Mathematical Representation and Explanation: Structuralism, the Similarity Account and the Hotchpotch Picture

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Submitted in accordance with the requirements for the degree of Master of Arts by Research

The University of Leeds

The School of Philosophy, Religion and History of Science

September 2020

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Acknowledgements

I would like to specially thank my supervisor Dr. Juha Saatsi for his dedicated contribution and inspiring discussion on this thesis and the philosophical issues it addresses. This year was not easy to me, in life and academically. Juha gave me lots of helps and suggestions in supervision meetings. Without him, this thesis is impossible.

I would also thank my friend Nicholas Lanctôt for his editorial suggestions. He indicated errors in the grammar, word choice, and writing style in earlier versions of this thesis.

Abstract

This thesis starts with three challenges to the structuralist accounts of applied mathematics. Structuralism views applied mathematics as a matter of building mapping functions between mathematical and target-ended structures. The first challenge concerns how it is possible for a non-mathematical target to be represented mathematically when the mapping functions *per se* are mathematical objects. The second challenge arises out of inconsistent early calculus, which suggests that mathematical representation does not require rigorous mathematical structures. The third challenge comes from renormalisation group (RG) explanations of universality. It is argued that the structural mapping between the world and a highly abstract minimal model adds little value to our understanding of how RG obtains its explanatory force.

I will address the first and second challenges from the similarity perspective. The similarity account captures representations as similarity relations, providing a more flexible and broader conception of representation than structuralism. It is the specification of the respect and degree of similarity that forges mathematics into a context of representation and directs it to represent a specific system in reality. Structuralism is treatable as a tool for explicating similarity relations set-theoretically. The similarity account, combined with other approaches (e.g., Nguyen and Frigg's extensional abstraction account and van Fraassen's pragmatic equivalence), can dissolve the first challenge. Additionally, I will make a structuralist response to the second challenge, and suggestions regarding the role of infinitesimals from the similarity perspective.

In light of the similarity account, I will propose the "hotchpotch picture" as a methodological reflection of our study of representation and explanation. Its central insight is to dissect a representation or an explanation into several aspects and use different theories (that are usually thought of competing) to appropriate each of them.

Based on the hotchpotch picture, RG explanations can be dissected to the "indexing" and "inferential" conceptions of explanation, which are captured or characterised by structural mappings. Therefore, structuralism accommodates RG explanations, and the third challenge is resolved.

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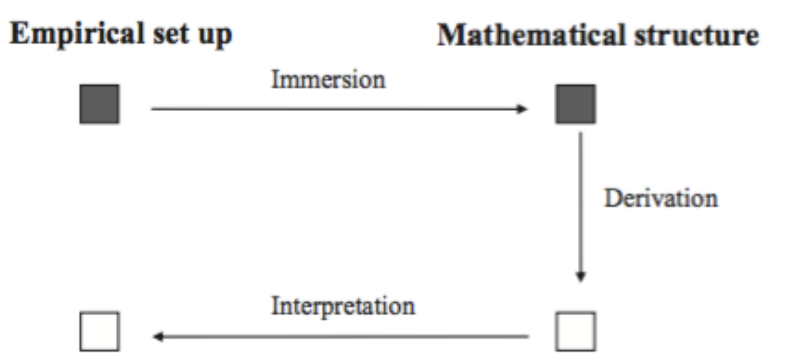

Fig 2a. The Inferential Conception of Applied Mathematics

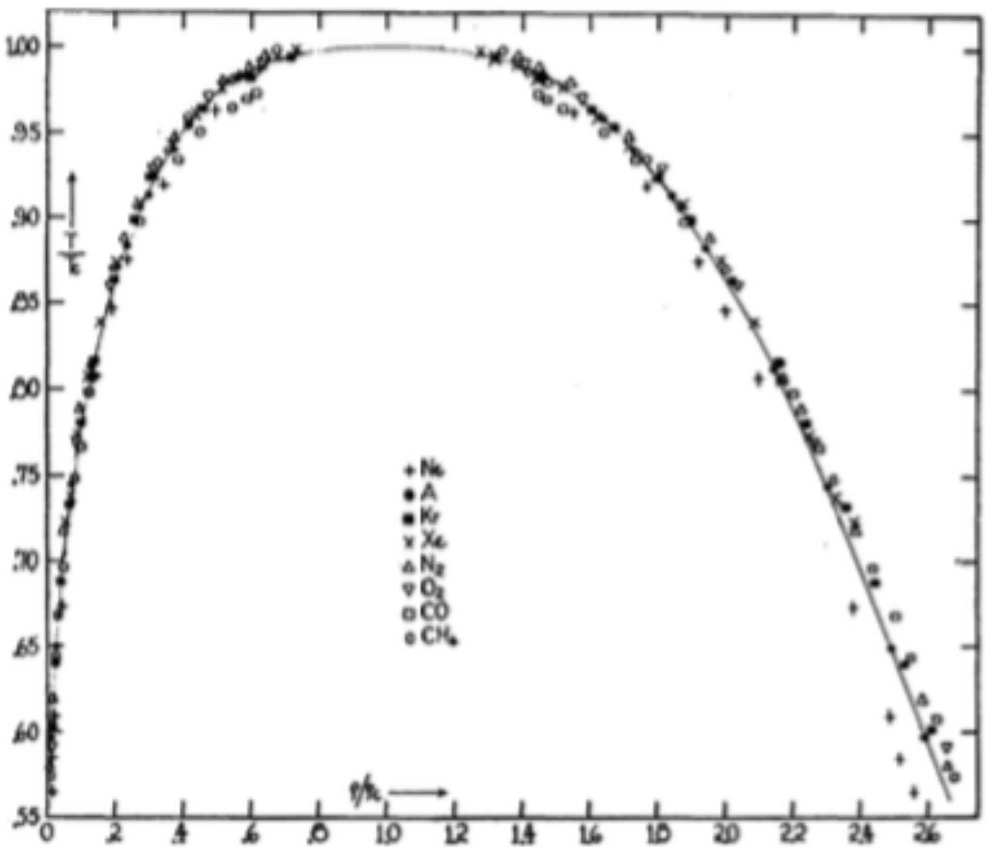

Fig. 4a Universality of Critical Phenomena

Phase Transition	Value of β
Mean Field Theory	1/2
$d = 2$ Ising Model Ferromagnet	1/8
$d = 3$ Liquid/Vapor CO_2, Xe	0.35
$d = 3$ Superfluid Helium ^{4}He	0.359
$d = 3$ Ising Model Ferromagnet	0.315

Fig. 4b Scaling Exponents for Different Transitions

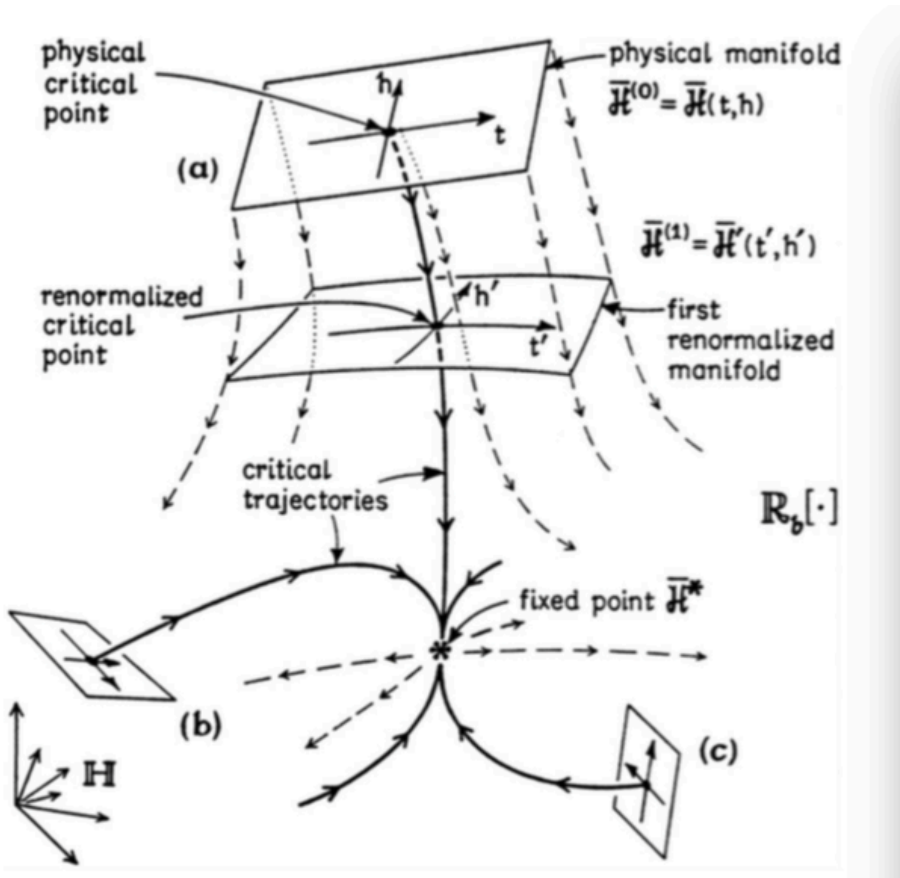

Fig. 4c RG flows to a Fixed Point

Chapter One
Introduction

This thesis centres on two questions regarding the applicability of mathematics:

(1) How does mathematics represent the world?

(2) How does mathematics contribute to scientific explanations?

To answer them, we can consult three mainstream theories of applied mathematics, which are linked under the heading of structuralism. The three theories are: the mapping account (Pincock 2012), the partial structure variant (Bueno & French 2018), and the inferential variant (Bueno & Colyvan 2011). The reason I use the word "variant" for the latter two is that they develop from the mapping account.

The mapping account is based on the central idea of structuralism. According to it, applied mathematics should be captured as a matter of a structural mapping between the mathematical structure and the target-ended structure (Pincock 2012). Interestingly, the notions "structure" and "mapping" are all understood as mathematical objects, or at least, are characterised with the set-theoretical language. The structure is defined as a composite of a family of objects and a family of relations that the objects bear. The structural mapping is defined as an isomorphism or a homomorphism between the structures in the mathematics and the target system.

Unsurprisingly, there has been many hostilities to this philosophical account of applied mathematics. Most challenges draw on the practice of idealisation and abstraction in science, doubting that the holding of a structural relationship can be sufficient and necessary for a mathematical representation.[1] Alternative accounts do not reduce the representation to another relation as structuralists do. Rather, the concept of representation is associated with its uses and is characterised in terms of its functions in practice of modelling.[2]

The partial structure and the inferential variants aim to accommodate idealisations and

[1] The literature on this topic is large: see Frigg 2006 and Suárez 2003 for detailed summaries. Also, note that the discussion often conflates the issue of model/theory representations to the structuralist semantic view with that of mathematical representations to the structuralist accounts that I discuss in this thesis. Despite some conceptual difference between the structuralist view and the semantic view (emphasised by Suárez & Pero 2019), I believe it is fine to accept this conflation, for mathematical representations rely on model-building and most models applied are occupied by mathematical structures.

[2] See, for example, Suárez 2004, Giere 2010, Hughes 1997, and Contessa 2007.

abstractions, as well as the practical aspect of representations. The former's strategy is to divide the mathematical model and the corresponding target system into different blocks of relations, isolating the part of structure that correctly represents from the idealised and abstracted parts. Call such a set-theoretical construct a "partial structure." The corresponding "partial isomorphism" or "partial homomorphism" can be constructed between the partial structures in the mathematics and the target. A mathematical application can be captured as a matter of making an inference based on the partial structural mapping between the partial structures. Other practical aspects, e.g., the role of interpretation or intentionality in scientific representations, can be accommodated in the inferential variant of the structuralist conception of applied mathematics.

I will address three challenges to the foregoing structuralist accounts, developing a more flexible and broader similarity conception of mathematical representation and explanation. I will also offer some methodological reflections on our studies of representation and explanation. To these ends, I will advocate a "hotchpotch" picture to rethink the relationship among various accounts that are thought of as competitors, making use of them to appreciate different aspects of a representation or an explanation, and assembling these aspects to an account of the representation or explanation.

I will now summarise the three challenges and my responses to them.

The first challenge. This is about the gap between the world and mathematics in scientific representation – if mathematics represents a target system through a structural mapping (that by itself is a mathematical entity), then how is it possible for the non-mathematical target to be represented *mathematically*? This is coined the "bridging problem" by Contessa (2010) in his commentary on van Fraassen's (2008) empiricist revision of the structuralist approach.

There have been many attempts to solve or dissolve this challenge. For example, Tegmark (2008) treats the world as fundamentally mathematical, in which case it is natural to see a mapping function straddling functions and physical entities. Van Fraassen (2008) attempts to solve this issue in the pragmatic context of using models to represent the world – urging that there is no pragmatic difference between mathematics accurately representing a target system and the data extracted from it. Pincock (2012) treats the representational relationship as an instantiation of a respect of the world in a mathematical structure. Nguyen and Frigg (2017) claim that mathematics is not mapped to the structure in the world, but a structure generated from a story of the world.

However, each approach has its shortcomings. Tegmark says nothing about applications in an everyday, non-fundamental level (Nguyen & Frigg 2017). Van Fraassen's strategy appears to commit an agent applying mathematics to believe that the target is identical with what it is represented as in the data model (Nguyen 2016; Contessa 2010). Pincock's response is threatened by Newman's objection that structuralists say nothing about why a specific structure is selected from the target to be mapped to the mathematical entity (van Fraassen 2008; Nguyen & Frigg 2017), and Nguyen and Frigg's strategy avoids answering the challenge itself – how a mathematical representation can be directed to the world *itself*, instead of just being a descriptive proxy of it.

My own response will appeal to the similarity account proposed by Giere (1999, 1988, 2004) and Teller (2001). For me, the central insight of the similarity account is not to reduce a representational relationship into a similarity relation, as Suárez (2010) summarises. Rather, the value of this account is to offer a pragmatic framework for *forging* mathematics (or a mathematical model) *into* a context of a representation and establishing a standard of representational accuracy without presupposing a general account of mathematical representations. I see the notion of similarity as a "glue" to interconnect elements of mathematical objects, idealisations, approximating techniques, illustrations of background theories, forging them into a context of representation, in which the mathematical objects are involved and are directed to represent their targets.

In this way, one need not treat the various accounts of representations as competing pairs. The crux is to treat these accounts as capturing of distinct aspects of representations and assembling them into an overarching account of a specific representation. I call this methodological style of studying *X* as holding a "hotchpotch" picture of *X*.[3] The similarity account,

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[3] Chakravartty (2010) also emphasises that the accounts of scientific representations can be analysed into two camps, which appreciate the informational, and pragmatic, aspects of scientific representations. Bueno and Colyvan (2011) holds a similar

drawing on other attempts (e.g., Nguyen and Frigg's and van Fraassen's), can resolve the bridging problem.

The second challenge. Based on the practice of early calculus, McCullough-Benner (2019) argues that applying infinitesimal-based algorithms to obtain accurate physical representations does not need consistent mathematical structures. The mapping account fails to accommodate this non-rigorous application, since the use of infinitesimals in a single algorithm is inconsistent when being stated in a propositional form. McCullough-Benner also urges that even if the partial structure variant can accommodate the inconsistency here, it does not give a satisfactory explanation of how mathematics places constraints on physical representations, since a single representation can be obtained with more than one partial structure. This motivates him to argue that inferences are explanatorily prior in developing an account of mathematical application. He proposes the "robustly inferential account" – according to which mathematics provides a privileged collection of inference patterns, by which a target system must perform in a way that makes the mathematical inferences valid.

I will argue that this robustly inferential account is superfluous, for it does not explain why a specific inference pattern is picked up for producing physical representations. Given this, the robustly inferential account does not look better than the partial structure variant. Rather, I suggest that the structural similarity between arithmetic operations and geometric properties motivates and explains how infinitesimals are used in a specific way to form an algorithm. The structuralist accounts give a more perspicuous explanation than the robustly inferential account as to how mathematics constrains physical representations.

The third challenge. Batterman (2010) argues that structuralism fails to accommodate certain mathematical operations and their role in renormalisation group (RG) explanations of universality phenomena. The universality, here, refers to the striking fact that a large class of microscopically distinct systems share the same behaviour at the macroscale. To explain this, scientists employ a mathematical technique called "renormalisation group" that washes out

picture with mine – the inferential picture brings pragmatic aspects of representations into a partial structural mapping between the empirical set up and the mathematical entity. Nonetheless, the similarity account provides a hotchpotch picture of how an empirical set-up is formed – how mathematics is forged into a context of representation – and it is deeper than the inferential picture.

micro-details of each system and transforms all scale-invariant behaviours from a microscale to a macroscale. During this transformation, all systems are attracted to the same topological structure called the "fixed point." This explains the universality fact at issue.

The issue is that to cash out this RG transformation, one must idealise the target system as one with infinite degrees of freedom or an infinite number of particles. There is no physical analogue to this mathematical singularity; thus, the mapping account fails. In addition, it is unclear what insight structuralism can provide to characterise the explanatory power of RG operations, since the explanatory force does not come from the representational goodness – the structural mapping between the RG operations (or minimal models employed to cash out the operations) and target systems (Batterman 2010; Batterman & Rice 2014).

Many reactions have been raised to Batterman's challenges.[4] I take Bueno and French's (2012) response as a starting point. Their response is that even if RG indeed is explanatory, the structural accounts – theories of mathematical representations – need not provide any account of it, but only provide a framework for accommodating it. To satisfy their response, one must figure out what it means to say that structuralism provides an accommodating framework, as well as how the RG transformation obtains its explanatory force.

I appeal to the hotchpotch picture, in order to offer a more suitable model to characterise RG explanations. Roughly, an RG analysis is a multistage activity with identification, inference and justification. The explanatory force of RG can be dissected into two aspects – each of which is appropriated by a distinct conception of explanation. A conception of explanation, here, refers to a way of how mathematics contributes to scientific explanations. As a result. an RG explanation is dissected into the indexing and the inferential conceptions, which can be captured and characterised by a mapping function, respectively.

The following three chapters will examine in detail these challenges and responses. I will conclude this thesis by identifying shortcomings of my analysis and making suggestions for future research.

[4] Bueno & French 2012, Saatsi & Reutlinger 2018, Lange 2015, Reutlinger 2017, and Povich 2018.

Part I. Representation

Chapter Two
The Bridging Problem: Structuralism and Similarity

1. Introduction

This chapter aims to solve the 'bridging problem' facing a structuralist account of applied mathematics. Structuralism argues that mathematics applies to a target in virtue of a mapping function between a mathematical structure and the system (Pincock 2012; Bueno & Colyvan 2011; Bueno & French 2018). For instance, natural numbers and their properties guide our counting practice, and manifolds apply to the curvature of space-time. The bridging problem says that if the target of mathematical representation is not mathematical, then the mapping function is impossible between the target and the mathematical object (van Fraassen 2008; Nguyen & Frigg 2017). There is still a gap between the world and mathematics.

There have been four attempts to dissolve this issue, summarised and proposed by Nguyen and Frigg (2017):

(1) The world is by nature mathematical. There is nothing mysterious about how a mapping function connects the world and mathematics (Tegmark 2008).

(2) Mathematics represents the world through an instantiation relation. The mathematical structure is instantiated by the world (Pincock 2012).

(3) Mathematics represents the world indirectly by data models, and there is no pragmatic difference between mathematics accurately representing the world and the data model[5] extracted from it (van Fraassen 2008).

(4) Mathematics represents the world indirectly by a "structure-generating description:" a mathematical structure applies to the structure abstracted from the physical description of the world (Nguyen & Frigg 2017).

However, these four attempts have their own weakness. Concerning (1): as Nguyen and Frigg (2016) indicate, although the world, at a fundamental level, is mathematical, it is unclear why

[5] Data refer to data points. A data model includes both data points and a stable pattern among them. I will use these two words interchangeably. The ambiguous use is benign as I only concern their abstract and mathematical metaphysical status.

this works for mathematical applications at the everyday level. For example, it is unnatural to see a rabbit system, which consists of rabbit individuals, as a mathematical structure.

Concerning (2): although it looks the most natural and promising among the stated options, Nguyen and Frigg (2017) complain about its "incompleteness." Indeed, (2) does not explain why we specify one structure, instead of another, to be mapped to a mathematical structure. To take the methane molecule as an example, we can either select atoms as individuals and bonds between them as relations or select the bonds as individuals and atoms as relations. (2) says nothing about which structure should be selected.

Concerning (3): Nguyen (2016) and Contessa (2010) argue that the pragmatic equivalence between representing the world and data extracted from it will commit those engaged in applied mathematics to the false identity relation between the world and the way it is represented.

