representation and its usefulness through the notion of partial truth and the partial structures framework. Idealisations can be understood as 'as if' descriptions: descriptions that are treated 'as if' they are true in the appropriate theoretical contexts. This notion of being treated 'as if' they were true can be handled by partial truth, while a measure of how partially faithful a representation is can be gained from the notion of how partial a partial structure is (i.e. the 'size' of $R_i$ of the vehicle compared to the 'size' of the $R_i$ of the target). This is an approach that works very well for Galilean idealisations, while a threat was thought to come from singular limit idealisations. The issue originated in attempting to understand such idealisations as surplus structure, a position proponents of the IC hold (Bueno & French 2012, pg. 91).

Much useful work was done in investigating the notion of surplus structure, such as the splitting of the notion into four roles and assigning the individual roles to particular parts of the SV and IC. A problem was found with understanding singular limits as idealisations. As idealisations on the IC are to be understood as 'as if' descriptions, one could only have an idealisation once one has a theoretical context, something unobtainable during the derivation step.

This meant that singular limits should not be understood as idealisations until the end of the interpretation step.[1]

Such limits were thought to introduce surplus structure during the immersion stage. I argued that the limits would be surplus structure of the first type, that which would never have a physical interpretation. A consequence of adopting partial structures in the derivation in order to accommodate surplus structure of this type causes one to face the following dilemma:

> **Very Large $R_3$ Problem** the partial structure in the derivation step must contain all mathematical structures in the $R_3$ component (in case any mathematical structure is required to be adopted as surplus structure in the future).
>
> **Mathematical Restriction Problem** it is unclear what can motivate a restriction in what mathematical moves we hold as possible moves, and hence limit what is placed in $R_3$ (as the derivation step takes place within the (uninterpreted) mathematical domain, no physical considerations can be appealed to).

Possible solutions to both horns of the dilemma were investigated, but all require work. I'll cover the most promising response to each horn here.

Solutions offered up to the Mathematical Restrictions Problem were split into two types: those that are 'internal' to the mathematics, i.e. rely solely on features of the mathematics at hand, and those that are 'external' to the mathematics. The most promising internal solution was to appeal to a notion of there being natural extension of the mathematics included in the first model, and that only the mathematics that constitutes this natural extension be included in the $R_3$. Support for this approach came from the analysis of the CAM approach: I argued that one only needed the typical mathematical resources that come with transforming a real function into the complex plane

[1] See page 162 for the discussion of this point.

and the Euler identity, and that if anything was a candidate for a natural extension, it is the move into the complex plane for real functions. I also argued that the Euler identity was a similar example of a natural extension. However, more work is needed to generalise this solution; it is unclear whether a general notion of 'natural extension' can be explicated that is not too restrictive in some cases and too permissive in others (though even these notions require specification).

The solution to the Very Large $R_3$ Problem was to bite the bullet, to admit that one is committed to all other mathematical structures being 'in' the $R_3$. The most promising approach to this was to argue that the partial structures are representational devices and so there is only the appearance of a problem (as being representational devices, there is no problematic commitment in play). One reason for this approach being the most promising will be developed below: recognising that the IC is operating at the 'meta-level' allows one to make use of the partial structure framework in a way that does not require reifying the partial structures. Working at this meta-level allows one to avoid taking the representation relation, partial structures in the derivation step, and so on as *literally* set theoretic relations (i.e. as partial morphisms).

A solution to the dilemma is required for the IC to be held as a viable account of representation. For the sake of argument, I will assume that a solution can be found. There are still problems for the IC, however, brought out by the $\beta \to \infty$ idealisation. This idealisation demonstrated that not all singular limits introduce surplus structure. Recognising this required the IC to be adjusted as to when one could say surplus structure was introduced via a singular limit. This is not a destructive result for the IC, but the alterations prompted by the $\beta$ idealisation caused slight worries.[2]

While the CAM approach was found to be a great example of surplus

[2]Specifically, the idea that that we could take a singular limit from an empirical set up seemed to be 'too quick'. I attempted to assuage these worries. See §6.3.1 for details.

structure, it also introduced a further problem. Proponents of the partial structures framework often talk about its ability to handle both model creation and use. The CAM approach, and Pincock's analysis of it, threaten this unified approach, however, for the specific way in which partial structures are used in the IC. It appeared that there were good reasons for doubting that the IC would be able to accommodate the application of mathematics in model creation. A rough sketch of a solution was offered, the key idea being that the IC is a third axis in the SV, where there is a 'horizontal' axis for inter-theory relations (such as model creation) and a 'vertical' axis between abstract models and data models (French 2000, pg. 106).

### 8.1.2 Pincock's Mapping Account

For the PMA, it was found that different structural resources were required for different types of idealisations. This resulted in a different relationship between faithfulness and usefulness for different types of idealisations. For simple and Galilean idealisations, the PMA provided a good account of both the content of the representations and of the relationship between the faithfulness and the usefulness of representations. The notion of enriched content is used to account for simple idealisations such as the use of real numbers to represent magnitudes, or regions of space involved in the representation of temperature. Galilean idealisations are handled by adopting the notion of schematic content, where part of the mathematics is decoupled from its interpretation. These are then explained to be partially faithful due to the structural similarity between the enriched and genuine contents, related by the structural relation. The usefulness of these idealisations is accounted for by the specification relation, which provides the interpretation of the mathematics. One can be reductive and claim that these idealisations are useful because we design them to be.

A problem arose, however, when I attempted to understand the relationship between faithfulness and usefulness for representations that involved singular limits, which were cast as involving singular perturbations by Pincock. In general, these idealisations are handled by all content being schematic content, which is then, in part, given a new interpretation, i.e. take part in a new specification relation. The first example of the boundary layer theory did not pose a problem by itself, though it was challenging to understand how it could produce successful predictions on Pincock's account. Pincock adopts an instrumentalist line towards the representation, which would indicate that the content remained entirely schematic. Thus, the boundary layer theory demonstrated the unusual position Pincock has adopted towards content and prediction, with predictions not being part of the content of a representation. The motivation for adopting this instrumentalist position was that the singular perturbation involved a physical split of the domain, which is physically unmotivated.

The Bénard cells and damped harmonic motion examples motivated Pincock's adoption of metaphysical agnosticism. He rejected the instrumentalist and metaphysical (i.e. reductionism and emergenticism) positions as interpretations of the singular perturbations. I was unconvinced by Pincock's arguments against the instrumentalist position and his arguments in favour of rejecting the metaphysical options. I concluded this first analysis of metaphysical agnosticism with the claim that there is explanatory gap between the partial faithfulness and usefulness of these representations left by the refusal to provide any sort of metaphysical, or at least some explicit structural, link between the target system and the representation. A result of this explanatory gap is that circularity threatens: the representations are useful because they provide accurate predictions, and because they provide accurate predictions, we take them to be partially faithful, and so can take them to be useful.

In an attempt to get clearer on what metaphysical agnosticism is, I turned to the rainbow, and the $\beta \to \infty$ idealisation and the CAM approach again. Unfortunately my analysis of the $\beta \to \infty$ idealisation provided no clarification, and only served to damage the PMA: Pincock's warning that mathematical links might preclude interpretations is either vacuous (because of being too general) or is as a result of misunderstanding the physics. Discussion of the CAM approach could again be split over the issues of model creation and use. However, little could be said that was of benefit to understanding metaphysical agnosticism even when this distinction is made. The PMA might well be a more plausible account of how model creation occurs than one might able to find on the SV and IC, but I disagree with Pincock over the physics in the use case. The CAM approach was useful for gaining a better understanding of metaphysical agnosticism: Pincock makes use of it to contrast his view to those of Batterman and Belot. My analysis of this discussion lead me to conclude that metaphysical agnosticism either requires further work to be a viable position, or that it collapses into a type of reductionist position.

The rejection of metaphysical agnosticism left a hole in Pincock's account: the relationship between faithfulness and usefulness was still unexplained. At the end of §7.2.2 - *Metaphysical Agnosticism* an attempt of how to save Pincock's account was sketched. Pincock is not providing a unified framework, but rather a set of tools to be adopted and adapted towards each case study. Thus, the position one holds will depend on the example at hand, in a more relativised way than one would in following the IC. I proposed that this was due to a methodological difference between the IC and the PMA. This is an idea that will be pursued in the rest of this chapter. The viability of Pincock's account will depend on whether the problems outlined in Chapter 7 can be solved given the results of the investigation into the methodology he has adopted.

Before the methodological discussion, I will show how each account in turn can answer the questions set out in Chapter 2.

## 8.2 Are the Accounts of Representation Successful?

The last few chapters were concerned with identifying the internal problems and issues with the structural accounts of representation. There are questions these accounts have to be able to answer that are external, namely the questions and problems raised in Chapter 2. We are now in a position to see whether the Inferential Conception and Pincock's Mapping Account are capable of answering these questions. The applied metaphysical question, the problem of relating the mathematical domain to the world, was solved in §2.4 where I argued in favour of structural relations. However, as emphasised by Pincock's analysis of these questions, the applied metaphysical problem set a very low bar for its answers. I argued further that specifying exactly what what was required of the relation fell under the problems of representation. A structural relation was chosen in part because it could serve as (part of) a representation relation, hence the endorsement of structural relation accounts of representation in Chapter 3.

Two problems are straightforward to answer on the IC: the multiple interpretations problem and the isolated mathematics problem. I think that the IC can also offer a straightforward answer to the novel predictions problem, though some argumentation is required to establish that the answer is naturalistically acceptable. Similarly, the PMA is capable of providing straightforward answers to the multiple interpretations and the isolated mathematics problems. I also think that the PMA can be extended to answer the novel predictions problem (Pincock (2012) does not discuss any problems related to novel predictions). Whether the answer that the PMA supplies can be considered a naturalistically acceptable one also requires discussion. This discussion over (naturalistic) methodology is not to be confused with the discussion over the problems of representation. In the novel prediction case, the methodology at stake is that of naturalism, specifically the 'strict' naturalism Bangu (2008) identifies as being incompatible with his Reification Principle. In the case of the problems of representation, in particular the relationship between faithfulness and success, the methodology under discussion is that of the philosophy of science: what role should philosophy of science, examples, and so on, be playing in our analysis of science. I will survey the answers to the problems of misrepresentation I have already given (in §3.3.1 - *The Argument from Misrepresentation* and §3.5), before discussing the methodological approaches of Pincock and the advocates of the IC, as the answers to the final questions (misrepresentation due to idealisation and abstractions, and what the relationship between the faithfulness and usefulness of a representation is) depend on the outcome of the methodological debate.

### 8.2.1 The Isolated Mathematics Problem

The $SU(3)$ example shows that a detailed and complete history of the mathematics is required to establish whether the mathematics can be truly held to be isolated. If it is the case that the mathematics is isolated, then a (presumably) unique explanation of why it is applicable must be found. This is not the case. Both the IC and PMA can straightforwardly answer this problem in a general way.

The IC and PMA attribute the applicability of mathematics to the structural relations between the physical and mathematical domains. All that is required of mathematics is that it provides a structure that is sufficiently similar to the physical structure it is applied to (depending on issues related to representation). The origins of the mathematics are irrelevant to this way of understanding applicability and representation. How a mathematician or a scientist comes to find or construct a piece of mathematics is an interesting historical question, the answer to which has no bearing on the structural resources provided by the mathematics. On this account of representation, the isolated mathematics problem is fact a non-problem: the 'isolation' of the mathematics is irrelevant, and so there is nothing relevant about the mathematics to cause a problem for these accounts.

### 8.2.2 Multiple Interpretations

The multiple interpretations problem was motivated by the very different ways in which the negative energy solutions of the Dirac equation were interpreted by Dirac. They were first rejected as being non-physical, then interpreted as 'holes' in a sea of negative energy electrons, and finally as positrons. The problem asked how these different physical interpretations could have any predictive success. The start of an answer can be found when the authors of the IC discuss how one might understand the prediction of the positron on the IC: (Bueno & Colyvan 2011, pg. 365):

> This example [the prediction of the positron] also illustrates how the inferential conception explains which parts of the mathematical models refer and which do not. First, the conception provides a framework to locate and conceptualize the issue: the work is ultimately done at the interpretation step. Some interpretations are empirically inadequate, and thus fail to provide an entirely successful account of the application process. Dirac's interpretation of the negative energy solutions as 'holes' clearly illustrates this point. However, despite being at best only partially successful, such empirically inadequate interpretations can be very helpful in paving the way for empirically successful interpretations. They offer some understanding of how the world could be if the interpre-

> tation were true, and they can lead the way to interpretations that are empirically supported. Again, Dirac's interpretation of the negative energy solutions in terms of the 'positron' beautifully illustrates this point. As a result, the inferential conception sheds light on the issue of how mathematical models with non-referring elements can be useful.

Remember that on the IC an interpretation is a partial mapping from the mathematical structure to a (partial) empirical set up structure. While each interpretation will require a different partial mapping to differently (partially) structured empirical set ups, the partial success of the different interpretations can be explained through there being partial mappings between the empirical set ups. That is, the structure of the empirical set up that gives the 'hole' interpretation might be partially isomorphic to the structure of the empirical set up that gives the positron interpretation.

The PMA can provide a similar story, though obviously cannot take advantage of the partial structures framework. Here the interpretation comes from the specification relation. Thus different interpretations of the same mathematics will require different specification relations. There is a slight complication, however, as Pincock might not hold all of the mathematics involved in the derivation of the negative energy solutions as being part of the representational content. Assume that the negative energy solutions themselves, as predictions, are not part of the content of the representation. One cannot then reinterpret the negative energy solutions by relating them to the target via different specification relations because they are not part of the representational content and thus not actually related by the specification relation. Rather, we have to relate the original content by a different specification relation (and therefore possibly a different structural relation) and then follow the steps through the extrinsic mathematics to get the same solutions. These solutions will now have a new interpretation due to following from the differently interpreted (due to the new specification relation) representational content. We can then talk about the similarities between the specification and structural relations as being responsible for the success of the different interpretations.

The solutions to the multiple interpretation problems on both accounts show that the problem arises not due to differences in the mathematics, but rather in properly relating that mathematics to the world. The solutions talk about similarities between the structures of the interpreted mathematics, the empirical set ups, or the target of the specification and structural relations, rather than about the mathematics itself. The idea that can be pushed here is that the mathematics is describing the correct structure, but that a physical understanding of the structure has not been obtained yet. Thus progress is slowed, because the correct *physical* consequences cannot be deduced. We might be able to predict certain behaviours or values via an incorrect interpretation, such as the negative energy solutions describing a positively charged particle's movement in an electric field correctly when interpreted in terms of 'holes'. But this is due to the mathematics representing the correct physical structure (in part), rather than the interpretation. The 'hole' interpretation was rejected due to physical considerations, such as attempting to understand it as a proton, which would require the mass of an electron and the proton to be the same. The underlying idea to the solutions, then, is that the mathematical structure is correctly describing the world, and thus a Formalist reading of the mathematics is being used. As such, the solutions to the multiple interpretations problem actually rest upon a solution to the novel predictions problem.

### 8.2.3 The Novel Predictions Problem

The novel predictions problem can be set out as follows:

(i) Can we provide an explanation as to why Formalist/Pythagorean prediction is successful (if one is available)?

(ii) Can we provide a consistent methodological understanding of how novel predictions are made? (Whether we can have a consistent methodological treatment of the positron and $\Omega^-$ predictions, of D-N type and Formalist/Pythagorean type predictions.)

