What Is "Applied Mathematics" Anyway?

How the History of Fluid Mechanics Demonstrates the Role of Concepts in Applied Mathematics

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Contents

Chapter 0

Introduction: Intepreting Physical Theory in Modern Science

Modern science, particularly modern physical theory, has provided us with views of the world seemingly unprecedented in their richness and departure from commonsense intuition. From the rejection of a static spacetime in general relativity to the quantum strangeness that seems to occur at the micro level, all the way to proliferation of quantum theories of gravity that question the fundamentality of space and time. Theories and models extending into domains of greater and greater complexity, from the climate of the planet to chaotic dynamical systems. With such great departures from simple everyday experience, the problem of interpreting physical theories becomes ever more pressing.

The question of how to interpret physical theories is not a new one; debates concerning scientific realism, the relationship between metaphysics and science, and intertheoretic relations have taken place throughout the intertwined histories of philosophy and science. The debates came to prominence with the rise of the Vienna Circle, through the work of early philosophers of science such as Popper, Hempel, and Carnap, and contemporary philosophers/theorists have continued to refine and revise the debates and competing viewpoints since.

However, there is good reason to believe that questions regarding the interpretation of physical theory have reached a new level of significance. With the diminishing reliability of commonsense intuition in interpreting and adjudicating between various physical theories, we are left searching for new guides, new governing principles, new approaches to questions of how we should interpret the views of the world developed by scientists. Indeed, this is a question scientists themselves are facing, despite the "Shut up and calculate" sentiment Feynman suggested in relation to quantum strangeness. Particularly when our physical theories become increasingly divorced from experimentation, from Popper's falsifiability criterion, as in many cases for quantum theories of gravity, we are left wondering what we are doing, what exactly physical theorizing is.

In this thesis I am for the most part concerned with the role of mathematics in physical theory and the role that it plays in interpretation of physical theory. What does it mean when our fundamental physical theories are "expressed in the language of mathematics," and how do we interpret the mathematical portions of physical theory? What can we extrapolate from the mathematics of a physical theory? Can mathematical consistency serve as a theoretical virtue when we are adjudicating between competing theories?

These sorts of questions will naturally lead into more general questions about applied mathematics and the nature of mathematics itself. Why is mathematics so useful in describing the world, in helping us navigate our surroundings? What sorts of things are mathematical entities and operations?

These questions are intertwined with one another, and I will address most of them in the course of this thesis, even if it is just to sketch out an answer space. Most pertinently, my aim in this paper is set the foundations for an account of mathematics, informed by these questions as well as episodes from the history of the mathematical sciences. From this, I will draw several conclusions about mathematics and its role in physical theory, arguing that mathematics should be regarded not as "*the* language *of* Nature," but rather as "*a* language *that we have developed to describe* Nature." From this, it follows that the distinction between "pure" and "applied" mathematics is not just hazy, but purely sociological. It also follows that the formulation of our physical theories in mathematics is not unique; theories need not necessarily be formulated in the language of mathematics. Further, it follows that mathematical consistency is not necessarily a theoretical virtue we can use to adjudicate between competing physical theories.

These are controversial ideas that require sound and detailed defense. Here, I argue for the foundations of the view that supports them.

0.1 The Syntactic and Semantic Views of Scientific Theories

Before jumping into detailed questions about the role of mathematics in physical theory, it is best to contextualize my arguments by briefly covering the major debates and views in the interpretation of physical theory. I will begin here by discussing the syntactic and semantic views of theories, and then I will move on to discuss the dialectic that occurs between science and metaphysics.

In considering laws of nature and physical theories, the tendency around the early 20^{th} century among members of the Vienna Circle was to treat them as statements, usually as logical statements. For example, an article by Dretske written in 1977 [14, p. 833] attributes to many empiricists the view that laws of nature belong to a certain sort of universal truths, of the logical form

$$(x)(Fx \supset Gx) \quad \text{or} \quad (x)(Fx \equiv Gx)$$

where F and G are "purely qualitative (nonpositional)" predicates. Likewise, Jerry Fodor's 1974 paper on the special sciences treats laws as being of the form

$$S_1x \to S_2x$$

where this is "read as something like all S_1 situations bring about S_2 situations." [16, p. 955]

While these are more focused on laws of nature having the logical structure of a conditional subject to a universal quantifier, this is an instance of the more general *syntactic view of theories*, the treatment of theories as statements, usually logical. According to this view, a *theory* consists of a set of logical statements, valid logical inferences, etc that can then be used to draw logical conclusions about the world. That is, theories serve as tools that aid in our construction of logical arguments, just like the principles of logic.

This view certainly has its appeals, especially in an environment where we are concerned with what valid logical inferences we can make, which was exactly the environment of the Vienna Circle and the rational reconstruction project. Indeed, valid logical inference is one way to think about rationality, how we navigate the world. However, this is not the only way we can think about how humans navigate the world, and so it is not the only way that we need to think about scientific theories.

Another way to think about how humans navigate the world is thinking about representations and how humans use representations to simulate possible courses of action. This notion is closely tied with what is called the *semantic view of theories*, which treats a scientific theory as providing a set of associated models, and regarding these models as entities in their own right, integral to the usage of the theory.

A cornerstone in work treating scientific theories in this way is the collection of essays edited by Mary Morgan and Margaret Morrison, *Models As Mediators* [31]. In the beginning of this, Morgan and Morrison argue that models are "autonomous agents," related to but distinct from both theory and world/data. The idea is that models, though derived using elements of the theory and elements of the world/the data, are functionally separate from both theory and world. Models are able to probe the limits of inference about both the theory and the world.

Thus, the picture Morgan and Morrison paint is somewhat of a ternary relationship, with two-way roads connecting each of the three nodes of world, data, and model. The picture is, of course, more complicated than this, especially given that I've only given the broad brushstrokes of Morgan and Morrison's view. There are still ambient issues such as the theory-ladenness of data/observation. However, in this thesis, I am going to argue that there is an integral clarification to be made here, and that is the role that physical concepts play in mediating between mathematical model, physical theory, and the world You might say that I'm arguing for "concepts as mediators," refining Morgan and Morrison's picture as to what is involved in connecting those three main nodes.

0.2 Scientific Theories and Metaphysics

Now, while the shift from the syntactic to the semantic view of theories was going on, there was all the time persistent inquiries as to whether and how science and metaphysics should be related. Much of this debate within philosophy of science has fallen under the category of the "scientific realism" debate. The essential question guiding this debate is usually stated as "Should we accept our best physical theories as telling us what the world is, what's out there?" So, for example, modern fundamental physics includes such things as particles and/or fields, possibly even loops and strings and extra dimensions, as well as a spacetime that is curved, the curvature being dependent upon the distribution of mass/energy in the universe. Many of these are theoretical entities come from well-confirmed theories, quantum mechanics and general relativity. Should we, on that basis, accept these entities into our ontology?

This sort of question has been around for centuries, but has become significantly more salient amount work in philosophy of science in the past half century or so.[1] There are several common arguments for each side; on the one hand, there's the "no-miracles" argument *for* realism, and on the other hand, there's the argument from pessimistic meta-induction against realism. The "no-miracles" argument in general argues that the success of our scientific theories shouldn't be a miracle, and so surely the scientific theories latch onto something in the world. The argument from pessimistic meta-induction retorts that we have plenty of examples of theories that were believed to be the best for a time but subsequently proven wrong, and thus we do not have license to draw our ontology from our current best theories.

However, apart from the traditional scientific realism debate, there are finer *and* broader issues being addressed on the topic of science and metaphysics. For example, there is a more general question regarding whether metaphysics should constrain science or vice versa, or whether they should have a relationship to one another at all. For example, James Ladyman, Don Ross, David Spurrett, and John Collier argue in their book *Everything Must Go: Metaphysics Naturalized* [27] for an extremely one-sided relationship between metaphysics and science, where essentially science replaces metaphysics.

On the other hand, there are many philosophers, and even scientists, who attempt to extrapolate metaphysics or ontology from physical theory and debate how this is to be done. The proliferation of interpretations of quantum mechanics is an example of such a phenomenon. There are debates within the philosophy of quantum mechanics as to whether we should regard configuration space as "real" space, what use we can make of nonrelativistic quatnum mechanics, and what makes a comprehensive physical theory. However, much of the debate about interpretations of quantum mechanics boils down to questions about how we interpret a scientific theory that enjoys empirical success. What can we extrapolate from the successes that a theory has? In particular, what

[1] For example, the "Epiricism and Scientific Realism" chapter in Curd, Cover, and Pincock's philosophy of science anthology [11] consists of papers originally published in 1962, 1980, 1981, 1982, 1984, 1985, 1989, 1993, 1994, and 2005.

can we extrapolate from a successful mathematical apparatus? And this carries us into the subject of my thesis, the use of mathematics in physical theory.

0.3 A Problem: Mathematics in Our Physical Theories

At the superficial level, the presence of mathematics in our physical theories is surprising in numerous ways. Most strikingly, there is such a difference in nature between mathematical objects and physical objects, mathematical knowledge and physical knowledge, methods in mathematics and methods in the physical sciences. Given this extreme contrast, it is unclear at best how and why mathematics should be applicable to the world.

This sentiment is most notably expressed in Eugene P. Wigner's classic 1960 paper, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences." [46] In this paper, Wigner wonders at the curious ways in which mathematics enables science to have its predictive utility. Primarily, Wigner states that mathematics shows up in a good portion of the natural sciences, enhancing accuracy in describing phenomena, and we have no idea why it shows up where it does and is successful. This sort of Wignerian wonderment can at least motivate us to look more closely at the ways in which mathematics is applied to the world. And in fact, I will attempt to argue that, even though there is no mystical connection underlying the math-world connection, we should still marvel at how mathematics gets applied to the world due to the rich and immense amount of work by humans that goes into facilitating that connection.

In a way, then, the account I give is somewhat deflationary. There isn't some deep metaphysical connection between mathematics and the world, Nature isn't written in numbers and figures. However, I like to think that the account I argue for here is even better. I argue, in the end, for an account of mathematics that takes into account the role that physical concepts and the aims of modeling play in the construction of mathematical representations, and this highlights the work that humans do at every turn. Describing the world in a way that allows for precise predictive power in a colossal cognitive feat, and one that takes years, decades, centuries even. If that isn't something worthy of wonder, I'm not sure what is.

In the course of this thesis, though I will try to deal with several misconceptions concerning mathematics, including the relation between pure and applied mathematics, how mathematical notions come about, and how it is that an arithmetical equation can model a kindergarten sharing problem. These misconceptions about mathematics come from a variety of sources, but all draw on a caricatured picture of mathematics imparted by public image of mathematics. The notion that it is a whiteboard sport, just professors in armchairs thinking, writing and rewriting equations. Completely abstract and detached from reality. But nothing could be more opposite to the truth. Pure mathematics as a social grouping did not evolve in France and Germany, for example, until the mid-19^{th} century. For the greater portion of its history, mathematics has had solid traction with worldly concerns, and this persists to this day, despite public perceptions.

Through the breaking-down of these misconceptions, the goal is to build a better conception of mathematics, founded upon the history of mathematical practice and contemporary mathematical practice and further informed by deep analysis of the role that physical concepts play in the math-world connection. Though I will not be able to develop this account in full in this thesis, I hope to take the first steps toward such an account and suggest what implications it may have for many of the aforementioned issues concerning the interpretation of and adjudication between physical theories.

0.4 Plan of the Paper

I will begin this thesis in Chapter 1 by considering one of the dominant families of philosophical approaches to applied mathematics, mapping accounts. I will focus on the mapping account developed by Christopher Pincock for reasons to be discussed in Section 1.1. After introducing the basic idea of Pincock's mapping account and several refinements he has made over the years, I will spend section 1.2 formulating a general critique of Pincock's account. This critique will be aimed primarily at the metaphysical implications of the mapping intuition and so generalizable to other accounts that take the mapping intuition as their core.