Concerning (4): Nguyen and Frigg's proposal provides an incomplete picture: mathematics only applies to "a sea of stories" about the world, rather than the world itself, and neither is it clear about how a standard of accuracy regarding mathematical representations of the world can be achieved when a structural mapping only applies to the structure generated from a description of the world, nor does it provide an account for representational accuracy regarding the description.

In this chapter, I will not focus on (1) for the reason given above. I will also not focus on (2), since – as I will argue in section 2 – the instantiation relation is rarely exact. Thus, in what follows, I will be concerned with (3) and (4).

My primary thesis is that a representational relationship is fundamentally a matter of the holding of a similarity relation. Structuralism is a tool for representing this similarity in a set-theoretical form. To capture the central insight of structuralism – that mathematics represents the world by carving structural information from it – need not define a representation as a structural mapping, but specifies the respect and degree of similarity in question. This suggests that the similarity account provides a broader conception of mathematical representations, and in light of this broader conception, I will also sketch the "hotchpotch" picture for our study of mathematical scientific representations.

My secondary thesis is that the similarity account, combined with either (3) or (4), can dissolve the bridging problem. A similarity relation between the world and data extracted from it can enable a pragmatic equivalence: the specification of the respect and degree of similarity commits a modeller to the accuracy of representation, incurring an asserting force that underpins the pragmatic equivalence. A similarity between the structure-generating description $\boldsymbol{D_S}$ of the world and the corresponding structure S_T also allows a modeller to establish a standard of representational accuracy for the $\boldsymbol{D_S}$ by specifying the degree of similarity in the S_T side.

2. Structuralism and the Similarity Account

I will first outline three variants of structuralism – the mapping account, its partial structure variant, and inferential variant. Then, I will introduce the similarity account, and argue that it characterises representations better than structuralism: it is the specification of the respect and similarity that mediates between a mathematical object and its target, directing the object to represent the target in a concrete context. In light of the similarity account, I will also briefly propose the hotchpotch picture for the study of representation. Additionally, I will explore the relationship between the structuralist accounts and the similarity account: the latter provides a broader conception of representation. Although representations need not be characterised by structural mappings, the central insight of structuralism is preserved – it can be treated as a tool for explicitly articulating or representing similarity relations set-theoretically. I will conclude this section by responding to some common objections against the similarity account.

Structuralist Accounts

The core idea of structuralist theories of applied mathematics is captured by the mapping account: mathematics applies to a target system in virtue of a structural relation between the mathematics and the system (Pincock 2012). This account has three elements: a mathematical structure, a target-ended structure, and a structural relation between them.

The first element is to assume that mathematics has a rich source of structures. A structure

S is a composite entity <D, R> where D is a non-empty set of individuals x_i of S, and R is a non-empty set of relations r_j on D ($i = 1, 2, 3, \ldots, m$) ($j = 1, 2, 3, \ldots, n$).

The second element is to assume that when talking about an application to a target system, one refers to the structural aspect of the system: a structure exists 'in,' or is abstracted from the system, and mathematics applies to this structure.

The third element is the structural relation between structures in mathematical and target-ended domains. It comes in many kinds.[6] The simplest kind is an isomorphism – a mapping function between structures A and B f: $A \rightarrow B$, such that (i) f is one-to-one (bijective); (ii) for any j, for all x_i in D^A, $r_j^A(x_i)$ iff $r_j^B(f(x_i))$. When the function f is not bijective, the structural relation can be a homomorphism.

It is noteworthy that the 'structure' preserved by the function f is formal and extensionally defined. What an r_j operates upon is merely a placeholder or a dummy object and has nothing to do with objects of specific intensions. There is no "relation in itself" (Nguyen & Frigg 2017). One can still add the intensional content to the structure. For instance, physical meanings are given to the mathematical variables when applying Newton's second law $F = ma$ to a target.

The three elements have been challenged. Criticisms centre on the third element – whether mapping functions are constitutive of scientific representations.[7] Additionally, McCullough-Benner's (2019) studies on the practice of using early calculus and Peressini's (2010, 2020) on the numeral analysis suggest that the algorithmic style of applying mathematics need not single out or reconstruct consistent, rigorous mathematical structures. For our purpose in this chapter, we will only focus on the second element. To have a completer picture of structuralism, I should also introduce the partial structure account and the inferential conception.

The partial structure account is a liberalised form of the mapping account (Bueno & French 2018). The difference between the mapping account and the partial structure variant lies in the way a structure is singled out for a target system. A partial relation is not defined over the whole

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[6] For example, isomorphism, partial isomorphism, isomorphic embedding (van Fraassen 2008), or "Δ/Ψ-morphism" (Swoyer 1991).

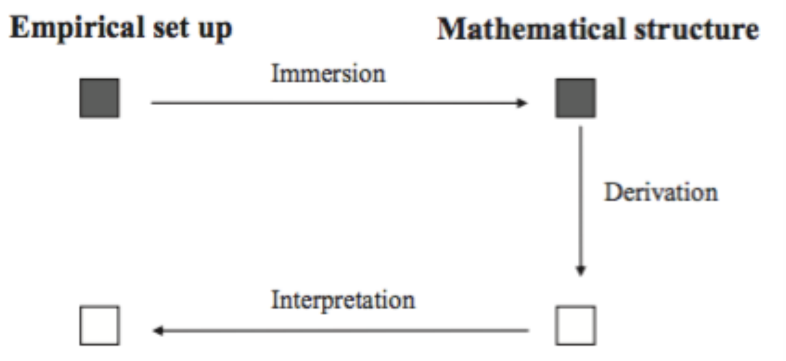
[7] More detailed discussions can be found in Teller (2001), Suárez (2003), Pero & Suárez (2016), Landry (2007), Giere (1999, 2004), van Fraassen (2008) and Frigg (2002, 2006).

domain D. It is defined as a triple $< R_1, R_2, R_3 >$ where R_1, R_2 and R_3 are mutually disjoint sets with $R_1 \cup R_2 \cup R_3 = D^n$. R_1 is the set of n-tuples that (we know) belong to R; R_2 is the set of n-tuples that (we know) do not belong to R; R_3 is the set of n-tuples that we do not know whether they belong to R or not. A partial structure A can be codified as $< D, R_i >_{i \in I}$ where D is a non-empty set, and $< R_i >_{i \in I}$ is a family of partial relations defined over D. Given two partial structures A and B, a partial isomorphic function can be built between A and B, such that (i) f is bijective; (ii) for every R_1 and every R_2, for every individuals x_i defined in D (where i = 1, 2, 3, …, m): $R_1^A(x_i)$ iff $R_1^B(x_i)$, and $R_2^A(x_i)$ iff $R_2^B(x_i)$. If the f is not bijective, one obtains a partial homomorphism.

As Bueno and French (2018) argue, the partial structure account can easily accommodate the use of idealisation and abstraction in mathematical representations. For example, studying a simple pendulum and its period of oscillation, we abstract away the colour of pendulum from the system, and idealise air resistance and friction as 'zero,' reconstructing a partial structure to correspond to the real pendulum system as follows: the gravity the pendulum bears, its length and the period of its oscillation belong to the block-R_1, while air resistance it bears, its colour and other idealised relations belong to the block-R_2. Given the isolation of the block of relations we aim to represent, the period formula $T \approx 2\pi\sqrt{\frac{l}{g}}$ applies to a real pendulum system through a partial isomorphism, even in some respect it is idealised or abstracted.

This advantage of accommodating idealisations and abstractions is related to the bridging problem and the second element. It is often argued that mathematics only applies to an idealised model system, instead of the target system itself. There is still a gap between mathematics and the target system. The partial structure account tells us that mathematics accurately applies to the target system with respect to relations in the block-R_1 once we can isolate the relations from other idealised or irrelevant parts.

The inferential conception develops from the partial structure account, characterising an application as a three-stage inferential procedure through a partial mapping function between the empirical set-up and mathematics (Bueno & Colyvan 2011). Applying a quadratic function to represent the trajectory of a cannonball, one embeds its spatial coordination to the function, derive its ending position from an initial position with parameters (gravitational acceleration, time, and velocity) and assign physical analogues to mathematical variables in accordance with pragmatic constraints. This procedure can be schematised as follows:



(Fig 2a. "The Inferential Conception of Applied Mathematics" cited in Bueno & Colyvan 2011: 353)

The mapping function in the interpretation need not be identical with that in the immersion.

This inferential picture allows structuralists to accommodate the pragmatic and cognitive aspects of the representing practice. This is related to Chakravatty's (2010) distinction between informational and functional theories of scientific representation. The former theories capture a scientific representation as a mind-independent, objective relation between mathematics and its target. This fits with the central insight of structuralism – a mapping function holds between the empirical set-up and the mathematical entity, and the entity is applied to carve the structural content from the set-up. In contrast, functional theorists emphasise the pragmatic and cognitive aspects of representations and treat those representations as what facilitate cognitive activities including interpretation, inference, exemplification, or a mixture of them (e.g., the DDI model of representation[8]). The inferential picture offers an umbrella picture for all these functional respects of representations. Nonetheless, the emphasis on functions of representations need not conflict with the informational theories. As Chakravatty suggests, informational and functional theories just reflect two perspectives of studying scientific representations: the former concerns what a scientific representation is, and the latter what we do with a representation. Therefore,

[8] More details can be found in Hughes 1997.

the inferential conception can easily accommodate functional aspects of representations, while preserving the central insight of structuralism.

In Favour of the Similarity Account

The similarity account of representation has been subjected to criticisms and accused of being vacuous. However, these charges are unfair. I will argue that it is the similarity relation – the specification of its respect and degree – mediating between mathematics and the world, and doing the work in a representation of the world. The partial structure variant fails to provide sufficient resources for some idealisations for representations in concrete contexts. Given this, one is better off appealing to a similarity relation as a more fundamental conception of scientific mathematical representation.

Giere (1988) argues that a model represents the world through a similarity relation that holds between them. By a "model," he means an abstract entity or an idealised system, which satisfies certain equations or mathematical relationships. The "world" refers to a real system, and a process or a pattern in this system. The appropriate relationship between the model and the world is a similarity relation. To build this relation, a modeller must form a hypothesis that specifies its respect and degree. The hypothesis, unlike the model, is a linguistic entity i.e., a statement about the real system. The respect of similarity refers to the respect of the system we aim to represent. For instance, to represent the oscillation period of a simple pendulum and what it depends on, we can designate a statement that the real pendulum system is, to a high degree of approximation, a simple pendulum system from which we remove the air friction, the mass of the rod from the consideration, idealise the rod as a rigid body, and so on. The "degree of approximation" (the degree of similarity) can be further characterised by adding a margin of error δ to the hypothetic equation $T = 2\pi\sqrt{\frac{l}{g}} \pm \delta$.

Toon (2012: 249) summarises the foregoing similarity account as follows:

"*M* [a mathematical model] model-represents *T* [a target system] if a scientist(s) *S* exploits similarities between *M* and *T* by forming theoretical hypotheses specifying these similarities, for purpose *P*."

The purpose P includes the goal of the modeller, precision of instruments and other pragmatic factors that constrain the modeller's specification of the respect and degree of similarity under investigation. Note that Toon's summary does not give any account of what a similarity relation is. Actually, Giere (2010) and Teller (2001) argue that there is no general, unified account of a similarity relation. The respect and degree of similarity is context-dependent: it depends upon what aspect of a target system a modeller aims to represent and to what extent the accuracy of representation would be.

Before going to the arguments in favour of similarity, we should see what motivates us to adopt the similarity account, and how it relates to the partial structure variant of structuralism. The motivation is that the representational relationship between mathematics and the world "is rarely, if ever, exact" (Teller 2001). Regarding inexact representations, one often refers to the idealisations and abstractions above. The similarity advocate claims that the similarity relation allows for a broader room for the idealisations and abstractions than an isomorphism.

In a sense, the partial isomorphism is a *precisified* version of a similarity relation. French and Ladyman (1999) complain that the notion of similarity is too vague. Regarding the respect of a target system we aim to represent, to capture the similarity between a model and the system, we must pinpoint a one-to-one correspondence between the relevant relations in the model and the relations in that respect of the system. Regarding the task of accommodating idealisations and abstractions, the partial structure puts the distorted or removed relations into the block-R_2 or -R_3, so that the relations in the block-R_1 can be represented accurately with a mathematical characterisation. In the simple pendulum case, we place air friction into the block-R_2 where we know they do not hold for the pendulum, and the length of the rod, the gravitational constant and the period of oscillation are placed in the block-R_1 that we consider holding to represent the period of oscillation for this pendulum. This is about the partial structure specification of the respect of similarity. Concerning the specification of its degree, structuralists might concede that the approximating techniques and the standard of what counts as an accurate representation for a specific measurement set-up are 'external' to partial structure reconstruction of a physical system and the partial isomorphism (French 2017). Nonetheless, the use of approximation and contextual factors for theory confirmation are inessential to the source of representational force of a mathematical structure to the physical system. One would still require a mapping function to give a mathematical structure or mathematical vocabularies with physical interpretations and form the theoretical hypothesis about the physical system. Put differently, the representational force from a mathematical structure to a physical system is endowed by the partial isomorphism between them. As to the pendulum system, the introduction of the margin of error has nothing to do with how we obtain the oscillation period formula.

It seems that one should favour the partial structure variant, for it provides a more precise form of representations than the similarity account. However, the partial structure variant does not exhaust the central insight of the similarity account. The use of approximating techniques is *essential* to accounting for the representational force of a mathematical structure to a physical system *in reality* – the system we actually refer to in a representing practice.[9] When claiming Newton's laws apply to the Sun-Earth system, we refer to the Sun and Earth in reality, instead of the idealised two-body system. To capture this insight, it is better to appeal to a broader similarity relation.

First of all, let us distinguish between a 'how-possibly' representation and a 'how-actually' representation. A 'how-possibly' representation refers to the representation of physical systems that we consider in a counterfactual, or a 'what if …' sense. In the single pendulum case, what we obtain from an idealised system is all about the pendulum when it is treated as if there were no air resistance, or when air resistance would not exist. A 'how-actually' representation refers to the representation of physical systems in an actual experimental (or measurement) set-up. The distinction between the two is not sharp: when experimentation satisfies the counterfactual condition that a how-possibly representation assumes, the how-possibly one is transformed to the how-actually one. Still, it is easy to distinguish between the how-possibly and how-actually kinds. To make this distinction clearer, we can portray the harmonic oscillator in question in a phase space:

$$\frac{\theta^2}{A^2} + \frac{\theta'^2}{A^2\omega^2} = 1 \tag{P}$$

[9] The representational force from A to B is the capacity of directing A to represent B.

In this portrait, the path of the pendulum is an eclipse determined by the length of the pendulum, the gravitational constant and the initial state of the pendulum. If air resistance and friction are considered, the path will return to the original point in the portrait when the bob stops. What the equation literally represents is a system as if there is no air friction (that is why we place it into the block R_2).

Additionally, it is crucial to distinguish between a how-possibly representation and a how-actually representation because it is crucial to distinguish between an imagination and a reality. When scientists apply Newton's laws to represent the Sun-Earth system, it is crucial to be aware of the difference between the real dynamics between the planets and the idealised two-body system that mathematics applies.

The issue for structuralism is that the partial isomorphism-based representational force (for a mathematical structure) only suffices for a how-possibly representation of a physical system, but not a how-actually representation. For example, if we take the trajectory characterised by the phase equation (P) literally, it represents a simple harmonic oscillator. By "literally" I mean, there is an isomorphism between the trajectory (in R_1) and the trajectory smoothed out from the real pendulum. Given the existence of the idealised conditions, the trajectory should not be counted as a literal representation of the real pendulum and what occurs for the system. In other words, without approximating techniques, the mere partial isomorphism fails to distinguish a how-actually representation from a how-possibly one.

Structuralists might respond that they have resources to distinguish between the two kinds of representations insofar they can provide different arrangements of the idealised conditions into blocks of relations. As to how-possibly representations, the idealised conditions should be put into the block-R_1, since the equation (P) is used to represent a system when the conditions were true. As to how-actually representations, the idealised conditions should be placed in the block-R_2, since they are not what (P) aims to represent. Given this distinction, structuralists might claim that equipped with an appropriate partial structure reconstruction of the target, a partial isomorphism can suffice for a how-actually representation. The use of approximation is merely an indicator for contextual factors on which we rely to establish the standard of accuracy for the representation in an actual measurement set-up.

However, I doubt that the partial structure variant has the resources to make this distinction between a representation of an idealised system (where the idealised relation is put in R_1) and a representation of a real system (where the idealised condition in R_2). This is due to a doxastic inconsistency in the modelling practice in how-actually representations.[10] The modeller indeed concedes the existence of air resistance or other friction when they apply the phase equation to a real pendulum system. Otherwise, why need they introduce the margins of error and restrict the range of representation in a small amplitude and a period of recording? The attitude of the modeller is subtle: they concede the existence of factors that idealisation should have removed, and pretend to treat them *as if* non-existent along with the practice of using idealisation.[11] Here is the inconsistency: On one hand, when applying the equation to the real system, they believe that there is air resistance in this system. On the other, with an idealising practice, they 'believe' (or pretend to believe) that the system they represent has no air resistance.

Another misgiving concerning whether the partial structure variant has resources to make the distinction in question is that the arrangement of idealisations to different blocks does not define a unique truth-condition for the mathematical equation applied to represent it. To keep consistent with the concept of 'partial structure,' Bueno and da Costa (2007: 338) redefine the notion of truth and call it 'quasi-truth.' To define quasi-truth, we extend a partial structure *A* to a full, total structure, called A-normal structure: $B = < D', R_i' >_{i \in I}$, with the same domain with *A* and the same interpretation for relations and individuals in *A*. The difference between *A* and *B* is that R_i' is not defined for all *n*-tuples of individuals in *D'*. A sentence is quasi-true in *A* iff it is true in *B*; quasi-false iff it is false in *B*. The issue is that both sentences describing the real system, and sentences describing its corresponding idealised proxy, share the same quasi-truth condition for the mathematical equation applied to represent. To define quasi-truth, we take the relation in the block R_2 as a R_1-like relation that holds for the individual. Bueno

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[10] Pincock (2014) also mentioned this issue.

[11] The significantly and trivially idealised relations put in block R2 should be distinguished. The idealisation of air resistance is significant, since its existence in fact affects the equation we obtain for a representation. The idealisation of the bob's colour is trivial, since whether it belongs to the pendulum system or not will not affect what equation we obtain. In discussions about the inconsistency here, I am concerned with the significant idealisation.

and da Costa might simply accept this criticism and respond that this only means the notion of quasi-truth is flexible for the same equation to represent across contexts. I am not satisfied with this response. It is absurd to claim that an equation is used to represent a real system (instead of its idealised proxy), while failing to distinguish the quasi-truth condition for sentences about that real system from the quasi-truth condition for its idealised proxy.

Concerning doxastic inconsistency in modelling practice, one might dismiss it as a logical trickery. There is no real inconsistency if one takes the modeller's 'pretence' in the idealising practice into consideration. I agree. However, the real issue here is whether the partial structure variant has resources to capture the 'pretence' attitude for an idealised respect of the system, or whether it has resources to capture the discriminating attitudes towards different blocks of relations in a partial structure.

More precisely, a modeller holds a 'factual' attitude towards the relations in R_1 that (they know) *belong to* R. Using the phase equation (P) to represent the simple pendulum, we believe that the network of relations (P) grasps holds for the pendulum, and this belief expresses a fact in this world; thus, we can say that the relations in R_1 express the fact in the pendulum system. The factual attitude identifies R_1-relations with the respect of a system that mathematics aims to represent. The 'pretence' attitude towards a belief in p refers to a situation where we (in fact) disbelieve that p, but pretend to believe that p (or believe that 'as if' p). One should not hold the factual attitude towards some idealised relation (e.g., the absence of air resistance) in R_2, as air resistance does belong to R and affect what system would perform – that R_1-relations represent. I describe such an idealisation as a significant one, as it does hold for a system and affect what equation we use to represent the system. There are trivial idealisations: even if the bob's colour is not defined for R, it will not affect what we obtain in the R_1 block of relations because whatever colour the bob has, the R_1-relations holds. Here, I am concerned with the significant idealisation.

In my view, the partial structure definition that the relations in R_2 are what (we know) *do not belong to* R, is not sufficient for explaining the pretence attitude towards idealised relations. It does not provide an account for why we should, and how we can, hold the pretence attitude (or a discriminative attitude from the factual one) towards R_2-relations. The partial structure theorist might waive this duty of explanation and claim that they only need accommodate the pretence attitude. Treating the pendulum system as if there were no air resistance or friction, they only need define the idealised condition as what does not hold for the system. However, as argued, without approximations, if one takes the equation (P) (in R_1) literally, there is no barrier for them to be committed to the belief that 'air resistance and friction do not exist for the system.' Otherwise, how can (P) hold in the R_1 and express a fact in the pendulum? Again, this pushes the modeller into the doxastic inconsistency.