(iii) If we can have such a consistent methodological treatment, can it be one that conforms with the strict naturalistic view? Or must it be Bangu's methodological opportunism, or similar?

(iv) If we cannot have such a consistent methodological treatment, must we accept Steiner's pluralism, or a similar position?

In Chapter 2 I surveyed Bangu's analysis of Steiner's discussion of prediction and his, Bangu's, reconstruction of the argument at the heart of that discussion. I highlighted that Bangu's naturalist holds there to be no explanation of why the Reification Principle (RP) succeeds or fails.[3] This being the case, the naturalist would answer negatively to (i). I think that the IC and the PMA can provide an explanation for why the RP succeeds in some instances and fails in others. The answer is actually one of the responses that Bangu argued fails, namely that what is doing the work is the interpretation of the mathematics.

The interpretation response as set out by Bangu begins by claiming that "the crucial reificatory step ... was taken by interpreting the mathematical formalism, [in that] it is not the formalism itself that predicts, as it were, but our interpretation of it: ... without a physical interpretation, no empirical predictions could ever be obtained from any formalism" (Bangu 2008, pg. 255-256). This much I agree with. We need to physically interpret the mathematics

[3]Bangu's (RP) is as follows: "If $\Gamma$ and $\Gamma'$ are elements of the mathematical formalism describing a physical context, and $\Gamma'$ is formally similar to $\Gamma$, then, if $\Gamma$ has a physical referent, $\Gamma'$ has a physical referent as well" (Bangu 2008, pg. 248).

in order to obtain any kind of physical information. The problem with this response, as Bangu sees it, is that there is a different notion of interpretation at work in the case of Formalist or Pythagorean prediction than in typical cases of mathematical representation. He contrasts the interpretation of the $F$ in $F = ma$ as force with the interpretation in the case of the prediction of the $\Omega^-$. He describes the $F$ interpretation as being synonymous with "specify[ing]", targeting which sort of force we are representing (elastic, electrical, etc.), whereas he describes the $\Omega^-$ interpretation as "essentially, ... reifying a piece of formalism". He argues that Gell-Mann's interpretation of the formalism "proceeded (consciously) along analogical-pythagorean lines and was brought to a specific conclusion by using the RP".

The root of the problem with the interpretation response, according to Bangu, is that interpretation, for the naturalist, cannot proceed by 'reifying mathematics', due to it resting on an application of the RP which is naturalistically unacceptable. Thus the solutions offered by the IC and the PMA need to do two things: explain how the RP can be naturalistically acceptable; and provide consistent ways of interpreting mathematics. Both accounts satisfy the second of these conditions: all interpretations on the IC are (partial) mappings from pure mathematical structures to the suitably interpreted structures of the empirical set up. As explained in §6.2.1, the empirical set ups are physical interpreted mathematical structures, those that compose the models used in the SV. Similarly, all interpretations on the PMA are due to the specification relations. Pincock's variable notions of representational content complicate matters slightly, in that at times there will be schematic content. But this complication only concerns which piece of mathematics is interpreted in a given representation; it does not effect what interpretation consists in on the PMA.

The IC and the PMA can give the same general answer for how the RP can be successful or unsuccessful by leveraging their answer to the applied metaphysical question and constitutive question for representation: the structural relations. The structural relations will relate a part of the physical structure of the target to a part of a large mathematical structure. The idea is that when the RP succeeds, the physical structure used as the target of the representation and the mathematical structure used as the vehicle are similar enough[4] that the derivations and mathematical manipulations we perform 'reveal more' of the mathematical structure that can then be interpreted to 'reveal more' of the physical structure. In cases where the RP fails, the two structures would be insufficiently similar. The idea here is that the mathematical moves 'reveal more' mathematical structure, but that these moves do not relate to the further unknown physical structure. What grounds this idea is that we can follow the structure more easily in the mathematics, i.e. we can use mathematics to draw surrogative inferences, but due to the large size of the mathematical structures, we can make 'false' moves, i.e. follow relations which are available in the mathematics but not present in the physical structure. We need a sufficiently similar mathematical structure as our representational vehicle in order to successfully apply the RP.

Where this response comes up short is that we cannot, ahead of time, know whether a particular interpretation will be successful. This is only a problem if one retains the idea that interpretation *qua* RP is different to the interpretation of $F = ma$, but there is no difference between these interpretations on the IC and PMA. Thus there is no special problem of explaining why 'Pythagorean' prediction fails, as it fails for the same reason and traditional prediction fails.

With this answer to (i), we can provide a positive answer to (ii) as well: both cases of prediction of the positron and the $\Omega^-$ can be consistently ac-

[4] In the IC case, sufficiently partially morphic; in the PMA case, whatever notion of structural similarity Pincock is happy to use.

counted for. On the IC, novel prediction is simply the mapping from a piece of mathematics to a new empirical set up, where that empirical set up is found to be empirically adequate and the empirical set up contains a novel particle, behaviour, etc., that constitutes the novel prediction. The difference between the prediction of the positron and the $\Omega^-$ consists in a difference in the response to the interpretations of the mathematical structures involved in the predictions. In the case of the prediction of the positron, we had a mathematical structure that was incorrectly, partially, interpreted which lead to anomalous behaviour and a reinterpretation of the mathematics. For the $\Omega^-$ prediction, the mathematical structure could be explored more quickly than the physical structure in combination with a very close similarity between the mathematical and physical structure. Thus the prediction of the $\Omega^-$ appears to be different because no verification of the results could be carried out until much later. However, the process was the same. The mathematics was not 'reified', but used to explore what the physical structure might be. It played the representational role of allowing for surrogative inferences to be drawn about the target, the world. When one makes 'Formalist' or 'Pythagorean' predictions and analogies, one is not reifying mathematics but exploring the structural relations that might exist in the physical structure. One is predicting what the *larger* physical structure will be like, given the *larger* mathematical structure. I think that this answer is compatible with a strict naturalistic approach, and so answer positively to (iii). As the IC is acceptable to the naturalist, its account of interpretation should also be acceptable.

On the PMA, we can give the same sort of response in general. We are using mathematics to obtain surrogative inferences, thus in exploring the mathematical structure we are exploring the possible physical structure that we related via the structural relation. Thus novel prediction will involve relating the mathematics to a physical structure that contains a novel prediction. Things are made more complicated, however, when we take into account the various notions of representational content Pincock employes, and that predictions are not part of the representational content. The first complication is that this actually seems to block novel prediction of entities: while predictions might not be part of the representational content, they get their interpretations from the specification relation and the enriched and genuine content involved in the representation. This implies that we cannot actually make predictions of novel entities, as they will not be part of the initial specification relation. Thus if we start a representation interpreting the mathematics of the Dirac equation in terms of 'holes', or where negative energy solutions are not physically meaningful, we cannot predict a positron as there is no mathematics without an interpretation that we can incorporate as 'new structure', where this 'new structure' becomes the positron when interpreted. The response is to employ schematic content. Something might encourage us to reject the interpretation that we started with, and adopt a schematic content for a piece of mathematics which we can then either perform further work on, or adopt a new interpretation of, i.e. adopt a different specification relation.

While this response appears to solve the problem, it raises a second complication. The situation is the same as occurs during the use of singular perturbations. As outlined in §7.2.2, when one makes use of a singular perturbation, one has to move to wholly schematic content before adopting a new interpretation of the mathematics. However, this is problematic as it is unclear how the faithfulness and usefulness of representations are related. The same worry exists here: what justifies the adoption of the new specification relation?

### 8.2.4 The Problem of Misrepresentation

In §3.5 I argued that some of the problems of misrepresentation were relevant and legitimate problems for accounts of scientific representation. This argument consisted in a rejection of a unified account of representation and the adoption of Contessa's accounts, accepting his notions of epistemic and faithful epistemic representation. An epistemic representation is held to be one that allows for valid surrogative inferences about the target to be drawn, a (partially) faithful epistemic representation is one that results in (some) sound surrogative inferences to be drawn. Thus misrepresentation can be understood as the drawing of false surrogative inferences about the target. Suárez (and Frigg) argued against structural relation accounts of representation by claiming that they could not account for cases of misrepresentation. In Suárez' argument, he distinguished two kinds of misrepresentation, each of which involves two further types of misrepresentation: mistargeting; and inaccuracy. Mistargeting arises when either a representation's target does not exist (such as in the case of the mechanical ether or phlogiston), or where one misidentifies the representation's target (such as in Suárez' example of the Pope Innocent X painting and one's friend). I dismissed inaccuracy due to empirical inadequate results as being a general problem for philosophy of science, and claimed that it did not need to be addressed specifically by an account of representation. I also argued that mistargeting due to mistaken targets is caused by agents, rather than the structural relation, and so was not a problem to be dealt with by accounts of representation, claiming that recognising the fault lies in the agents' intentions and uses of representational vehicles solves the problem.

I argued that one's response to the problem of mistargeting due to nonexistent targets would depend on what use one was attempting to put one's representation. The use will dictate whether the representations are successful or not. If one is attempting to make inferences about the target phenomenon that are restricted to the phenomenon's behaviour, then the problem of nonexistent targets need not be a problem, so long as one obtains (suitably) accurate results. However, if one is attempting to use the representation to obtain inferences about the ontology of the world, then misrepresentation due to the non-existence of a target is a major problem.

The issues of idealisation and abstraction[5] were dealt with in §6.2 for the IC and in §7.2 for the PMA. I argued that the IC could account for both types of idealisation (Galilean and singular limit) that I investigated. It could also answer the important question raised by adopting Contessa's framework, of explaining how the faithfulness and usefulness of a representation could be linked. The PMA did not fare as well. I could only establish an answer to the faithfulness and usefulness question for Galilean idealisation, and this answer was not particularly satisfying (that Galilean idealisations are faithful and useful because we make them be). In the case of the singular limit idealisations, recast by Pincock as representations that involve singular perturbations, I found there to be no clear explanation for the relationship between faithfulness and usefulness. I suggested in the previous chapter that this lack of clear explanation was due to a difference in the methodology adopted by Pincock compared to Bueno, Colyvan, French and Contessa. I will now turn to discussing the methodologies, in the hope that this will provide a reason for the lack of explanation of how faithfulness and usefulness are related on the PMA.

## 8.3 Methodologies

Above I suggested that the methodology Pincock adopted towards creating his account was the cause of the issue over how faithfulness and usefulness are related. To form an argument based on this suggestion, I wish to draw parallels between Pincock's approach and that of Brading and Landry in their debate over structural realism with French. Brading and Landry argue[6] that the partial structures framework is not required to account for the applicability

[5] I did not address abstraction directly, claiming that what was said for abstraction could be adapted to accommodate abstraction.

[6] Brading & Landry (2006), Landry (2007), Landry (2012).

of mathematics, in that the mathematical relations themselves are sufficient. French (2012) responds that the root of this rejection of the partial structures framework is a fundamental disagreement over what the role of philosophers of science is. This disagreement consists in what French describes as the rejection of a 'meta-level', a level 'above' scientific theories, which are described as the 'object level'. It is at the meta-level that the philosopher of science operates and employs the partial structures framework as a representational aid to answer questions concerning the structure of, and relationships between, scientific theories, the applicability of mathematics, and questions of scientific realism. After setting out this debate in further detail, based on the most recent contributions, I will argue that Pincock's attitude towards the representation relation is sufficiently similar to Brading and Landry's attitude to the mathematical relations to call into question whether he thinks we should be working with a meta-level.

### 8.3.1 The Brading, Landry & French Debate

French defends the 'meta-level' analysis and use of the partial structures framework by arguing that Brading and Landry's arguments are based upon two problematic positions: a misconstrual of the kinds of activity philosophers of scientists should engage in; and a failure to distinguish between, for example, the work done by group structure at the 'object level' and the work done by the set-theoretic approach required by philosophy of science at the 'meta-level' (French 2012, pg. 3). That theories and the relationships between them are identified with set-theoretic structures and relations is a common mistake put forward as a criticism of the the partial structures framework. As far as French is concerned (French 2012, pg. 5):

> set theory offers an appropriate representational device for the philosophy of science and the structural realist in particular; its use should

> not be taken to imply a particular ontological stance with regard to either theories themselves or the nature of the structure 'in' the world the realist takes them to represent

Landry's central claim is that "debates about, or accounts of, scientific realism, need neither a metaphysical, logical nor mathematical *meta-linguistic* framework" (Landry 2012, pg. 30). She aims to show this by arguing that (Landry 2012, pg. 33):

> mathematical structure itself, and yet not just that of physics, is the 'crucial objectifying structure for science', so there is no need to 'deploy the resources of logic' to 'superadd' to mathematical structure to give a structural account of concepts, or to give, what [she explains] as, a structural presentation of kinds of objects

and to

> consider what work mathematical structure does at the 'object level' of scientific practice and, contra ... French ... forego consideration of what work logical or mathematical structure does at the 'meta-level' of philosophy of science.

Her argument turns on a distinction between the presentation and representation of objects (Brading & Landry 2006, pg. 573):

> [F]or physical theories; theoretical objects, as kinds of physical objects, may be *presented* via the shared structure holding between the theoretical models. However, at the ontological level, a physical theory insofar as it is successful, must also *represent* particular physical objects and/or phenomena and not merely present kinds of physical objects.

The idea behind this distinction is that structural mappings are only capable of identifying shared structure, and that all that is achievable through identifying this shared structure is the identification of what kinds of objects the theory talks about. What is needed is an account of representation that provides an account of how the theory can move from talking about kinds of objects to be about particular objects (Brading & Landry 2006, pg. 576).[7]

Her argument against a meta-level approach is that "unless we presume that the world is set-structured, appeals to *the* meta-linguistic structure of scientific *theories* have no ontological bite. That is, there is no *global*, meta-level, 'philosophical' perspective of the structure of scientific theories from where [the No Miracles Argument] can be put to use" (Landry 2012, pg. 47). However, she does believe that we can adopt a "*local*, object level, 'scientific' perspective of the structure of a *particular* scientific theory from where [the No Miracles Argument] can be used to 'cut nature"' (Landry 2012, pg. 48). The argument for this is in part the rejection of the appeal to partial structures (due to the claim that they do no work in presenting the structure of, for example, quantum mechanical structures, or in the representation of quantum mechanical objects) and in part an explanation of, for example, how group-theoretic structure and group-theoretic morphisms "*do the real work*". The idea is that when one expresses quantum mechanics group-theoretically, one forms a theory that presents the structure of quantum mechanical objects in group-theoretical terms: "e.g. it presents bosons and fermions as group-theoretically 'constituted' *kind of objects*" (Landry 2012, pg. 50). French's position on how set theory should be used, as a representational device at the meta-level, suggests that Landry is wrong to think that we have to believe the world is set-theoretically structured in order to have "ontological bite" from the set-theoretical structure when it operates at the meta-level. Further to this, French reminds us that "the structure that the structural realist is concerned with should not be, and should never have been, construed as 'pure'

[7] I will leave the details of these positions to the side, as they concern the debate over structural realism, a debate I am not interested in in this thesis.

logico-mathematical structure; it was always intended to be understood as *theoretically informed* structure" (French 2012, pg. 6).