I will then move on, in Chapter 2, to give a more concrete objection to Pincock's mapping account by discussing a problem case that Pincock himself raises in [36], the case of Prandtl's boundary layer solution the the Navier-Stokes equations in fluid mechanics. I will argue that Pincock's mapping account cannot answer the question of how Prandtl's boundary layer solution aids in our understanding of fluid flow. I will do this by sketching out my own answer to this question, first by giving a brief historical narrative of the development fo the Navier-Stokes equations and Prandtl's solution in section 2.2 and then following up with a philosophical analysis in section 2.3.

After having provided these critiques of Pincock's account, I will move on to considering what a satisfactory account of applied mathematics must look like. I will spend Chapter 3 arguing that such an account must also be an account of mathematics in general. I will do this by arguing that pure and applied mathematics are not separable, as is commonly believed. I will provide several motivations for this. First, in section 3.1, I will discuss how developments in spherical and planar trigonometry in medieval arab mathematics were motivated by a worldly, social problem. Then, in section 3.2, I will argue that the claim that complex numbers were developed for "reasons purely internal to mathematics" is at best unintelligible, and will proceed to provide a brief analysis of the development of complex numbers to support this. I will then move on to discuss the langauge metaphor I use to think about mathematics in section 3.3 and then give an argument for this blending of pure and applied mathematics in section 3.4. Finally, in section 3.5, I will generalize several points from the previous philosophical analysis of the viscosity concept to develop my notion of conceptual infrastructure and argue for the important role it plays in a satisfactory account of mathematics.

Finally, in the conclusion, I will synthesize my arguments from all previous chapters ion order to sketch out what a satisfactory account of applied mathematics must include, at minimum. I will argue that such an account must cover the spectrum from pure to applied mathematics, with no hard and fast boundary between the two. Such an account must handle pieces of mathematics on a case-by-case basis, assuming no general, axiomatic, schematic form to answers in questions about applied mathematics. Such an account must advocate analyzing mathematics in use, rather than in the abstract, and must include multiple tools of analysis, including historical analysis and conceptual analysis. I will end the thesis with some quick considerations of some implications this sort of account would have for physical theories. I will specifically assert that such an account implies that (i) we are not automatically licensed to read metaphysics off of the mathematical articulation of a theory, and (ii) mathematical consistency cannot, in general, serve as a theoretical virtue.

0.5 Acknowledgments

The first two chapters of this project originated from a term paper for a philosophy of science class with Julia Bursten just over two years ago, the third from a term paper for a science and values class with her in fall 2020. I owe her uncountable and infinite gratitude for her guidance, discussions, critiques, and comments throughout the development and evolution of this project. It simply would not have occurred without her, and many of its strong points are due in no small part to her guidance. She provided resources for my research, starting places and new areas to explore, as well as invaluable support throughout the second half of my undergraduate career at UK.

I would also like to give considerable thanks to members of the Bursten Research Group for helpful discussions and comments throughout both the early and later periods of this project. In particular, I would like to thank Madison Von Deylen and Sarah Jane Robbins for valuable early discussions about how mathematics gets used in science. I would also like to thank Madison and Sarah, as well as Christopher Grimsley for helpful feedback on practice presentations of various aspects of this project.

I also owe much thanks to my father, mathematician, and theoretical physicist Peter Perry. I cannot count the number of conversations I had with him regarding my perception of mathematics and physical theory, as well as sparring with him on several of the ideas I present in chapter 3 and the conclusion. He has often served as one of the people who I run my more radical ideas by, usually helping me to temper my ideas or at least be aware of many possible objections, especially from the mathematicians' side.

I would also like to thank the Philosophy of Science Association for allowing me to present a portion of this work as a poster at the 27th Biennial Meeting of the Philosophy of Science Association virtual poster session in early 2021. I benefited greatly from discussions during that presentation. It also allowed me to have further discussions about physically similar systems and the SI system with Susan Sterrett, which I found very helpful in developing my thoughts about mathematics presented in Chapter 3, as well as providing future directions for research.

My participation in the 2020 Midwest Summer School in Philosophy of Physics also greatly influenced the direction that my thesis took, notably the gravitation towards interpretation of physical theories as a way to frame my ideas, as well as prodding my thoughts about mathematical consistency and reading metaphysics off of mathematics. In a similar vein, I would also like to thank Michael Miller and Trevor Teitel for allowing me to sit in on several sessions of their Metaphysics of Quantum Mechanics seminar at the University of Toronto in spring 2021.

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I would also like to thank the University of Kentucky Libraries for providing the resources that made much of my research possible, including many interlibrary loans and requests for books in remote storage, as well as help with search techniques.

Finally, I would like to thank my friends and family for putting up with long conversations, usually me getting overly excited about an episode of history in fluid mechanics or complex numbers. Their support was invaluable to me throughout the years in which I have worked on this project, and will continue to be invaluable as we all move forward in life.

I want to emphasize that, although I am thankful for the conversations and guidance and support I have received from everyone listed here and beyond, I ultimately take responsibility for any mistakes or wrong turns in this thesis.

Chapter 1

Accounting for Mathematics in Physical Theories

1.1 Pincock's Mapping Account of Applied Mathematics

The central question for an account of applied mathematics concerns *why* mathematics is able to aid in our understanding of the world. Another way to frame this question is as asking why mathematical models are able to aid in our understanding of the phenomena/systems that these models are supposed to represent.

I think that a common intuition for many of us when asked this question is to look for a relation that holds between the mathematical model and the physical system[1] in question. Surely, some relation must hold between the two; there must be some connection in order for the mathematics to provide any information about the system. This natural way of thinking is best illustrated through a simple arithmetic example. Consider the following elementary arithmetic problem:

> *Keisha has eight apples, and Angeline has three apples. How many apples do Keisha and Angeline have together?*

It seems natural to model this situation with the arithmetic equation $8 + 3 = x$, where there is a natural way that 8 and 3 represent Keisha and Angeline's apples respectively, a natural way that x represents the total of Keisha and Angeline's apples, a natural way in which the operation of addition corresponds to the

[1] For brevity from now on, I will refer to the "system" or "physical system" modeled. I focus in this paper on the natural/physical sciences, and so am solely concerned with models of physical systems/phenomena.

action of combining Keisha and Angeline's apples.[2]

The intuition behind this "natural way" of representing the apples situation is the same intuition underlying our common response to the question of applied mathematics. I will call it here the "mapping intuition." It is the idea that the pieces of mathematics in a mathematical model somehow hook onto, or map onto, various aspects of the system being modeled.

This mapping intuition forms the core of a family of accounts known as "mapping accounts of applied mathematics." In such accounts, the idea of "mapping onto" is formalized, given a clearer meaning, in addition to extra bits begin added on in order to account for more difficult and complex cases. In this thesis, I will focus on the formulation of the mapping account developed by Christopher Pincock.[3] There are two reasons for this, one sociological and one substantive. The sociological reason is that Pincock's is simply one of the most discussed mapping accounts; when mapping accounts are discussed, his is usually mentioned. The more substantive reason for focusing on Pincock's mapping account is that, as I will show below, it gives one of the simplest, purest formalizations of the mapping intuition. Thus, by critiquing Pincock's mapping account, I will formulate a strategy for critiquing mapping accounts in general, since all take the mapping intuition as their core.

1.1.1 The Simple Mapping Account

The essential idea behind Pincock's account, what he calls the "basic contents" of a mathematical representation, is the existence of a structure-preserving relation between mathematical model and physical system, usually an iso- or homomorphism. Pincock states this in his book by saying that

> offering a mathematical scientific representation can be schematically summarized as claiming that the concrete system S stands in the structural relation M to the mathematical system S^*. If both systems exist and the structural relation obtains, then the representation is correct. Otherwise, it is false. [38, p. 28]

In a previous article, Pincock discusses the structural relation with more detail, saying that his

> proposal is that a wholly mathematical model represents a phsyical system in virtue of a structure-preserving mapping like an isomor-

[2]One worry that I would like to address immediately regarding this intuition and the apples case is that I have already snuck in mathematical assumptions in how I describe the apples problem. In the wording of the problem, I use words/phrases like "eight," "three," and "how many." The first two of these can be dealt with by providing illustrations of Keisha and Angeline's apples, but the latter one is harder to eliminate. What it comes down to is that what we care about in this situation involves quantity, and so asking how many (and therefore the use of arithmetic) is warranted by what we care about. I discuss later the idea that mathematics is a language whose use in a problem depends upon the values of the enquirers, and so I will come back to this issue later; it should not worry us at this point.

[3]For Pincock's formulation, see [34], [35], [36], [37], and [38]. One of the other notable mapping accounts is the inferential account developed by Otávio Bueno and Mark Colyvan, [6].

> phism or an homomorphism between the physical situation and the mathematical model. [34, p.960]

Pincock's account thus provides a more formal expression of the mapping intuition using the mathematical notions of structure-preserving relations, isomorphisms and homomorphisms. It would be good to know, then, what exactly these relations are; even if the formal mathematical notions Pincock uses are not exactly what he believes facilitate the model-system connection, he still seems to believe that the mathematical notions give a close approximation of whatever true connection holds.

A structure-preserving relation, generally speaking, holds between two things just in the case the essential structure is the same in each. A common example of this is scale models; even though the scale model and the actual object differ in many respects (size, composition, etc), the basic structure is the same. There exists a map from points on the scale model to points on the actual object, and this map is such that, if two points are next to each other on the scale model, then the respective two points on the actual object are also next to each other. This should give the basic flavor of a structure-preserving map, but it is worth digging into the more formal notions of homomorphism and isomorphism, since what we are dealing with here are mathematical models that, at least on the surface, are nothing like scale models.

A homomorphism is a relation that holds between two sets, $\mathscr{A}$ and $\mathscr{A}'$, where each of these sets has some number of relations defined on it. Now, $\mathscr{A}$ consists of a set of elements, $A = \{a_1, a_2, \dots\}$, and a set of relations $R = \{\mathscr{R}_1, \mathscr{R}_2, \dots\}$. These relations can be defined as being sets of ordered n-tuples of elements of A, where n depends on the relation in question.

This is a bit abstract, so a concrete example is useful here. Consider the set of integers, $\mathbb{Z}$, which has addition defined on it. Addition can be thought of as a three-place relation $+$, a set of ordered triples of integers. Since $2+1=3$, then the ordered triple $(2,1,3)$ is an element of $+$. We write this using set notation as $(2,1,3) \in +$. Likewise, $(1,2,3) \in +$, since $1+2=3$. However, $(2,2,5) \notin +$ and $(3,1,2) \notin +$ since $2+2 \neq 5$ and $3+1 \neq 2$. So, a relation is not necessarily *all* ordered n-tuples of elements.

We then have one set, $\mathscr{A} = \{A, R\}$, which has set of elements A and set of relations R. We have another set $\mathscr{A}' = \{A', R'\}$, which has set of elements A' and set of relations R'. Then a homomorphism between $\mathscr{A}$ and $\mathscr{A}'$ is a map $\phi : A \to A'$ which maps elements of A to elements of A' such that, if $(a_{k_1}, a_{k_2}, \dots, a_{k_n}) \in \mathscr{R}_i$ for some i, then $(\phi(a_{k_1}), \phi(a_{k_2}), \dots, \phi(a_{k_n})) \in \mathscr{R}'_i$.