In sum, the use of approximating techniques is essential for making a mathematical object be a how-actually representation of a system. In the pendulum case, these techniques allow the modeller to distinguish a how-actually representation (the effect of air friction is absorbed in a margin of error) from a how-possibly one (if air friction did not exist), and direct the equation (P) to represent a system in a measurement set-up, rather than idealised proxy. Given this, the modeller can safely hold a pretence attitude towards the idealisation regarding air resistance, and this motivates them to place the idealised condition in the block R_2 that are not defined for the pendulum system. The similarity advocate can respond to the partial structure theorist (e.g., French 2017) as follows: if the use of approximation is 'external' to a model, then the formalism in the model – the set-theoretical formulation of relations in different blocks and the corresponding partial isomorphism – should not be treated as the machinery that mediates the model to the world, in a how-actually representation, a representation of a system in reality.

The application of mathematics to a physical reality is always specific and contextual. One should not hold a 'core-context' picture to grasp a how-actually representation. In this picture, the primary thing is to capture the 'core' of representational force using a mapping function, and everything else is viewed as 'contextual' and additional to our structuralist characterisation of the 'core.'[12] Rather, from the similarity account of representation, I would like to propose ***a hotchpotch picture*** for our study of mathematical representations: a representation of a system in reality is treatable as a series of practices – e.g., idealisation, approximation, illustration of

[12] This picture guides all variants of the structuralist approach to representations. See Bueno & Colyvan 2011.

background theories and the building of the measurement set-up – which bring a mathematical structure in and form it into a representation of the system in that set-up.[13] The representational force results from these practices, and it is not the source of attaining these practices. In light of this insight, it is better to see different theories of representation as appreciations of distinct aspects of representations, and the similarity account is better than the structuralist accounts as it provides a broader conception of representation.

Let us return to the thesis – that it is the specification of the respect and degree of similarity *doing the work* in mathematical representations. In our pendulum case, the respect of similarity is specified as what a simple oscillator performs when using (P) to represent a real pendulum, carving structural information from the real pendulum. Note that the specification is not simply to define a partial structure for the pendulum and make a partial isomorphism between (P) and the partial structure. An appropriate approximation must be employed to constrain the range of representation (based on empirical assumptions and conditions of instrumental set-ups), ruling out idealised or abstracted relations as irrelevant, or absorbing them into margins of error. In this way, the degree of similarity is also specified. It is critical to note that it is these specifying works that mediate between (P) and its target in reality.

The Relationship Between Structuralism and the Similarity Account

There are three features regarding the relationship between structuralism and the similarity account. First, compared with structuralist accounts, the similarity account provides a broader notion for capturing representational relationships between mathematics and real systems. It is broader in three senses.

The first sense is that although the partial structure variant is treatable as a programme of precisifying a similarity relation set-theoretically, it only specifies *the respect of similarity*. It does not exhaust the role of the specification of the degree of similarity in representations of systems in reality. The use of approximating techniques is essential to the how-actually kind of representations – without it, the partial structure approach alone fails to direct a mathematical

[13] The "hotchpotch" picture is quite *rough* here. I will specify it to characterise RG explanations in chapter 4.

representation to a real system rather than its idealised proxy. Since the use of approximation is a crucial way of specifying the degree of similarity, the partial structure programme does not account for the specification of the degree of similarity. Thus, from the similarity perspective, a partial structure and a partial isomorphism only capture a similarity relation in its respect, but not its degree.

The second sense is that if representations are grasped as a series of practices, then partial structures and partial isomorphism only characterise the last stage in the process of making the representations. The use of approximation organises the empirical assumptions, measurement instruments, and mathematical entities into a representation of their targets to some degree. The partial structure formulation of the target systems (e.g., putting idealised relations in R_2 one aims not to represent) is motivated and justified by the successful use of approximations.

The third sense is related to the inferential conception – the most advanced variant of the structuralism. It can be argued that the inferential variant accommodates the representational work captured by the similarity account by incorporating the modeller's intention, instrumental conditions, and other pragmatic factors, while preserving the structuralist conception of representation. I disagree with this claim for two reasons. First, the notion of "empirical set-up" that the inferential variant relies is too coarse to distinguish a real system from its idealised proxy. Bueno and Colyvan base the inferential procedure on the partial structure reconstruction of the system and the partial isomorphism between it and mathematics. However, it is obscure how we are to obtain the inferential capacity on the basis of the structuralist conception, since the partial structure theorists have no resources for making the distinction in question and directing the representation to the system rather than its idealised proxy. Given the relation between the notion of representational force and the modeller's intentionality in representing practice, the inferential variant might use intentionality to explain the representational force and waive their duty of explanation based upon partial structure formulation and relevant mappings alone. Yet, this is not the best strategy for structuralists as the appeal to intentionality makes the conception of representation too flexible and too easy. More importantly, from the similarity perspective, the crux about the representational force is not just about intentionality, but *how* mathematics negotiates with what structuralists recognise as "contextual factors" to forge a representation of its target. This is also my second reason: the inferential variant does not provide an account of how modeller's purposes, empirical assumptions and the measurement set-up are organised together to form a context of representation – what Bueno and Colyvan call the "empirical set-up," which is further to be represented mathematically – and how mathematics *enters into* the context. Thus, the similarity account is "broader" than the inferential variant in a sense that the former tells the story of how an empirical set-up is formed.

The second feature of the relationship between structuralism and the similarity account is: although representations are captured by similarity relations, the central insight of structuralism is preserved – mathematics represents its targets by carving structural information from them, and this structural information is reckoned as mathematical. It is worth nothing that to capture this insight, one need not define what kind of object a representational relationship would be. To establish the standard of representational accuracy, one need not define the representational relationship as a structural mapping.

The third feature is related to the second. Structuralism is treatable as a tool for explicitly articulating similarity relations and the specification of their respects in a set-theoretical form. A similarity relation can be translated as 'approximately, a partial isomorphism.'

Replies to Objections against the Similarity Account

There have been various criticisms of the similarity account. Most of them focus on the vagueness of similarity and its conflict with the concept of truth. I have shown how Giere's emphasis on the specification of similarity's respect and degree dispels the vagueness charge. The second charge is dissolved if we insist using the everyday, vague sense of truth, rather than its "exact" sense (Giere 2010: 273). The notion of similarity 'approximately, *p*' is enough, and there is no need to invoke the additional notion, such that '*p* is approximately true, (but exactly false)' (ibid: 274). A scientific theory need not dictate a literal truth about the world, but always provides an approximate representation of it.

There is also a concern about specification of the respect and degree of similarity. Giere argues that the specification is interest-relative, i.e., dependent upon modellers' purposes, so it seems that there is no objective principle to define the appropriateness of the respect and degree of similarity. This is true. Yet, this is not an issue once one considers disciplinary normativity behind representations. Some disciplines, like art, might impose a much looser constraint on the degree of similarity. Appreciating *Guernica*, I need not follow the traditional interpretation that it represents the brutality of Fascism. I can reinterpret its significance by relying on a similarity between the yelling of women and broken oxen and a massacre that happened in my hometown and making *Guernica* represent and criticise the violence. With respect to the sciences, there is a stricter and more codified specification of degree of similarity. For instance, the coefficient of determination applies to specify the goodness of fit between real data-points and regression prediction of a hypothetic model. It is true that, very often, the threshold of what counts a good fit depends on scientists' agreement. However, this does not mean that this threshold is selected arbitrarily or purely conventional (Giere 1988). Scientists can reasonably adjust the degree of similarity in confirming or disconfirming a similarity relation, based on the use of instruments and representing techniques, and circumstantial conditions for data collection.

Related to the preceding, I would like to respond to an objection given by Toon (2012). He argues that Giere's account does not give a sufficient condition for the representational status of objects concerned. A representational status indicates whether an object is representational, or represented, or neither. A model represents a target by making a theoretical hypothesis about the target, which specifies the respect and degree of similarity between them. However, this hypothesis is made by the "stipulation" of modellers, and what changes the representational status of the objects concerned is their stipulation. As Toon (ibid: 253) illustrates, using a block to represent a methane block as tetrahedral, the spatial similarity between them is not sufficient for changing the representational status of the block. It is our interpretation of the block – the interpretation of the shape of the block as similar to that of the methane molecule – that makes the block as something representing. Toon's point is that without such an interpretation, the similarity account would fail to distinguish between *an act of comparison* and *a representing* *act* between them, and a similarity relation only suffices for the former, but not the latter. It is possible to use the block to represent the methane, but this is not necessary. The similarity relation does not exclude cases that two objects are similar, but neither is representational.

My response simply is: 'so what?' We can accept this critique by adopting Chakravartty's distinction between functional and informational theories regarding scientific representation. Toon assumes a functional theory that a representation must be formed with a cognitive activity. However, this does not undermine Giere's insight that a representational *relationship* between objects is captured as a similarity, and this is not about the representational *status* of the objects. Thus, Toon's critique misconstrues the real issue Giere addresses.

Toon might reply that the similarity between a model and its target system is not 'natural,' but constructed with an interpretation of the system. For example, the spatial similarity between methane and blocks is because both of them are interpreted *as* tetrahedral. The representational relation is built on the cognitive activity with a representation.

In reply, this is not an issue for the similarity account. In the context of applying scientific models, the specification of the respect and degree of similarity is not wishful thinking, but involves representing techniques and instruments, which causally interact with the system we aim to represent. For instance, if our theory about electron diffraction is approximately correct, and the transmission electron microscope is reliable, then we can infer from data to the spatial structure of molecules, suggesting a structural similarity between what the data pattern would be and what the spatial structure would be. If the data obtained 'fits' the data that we predict a tetrahedral molecule would have, then we are more confident to say that a tetrahedral object (e.g., a block) is similar to a methane molecule, in respect of their spatial structure, within a margin of error. The hypothesised similarity is, of course, built with the stipulation of modellers (because we are imaginative creatures!). However, if we can testify and confirm the 'stipulated' similarity experimentally with correct background theories and reliable instruments, then it is reasonable to believe that the similarity exists between the model and the world.

Toon might criticise that my response entails a regress, for I appealed to another similarity (between the phenomenon and data extracted from it) to confirm or disconfirm the similarity relation between blocks and methane molecules. This is true. But, I do not think this regress is vicious. Rather, this merely reflects a holistic picture of scientific confirmation. For instance, we use the physical equation $x = vt$ to represent uniform linear motion of an object. If one doubts how this abstract equation (or its graphic representation) is similar to motion in reality, we can just show them that the similarity at issue relies on a more primitive similarity: the hypothetic equation can be visualised as motion of a mass-point, and we confirm its visual similarity to motion in reality. It is this visual similarity allowing us to claim that a similarity exists between an object's motion in reality and a 'smoothed' data model extracted from it. In this sense, the regress is benign, and it is just a journey tracing back to the evolution and history of scientific theories, models and instruments.

3. How the Similarity Account Dissolves the Bridging Problem

I will first outline the bridging problem. Then, I will argue that the similarity account provides a pragmatic framework to supplement other solutions to the bridging problem by establishing a standard of representational accuracy without presupposing an account of representation.

The Bridging Problem and Similarity

The bridging problem starts by highlighting a category mistake in structuralist accounts of mathematical (model) representation, as put by van Fraassen (2008):

> "How can an abstract entity, such a mathematical structure, represent something that is not abstract?" (p.240)

Or, more precisely:

> "If the target [of representation] is not a mathematical object then we do not have a well-defined range for the function, so how can we speak of an embedding or isomorphism or homomorphism or whatever between that target and some mathematical objects?" (p.241)

Simply put, if mathematics represents through a mapping function, then it is impossible for the non-mathematical world to be represented mathematically.

This also poses a problem for the similarity account, for it is unclear how a mathematical object is similar to a physical object. The mathematical object is abstract i.e., defined in the realm of axioms and set-theory, while the physical object need not be defined in this way. In this sense, the similarity account faces the same problem.

Teller and Giere offer a realist response to the problem:

> "concrete objects HAVE properties and that properties are PARTS of [mathematical] models" (Teller 2001: 399)
>
> "One way scientists [use mathematical models to represent the world] is by picking out some specific features of the model that are then claimed to be similar to features of the designated real system to some degree of it" (Giere 2004: 747-8)
>
> "…one can formulate empirical claims as theoretical hypotheses about how the real system should behave if it is indeed similar to the model in the requisite respect" (Giere 1999: 41)

No matter whether a target of representation is mathematical or not, the modeller can designate it *as having* properties, or patterns of behaviour, which are similar to (or partially mapped to, if only the respect of similarity is specified) a mathematical model that represents it.

Van Fraassen (2008: 242) objects that this realist response is vacuous. It begs the question: how can this mathematical model represent a concrete physical target? For we assume that the target can be represented *as* what the model represents.

However, this is not an issue to the similarity account. The advocate of similarity need not hold a realist attitude towards the target of what a mathematical object is similar to. Suppose an abstract triangle is visually similar to a piece of triangularly shaped paper. Does the paper have a triangular shape? No! If we look the paper microscopically, there is no sharp vertex or straight edge a triangle has. However, at the level of macroscopic observation, it is reasonable to assume the paper *as having* a triangular shape with vertices and edges that are measureable by protractors and rulers. I do not know whether this representation-as supports a realist or an empiricist position of representation – nor need I. The similarity account is open to empiricist interpretations – what bears a similarity relation can be an "empirical substructure" that van Fraassen has in mind, and mathematics only represents what is observable, no matter what the observability is defined.

So, how does the similarity account dissolve the bridging problem? If, as Giere and Teller emphasise, there is no unified, general account of similarity, how can we expect it to provide a principled account of representation to bridge the gap between mathematics and the world? At the current stage, the similarity account is more of a pragmatic framework to forge mathematics into a representation of systems in reality. Unlike structuralism, which reifies a representation as an isomorphism or whatever else, there is no 'core' for what a similarity is and ought to be. How can we expect similarity *qua an account* to address the bridging problem?

I do not think of this as a weakness of the similarity account. The value of the similarity is not to provide a universal theory reducing a representation into something else. Rather, its value is to provide a pragmatic way of constructing a representation with an appropriate standard of accuracy, without a prerequisite account of similarity and representation (although the respect of similarity is often formulated structurally). From this perspective, one can be confident of designating a real system as having a property or a pattern of behaviour, and claiming that this property or pattern is (or is partially mapped to) a part of a model. Once the degree of similarity is specified *appropriately* (and confirmed),[14] the similarity can be "assumed" to hold between the designated aspect of the system and the representing part of the model. The similarity can bridge the gap between mathematics and the real system on a case-by-case basis.

There are at least two ways of designating a similarity relation. The first way is by making *a visual similarity*. As Giere (1994) illustrates, Newtonian principles do not directly guide the motion of real objects. Rather, these principles are used to define a paradigmatically idealised system, which is further applied to represent the systems in reality. The similarity between the idealised system and the real one can be visualised. For example, the single harmonic oscillator can be visualised as an idealised pendulum, which is visually similar to the real pendulum (with a heavy bob, a small swing, no driving force, etc.) The phase equation (P) that captures dynamic

[14] To specify degree of similarity appropriately, the modeller must draw on correct empirical assumption, reliable instruments and statistic techniques, providing a collection of inference patterns from phenomena to data, in accordance with the similarity in the designated respect. Detailed case studies can be found in Kaiser 1991. It is noteworthy that that the inferential conception of representation I implicitly appeal here does not conflict with the similarity account. Rather, it is more of a supplement for the similarity account concerning the question of how a 'context of representation' is formed.

properties of the idealised pendulum represents the real one through this similarity between the idealised oscillation and the real one.

This visual similarity also supports another kind of similarity – the ‘fit’ defined along with statistical techniques. As a measurement practice can translate the idealised pendulum to a data set and map the idealised oscillation to a model of data, it is reasonable to do the same practice over the real pendulum and map the real oscillation to data points, which further is smoothed out to be a continuous curve. Given a structural mapping between the idealised pendulum and (P) that characterises its oscillation with measurable physical quantities, it is reasonable to say that there is a structural mapping between the real oscillation and the data model obtained from the same measurement practice, and to define the “fit” between the phase trajectory and the trajectory smoothed out by specifying a margin of error δ, such that

$$\frac{\theta^2}{A^2} + \frac{\theta\prime^2}{A^2\omega^2} = 1 \pm \delta.$$

If the measurement instruments are reliable, and the resultant curve fits with the hypothetical trajectory within the margin of error, then it is reasonable to say that the phase equation bears a structural similarity to the real oscillation.

Therefore, the gap between mathematics and the world is bridged by a similarity assumed to hold between the designated aspect of the world and the mathematical model. The similarity can be visualised or structurally mapped. No matter how the aspect of the world is designated to have features, which are similar to a mathematical structure and represented mathematically, *once* the degree of similarity is specified and the standard of representational accuracy is built, the designated aspect of the world will be claimed to have the features when the representing practice is successfully cashed out within the degree and respect of similarity.

So, what does it mean to *designate* a target as having features allowed to be mapped to a mathematical structure? There have been two similar approaches offered to resolve this issue. One is to say that a model represents the world by interpreting objects and relationships in the world as the objects and relationships in the model, which allows scientists to infer from the model to the world (Contessa 2007). The other approach is to make a description of the world, from which a structure can be abstracted and mapped to a mathematical structure (Nguyen &

Frigg 2017).

In what follows, I will supplement the second approach with the similarity consideration because it directly responds to the issue of how mathematics applies to the world. Then, I will show what insight we can draw to rescue van Fraassen's pragmatic solution.

Similarity, Structure-Generating Description and Pragmatic Equivalence

Nguyen and Frigg (2017) approach the bridging problem by resolving the issue of how a target system obtains a structure. The system is described with certain physical relations and physical objects, which are related and ordered in a specific way; then, we abstract a structure from this description by replacing these relations and objects with abstract relations and objects.

For instance, three physical relations (distance, velocity and time) are used to describe a physical object's one-dimensional motion, in which velocity is proportional to distance, and inversely proportional to time. The description $\boldsymbol{D_S}$: there is an object $\boldsymbol{o}$, which has properties, velocity $\boldsymbol{v}(\boldsymbol{o})$, distance $\boldsymbol{d}(\boldsymbol{o})$, and time $\boldsymbol{t}(\boldsymbol{o})$, such that "$\boldsymbol{v}\ (\boldsymbol{o}) \propto \Delta \boldsymbol{d}(\boldsymbol{o})$ given the same $\Delta \boldsymbol{t}(\boldsymbol{o})$"; "$\boldsymbol{v}\ (\boldsymbol{o}) \propto \frac{\boldsymbol{1}}{\Delta \boldsymbol{t}(\boldsymbol{o})}$ given the same $\Delta \boldsymbol{d}(\boldsymbol{o})$." One can replace these physical relations and objects with dummy variables and form a structure S_T <D, R> that the domain D is defined over an object o; the relation R is defined over $v(o)$, $d(o)$, $t(o)$, and $v\ (o) \propto \frac{\Delta d(o}{\Delta t(o)}$. A real-number function S_M: $y - ax - b = 0$ can apply to this structure. The $\boldsymbol{D_S}$ constrains what structure will be abstracted and what respect of a target system represented. Nguyen and Frigg argue that this "structure-generating description" provides a general account of mathematical application without committing modellers to the accuracy of representation. This is correct as mathematics can apply to represent a fictional model.

However, in the context of using scientific models to represent the world, the standard of representational accuracy should be considered. Nguyen and Frigg's account does not provide such an account because if the accuracy condition is given the structural mapping, then nothing can be said about the $\boldsymbol{D_S}$ that directly characterises the world. To supplement their account with a standard of representational accuracy, it is natural to suggest the abstraction relation between a $\boldsymbol{D_S}$ and S_T to be a similarity relation.

For example, regarding uniform linear motion, although the physical object $\boldsymbol{o}$ and physical relations $\boldsymbol{v}(\boldsymbol{o})$, $\boldsymbol{d}(\boldsymbol{o})$, and $\boldsymbol{t}(\boldsymbol{o})$ are non-mathematical, they *are arranged in a specific way*, so that a structure can be *correspondingly* abstracted from this way of arrangement. It is natural to suggest *a structural similarity relation* between the arrangement in $\boldsymbol{D_S}$ and S_T, which allows us to make this abstraction.

Another example: the visual similarity between random walks in plate grids and Brownian motion in a two-dimensional plate allows us to designate a $\boldsymbol{D_S}$ about Brownian motion of a particle: there is an object, which moves with random directions and a random distance. Then, a 'random walk' structure S_T can be abstracted from this $\boldsymbol{D_S}$, with some specifications on the directions and units of distance.