The point of contention between French and Landry I want to focus on is how they each specify the notion of shared structure between the physical (data or phenomena models) and the mathematical models. For French, this is specified by the notions of partial structure and partial morphisms. For Landry, the shared structure is made precise at the local level, that is "the shared structure can be made appropriately precise via the notion of a morphisms and the context of scientific practice determines what kind of morphism" (French 2012, pg. 6). French quotes her claim that (Landry 2007, pg. 2):

> mathematically speaking, there is no reason for our continuing to assume that structures and/or morphisms are 'made-up' of sets. Thus, to account for the fact that two models share structure we do not have to specify what models, *qua* types of set-structures, are. It is enough to say that, in the context under consideration, there is a morphism between the two systems, *qua* mathematical or physical models, that makes precise the claim that they share the appropriate kind of structure.

This context dependency is brought out clearly in what Landry says about the application of group theory to quantum mechanics. She argues that the morphisms that are doing the work in connecting the models are group-theoretic, not set-theoretic (e.g. Lie group transformations), and that (Landry 2007, pg. 10-11):

> what does the real work is not the framework of set theory (or even category theory); it is the group-theoretic morphisms alone that serve to tells us what the appropriate kind of structure is.

French generalises this point as "the use of the concept of shared structure that determines the kind of structure and characterises the relevant meaning and all the relevant work is done by the contextually defined morphisms" (French 2012, pg. 10).

French's argument can now be summarised: he concentrates on what counts as relevant 'work' from the above quotation by asking the questions 'who's using?' and 'what's working?' In the group theory and quantum mechanics example, it's clear that the physicists and mathematicians use group theory, rather than partial set-theoretic structures, in the relevant physical contexts: "in the context of the quantum revolution, it was group theory, not (partial) set-structures that was effectively doing the (physical, mathematical and hence object level representational) work" (French 2012, pg. 21). Philosophers of science use the partial set-theoretic structures to represent theories, their inter-relationships, etc., in order to analyse the structure of scientific theories, the inter-relationships between theories, answer questions concerning the applicability of mathematics (e.g. how are the faithfulness and usefulness of a representation related?), settle ontological questions, and so on. While this is a representational activity, as well, the representational target is clearly different. Philosophers of science are operating at the 'meta-level', where the target is theories, rather than the world, as is the case at the object level. Set theoretic structures are doing the work in this representational activity at the 'meta-level'. So the disagreement comes from . . . :

> the need for a meta-level representational unitary framework (provided by set-theory, category theory, whatever). Brading and Landry insist that it is 'shared structure' (group-theoretic, in the above case study) that does all the work but the work that is being done is 'physical' (!) work and while I agree that this is appropriate for physicists, philosophers are doing a different kind of work, that requires a different set of tools. To insist that this form of work should be dismissed would be a radical step that would fundamentally revise our conception of what the philosophy of science is all about.

French concludes that proper philosophical analysis cannot proceed without employing the appropriate tools at this meta-level, and that a rejection of this level turns philosophy of science into something it should not be (French 2012, pg. 26):

> Without some formal framework, set-theoretic or otherwise, that can act as an appropriate mode of representation at the meta-level, our account of episodes such as the introduction of group theory into quantum mechanics would amount to nothing more than a meta-level positivistic recitation of the 'facts' at the level of practice. Any concern that the choice of a set theoretic representation of such an account would imply that set theory is constitutive of the notion of structure can be assuaged by insisting on the above distinction between levels and modes of representation.

The "positivist recitation of the 'facts"' is the claim that the group-theoretic morphisms account for all of the features of science we are attempting to explain as philosophers of science. I agree with French's responses to Brading and Landry. Philosophers of scientists are operating at a 'meta-level' and so require the appropriate tools to operate at this level.

The debate between French and Brading & Landry is over whether we need the 'meta-level' analysis, and so over whether there is any work to be done by the set-theoretic structures. Part of Brading and Landry's rejection of the meta-level is the argument that the context determined morphisms are sufficient in explaining how objects are presented. Another is a refusal to identify (or reduce) these morphisms to any underlying, unifying framework (either set theory or category theory). These two positions lead Brading & Landry to argue that applications of group theory to quantum mechanics can be explained in terms of group-theoretic morphisms, such as the transformations between the Lie groups. To me, this position is similar to Pincock's view that the structural relation that forms the representation relation is context dependent, in that it can contain mathematical terms. I will now outline how I view Pincock's methodology, looking at the role specific examples play on his account. I will argue that Pincock does adopt some form of meta-level/object level distinction, but that the context dependence he embraces is the root of the difficulties I have found with his account thus far.

### 8.3.2 Pincock's Methodological Approach

Pincock holds that mathematical representations obtain their content in three stages: we fix the mathematical structure used as the representational vehicle (the model); we assign (parts of) this model physical significance through denotation or reference relations; and a structural relation is given so that parts of the mathematical structure can be mapped to the target system (Pincock 2012, pg. 257). Pincock says very little about how this structural relation should be understood. When outlining his various notions of content, he argues that we need more complicated relations than isomorphisms and homomorphisms (Pincock 2012, pg. 31). This greater complexity was to be introduced through the introduction of mathematics into the structural relation. For the temperature diffusion example employed during the introduction of the notion of enriched contents, this consisted in including an error term with an isomorphism as the structural relation. More generally, this can be phrased as the enriched contents being "specified in terms of the aspects of the mathematical structure", via the structural relation. When providing his answer to the applied metaphysical question, Pincock argues the simplicity in answering the problem allows for numerous answers to be given. He rejects Steiner's set-theoretic solution, arguing that his structural relations (those set out in Pincock (2012), Chapter 2), are a possible solution. Pincock endorses this solution is one does not identify the relations with sets (Pincock 2012, pg.

174).[8] Finally, when discussing the indispensability of mathematics (and the agreement of the anti-realist positions of Hellman and Lewis with his version of the indispensability argument), he again comments on the structural relation being specified, i.e. including, mathematical terms (Pincock 2012, pg. 197). Given that enriched contents are required for the majority of representations (ones which go beyond the simple basic contents which require isomorphisms between target and vehicle), I take it that nearly all mathematical representations will involve structural relations that include mathematical terms in some way.

The existence of mathematics in the structural relation brings Pincock's position close to Brading's and Landry's. This is because the mathematics involved in the structural relation will be dependent upon the specific target of the representation. This is clearly seen in the temperature example, where the structural relation is held to include an error term in order to accommodate temperature being defined over regions, while the real numbers describe a dense continuum (Pincock 2012, pg. 31). Pincock does not discuss any other representation in such detailed terms, so it is unclear what he would take to be included in other cases. One can make an educated guess however. In the irrotational fluid example (used in his exposition of abstract varying representations (Pincock 2012, §4.2)), the Navier-Stokes equation is simplified by adopting several idealisations: that the fluid is incompressible and homogenous, which results in a constant density; that there is a steady flow of fluid, which results in the equations being time independent; and that we ignore the viscosity of the fluid, which involves setting the term that represents viscosity, $\mu$, to 1 (Pincock 2012, pg. 69). I think it is reasonable to take the structural relation required for this representation to include the setting of the density to a constant value and $\mu = 1$. It would also not relate to time, though this leads

[8]This mirrors Landry's position of refusing to identify the morphisms with set-theoretic or category-theoretic morphisms (Landry 2007, pg. 2).

to schematic content.[9] In the rainbow example, Pincock talks of the critical points being connected to the rainbow through both ray and wave theoretic concepts (Pincock 2012, pg. 238). While I disagree with Pincock's analysis of the representation of the rainbow, if we are to accept what Pincock says here, I think that the structural relation involved would have to include ray and wave mathematics in order to do its job of relating the CAM model to the rainbow in the way Pincock claims it is. From these examples and how the structural relations need to be structured, i.e. what mathematics they need to include, I can infer how Pincock would analyse Landry's group-theoretic quantum mechanics example. It requires either a group-theoretic mapping as the structural relation in the extreme case, or group-theoretic concepts in the structural relation in the conservative case.

One might infer from the above argument that I take Pincock to implicitly reject the need for a meta-level analysis of representation. I do not think that this is the case. Rather I think that his focus on what he considers to be the epistemic benefits of mathematics and a focus on case studies has shaped his account in a way that prevents it from properly answering the question over how faithfulness and usefulness are related. When outlining his approach, Pincock is explicit in looking for a "general strategy" (Pincock 2012, pg. 5) for answering the questions he is looking at (Pincock 2012, pg. 3):

> [F]or a given physical situation, context, and mathematical representation of the situation [we can ask:]
>
> (1) what does the mathematics contribute to the representation,
>
> (2) how does it make this contribution, and
>
> (3) what must be in place for this contribution to occur?

Pincock's account of representation is therefore aimed at providing general an-

[9] Pincock explains why this is the case when outlining the moves involved in establishing a 'steady state' representation of traffic flow (Pincock 2012, pg. 55).

swers to these questions, and should be capable of providing general answers to the questions I raised in Chapter 2. Indeed, he must think his account is capable of this, as he devotes an entire chapter to analysing Steiner's questions (Pincock 2012, §8).[10] What, then, has gone wrong for Pincock? In §8.3.3 I will provide a brief survey of what he takes to be some of the main epistemic benefits of the use of mathematics in scientific representations. This survey indicates that all of these can be accounted for by the IC (and thus by accounts of representation that explicitly focus on the surrogative inferences). So the problem is not caused directly by the different focus on the epistemic benefits itself. Rather, I think the problem is that the structural relation includes mathematics specific to the representation at hand. This trivialises the obtaining of faithful representation, which violates Contessa's maxim that while obtaining epistemic representation is cheap, obtaining faithful epistemic representation is costly.

I will now provide the brief survey of the epistemic contributions and outline how they can be accommodated through surrogative inferences. Two of these contributions will be given further space as I take Pincock's arguments for how his account accommodates them to be based on a position I do not think he should hold.

### 8.3.3 Accounting For Pincock's Epistemic Contributions

The epistemic contributions Pincock believes mathematics makes to science include (Pincock 2012, pg. 8):[11]

i) "aiding in the confirmation of the accuracy of a given representation through prediction and experimentation";

[10] I even relied on some of this analysis in my Chapter 2.

[11] Throughout this thesis I have not discussed the constitutive role (or the derivative role) of mathematics that Pincock outlines in his Chapter 6, as I do not take it to be relevant to the questions I am concerned with. As such, the epistemic contributions Pincock takes mathematics to contribute due to these roles have been ignored here.

ii) "calibrating the content of a given representation to the evidence available";

iii) "making an otherwise irresolvable problem tractable";

iv) "offering crucial insights into the nature of physical systems".

This is in addition to the "metaphysical role of isolating fundamentally mathematical structures inherent in the physical world". 'Inherent' here should be read in a way that is appropriate for one's account of representation. In the case of the IC, this would be that the target systems could be structured in the way required to be considered empirical set ups. In Pincock's case, this would be cashed out in terms of the notion of instantiation he adopts (Pincock 2012, pg. 29). Pincock also identifies further epistemic contributions that come from the various types of representations he identifies (Pincock 2012, pg. 9-11). Abstract acausal representations offer the following contributions:

(v) if the acausal representation was obtained from a causal representation, then we might gain information about the system that was not explicit in the casual representation;

(vi) the acausal representation is easier to confirm, due to having less content.

The abstract varying representations also involve particular epistemic contributions:

vii) enable the transfer of evidential support from one representation (in the family) to another;

viii) indirect contribution from failures of the representation to treat all members of the family equal (Pincock 2012, pg. 82).

Either due to:

(viii.a) there being more shared structure than identified by the representation;

(viii.b) overextension demonstrating that a member of the family is different to an/the other member/s in some respect.

And finally the representations concerned with scale supposedly provide the contribution of (Pincock 2012, pg. 119):

ix) a third response to the debate over metaphysical interpretations and instrumentalism concerning 'emergent' properties

I have already looked at the viability of ix) in §7.2.2 through §7.3.2, and the arguments in this chapter concerning the methodology Pincock has adopted. Contributions iii), iv) and (v) are all straightforwardly handled through the ability to draw sound surrogative inferences in our representational vehicle. I understand the contribution of i) to simply be the obtaining of sound surrogative inferences, that is, of establishing a (partially) faithful representation. I take the idea behind the notion of "calibrating the content" included in ii) to be that one aims to include only what one is is aiming to represent in the world in one's representation, though this is masked by being set out in a way that appears rather specific to Pincock's account. As this will involve finding suitable mathematics on any account of representation, I do not take this to be a specific contribution obtained only through Pincock's account. I take (vi) to be independent of one's account of representation: it is a claim about one's account of confirmation and how that relates to 'how much' content a representation has. I take that any representation that results in less content (however that is measured) will be easier to confirm if one adopts a similar account of confirmation as Pincock does (i.e. a Bayesian inspired account.)[12]

The contributions identified in (viii.a) and (viii.b) require individual analysis. These are both due to abstract varying representations. These are rep-

[12]See Pincock (2012, §2.8-2.9.

resentations that involve a mathematical model that is variously interpreted in terms of different systems. These systems might be different only in that they are individual systems of the same type, e.g. two copper springs, or different in nearly every way, such as (in Pincock's example) irrotational fluids and electrostatics.

The two contributions in (viii.a) and (viii.b) are held to be due to there being some shared structure between the two systems (this is the basis behind there being abstract varying representations in the first place). However, Pincock places more emphasis on this shared structure than I think he should be able to if he is following his account properly. In particular, given the claims he makes over how mathematics is interpreted during a representation.

Both cases are held to be indirect contributions due to there being inaccuracies when applying the abstract varying representations. (viii.a) occurs when "there is some underlying mathematical similarity to all the systems, but the current family of representations has missed this in some respects" (Pincock 2012, pg. 82). (viii.b) occurs when one member of the family can be represented with an extension of the mathematics, but another member cannot. This can lead to differences between the members being identified. The idea behind these contributions is that the targets of the abstract varying representations share a certain amount of structure that is partially reproduced in the representation. These arguments also require that one is able to discuss the mathematical similarities or dissimilarities between the representations and draw conclusions from this. Such a discussion requires the ability to divorce the mathematics from its interpretation (to discuss the mathematical structure itself) and then hold that the mathematical structure is in some sense shared/instantiated/etc. (depending on one's account of representation) between the targets. But Pincock faces a problem with both of these requirements. First, Pincock can only talk of the content of representations in uninterpreted terms when it is shifted to a schematic content. This means that the abstract varying representations would only be able to provide some of their epistemic benefits when they are not, in fact, being 'various interpreted', but when they are shifted to schematic versions that do not have interpretations. Second, Pincock is clear in outlining these epistemic benefits as being obtainable when mathematics is not playing a "metaphysical role", that is, when it is not being claimed to be shared/instantiated/etc. in the target system (Pincock 2012, pg. 8). But this second requirement only makes sense if mathematics is playing the "metaphysical role": unless one claims that the targets of the abstract varying structures have some structure in common that the mathematics is reproducing (as per one's account of representation), then the mathematical arguments are unfounded.