Again, a concrete example will be useful. Consider the two sets $\mathbb{Z} = \{\mathbb{Z}, +\}$ and $2\mathbb{Z} = \{2\mathbb{Z}, +\}$. The first of these is just the regular set of integers with regular addition. The second of these is the set of even integers with regular addition. We can show that a homomorphism holds between these two. Consider the map $\phi : \mathbb{Z} \to 2\mathbb{Z}$, where $\phi(x) = 2x$ for all $x \in \mathbb{Z}$. For example, $\phi(3) = 6$. This map takes in an integer and spits out an even integer. All we need to do to confirm that it is a homomorphism is confirm that the relation $+$ is preserved. That is, for any triple $(x_1, x_2, x_3) \in +$ with $x_1, x_2, x_3 \in \mathbb{Z}$, we want to show that $(\phi(x_1), \phi(x_2), \phi(x_3)) \in +$. This is easy to show. If $(x_1, x_2, x_3) \in +$, then

$x_1 + x_2 = x_3$. Now, we want to know if $(\phi(x_1), \phi(x_2), \phi(x_3)) \in +$, that is, if $2x_1 + 2x_2 = 2x_3$. But clearly this is true, since, dividing by 2, this is just $x_1 + x_2 = x_3$. So, the integers with addition is homomorphic to the even integers with addition.[4]

An isomorphism is just a special type of homomorphism, with two special constraints. The map ϕ must also be what's called one-to-one and onto. To be one-to-one, a map $\phi : A \to A'$ must map every element of A to an element of A' such that every element of A' must get mapped to by at most one element of A. To be onto, every element of A' must be mapped to.

Consider again the map $\phi : \mathbb{Z} \to 2\mathbb{Z}$, given by $\phi(x) = 2x$. To check that ϕ is one-to-one, suppose two elements $x_1, x_2 \in \mathbb{Z}$ were mapped to the same element $y \in 2\mathbb{Z}$. Then $2x_1 = y = 2x_2$. But this implies $x_1 = x_2$. Thus, ϕ is one-to-one. To see that ϕ is onto, consider an arbitrary element $y \in 2\mathbb{Z}$. Since y is an even integer, it can be expressed as $y = 2k$ for some integer k. But this means $\phi(k) = y$, and so y does get mapped to. Thus, every element of $2\mathbb{Z}$ gets mapped to, and so we can conclude that $\mathbb{Z}$ with addition is actually isomorphic to $2\mathbb{Z}$ with addition.

These are the more precise, mathematical notions of homomorphism.[5] I will close this discussion of the basic idea of Pincock's mapping account with one of Pincock's own examples of applying these notions to a piece of applied mathematics, the problem of the Bridges of Königsberg.[6] The problem goes as follows:

> *There are seven bridges connecting two islands and two shores in the city of Königsberg. (See Fig 1.1) Is it possible to walk all seven bridges using each bridge exactly once?*

This problem was solved in 1736 by Leonhard Euler, who modeled the network of bridges with a graph on four vertices, kicking off the subfield of mathematics called graph theory.[7] In the graph, the vertices are supposed to correspond to the two shores and the two islands, and the edges connecting vertices are supposed to correspond to the bridges connecting respective regions.

The problem of walking the seven bridges of Königsberg then becomes a problem of finding a *path* on the respective graph such that each edge is used once and only once (which also entails that every vertex is visited). Euler noticed that, for a given graph, in order to form a path over all vertices of the graph using every edge once, each vertex in the path except the first and last must always be entered and exited (unless the first vertex *is* the last vertex, in which case the path is called a *circuit*). So, all but two vertices of the graph (or all

[4] For a more detailed treatment, see [18].

[5] For brevity, I will from now on just say "homomorphism" instead of "iso- or homomorphism."

[6] Pincock's explanations of this example often involve discussions of what Pincock calls abstract acausal explanation and can be found in [35, pp. 257-60] and [38, pp. 51-4]. My own discussion draws from these, as well as from [45].

[7] For those unfamiliar with "graphs" in this sense, a graph is a formal mathematical structure, consisting of two parts: a set of vertices and a set of edges. Graph theory is a branch of mathematics devoted to studying properties of various types of graphs as well as graphs in general.

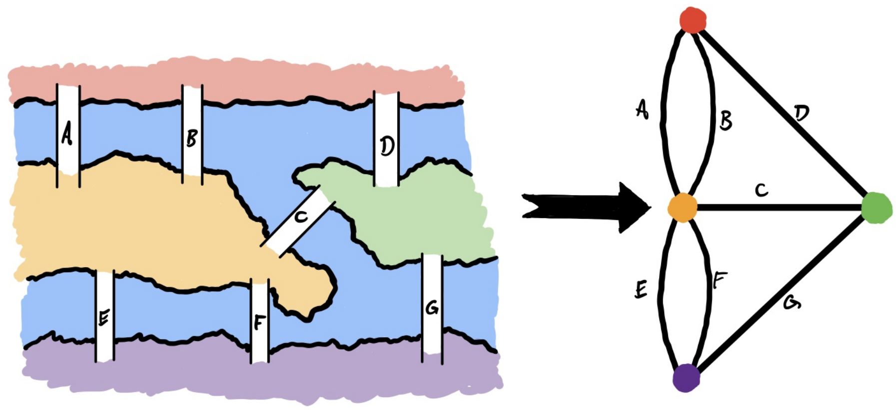

Figure 1.1: The Bridges of Königsberg. Vertices of the graph on the right correspond with the similarly-colored regions of land in the map on the left. Similarly, edges of the graph correspond with similarly-labeled bridges in the map. (Author illustration)

vertices of the graph) must have *even degree*, that is, an even number of incident edges. If this holds, then the graph admits of a path (or circuit) that includes every vertex and uses every edge exactly once; this is called an *Eulerian path* (or *circuit*; a graph which admits an Eulerian circuit is called an *Eulerian graph*).

Looking at the graph modeling the bridges of Königsberg, it is immediately clear that *every* vertex has odd degree. This means that the graph does not admit an Eulerian path; there is no path through all of the vertices that uses each edge exactly once. This is equivalent to saying that it is impossible to walk all seven bridges of Königsberg using each bridge only once. And so, Euler concluded that the answer to the problem was that it was impossible.

Here, the mapping account asserts a homomorhpism between the graph and the actual bridges in the city of Königsberg. There is a natural way to see this homomorphism; we can sinmply overlay the graph onto a map of the city, lining up the vertices and edges of the graph with the appropriate regions of the city and bridges. And we see that the adjacency relation, which specifies if two places (regions on the map or vertices in the graph) are connected, is preserved, as is the relation specifying how many connections there are between two adjacent regions. In this way, the structure preserved in the relation between graph and city is such that we can use the graph to answer questions we have about the bridges in Königsberg.

1.1.2 Idealization and Matching Models

Very few, if any, instances of applied mathematics are as simple as "there exists a homomorphism between mathematical model and physical system." Pincock therefore makes several additions to the "basic contents" discussed in the previous section. Pincock has discussed these additions in multiple ways throughout his work; in his work on idealization, he discusses matching models, while, in his book, he discusses what he calls the "enriched, schematic, and genuine contents" of a mathematical representation in addition to the basic contents [38, pp. 29-33]. I will focus here on his discussions of idealization, and then say a bit about his enriched, schematic, and genuine contents.

Generally speaking, idealization involves accepting assumptions known to be false. In the context of mathematical modeling, this often involves accepting a false assumption about the physical system in order to make the mathematics tractable. Consider, for example, the field of continuum mechanics, which studies macroscale properties of matter. In continuum mechanics, matter is modeled as if it were continuous whereas we know that matter is discrete, being composed of finitely many particles. However, the treatment of matter as continuous allows modelers to use the mathematical tools of infinitesimals and calculus in analyzing the properties of matter.

Pincock considers the example of the continuum-mechanical treatment of heat dispersion:

> My example involves replacing a difference equation, that is, an equation put in terms of discrete differences, with a differential equation. What is sometimes called Newton's law of cooling states that the amount of heat per unit of time that passes from a warmer plate 2 to a cooler plate 1 is
>
> $$\Delta Q/\Delta t = (\kappa A|T_2 - T_1|)/d$$
>
> where T_2 and T_1 are the respective temperatures of the plates, A is their area, d is their distance from each other, and κ is the thermal conductivity of the material ... Now, I do not want to claim that no amount of mathematical idealization went into producing this representation, but will emphasize only that it is formulated in terms of finite differences of heat over finite periods of time. For this reason, it stands much closer to experimental practices than my second equation, the one-dimensional heat equation:
>
> $$\alpha^2 u_{xx} = u_t$$
>
> where $\alpha^2 = \kappa/\rho s$, κ is again the thermal conductivity of the material, ρ its density, and s the specific heat of the material. Here, $u(x,t)$ tracks the temperature of point x at time t, and the subscripts indicate partial differentiation with respect to that variable. [34, pp. 957-8]

The movement from the finite, discrete quantities of Newton's law of cooling to the infinitesimals in the one-dimensional heat equation is a prime example of the mathematical idealization employed in continuum mechanics. The natural question then, given the mapping account as described in the previous section, is how the mapping account should interpret the success of the one-dimensional heat equation. After all, the very construction of the one-dimensional heat equation, involves the deliberate acceptance, coded in the mathematics, of the false notion that matter is continuous; the use of derivatives of temperature u over the distance variable x requires that u be well-defined and continuous over arbitrarily small variations in x. However, we know that matter is made of atoms, and temperature, or u, is generally thought of now as a measure of the average kinetic energy of molecules in a given region. So, based on these theoretical considerations, u is only definable in regions containing at least one molecule. Thus, u cannot, theoretically, be well-defined in arbitrarily small regions. And so we have an instance of idealization.

In order to take this example into the purview of the mapping account, Pincock introduces an intermediate stage, called the "matching model," in the relation between the physical system and the model, which he now calls the "equation model." Pincock writes that "the role of the matching model is to parallel perfectly all the physical features of the physical situation," [34, p. 962] noting that this results in a trivial isomorphism between physical system and matching model. Pincock then envisions a variety of mathematical transformations connecting the matching model and the equation model, not strictly limited to homomorphisms. That is, Pincock seems to imagine that we have the physical phenomenon, as it is in the world, from which we can derive his "matching model," which "parallels perfectly all the physical features of the physical situation." He then imagines that there exists a series of mathematical transformations, such as limiting processes, that transform the matching model into the equation model, the piece of applied mathematics being analyzed.

In the case of the heat equation, Pincock labels the transformation carrying us from the matching model to the equation model a sort of "smoothing out," continuing on asking

> But smoothing out how? If we place no restrictions on this relationship, then we have no account of what makes an idealized scientific representation good. And if we place rigid restrictions, it seems that we risk lableing as bad some representations that scientists clearly think of as adequate.
>
> My proposal is to go contextual. We bring in the goals that the scientists have in mind for their representation. In the heat equation case, the goal is most likely to be to represent the medium scale temperature dynamics of the iron bar for a short period of time. This provides for a certain threshold of error. So, in such a case, if there is a mathematical transformation from the equation model to the matching model that falls within this threshold, then we have a good or adequate idealized representation. If, despite the beliefs and intentions of the scientists, there is no such mathematical transformation, then the idealized representation is bad or inadequate. [34, p. 962]

And this is how Pincock envisions accounting for mathematical idealizations in his mapping account.

Now, there are a couple of points to note about Pincock's introduction of matching models here. The first, more general point is that Pincock seems to be keeping mapping as the guiding light in extending his account to mathematical idealizations. I interpret this as Pincock taking true representation to be the core feature of mathematical representations. The role of matching models and the transformations that take us to the equation model seems to be that of indicating a certain *approximation* of truth, a certain *approximation* of true representation, true mapping. The pragmatics of context and tractability play a secondary role in determining the error thresholds allowed, the allowable transformations between matching model and equation model in order to arrive at a "good or adequate representation." Despite concession that pragmatics and context must play some sort of role in an account of applied mathematics, these are still subservient to the ideal of true representation. This will be a point I return to, as the account I sketch out does not hold true representation as the primary ideal and standard by which applied mathematics is judged. But more of that later.

A second point to note at this stage is that Pincock seems to assume the existence of a "matching model." Pincock never seems to entertain the possibility that there does not exist a matching model for a situation (nor does he seem to doubt their uniqueness), rather focusing on whether or not there exist admissible mathematical transformations between that and the equation model.

A third and final point to note is that Pincock introduces matching models, not necessarily as a methodological tool scientists use when deriving equation models, but more as tools to be used in the analysis of equation models when we want to judge their adequacy. There certainly may be a sense in which matching models play a role in the historical derivation of mathematical models, but this is far from clearly the case, and I have doubts about whether it is true. So, the crucial notion to hold onto is the lack of attention to historical derivation in Pincock's treatment, as this will be another place where I believe his account and mine to part ways.