Given the similarity relation that straddles both $\boldsymbol{D_S}$ and S_T, if one can specify the degree of similarity in the S_T side (e.g., by applying margins of error for parameters of equations deduced from the random walk along with the support of empirical assumptions and sufficient precision of instrument), then one can set a standard of representational accuracy for the $\boldsymbol{D_S}$.

I believe this similarity between $\boldsymbol{D_S}$ and S_T gives an insight into scientific practice of using models to represent the world and rescues the pragmatic equivalence, in particular.

Let us consider van Fraassen's (2008) thought about it. He introduces it to dissolve the so-called 'Loss of Reality' objection to his empiricist structuralism of scientific representation. The empiricist position consists of two theses:

(1) "Scientists represents the empirical phenomena as embeddable in certain *abstract structures* (theoretical modes)." (p. 238)

(2) "Those abstract structures are describable only up to structural isomorphism" (ibid).

The Loss of Reality objection says that given the distinction between phenomena and data, for mathematics is only isomorphic to a data model, but not phenomena, mathematics fails to represent the latter and 'loses the reality.'

We assume Bogen and Woodward's (1988) distinction between phenomena and data. The former refers to the regular, stable objective process in this world, and we can infer its existence from a dataset (provided the data are reliable). A scientific theory represents phenomena. Data are a "public record [of] produced measurement and experiment that serves as evidence for the existence or features of phenomena," involving idiosyncratic features of experimental contexts (Woodward 2011: 166). Scientists aim to filter a regular, stable pattern from the dataset, which corresponds to a phenomenon (McAllister 1997).

To dissolve this objection, van Fraassen (2008: 259) appeals to a pragmatic tautology:

"For us, the claims

(A) that the theory is adequate to the phenomena and the claim that

(B) that it is adequate to the phenomena as represented, i.e. *as represented by us*

are indeed the same!" (the emphasis original)

There is no pragmatic difference between mathematics accurately representing a phenomenon and a data (model) extracted from it. He continues to illustrate that this pragmatic equivalence is analogous to an undeniable assertion. Suppose the data model is reliable: for scientists using it to represent a phenomenon, it sounds paradoxical for them to believe that a theory applies to represent the data model, but not the phenomenon.

To take van Fraassen's example: when scientists use a data model *D* to represent a deer population *T*, they represent *T as* Π i.e. what they describe *T* in the data model (the estimated total numbers, age distribution etc.). The pragmatic tautology is that it is contradictory for them to claim that they use *D* to represent *T*, while denying that *T* is Π.

Nguyen critiques the analogy between the pragmatic tautology and Moore's Paradox for an act of assertion. Moore's paradox says that to assert that "there is no sentence," even if it is true in another logically possible world, for those who live in *this* world, is defeated by the act of assertion. However, Nguyen indicates the premise that "the (pragmatic) content of *S* using *D* to represent *T* as Π [what *T* looks like in *D*] includes *S* believing that *T* is Π" is simply false. An act of representation need not commit scientists to believe that the system represented *is* what it is represented as. Nguyen provides a case: to represent Margaret Thatcher as draconian, we need not believe she is draconian. Put in another way, the pragmatic equivalence assumes a false identity relation between phenomena and data.

I accept this critique to van Fraassen's pragmatic tautology. However, this does not mean that we should discard the pragmatic tautology, once we clarify *how* "the indexical word "us" functions to denote in an assertion" (van Fraassen 2008: 259). To bring the indexical "us" into the scientific context of applying models, the advocate of similarity can suggest that for "us" (or scientists), to use D to represent T as Π does not commit "us" to believe that T is Π i.e., the deer population *is* a data-point or a function in the data model, but only commits "us" to believe that the deer population *is similar to* the data-point or the function, within some respect and some degree. *To specify* its respect and degree is what "we" need to consider and function in the *concrete* context.

Once the degree and the respect are (successfully) specified, "we" or scientists have set a standard of representational accuracy, so that "we" are able to cash out a pragmatic tautology, such that (I copy Nguyen's (2015: 182) argument reconstruction):

1. The pragmatic content of S using D to represent T as Π includes S believing that T is similar to Π (within a respect and a degree).
2. If S is able to take mathematics M to accurately represent D, but not T, then S is able to express disbelief in any proposition concerning T that S commits herself to in using D to represent T. (For example, the data collection is unreliable and beyond the degree of similarity specified.)
3. S uses D to represent T as Π.
4. If S is able to take M to accurately represent D, but not T, then S is able to express that T is not similar to Π (within the respect and the degree). (From 1, 2, 3)
5. It is not that case that S is able to express disbelief that T is similar to Π (within the degree and the respect) while using D to represent T, on the pain of pragmatic contradiction.
6. Therefore, it is not the case that S is able to take M to accurately represent D, but not T. (from 4, 5)

What underpins this argument is that once the respect and the degree of similarity are specified, we are committed to the accuracy of representation, and this commitment incurs an asserting force or something like it in our act of representation.

Nguyen might object that I am not allowed to offer an account of accurate representation until I offer an account of representation. However, we need not follow this order of explanation until Nguyen provides an argument that an account of representation is the prerequisite for an account of representational accuracy. The similarity account provides a pragmatics of setting a criterion of representational accuracy without presupposing a general account of representation.

4. Conclusion

In this chapter, I have proposed the similarity account of mathematical representation. It is the specification of respect and degree of similarity that brings mathematical objects into contexts of representation and directs the objects to represent their targets in measurement set-ups. The similarity account gives a broader conception of representations than structuralism. The latter is treatable as a tool for explicating the respect of similarity set-theoretically. Additionally, I have argued that a similarity relation rescues van Fraassen's "pragmatic equivalence," and have supplemented Nguyen and Frigg's "structure-generating description," to dissolve the bridging problem

Chapter Three
Inconsistent Early Calculus: The Robustly Inferential Account and Structural Similarity

1. Introduction

In his paper “Representing the World with Inconsistent Mathematics,” McCullough-Benner (2019) argues that structuralism fails to explain how inconsistent theories of mathematics are used to constrain scientific representations, and proposes what he calls “robustly inferential account” as a better account of mathematical application.

This chapter aims to rebut McCullough-Benner’s arguments and defend structuralism. I will defend the partial structure variant from McCullough-Benner’s two critiques, emphasising that the robustly inferential account has no advantage over structuralism, since it does not give an account of why some inference pattern is privileged. In the concluding section, I will suggest that with a similarity supplement, structuralism can provide a more perspicuous account of how mathematics constrains physical representations.

2. Inconsistent Early Calculus

What is the “early calculus?” The project of “calculus” is motivated by the attempt to solve problems in the four following areas: (1) given the formula describing the distance of an object as a function of time, to find instantaneous velocity, or conversely, to find the distance travelled, given a formula describing acceleration of the object as a function of time; (2) to find the tangent of a curve; (3) to find the maximum and minimum values of a function; (4) to find the length of a curve (Kline 1972). These four areas are intuitively unified in a study of continuity, which were historically represented in two forms: (a) the geometrical demonstration of curves, and (b) the arithmetic operation over finite or infinite series. Here, we have a roughly unified picture of the “early calculus,” which consists of four separate areas of study.

So, how is the historical entity 'early calculus' unified? McCullough-Benner (2019) and Vickers (2007) claim that the early calculus is all about the "calculus" of infinitesimals. The "calculus" here should be understood in terms of following a bare algorithm. For example, to take a derivative of a function $f(x)$, we should go through a procedure, such that

(1) Put your equation in the form $y = f(x)$

(2) Calculate $\frac{f(x+\varepsilon)-f(x)}{\varepsilon}$, and simplify

(3) Remove any terms which are multiples of ε

where ε is an infinitesimal, which is taken as something "infinitely little" or "approaching zero" (McCullough-Benner 2019: 4). One should not worry whether the term ε is justified or what ontology should be given to ε, and only need follow this procedure to take the derivative of a function, which works for other applications (e.g., to find a tangent of a curve or a velocity function). In addition, as one only cares about the "reals" (real-number functions and their real-number derivatives), and items containing infinitesimals are removed in the step (3), they can just do the calculus.

The inconsistency lies in the level of justification or explanation. If one takes step (1) to (3) as propositions, then the reasoning will be inconsistent. To attain step (2), it appears that ε is a non-zero quantity, while to attain step (3), it appears that ε is a zero. Therefore, if the early calculus applies *as a single theory of mathematics*, then two propositions denoting arguments in the steps (2) and (3) are inconsistent with each other.

Of course, if we stick to an algorithmic style of treating infinitesimals, it appears that no inconsistency exists, for we only care about the "reals" in steps (1) and (3). The infinitesimals are more of an artefact that brings us from one mathematical object to the other.

Nonetheless, to take the structuralist accounts, e.g., the mapping account, seriously, when mathematics represents, there must be a mathematical structure to be applied. For a structure is defined over a family of objects and a family of relations, and we use propositions to describe these objects and relations. The consistency in the propositions regarding infinitesimals will be translated to the structure, making it impossible to form a consistent structure. Therefore, if we assume a greater, hidden mathematical structure to back up the entire algorithm and the early calculus, the mapping account fails.

3. McCullough-Benner's First Critique and Responses

This inconsistency in the early calculus has been recognised by many scholars. The partial structure variant is a theory accommodating the inconsistent case. The strategy is to place the relations that we aim to represent in block R1 that belongs to the objects, while placing other idealised, abstracted or inconsistent relations in block R2 that does not belong to the objects, or R3 that it is indeterminate whether they belong to the objects or not. Regarding the calculus of infinitesimals, we can place items containing infinitesimals in R3, retaining an indeterminate attitude towards them when we have no idea how to justify or explain the infinitesimals consistently. In this way, we can keep a consistent physical interpretation of items in R1, thereby forming a consistent scientific representation that makes the inconsistent use of infinitesimals intelligible by the lights of structuralism.

I will start by outlining McCullough-Benner's first critique of the partial structure variant and his robustly inferential account. I will argue that his account does not have advantages over the partial structure variant in terms of how mathematics constrains physical representations because it does not explain why some inference pattern is privileged over others. To these ends, I will suggest that the robustly inferential account is based on certain structuralist programme.

The first criticism is that the partial structure reconstruction of the application regarding infinitesimals does not specify a constraint on the physical representation. McCullough-Benner (2019: 9-10) notes that:

> There is more than one partial structures for the representation with the same accuracy condition i.e., with the same structural content grasped in the R1.

For example, the items containing infinitesimals can be put in R2 and will not belong to the target system. The same representation of the target system will obtain even if a different partial structure is adopted. The notion of partial structure is too plastic to select a right partial structure to constrain what a target system ought to be.

McCullough-Benner (2019: 15) gives his robustly inferential account, the central thesis of which is:

> "… mathematics places constraints on what a target system must be like by specifying inferences that must be valid by the target system."

More precisely, there is a collection of privileged inference patterns that specify algorithms for generating physically interpreted claims regarding the target system. Through these inference patterns, a physical representation of the target can be obtained by adding physical contents to the inferred mathematical result on the basis of the initial physical interpretation of the system. For instance, to obtain the period formula of the harmonic oscillator, what scientists need is to construct an initial setting for the oscillator, which contains information including its mass and restoring force in the system, and do calculus, reaching the relevant mathematical equation and adding relevant physical correlates to the mathematical variables. McCullough-Benner's point is that the oscillation period is constrained, in part, by mathematical inferences scientists have done.

This inferential account is distinct from the structuralist views – that a structural mapping is used to place mathematical constraints on a physical representation. For McCullough-Benner, whether the mapping function exists or not does not matter because the accuracy condition (the structural content) in the physical representation is obtained by making a valid inference from the initial condition of a target system to its resulting physical interpretation, which is specified by the mathematical algorithm.

Unlike the partial structure approach bothered by the issue of underdetermination about the selection of the right partial structure, the robustly inferential account neatly fits our practice of applying the algorithm of taking the derivative of a function above as we only need consider whether propositions concerning mathematical objects in step (1) (the real-number function) and step (3) (the real-number derivative of the function) are consistent with each other, as well as whether this algorithm works for our purpose of applying it. It is the inference captured by the algorithm, which restricts what a target system ought to be after the algorithm applies. As McCullough-Benner illustrates,

> "…the robustly inferential conception can represent [the constraint on representations of a target system] in a way that more *directly captures* the information about the practice of applying the calculus that is ultimately used to determine which partial … structures and mappings are appropriate to represent a given application. Which structures and mappings are appropriate *are largely determined by* which inferences scientists allowed themselves to make on the basis of the relevant mathematics. The mathematical part of these inferences *is directly captured by* the privileged set of mathematical inference patterns, while the physical part is captured by the partial physical interpretation of the mathematical vocabulary." (ibid: 20 emphasis added)

In short, the privileged set of mathematical inference patterns plus physical interpretations is what is *doing work* for a representation.

I disagree with this claim – the robustly inferential account, as a meta-level theory of how the early calculus works, at best *describes* what early mathematicians were doing, but *does not explain* what they were doing. (The demand to explain is not necessarily to justify the early calculus with a rigorous proof, but only to provide a story of why people practicing the calculus selected a particular inference pattern over others.) The robustly inferential account appears to be a mere generalised restatement of mathematical practice with a specific inference pattern in a specific period. However, what matters here is why this specific pattern was picked up. If the application merely means following an algorithm and adding relevant physical interpretations at the final step of the algorithm, then it is unclear what role infinitesimals play in making this inference for taking derivatives.[15]

In addition, if there is no principled account of why one should adopt the inference pattern,

[15] McCullough-Benner (2019: 21-22) indeed is aware of this shortcoming. As he says, "[i]f none of the intermediate steps in calculating a derivative are physically interpreted, the derivative is treated as a black box in the physical representation, making it mysterious why this procedure yields the function representing an object's velocity when applied to the function representing its displacement." However, I do not see how he responds to this shortcoming. He appears to hold a pragmatic justification of his account: "That said, despite these shortcomings, such a representation [the representation requiring "inferential restrictions"] can be very useful both when no suitable representation appealing to consistent mathematics is available and when it is simply more computational convenient to continue to use the representation appealing to inconsistent mathematics." (ibid: 22) But, this pragmatic justification would trivialise the robustly inferential account, since it appears to demand a meta-level theory of applied mathematics *only* to describe what mathematicians are doing – indeed, when mathematicians work, they always make inferences. Also, this implicit appeal to pragmatics does not answer the question of why infinitesimals matter for the algorithm of taking derivatives. Last, the pragmatic justification can be also used by the partial structure theorists to restrict an appropriate partial structure for consistent physical representations.

from the function in step (1) to its derivative after removing items containing infinitesimals in step (3), instead of the inference pattern from step (1) to (2) where infinitesimals remain, then it would face the same underdetermination issue that McCullough-Benner places on the partial structure approach.

McCullough-Benner might object that my critique just focuses on the algorithmic part of making a mathematical inference. He would appeal to what he calls "inferential restrictions" to restrict the practice of applying mathematics in an inference pattern other than others (ibid: 21). The inferential restriction includes mathematical inference patterns, logical rules, and the demand of consistency for the part of mathematics being physically interpreted. For example, to find tangents of curves, we should use the algorithm of calculating derivatives, rather than the algorithm in which infinitesimals remain, both because if the infinitesimals remain, then we only obtain a secant, but not the tangent, of the target curve, and because we (might) have no idea how to interpret the infinitesimals physically in a consistent way. In terms of these restrictions, we only need consider the "reals," but not infinitesimals, when applying the algorithm at issue and forming a physical representation. In other words, the robustly inferential theorists can cite the restrictions in the local context of making a mathematical inference, to support or motivate their selection of an inference pattern.

However, it is noteworthy that these "restrictions" are *external* to the selection of inference patterns, suggesting that the source of constraints on physical representations is partially in the local context of applying mathematics. These restrictions are what structuralists recognise as "contextual/pragmatic factors" that restrict the reconstruction of a partial structure for a specific target (cf. Bueno & French 2018). The contextual/pragmatic factor includes agents' purpose, idealisation, and which part of mathematics can be interpreted physically in a consistent way. In this way, what McCullough-Benner calls "inferential restrictions" are also open to the partial structure theorists so that they can be well-motivated to model a target with a partial structure. Applying the infinitesimal calculus, early mathematicians were indeed aware that they should not place items containing infinitesimals in R1 to avoid unnecessary inconsistency in making physical representations.

Here, the mere difference between the robustly inferential account and the partial structure account is that the former "directly captures" (or describes) what mathematicians are doing when applying the early calculus. However, this does not suggest that the robustly inferential account explains how mathematics constrains physical representations better than the partial structure account in any interesting way. Concerning the calculus of infinitesimals, scientists must dive into the local context of applying algorithms and locate the "inferential restrictions" to motivate the infinitesimal-involved algorithm of calculating derivatives. Here, a circularity issue looms. The algorithm applies to constrain a physical representation. However, the validity of applying this algorithm comes from the physical interpretations, in the local context, which are used to test which part of (structural) content the algorithm captures is valid and which part is not.[16] Since the interpretations are supported by structural mappings, the robustly inferential account appears to be based on structuralist programmes in a sense that the source of constraints of mathematics on physical representations comes from the success of structural mappings in question.

4. McCullough-Benner's Second Critique and Responses

I will introduce McCullough-Benner second critique and make direct responses from the partial structure perspective.

McCullough-Benner's (2019: 10-12) second criticism is that the partial structure approach does not represent the full range of interpretations regarding infinitesimals for representations of target systems on the basis of inconsistent mathematics. In addition to the most natural view – that infinitesimals are interpreted as a mere artefact of mathematics – there are two alternative interpretations. The first interpretation is to say that there are infinitesimal physical quantities (temporal and spatial) and the instantaneous velocity is explained in terms of the two quantities. A structural mapping is expected for physical representations of the inconsistent conception of the infinitesimal physical quantities (that are interpreted both as zero and non-zero).

[16] A physical representation is a structural content plus a physical interpretation.

McCullough-Benner (ibid: 10-11) argues that the partial structure putting infinitesimals in R3 is misleading – if the items containing infinitesimals are placed in R3, then the target system must be taken to be "partial" in a partial structure sense (that infinitesimal physical quantities are neither non-zero nor zero), which is absurd, and given this absurdity, we conclude that it is impossible to form a physical representation with inconsistent use of infinitesimals. However, this is not what motivates us to claim that such a representation is impossible. The physical representation is impossible, not because it is impossible for a target system to instantiate a partial structure, but because it is physically inconsistent (the infinitesimal physical quantities are both zero and non-zero).

But, concerning this interpretation, McCullough-Benner misconstrues the partial structure approach. The partial structure approach does not demand that there must be only one single partial structure for applying mathematics. To stratify the interpretation above, we should put the infinitesimals in R1, since we aim to represent them! Obviously, it is impossible to form a consistent structure with infinitesimals in R1. This impossibility entails that it is impossible to form a consistent physical representation based on an inconsistent conception of infinitesimal physical quantities. In terms of this, the partial structure approach can explain the impossibility of physical representation, under the interpretation at issue, in an appropriate way.

The second interpretation by McCullough-Benner (2019: 11-12) is that there is a physical correlate to the infinitesimals, which is either distinct from treating infinitesimals as an artefact that should not be interpreted physically, or treating them as a cause of inconsistent physical representations. McCullough-Benner draws upon two modern reconstructions of infinitesimals: *non-standard analysis* and *smooth infinitesimal analysis*, which are structurally similar to the interpretation at issue.[17] For example, the central idea of non-standard analysis is to interpret

[17] The strategy of non-standard analysis is to extend the standard universe containing ordinary real objects to a non-standard universe containing both real objects and non-standard objects. Hyperreal numbers constitute such an extended set from the set of real numbers, where "all first-order properties are preserved in the passage to or "transfer" from the standard to the non-standard universe [and the corresponding set]." (Bell 2013) The infinitesimal is defined as a kind of hyperreal a, such that "its absolute value $|a|$ is smaller than $1/n$ for every $n \in \mathbb{N}$" (ibid). Smooth infinitesimal analysis, making use of intuitionistic logic and category theory, reverses the explanatory order between the continuous and the discrete. The notion of continuity should be studied independently, but not explicably in terms of discrete "points." One directly assumes the function $f(x)$ describing a curve is smooth or "infinitesimally straight," i.e. setting $x = 0, f(\varepsilon) = f(0) + \varepsilon D$, for all ε (where D is the slope of the curve). Taking this equation as an axiom for the world of the smooth, one defines the derivative of a function, and derives other rules about derivatives (ibid). Both methods interpret the infinitesimals consistently.

infinitesimals in a way that transfers theorems on properties of “reals” to those involved with infinitesimals. Applying this idea in scientific representations, a special physical interpretation should be given to infinitesimals, which is distinct from the naïve, fixed-quantity interpretation above. McCullough-Benner’s point is that the partial structure approach has no conceptual resources to accommodate this interpretation in question as a partial structure only offers three coarser-grained options to recruit infinitesimals – i.e., existent, non-existent, or indeterminate, but says nothing about the distinct interpretation of the infinitesimals.