These advantages can be accounted for by the IC: we map to the same mathematical model in each case due to each of the target systems being partially -morphic to the repeatedly used mathematical model. As we are now in the domain of uninterpreted mathematics, we can make the necessary mathematical arguments to derive our conclusions. The interpretation mappings then let us attempt to relate our mathematical results to the target systems, and the failure (or not) of these partial mappings can inform the fit of the mathematical conclusions to the target system. (viii.a) will occur when we have mapped 'too small' a partial structure to the mathematical domain from the empirical set up, and the same partial mappings are used in the immersion and interpretation steps in all of the varying representations.[13] Thus the same piece of the target systems' structures are missed in each application. (viii.b) occurs when the same partial mapping is used in the immersion step, but the

[13]By 'same' mapping here, I mean a mapping that takes the same mathematical elements and relations to suitable elements and relations in the empirical structured in the same way. That is, the mappings will result in the same structure in each empirical set up. The different interpretation comes from the structures in each empirical set up being of different physical systems.

resultant structure after the 'over extension' cannot be mapped to different empirical set ups using the same interpretation mapping. The idea here is that we have the same partial structure mapped from the different empirical set ups to the mathematical domain, but that there is no 'larger' partial structure in common between the empirical set ups. Hence some extension of the 'small' mathematical structure to a 'larger' mathematical structure will result in us being unable to use the same interpretation mapping in all instances.

As the IC (and other accounts of representation that focus on surrogative inferences) can accommodate the epistemic contributions outlined in this section, another argument in favour of Pincock's account is blocked. If these types of account could not accommodate the epistemic contributions, then they would be missing something related to mathematical representation that Pincock's account has identified. However, this is not the case. Thus Pincock's account does not appear to offer anything over and above what the IC itself is capable of providing.

I will now, finally, choose between the IC and the PMA as my favoured account of mathematical representation.

## 8.4 Choosing An Account

As I highlighted above, the irrotational fluid example included several idealisations (e.g. the fluid is incompressible and homogenous, so the density is a constant), which would require mathematics specific to that example being included in the structural relation. I then argued that in more complex cases, the structural relation would end up being close to the type of relation that Brading and Landry claim as being sufficient to account for the representations. This same relation is one that French would understand to be operating at the 'object level'. I agreed with French's argument that the work we are required to do as philosophers of science require its own set of tools, and that set theory is one such tool. It is important to remember is that partial structures are being used to represent structures and relationships at the object level in our philosophical work at the meta-level. By including mathematics in the structural relation in the way I understand Pincock to be, the distinction between the object level and meta-level is blurred. It becomes unclear how we can use mathematics (that is, structural relations that include mathematical terms) to represent the relationship between mathematics and physical systems to answer questions of how mathematics is applicable, in general, at the meta-level. A consequence of this is that the work required of the philosopher of science becomes more difficult to achieve. Hence my difficulty in identifying the relationship between faithfulness and usefulness in Pincock's analysis of the singular perturbation representations. If the structural relations responsible for representational content are specified in the way Pincock requires, making use of case specific mathematics, then accounting for our ability to use representational vehicles to draw sound surrogative inferences is a simple matter of pointing to the structural relation. As I claimed when discussing Pincock's analysis of Galilean idealisations, and what now appears to hold for all representations on Pincock's account, representations are faithful and useful on Pincock's account because we design them to be. The idea is that we design our representations to have a sufficiently close a fit to our target system such that the results are more than likely to be correct, and so we trivially obtain faithful representations.

I subscribe to Contessa's maxim that faithful epistemic representation does not come cheap (Contessa 2011, pg. 127). His brief argument for this is that it doesn't take much to draw surrogative inferences with a representational vehicle about a target (hence epistemic representation being cheap), but it does take a lot to explain how we are able to construct faithful epistemic representations that are sufficiently faithful for our (scientific) purposes. This is a maxim made and argued for from the meta-level perspective. If we ask how representations are able to grant us sound inferences at the object level, the answer is rather obvious: the representations are physically interpreted mathematics that are related to the target system through (very specific) mathematical relationships. But these object level questions can only provide case specific answers. When operating at the meta-level, we want general answers to our questions, as the questions at this level are general themselves. Providing adequate answers to questions at this level is difficult, given the range of cases that such an answer will have to cover, hence the cost involved in providing a suitable account of faithful epistemic representation. Due to Pincock's blurring of the object and meta-levels, I take his answers to trivialise this difficulty, providing answers that are too case specific than is suitable when operating at the properly distinguished meta-level. For this reason I am inclined to reject his account as a viable account of representation.

In contrast to Pincock's Mapping Account, the Inferential Conception has answered the questions set out in Chapter 2. It is able to account for relationship between the faithfulness and usefulness of representations in a way that respects the costs involved in answering the question, and does so at what I take to be the appropriate level (a clearly distinguished meta-level). While the account still requires some work (see §6.2.3 onwards), it is a more successful account of representation than Pincock's.

A final worry can be levelled at the IC. The arguments in this chapter rely on the meta-level/object level distinction. By adopting this distinction, I am committing myself to employing the partial structures framework in a representational capacity. Thus, my answers to the problems of the applicability of mathematics are at the meta-level. This role and the meta-level/object level distinction was employed by French to resist the reductive moves Landry wished to make (e.g. that group theory was really set theory, and so set theory accounts for quantum phenomena). One might therefore take the answers I have supplied via the IC to not actually answer the constitutive question of Chapter 3, nor the applied metaphysical question of Chapter 2. If the partial structures framework of the IC is in fact representing relationships at the object level, then I have not provided an account of what the representation relation, or the relation responsible for the applicability of mathematics, is at the object level. And it at this level that the constitution question and applied metaphysical question are operating. My response to this charge to insist that these questions operate at the meta-level. The meta-level is simply the level at which philosophers of science operate. Thus any philosophical question is a question at the meta-level. Confusing the levels causes the problems found in Pincock's account.

## 8.5 Conclusion

In this chapter I have brought the questions of Chapter 2 to bear on the IC and the PMA. The both accounts could provide answers to the isolated mathematics, multiple interpretations and novel predictions problems. The IC provided a good answer to the problems of representation. The attempt to answer these questions on the PMA prompted an investigation into the methodology Pincock adopted in constructing his account. The conclusion of this investigation was that Pincock's methodology trivialised the obtaining of faithful epistemic representation. This occurred due to the focus on examples and inclusion of mathematics in the structural relation. The trivialisation of faithful epistemic representation in combination with the quality and cohesiveness of the IC's answers to the other problems prompted me to choose the IC as the superior account of mathematical representation.

I will now draw the thesis to a close by summarising the conclusions of each chapter and the thesis as a whole in the next chapter. I will also identify where further work can be pursued, and whether such work can improve the position of each account.

# 9 | Conclusion

In this thesis I set out to answer several of the problems of the applicability of mathematics. In Chapter 2 I argued that there are nine problems that fall under this banner, and that the problems of representation were the most significant problems. Answers to these problems set limits on the relation that answers the applied metaphysical problem and form the basis for answering the problem of isolated mathematics and the novel predictions problem. I argued in favour of structural accounts of representation in Chapter 3, concluding that they offered the resources necessary to provide accounts of faithful epistemic representation. A partially faithful representation is one that facilitates the drawing of at least one sound surrogative inference from vehicle to target. I argued in favour of the Inferential Conception of the Applicability of Mathematics, set out in Chapter 6, throughout the rest of the thesis and rejected Pincock's Mapping Account, set out in Chapter 7. I introduced the case study of the rainbow, concentrating on the $\beta \to \infty$ idealisation and the CAM approach, which is required to explain the supernumerary bows. I used the case study to establish whether each account could explain the relationship between the faithfulness and usefulness of representations. My argument against Pincock's Mapping Account relied on a problem with the methodology Pincock adopted in constructing his account. His focus on examples and incorporating mathematics into the structural relation that formed part of his representation relation trivialised the relationship between the faithfulness and usefulness of representations. Contessa's maxim holds that establishing how the faithfulness and usefulness of representations are related is a costly exercise. Pincock's account therefore violates this maxim, and subsequently was rejected.

According to the Inferential Conception, mathematics is applicable due to a structural relation between the mathematical and physical domains. This relation should be understood as a partial mapping between partial structures. The IC is capable of providing answers to the problems of representation (the multiple interpretations problem and misrepresentation due to abstraction and idealisation), isolated mathematics and novel prediction problems. It holds that whether mathematics is isolated or not is irrelevant to its capacity to be applied; all that matters is whether it can enter the appropriate structural relationships with the world. The multiple interpretation problem is answered by attributing the success of inconsistent interpretations of a piece of mathematics to the structural similarities between the empirical set ups, the targets of the representation. The novel predictions problem is answered straightforwardly; all interpretation on the IC consists in a partial mapping between the mathematics and the empirical set up, thus there is no difference between so called Pythagorean or Formalist predictions and typical examples of prediction. Thus a novel prediction occurs when there is a novel entity, behaviour or so on in the empirical set up irrespective of the supposedly distinct types of prediction used to obtain that novelty. How the IC answers the issues related to misrepresentation due to abstraction and idealisation will be outlined below, after I outline how the IC might contribute to answering one of the problems of applicability I did not address in this thesis.

The most relevant problem of applicability to the IC that I did not address is the mathematical explanation problem: how does mathematics provide genuine explanations of physical phenomena? This has already been touched on by proponents of the IC, such as Bueno & French (2012). At minimum, the IC provides a framework to approach the question. First, it allows us to identify where the explanation might occur. Within the derivation step, the mathematics is all uninterpreted, and so any putative mathematical explanation would be an 'internal' mathematical explanation, whereas what is required is an 'external' explanation: an explanation of a physical phenomena that is external to the mathematical domain.[1] Thus the explanation would have to come from some mathematics within the final empirical set up. A strict reading of the IC would then appear to rule out any mathematical explanation, as the mathematics in the empirical set up is interpreted. i.e. Physical facts would be doing the explanatory work, rather than mathematical facts. A possible response is that the mathematics in the $R_3$ component of the empirical set up could provide a mathematical explanation as it is still uninterpreted. Second, accounts of explanation that might allow for mathematical explanations would have to be consistent with the IC. The IC looks to be a promising way to approach this issue. Hopefully it can also provide a similar framework to approach the semantic problem.

The IC was found to be a fairly successful account of faithful epistemic representation. The relationship between the faithfulness and usefulness of representations could be accommodated by the partial structures framework and partial truth. A 'measure' of the partiality of a representation could be achieved by comparing the size of the $R_i$ between the vehicle and target. Representations can be considered useful, however partially faithful they are, as they can be considered as 'as if' descriptions. Here the notion of partial truth comes into play. Within a theoretical context, provided by the interpretation of the mathematics, the representation can be considered 'as if' it were true. This requires a revision to when we can claim a piece of mathematics as constituting

[1]Here I am adopting the notions of internal and external mathematical explanation outlined by Baker (2009).

an idealisation: strictly, there are no idealised representations on the IC, as the mathematics is uninterpreted while it is a representational vehicle. We can only call the mathematics an idealisation once it has a theoretical context, which comes when we interpret it. Thus we have idealised representations when the empirical set ups are used as the representational vehicles in other parts of the Semantic View.

Another issue related to this is the notion of surplus structure. I argued that the fourth role of surplus structure, that of accommodating Hesse's tripartite analysis of analogies, should be restricted to the horizontal axis of the SV, while the second (surplus mathematics that is initially not interpreted, but later does receive an interpretation) and third roles (the extra mathematics involved in idealisations such as representing magnitudes by real numbers) should be restricted to the final empirical set ups, after the interpretation mappings. The first role of surplus structure, mathematics that never receives a physical interpretation, should be restricted to the first step of the IC, the immersion step. I argued that in order to accommodate this role of surplus structure several changes would have to be made to how singular limit idealisations are typically addressed and how the 'models' of the IC are understood would have to be altered. First, the SV could no longer be committed to the idea that a singular limit necessitated a move to surplus structure. This was shown to be the case through the $\beta \to \infty$ idealisation. Second, surplus structure has to be understood as a family of structures that provide novel surrogative inferential relations. A consequence of this is that when we iterate the immersion mapping to obtain a second model, the surplus structure, this 'model' is actually a family of structures. Third, I generated a dilemma for accommodating the surplus structure within the $R_3$ component of the partial structures. Briefly, either one has to be committed to what I named the Very Large $R_3$, i.e. all mathematics (or structure suitably (partially) -morphic to all mathematics) would have to be included in the $R_3$ component, or one must find some way to restrict what is included in the $R_3$ component. I sketched several solutions to each horn of this dilemma. The most promising of these solutions was the 'natural' restriction solution to the Mathematical Restriction Problem. This solution requires one to include only that mathematics which could be considered a natural extension to the mathematics one is currently employing. This solution gained support through my analysis of the CAM approach.

There is still further work to be done on these solutions. All of the potential solutions for the surplus structure dilemma require further development. For example, the notion of 'natural' central to the natural restriction solution needs to be explored to ensure that it is not too permissive or too chauvinistic in other cases. Perhaps the most significant further work that needs to be carried out is to develop the IC as a third axis to the SV. The SV can be considered to have a horizontal axis, which consists of inter-theory relationships, and a vertical axis, which consists of relationships between data, phenomena and theoretical models (French 2000, pg. 106). The 'points' along each of these axes are the models of the SV, which are the representational targets of the the IC.[2] Each of these models will include mathematics to some degree, and so require the resources of the IC to explicate how the mathematics represents in each instance. Exactly how the IC can fit as a third axis with the other two required development. Issues that need to be investigated include how the IC fits with the dynamic inter-theory relations along the horizontal axis and whether the problems of model creation and model use are solved through adopting the IC as a third axis.

Pincock's Mapping Account was initially promising. His various notions of content and more relaxed approach to the structural relations (by including

[2]The axis metaphor is not to be taken too literally here. A model should not have a three valued co-ordinate. Rather 'axis' should be thought of more in terms of dimensions along which moves can be made to other models of varying features.

mathematics in the relations) appeared to provide a good framework within which idealisations could be easily accommodated, and the relationship between the faithfulness and usefulness of the representations could be accounted for. This initial hope was misplaced however. The answers I obtained from Pincock's Mapping Account regarding the issues of how the faithfulness and usefulness of representations are related were found wanting for both types of idealisation. Galilean idealisations were found to be faithful due to the structural similarities between the vehicle and the target, and useful due to the structural relations. However, the justification for why the structural similarities and relations ground the notions of faithfulness and usefulness appeared to be based on the notion that we made these idealisations faithful and useful for our purposes, which does not properly answer the question.

With respect to singular limit idealisations (or as cast by Pincock: singular perturbation idealisations), Pincock's account failed in different ways depending on what sort of singular limit idealisation was involved. Apparently instrumental cases, such as the boundary layer, provided no explanation for how the faithfulness and usefulness of the representation were related. Noting that the representation has to involve genuine content to obtain a prediction did not help, as this merely involves specifying parameters for the case at hand; it does not explain why the idealisations and mathematics actually produce useful answers. Initially, it seemed that adopting a position of metaphysical agnosticism towards other singular perturbation cases, such as the Bénard cells, resulted in a circular justification for why these representations were faithful and useful. I conducted further investigation into the metaphysical agnosticism position by comparing and contrasting it with what Pincock had to say about the $\beta \to \infty$ idealisation and CAM approach. The conclusion of this investigation was that the metaphysical agnosticism position was either unjustified (due to being underdeveloped) or collapses into some kind of reductionist position, given Pincock's theory, representation and model distinction.