I would like to finish this section by giving some attention to the ideas that Pincock discusses in his more recent book, those of the enriched, schematic, and genuine contents of mathematical models. Recall that Pincock labels as the "basic contents" of a mathematical representation the structure-preserving relation between physical system and mathematical model.

Pincock's notions of enriched and schematic content are closely related to the matching models that he discusses in previous work. The enriched content of a mathematical model refers to derived mathematical elements of the mathematical model and further allowable structural relations between physical system and mathematical model. The schematic content of a mathematical model refers to elements of the mathematical model that become decoupled from physical interpretation.

In the case of the heat equation, Pincock gives two examples of enriched content. In specifying the relation between values of the function $u(x, t)$ and the temperature in a given iron bar, we want to consider small regions of the (x, t) plane in addition to points, because temperature change as a property of matter over time is defined in these regions, rather than at individual spatiotemporal points. That is, there may be a sort of coarse-graining procedure involved in the structural relation between the heat equation and a given physical system meant to be represented by it. Pincock states that

> The threshold here can be set using a variety of factors. These include our prior theory of temperature, the steps in the derivation of the heat equation itself, or our contextually determined purposes in adopting this representation to represent this particular iron bar. [38, p. 31]

So, again, we see the pragmatics of context, involved physical concepts, and the history of a given representation playing a role in Pincock's account, but again these are involved only in calibrating how closely the representation approximates true mapping, true representation. Additionally, Pincock adds that the characteristic constant α in the heat equation can be regarded as part of the enriched contents, since there is not a direct mapping to a fundamental property of the bar. We will return later to the treatment of characteristic constants, but, on the sort of account that I advocate in the conclusion of this thesis, Pincock's approach overly flattens the work that these constants do.

Pincock gives one example of schematic content in the heat equation; he notes that when scientists use the heat equation to model temperature dynamics in a given iron bar, they sometimes represent the bar as infinitely long; they allow x to go to infinity. In this sense, the "length" of the bar in the representation becomes decoupled from the length property of the actual bar being represented; it is a feature of the representation that has no physical analog, and should not be given a physical interpretation.

Pincock concludes his discussion of contents by noting that the schematic contents leaves unspecified parameters in the content of the representation; in the example of the heat equation, the length being represented. When these parameters are fixed, we arrive at what Pincock calls the genuine contents of the mathematical representation. This fixing of the the unspecificed parameters in the schematic content is again context-dependent, and Pincock notes that "[t]he fixing of the parameters of the schematic content can be a complex and case-specific affair" and "may be fleshed out differently for different target systems." [38, p. 32]

This, then, is the apparatus by which Pincock proposes to analyze mathematical representations, an apparatus whose goal is to be able to specify accuracy conditions. That is, an apparatus that specifies conditions by which a mathematical representation is judged to be a good representation of its target system.

1.2 Mapping and Analytic Mathematics

Going back to basic contents, that structural relation that is supposed to obtain between the mathematical representation and the target system, it was clear in the Bridges of Königsberg case how this relation plays out. In fact, it was simply a matter of overlaying the graph on a map of the bridges; the homomorphism holding between the graph and bridges was quite explicit and easy to see. However, this overlaying method does not work for all cases of applied mathematics. It may seem to work well for pieces of applied mathematics falling under the more geometric fields of mathematics, such as topology, Riemannian geometry, and graph theory. But we cannot use this overlaying method for instances of more algebraic and analytic pieces of mathematics, such as those using group theory, the differential calculus, and complex-valued functions.[8] In fact, it is unclear at best how a structural relation is to obtain at all between a representation utilizing this type of mathematics and a physical system.

I will use analytic mathematics in particular as an impetus to push back against Pincock's mapping account, but I hope to make clear that Pincock's mapping account is also insufficient in analyzing pieces of applied mathematics drawing from geometric and algebraic mathematics.

Pincock considers an example of analytic mathematics, the heat equation, which is a partial differenital equation (PDE), and so we should look at how he sees a mapping holding in this sort of case. To review, the heat equation is given by

$$\alpha u_{xx} = u_t$$

or, using the more familiar symbols for partial differentiation,[9]

$$\alpha \frac{\partial^2 u}{\partial x^2} = \frac{\partial u}{\partial t}$$

Pincock states that

> we think of the equation as cutting down the complete class of models reflecting all logically possible combinations of position, time, and temperature to those that the equation will permit. Each such model will have as its domain all triples of real numbers, and its second position will have an admissible trajectory that selects a series of triples of position, time, and temperature that are consistent with the heat equation... For this class of models to become a representation of some iron bar, a scientist must believe that there is an isomorphism between the temperature states of the iron bar over time that preserves the position, time, and temperature magnitudes instantiated in the iron bar. If there is such an isomorphism, then the representation is true. If not, then it is false. [34, pp. 960-1]

[8]The field of mathematics is generally divided into three main subfields: algebra, geometry, and analysis. The first is said to derive from counting and arithmetic, the second from spatial reasoning, and the third from infinity and infinitesimals. These are not hard distinctions, nor are they necessarily comprehensive in a satisfactory way (whence combinatorics?), but it is nonetheless the generally-accepted division of academic mathematicians, as evidenced in the design of graduate programs in mathematics.

[9]For those unfamiliar with partial differentiation, it is intuitively very similar to ordinary differentiation. The partial derivative of a multivariable function $f(x, y, z, \dots)$ with respect to one of the variables, say x, at a point $(x_0, y_0, z_0, \dots)$ is defined as

$$\frac{\partial f}{\partial x} = \lim_{h \to 0} \frac{f(x_0 + h, y_0, z_0, \dots) - f(x_0, y_0, z_0, \dots)}{h}$$

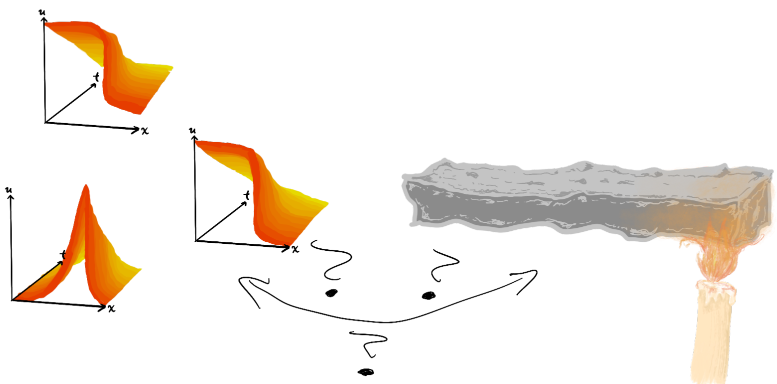

Figure 1.2: The heat equation and its represented system. The mathematical structure is "trajectories in $\mathbb{R}^3$" and the physical system is a heated iron bar. How do we draw the structure-preserving map here? (Author illustration)

So Pincock identifies a "structure" that is given by the heat equation: a set of trajectories in $\mathbb{R}^3$, the 3-dimensional space of real numbers.[10] (See Fig 1.2) He then asserts that "the scientist must believe" that an isomorphism holds between possible states of the iron bar and the set of admissible trajectories in $\mathbb{R}^3$. Even granting that Pincock identifies the "correct" structure associated with the heat equation, there are still several questionable assumptions that go into this analysis.

First, it is not at all clear how an isomorphism can hold even between a set of trajectories in $\mathbb{R}^3$ and possible states of an iron bar over time. A trajectory in $\mathbb{R}^3$ is a set of triples of real numbers, whereas a possible evolution of the state of an iron bar over time is decidedly not. How can we then draw an isomorphism between the two? Pincock begins to address this at one point, noting that

> there is an important difference between talking about a concrete system made up of objects and linked together by concrete relations involving quantities and properties and a set theoretic structure... [38, p. 28]

Pincock then proposes a solution to this gap between set-theoretic structure and concrete system:

> Suppose we have a concrete system along with a specification of the relevant physical properties. This specifciation fixes an associated structure. Following Suárez, we can say that the system instantiates the structure, relative to that specification, and allow that structural relations are preserved by this instantiation relation. [38, pp. 29]

But all Pincock seems to do here is kick the gap back a step; we are now faced

[10] By $\mathbb{R}^3$, mathematicians denote the set of all triples of real numbers (x, y, z).

with the question of how a concrete system can "instantiate" a set-theoretic structure.

This addition of an instantiation step between mathematical representation and concrete system is very similar to Pincock's earlier addition of the matching model between equation model and target system, and the same problem plagues both. The matching model is supposed to be trivially isomorphic to the target system, and there is supposed to be an easy way the instantiation relation holds between a set-theoretic (mathematical) structure and a concrete system, but it is unclear how this is possible if the matching model/set-theoretic structure are mathematical structures and the target system/concrete system are decidedly not. The intermediary step does not solve the underlying problem of how something mathematical hooks up to something concrete.

One hint of how Pincock sees this problem being resolved can be seen in his mention of "quantities," "magnitudes," and "physical properties" when he discusses matching models and the instantiation relation. Pincock seems to assume that quantities, magnitudes, and physical properties all lie in the physical world, or at least have analogues in the physical world such that a translation of these into the language of mathematics is "trivial," as exhibited by Pincock's assertion that there exists a trivial isomorphism between the matching model and physical system [34, p. 962]. But it is unclear at best that this is even close to being the case! There is no reason for us to assume that Nature is written in numbers. There is no reason for us to assume that there is an easy correlation between the state of a system and a set of numbers. There is no reason for us to assume even that the physical concepts of our scientific theories pick out physical properties in the world in a way that facilitates "trivial" structural relations between mathematics and world.

To be sure, there have been arguments put forward for all of these positions. As to the first, many of arguments advocating that mathematics is the language of Nature rest on the success and ubiquity of mathematics in physical theory, the very fact that we are trying to account for in developing an account of applied mathematics. So we should not take this as a starting place. As to the second, the various efforts to develop philosophical accounts of measurement should persuade us that the correlation between physical states and sets of numbers is anything but trivial.

The third of these, the assumption about how the physical concepts of our scientific theories connect to the world, is a little trickier to navigate. I should note that Pincock recognizes the controversiality of assuming that concepts, both physical and mathematical, hook up to the world in the way that we intend them to and in a way that facilitates a "trivial" structural relation holding between a mathematical structure and a concrete system.[11] Additionally, af-

[11] Pincock states that "In both the mathematical and physical cases, then, I ascribe to agents an ability to refer. A convenient shorthand for these abilities is to say that these agents possess the relevant concepts. It is a delicate issue to determine how we acquire these concepts and what they contribute to the content of our representations."[38, p. 26]. Pincock then goes on to state that he assumes semantic internalism for mathematical concepts and semantic externalism for physical concepts.

ter Pincock develops his account in a more detailed way, he does return to the question of concepts. However, it does not seem right to me that we build our account of applied mathematics on so shaky an assumption, even if it is weakened/removed at a later stage. I will argue in the remainder of this thesis that physical concepts perform much of the work in mathematical representations, and so we must begin building an account of applied mathematics from a realistic understanding of what physical concepts are and the role that they play.

This more realistic understanding of physical concepts and the role that they play is painted well by Hasok Chang [7, 8, 9] and Mark Wilson [47]. Chang, through his work on the concepts of temperature, water, and acidity, shows clearly how concepts evolve through time, and how old notions regarding a concept may become encapsulated in future iterations of that concept. He also shows how these older notions and the historical trajectory of a concept may come into play as scientists attempt to extend the domain of the concept, as happened with the expansion of the temperature concept above and below temperatures admissible for standard air and mercury thermometers.