Again, this is not a fair criticism of structuralist accounts. First, McCullough-Benner cites non-standard analysis and smooth infinitesimal analysis to support the second alternative interpretation. Why should we not employ the two mathematical theories to form structures for consistent physical representations? For example, non-standard analysis provides a logically consistent extension of the real number system to the system containing infinitesimals and real numbers. Structuralists have sufficient resources to incorporate the interpretation that posits a special physical correlate to the infinitesimals. Second, even if the structuralists are not allowed to use these modern reconstructions, it is noteworthy that it was hard for early mathematicians to distinguish between giving a physical correlate to infinitesimals in a way that makes physical quantities (the “reals”) under investigation consistent *and* interpreting infinitesimals in a naïve, inconsistent way. It follows that the proper choice of them would be to keep an indeterminate mind of whether, and how, the infinitesimal is physically interpreted, and the partial structure putting infinitesimals in R3 is well-motivated.

McCullough-Benner might insist that not all early mathematicians kept an indeterminate mind of infinitesimals or treated them as a heuristic device, but interpret them in a way that his second alternative interpretation suggests. I agree. But his interpretation that gives a physical correlate to infinitesimals but does not say enough to make physical quantities inconsistent, is too coarse-grained to reveal the actual attitude of people practicing the calculus. For instance, Leibniz once interpreted infinitesimals as something real as the real numbers, but changed his mind, treating them as a fiction. Leibniz, in the latter period, was likely to place infinitesimals in R2, even if smooth infinitesimal analysis – the modern reconstruction of his views – tends to place them in R1 (See Arthur 2013). Consider Newton: when using the "method of fluxions" to justify his use of infinitesimals, he appeared to interpret an infinitesimal as a "moment" of a fluxion (the derivative of a function varying in "time") (Kitcher 1972). If he was committed to the existence of "time" and "motion," which generates the continuous curve described by the function, then he might place the infinitesimal in R1. When using the method of "first and last ratios," he appeared to directly justify the method of fluxions and infinitesimals in the synthetic, geometric grounding, where the infinitesimals would not be interpreted and ought to be put in R2.[18]

My point is that McCullough-Benner's second alternative interpretation is too obscure and coarse to marshal the requisite historical back-up to formulate a solid critique of structuralism. If one looks back on figures of early calculus, the proper partial structure, at most times, can be formulated for their interpretations of infinitesimals. For those following bare procedures, but being unserious of their justification, the arrangement of infinitesimals in R3 is proper.

5. Concluding Remarks and Suggestions from the Similarity Perspective

To summarise, I have argued that McCullough-Benner's second critique is unfair to the partial structure variant – this approach can appreciate three interpretations concerning infinitesimals for producing scientific representations. I have also argued that his robustly inferential account does not provide any better explanation of how mathematics restricts physical representations than the structuralist accounts, since his account does not provide any story of why an inference pattern is selected. This is what his first critique concerns.

Nonetheless, I have to concede that I did not give an explicit and perspicuous account of how structuralists explain the source of mathematical constraints on scientific representations although in section 3, I suggested that the robustly inferential account should be based on some structuralist programme. Particularly, I did not account for the role in infinitesimals of forming

[18] Here, Newton's "method of first and last ratios" implicitly appeals to the Archimedean Axiom – that if a is a geometric quantity, there are always b and n, such that $b < a$, and $bn > a$ (Arthur 2008). Obviously, the infinitesimal does not satisfy this Axiom and should not be interpreted geometrically (and physically for early mathematicians).

specific algorithms that produce accurate physical representations.

Regarding the infinitesimal calculus, I would like to make a suggestion from the similarity perspective. The central insight of calculus is to provide simple approximations of targets and approach accurate representations of them. The use of infinitesimals can be appreciated as an approximating technique and a method for specifying the degree of similarity.

Consider the algorithm of taking a derivative of a function $f(x)$ at P $(x_P, f(x_p))$ in section 2. This algorithm can be visualised as follows:

(1) Construct a secant of the function curve (through P and Q $(x_P+\varepsilon, f(x_p+\varepsilon))$, the slope of which is $\frac{f(x_P+\varepsilon)-f(x_P)}{\varepsilon}$, where Q is a point at this curve in the neighbourhood of P.

(2) As ε approaches zero, Q approaches P, and the slope of PQ approaches the slope of tangent at P. This finding can be justified (non-rigorously) based on numeral data or geometric demonstration.

The role of infinitesimals can be grasped as *a variable controlling the margin of error*, such that the use of infinitesimal enables the slope of PQ is as close as that of the tangent *as desired*.[19] It is well-motivated (albeit non-rigorously justified) to believe that as ε 'becomes' zero, the slope of PQ (the approximation) will become that of the tangent – the accurate representation of the target. This also motivates us to remove the multiples containing ε – which indicate the margin of error – and finalise the algorithm from steps (1) to (3).

This idea can apply to integrals. The concept of infinitesimals motivates us to approximate the area of (finite) curvilinear figures by using "infinitesimally small" polygonal "slabs," the summation of which approximates to the target figures. As ε approaches zero, the number of "slabs" n approaches infinite, and we can approximate the target figure as accurately as desired. Given the target figures are finite, it is well-motivated to believe that when n becomes infinite, the multiples containing n will converge to zero, and exact value of the area obtains.

This is admittedly very programmatic and requires further elaboration, but my suggestion is that the use of infinitesimals can be grasped as a matter for specifying the degree of similarity.

[19] This is similar to Leibniz's syncategorematic interpretation of infinitesimals – that *as if* ε is an entity incomparably smaller than finite quantities, but really standing for variable finite quantities that can be taken small as desired (Arthur 2013).

This also appears to explain why an inference pattern is selected.

Part II. Explanation

Chapter Four
A Hotchpotch Picture of Mathematical Explanation: On the Case of Universality and Renormalisation Group

1. Introduction

Recent research has suggested that structuralism fails to accommodate the renormalisation group (RG) explanation of universality (Batterman 2010; Batterman & Rice 2014). Here, the issue is that in order to attain an RG explanation, certain mathematical singularities are required. For example, the degrees of freedom of a target system and the correlation length that quantifies the interactions of the particles in the system must be taken to be infinite. However, there is no mapping function between these singularities and their finite physical targets, suggesting that structuralism does not capture the explanatory role of these limiting operations. Moreover, it is also argued that an RG explanation is obtained by showing the independence of universality phenomena from irrelevant micro-details. However, it would appear that structuralism does not provide any insight in this explanatory aspect of RG, since it understands applied mathematics in terms of a mapping relationship between the world and mathematics. Bueno and French (2012) respond to these criticisms by stating that structuralism – the theory of mathematical representations – need not provide an account of the RG explanations or the explanatory roles of the mathematical operations in question; rather, it merely provides a framework for accommodating them. I believe this response is on the right track. However, there exist two attendant lacunae. First, what does it mean to say that structuralism provides a framework for an account of mathematical explanation? Second, what is a principled account of RG explanations? Only after addressing these aspects can it be shown how structuralism accommodates RG explanations.

With a schematic illustration of the ‘framework’ responsibility that structuralism claims and a close study of the RG, I propose a *hotchpotch* picture of mathematical explanations, that is, an explanation is an iterative cognitive activity (with identification, inference and justification) built up using multiple stages. Mathematics contributes to the explanation in a distinct way at each stage. Given this picture, I argue that the structuralist framework accommodates an RG explanation in an 'unsurprising' way in that the explanation is analysed in terms of several distinct conceptions of explanation, a number of which are anchored in structural mapping functions.

Philosophers often formulate their theories by drawing on so-called 'toy cases'. The cases 'prime life-periods of cicadas', 'the honeycomb theorem', 'the tourability of Königsberg's bridge', 'the failure to unknot a trefoil knot', 'Plateau's laws', 'the failure to evenly divide 23 apples to three children', etc. frequently appear in discussions,[20] and they are often reinterpreted as evidence to defend or introduce an ambitious philosophical account of mathematical explanations. This use of toy cases is pedagogically conducive and communicatively easy. But, a negative effect of these cases is to produce the inappropriate impression that an explanation in its entirety should be appropriated by a single theory, as well as a misleading agenda in determining that what theorists need to do is to expand their theories to other cases, or to divide up the 'territory' for their own theories in opposition to others.[21] The problem of the heavy reliance on such toy cases is that, at best, they present an oversimplified picture of explanatory practices, that is, they do not reflect the complexity of a scientific project, which requires decades of reworking and refinement of previous works from scholars across various fields. The RG explanation is not a toy case, since it involves a summary of empirical data, a guess regarding the explanatory relationship, a justification of the use of minimal models, etc. The RG reflects the complexity of an explanatory practice in daily science. In characterising its explanatory nature, it is somewhat tenuous to apply one single theory to such a complicated task. As such, I suggest analysing this task in terms of distinct aspects and applying different theories of explanation to each of them.

This chapter is structured as follows. Section 2 addresses the question of in what sense

[20] I gathered these cases from Colyvan (2011), Pincock (2007; 2015), Lyon (2012), and Lange (2013).

[21] This style of agenda-setting goes like this. Scholar *A* searches for an unusual case in scientific practice, formulating a novel theory about it to challenge the dominating theory. Scholar *B*, the supporter of the dominating theory, replies that 'the theory you formulate is more like a restatement of the case. The case you represent does not fit into any pre-existing theory, so it is not even explanatory.' The debate often reaches a dead-end if scholar *A* states: 'we should respect scientific practice in reality.' One can see this dialectic style, for example, in Pincock (2015) and Khalifa (2019).

structuralism can present a framework for mathematical explanations, while sections 3 and 4 are dedicated to a case study involving RG explanations of universality. Here, I propose a hotchpotch thesis to characterise an RG analysis and justify this thesis through a subtler exploration of its explanatory structure. I also argue that existing theories of explanation commonly thought of as a competing pair can be integrated into a unified account of RG explanations in such a way that each of the theories characterises each aspect of RG. Finally, in section 5, the question of how structuralism accommodates RG explanations is addressed.

2. Structuralism as a Framework for Accommodating Scientific Explanations: Three Conceptions

Although it is commonly agreed that structuralism does not, and need not have a principled account of the explanatory role of mathematics, it is also agreed that it provides a framework for the account (Pexton 2014; Bueno & French 2012). I will explore three conceptions of how this framework is cashed out.

I will begin by clarifying the 'substantive' role of mathematics in scientific explanations, which a structural mapping aims to accommodate; then, indicate three conceptions of how this 'substantive' role is achieved in the structuralist framework.

The Substantive Role of Mathematics in Explanation

Recall Bueno and Colyvan's (2011) inferential conception: Mathematics is applied through a three-stage inference: interpretation, derivation, and interpretation.[22] It is commonly agreed that mathematics can contribute to an explanation in the stage of derivation or interpretation (Bueno & French 2012; Pincock 2012; Bueno & Colyvan 2011). I take this as an assumption, since our purpose in this chapter is to find how a structuralist framework accommodates the explanatory role of mathematics in scientific explanations. So, the next two questions are: First, is there any difference between explanatory contributions in derivation and interpretation?

[22] I adopt the inferential conception for discussions as it provides a more flexible picture than the mapping account.

Second, if so, what, specifically, is the difference?

Saatsi's (2016) distinction between "thick explanatory role" and "thin explanatory role" of mathematics can respond to the two questions. With a 'thick' role, mathematics bears an "ontic relation of explanatory relevance to the *explanandum* in question" (ibid: 1056). Mathematics represents an objective relation or mechanism that is explanatory. Mathematics playing a 'thin' role is a mere device for one to identify the explanatorily relevant factors. Thus, the difference between the explanatory contributions in derivation and interpretation is that the latter indicates an ontic explanatory structure, which accounts for *explananda* facts, while the former merely allows us to capture the structure.

Nonetheless, I think we can hold a subtler distinction between Saatsi's distinction and what the inferential conception requires if we remove the taken association between the notion of 'explanatory relevance' and the ontic conception of explanation. Specifically, the contribution in interpretation contains relevant information, which accounts for *explananda* facts, no matter whether this 'accounting for' is cashed out in an ontic explanatory structure, or an inferential relation, or whatever other appropriate ways. Let us call this kind of contribution as *substantive*. In contrast, what we might call an *instrumental* contribution is what allows us to indicate information playing a substantive role. Note: I am not saying that Saatsi's distinction is flawed. Nonetheless, one can adopt the subtler distinction to make the structuralist framework more flexible and accommodate a larger class of explanatory roles.

Let us illustrate this (subtler) distinction with two cases.

Cicadas. Many species of cicadas have a similar life-cycle of 13 or 17 years. To explain this fact, in addition to the biological law (that a life-cycle period minimising interaction with other periods is evolutionarily advantageous) and the ecological constraint (that the cicadas under investigation are constrained to periods from 12 to 18 years), we must also cite a number-theoretical fact (or a generalised biological principle) that the prime period minimises intersection (see Baker 2005 for details).[23]

[23] There is a debate about whether the mathematical fact is necessary here (see Baker 2009 for details). My view is that even if the mathematical fact is not necessary, it can still contribute to an explanation by indexing the generalised biological principle that is explanatory.

The Thermodynamic Limit. In an explanation of universality, by taking the thermodynamic limit (the degrees of freedom of a system approaches infinity), one can represent the diverged correlation length, and rule out scale-sensitive factors, which further allow one to define a new class of universal phenomena, and to identify explanatorily relevant factors (see Batterman 2010, 2019; Pexton 2014; Saatsi & Reutlinger 2018 for details).

In the *Cicadas* case, the number-theoretical fact contains the relevant information, from which one can interpret or derive the *explanandum* fact. Similar cases include Taylor's introduction of (M, ε, δ)-minimal set for an explanation of Plateau's laws; the graph-theoretical explanation of tourability of Königsberg's bridges system (Lyon 2012; Pincock 2012, 2015). Among them, the mathematical theorem or fact contains what one can draw on to explain a physical fact. On the other hand, the thermodynamic limit is more like an ineliminable tool, which allows us to address an *explanans* function (or a function indexing a non-mathematical *explanans* principle) and derive the wanted results from them. Similar cases include various continuum idealisations of physical properties – so that we can take derivatives or integrals of them, which contains the explanatorily relevant information.

Three Conceptions of Explanations

Given that the instrumental role is to identify information playing the substantive role, if structuralism can accommodate the substantive role, then it can accommodate the instrumental role, so let us focus on the substantive role. I will now review three conceptions of explanation as to how structuralism accommodates them.

They are INDEXING, DEPENDENCE and INFERENCE. The first two belongs to the ontic conception of explanation, and the last to the epistemic conception.[24]

1. INDEXING. Mathematics contributes to an explanation by indexing (or representing) the

[24] In order to have a completer taxonomy for these options, I here adopt Salmon's (1984) three conceptions of explanations: ontic, epistemic and modal. The ontic conception reveals the *explanandum* fact in an ontic structure of the world. The epistemic conception provides an understanding to the *explanandum* fact. The modal conception is to render the *explanandum* fact more necessary (typically mathematically or logically) (also see Lyon 2012; Lange 2013). The three options are categorised to ontic ("INDEXING" and "DEPENDENCE") and epistemic ("INFERENCE") conceptions, respectively. I do not consider the modal one, since it is unclear how a mathematical fact 'renders' the *explanandum* fact more necessary without any ontic conception (Povich forthcoming); since the so-called modally stronger fact is easily slipped to a modally weaker physical generalisation (Pincock 2015; Jansson & Saatsi 2019).

(non-mathematical) objective structure or mechanism that accounts for the *explanandum* fact (e.g., Pincock 2012; Bueno & French 2012).

2. **DEPENDENCE**. Mathematics contributes to an explanation in virtue of an ontic dependence between the mathematical entity (and its property) and the *explanandum* fact (Pincock 2015; Povich 2019).
3. **INFERENCE**. Mathematics contributes to an explanation in virtue of an inferential relation from a set of premises (at least, one of which is the ineliminable mathematical fact) to the *explanandum* fact as a conclusion (e.g., Baron 2019).

Concerning INDEXING:

This conception shares the central insight of structuralism that mathematics applies by carving structural information from the world. Our focus lies in how to characterise the ontic structure or mechanism, in which the *explanandum* fact is situated. In this chapter, I will focus on the 'counterfactual dependence relation' because it is taken as the most promising, general type of relation to unify both causal and non-causal explanations, and because, as the current literature suggests, most explanatory relationships are a counterfactual relation between the *explanans* and the *explanandum* variables.[25]

It can be argued that the INDEXING role does not in itself provide a principled account for explanations.[26] I agree. Nonetheless, our purpose is about the role of mathematics in scientific explanations rather than merely about distinctively mathematical explanations, so I believe it is fine to include the indexing role into discussions.

Concerning DEPENDENCE:

According to this conception, the ontic explanatory structure does not reside in the world (the empirical set-up), but lies in between the world and mathematics. Based on Povich's (2019) arguments, there are two interpretations of ontic dependence: the instantiation relation and the grounding relation. My suggestion here is that the former is captured by a structural mapping,

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[25] See Woodward 2003; Reutlinger 2016

[26] Thanks to Juha Saatsi for indicating this point.

or even appeals to the indexing role above, while the latter cannot.

The first interpretation is to say that mathematics explains an *explanandum* fact because the fact instantiates a mathematical structure (Pincock 2015; Povich 2019). To clarify, one can refer to Shapiro's (1997: 248) account of application of mathematics: "mathematics is to reality as universal is to instantiated particular."[27] That is, if mathematics offers an abstract structure which is isomorphic to a physical reality characterised by a law, then the system of related objects governed by the law instantiates the mathematical structure. The explanatory force comes from the instantiation dependence relation, i.e., what it is possible or impossible for a mathematical structure is, and explains, what the system of objects can do or cannot do. The isomorphism (or a structural mapping, in general) constitutes the instantiation relation and thus the explanatory relation. In terms of this, structuralism can capture the 'instantiation' type of explanatory dependence.

Also, since it is difficult to distinguish between the mathematical structure and the physical principle expressed mathematically when formulating an explanation, the instantiation relation can be reinterpreted as an appeal to the indexing role above. That is, the mathematical structure instantiated can be viewed as a representation of an abstract physical principle that explains the *explanandum* fact.[28]

Another candidate of the dependence relation is a 'grounding' or an 'in-virtue-of' relation (Povich 2019). One motivation to adopt the notion of grounding is that both the grounding relation and explanation relation are asymmetric. For example, mother's failure to divide her 23 strawberries evenly among her three children is grounded in, and is explained by, the fact that 23 is not divisible by 3 (ibid: 23). Like Povich, I shall not go in detail to characterise the grounding relation. However, I should indicate that it is hard to see how the mapping function can bear the 'dependence' relation in question, since the mapping function is symmetric, and the grounding is not. Also, given the *ontic* nature of grounding, structuralists must provide an account of why a mapping function can capture the explanatory element embedded in a

[27] Thanks to Juha Saatsi for indicating this point.

[28] The stationary action principle is such a case. Formulating Newtonian description of a conservative system to a Lagrangian description, one can derive that the action of the system is stationary to the first order. This mathematical result can serve as a more general physical principle to understand evolution of the conservative system.

grounding, in addition to the representational content, and they are not allowed to excuse this account by saying that an explanation is a cognitive activity along with representation (cf. Chakravartty 2010). Lastly, to see the ontic dependence as a grounding, and to connect the grounding and explanatoriness still require an argument. However, the structuralists still lack the account or the argument.

Nonetheless, I suggest that although the grounding relation is not a structural mapping, or cashed out in virtue of the mapping, it is still possible for the mapping at issue to characterise the structural correspondence between *explananda* and mathematical facts, which is generated by a grounding relation of the former in the latter.