A final attempt to understand what Pincock might say concerning the relationship between the faithfulness and usefulness of representations was conducted by assessing the methodology Pincock adopted in constructing his account. I argued that Pincock's approach of adopting structural relations that incorporate mathematics dependent upon the example at hand trivialises the relationship between faithfulness and usefulness on his account. For this reason, I feel that he not satisfied Contessa's maxim that it should be difficult to obtain faithful epistemic representation, and so rejected his account in favour of the IC. It is not clear that any adjustments could save Pincock's account. Any restriction of the structural relation to the typical set theoretic structural mappings of iso- or homo- morphisms would restrict one's ability to adopt enriched contents. A move to partial structures might be one way to go. The obvious question to ask in response to this is whether Pincock's account would collapse into, or could be merged with, the IC. One reason I think this is unlikely is that the PMA allows mathematics to be interpreted while acting as a representational vehicle, while the IC does not. This is an insurmountable difference.

# A | Appendix: Mathematics of the Rainbow

## A.1 Introduction

In this appendix I will set out the derivation of the exact Mie solution for the scattering of an electromagnetic spherical wave by a transparent sphere. This derivation will be mostly taken from Liou (2002) and Grandy Jr (2005). I will also set out a large part of the derivation of the Debye expansion of the Mie solution, taken from Nussenzveig (1969a, 1992). I will briefly mention the relevant equations for the Poisson sum formula, which is used in extending real functions to the complex plane, before outlining the important points of the derivation of the unified CAM approach, in particular the CFU method for evaluating the contributions of the saddle points, taken from Nussenzveig (1992). Finally, I will provide a brief summary of mathematics behind poles, which are the other kind of critical point involved in the CAM approach, the Regge-Debye poles.

## A.2 Mie Representation[1]

We start with Maxwell's equations, where **E** is the electric vector, **D** is the electric displacement, **j** is the electric current density, **B** is the magnetic induction vector, **H** is the magnetic vector, **c** is the speed of light, **t** is time and $\rho$ is the density of charge:

$$\nabla \times \mathbf{H} = \frac{1}{c}\frac{\partial \mathbf{D}}{\partial t} + \frac{4\pi}{c}\mathbf{j} \tag{A.1}$$

$$\nabla \times \mathbf{E} = -\frac{1}{c}\frac{\partial \mathbf{B}}{\partial t} \tag{A.2}$$

$$\nabla \cdot \mathbf{D} = 4\pi\rho \tag{A.3}$$

$$\nabla \cdot \mathbf{B} = 0 \tag{A.4}$$

Since $\nabla \cdot \nabla \times \mathbf{H} = 0$, performing a dot product on (A.1) leads to

$$\nabla \cdot \mathbf{j} = -\frac{1}{4\pi}\nabla \cdot \frac{\partial \mathbf{D}}{\partial t} \tag{A.5}$$

Differentiate (A.3) to get:

$$\frac{\partial \rho}{\partial t} + \nabla \cdot \mathbf{j} = 0 \tag{A.6}$$

To get a unique determination of the field vector from a given distribution of current and charges, the above equations must be supplemented by relationships that describe the behaviour of substances under the influence of the field.

[1] The majority of the derivation is taken from (Liou 2002, pg. 177-188), and (Grandy Jr 2005, pg. 100-101).

These are given by:

$$j = \sigma \mathbf{E} \tag{A.7}$$

$$\mathbf{D} = \varepsilon \mathbf{E} \tag{A.8}$$

$$\mathbf{B} = \mu H \tag{A.9}$$

where $\sigma$ is the specific conductivity, $\varepsilon$ the permittivity and $\mu$ the magnetic permeability.

Consider the situation where there are no charges, $\rho = 0$, no current, $|\mathbf{j}| = 0$, and the medium is homogeneous so that $\varepsilon$ and $\mu$ are constants. Thus we can reduce the Maxwell equations to:

$$\nabla \times \mathbf{H} = \frac{\varepsilon}{c}\frac{\partial \mathbf{E}}{\partial t} \tag{A.10}$$

$$\nabla \times \mathbf{E} = -\frac{\mu}{c}\frac{\partial \mathbf{H}}{\partial t} \tag{A.11}$$

$$\nabla \cdot \mathbf{E} = 0 \tag{A.12}$$

$$\nabla \cdot \mathbf{H} = 0 \tag{A.13}$$

These equations, (A.10) - (A.13), are used to derive the electromagnetic wave equation. We consider a plane electromagnetic wave in a periodic field with a circular frequency $\omega$ so that:

$$\mathbf{E} \to \mathbf{E}e^{i\omega t} \tag{A.14}$$

$$\mathbf{H} \to \mathbf{H}e^{i\omega t} \tag{A.15}$$

This allows (A.10) and (A.11) to be transformed into:

$$\nabla \times \mathbf{H} = ikm^2\mathbf{E} \tag{A.16}$$

$$\nabla \times \mathbf{E} = -ik\mathbf{H} \tag{A.17}$$

where $k$ is the wave number ($k = \frac{2\pi}{\lambda} = \frac{\omega}{c}$), $m = \sqrt{\varepsilon}$ is the complex refractive index of the medium at frequency $\omega$, and $\mu \approx 1$ is the permeability of air.

We perform the curl operation on (A.17). Then, making use of the vector identity $\nabla \times (\nabla \times \mathbf{V}) = \nabla(\nabla \cdot \mathbf{V}) - \nabla^2\mathbf{V}$ and noting that $\nabla \times \nabla \times \mathbf{E} = 0$ and $\nabla \cdot \mathbf{E} = 0$, and similarly for $\mathbf{H}$, we can obtain:

$$\nabla^2\mathbf{E} = -k^2m^2\mathbf{E} \tag{A.18}$$

$$\nabla^2\mathbf{H} = -k^2m^2\mathbf{H} \tag{A.19}$$

From these two equations, we can see that the electric vector and the magnetic induction in a homogeneous medium satisfy the vector wave equation in the form of (A.20), where $\mathbf{A}$ can be either $\mathbf{E}$ or $\mathbf{H}$:

$$\nabla^2\mathbf{A} + k^2m^2\mathbf{A} = 0 \tag{A.20}$$

If $\psi$ satisfies the scalar wave equation (A.21), the variables of which can be separated through the definition of (A.23). The scalar wave equation is given in spherical coordinates in (A.22).

$$0 = \nabla^2\psi + k^2m^2\psi \tag{A.21}$$

$$0 = \frac{1}{r^2}\frac{\partial}{\partial r}\left(r^2\frac{\partial\phi}{\partial r}\right) + \frac{1}{r^2\sin\theta}\frac{\partial}{\partial\theta}\left(\sin\theta\frac{\partial\psi}{\partial\theta}\right)\frac{1}{r^2\sin\theta}\frac{\partial^2\psi}{\partial\phi^2} + k^2m^2\psi \tag{A.22}$$

$$\psi(r,\theta,\phi) = R(r)\Theta(\theta)\Phi(\phi) \tag{A.23}$$

we can define vectors $\mathbf{M}_\psi$ and $\mathbf{N}_\psi$ which satisfy (A.20). These vectors in spherical coordinates $(r, \theta, \phi)$ are:

$$\begin{aligned}
\mathbf{M}_\psi &= \nabla \times [\mathbf{a}_r (r\psi)] \\
&= \left(\mathbf{a}_r \frac{\partial}{\partial r} + \mathbf{a}_\theta \frac{1}{r}\frac{\partial}{\partial \theta} + \mathbf{a}_\phi \frac{1}{r\sin\theta}\frac{\partial}{\partial \phi}\right) \times [\mathbf{a}_r (r\psi)] \\
&= \mathbf{a}_\theta \frac{1}{r\sin\theta}\frac{\partial (r\psi)}{\partial \phi} - \mathbf{a}_\phi \frac{1}{r}\frac{\partial (r\psi)}{\partial \theta} \qquad \text{(A.24)}
\end{aligned}$$

$$\begin{aligned}
mk\mathbf{N}_\psi &= \nabla \times \mathbf{M}_\psi \\
&= \mathbf{a}_r r \left[\frac{\partial^2 (r\psi)}{\partial r^2} + m^2k^2 (r\psi)\right] + \mathbf{a}_\theta \frac{1}{r}\frac{\partial^2 (r\psi)}{\partial r \partial \theta} + \mathbf{a}_\phi \frac{1}{r\sin\theta}\frac{\partial^2 (r\psi)}{\partial r \partial \phi} \qquad \text{(A.25)}
\end{aligned}$$

The terms $\mathbf{a}_r, \mathbf{a}_\theta, \mathbf{a}_\phi$ are unit vectors in the spherical coordinates.

Using two independent solutions to the scalar wave equation, $u$ and $v$, we find electric and magnetic field vectors (A.26) and (A.27) respectively, that satisfy (A.16) and (A.17) respectively. This allows us to write $\mathbf{E}$ and $\mathbf{H}$ explicitly as follows in (A.28) and (A.29):

$$\mathbf{E} = \mathbf{M}_v + i\mathbf{N}_u \qquad \text{(A.26)}$$

$$\mathbf{H} = m(-\mathbf{M}_u + i\mathbf{N}_v) \qquad \text{(A.27)}$$

$$\begin{aligned}
\mathbf{E} = {} & \mathbf{a}_r \frac{i}{mk}\left[\frac{\partial^2 (ru)}{\partial r^2} + m^2k^2 (ru)\right] + \mathbf{a}_\theta \left[\frac{1}{r\sin\theta}\frac{\partial (rv)}{\partial \phi} + \frac{i}{mkr}\frac{\partial^2 (ru)}{\partial r \partial \theta}\right] \\
& + \mathbf{a}_\phi \left[-\frac{1}{r}\frac{\partial (rv)}{\partial \theta} + \frac{1}{mkr\sin\theta}\frac{\partial^2 (ru)}{\partial r \partial \phi}\right] \qquad \text{(A.28)}
\end{aligned}$$

$$\begin{aligned}
\mathbf{H} = {} & \mathbf{a}_r \frac{i}{k}\left[\frac{\partial^2 (rv)}{\partial r^2} + m^2k^2 (rv)\right] + \mathbf{a}_\theta \left[-\frac{m}{r\sin\theta}\frac{\partial (ru)}{\partial \phi} + \frac{i}{kr}\frac{\partial^2 (rv)}{\partial r \partial \theta}\right] \\
& + \mathbf{a}_\phi \left[\frac{m}{r}\frac{\partial (ru)}{\partial \theta} + \frac{i}{kr\sin\theta}\frac{\partial^2 (rv)}{\partial r \partial \phi}\right] \qquad \text{(A.29)}
\end{aligned}$$

We separate the variables of the scalar wave equation in spherical coordinates by substituting (A.23) into (A.22), then multiply by $r^2 \sin^2\theta$ to obtain:

$$\left[\sin^2\theta \frac{1}{R}\frac{\partial}{\partial r}\left(r^2\frac{\partial R}{\partial r}\right) + \sin\theta \frac{1}{\Theta}\frac{\partial}{\partial\theta}\left(\sin\theta\frac{\partial\Theta}{\partial\theta}\right) + k^2m^2r^2\sin^2\theta\right] + \frac{1}{\Phi}\frac{\partial^2\Phi}{\partial\phi^2} = 0 \tag{A.30}$$

Due to the dependence of the first three terms of (A.30) on $r$ and $\theta$, but not $\phi$, the equation is only valid when the following holds:

$$\frac{1}{\Phi}\frac{\partial^2\Phi}{\partial\phi^2} = \text{ constant } = -\ell^2 \tag{A.31}$$

where we set the constant $\ell$ equal to an integer for mathematical convenience. Substituting (A.31) into (A.30) and dividing through by $\sin^2\theta$ we can write:

$$\frac{1}{R}\frac{\partial}{\partial r}\left(r^2\frac{\partial R}{\partial r}\right) + k^2m^2r^2 + \frac{1}{\sin\theta}\frac{1}{\Theta}\frac{\partial}{\partial\theta}\left(\sin\theta\frac{\partial\Theta}{\partial\theta}\right) - \frac{\ell^2}{\sin^2\theta} = 0 \tag{A.32}$$

which requires the following to hold, where $n$ is an integer:

$$\frac{1}{R}\frac{d}{dr}\left(r^2\frac{dR}{dr}\right) + k^2m^2r^2 = \text{ const } = n(n+1) \tag{A.33}$$

$$\frac{1}{\sin\theta}\frac{1}{\Theta}\frac{d}{d\theta}\left(\sin\theta\frac{d\Theta}{d\theta}\right) - \frac{\ell^2}{\sin^2\theta} = \text{ const } = -n(n+1) \tag{A.34}$$

Rearranging (A.31), (A.33) and (A.34) leads to:

$$\frac{d^2(rR)}{dr^2} + \left[k^2m^2 - \frac{n(n+1)}{r^2}\right](rR) = 0 \tag{A.35}$$

$$\frac{1}{\sin\theta}\frac{d}{d\theta}\left(\sin\theta\frac{d\Theta}{d\theta}\right) + \left[n(n+1) - \frac{\ell^2}{\sin^2\theta}\right]\theta = 0 \tag{A.36}$$

$$\frac{d^2\Phi}{d\phi^2} + \ell^2\phi = 0 \tag{A.37}$$

The single-value solution for (A.37) is:

$$\phi = a_\ell \cos \ell\phi + b_\ell \sin \ell\phi \tag{A.38}$$

where $a_\ell$ and $b_\ell$ are arbitrary constants.

(A.36) is the equation for spherical harmonics. By introducing $\mu = \cos\theta$ we can rewrite (A.36) as:

$$\frac{d}{d\mu}\left[(1-\mu)\frac{d\Theta}{d\mu}\right] + \left[n(n+1) - \frac{\ell^2}{1-\mu^2}\right]\Theta = 0 \tag{A.39}$$

so that its solutions can be expressed by the associated Legendre polynomials (which are spherical harmonics of the first kind) in the form of (A.40).

$$\Theta = P_n^\ell(\mu) = P_n^\ell(\cos\theta) \tag{A.40}$$

We can solve (A.35) by setting the variables $kmr = \rho$ and $R = \frac{1}{\sqrt{\rho}}Z(\rho)$ to obtain (A.41):

$$\frac{d^2Z}{d\rho^2} + \frac{1}{\rho}\frac{dZ}{d\rho} + \left[1 - \frac{\left(n+\frac{1}{2}\right)^2}{\rho^2}\right]Z = 0 \tag{A.41}$$

The solutions to this equation can be expressed by the general cylindrical function of order $n + \frac{1}{2}$, given by:

$$Z = Z_{n+\frac{1}{2}}(\rho) \tag{A.42}$$

Thus the solution of (A.35) is:

$$R = \frac{1}{\sqrt{kmr}} Z_{n+\frac{1}{2}}(kmr) \tag{A.43}$$

Combining the solutions (A.38), (A.40) and (A.43) allows us to express the elementary wave functions at all points on the surface of a sphere as:

$$\psi(r,\theta,\phi) = \frac{1}{\sqrt{kmr}} Z_{n+\frac{1}{2}}(kmr)\, P_n^\ell(\cos\theta)\,(a_\ell \cos\ell\phi + b_\ell \sin\ell\phi) \tag{A.44}$$

The cylindrical functions in (A.43) can be expressed as a linear combination of two cylindrical functions, the Bessel function $J_{n+\frac{1}{2}}$ and the Neumann function $N_{n+\frac{1}{2}}$. We thus define the Riccati-Bessel functions as:

$$\psi_n(\rho) = \sqrt{\frac{\pi\rho}{2}} J_{n+\frac{1}{2}}(\rho) \tag{A.45}$$

$$\chi_n(\rho) = \sqrt{\frac{\pi\rho}{2}} N_{n+\frac{1}{2}}(\rho) \tag{A.46}$$

$\psi_n$ are regular in every finite domain of the $\rho$ plane including the origin, whereas $\chi_n$ have singularities at the origin $\rho = 0$, where they become infinite. Because of this, we can use $\psi_n$ to represent the wave inside the scattering sphere, but not $\chi_n$. Utilising the Bessel and Neumann functions, (A.43) can be rewritten as

$$rR = c_n\psi_n(kmr) + d_n\chi_n(kmr) \tag{A.47}$$

where $c_n$ and $d_n$ are arbitrary constants. (A.47) is the general solution of (A.35).