Wilson, on the other hand, has challenged the "classical picture" of concepts, that a direct, intended correspondence holds between concept and world, saying that

> straightforward classicists such as Bertrand Russell invariably assume that the nature of a given predicate's worldly correspondence is inherently self- guaranteeing, in the sense that once we adequately grasp a term's meaning, then we will be able to discern, after sufficient armchair analysis, the basic structure of its intended correspondence with the world. [47, p. 80]

Wilson instead argues that the underpinnings of our concepts are constantly changing as we try to bring them in closer contact with the world. Wilson provides several notions by which we can begin to understand the complex relationship between our concepts and the world, including the notions of variable reduction and theory facades. In assuming, even just at the beginning, that physical concepts have this sort of intended, self-guaranteeing correspondence with physical properties in the world, Pincock has built the foundations of his mapping account on a sort of "Wilsonian sin."

There is a second point to note about Pincock's mapping account regarding metaphysical assumptions. There are several questionable metaphysical assumptions, both about math and about world, in Pincock's account. First, in asserting a structural relation between mathematics and physical world, Pincock assumes the existence of structure both in the world and in mathematics. Further, in asserting that "the content [of mathematical scientific representations] is exclusively structural," [38, p. 25] Pincock is asserting that the primary subject of interest in analyzing a mathematical representation to understand how it aids in our understanding of the world is the structure of the mathematics. Now, if we take a structuralist approach to mathematics[12] and a structuralist

[12] As, for example, argued for in [39], [40], and [33].

scientific realist position with regard to the physical world,[13] this view makes sense. However, these positions would need satisfactory arguments that do not reference what we wish to explain using an account of applied mathematics, and I have not seen an argument that I find satisfactory.

There is an additional metaphysical difficulty in assuming anything about the nature of "properties" in the physical world. What these are, how they are in the world, and what epistemic access we have to them are all questions that need to have at least a sketch of an answer in order for properties to either provide the foundations of a structure or play a role in the structural relations that are supposed to hold between mathematics and world.

The view of applied mathematics that I will advocate later on in the Chapter 3 makes use of features of mathematics far beyond structures and does not have its metaphysical grounding in the physical world, thereby avoiding the tangled views regarding the metaphysical status of physical properties in the base of the view. The view that I advocate will have its grounding instead in the activities of humans, the efforts of scientists to craft tools that can be used to navigate the world around, tools that scientists try to bring into closer and closer contact with the world as it is.

[13] As advocated for in [26].

Chapter 2

Case Study: Prantl's Boundary Layer Solution

2.1 An Interpretive Problem: Prandtl's Boundary Layer Solution

Before continuing on, I want to get a clearer articulation of what exactly it is that we want an account of applied mathematics to do for us, as this will then allow us to make stronger judgments regarding Pincock's mapping account, what it does well and what it is missing. It will also allow us to draw lessons from problem cases in order to begin sketching out what an account of applied mathematics should look like.

Recall that the central question that we would like an account of applied mathematics to answer is, "Why does applied mathematics aid in our understanding of physical phenomena?" Notice that this question can take on a variety of interpretations; most significantly, it can vary in specificity. At its most general, this question takes the form, "How does applied mathematics as a whole aid in our understanding of the physical world?" However, the most specific form is also of interest, asking, "How does this specific piece of applied mathematics aid in our understanding of this specific physical system?"[1] We want an account of applied mathematics to be able to provide answers to both of these questions, and it is the second of these that the mapping account struggles with.

Consider again the Bridges of Königsberg case; we can identify a very explicit sort of homomorhpism between the graph and the actual bridges. However, we should ask whether the identification of this mapping is explanatory, whether the mapping in itself answers the question of how the graph aids in our understanding of the actual bridges.

[1] One way to think of these two sorts of questions is in relation to Batterman's type-(i) and type-(ii) why-questions [2]. There is an interesting parallel between the importance Batterman accords to type-(ii) why-questions as opposed to type-(i) and the importance that I accord to the second type of question as opposed to the first.

At the very least, the answer to this is not a resounding yes; it is unclear if the mapping itself is explanatory. In a sense, it is, since the mapping provides a way to connect the nonexistence of an Eulerian path on the graph with the impossibility of walking the seven bridges of Königsberg once. However, in a sense this is not explanatory, since it just identifies a connection without providing any sense of why this connection should hold. The reason that the graph aids in our understanding of the bridges problem is that it abstracts away all features unnecessary to answering the question, leaving behind a formal structure of a particular kind, the properties of which can be studied. Whether this can be regarded as part of Pincock's mapping analysis or what is extrapolated from it is a difficult question to answer, but it is not clearly one way or the other.

Consider the case of the heat equation. The answer that the mapping account gives to the question of how the heat equation contributes to our understanding of heat dispersion in iron bars is even less satisfactory than the answer it gave for Königsberg. The mapping account asserts a certain structural relation between trajectories in $\mathbb{R}^3$ and possible evolutions in the state of a given iron bar over time, that "the equation [cuts] down the complete class of models reflecting all logically possible combinations of position, time, and temperature to those that the equation will permit," [34, p. 960] but this does not even begin to give an answer to our question. When faced with this question, a more likely answer to be given by a scientist is that the heat equation isolates a dependency between temperature distribution and position and time, and that it identifies the general form of this dependency. The scientist's answer may also include a history about the characteristic constant α, which is barely given any mention in Pincock's analysis but seems as though it should play a major role in the answer to our question.

This is the sort of answer that we should be able to provide when faced with this sort of question. Notice that this answer takes almost a narrative form; a dependency is isolated, and then a general form is given for this dependency; there is a story to be told about the incorporation of constants, what aspects of the physical system they account for, and why those aspects are represented using constants instead of variables. We want our answer to explain *how* the mathematical representation comes to aid in our *understanding* of the system, not *whether* it *represents* the system. In this respect, the mapping account offers little more than the mapping intuition; simple assertion and identification of a "hooking up" between mathematics and world, no matter how detailed and nuanced, does not provide the explanatory narrative that we need.

Further, the proponent of the mapping account cannot just shrug her shoulders and assert that the mapping account "isn't meant to answer these sorts of questions." After all, this is the sort of question that we want an account of applied mathematics to be able to answer. While the mapping account may fare well in some cases and may be aimed more at the general question, if it fails at answering the specific question, it cannot serve as a comprehensive account of applied mathematics.

So far, my critiques of the mapping account have been quite abstract and general; even my critique of the mapping account's ability to answer the specific

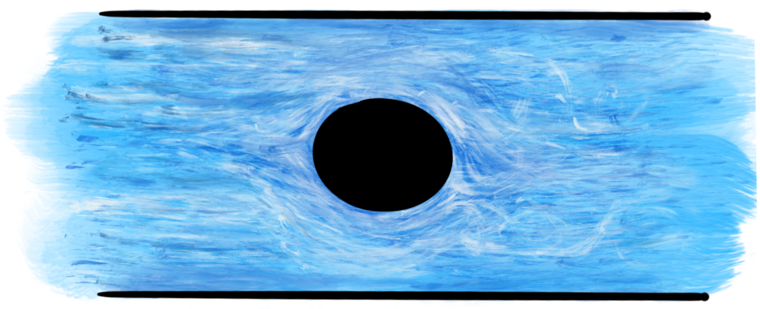

Figure 2.1: We can conceive of the Navier-Stokes equation as Newton's second law for an infinitesimal volume of fluid, as shown on the right. The velocity of the volume of fluid is shown in red. The force of viscosity on each surface of the volume is shown in green, the force of pressure in orange. (Author illustration)

question has been sketched out rather crudely. I will now turn to a more detailed case study in order to flesh out where exactly the mapping account goes wrong in answering the specific question, and what a more satisfactory account of applied mathematics needs to be able to do in order to tackle this case.

The piece of applied mathematics I consider here is Prandtl's boundary layer solution to the Navier-Stokes equations. This is an example that Pincock himself discusses, and Pincock recognizes several interpretive problems that the mapping account runs into when trying to analyze Prandtl's solution.

The Navier-Stokes equations are regarded as the most important equations in fluid mechanics; they are the best model we have of fluid flow. The main interpretation of them is that they consider an infinitesimal volume of fluid and apply Newton's second law (see Fig 2.1). The equations are given by:[2]

$$\rho\left(\frac{\partial \mathbf{v}}{\partial t} + \mathbf{v}\cdot\nabla\mathbf{v}\right) = \mu\Delta\mathbf{v} - \nabla P - \rho f(\mathbf{x}, t) \tag{2.1}$$

$$\nabla\cdot\mathbf{v} = 0 \tag{2.2}$$

where ρ is the density of the fluid (conceptualized, in the main interpretation, as the "mass" of the infinitesimal fluid volume), $\mathbf{v}$ is the velocity of the given infinitesimal fluid volume, μ is the viscosity constant characteristic of the given fluid being modeled, P is the pressure field acting on the fluid, and $\rho f(\mathbf{x}, t)$ is the sum of external forces per unit volume acting on the fluid volume under consideration. Reading equation 2.1 as Newton's second law, we have the "mass" of the infinitesimal fluid volume, ρ, multiplied by its acceleration, the term in parentheses, and this is set equal to the sum of forces acting on the fluid volume. Equation 2.2 expresses the incompressibility restraint, a form of the conservation of mass for incompressible fluids.

[2]These are the equations as presented in [19].

Despite Navier-Stokes being the best model we have for fluid dynamics, engineers face problems when trying to apply it to practical contexts; in most practical contexts, the Navier-Stokes equations are intractable. It is almost impossible to extract a solution analytically in most cases, and numerical methods are only so effective, depending on the required level of precision for the given context.

One particularly important case where Navier-Stokes remains analytically intractable is that of fluid flow bordering a solid (as shown in Fig 2.1). This is important to be able to model, not only for pipe flow and various other fluid channels, but also for aerofoil design, which requires a high degree of precision. Faced with the intractability of Navier-Stokes in such cases and the need for higher precision, Ludwig Prandtl developed a solution for these cases that provides for a much greater degree of tractability, allowing solutions to be obtained analytically in many cases and thus resolving the precision problem. Since the intractability of Navier-Stokes results largely from the viscosity term, which contains second-order partial derivatives [36, p. 179], Prandtl divided fluid flow into two regions, or patches. In Prandtl's solution, there is a "boundary layer" region of fluid flow, which is the region arbitrarily close to the fluid-solid boundary, and a bulk flow region, which models the rest of the fluid flow.

The motivation for this division is that in the region close to the fluid-solid boundary, the boundary layer, viscosity's effect is significantly large compared to other terms in Navier-Stokes. However, elsewhere in fluid flow, in the bulk flow region, the effect of viscosity is negligible compared to other terms of the equation. So, flow in the boundary layer is modeled by a simplification of the Navier-Stokes equations licensed by the significant contribution of the viscosity term, and flow in the bulk region is modeled by a simplification of Navier-Stokes that simply eliminates the viscosity term, due to its negligible contribution there. Various procedures are then taken in order to match the two regions at their interface in a satisfactory way, providing a complete, tractable model of fluid flow bordering a solid.

Now, there are two major interpretive problems Pincock sees his mapping account analysis running into with Prandtl's solution. First, he notes that

> [o]ne concern is the reference of predicates across patches. If the rules governing each patch are so different, then how might we make sense of the claim that the predicates appearing in each patch pick out the same physical property? [36, p. 186]

That is, on a mapping account analysis of Prandtl's solution, there is no guarantee, for example, that the pressure variable P, which appears in the equations modeling both the boundary layer and the bulk flow, refers to the same "property" of pressure in both regions. Note that this is a problem resulting from the rather hefty assumptions about concepts and properties that I flagged earlier. As I will argue later, we do not need to assume this in order to arrive at an understanding of how Prandtl's solution contributes to our understanding of fluid flow bordering solids. Further, a satsifactory answer to the the question of how Prandtl's solution facilitates this understanding alleviates this worry anyways.

The more significant worry Pincock brings up is that, regarding the interface between the boundary layer and the bulk flow in Prandtl's solution,

> there is no real physical variation in the fluid that we can easily match to this edge, for example, it is not as if the fluid suddenly becomes non-viscous when it gets a certain distance from the object. [36, p. 187]

That is, a mapping account analysis of Prandtl's solution "sees" two distinct structures in the mathematics[3] and so would seem to conclude that there are two distinct structures in the physical system. But this is clearly not the case; there is no obvious sense in which the flow of a single fluid contains two distinct structures or regions.