Concerning INFERENCE:

Unlike the conceptions above that associates the 'explanatory relevance' with the ontic structure of the world, the inferential conception of explanations defines what is explanatorily relevant as a special kind of inferential relations from mathematical facts to *explanandum* facts. I here refer to a DN-type theory developed by Baron (2019) – the key idea of which is to grasp the explanatory relevance between mathematical and physical facts in terms of an 'information-containment' relationship. Baron treats extra-mathematical explanations as arguments guided by relevance logic, and the relevance relation as an 'information containment' relationship.[29] I recognise that the Baronian DN theory might not provide an entire account for some special version of extra-mathematical explanations.[30] Nonetheless, it does not mean that the appeal to 'information containment' does not reveal any insight of what mathematics contributes to an explanation. I assume that this information containment can suffice for the relevance relation,

[29] "What it means to say both that $\Gamma \vDash \Delta$ only if Γ is relevant to Δ and that $\Gamma \rightarrow \Delta$ only if Γ is relevant to Δ is this: (i) all of the information contained within Δ is contained in Γ and (ii) each member of Γ contains some part of the information in Δ." (Baron 2019: 700)

[30] I make two quick defences for Barron's DN or DM (M for mathematics) theory. Povich (2019) critiques that Baron's theory fails to characterise distinctively mathematical explanations (DMEs), since it does not satisfy the "distinctiveness desideratum" (DMEs are distinguished from what are not) and the "directionality desideratum" (the directionality of DMEs). I accept these two critiques, but make two following points. First, the failure to satisfy the "distinctiveness desideratum" is unfair to Baron's DM theory, for DM's first constraining condition "Razor-Sharp Essential Deductive Constraint" only aims to give *a ranking rule* to justify why extra-mathematical explanations are genuine *compared with their purely physical counterparts*, but not a rule making extra-mathematical explanations unique from any other types as this will make the 'comparison' above impossible (Baron 2019: 693). Second, the directionality of DMEs should not be the focus. The directionality often depends on an agent's purpose in forming explanations, and should be counted as being independent of an account of the DMEs. The focus should lie in *the explanatory relevance* between mathematical and physical facts.

and this relevance can suffice for an explanatory relationship. I will explore two options of how information containment is related to explanatory relevance.

For starters, let us note that no consensus has been formed about the nature of information. Still, it can be intuitively defined that a proposition gets its information from the fact that makes it true (Baron 2019: 701). The Pythagorean theorem gains its information from mathematical facts about the rectangular triangle, lengths of its sides and their relationship. That an apple will fall to the earth gains its information from the facts about the apple and the earth, and the information is made true by the trajectory of the apple.

So, here is the issue: if the claim 'information A contains information B' is to say that 'B is a part of A,' then, in an extra-mathematical explanation, how is it possible for a claim (about a mathematical fact) to contain physical information? This leads us to two possible resolutions. The first one is to hold that although mathematical and physical facts are ontologically distinct, their information can be structurally mapped (Baron 2019: 706-7). The non-descriptive modal information can be translated from a mathematical structure, through a structural mapping, to a specific physical system, and determine what can occur and what cannot in the system. If this is plausible, then one can obtain an informational containment and an inferential, explanatory relation in virtue of a structural mapping from a mathematical structure to a physical set-up.

Nonetheless, this is not an appeal to the indexing role of mathematics. This is because what an explanation draws on *is* a mathematical fact; the explanation is constituted by an inferential relation with the relevance logic. One can infer modal information from a mathematical fact to an *explanandum* fact in virtue of a mapping (suppose this is what an explanation is about), but this mapping *per se* is not sufficient for the explanation (Baron 2019: 709).

The second option is to reverse the information containment relationship by claiming that the *explanandum* fact carries mathematical information (Baron 2019: 711). To make this idea clearer, we can cite Rizza's (2013) case study on Arrow's theorem. Without going into the study in detail, the interesting thing Rizza illustrates is that mathematics can be applied by identifying *formal* properties of an empirical set-up and deducing them to a conclusion concerning the set-up. If one identifies the formal properties as a mathematical kind, it can be concluded that the mathematical concepts, entities and their properties provide information about the empirical set-up. Thus, the set-up and the *explanandum* fact deduced from the formal property in question contain the mathematical information.

Moreover, it should be emphasised that this type of applied mathematics does not need a mapping between empirical and mathematical structures: one can reason mathematically about the formal properties directly. The structuralist framework would fail to accommodate this type of explanatory contribution.

I concede that Rizza's argument indeed indicates a lacuna of structuralism concerning how an empirical set-up is formed (also see Nguyen & Frigg 2017). Nonetheless, I should indicate that Rizza does not provide an account of formality and seems to conflate the conception of a physical property with that of formal or mathematical properties.[31] My suggestion is that once one can unitarily articulate or formulate the conception of formality and mathematics in a set-theoretical basis, one can distinguish a physical property (no matter how abstract it is) from its formalised counterpart, which is extensionally defined (Nguyen & Frigg 2017).[32] For example, the relation of 'being higher than' can be formalised as an order pair (x, y) over a set of dummy objects. Mathematical reasoning deals with the order pair, instead of the physical relation of height comparison. Mathematical concepts apply to grasp structural information of formalised physical concepts. It follows that Rizza's 'argument' picture of extra-mathematical explanation threatens to collapse to intra-mathematical ones. Therefore, to account for extra-mathematical explanations, the issue is not whether a physical fact carries a formal property, but how a formal property is generated from its physical counterpart. *The process of formalisation* grounds the explanatory relevance between physical and mathematical concepts, in a sense of providing a novel conceptual connection between them.

[31] Rizza seems to assume that a formal property exists in a physical system, and it is quite natural to isolate it from the system. However, as Nguyen and Frigg (2017) argue, this is not a natural move, but requires a cognitive effort that translates a physical relation to a formal relation, which is linked to a mathematical structure. This issue is not that serious in the context of proof of Arrow's theorem Rizza analyses, for the context of characterising individuals' votes is already highly idealised to specify the formal relations. The issue, however, is how, and why, the context is idealised in this specific way that allows us to deduce formally.

[32] Rizza himself appears not to support this extensional conception of formality. He sees a formal concept (and a mathematical concept) as an intensional one, over which one can reason and infer. This view is correct in the sense that the intension of a concept *motivates* an inference in one direction rather than another. However, it should be emphasised that the inference *operates* according to, or *is justified* in virtue of, the extensional formulation of the concept. Given this, the structural mapping is not surprise to come.

Also, although the process of formalisation is beyond structuralism, one can characterise *post factum* the formalisation in virtue of a mapping, such as, a mapping between the being-higher-than relation and the order pair. Thus, there is an information containment relationship generated by the process of formalisation, which is not, but can be characterised by, a structural mapping.

In sum, we have reviewed three different conceptions of explanation that mathematics might satisfy in scientific explanations. A mapping function can capture the INDEXING conception, the instantiation relation (a part of the DEPENDENCE conception), and a part of the INFERENCE conception. By 'capture' I mean, each conception of explanation is cashed out in virtue of a structural mapping. The structural mapping is constitutive of, and explanatorily prior to, these conceptions. A mapping function can also characterise the 'process of formalisation' (a part of the INFERENCE conception) (and perhaps the grounding relation, a part of the DEPENDENCE conception) By 'characterise' I mean, a structural mapping can describe the correspondence between mathematical structures and *explananda*. Yet, the explanatory relationship between them is explanatorily prior to the mapping.

3. Universality of Critical Phenomena: The Hotchpotch Picture Proposed

I will introduce the universality phenomenon and propose a hotchpotch picture for explanatory inquiry into the phenomenon. More precisely, I will introduce three related *explananda* about universality facts, suggesting that the hotchpotch picture characterises universality explanations more accurately than the existing accounts of mathematical explanations.

Universality of Critical Phenomena and Three Explananda

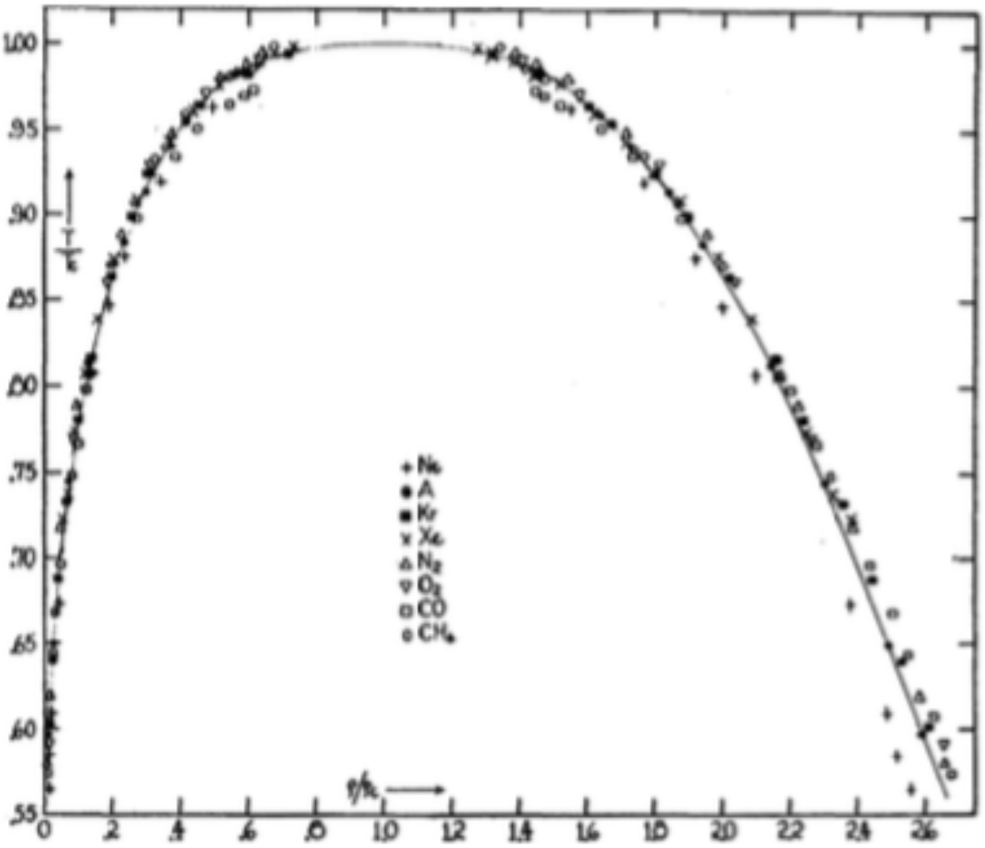
Critical phenomena involve the second-order (or continuous) transition near criticality. To take the water system as an example, there are three phases of water: vapour, liquid and solid. The continuous transition occurs when the water system crosses the critical temperature. Below the critical temperature, if one wants the system to change its phase from liquid to gas, then it must go through a liquid-gas coexistence state. Beyond the temperature, it is possible to change the phase from liquid to gas directly by increasing the pressure and decreasing the pressure and temperature. The water system's transition pattern changes abruptly at the critical point (critical temperature and critical pressure). Call this a continuous phase transition.

The critical behaviour happens to the water system near criticality. A crucial feature in the continuous transition is that the correlation length, which quantifies the collective behaviour of the water particles, diverges at the critical point, meaning that at and near criticality, there will emerge some macroscopic, collective behaviours in the water system. More specifically, some thermodynamic properties (e.g., liquid-gas densities, heat capacity, and compressibility) obey power laws with a characteristic critical exponent (a dimension-less constant), as a function of 'reduced temperature' t, which measures how close a system's temperature T is to its critical temperature T_C (where $t = \frac{T-T_C}{T_C}$) (Saatsi & Reutlinger 2018). To take the simplest case: the order parameter Ψ (that denotes liquid-gas densities) obeys a power law such that:

$$\Psi \propto t^{\beta}$$

where β is a critical exponent. The critical behaviour of the water system is characterised by the critical exponent (Batterman 2010).

The remarkable experimental fact is that the same critical exponent characterises various 'fluid,' and ferromagnetic, systems that are distinct in their molecular structures. One can learn this remarkable fact more intuitively from the (Fig. 4a) below: the coexistence curves for eight distinct fluids near the critical point collapse into the identical one, indicating that they are all characterised by the same critical exponent.



(Fig. 4a Universality of Critical Phenomena Guggenheim 1945 cited in Batterman 2019)

Here, we have reached the first *explanandum* about universality: why do these systems that are molecularly heterogeneous share the same critical exponent? One can also generalise this type of the universality *explanandum* as follows:

> **UNIVERSALITY-I**. Why do systems that are heterogeneous at a microscale exhibit the same pattern of behaviour at the macroscale? (Batterman 2017; Batterman & Rice 2014)

However, as Saatsi and Reutlinger (2018) indicate, UNIVERSALITY-I is "blunt," for not all systems perform the similar critical behaviour. Rather, the critical behaviour only occurs for systems in a universality class. Different universality classes have different critical exponents, and different critical exponents and universality classes depend upon the spatial dimensionality and the symmetry of the order parameter.[33] Call these two features as the '***common features***' of systems in the same universality class. The table (Fig. 4b) below summarises different scaling (critical) exponents for different phase transitions and how the critical exponents are

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[33] The symmetry of Hamiltonian describes the invariance of Hamiltonian of a system under operations over all spin parameters. Hamiltonian codifies details about the micro-interactions of the system.

dependent upon the spatial dimensionality of systems. For example, 2-dimensional and 3-dimensional magnets belong to different universality classes, while despite striking differences in molecular structures, liquid/vapour systems, superfluid helium and 3-dimensional magnets are all in the same universality class (Batterman 2019: 34).

Phase Transition	Value of β
Mean Field Theory	1/2
$d = 2$ Ising Model Ferromagnet	1/8
$d = 3$ Liquid/Vapor CO_2, Xe	0.35
$d = 3$ Superfluid Helium ^4He	0.359
$d = 3$ Ising Model Ferromagnet	0.315

(Fig. 4b Scaling Exponents for Different Transitions cited in Batterman 2019)

In terms of these examples, the genuine explanatory question Saatsi and Reutlinger suggest asking is: what does a universality class depend upon? Again, this question can be generalised as follows:

> **UNIVERSALITY-II**. Why is there a universality class in which systems exhibit the same pattern of behaviour at the macroscale? (Saatsi & Reutlinger 2018; Reutlinger 2016)

A difference between UNIVERSALITY-II and UNIVERSALITY-I is that the former expresses the sense of robustness, or stability, of macro-behaviours under the perturbation of micro-details, and the latter does not rule out situations that two molecularly distinct systems can happen to exhibit the same property or pattern of behaviours. For instance, an apple and a pen can happen to share the same colour, say, green.

Another difference between them is that UNIVERSALITY-II is answered by indicating the common features that distinguish one universality class from others, while UNIVERSALITY-I is answered by showing the independence of macro-behaviours from micro-details. Based on the two differences, we can formulate a synthetic *explanandum* about the universality fact:

> **UNIVERSALITY-III.** Why is there a universality class in which systems heterogeneous at a microscale exhibit the same pattern of behaviours at the macroscale?

UNIVERSALITY-III is stronger than UNIVERSALITY-II because it rules out the coincident sense that it would happen to be a certain universality class, as shown by experimental data (Fig. 4b), correlated with the common features in question. In this sense, to answer UNIVERSALITY-III, one must also answer the question of why a universality class depends upon the dimensionality of systems and the symmetry of order parameters (once the range of microscopic interactions of the systems is given). This is one sense of saying that UNIVERSALITY-III is a composite question: to explain the universality, one must (a) find what a universality class depends on and (b) justify why the class is dependent upon what it depends upon.[34]

There is another simpler sense of treating UNIVERSALITY-III as a composite question: (i) Why is there a universality class? (ii) In the given class, why do systems heterogeneous at a microscale exhibit the same macro-behaviour? This 'composite' feature is associated with the methodology in answering the universality *explanandum*. That is, one must rely on *a* minimal model with the common features and some qualitative features of Hamiltonians that codify the micro-interactions of the system. Working on this minimal model and showing that the minimal model system can perform the similar macro-behaviour for the target system, one should note (1) that both minimal model and target systems are *in the same universality class* and share the same common features, so to cite the common feature adds no further value to the universality *explanandum*, and (2) that the focus of explanation has been shifted to the question of why the macroscale behaviour of *a* specific system, when it approaches criticality, 'loses' its connection to its molecular constitution, and exhibits the same pattern fixed by the common features. This is an answer to the question (ii) in UNIVERSALITY-III.

Of course, there is an objection that the appeal to the question (i) (or UNIVERSALITY-II) is independent of the question (ii), since (ii) is only about why the minimal model is justified; this is a different question from an inquiry about the universality (Woodward 2018a). I disagree with this objection, for two reason. First, an answer to (ii) provides more than a justification of

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[34] Compare UNIVERSALITY-III with another 'composite' question – "how is universality possible?" Batterman (2019) treats this how-possibly question as a 'composite' question of (1) why molecularly distinct systems exhibit stable critical behaviours (UNIVERSALITY-I) and (2) why a universality class depends upon relative common features. Note that these two sub-questions combined do not answer the question of how universality is possible, but an incomplete form of UNIVERSALITY-III. First, to answer a how-possibly question, one only need to find the precondition of the possibility of universality facts, i.e. the common features. In this sense, the how-possibly question is UNIVERSALITY-II. Second, by "incomplete" I mean, an answer to UNIVERSALITY-I is too weak to answer UNIVERSALITY-II, which is one part of UNIVERSALITY-III. Given the two observations, Batterman's how-possibly question ignores the explanatory contribution concerning UNIVERSALITY-II. As I will illustrate in section 4, this issue is related to his implicit reference to 'real-space RG' when he interprets it.

our use of the minimal model. (ii) is associated, as I will illustrate below, with the central insight of RG transformations: to filter out the order parameter, whose Hamiltonian is rescaled from the micro-interactions, and to rule out what is not rescaled. Second, even if (ii) was just about the justification of the use of minimal models, the justification of their use should be contained in our inquiry about question (b) – why a universality class is dependent upon the common features – and, thereby, about question (a)/(i)/UNIVERSALITY-II. The reason is simply that the entire RG analysis is done on the idealised system provided by the minimal models; this is too strange to exclude the justification for their use from our investigation in question.

To summarise, a universality explanation starts with UNIVERSALTY-II: what a universality class depends upon. To rule out the coincident correlation between the university class and the common features, one must answer the question of why a universality class is dependent upon relative common features. Due to the use of minimal models in the RG analysis, this question is answered in the context of 'in the same universality class.' This suggests one appealing to the answer of UNVERSALITY-III – why there is a universality class where systems molecularly heterogeneous exhibit the same macro-behaviours – as the strongest and the most appropriate question for the universality fact. The strongest, since it rules out any coincident connection between *explanans* variables and the universality fact. The most appropriate, since it considers the methodological character of the RG explanation.

Traditional Views and the Hotchpotch Picture

The inquiry into universalities challenges the traditional methodology in the philosophy of explanation. It is often assumed that an explanation can be formulated individually in a formula that codifies an explanatory relationship between *explanans* factors and *explanandum* facts, or an inference between them, and that a composite *explanandum* (e.g., UNIVERSALITY-III) can be sliced as independent sub-questions that are answered independently.

However, it seems to me that the answer to UNIVERSALITY-III is not a one-stage process, but a multi-stage process of identifying the common features, inferring from the features to the universal critical behaviour, and justifying the universality by ruling out irrelevant details that distinguish target systems far from criticality. This multi-stage process is iterative: one must continuously rework on the previous stage and reach a further stage until UNIVERSALITY-III is answered. Call this iterative, multi-stage style of explanation ***the hotchpotch picture***.

To be specific, it is easy to locate a formula between the common features and universality classes (as suggested by the table Fig. 4b). However, a mere citation of the relationship is too weak to answer UNIVERSALITY-III. Instead, it is a beginning stage of the explanatory work in its entirety. Also, it is hard to slice UNVERSALITY-III into two *entirely independent* questions: say, UNIVERSALITY-II (why there is a universality class) and UNIVERSALITY-I (why there is the same critical macro-behaviour among molecularly distinct systems). Instead, the sub-questions for UNIVERSALITY-III are connected: say, (i) why there is a universality class and (ii) *being in a universality class*, why do molecularly distinct systems perform the identical critical macro-behaviour: One is built upon the other.

Given these observation, I suggest that the hotchpotch picture characterises the answer to UNIVERSALITY-III more accurately than the traditional view. In the next section, I am going to show that the subtler explanatory structure of RG analyses fits this picture.

4. The Subtler Structure of RG Explanations: The Hotchpotch Picture Justified

The preceding section concerned the ‘question’ side of the universality explanation; this section concerns the ‘answer’ side. I will explore the subtler stages of RG explanations and justify the hotchpotch picture. This section has four parts:

(1) I will offer a big picture of RG explanations and identify two theories of it (the common features and irrelevance theories), and suggest that the irrelevance theory by itself fails to account for RG’s obtaining of explanatory force.

(2) I will focus on one variety of the common feature theory – counterfactual theory of explanation (CTE) – arguing that the CTE *per se* does not have sufficient conceptual recourses to characterise an RG explanation.

(3) I will suggest adding a further interventionist condition to the CTE, in order to make it

characterise an RG explanation, and argue that the irrelevance aspect of RG provides conceptual resources for this addition of the interventionist condition. It follows that both relevance and irrelevance aspects of RG can be drawn upon together to develop an interventionist CTE to capture RG's explanatory character in its entirety.

(4) There is an objection that the addition of the interventionist condition does not require the irrelevance aspect of RG, for the 'irrelevance' of irrelevant details is conditional on the identification of relevant 'common features.' I argue against it by indicating that in RG explanations, the irrelevance at issue is conceptually independent of the relevance of common features.