The general solution of the scalar wave equation (A.22) can then be expressed as:

$$r\psi(r,\theta,\phi) = \sum_{n=0}^{\infty}\sum_{\ell=-n}^{n} P_n^\ell(\cos\theta)\,[c_n\psi_n(kmr) + d_n\chi_n(kmr)]\,(a_\ell\cos\ell\phi + b_\ell\sin\ell\phi) \tag{A.48}$$

As $c_n$ and $d_n$ are arbitrary constants, we can set them to $c_n = 1$ and $d_n = i$

so that we can write the section of (A.48) in square brackets as follows:

$$\psi_n(\rho) + i\chi_n(\rho) = \sqrt{\frac{\pi\rho}{2}} H^{(2)}_{n+\frac{1}{2}}(\rho) = \xi_n(\rho) \tag{A.49}$$

where $H^{(2)}_{n+\frac{1}{2}}$ is the half-integral-order Henkel function of the second kind, which vanishes at infinity in the complex plane, and is thus suitable for representing the scattered wave.

The general solution of the scalar wave equation (A.22) can now be expressed as follows in terms of the spherical Riccati-Bessel and Riccati-Hankel functions:

$$r\psi(r,\theta,\phi) = \sum_{n=0}^{\infty} \sum_{\ell=-n}^{n} P_n^{\ell}(\cos\theta)\, \xi_n(kmr)\,(a_\ell \cos\ell\phi + b_\ell \sin\ell\phi) \tag{A.50}$$

Above, we have solved the vector wave equation in general. We can use the above analysis to describe the scattering of a plane wave by a homogeneous sphere. For simplicity we assume that the medium outside of the sphere has a refractive index of 1 (it is a vacuum), that the sphere has a refractive index of $m$ and that the incident radiation is linearly polarised. This allows us to chose a Cartesian coordinate system with its origin at the centre of the sphere, the positive $z$ axis along the direction of propagation of the incident plane wave. If one normalises the amplitude of the incident wave to 1, then the incident electric and magnetic field vectors are given by:

$$\mathbf{E}^i = \mathbf{a}_x e^{-ikz} \tag{A.51}$$

$$\mathbf{H}^i = \mathbf{a}_y e^{-ikz} \tag{A.52}$$

where $\mathbf{a}_x$ and $\mathbf{a}_y$ are unit vectors along the $x$ and $y$ axes respectively. We can transform the components of any vector in Cartesian coordinates, e.g. $\mathbf{a}$, to spherical coordinates. If $\mathbf{a}_r, \mathbf{a}_\theta, \mathbf{a}_\phi$ are the relevant unit vectors in spherical coordinates, then we can make use of the following geometric identifies to perform this transformation:

$$\mathbf{a}_r = \mathbf{a}_x \sin\theta\cos\phi + \mathbf{a}_y \sin\theta\sin\phi + \mathbf{a}_z \cos\theta \tag{A.53}$$

$$\mathbf{a}_\theta = \mathbf{a}_x \cos\theta\cos\phi + \mathbf{a}_y \cos\theta\sin\phi - \mathbf{a}_z \sin\theta \tag{A.54}$$

$$\mathbf{a}_\phi = -\mathbf{a}_x \sin\theta + \mathbf{a}_y \cos\phi \tag{A.55}$$

Thus the electric field vector can be expressed in spherical coordinates as follows:

$$E_r^i = e^{-ikr\cos\theta}\sin\theta\cos\phi \tag{A.56}$$

$$E_\theta^i = e^{-ikr\cos\theta}\cos\theta\cos\phi \tag{A.57}$$

$$E_\phi^i = -e^{-ikr\cos\theta}\sin\phi \tag{A.58}$$

The magnetic field vector can be ignored, as to derive the coefficients that will be presenting in the scattering functions only one of the components of either the electric or magnetic field vectors will be required. We will use the component $E_r^i$. Looking to (A.28), we can see that the first term is this component. Thus ($m = 1$):

$$E_r^i = e^{-ikr\cos\theta}\sin\theta\cos\phi = \frac{i}{k}\left[\frac{\partial^2\left(ru^i\right)}{\partial r^2} + k^2\left(ru^i\right)\right] \tag{A.59}$$

This allows us to find the potentials $u$ and $v$. To do so we make use of the following identities (A.61)-(A.63) and Bauer's formula (A.60) (Liou 2002, pg.

183):

$$e^{-ikr\cos\theta} = \sum_{n=0}^{\infty} (-i)^n (2n+1) \frac{\psi_n(kr)}{kr} P_n(\cos\theta) \tag{A.60}$$

$$e^{-ikr\cos\theta} \sin\theta = \frac{1}{ikr} \frac{\partial}{\partial\theta} \left(e^{-ikr\cos\theta}\right) \tag{A.61}$$

$$\frac{\partial}{\partial\theta} P_n(\cos\theta) = -P_n^1(\cos\theta) \tag{A.62}$$

$$P_0^1(\cos\theta) = 0 \tag{A.63}$$

In light of the above four equations we can write (A.56) as:

$$e^{-ikr\cos\theta} \sin\theta\cos\phi = \frac{1}{(kr)^{n-1}} \sum_{n=1}^{\infty} (-i)^2 (2n+1) \psi_n(kr) P_n^1(\cos\theta)\cos\phi \tag{A.64}$$

We take a trial solution of (A.59) by using an expanding series of a similar (mathematical) form:

$$ru^i = \frac{1}{k} \sum_{n=1}^{\infty} \alpha_n \psi_n(kr) P_n^1 \cos\phi \tag{A.65}$$

We can then substitute (A.65) and (A.64) into (A.59), and through a comparison of coefficients we obtain (A.66):

$$\alpha_n \left[k^2 \psi_n(kr) + \frac{\partial^2 \psi_n(kr)}{\partial r^2}\right] = (-i)^n (2n+1) \frac{\psi_n(kr)}{r^2} \tag{A.66}$$

In order to establish the coefficient $\alpha_n$ we must refer back to (A.47). Since we know that $\chi_n(kr)$ become infinite at the origin (through which the incident wave must pass), we can set the coefficients (which are arbitrary) as follows: $c_n = 1$ and $d_n = 0$ (such that the $\chi_n$ do not feature). It follows that:

$$\psi_n(kr) = rR \tag{A.67}$$

As (A.47) is the general solution of (A.35), (A.67) is as well, so long as $\alpha =$

$n(n+1)$ in (A.68):

$$\frac{d^2\psi_n}{dr^2} + \left(k^2 - \frac{\alpha}{r^2}\right)\psi_n = 0 \tag{A.68}$$

Thus, comparing (A.68) and (A.66) we find $\alpha_n$ to be

$$\alpha_n = (-i)^n \frac{2n+1}{n(n+1)} \tag{A.69}$$

Similar procedures gives $v^i$ (the potential $v$ for the incident plane wave) from (A.29). Thus for the incident waves *outside of the sphere*, hence superscript $^i$, we have the following:

$$ru^i = \frac{1}{k}\sum_{n=1}^{\infty}(-i)^n \frac{2n+1}{n(n+1)}\psi_n\left(kr\right)P_n^1\left(\cos\theta\right)\cos\phi \tag{A.70}$$

$$rv^i = \frac{1}{k}\sum_{n=1}^{\infty}(-i)^n \frac{2n+1}{n(n+1)}\psi_n\left(kr\right)P_n^1\left(\cos\theta\right)\sin\phi \tag{A.71}$$

In order to find the scattered wave potentials, we need to match $u^i$ and $v^i$ with the potentials for the scattered wave inside and outside of the sphere. We do this by expressing these potentials in a similar form, but with arbitrary coefficients. These potentials are derived from (A.50), which was explained above to be suitable to represent the scattered wave.

For internal scattered waves, i.e. transmitted waves hence superscript $^t$, as $\chi_n\left(mkr\right)$ are infinite at the origin, only the $\psi_n\left(mkr\right)$ feature:

$$ru^t = \frac{1}{mk}\sum_{n=1}^{\infty}(-i)^n \frac{2n+1}{n(n+1)}c_n\psi_n\left(mkr\right)P_n^1\left(\cos\theta\right)\cos\phi \tag{A.72}$$

$$rv^t = \frac{1}{mk}\sum_{n=1}^{\infty}(-i)^n \frac{2n+1}{n(n+1)}d_n\psi_n\left(mkr\right)P_n^1\left(\cos\theta\right)\sin\phi \tag{A.73}$$

However, for external scattered waves, hence superscript $^s$, the $\psi_n\left(mkr\right)$ functions must vanish at infinity. Thus we make use of the Hankel functions expressed in (A.49):

$$ru^s = \frac{1}{k}\sum_{n=1}^{\infty}(-i)^n \frac{2n+1}{n(n+1)} a_n \xi_n (kr) P_n^1 (\cos\theta) \cos\phi \tag{A.74}$$

$$rv^s = \frac{1}{k}\sum_{n=1}^{\infty}(-i)^n \frac{2n+1}{n(n+1)} b_n \xi_n (kr) P_n^1 (\cos\theta) \sin\phi \tag{A.75}$$

In order to establish what the coefficients $a_n, b_n, c_n, d_n$ are, we make use of the boundary conditions at the surface of the sphere: the components of **E** and **H** are continuous across the spherical surface $r = a$ such that:

$$E_\theta^i + E_\theta^s = E_\theta^t \tag{A.76}$$

$$E_\phi^i + E_\phi^s = E_\phi^t \tag{A.77}$$

$$H_\theta^i + H_\theta^s = H_\theta^t \tag{A.78}$$

$$H_\phi^i + H_\phi^s = H_\theta^t \tag{A.79}$$

Comparing (A.28) and (A.29) to (A.70)-(A.73), and making use of the boundary conditions above, we see that the expressions $v$ and $\frac{1}{m}\frac{\partial(ru)}{\partial r}$ for the electric field and $mu$ and $\frac{\partial(rv)}{\partial r}$ for the magnetic field are continuous at the boundary, $r = a$. Thus:

$$\frac{\partial}{\partial r}\left[r\left(u^i + v^i\right)\right] = \frac{1}{m}\frac{\partial}{\partial r}\left(ru^t\right) \tag{A.80}$$

$$u^i + u^s = mu^t \tag{A.81}$$

$$\frac{\partial}{\partial r}\left[r\left(v^i + v^s\right)\right] = \frac{\partial}{\partial r}\left(rv^t\right) \tag{A.82}$$

$$v^i + v^s = v^t \tag{A.83}$$

From (A.80)-(A.83) and (A.72)-(A.75) we have:

$$m\left[\psi_n'(ka) - a_n\xi_n'(ka)\right] = c_n\psi_n'(mka) \tag{A.84}$$

$$\left[\psi_n'(ka) - b_n\xi_n'(ka)\right] = d_n\psi_n'(mka) \tag{A.85}$$

$$\left[\psi_n(ka) - a_n\xi_n(ka)\right] = c_n\psi_n(mka) \tag{A.86}$$

$$m\left[\psi_n(ka) - b_n\xi_n(ka)\right] = d_n\psi_n(mka) \tag{A.87}$$

We can eliminate $c_n$ and $d_n$ to find $a_n$ and $b_n$ in terms of the size parameter $\beta = ka = \frac{2\pi}{\lambda}a$:

$$a_n = \frac{\psi_n'(m\beta)\psi_n(\beta) - m\psi_n(m\beta)\psi_n'(\beta)}{\psi_n'(m\beta)\xi_n(\beta) - m\psi_n(m\beta)\xi_n'(\beta)} \tag{A.88}$$

$$b_n = \frac{m\psi_n'(m\beta)\psi_n(\beta) - \psi_n(m\beta)\psi_n'(\beta)}{m\psi_n'(m\beta)\xi_n(\beta) - \psi_n(m\beta)\xi_n'(\beta)} \tag{A.89}$$

$$c_n = \frac{m\left[\psi_n'(\beta)\xi_n(\beta) - \psi_n(\beta)\xi_n'(\beta)\right]}{\psi_n'(m\beta)\xi_n(\beta) - m\psi_n(m\beta)\xi_n'(\beta)} \tag{A.90}$$

$$d_n = \frac{m\left[\psi_n'(\beta)\xi_n(\beta) - \psi_n(\beta)\xi_n'(\beta)\right]}{m\psi_n'(m\beta)\xi_n(\beta) - \psi_n(m\beta)\xi_n'(\beta)} \tag{A.91}$$

In all "practical applications", observations are made at the "far-field zone" (Liou 2002, pg. 186). This 'zone' allows the introduction of an approximation: the Hankel functions of (A.49) reduce to the form:

$$\xi_n(kr) \approx i^{n+1}e^{-ikr} \qquad kr \gg 1 \tag{A.92}$$

which results in the following approximations:

$$ru^s \approx -\frac{ie^{-ikr}\cos\phi}{k}\sum_{n=1}^{\infty}\frac{2n+1}{n\left(n+1\right)}a_n P_n^1\left(\cos\theta\right) \tag{A.93}$$

$$rv^s \approx -\frac{ie^{-ikr}\sin\phi}{k}\sum_{n=1}^{\infty}\frac{2n+1}{n\left(n+1\right)}b_n P_n^1\left(\cos\theta\right) \tag{A.94}$$

$$E_r^s = H_r^s \approx 0 \tag{A.95}$$

$$E_\theta^s = H_\phi^s \approx \frac{-i}{kr}e^{-ikr}\cos\phi\sum_{n=1}^{\infty}\frac{2n+1}{n\left(n+1\right)}\left[a_n\frac{dP_n^1\left(\cos\theta\right)}{d\theta} + b_n\frac{P_n^1\left(\cos\theta\right)}{\sin\theta}\right] \tag{A.96}$$

$$E_\phi^s = H_\theta^s \approx \frac{-i}{kr}e^{-ikr}\sin\phi\sum_{n=1}^{\infty}\frac{2n+1}{n\left(n+1\right)}\left[a_n\frac{P_n^1\left(\cos\theta\right)}{\sin\theta} + b_n\frac{dP_n^1\left(\cos\theta\right)}{d\theta}\right] \tag{A.97}$$

The radial components, $E_r^s$ and $H_r^s$ can be ignored in the far-field zone. We can simplify (A.96) and (A.97) through the introduction of two *scattering functions*:

$$S_1\left(\beta,\theta\right) = \sum_{n=1}^{\infty}\frac{2n+1}{n\left(n+1\right)}\left[a_n(\beta)\pi_n\left(\cos\theta\right) + b_n(\beta)\tau_n\left(\cos\theta\right)\right] \tag{A.98}$$

$$S_2\left(\beta,\theta\right) = \sum_{n=1}^{\infty}\frac{2n+1}{n\left(n+1\right)}\left[b_n(\beta)\pi_n\left(\cos\theta\right) + a_n(\beta)\tau_n\left(\cos\theta\right)\right] \tag{A.99}$$

where:

$$\pi_n\left(\cos\theta\right) = \frac{P_n^1\left(\cos\theta\right)}{\sin\theta} \tag{A.100}$$

$$\tau_n\left(\cos\theta\right) = \frac{dP_n^1\left(\cos\theta\right)}{d\theta} \tag{A.101}$$