However, I will argue that there is a third, even more significant problem with the mapping account analysis of Prandtl's solution; it is simply cannot provide a satisfactory answer to the question of how Prandtl's solution contributes to our understanding of fluid flow. I will argue that a satisfactory answer to this question must center the rich history lying underneath the surface of the Navier-Stokes equations, particularly in the concept of viscosity. In the next section, I will show that the Navier-Stokes equations contain, encased within, a compromise between two different concepts of viscosity which resulted from the divergence in the aims of fluid mechanics around the 18^{th} century. Then, in the following section, I will sketch out my own answer to the specific question, and I will show that this complex internal structure of the viscosity concept and the history of the Navier-Stokes equations play a vital role in this answer. I will then use this to argue that the mapping account's failure to account for these is what results in the problems outlined above.

2.2 The Derivation of the Navier-Stokes Equations and Prandtl's Solution

2.2.1 The Setting: French Mechanics at the Beginning of the 19^{th} Century

By the beginning of the 19^{th} century, France, and especially Paris, was a center for work in analysis and mechanics with a rich intellectual atmosphere. This is the environment in which Claude-Louis Navier (1785-1836) derived his equations for fluid dynamics, equations which eventually became the Navier-Stokes equations. As such, in order to understand the influences that shaped the development of the Navier-Stokes equation, we must explore the various currents in mechanics and mathematics at that time.[4]

[3] Though related through the aforementioned procedures matching the two regions at their boundary, these mathematical structures are still distinct in that they provide distinct equation models, thus, in Pincock's earlier parlance, cutting down the complete class of models in two distinct ways.

[4] As a historiographical note, I have chosen to use secondary sources for the most part for the current iteration of this project. However, I encourage the interested reader to delve into the primary texts mentioned, as I myself plan to do eventually in the next iteration of this project.

In the mid-18th century, the bulk of work in advancing the calculus and mechanics in Europe was done in Continental Europe. Ivor Grattan-Guinness [21, pp. 278-80] emphasizes four important figures arising in the 1720s and 30s, two of those four in Paris. Daniel Bernoulli (1700-1782) and Leonhard Euler (1707-1783) were both born in Basel in the Swiss Confederacy, with Bernoulli and Euler both serving at the Academy of Sciences in St. Petersburg at different points and Euler serving at the Academy of Sciences in Berlin for 25 years in the mid-1700s. Both wrote across the field of applied mathematics, making significant contributions in many fields. Meanwhile, Jean le Rond d'Alembert (1717-1783) and Alexis Claude Clairaut (1713-1765) spent most of their academic life in Paris, writing extensively on calculus and mechanics.

Ivor Grattan-Guinness notes that, for the latter half of the 18th century, "[v]irtually all major contributions [to analysis and mechanics] were made on the Continent, especially in France, and also in some German states and Russia." [21, p. 303] Euler, d'Alembert, and Bernoulli served as the major players in this period of extensive work in mechanics and analysis,[5] until their deaths. Grattan-Guinness then identifies three major figures that succeeded them: Joseph Louis Lagrange (1736-1813), Pierre Simon Laplace (1749-1827), and Adrien-Marie Legendre (1752-1833). Lalpace and Legendre spent their entire careers in Paris, and Lagrange, after holding a post at the Berlin Academy succeeding Euler, moved to Paris in 1787. Grattan-Guinness highlights one other trio of French intellectuals, Gaspard Monge (1746-1818), Charles-Augustin Coulomb (1736-1806), and Lazare Carnot (1753-1823), who worked in engineering mathematics [21, p. 304].

I will highlight two major programs in mechanics at the beginning of the 19th century that influenced Navier's derviation of his equations for hydrodynamics. The first of these is the analytical mechanics of Lagrange. Grattan-Guinness notes that, even from the beginning of his intellectual career, Lagrange's aim was a completely algebraic formulation of as much of mathematics as possible [21, p. 304]. Chief among his projects was the algebraization of mechanics. An important first step in this was the development, with Euler, of the calculus of variations in the 1750s, which involves considering small perturbations in a variable of a given function and the consequent behavior of the function.[6] This calculus of variations, in addition to the new partial differential calculus, was then gradually applied to problems in mechanics by both Euler and Lagrange.

[5] Grattan-Guinness describes "analysis" as covering "the calculus and related topics such as functions, series and differential equations."[21, p. 303]

[6] One example of the sorts of problems the calculus of variations was meant to solve is isoperimetric problems, optimization problems where some constraint is given. A simple and old example of this is finding, for a given perimeter, what shape with that perimeter has the largest area (hence the name "isoperimetric problems"). However, the sorts of problems the calculus of variations was meant to handle are more in line with the following problem from 1697: to "find the curve of quickest descent of a particle under gravity between two fixed points." [21, p. 293] Here, we are optimizing an integral given the constraint of the two fixed points, and we are looking for a function as the answer. Thus, we must consider variations in the function and how they affect the integral. As an interesting note, the answer to this problem is a curve called a brachistochrone.

The extension of Lagrange's work to the algebraization of mechanics took time. It was in a 1764 paper on lunar theory that Lagrange first explicitly stated his preference for a more analytical foundation of mechanics, that the foundations of mechanics be in d'Alembert's principle and the principle of virtual work.[7] These two principles were stated by Lagrange in the later *Mécanique analitique*, first published in 1788 as:

> **Princple of Virtual Work:** "If an arbitrary system of any number of bodies or mass points, each acted upon by arbitrary forces, is in equilibrium, and if an infinitesimal displacement is given to this system, in which each mass point traverses an infinitesimal distance which expresses its virtual velocity, then the sum of the forces, each multiplied by the distance that the individual mass point traverses in the direction of this force, will always be equal to zero." [28, Part I, Section I, §17]
>
> **d'Alembert's Principle:** "[I]f it is imagined that upon each body the motion that it must follow is impressed in the opposite direction, it is clear that the system would be at rest. Consequently, these motions should cancel those that the bodies would have and that they would have followed without their mutual interaction. Thus there must be equilibrium between all these motions or between the forces which can produce them." [28, Part II, Section I, §11]

Lagrange formally laid this foundation and more fully developed the algebraic (or analytic) mechanics that resulted in his manuscript *Mécanique analitique*, likely finished in 1782 and published with Legendre's help in Paris in 1788.

The *Mécanique analitique* was free of any diagrams; Lagrange wrote in the preface to the first edition that,

> No figures will be found in this work. The methods I present require neither constructions nor geometrical or mechanical arguments, but solely algebraic operations subject to a regular and uniform procedure. Those who appreciate mathematical analysis will see with pleasure mechanics becoming a new branch of it and hence, will recognize that I have enlarged its domain. [28, p. 7]

Lagrange's aim was to reduce all of statics and dynamics to d'Alembert's principle and the principle of virtual work. Lagrange used the principle of virtual work to reduce a statics problem to the problem of finding solutions to an equation where the sum of the products of each force and its "virtual velocity" is zero. Here, Lagrange defines the "virtual velocity" as the movement of a mass point at the point of application of the force along the force's line of action given an infinitesimal displacement. Lagrange calls the product described as the *moment* of the force, expressed as Pdp, where P is the force and dp its "virtual velocity." Lagrange then proceeds to describe how to free the principle

[7] Lagrange himself called it the "principle of virtual velocities," but, in order to avoid conceptual confusions, I will anachronistically refer to it as the principle of virtual work. Grattan-Guinness notes that "[Lagrange] probably chose the name 'virtual velocities' under the influence of d'Alembert's suspicions of notions such as force and work."[21, p. 327]

of virtual work from its geometric base, enabling a purely algebraic treatment.

Lagrange next discusses how to use d'Alembert's principle to reduce any dynamics problem to a statics problem. Notice that d'Alembert's principle is essentially an alternative formulation of Newton's second law, where the negated product of mass and acceleration is conceived of as a fictitious "inertial force." Thus, since the dynamics problem is now a statics problem, it may be solved using the principle of virtual work. This process, called the "method of moments" by Olivier Darrigol [12, p. 110], had a great influence on Navier's derivation of the equations of hydrodynamics. Additionally, it is imporrtant to take note of the fact that Navier was doing his work in fluid dynamics just as calculus and mechanics were becoming algebraized.

Now, the other dominant program in mechanics at the beginning of the 19^{th} century was the Laplacian molecular program, less a project in the mathematical methodology of mechanics and more a program in the metaphysical methodology of mechanics. Under the influence of Newton's law of universal gravitation, Laplace, along with Claude Louis Berthollet (1748-1822), insisted that all physical phenomena be explained through recourse to intermolecular forces acting over "insensible distances," with these forces sometimes between molecules of "ponderable matter" and sometimes between "ponderable matter" and "imponderable fluids." The equations for such forces were supposed to follow inverse square laws with regard to distance, as in Newton's universal gravitation, completing the analogy to Newton's law of universal gravitation. Laplace declared in the 1796 edition of his *Exposition du système de monde* that "we shall be able to raise the physics of terrestrial bodies to the state of perfection to which celestial physics has been brought by the discovery of universal gravitation."[8]

As far as the pervasiveness of the Laplacian molecular program, Laplace and his colleagues held positions in the École Polytechnique, the *Annales de chimie et de physique*, and in the Academy of Sciences,[9] and these positions created what Robert Fox describes as an almost "totalitarian" intellectual atmosphere in French physical sciences [17, p. 136]. Thus, the Laplacian program pervaded the atmosphere that many of the mathematicians and engineers working on fluid mechanics came of age in around the beginning of the 19^{th} century, with some becoming major players in the politics of the Laplacian program's dominance. In particular, Navier, Augustin-Louis Cauchy (1789-1857), Siméon-Denis Poisson (1781-1840), and Adhémar Barré de Saint-Venant (1797-1886), all graduates of the École Polytechnique, were educated in the Laplacian regime, and all of them proceeded to do work in fluid dynamics.

Now that I have given a clearer picture of the the atmosphere surrounding French mechanics/physics around the start of the century, I will briefly turn to the state of fluid mechanics in particular around that time.

[8]Translation from [17, p. 95].

[9]See [17] section 2 for a detailed description of the social positioning of Laplace, Berthollet, Poisson, Biot, and other advocates of the Laplacian style of phsyics and chemistry.

2.2.2 Practical Hydraulics vs. Rational Hydrodynamics

The study of fluids had remained a relatively cohesive field throughout its long history, from the early inventions of the Egyptians and Babylonians utilizing the properties of fluids, up through Roman aqueduct and pipe design and Leonardo da Vinci's sketches of fluid flow. However, around the middle of the 18^{th} century, the study of fluids diverged into two main branches: hydraulics and hydrodynamics.[10] This split was in large part due to a growing rift between two aims of studying fluids.

On the one hand, there were hydraulicians, practical engineers interested in studying fluid flow for the purposes of controlling it, for example in channels and fountains. The hydraulicians of the 18^{th} and 19^{th} century included, for example, Pierre du Buat (1734-1809), Gaspard de Prony (1755-1839), and Pierre-Simon Girard (1765-1836). As their focus was on coming to an understanding of fluids with an eye towards developing technology involving fluids, the hydraulicians aimed for empirical adequacy in mathematical representations of fluid flow.

On the other hand, there were the mathematicians, who wanted to develop hydrodynamics equations on clear, rational grounds with the utmost rigor. These included Euler, d'Alembert, and Lagrange, all falling in the tradition of the new rational mechanics. They centered rigorous derivation and generality above empirical adequacy of mathematical representations of fluid flow.