After all these, I will show that RG explanations fit the hotchpotch picture, and in section 5, I will return to the issue of how structuralism accommodates RG explanations.

The RG Explanation: Common Features and Irrelevant Details

The central idea of RG is to reduce the number of modelling parameters that characterise a system's behaviour. The correlation length approaches infinity near criticality, and there is no characteristic scale to measure between the atomic/lattice spacing and continuum (Batterman 2019). One must find a scale-invariant structure that fluctuations can be continuously rescaled (or renormalised) from a microscale to a macroscale and a 'coarse-grained' rule to cash out the renormalisation operation. In this process of renormalisation, details about micro-interactions will be washed out, and the long-distance (or system-wide) behaviour will emerge. The striking thing is that a large class of systems, under this renormalisation operation (or a RG flow) are attracted to the same 'fixed point' i.e. a topological structure in parameter spaces characterising each system, which gets mapped to itself (Saatsi & Reutlinger 2018). By studying the property of this fixed point, one can reveal and explain the critical phenomenon across all these systems.

Specifically, let us consider an Ising model, which consists of a *d*-dimensional cubic lattice with *k* basis vectors. Its Hamiltonian $\mathcal{H}$ can be formulated as follows:

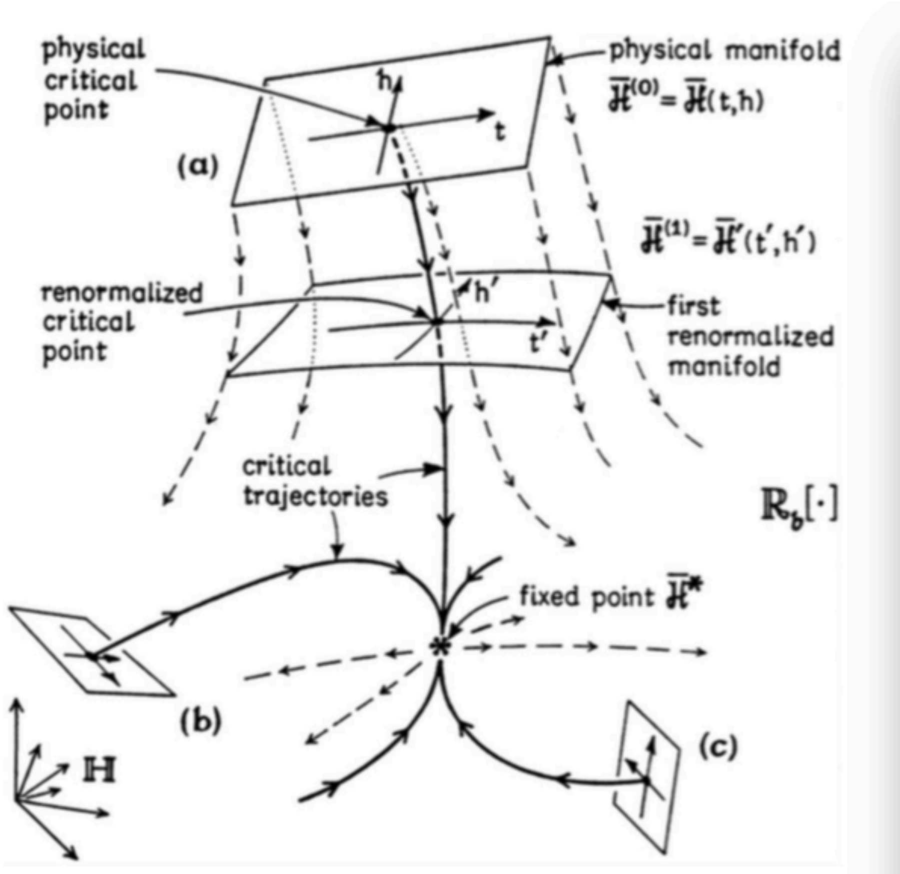
$$\mathcal{H}(d,n) = -j\sum_{k,k+\mu}\sigma_k\sigma_{k+\mu} - B\sum_k \sigma_k$$

This $\mathcal{H}$ characterises the interaction energy between components of a system and the effect of the external condition (e.g., a magnetic field) on this system. $\sigma_k \in \{\sigma_{k,1}, \sigma_{k,2}, \ldots \sigma_{k,n}\}$ is the spin parameter (a n-dimensional vector), which defines the symmetry number of the system's Hamiltonian. The former block of summation characterises the energy of all interacted pairs of spins, and j gives interaction energy. The latter block characterises the effect of the magnetic field B on the system. As known, different sets of $\{d, n\}$ determine different critical exponents and universality classes (Saatsi & Reutlinger 2018; Franklin 2018).

An RG function $\mathcal{R}$ transforms a set of coupling parameters $\{K\}$ to another set $\{K'\}$ as follows: $\mathcal{R}\{K\} = \{K'\}$ (Franklin 2018: 234). To find the scale-invariant parameter, one must search for a fixed point $\{K^*\}$ that gets mapped to itself under the RG transformation, such that $\mathcal{R}\{K^*\} = \{K^*\}$ (ibid: 234). By studying the vicinity of this fixed point, one can define what parameters are renormalisable and relevant to the occurrence of critical phenomena, and what parameters are non-renormalisable and irrelevant (ibid: 234). This is defined by a new rescaling factor b near the fixed point: b^y (if $y > 0$, then the OP, the functional of the order parameter, is relevant, if $y < 0$, then irrelevant; if $y = 0$, then marginally relevant) (ibid: 234). By deriving the Hamiltonian of the order parameter from the Ising model, one can obtain the approximation scheme for the critical exponent α as follows (ibid: 236):

$$\alpha = \frac{4-n}{2(n+8)}(4-d) + \frac{(n+2^2)(n+28)}{4(n+8)^3}(4-d)^2 + \cdots$$

One can also derive the eigenvalue y, which depends upon the dimensionality of the system under concern and the order parameter. I will not show the detail here.



(Fig. 4c RG flows to a Fixed Point, Fisher 1998 cited in Franklin 2019)

The diagram (Fig. 4c) illustrates RG transformations described above. The $R_b[\cdot]$ denotes the RG transformation with a scaling factor b. Physical manifolds represent Hamiltonians of a class of systems under investigation. When these systems approach criticality, the correlation length diverged, and this allows OPs (functionals of order parameters), or Hamiltonians, to be rescaled in a basin of critical manifolds and to be attracted to a fixed point. The topological structure of this fixed point reveals the critical behaviour in question.

So, in virtue of what does the RG technique explain the universality fact? There have been two competing camps regarding RG's explanatory characters: the ***common features theory*** and the ***irrelevance theory***. The former can be formulated with the following tenet:

COMMONALITY. An RG analysis explains a universality fact by citing common features shared by microscopically heterogeneous systems and showing that the common features are sufficient for the identical macroscale critical behaviour. (Lange 2015; Reutlinger 2017; Povich 2018; Saatsi & Reutlinger 2018)

In other words, COMMONALITY suggests that to explain a universality phenomenon, one must indicate ***the relevant aspect*** of RG explanations: that is, what relevant factors are and how they are related to the *explanandum* universality. More precisely, I think one can adopt what Saatsi and Reutlinger (2018) demonstrate: an RG explains by indexing the counterfactual dependence relation between *explanans* variables (the common features) and the *explanandum* variable (the universality class).

The irrelevant aspect of RG explanations is explored by Batterman and Rice (2014). They depict RG's obtaining of explanatory force in an opposite way:

IRRELEVANCE. An RG analysis explains a universality fact by showing the independence of the critical behaviour from irrelevant heterogeneous micro-details.

There have been many debates regarding which aspect of RG reflects its 'genuine' explanatory character. This is not my interest in this chapter, for I think both aspects are crucial. Nonetheless, I shall still clarify the underlying physics of the two theories and its philosophical implication.

First, I think the divergence between the common features and irrelevance theories is due to the different type of RG they implicitly refer to. As Franklin (2019) indicates, there are two ways of doing a RG analysis: real-space RG and field-theoretical RG. Without going in detail, the key difference between them is that the real-space RG is done through a blocking procedure, during which a 'block particle' replaces a group of particles with an 'averaging' rule, and one can capture the unchanged form of Hamiltonians, while the field-theoretical RG deals with the functional of order parameters directly and identifies the rescaled ones. The common features theory is more like a theory of field-theoretical RG as it tracks the process from the *explanans* features to the critical phenomena. The irrelevance theory dovetails with the real-space RG that does not consider the specificity of target systems and only provides a general framework that applies to any system.

Second, this implicit reference to the real-space RG makes IRRELEVANCE uninformative as a general RG framework does not distinguish significant heterogeneous details from trivial ones. A significant irrelevant detail is what could have influenced a system's behaviour, and when the system's behaviour loses the connection to it, a new universality class or a new class of explanatory dependences appear. A trivial irrelevant detail is what, whenever, will not influence. Concerning the universality of critical phenomena, the significant irrelevant details are non-renormalised parameters and molecular constitutions that determine a system's macro-behaviours when away from criticality. Only after this distinction between two kinds of details is made, one can see why it is crucial to see that phase transition stably occurs for systems in a universality class. Since if not, the RG technique has no special explanatory value, and it is just like claiming that collecting a class of green objects and claim they are green because properties other than colour are irrelevant.[35] In this way, I suggest that the irrelevance theory by itself is insufficient for an answer to UNIVERSALITY-III.[36]

Interventionism and Explanatory Counterfactual

This part will focus on the common feature theory. I will adopt the current agreement that the counterfactual theory of explanation (CTE) is a good framework for characterising an RG analysis (Saatsi & Reutlinger 2018). I will present two arguments. First, I argue that the CTE's three conditions are insufficient to capture an RG explanation. Second, I argue that one should add a fourth interventionist condition for the CTE to apply to the RG, and that RG's irrelevance aspect provides a conceptual resource for adding this interventionism.

Let us start with the CTE. According to it, an explanation is to identify a counterfactual dependence relation between *explanans* and *explanandum* variables: how the *explanandum* phenomenon would be different if the factors cited in the *explanans* has been shifted in a certain way (Reutlinger 2016: 736). There are three conditions to capture an explanatory counter-factual.

1. Veridicality Condition: G1, …, Gm [*explanantia* consisting of generalisations], S1, …, Sn [auxiliary statements], and E [a statement about the *explanandum* phenomenon] are (approximately) true.

[35] See Povich 2018 for more arguments. Povich also argues that the real-space RG cannot show why common features (as a by-product of RG transformations) are important and why there is a universality class.
[36] Batterman (2019) would argue that their theory concerns a more fundamental how-possibly question about the universality. However, as argued above, the how-possibly question ignores the question of what a universality class depends upon and fails to answer the universality fact in its entirety.

2. Implication Condition: G1, …, Gm, and S1, …, Sn logically entail E or a conditional probability P (E|S1, …, Sn) – where the conditional probability need not be 'high' in contrast to Hempel's covering law account.
3. Dependence Condition: G1, …, Gm support a least one counterfactual of the following form: had S1, …, Sn been different than they actually are (in at least one way deemed possible in the light of the generalisations), then E or the conditional probability of E would have been different as well.

Reutlinger argues that the three conditions capture RG explanations. The current literature has paid a lot of attention on whether the veridicality condition holds.[37] This is not my focus in this chapter. My interest lies in the dependence condition. Before checking whether this condition holds, it is better to reflect whether a counterfactual dependence is sufficient for an explanatory relationship in general.

Let us consider a critique from Khalifa et al (2019): the dependence condition is too weak and fails to distinguish an explanatory counterfactual from a non-explanatory one. By referring to the Moore's law (Transistors per computer chip) = $2^{(\text{year-1975})/1.5}$, Khalifa et al (ibid: 3) argue that this empirical law also supports a counterfactual, such that if the year had been 1975, then there would have been only one transistor per chip, and fits the dependence condition. However, the counterfactual and the empirical law are not explanatory, since the fake *explanans* variable "year" is merely correlated with real explanatory factors for the growth of transistor density in chips.

I suggest applying this critique into the relationship between RG's explanatory characters and the CTE. Saatsi and Reutlinger (2018: 473) argue that an RG explanation supports the three following counterfactuals:

1. If a physical system S had a different spatial dimensionality then it actually has, then

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[37] The issue lies in an ineliminable auxiliary condition: the thermodynamic limit seems not approximately true. I only indicate two strategies to fix this issue. One is to argue that although the thermodynamic limit ($N \to \infty$) enables us to deduce a novel, robust critical behaviour under investigation, by taking a very large finite N, one can still obtain the similar behaviour, albeit in a weaker form. Thus, the singular limit should be regarded as an eliminable approximation (and not an idealisation) of the real system (Butterfield 2011; Norton 2012; Belot 2005) The other is to argue that even if this asymptotic limit is explanatorily ineliminable, it only takes an instrumental role, allowing one to identify counterfactual dependence relations that are really explanatory (Saatsi & Reutlinger 2018). This asymptotic idealisation identifies irrelevant factors (the scale-relevant parameters) from the RG explanation; then, filters out scale-invariant parameters as relevant one (cf. Streven 2019). Given this pragmatic sense, one should worry about either the ontological or epistemic burden of this idealisation.

S would be in such-and-such a different universality class than it actually is in.

2. If a physical system S had a different a different symmetry of the order parameter then it actually has, then S would be in such-and-such a different universality class than it actually is in.
3. If a physical system S had a (sufficiently) different range of microscopic interaction then it actually has, then S would be in such-and-such a different universality class than it actually is in.

I agree. However, this does not mean that the dependence condition *sufficiently* characterises the RG explanation. The three counterfactuals threaten to be non-explanatory if we 'view' them from a different context. For example, the table (Fig 4b), which summarises the relationship between universality classes and spatial dimensionalities, also supports the first counterfactual Saatsi and Reutlinger suggest characterises RG explanations. However, there is no motivation to see this counterfactual as an explanatory one because it is simply unclear whether there is a 'relation' or a 'path,' which directs the explanatory force from the *explanans* variable to the *explanandum* universality class. To make an analogy with causal explanations, one must show that this is the 'intervention' on the *explanans* variable, which 'produces' or 'results in' the *explanandum* phenomenon. But, this is unclear where the 'interventionism' or something like this can be captured in the counterfactual provided by the empirically summarised table, which might turn out to be spurious.[38] It follows that holding the dependence condition does not mean that the CTE is sufficient for an RG explanation because the counterfactual can be undermined by the spurious table case and threatens to be non-explanatory.[39]

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[38] As I will argue below, different from the spurious table case, the 'interventionism' can be found in counterfactuals provided by RG explanations. The key issue is that we must demonstrate (and explain) the presumed explanatory relationship between dimensionality and universality is *robust* and independent of micro-details (that is the irrelevance aspect of RG).

[39] Saatsi (in online supervision) gives an objection to this. He argues that we should distinguish between "justification" and "explanation." Empirical data summarised in the 'table,' also enable us to hypothesise a "potential" explanatory relationship that an RG provides. The mere difference between the 'table'-based, and the RG-provided explanations is that the RG justifies, and "actualises," the explanatory relationship. However, whether being "actual" or "potential," or whether being "justified" or not, has nothing to do with the explanatory relationship *per se*. (One can also see a similar argument about connections between "understanding" and "explanation" in Saatsi 2019. Understanding, here, refers to an ability to answer what-if questions about *explanandum* phenomena.) I shall make two responses. First, what matters here is how *a philosophical theory of explanation* characterises explanations in scientific practice. An explanatory practice, when indicating a counterfactual, should also explain (or at least clarify) the mechanism or process of why, and how, *explanans* variables result in the *explanandum* variable. One option stratifying this embedded explanatory (or clarifying) work, is to clear all spurious correlations from the counterfactual or causal path from *explanans* variables to the *explanandum* variable. This interventionist character is what an RG analysis can provide, and not what the 'table' can. The "justification" of the counterfactual is just a by-product of clarification of this interventionist character. Second, although it is possible to conceptually distinguish between justification/understanding and

So, if one wants to retain the CTE as a framework for capturing RG explanations, they must strengthen it in a certain way. My argument is that we can strengthen the CTE by adding a fourth interventionist condition and extend this interventionist CTE to the RG explanations (assuming that they are non-causal explanation[40]).

Let us start with the interventionism, which is typically appropriated to causal explanations. According to Woodward and Hitchcock (2003), a causal explanation is formulated in a way that mirrors an ideal experimental investigation of a target phenomenon. A generalisation or a law is explanatory just in case it offers a counterfactual, which is invariant under intervention. The central role of interventionism is to clear irrelevant details, common causes, and spurious correlations from a causal path from the variation in the *explanans* variable to the *explanandum* variable.[41] Woodward and Hitchcock (ibid: 13-4) argue that a counterfactual is explanatory just in case that it gives information about what the effect variable *Y* would change as a result of an intervention *I* on the cause variable *X*. In this sense, one can see why Moore's law is not explanatory: the change of transistors per chip does not result from an intervention on the year (Khalifa et al 2019). Given this, we can add a further condition to the CTE, which will at least enable it to capture causal explanations and clear out non-explanatory correlations:

4. Interventionism Condition: A generalisation G supports a counterfactual dependence that if there had an intervention on the variable S, which switches its value from x1 to x2 (x1≠x2), then Y would switch from y1 to y2 (y1≠y2) as a result of the intervention.

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explanation, if we strip all epistemic things or understandings from an explanatory practice, then what remains is observable, counterfactual relations between different variables, and the mechanism and process behind these relations are in the 'black-box,' since most of them (e.g., chemical bonds) are idealised or imagined theoretical posits embodied in models for enhancing our understanding of, or our epistemic access to, the counterfactuals under investigation. Saatsi's conception of explanation appears too minimal to satisfy the requisite philosophical theory of explanation.

[40] RG explanations are not obviously non-causal (see Sullivan 2019 for more details). I assume that they are non-causal ones. Nonetheless, I think the distinction between causal and non-causal explanations is not that crucial to our discussion as even if the RG is taken to be non-causal, its explanatory character has inherited interventionist elements from causal explanations.

[41] Woodward and Hitchcock (2003: 12-13) define an intervention variable *I* for *X* (a cause) with respect to *Y* (an effect) as follows:

(1) *I* is causally relevant to *X*.
(2) *I* is not causally relevant to *Y* through a route that excludes *X*.
(3) *I* is not correlated with any variable *Z* that is causally relevant to *Y* through a route that excludes *X*, be the correlation due to *I*'s being causally relevant to *Z*, *Z*'s being causally relevant to *I*, *I* and *Z* sharing a common cause, or some other reason
(4) *I* acts as a switch for other variables that are causally relevant to *X*. That is, certain values of *I* are such that when *I* attains those values, *X* ceases to depend upon the values of other variables that are causally relevant to *X*.

The four conditions (1) to (4) mirror an ideal experimental circumstance where the sole causal process from *X* to *Y* is identified and other spurious correlations are ruled out. An intervention on *X* causes *Y* in case that there is an intervention that makes the variable *X* takes the value of *x*, and the *Y* switches to *y* as a result of this intervention.

Let us call the CTE with this interventionism condition the 'interventionist CTE.'

I suggest extending this interventionist CTE to RG explanations, such that an intervention on the 'common features' variables will result in a particular universality class. To make sense of this 'extension,' one does not have to define an intervention along with the causally relevant line. One can replace all "causal relevance" relations (in conditions (1) to (4), which define an intervention variable, see footnote 41) with "counterfactual dependence" relations, in order to define a non-causal intervention variable, obtaining a non-causal interventionist CTE (Khalifa et al 2019: 6). If this extension makes sense, one can apply this CTE to rule out the spurious 'table' example and characterise an RG explanation (at least, its relevance aspect) in a way that when we intervene on the values of spatial dimensionality or symmetry numbers, we obtain a particular critical exponent as a result of this intervention.

This involves a very critical issue: Suppose we can apply the interventionist CTE into an RG explanation (or more precisely, its relevance aspect), what is the conceptual resource or explanatory character that the RG explanation provides, in order to make this application done? The irrelevance aspect of RG offers such a resource to obtain the interventionist counterfactual because this aspect demonstrates that counterfactuals between common features variables and critical exponent variables are robust and independent of micro-details. In other words, it shows and explains why there is a non-causal interventionist path from the variation in *explanans* variables to the *explanandum* variable, and once question (ii)[42] is answered (by the irrelevance aspect), one can obtain an interventionist CTE to capture the relevance aspect of RG and an explanation of UNIVERSALITY-III.