Thus we may write the simplified versions of (A.96) and (A.97):

$$E_\theta^s = \frac{i}{kr}e^{-ikr}\cos\phi S_2\left(\theta\right) \tag{A.102}$$

$$-E_\phi^s = \frac{i}{kr}e^{-ikr}\sin\phi S_1\left(\theta\right) \tag{A.103}$$

We need to alter the notation of (A.88), (A.89), (A.100), (A.101) to match that used in the latter part of the derivation. The approach to doing this is taken from Grandy Jr (2005). To do this, we express (A.88), (A.89), (A.90) and (A.91) via new quantities:

$$P_n^e \equiv \psi_n(\beta)\psi_n'(m\beta) - m\psi_n(m\beta)\psi_n'(\beta) \tag{A.104}$$

$$Q_n^e \equiv \chi_n(\beta)\psi_n'(m\beta) - m\psi_n(m\beta)\chi_n'(\beta) \tag{A.105}$$

$$P_n^m \equiv \psi_n(m\beta)\psi_n'(m\beta) - m\psi_n'(m\beta)\psi_n'(\beta) \tag{A.106}$$

$$Q_n^m \equiv \chi_n'(\beta)\psi_n(m\beta) - m\psi_n'(m\beta)\chi_n(\beta) \tag{A.107}$$

where superscript $^e$ denotes the electric components and superscript $^m$ denotes the magnetic components. This allows us to express the coefficients as:

$$a_n = \frac{P_n^e}{P_n^e + iQ_n^e} \tag{A.108}$$

$$b_n = \frac{P_n^m}{P_n^m + iQ_n^m} \tag{A.109}$$

$$c_n = \frac{-im}{P_n^e + iQ_n^e} \tag{A.110}$$

$$d_n = \frac{im}{P_n^m + iQ_n^m} \tag{A.111}$$

Note that Grandy uses Hankel functions of the first type. The Hankel functions are complex conjugates of each other: $(\xi_n^{(1)}(z))^* = \xi_n^2(z)$. I believe this is the reason for the difference between the coefficients given in Grandy Jr. (e.g. his $b_n$ is equal to $-b_n$ from Liou: cf. equation (5.2.74) (Liou 2002, pg. 185) and equation (3.88b) (Grandy Jr 2005, pg. 87).) Grandy Jr. also makes use of the Wronskian of the $\psi_n(x)$ and $\xi_n(x)$ functions (A.113). This allows the nominators of $c_n$ and $d_n$ to be written in terms of $i$ and the complex refractive index, $m$:

$$W\{f(x), g(x)\} \equiv f(x)g'(x) - f'(x)g(x) \tag{A.112}$$

$$W\{\psi_n(z), \chi_n(z)\} = 1 \tag{A.113}$$

The Wronskian allows the numerator of $c_n$ to be transformed as follows:

$$m\left[\psi_n'(\beta)\xi_n(\beta) - \psi_n(\beta)\xi_n'(\beta)\right]$$

$$= m\left[\psi_n'(\beta)\left(\psi_n(\beta) + i\chi_n(\beta)\right) - \psi_n(\beta)\left(\psi_n'(\beta) + i\chi_n'(\beta)\right)\right]$$

Dropping the subscript $n$ and $(\beta)$, rearranging and then by (A.113)

$$= m\left[\psi\psi' + i\psi'\chi - \psi\psi' - i\psi\chi'\right]$$

$$= m\left[\psi\psi' - \psi\psi' + i\left(\psi'\chi - \psi\chi'\right)\right]$$

$$= m\left[-i\left(\psi\chi' - \psi'\chi\right)\right]$$

$$= m\left[-i\left(1\right)\right]$$

$$= -im$$

This notation allows one to gain "insight into the physical meaning of the coefficients" (Grandy Jr 2005, pg. 100). Taking $m$ to be real for a moment, one can define a real *phase shift* $\delta_n$ as:

$$\tan\delta_n^e \equiv \frac{P_n^e}{Q_n^e} \tag{A.114}$$

$$\tan\delta_n^m \equiv \frac{P_n^m}{Q_n^m} \tag{A.115}$$

so that the coefficients are equal to:

$$a_n = \frac{\tan \delta_n^e}{\tan \delta_n^e} = \frac{1}{2}\left(1 - e^{2i\delta_n^e}\right) \tag{A.116}$$

$$b_n = \frac{\tan \delta_n^m}{\tan \delta_n^m} = \frac{1}{2}\left(1 - e^{2i\delta_n^m}\right) \tag{A.117}$$

(A.116) can be expanded (and similarly for (A.117)):

$$a_n = \frac{P_n^{e2}}{P_n^{e2} + Q_n^{e2}} - i\frac{P_n^e Q_n^{2e}}{P_n^{e2} + Q_n^{2e}} \tag{A.118}$$

Grandy Jr. points us to scalar wave scattering and the expression for the partial scattering amplitude he derived:

$$f_n(k) = \frac{e^{-in\frac{\pi}{2}}}{2ik}\left[S_n\left(ka\right) - 1\right] \tag{A.119}$$

where $S_n\left(ka\right) \equiv e^{2i\delta_n}$ is the partial-wave 'scattering matrix' in terms of phase shifts. We can follow a similar path here by defining the matrix:

$$S_n \equiv \begin{pmatrix} S_n^E & 0 \\ 0 & S_n^M \end{pmatrix} \tag{A.120}$$

For real $m$, $S_n$ must be unitary (by conservation of energy). Thus $S_n^E$ and $S_n^M$ must be phase factors, and can only be those introduced above in (A.116) and (A.117):

$$a_n = \frac{1}{2}\left[1 - S_n^E\left(\beta\right)\right] \tag{A.121}$$

$$b_n = \frac{1}{2}\left[1 - S_n^M\left(\beta\right)\right] \tag{A.122}$$

Substituting into (A.88) and (A.89) allows us to express (A.121) and (A.122) as:

$$S_n^E(\beta) = -\frac{\zeta_n^2(\beta)}{\zeta_n^1(\beta)} \left( \frac{\ln' \zeta_n^2(\beta) - m^{-1} \ln' \psi_n(m\beta)}{\ln' \zeta_n^1(\beta) - m^{-1} \ln' \psi_n(m\beta)} \right) \tag{A.123}$$

$$S_n^M(\beta) = -\frac{\zeta_n^2(\beta)}{\zeta_n^1(\beta)} \left( \frac{\ln' \zeta_n^2(\beta) - m \ln' \psi_n(m\beta)}{\ln' \zeta_n^1(\beta) - m \ln' \psi_n(m\beta)} \right) \tag{A.124}$$

where we have changed to using $\zeta^{(1,2)}$ to denote the Riccati-Hankel functions of type 1 and 2 respectively, and we define the logarithmic derivative as $\ln f(x) = \frac{d \ln f(x)}{dx} = \frac{f'(x)}{f(x)}$. For convenience these two types of multipole can be combined into the single expression where $v_1 = 1$ and $v_2 = m^{-2}$, $j = 1, 2$:

$$S_n^j(\beta) = -\frac{\zeta_n^2(\beta)}{\zeta_n^1(\beta)} \left( \frac{\ln' \zeta_n^2(\beta) - m v_j \ln' \psi_n(m\beta)}{\ln' \zeta_n^1(\beta) - m v_j \ln' \psi_n(m\beta)} \right) \tag{A.125}$$

A further modification to the notation of (A.98) and (A.99) has to be made. The motivation behind this modification is to introduce angular functions, $p_n(\cos\theta)$ and $t_n(\cos\theta)$, that are "more useful for [analytic] extrapolation to complex indices" than those found in (A.98) and (A.99) (Grandy Jr 2005, pg. 133). These angular functions "contain only simple Legendre polynomials, remove the explicit appearance of numerical factors, and exclude values with $n = 0$". With $x = \cos\theta$ and for all $n \neq 0$ we define:

$$p_n(x) = \frac{[P_{n-1}(x) - P_{n+1}(x)]}{1 - x^2} = \frac{2n+1}{n(n+1)} \pi_n(x) \tag{A.126}$$

$$t_n(x) = -x p_n(x) + (2n+1) P_n(x) = \frac{2n+1}{n(n+1)} \tau_n(x) \tag{A.127}$$

Thus (A.125) can be written as follows:

$$S_j(\beta, \theta) = \frac{1}{2} \sum_{nS=1}^{\infty} \left\{ \left[ 1 - S_n^{(j)}(\beta) \right] t_n(\cos\theta) + \left[ 1 - S_n^{(i)}(\beta) \right] p_n(\cos\theta) \right\} \qquad (i, j = 1, 2 i \neq j) \tag{A.128}$$

This is the exact Mie solution for the scattering amplitudes.

The dimensionless scattering amplitude can be expressed as a partial-wave expansion in terms of the scattering amplitudes:

$$f(k,\theta) = \frac{1}{ika} \sum_{n=0}^{\infty} \left( n + \frac{1}{2} \right) [S_n - 1] P_n(\cos\theta) \tag{A.129}$$

## A.3 Debye Expansion[2]

The Debye expansion involves representing the multipole of index $n$ in the partial wave sums (A.123) - (A.125) in terms of incoming and outgoing spherical waves that are partially transmitted and partially reflected at the surface of the sphere (Grandy Jr 2005, pg. 144) (Adam 2002, pg. 283). We employ the Debye expansion as applying the Poisson sum formula, (A.155), to the Mie solution directly results in residue series that converge no quicker than Mie solution itself.

We start by introducing the radial wavefunctions for the interior region of the sphere, denoted as region 1, and the exterior region of the sphere, denoted as region 2. We can also introduce spherical reflection and transmission coefficients so that we can write the radial wavefunction in each region, for each partial wave, as:

$$\phi_{2,n}(r) = A \left( \frac{\zeta_n^{(2)}(kr)}{\zeta_n^{(1)}(\beta)} + R_{22} \frac{\zeta_n^{(1)}(kr)}{\zeta_n^{(1)}(\beta)} \right) \tag{A.130}$$

$$\phi_{1,n}(r) = A \left( T_{21} \frac{\zeta_n^{(2)}(mkr)}{\zeta_n^{(2)}(m\beta)} \right) \tag{A.131}$$

where $A$ is a normalisation constant. $\phi$ and its derivative, $\phi'$, must be continuous at the surface of the sphere, $r = a$. This results in the relation $T_{21} = 1 + R_{22}$ and gives us enough information for us to determine $R_{22}$ and $T_{21}$. The $R_{ij}$

[2]This derivation is summarised from (Grandy Jr 2005, pg. 144-153) and Nussenzveig (1969a,b).

are spherical reflection coefficients and the $T_{ij}$ are spherical transmission coefficients, where $i, j = 1, 2$, denoting the regions the spherical wave is reflecting from or transmitting to.

An outgoing spherical wave that interacts with the surface is described by:

$$\phi_{2,n}(r) = A\left(T_{12}\frac{\zeta_n^{(1)}(kr)}{\zeta_n^{(1)}(\beta)}\right) \tag{A.132}$$

$$\phi_{1,n}(r) = A\left(\frac{\zeta_n^{(1)}(nkr)}{\zeta_n^{(1)}(n\beta)} + R_{11}\frac{\zeta_n^{(2)}(nkr)}{\zeta_n^{(2)}(n\beta)}\right) \tag{A.133}$$

We can again match $\phi$ and its derivative $\phi'$ at the boundary $r = a$ to yield the relation $T_{12} = 1 + R_{11}$ and allows us to determine $T_{12}$ and $R_{11}$. Grandy Jr. gives these coefficients for the magnetic case, $j = 1$ with his equations (5.18a) - (5.18d) (Grandy Jr 2005, pg. 146).

It is also useful to see the algebraic derivation of these coefficients for both the electric and magnetic multipoles, from $S_n(\beta)$. We can rewrite (A.125) as (A.136) making use of the notation of Nussenzveig (1969a), with $j = 1, 2$:

$$[z] \equiv \ln' \psi_n(z) = \frac{\psi_n'(z)}{\psi_n(z)} \tag{A.134}$$

$$[jz] \equiv \frac{\zeta_n^{(j)\prime}(z)}{\zeta_n^{(j)}(z)} \tag{A.135}$$

$$S_n(\beta) = -\frac{\zeta_n^{(2)}(\beta)}{\zeta_n^{(1)}(\beta)}\frac{[2\beta] - mv_j\,[m\beta]}{[1\beta] - mv_j\,[n\beta]} \tag{A.136}$$

The second factor in (A.136) can be rewritten as:

$$\frac{[2\beta] - mv_j\,[m\beta]}{[1\beta] - mv_j\,[m\beta]} = 1 - \frac{[1\beta] - [2\beta]}{[1\beta] - mv_j\,[2m\beta]}\frac{[1\beta] - mv_j\,[2m\beta]}{[1\beta] - mv_j\,[m\beta]} \tag{A.137}$$

We can employ an identity (Grandy Jr.'s (5.11)) to transform the last factor of (A.137), and further algebraic manipulation to rewrite the right hand side of (A.137) as:

$$\frac{[2\beta] - mv_j [m\beta]}{[1\beta] - mv_j [m\beta]} = -\left( R_{22}(n,\beta) + \frac{T_{21}(n,\beta) T_{12}(n,\beta)}{1 - \rho(n,\beta)} \frac{\zeta_n^{(1)}(m\beta)}{\zeta_n^{(2)}(m\beta)} \right) \quad \text{(A.138)}$$

where:

$$\rho(n,\beta) \equiv -\frac{\zeta_n^{(1)}(m\beta)}{\zeta_n^{(2)}(m\beta)} \frac{[1\beta] - mv_j [1m\beta]}{[1\beta] - mv_j [2m\beta]} \quad \text{(A.139)}$$

(A.138) introduces the new functions for the spherical reflection and transmission coefficients:

$$R_{11}(n,\beta) \equiv \frac{\zeta_n^{(2)}(m\beta)}{\zeta_n^{(1)}(m\beta)} \rho(n,\beta) \quad \text{(A.140)}$$

$$R_{22}(n,\beta) \equiv -\frac{[2\beta] - mv_j [2m\beta]}{[1\beta] - mv_j [2m\beta]} \quad \text{(A.141)}$$

$$T_{12}(n,\beta) \equiv \frac{[1m\beta] - [2m\beta]}{[1\beta] - mv_j [2m\beta]} = 1 + R_{11} \quad \text{(A.142)}$$

$$T_{21}(n,\beta) \equiv \frac{[1\beta] - [2\beta]}{[1\beta] - mv_j [2m\beta]} = 1 + R_{22} \quad \text{(A.143)}$$

This allows us to rewrite (A.125) in terms of these coefficients:

$$S_n(\beta) = \frac{\zeta_n^{(2)}(\beta)}{\zeta_n^{(1)}(\beta)} \left( R_{22}(n,\beta) + \frac{\zeta_n^{(1)}(m\beta)}{\zeta_n^{(2)}(m\beta)} + \frac{T_{21}(n,\beta) T_{12}(n,\beta)}{1 - \rho(n,\beta)} \right) \quad \text{(A.144)}$$

We can introduce new forms of these coefficients by noticing that the ratios of the Hankel functions in (A.144):

$$R_n^{11}(\beta) \equiv \frac{\zeta_n^{(1)}(m\beta)}{\zeta_n^{(2)}(m\beta)} R_{11}(n\beta) \tag{A.145}$$

$$R_n^{22}(\beta) \equiv \frac{\zeta_n^{(2)}(\beta)}{\zeta_n^{(1)}(\beta)} R_{22}(n\beta) \tag{A.146}$$

$$T_n^{12}(\beta) \equiv \frac{\zeta_n^{(1)}(m\beta)}{\zeta_n^{(1)}(\beta)} T_{12}(n\beta) \tag{A.147}$$

$$T_n^{21}(\beta) \equiv \frac{\zeta_n^{(2)}(\beta)}{\zeta_n^{(2)}(m\beta)} T_{21}(n\beta) \tag{A.148}$$

which allows (A.144), the partial wave coefficients and the internal coefficients to be written as follows:

$$S_n(\beta) = R_n^{22} + \frac{T_n^{21} T_n^{12}}{1 - R_n^{11}} \tag{A.149}$$

$$\begin{Bmatrix} a_n \\ b_n \end{Bmatrix} = \frac{1}{2}\left(1 - R_n^{22} - \frac{T_n^{21} T_n^{12}}{1 - R_n^{11}}\right) \tag{A.150}$$

$$c_n = \frac{T^{21}}{1 - R_n^{11}} \tag{A.151}$$

$$d_n = \frac{T^{12}}{1 - R_n^{11}} \tag{A.152}$$

We can expand the denominator of (A.149) to obtain the *Debye expansion* of the amplitude functions, $S_n^j$. This can be expressed in terms of the spherical coefficients as in (A.153), or in terms of the scattering functions as in (A.154):

$$S_n^j = R_n^{22} + T_n^{21} \sum_{p=1}^{\infty} \left(R_n^{11}\right)^{p-1} T_n^{12} \tag{A.153}$$

$$S_j(\beta, \theta) = S_{j,0}(\beta, 0) + \sum_{p=1}^{P} S_{j,p}(\beta, \theta) + \text{remainder} \tag{A.154}$$

The first term in each case, $R_n^{22}$ and $S_{j,0}$, is associated with direct reflection from the surface and the $p^{th}$ term is associated with transmission after $(p-1)$

internal reflections at the surface.