It is important to briefly note that these distinctions are not without crossover; there are figures who I characterize as engineers who possessed great mathematical dexterity and were concerned with underlying theory. Likewise there are figures who I characterize as mathematicians who were aware of practical concerns and sometimes responded to them. These distinctions, therefore, more reflect general attitudes than completely disjoint camps.[11]

It was around the 18^{th} century that these two groups began to grow apart, especially as the mathematicians striving for a rational hydrodynamics set empirical adequacy to the side in their search for rigorous derivation. This can be seen, for example, in the Euler-d'Alembert paradox that resulted from Euler's hydrodynamics equations. Euler derived his hydrodynamics equations in a memoir published in 1755; his equations can be given in modern notation as[12]

$$\rho \left[\frac{\partial \mathbf{v}}{\partial t} + (\mathbf{v} \cdot \nabla)\mathbf{v} \right] = \mathbf{f} - \nabla P \tag{2.3}$$

where, as in the Navier-Stokes equations, ρ is the density of the fluid, $\mathbf{v}$ the velocity, $\mathbf{f}$ the external force, and P the pressure. Euler derived his hydrodynamics equation by considering a small volume element of fluid and applying Newton's second law, equating the sum of forces acting on the volume element with the product of the volume's mass and acceleration. Euler considered only

[10] The following discussion of the branching of hydraulics and hydrodynamics relies invaluably on the first section in [12] and chapters 6-10 of [41].

[11] In fact, pure mathematics as a discipline in and of itself only arose as the 19^{th} century progressed. [23, p. 3]

[12] From [12, p. 98].

the forces due to pressure exerted on the volume element and any external forces acting on the fluid. [12, p. 98]

Both Euler and d'Alembert found that, for certain bodies immersed in flow, the rational hydrodynamics of Euler's equations predicted that the immersed object felt no resultant pressure, a result that seems flatly absurd. Euler found in 1745 that any body immersed in steady flow experienced a resultant pressure of zero. Later in 1752, d'Alembert found that a specifically head-tail symmetrical body immersed in steady flow experienced a resultant pressure of zero. Euler's response to this paradox was to return to Edme Marriotte (1620-1684) and Newton's older theory of fluid resistance, according to which the resistance an immersed body felt was solely determined by the impact of fluid molecules on the front of the body.[13] Darrigol notes that, despite the "empirical inexactitude and theoretical weakness" of Euler's approached, it remained popular due to its explaining several other empirically-observed facts about resistance felt: that it was proportional to the density of the fluid, the squared velocity of the flow, and the cross section of the body [12, p. 99].

On the other hand, d'Alembert's response to the paradox was more hands-off, since, d'Alembert was far less concerned with practical problems than Euler was [12, p. 99]. In fact, when writing about the paradox in 1768, d'Alembert wrote that it was "a singular paradox which I leave to future geometers for elucidation."[14] One of d'Alembert's notable followers, Lagrange, followed a similar approach.

By the beginning of the 19^{th} century, this split between hydraulics and hydrodynamics had reached its height, with the general consensus being that rational hydrodynamics could not be used by engineers in practical problems involving fluid resistance and flow retardation. Further, the knowledge that the engineers drew on in such problems was in large part the result of extensive observation and collection of measurements performed by hydraulicians [12, p. 105]. Darrigol notes that it was through the engineers that the concept of internal fluid friction from Newton was revived, although it did not yet enter into equations modeling fluid flow.

It is important to look at why no one considered adding terms to Euler's hydrodynamics equations, published in 1755, before Navier did around 1822, especially given the Euler-d'Alembert paradox and other insufficiencies. Darrigol gives several possible explanations which will also help to provide a fuller context for this period in mechanics. First, Darrigol notes that "the new hydrodynamics was part of a rational mechanics that valued clarity, formal generality, and rigor above empirical adequacy." [12, p. 106] In this way, the empirical inadequacy of Euler's equations may not have provided strong enough motivation to modify the equations, since empirical adequacy was not the primary motivating value.

Second, Darrigol reminds us that Euler's equations were among the first partial differential equations written, as well as being nonlinear, and so required a great amount of mathematical competency to understand, much less modify.

[13] This is an example of the blurriness in my earlier distinction between the practical engineers and the rational mathematicians.

[14] Translation in [12, p. 99].

Further, even those who had the mathematical competency to modify Euler's equations still would have lacked a mature understanding of internal fluid friction, as this concept was still taking shape through the work of hydraulicians. [12, p. 106]

Finally, Darrigol writes that most French mathematicians in the business of inventing new partial differential equations accepted d'Alembert's princple, mentioned earlier, meaning that the hydrodynamics equations were to be gotten from the hydrostatics equations. However, Darrigol writes that the hydrostatics equations were already "solidly established" and so there seemed to be no other option than to just accept Euler's equations [12, p. 106].

So much for the mathematicians' treatment of fluids. What of the engineers? Darrigol states that, as little concerned as mathematicians were with the problem of accounting for the physical phenomenon of fluid resistance, they were even less concerned with practical problems such as modeling pipe and channel flow. As stated above, most available knowledge regarding fluid resistance and flow retardation was of an empirical nature. Further, since the publication of Marriotte's *Traité du mouvement des eaux* in 1683, many hydraulicians had assumed friction to be between the fluid and the walls bordering flow. This friction was supposed to be proportional to the "wetted perimeter" and to increase faster than the fluid's velocity [12, p. 100]. Various measurements had been performed by engineers measuring how much velocity was lost when water traveled through a pipe, the first being performed by Claude Antoine Couplet (1642-1722), who designed the water system in the palace of Versailles.

Gradually, more precise empirical data was compiled by hydraulicians, enabling them to form proportionalities (or lack thereof). For example, Du Buat proved that fluid friction did not depend on pressure, and Charles Bossut (1730-1814) found the retarding force to be proportional to the square of velocity. The measurements compiled by Couplet, du Buat, and Bossut informed the majority of flow retardation formulas by French and German hydraulicians, up until the middle of the 19^{th} century [12, p. 100]. However, the most popular formula, by de Prony in 1804, was also influenced by the study of fluid coherence by the engineer, Coulomb.

Now, before Coulomb, Du Buat in 1786 had already noticed that the "average fluid velocity" appearing in previous retardation formulas was not representative of actual flow, since, in actual flow, velocity increased with distance from the walls of flow, and flow velocity vanished at the wall in the case of very reduced flow. Du Buat even went on to propose a molecular mechanism for this, suggesting that a layer of fluid molecules adhered to the wall and that this layer's granular structure combined with molecular cohesion impeded the rest of flow. In 1800, Coulomb studied fluid coherence and came to similar conclusions, stating that

> The part of resistance which we found to be proportional to the velocity is due to the mutual adherence of the molecules, not to the adherence of these molecules with the surface of the body. Indeed, whatever be the nature of the plane, it is strewn with an infinite number of irregularities wherein fluid molecules take permanent res-

idence.[15]

In 1816, Girard, with a belief that Laplace's intermolecular cohesion forces used to explain capillarity could also be used to explain flow retardation in pipes, set out experimenting with fluid discharge from capillary tubes. Girard assumed that flow velocity vanished at walls, as noted by Du Buat, along with further assumptions from previous hydraulicians as to the form of the retardation formula. However, Darrigol notes numerous flaws in Girard's experimental methods. Most notably, both Girard's experimental methods and his theoretical speculations paled in comparison with the best French experimenters and theorists of his day [12, p. 105]. Even so, Girard was widely praised for his work.

2.2.3 Navier's and Others' Derivation

It was in the atmosphere of the Lagrangian analytical mechanics, the Laplacian molecular program, and the schism between hydraulics and hydrodynamics in which Navier came of academic age. Navier had actually been trained in both the traditions of practical engineering and rational mechanics, having attended both the École des Ponts et Chaussées and the École Polytechnique,[16] putting him in a unique position to tackle the problem of finding a mathematical representation for fluid flow that satisfied both the aim of empirical adequacy for the engineers and the aim of rigorous derivation for the mathematicians.

Before Navier began deriving equations for hydrodynamics, he worked on deriving the equations for the dynamics of elastic solids. It was in this context that Navier developed the method that he would eventually use to derive the equations of hydrodynamics. Darrigol notes that Navier was familiar with both Lagrange's new analytical mechanics, particularly the method of moments,[17] and Laplace's molecular program [12, p. 110]. Navier went on to combine the two approaches in order to derive the general equations of elasticity.

In deriving the equations of elasticity in a memoir presented in 1821,[18] Navier began with the solid in equilibrium and considered a macroscopic deformation of the solid. Navier proceeded to consider the restoring forces acting on the

[15] This passage is quoted in [12, p. 101] but is a translation of Coulomb's 1800 memoir "Expériences destinées à déterminer la cohérence des fluides et les lois de leur résistance dans les mouvements très lents."

[16] The École des Ponts et Chaussées gave training with a more practical bent, geared towards engineers, whereas the École Polytechnique had a more analytical training geared towards mathematicians. As an interesting aside, Gaspard de Prony was Engineer-in-Chief at the École des Ponts et Chaussées during Navier's time there, as well as a professor in analysis and mechanics at the École Polytechnique while Navier was there. Navier also just missed having Laplace as an examiner in analysis and mechanics at Polytechnique, with Laplace leaving in 1799 and Navier enrolling in 1802. See [22, p. 235].

[17] Darrigol writes that Navier particularly admired this method's ability to generate boundary conditions.

[18] Darrigol notes that Navier actually gave two derivations of the equations of elasticity, one "by direct summation of the forces acting on the given molecules" and the other "by the balance of virtual moments."[12, p. 110] Following Darrigol, I discuss the second of these, which Darrigol cites as Navier's favorite and the method which he eventually adapted to apply to hydrodynamics.

molecules of the solid, where this restoring force is attractive for increased intermolecular distance and repulsive for decreased intermolecular distance.

Navier then considered a virtual displacement of the molecules and proceeded to calculate virtual velocity with respect to the restoring forces, and he then calculated the moment that resulted, as in Lagrange's method of moments. Navier then obtained the total molecular moment that resulted, this being a discrete sum over two indices, which can be written as $\sum_{\alpha\beta}$, meant to represent summing over the moments between pairs of molecules α and β in the solid.

Next, Navier replaced one of the sums with a volume integral, proceeding to repeat this for the other sum. In both cases, the integrals are "weighted by the number of molecules per unit volume." [12, p. 111] As a result, Navier represented the total molecular moment by an integral, allowing him to then proceed by using the fact that the solid is only in equilibrium if this moment is counterbalanced by the moment of applied forces. Thus, Navier could find equations for the applied forces, and then, using d'Alembert's principle, equate this with the product of mass and acceleration.

In this way, Navier obtained the dynamical equations for an elastic solid. Navier then had the idea of adapting this method that utilized both a molecular ontology and the method of moments to find hydrodynamics equations. He similarly calculated the total molecular moment and again replaced a discrete sum over pairs of molecules with integrals. This led him to derive a new equation of motion for fluids that Darrigol [12, p. 114] writes (in modern vector notation) as

$$\rho\left[\frac{\partial \mathbf{v}}{\partial t} + (\mathbf{v}\cdot\nabla)\mathbf{v}\right] = \mathbf{f} - \nabla P + \epsilon\Delta\mathbf{v} \tag{2.4}$$

Note that this equation is essentially identical to the modern Navier-Stokes equation (2.1) and that it is identical to Euler's hydrodyanmics equation (2.3) with the exception of the added term $\epsilon\Delta\mathbf{v}$.

However, this was still not the end; this was still not the Navier-Stokes equations we know today. The Navier-Stokes equations as we know them today contain a boundary condition known as the "no-slip" condition, according to which fluid velocity vanishes at the fluid-solid boundary. In his first presentation of this derivation in March 1822, Navier assumed this condition, as Girard had previously in his own theory of fluid motion.

However, in a presentation in December 1822, Navier proceeded to give up this boundary condition based on empirical data from Girard's experiments with difference in fluid discharge for glass versus copper pipes. Thus, Navier chose to privilege agreement with empirical data over the theoretical considerations of Girard's and others' theory of fluids.

The return of the no-slip condition wasn't to come for another 23 years; in the interim, there were several more derivations of hydrodynamics equations by several others mathematicians. The next two derivations were by Cauchy and Poisson. Like Navier, both Cauchy and Poisson actually first derived equations for elasticity, only after that moving on to deriving hydrodynamics equations.