To summarise, with a reflection on how the interventionist CTE characterises the relevance aspect of RG, we find that the irrelevance aspect of RG provides an explanation for why one can obtain the interventionist counterfactual, by which 'common feature' variables explain the universality variable. This subtler structure of RG explanations also dovetails with the multi-stage process of answering UNIVERSALITY-III (why there is a universality class where systems heterogeneous at microscale exhibit the same critical behaviour). The relevance aspect of RG

[42] Why, being in the same universality class, do all molecularly distinct systems have the same critical exponent? Or, why do all molecularly distinct systems, characterised of the same critical exponent, require the same common features?

satisfies the question (i) (what a universality class depends upon), while the irrelevance aspect satisfies the question (ii); both aspects contribute to an answer to UNIVERSALITY-III.

Conceptual Independence of Irrelevance: A Reply to a CTEist Objection

I consider an objection from CTEists (e.g., Woodward 2018a). This objection starts with identifying an incoherence between the irrelevance aspect of RG and CTE-type counterfactuals. The irrelevance aspect supports a counterfactual, such that

> For all λ[43], and a system S in a universality class characteristic of β, if a change in λ had been put to S, then S would be still in the same universality of β.

This counterfactual seems antithetical to the Dependence Condition of the CTE: it requires a counterfactual independence of the *explanandum* phenomenon from the variation of *explanans* variables.[44]

This is not an issue if we insist the interventionist CTE only characterises the final stage of RG explanations, i.e. interventionist counterfactuals, and this CTE has no sufficient resource to grasp every explanatory aspect of RG.

However, there is still a CTEist response. The Dependence Condition can be extended to the counterfactual dependence case because this kind of cases also fits – 'being-still-the-same' is another way of 'what-has-been-different' (Woodward 2018a). For example, we can have:

5. Dependence Condition*: A generalisation G supports a counterfactual that: had the variable S switched its value from x1 to x2 (x1≠x2), then the Y would have the same value.

This DC* makes a counterfactual too general. For example, had I been not born, then the earth is still in the orbit of the sun; this counterfactual is simply nonsense. So, if one wants the DC* to support an explanatory counterfactual, then they would better base the DC* on a critical assumption: the 'irrelevance' of irrelevant factors must *be conditional on* the identification of a relevant one. Once relevant factors are found, irrelevant factors naturally become irrelevant.

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[43] λ-transformation represents the microscopic difference between systems.
[44] Khalifa et al (2019) argue for the same point, although they appeal to another question – why microscopically heterogeneous systems characterised by the same critical exponent have the same common features.

Once relevant variables are fixed, the independence of *explananda* from irrelevant details can be counterfactually characterised. Call this assumption "conditional irrelevance" (Woodward 2018b). If this assumption holds, then the DC* can also hold to capture the explanatoriness of counterfactuals that indicate the independence of *explananda* from irrelevant details.[45]

I will here argue this assumption does not apply to RG explanations. Let us first introduce Woodward's views on 'conditional irrelevance.'

Woodward (2018b) employs the notion of 'conditional irrelevance' to capture how we can make sense of upper-level causal explanations. An explanation is upper-level just in case that it involves macroscale variables (e.g., temperature) cited in a less fundamental theory. The lower-level variables refer to those much finer-grained ones, e.g., momentums of particles in a water container. The idea of conditional irrelevance is that once the upper-level, macroscale variable *X*, which is (causally) relevant to the *explanandum E*, and its value *Xi* are fixed, the variation of value of the lower-level, much finer-grained variable *Yi* will not influence the *E* and thus is (causally) irrelevant to the *E* (ibid).

Two things are noteworthy here. First, the notion of causal relevance is defined along with the interventionist line (Woodward 2018b).[46] It follows that although the notion of 'conditional irrelevance' applies to a causal explanation, one can extend this notion to a non-causal one by replacing 'causal relevance or irrelevance' by 'counterfactual dependence or independence' (as suggested above). Second, the conditional irrelevance is possible, due to a "striking empirical fact," which is:

> "… the difference-making features cited in many lower-level, fundamental theories sometimes can be *absorbed* into variables that figure in upper-level theories without a significant loss of difference-making information with respect to the *explananda* of those upper-level theories." (ibid, emphasis added)

To take the thermodynamic equilibrium as an example: information about the momentum of

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[45] This is also associated with the issue of whether UNIVERSALITY-III or -II is the legitimate universality *explanandum*. If this "conditional irrelevance" assumption holds, then UNIVERSALITY-III and IRRELEVANCE will be jettisoned.

[46] "*X* causes *Y* … if and only if there are distinct values of *X*, *x1* and *x2*, with $x1 \neq x2$ and distinct values of *Y* with $y1 \neq y2$ and some intervention such that if that intervention were to change the value of *X* from *x1* to *x2*, then *Y* would change from *y1* to *y2*." (Woodward 2018b)

each particle is absorbed to a macroscopic variable, say, temperature that indicates the averaged kinetic energy of the mass of particles under investigation. In this sense, the further variation in the finer-grained dynamic variables of particles is irrelevant to other macroscale variables in a thermodynamic equilibrium status, once the temperature is fixed.

But, I hold that the notion of 'conditional irrelevance' does not apply to RG explanations, for two reasons. First, the notion of 'conditional irrelevance' is employed to *grasp* the sticking fact that lower-level variables are absorbed to an upper-level variable without a loss of difference-making information about the *explanandum*, but not to *explain* this fact. Instead, this striking fact is one of preconditions of possibility of conditional irrelevance. In contrast, the irrelevance aspect of RG aims to show when all micro-features (Hamiltonians) are replaced by upper-level features, *why* some of them are preserved, and some are washed out or absorbed, by showing the breaking of connections between non-renormalised features and their status as a difference-maker. The irrelevance aspect of RG explains this 'striking fact.' It appears circular when one cites this 'fact' to define the irrelevance aspect of RG in terms of the relevance aspect of RG.

Second, in the case of the universality of critical phenomena, it seems strange to say that micro-details or lower-level features are *absorbed* to macroscopic common features, i.e., the spatial dimensionality and the symmetry of the order parameter. Being 'absorbed' means that the micro-details or lower-level features are finer-grained states of higher level variables. However, in the RG framework, whether OPs (functionals of the order parameter) and relative Hamiltonians are relevant *or* irrelevant depends upon an independent mathematical mechanism, which involves a derivation from the common features to the rescaling factor and its eigenvalue. The micro-details are irrelevant not because they are finer-grained, microscopic state of the spatial dimensionality and symmetry in question, but because of the independent mathematical mechanism, which simultaneously defines what are relevant. It follows that the 'irrelevance' of irrelevant details is conceptually independent of the identification of the relevant common features.

The irrelevance identification in RG analyses is different from the usual case, such that a thermodynamic equilibrium depends on pressure and temperature: the coarser-grained (albeit sufficient to exhibit the dependency pattern) factors indicating dynamics of a mass of particles. This is the *sui generis* character of RG explanations from the commonality explanation.

Therefore, it can be concluded that the irrelevance aspect is a critical component to the RG explanation, and more importantly, that this is a necessary condition for why the interventionist CTE can apply to the RG analysis.

The Hotchpotch Picture Justified

Recall: a hotchpotch explanation is a multi-stage process with identification, inference and justification. The RG explanation fits this picture: the relevance aspect of RG identifies relevant factors (the common features) and infers critical exponents from them; these allow one to locate a relevant counterfactual. Nonetheless, this counterfactual is not necessarily interventionist and counted as being explanatory. To justify (and explain[47]) the interventionist character, one must appeal to the irrelevance aspect of RG, which shows the independence of the universality from micro-details. Once this triple-stage cognitive process is done, one can obtain an interventionist counterfactual and apply the interventionist CTE to capture the RG explanation. In this sense, both common features and irrelevance theories are required for an entire understanding of the RG explanation.

5. How Structuralism Accommodates RG Explanations

Although the interventionist CTE can capture the explanatory counterfactual the RG supports, the irrelevance aspect of RG still should be appreciated independently. Concerning the issue of how structuralism accommodates RG explanations, one can separate an RG explanation to two distinct aspects – the relevance and irrelevance – and appropriate them by different conceptions of explanation, respectively. The indexing role of mathematics can easily capture the relevance

[47] I think the notions of "justification" and "explanation" overlap in a large extent when they can both be grasped as 'an answer to believe why p.' The explanation – the counterfactual independence of a universality from micro-details (except for common features) – also justifies why one can hold an interventionist mode to the counterfactual identified.

aspect: RG gives an approximation scheme, which represents (interventionist) counterfactuals between common features and critical exponents. In terms of this, I suggest that structuralism accommodates RG explanations in an 'INDEXING + X' form.

I will argue that this form will be 'INDEXING + INFERENCE.'[48] The indexing role captures the relevance aspect, and the inferential relation the irrelevance aspect. Structuralism captures the indexing role, and characterise the inferential relation. Three elements are required to grasp the irrelevance aspect: (1) to construct a space of possible systems; (2) to define the irrelevance of irrelevant factors; (3) RG flows converge to a fixed point. To appreciate them, one is better to appeal to the "process of formation," which grounds an information containment relationship between mathematical constructs and physical systems, which accounts for explanatory force of the irrelevance aspect.

Let us start with how mathematics works in the irrelevance aspect. There are three critical elements. The first element is to construct an abstract space of possible systems, "in which each point might represent a real fluid, a possible fluid, a solid, and so on." (Batterman and Rice 2014: 362). This abstract space is the modal source of the independence of universality – "for all λ, and a system S in a universality class characteristic of β, if a change in λ had been put to S, then S would be still in the same universality of β." λ-transformation, which represents the whole spectrum of systems attracted to the same fixed point, captures this abstract space. As Kadanoff (1971 cited in Batterman 2019) say:

> "[Theorists] imagined that yet another field is inserted into the free energy. Call that other field λ and the operator which is its thermodynamic conjugate *U*. Here, λ represents a parameter in the Hamiltonian. Continuous variation from $\lambda = 0$ to $\lambda = 1$ might represent the change in the Hamiltonian which takes us from the Ising model to the Heisenberg model, or from Ni to Fe or from a nearest neighbour interaction to a next a next nearest neighbour interaction."

It is noteworthy that there is no *uniformed* structural mapping between all possible systems and

[48] Other candidate conceptions of explanation for RG's irrelevance aspect are indexing, instantiation relations, grounding relations and inferential relations based on structural mappings.

the values of λ, although we use λ to *denote* any possible systems in the universality class.[49]

The second element is the rescaling factor and its eigenvalue, which allow one to define 'irrelevance' of Hamiltonians that are washed out and no longer represented. The third element is the convergence of RG flows to a fixed point, which delimits a universality class. The last two elements contribute to the explanatory force of RG's irrelevance aspect by indicating the autonomy of critical behaviours across all molecularly distinct systems with the same common features.

I argue that the 'process of formalisation' constructs the explanatory relevance between the three elements and relevant aspects of target systems, and based on the relevance, one can infer from the convergence of RG flows to universality of critical phenomena across systems that are distinct at a microscale.

Recall the 'process of formalisation' (see Nguyen & Frigg 2017): One first formulates a physical description of a target system, and manages physical relations and physical objects in a specific way. Then, a set-theoretical structure is abstracted from the physical description, and extensionally defined in the specific way of how the physical relations interacts with dummy objects. This formalisation aims to build an information containment relationship between the set-structure abstracted and our physical description of the target system. Also, given that we assume that the experimental result about critical exponents, the value of λ, common features variables and fundamental theories about particle interactions are true or approximately true, we can interpret the formal structure as an accurate or approximately accurate characterisation of the target systems under study. In other words, we can interpret that target systems contain those formal structures.

First of all, consider the value of λ and λ-transformation. We first formulate an imagined physical field λ, the change of whose value is continuous, to denote microscopic differences between systems. The exact physical analogue of it varies according to what we are investi-

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[49] Using λ-transformations to denote microscopic differences between systems, I adopt Frigg and Nguyen's (2017) DEKI (Denotation-Exemplification-Key-Interpretation) account of scientific representation. However, this does not undermine our structuralist view that the approximation scheme indexes the explanatory counterfactual, since the spatial dimensionality, the symmetry of order parameters and critical exponents can be easily isolated from other fictional, idealised parts of the minimal model.

gating in particular.[50] Then, we abstract a structure (that is, a mathematical structure just like other free energy functions) to define the stability of phase transitions in a mathematical sense (see Kadanoff 1971; Batterman 2019 for more details). Given this 'formalisation,' and the truth of experimental results (for values of λ), we can interpret that the microscopic difference of target systems contains information about λ-transformations.

Second, consider the rescaling operations and the convergence to a fixed point. This two elements are linked to the 'block-spin' method in real-space RG. This method, as mentioned in section 4, aims to yield a greater-scale lattice of block spins, which represents the couplings between spins at a smaller scale, and the new, rescaled lattice appears identical with the original lattice (Fisher 1998). In spite of mathematical formalism in making this procedure, the central idea also follows the process of formalisation: we formulate a new lattice of block spin that (we suppose) is equivalent with the sum of lattices of block spins at a microscale. Then, we structurally define the rescaled lattice as what the original lattice maps to itself with a scaling factor.[51] The process of formalisation is critical as it abstracts a mathematical structure from numeral simulation about micro-couplings governed by some fundamental theories. Not only is this the key to cash out an RG flow, but reflects explanatory power and depth of RG methods that provide a novel conceptual connection between the fundamental theory and the theory for macroscopic phenomena. In this sense, since we assume those fundamental theories state the fact in the world, the process of formalisation enables us to say that the real target system near criticality contains the scale-invariant information that the RG analysis reveals.

Based on these formalisations that ground information containment relationships between real target systems and RG transformations, and based on these relationships, one can infer from the convergence to a fixed point to a similar pattern governing couplings and interactions between particles (or 'block particles') from a microscale to a macroscale, regardless of micro-

[50] The field λ denotes "the ratio of next nearest neighbour spin coupling to that nearest neighbour coupling" for ferromagnetic systems, "De Boer's parameter" for liquid-gas, H_x for anti-ferromagnetic systems, and μ for super-fuild etc. (see Table II in Kadanoff 1971: 105)

[51] Of course, we can prove that Hamiltonians involved in the two lattices are "asymptotically equivalent" by taking a diverged correlation length, which means the concept of 'formalisation' is unnecessary to cash out an RG transformation. Nonetheless, the diverged correlation length is a *mere* approximation of couplings in target systems near criticality, and the RG flow should better be treated as an approximation of what governs particle interactions in the real systems from a microscale to a macroscale (e.g., Norton 2012; Franklin 2018). This suggests that the concept of 'formalisation' is required for making the information containment relationship – i.e., inscribing a certain mathematical structure into systems under different scales.

constitutions of the systems. This inferential relation accounts for the explanatory force from RG's irrelevance aspect (that is, many microscopically distinguishing details are washed out) to the counterfactual impendence of critical behaviours.

6. Conclusion

In this chapter, I have specified a hotchpotch picture to characterise RG explanations. An RG analysis is a multistage activity with identification, inference, and justification. There are two aspects of RG explanations: the relevance aspect indexes a counterfactual between common features *explanantia* and universality *explananda*; the irrelevance aspect allows one to attain the interventionist character for this counterfactual, making it cross the threshold of being an explanatory counterfactual. The interventionist CTE applies to the RG explanation in its final stage. Given this picture, an RG explanation can be accommodated in a structuralist framework by dissecting it into INDEXING and INFERENCE, which is either captured or characterised by mapping functions.

Concluding Remarks and Future Work

I would like to share several take-home messages in this concluding section:

First, I have defended structuralism from three challenges – “the bridging problem,” “the inconsistent early calculus” and “RG explanations” – from the similarity perspective and the hotchpotch picture. In doing so, I have proposed a broader conception of applied mathematics – the similarity account. It is the specification of the respect and degree of similarity mediating a mathematical model to its target system in reality. The insight of this account is not to reduce representations to similarity relations, but gives a pragmatic framework for building a standard of representational accuracy without presupposing a general account of representation.

The similarity account gives a pragmatic framework to supplement other solutions to the bridging problem. A similarity relation between a system and data extracted from it can enable van Fraassen’s pragmatic equivalence – between representing the system and the data. The specification of the respect and degree of similarity commits a modeller to the corresponding accuracy of representation, incurring an asserting force that underpins the pragmatic equivalence. As to Nguyen and Frigg’s solution – that a system obtains its structure S_T by designating a “structure-generating description” $\boldsymbol{D_S}$ of the system, from which the S_T is abstracted – a similarity between the $\boldsymbol{D_S}$ of the system and the corresponding S_T also allows the modeller to set a standard of representational accuracy for the $\boldsymbol{D_S}$ by specifying the degree of similarity in the S_T side.

I also responded McCullough-Benner’s critiques of the partial structure approach. I gave a *tu quoque* response to his “robustly inferential account” in terms of how mathematics places constraints on physical representations – his account does not explain why an inference pattern is privileged. Also, the partial structure approach can appreciate three different interpretations of infinitesimals in producing physical representations by arranging infinitesimals in different blocks of relations – viz., designating different partial structures to each interpretation. From the similarity perspective, I suggested grasping the role of infinitesimals as an approximating technique and a method for specifying the degree of similarity. If plausible, we can also explain how to finalise an algorithm using infinitesimals.

Additionally, inspired by the similarity account, I also proposed a rough hotchpotch picture as a further methodological reflection for our study of scientific representation and explanation. Different from the analytical tradition – that a representation or an explanation is reduced to a mapping or an inference – the hotchpotch picture requires us to dissect a representation or an explanation into several aspects and use different theories (that are often thought of competing) to appropriate each of them.

Perhaps we could use the metaphor "assemblage" to depict the hotchpotch picture. There is no 'core' for representations or explanations. Rather, the crux is to see how we can *organise* a mathematical structure and other elements (the modeller's purpose, empirical assumptions, instruments, etc.) together and forge them into a context of representation (or an empirical set-up) and a source of explanatory force. The application of approximation is crucial here, since it is the machinery for this organisation.

As an illustration of this hotchpotch picture, I argued that RG explanations can be dissected into two aspects – the relevance aspect and the irrelevance aspect. Regarding the former aspect, I developed an interventionist counterfactual theory to characterise explanatory counterfactuals between common features *explanantia* and universality *explananda*. The irrelevance aspect of RG contributes to explanatory force by giving conceptual resources to obtain the interventionist mode to the counterfactuals in question. In this picture, RG explanations can be accommodated in a structuralist framework by dissecting them into indexing and inferential conceptions of explanation, which are either captured or characterised by mapping functions.

There are four questions left for future work.

First, although I specified what a hotchpotch picture would be to capture RG explanations (in chapter 4), I did not say too much about mathematical scientific representations. Specifically, I have merely proposed a very *rough* hotchpotch picture; however, I have not formulated it into a solid research programme. This will be what I shall work on in future.

Second, in the second part of section 2 of chapter 2 when I illustrated how the similarity account can rescue van Fraassen's and Nguyen and Frigg's solutions to the bridging problem. It appears epistemically circular: when I argued that the specification of the degree of similarity in the S_T side can be transferred to the specification in the $\boldsymbol{D}_S$ side, I implicitly appealed to van Fraassen's pragmatic equivalence. However, I tried to rescue the pragmatic equivalence in light of Nguyen and Frigg's structure-generating description.

To resolve this circularity, one must appeal to the inferential aspect of representation – that reliable instruments and statistical techniques allow us to infer from phenomena to data. In this way, the $\boldsymbol{D}_S$ and the S_T is connected so that we can specify the degree of similarity of the $\boldsymbol{D}_S$ in the S_T side. Also, the similarity between phenomena and data is well-motivated, which incurs the pragmatic equivalence. Future work should be on how the inferential theories for scientific representations can be appreciated in the similarity framework.

Third, McCullough-Benner's "robustly inferential account" gives a convenient perspective to observe the early stage of mathematical techniques (e.g., Dirac delta function and operational calculus) and the informal context of applying mathematics. This is the "context of discovery." Structuralism is more about the applied mathematics in the "context of justification" along with a large collection of consistent, rigorously justified structures.

Is there any dynamic association between the two contexts? If so, what are the association and underlying dynamics? I believe the similarity account offers such a framework to track the association and dynamics. As suggested in the end of chapter 3, the use of infinitesimals should be appreciated as a method for specifying the degree of similarity – between targets and their approximations – which motivates us to select an algorithm using infinitesimals and constrain representations structurally and consistently in R1. Future research should be more on how the similarity account applies to the algorithmic use of infinitesimals, and how a formal, rigorous, and structuralist-friendly formulation were raised to articulate the similarity in question. I also wonder this similarity picture works for the evolution of other mathematical techniques.

Fourth, it appears that applied mathematics heavily relies on approximating and relevant statistical techniques, and these techniques are essential to the formation of empirical set-ups. However, this topic is underappreciated in current literature, or simply incorporated as "what has been cut off in a power series" in the structuralist programme. More research can be done in this topic – especially about how approximating techniques organise all elements required into a context of representations or a source of explanatory force.

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