## A.4 Poisson Sum

The Poisson sum formula is given in (A.155). This allows for an infinite sum over a real function to be transformed into an integral over complex variables.

$$\sum_{n=0}^{\infty} \psi(\Lambda, \mathbf{r}) = \sum_{m=-\infty}^{\infty} (-1)^m \int_0^{\infty} \psi(\Lambda, \mathbf{r})\, e^{2im\pi\Lambda}\, \mathrm{d}\Lambda \tag{A.155}$$

where $\psi$ is a certain function, $\Lambda = n + \frac{1}{2}$, $n$ is the order of expansion of the scattering functions, and $\mathbf{r}$ is a position vector.

## A.5 Unified CAM Approach

Applying the modified Watson transform to the third term of the Debye expansion allows us to obtain a contour integral. We extend to the complex plane so that we can deform the path to make the integration easier. In doing so, we are able to "concentrate dominant contributions to the integrals into the neighborhood of a small number of points in the $\lambda$ plane, that will be referred to as *critical points*. The main type of critical points are *saddle points*, that can be real or complex, and *complex poles* such as the Regge poles" (Nussenzveig 1992, pg. 62). In this section I will set out the mathematics of the Watson transformed third Debye term and the mathematics of the Chester-Friedman-Ursell (CFU) method for resolving the collision between two saddle points. The concept of poles is outlined in the final section.

### A.5.1 Third Debye Term[3]

Based on Nussenzveig's work, Adam offers a version of the Debye expansion for the total scattering function:

$$f(\beta,\theta) = f_0(\beta,\theta) + \sum_{p=1}^{\infty} f_p(\beta,\theta) \tag{A.156}$$

$$f_p(\beta,\theta) = -\frac{i}{\beta}\sum_{n=-\infty}^{\infty}(-1)^n \int_0^{\infty} U(\lambda,\beta)\left[\gamma(\lambda,\beta)\right]^{p-1} P_{\lambda-\frac{1}{2}}(\cos\theta)\, e^{2in\pi\lambda}\lambda\, \mathrm{d}\lambda \tag{A.157}$$

where $f_p$ is for $p \geq 1$. $U(\lambda,\beta)$ and $\gamma(\lambda,\beta)$ in $f_p$ are given in (A.158) and (A.159) respectively.[4]

$$U(\lambda,\beta) = T_{21}(\lambda,\beta)\frac{\zeta_\lambda^{(1)}(\alpha)\,\zeta_\lambda^{(2)}(\beta)}{\zeta_\lambda^{(2)}(\alpha)\,\zeta_\lambda^{(1)}(\beta)}T_{12}(\lambda,\beta) = U(-\lambda,\beta) \tag{A.158}$$

$$\gamma_j = \left[\frac{\zeta_n^{(1)}(\alpha)}{\zeta_n^{(2)}(\alpha)}\right] R_{11}^j \tag{A.159}$$

From applying the modified Watson transformation to each individual term of the Debye expansion, we can write the third term as:

$$f_2(\beta,\theta) = -\frac{i}{\beta}\sum_{m=-\infty}^{\infty}(-1)^m \int_0^{\infty} \gamma(\lambda,\beta)\, U(\lambda,\beta)\, P_{\lambda-\frac{1}{2}} e^{2im\pi\lambda}\lambda\, \mathrm{d}\lambda \tag{A.160}$$

After rearrangement and a shift in the path of integration to above the real axis, (A.160) can be written as:

$$f_2(\beta,\theta) = -\frac{1}{2\beta}\int_{-\infty-i\epsilon}^{\infty-i\epsilon} \gamma U P_{\lambda-\frac{1}{2}}(\cos\theta)\, e^{i\pi\lambda}\frac{\lambda}{\cos\pi\lambda}\, \mathrm{d}\lambda \tag{A.161}$$

[3] The derivation here is summarised from Adam (2002), who bases his work on Nussenzveig (1969b).

[4] The spherical coefficients should be the same between Adam (2002), Grandy Jr (2005) and Nussenzveig (1992); any difference will be due to a difference in notation or rearranging of terms.

for $\epsilon > 0$, due to the function being odd. This integral can be decomposed into (A.162), where $f^{\pm}_{2,0}(\beta,\theta)$ is given in (A.163) and $f^{\pm}_{2,r}(\beta,\theta)$ is given in (A.164).

$$f_2(\beta,\theta) = f^{+}_{2,0} + f_{2,r} = f^{-}_{2,0} - f_{2,r} \tag{A.162}$$

$$f^{\pm}_{2,0}(\beta,\theta) = \pm\frac{i}{\beta}\int_{-\infty\pm i\epsilon}^{\infty\mp i\epsilon} \gamma U \mathcal{Q}^{(2)}_{\lambda-\frac{1}{2}}(\cos\theta)\,\mathrm{d}\lambda \tag{A.163}$$

$$f_{2,r}(\beta,\theta) = \frac{1}{2\beta}\int_{-\infty-i\epsilon}^{\infty+i\epsilon} \gamma U e^{2i\pi\lambda}\frac{\lambda}{\cos\pi\lambda}\mathrm{d}\lambda \tag{A.164}$$

Above, $\mathcal{Q}^{(2)}_{\lambda-\frac{1}{2}}$ is a Legendre function of the second kind. It can be shown that there will always be some neighbourhood of the imaginary axis where the integrand in (A.163) will diverge to infinity. This is true for any value of $\theta$, and so there is no domain of $\theta$-values for which $f^{\pm}_{2,0}$ and hence $f_2(\beta,\theta)$ can be reduced to a pure residue series. i.e. We need to use the saddle points.

### A.5.2 CFU Method for Saddle Points

The rainbow is described as the collision between two saddle points. The typical approach to finding the contribution to an integral from saddle points is the method of 'steepest descent'. This method fails, however, when the range of two saddle points overlap. de Bruijin gives an informal definition of the range of saddle points: "if $\zeta$ is a saddle point of the function $\psi$, then the range of $\zeta$ is a circular neighbourhood of $\zeta$, consisting of all $z$-values which are such that $\left|\psi''(\zeta)(z-\zeta)^2\right|$ is not very large" (de Bruijn 1958, pg. 91).

The principle behind the CFU method is to alter the exponent involved in a general complex integral from a Gaussian type to an exact cubic.

#### Steepest Descent

Consider the general complex integral (A.165). The integral is over some path in the $w$ plane, where $\kappa$ is an asymptotic expansion parameter and is large and positive, and $\varepsilon$ is an independent parameter. If this integral is dominated by a single saddle point $\bar{w} = \bar{w}(\varepsilon)$, around which $f$ and $g$ are regular, one can approximate $F(\kappa, \varepsilon)$ as $f(w, \varepsilon)$ in this neighbourhood, where $f''_w$ denotes the second derivative with respect to $w$.

$$F(\kappa, \varepsilon) = \int g(w) e^{[\kappa f(w,\varepsilon)]} \, \mathrm{d}w \tag{A.165}$$

$$f(w, \varepsilon) \approx f(\bar{w}, \varepsilon) + \frac{1}{2} f''_w(\bar{w}, \varepsilon)(w - \bar{w})^2 \tag{A.166}$$

Choosing the steepest descent through the saddle point $\bar{w}$ alters the integral to a Gaussian type. A change of variables allows us to transform the exponent into an act Gaussian. This allows an asymptotic expansion of the integral to be found by adopting a power series expansion of $g$ around $\bar{w}$ and integrating term by term.

The method of steepest descent allows for the contribution of saddle points to be established. Provided that the range of two saddle points do not overlap, then their contributions can be added independently. If their ranges *do* overlap, then the method of steepest descent fails and we require the CFU method. This overlap occurs in the rainbow region.

**CFU Method: In General**

The basic idea behind the CFU method is to transform the exponent of (A.166) into an exact cubic through a change of variables, e.g. (A.167). This transforms the two saddle points, $\bar{w}'$ and $\bar{w}''$ into $\pm\zeta^{\frac{1}{2}}(\varepsilon)$. $f(w, \varepsilon)$ is then substituted back into (A.165), allowing for a suitable expansion of the integrand to obtain a suitable equation for $F(\kappa, \varepsilon)$.

$$f(w, \varepsilon) = \frac{1}{3} u^3 - \zeta(\varepsilon) u + A(\varepsilon) \tag{A.167}$$

**CFU Method: The Rainbow**

The CFU method applied to the rainbow requires an adjusted form of (A.160):

$$\tilde{f}_{2,g}(\beta,\theta) = -\frac{i}{\beta}\int_{-\sigma_1\infty}^{\sigma_2\infty} \rho U \mathcal{Q}^{(2)}_{\lambda-\frac{1}{2}}(\cos\theta)\,\lambda\,\mathrm{d}\lambda \tag{A.168}$$

where:

$$\mathcal{Q}^{(2)}_{\lambda-\frac{1}{2}} = e^{2i\pi\lambda}\mathcal{Q}^{(1)}_{\lambda-\frac{1}{2}}(\cos\theta) - ie^{i\pi\lambda}P_{\lambda-\frac{1}{2}}(-\cos\theta) \tag{A.169}$$

Here the path of integration from (A.163) has been deformed into the path of deepest descent, denoted by the change in limits of the integral to $(-\sigma_1\infty, \sigma_2\infty)$. This entails moving across the poles. This integral may be rewritten as:

$$f_{2,g} = 2e^{\frac{-i\pi}{4}}N\left(\frac{2\beta}{\pi\sin\theta}\right)^{\frac{1}{2}}F(\beta,\theta) \tag{A.170}$$

$$F(\beta,\theta) = \int_{-\sigma_1'\infty}^{\sigma_2'\infty} g(w_1)e^{2\beta f(w_1,\theta)}\,\mathrm{d}w_1 \tag{A.171}$$

$$f(w_1,\theta) = i\left[2N\cos w_2 - \cos w_1 + \left(2w_2 - w_1 - \frac{\pi-\theta}{2}\right)\sin w_1\right] \tag{A.172}$$

$$g(w_1) = (\sin w_1)^{\frac{1}{2}}\cos^2 w_1\cos w_2\left[\frac{\cos w_1 - N\cos w_2}{(\cos w_1 + N\cos w_2)^3}\right]\left[1+\mathcal{O}(\beta^{-1})\right] \tag{A.173}$$

Above, equations (A.165) and (A.166) set out the general form of the equations needed for the method of steepest descent. The specific versions of these equations for the rainbow are equations (A.171) and (A.172) respectively.

The rest of the mathematics are too complex to include here. Further details can be found in Nussenzveig (1969b) and Adam (2002). Nussenzveig (1992) provides a summary of the derivation, which finished with equation (A.174). This equation provides the form of dominant contribution to the third Debye term in the rainbow region:

$$S_{j2}(\beta,\theta) \approx \beta^{\frac{7}{6}} e^{[2\beta A(\varepsilon)]} \Big\{ \left[ c_{j0}(\varepsilon) + \beta^{-1} c_{j1}(\varepsilon) + \dots \right] Ai \left[ (2\beta)^{\frac{2}{3}} \zeta(\varepsilon) \right] + \left[ d_{j0}(\varepsilon) + \beta^{-1} d_{j1}(\varepsilon) + \dots \right] \beta^{-\frac{1}{3}} Ai' \left[ (2\beta)^{\frac{2}{3}} \zeta(\varepsilon) \right] \Big\} \quad (A.174)$$

## A.6 Poles[5]

[A function] is analytic in a region of the complex plane if it has a (unique) derivative at every point in the region. The statement '$f(z)$ is analytic *at a point* $z = a$' means that $f(z)$ has a derivative at every point inside some small circle about $z = a$.

A *regular point* of $f(z)$ is a point at which $f(z)$ is analytic. A *singular point* or *singularity* of $f(z)$ is a point at which $f(z)$ is not analytic. It is called an *isolated* singular point if $f(z)$ is analytic everywhere else inside some small circle about the singular point.

Laurent's Theorem:

> Let $C_1$ and $C_2$ be two circles with centre at $z_0$. Let $f(z)$ be analytic in the region $R$ between the circles. Then $f(z)$ can be expanded in a series of the form
>
> $$f(z) = a_0 + a_1(z - z_0) + a_2(z - z_0)^2 + \dots + \frac{b_1}{z - z_0} + \frac{b_2}{(z - z_0)^2} + \dots \quad (A.175)$$
>
> convergent on $R$. Such a series is called a *Laurent series.* The '$b$' series in (A.175) is called the *principle part* of the Laurent series.

If all the $b$'s are zero, $f(z)$ is analytic at $z = z_0$, and we call $z_0$ a *regular point.* If $b_n \neq 0$, but all the $b$'s after $b_n$ are zero, $f(z)$ is said to have a *pole of order* $n$ at $z = z_0$. If $n = 1$, we say that $f(z)$ has a *simple pole.* If there are an

[5] This entire section is reproduced directly from Boas (2005), pg. 667-670, and pg. 680.

infinite number of $b$'s different from zero, $f(z)$ has an *essential singularity* at $z = z_0$. The coefficient $b_1$ of $\frac{1}{z-z_0}$ is called the *residue* of $f(z)$ at $z = z_0$.

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