Cauchy was, like Navier, trained in both the rigorous mathematical and practical engineering traditions, having attended both the École Polytechnique and the École des Ponts et Chaussées. In a memoir presented in 1822, Cauchy developed a treatment of elastic solids that we now recognize as the stress/strain system and was able to use various symmetries to derive equations of elasticity generally similar to those derived by Navier up to a change in a coefficient. Importantly, Cauchy did not use molecular assumptions in his derivation, relying only on the pressure system acting on the solid and giving a seemingly continuous treatment. However, Darrigol notes that Cauchy did hold atomist beliefs and that the fact that he was able to derive these equations without reference to a molecular ontology simply showcases his extreme mathematical dexterity [12, p. 120].

Cauchy did not immediately apply this general equation of elasticity to fluids, although he did consider the case of a "non-elastic solid," that is, the case where stresses at a given time depended only on change in form of the body right before that time. For this case, when incompressibility was assumed, Cauchy got equations identical to those Navier had derived for viscous fluids with the exception of the pressure term. However, Cauchy made no mention of this comparison.

Poisson, on the other hand, was an intensely devoted follower of the Laplacian program. Poisson had only been trained at the École Polytechnique and so lacked the engineering training and experience Cauchy and Navier got at the École des Ponts et Chaussées, being more interested in fundamental physics. In an 1828 memoir, Poisson aimed for a derivation of the general equations of elasticity from molecular assumptions, avoiding the Lagrangian method of moments used by Navier. Poisson also retained the discrete sums, instead of replacing them with integrals as Navier had. Indeed, Poisson actually attacked Navier's replacement of discrete sums with integrals, announcing himself as being the first to have developed a truly molecular theory of elasticity [12, p. 124].

There followed, then, "a long, bitter polemic in the *Annales de Chimie et de Physique*" between Navier and Poisson [12, p. 125]. Navier defended his replacement of sums with integrals. Interestingly, Darrigol gives the following analysis of Navier's defense:

> From this extract of Navier's defense, one may judge that he was hesitating between two strategies. The first option was to deny the general applicability of the Laplacian doctrine of central forces, and to deal only with forces that arise out of the disruption of an equilibrium of unknown nature. This option agreed with Navier's positivist sympathies and with the style of applied physics that he embodied at the [École des] Ponts et Chaussées. It could accomodate later, unforseen changes in molecular theory. (footnote: Physicists today regard the existence of the equilibrium state of a solid as a quantum property but they nevertheless allow a classical treatment of small perturbations of this state.)
>
> The second option was to admit the general Laplacian reduction to central forces and to show that the appropriate results could nevertheless be obtained by substituting integrals for sums. Here Navier erred, because a Laplacian continuum, that is, a continuous set of

> material points subjected to central forces acting in pairs, cannot have rigidity. [12, p. 125]

It will be good to hold onto this point when we come back to analyze differences in methods of derivation and interaction with the aims of modeling. For now, it is sufficient to note that Navier seemed to be wavering between treating his method as a heuristic approximation to a true fundamental theory and treating his method as more fundamental, denying the general applicability of the existing fundamental theory.

Poisson's other objection to Navier's derivation was that Lagrange's method of moments was not applicable to molecular systems. However, Darrigol writes that, although Lagrange had successfully applied the method of moments to continuous media and not to molecular systems, there was no assumption in Lagrange's method of continuity of the material it was applied to, and therefore no theoretical constraint barring the application of Lagrange's method to discrete media [12, p. 126].

Darrigol concludes that Poisson's attack on the coeherence of Navier's method was untenable, and further that Navier's method had several significant advantages, including weakening assumptions regarding molecular forces and providing a "direct link" between these molecular assumptions and macroscopic phenomena [12, p. 127]. However, Darrigol also concedes that Poisson was essentially correct in his assertion that discrete sums cannot in general be replaced by integrals, although it worked in this case.

Now, while Poisson was working on his molecular theory of elasticity, so was Cauchy, and they were in heated competition as to who would finish first. Poisson won the race, presenting his memoir in 1828. However, he was forced in 1829 to correct various flaws in his derivation, flaws brought to light by Cauchy's memoir where he presented his molecular theory of elasticity.[19] Darrigol goes so far as to state that, although Cauchy and Poisson essentially used the same method, Cauchy's derivation went above and beyond Poisson's in many respects, particularly with regard to the details of calculation and compactness [12, p. 121].

Howevever, in his 1829 memoir, Poisson also discussed the application of his theory of elasticity to fluids. Poisson drew an analogy between fluids and elastic solids, notably supposing that both experience stresses, the difference being that fluids immediately relax [12, p. 127]. That is, fluids undergo a constant cycle of stresses and relaxations. Poisson was able to utilize the stress-strain treatment of elastic solids and adapt it to fluids. From this, Poisson obtained equations for fluid motion nearly equivalent to the Navier-Stokes equations, although he made no mention of Navier's derivation or Cauchy's note about "perfectly inelastic solids."

The next person to derive equations for fluid motion was Saint-Venant. Saint-Venant was trained at the École Polytechnique and went on to serve as an engineer before going on to teach mathematics at the École des Ponts

[19] Note that this derivation is different from Cauchy's previous derivation of the general equations of elasticity, which did not rely on molecular assumptions.

et Chaussées.[20] Saint-Venant aimed to bridge the gap between engineering and rational mechanics, holding personal distaste for both strict empiricism, unaided by theory, and the "arbitrary idealizations of French rational mechanics." [12, p. 131] Thus, Saint-Venant developed a methodology that traveled the middle path through these two poles. Darrigol summarizes Saint-Venant's method in five steps:

> i) Start with the general mechanics of bodies as they are in nature, which is to be based on the molecular conceptions of Laplace, Poisson, and Navier.
>
> ii) Determine the macroscopic kinematics of the system, and seek molecular definitions for the corresponding macroscopic dynamics.
>
> iii) Find macroscopic equations of motion if possible by summation over molecules, or else by macroscopic symmetry arguments; the molecular level thus being, as it were, "blackboxed" in adjustable parameters.
>
> iv) Develop analytical techniques and methods of approximation to solve these equations in concrete situations.
>
> v) Test consequences and specify adjustable parameters by experimental means. [12, p. 131]

Incidentally, Saint-Venant developed this method while working on the problem of elastic solids.

What is important to note in Saint-Venant's method here is the balance it strikes between molecular ontology for explanation on the one hand and the need for equations to be usable for practical problems on the other. The desire for a molecular base comes from Saint-Venant's atomist beliefs; indeed, he, as many others in this period, proved the impossibility of a continuous solid as proof that matter must be discontinuous. Thus, physical phenomena had to be explained, at base, through forces acting between molecules [12, p. 131]. However, Saint-Venant also was clear about the viability of methods of approximation, stating that "[b]etween mere groping and and pure analysis, there are many intermediaries," going on to list a variety of known approximation techniques [12, p. 132].

Saint-Venant submitted a never-published memoir on hydrodynamics in 1834 to the Academy of Sciences. In this memoir, Saint-Venant criticized the notion of ideal, continuous solids, arguing for the molecular approach by proving the discontinuity of matter, as many had. He further characterized the pressures acting within a fluid through molecular interactions and was able to show that there existed what he called "transverse pressures" in a moving fluid. He described these transverse pressures as "opposed to the sliding of successive layers

[20]As an interesting historical note, Saint-Venant studied under physicist/chemist Joseph Louis Gay-Lussac (1778-1850) and succeeded mathematician Gaspard-Gustave de Coriolis (1792-1843) at the École des Ponts et Chaussées.

of the fluid on one another." [12, p. 133] However, Saint-Venant ended up taking an approach based more on symmetry (similar to Cauchy) instead of a purely molecular treatment, ending up with a hydrodynamics equation that did not quite resemble Navier's. Saint-Venant's equation was much more complex, containing five parameters instead of one, with these parameters dependent upon microscopic details of fliuid motion, as well as unknown functions, which Saint-Venant intended be determined experimentally [12, p. 134]. Saint-Venant even began to design experiments which would determine these.

However, Saint-Venant later corrected a couple of mistakes in his 1834 memoir, as well as going on to consider internal fluid friction and the problem of accounting for fluid resistance. Saint-Venant in 1837 re-derived his equations of hydrodynamics which, if a parameter ϵ in his equation was constant, yielded the hydrodynamics equations found by Navier, Cauchy, and Poisson. However, Saint-Venant believed that ϵ must be variable, that it reflected the ways in which localized irregular motion influenced internal fluid friction [12, p. 135]. In Darrigol's analysis, he states that

> Whether or not Saint-Venant regarded Navier's equation with constant ϵ as valid at a sufficiently small scale is not clear. In any case, he believed that the value of ϵ should be determined experimentally without prejudging its constancy from place to place or from one case to another. [12, p. 135]

Further, Saint-Venant initially, in his 1834 memoir, only intended the aforementioned "irregularities" to be "mere undulations of molecular paths," but later expanded this to include the whirling motions observed by those such as Leonardo da Vinci (1452-1519), Bernoulli, and Giovanni Battista Venturi (1746-1822).

In his 1839 edition of his *Introduction à la mécanique industrielle, physique ou expérimentale*, Jean-Victor Poncelet (1788-1867) discussed these whirls and emphasized their complexity, stating that they were "much more complicated than one usually thought."[21] Poncelet included in this category of "whirls" such motions as pulsations, intermittences, and the cascading effect of larger whirls producing smaller and smaller whirls. Both Poncelet and Saint-Venant conceived of these motions as

> one of the means that nature uses to extinguish, or rather to dissimulate the live force in the sudden changes of motion of fluids, as the vibratory motion themselves are another cause of its dissipation, of its dissemination in solids. [22]

Saint-Venant considered these whirls to be dissipated by internal fluid resistance and friction and went on to use these observations in his discussions of flow retardation and the derivation of retardation formulas.

Saint-Venant went on to derive the Euler-d'Alembert paradox in 1846, as well as proving that consideration of fluid resistance solved the paradox, the key being treating the fluid as molecular and considering friction and non-translatory

[21] From [12, p. 136].

[22] An excerpt from Poncelet's *Introduction à la mécanique industrielle, physique ou expérimentale*, pp. 528-530, quoted in [12, pp. 136-7]

motions (i.e. the whirls). However, one of Saint-Venant's most notable thoughts regarding hydrodynamics equations was that, although Navier's equation did not include his variable ϵ, Navier's equation could be conceived of as modeling the "average, smoothed out flow" of a fluid [12, pp. 138-9]. And thus was the contribution of Saint-Venant to hydrodynamics.

There is one last mathematician to consider with regard to the derivation of the Navier-Stokes equations, the first of these not based in France, and that is the Irish mathematician Sir George Gabriel Stokes (1819-1903). Stokes, like Saint-Venant, was especially sensitive to the gap between the ideal theories of rational mechanics and the empirical complexities of real phenomena [12, p. 140]. Stokes had been trained in mathematics at Cambridge, from 1837-1841, where he learned of French developments in mathematics, particularly Euler and Lagrange's hydrodynamics. Writing on this, Stokes introduced the idea which would become central to later hydrodynamics, the idea of the stability of a given flow.

Stokes argued that, just because a given motion is possible, it need not necessarily occur. Rather, there might exist several possible motions, all compatible with the given boundary conditions. What might separate these motions and determine which occurs is whether or not they are stable. In fact, Stokes wrote in his 1842 paper, "On the steady motion of incompressible fluids," that "[t]here may even be no stable steady mode of motion possible, in which case the fluid would continue perpetually eddying."[23] Stokes then went on to show how introducing this notion began to account for discrepancies between theoretical and real flows, or "perfect" and "imperfect" fluids.

Stokes was also interested in Edward Sabine's 1829 pendulum experiments, and it was this that led Stokes to consider the problem of fluid resistance. German astronomer Friedrich Bessel (1784-1846) had already begun considering the inertia of air carried with the pendulum, and Sabine, studying this, encountered an anomaly in experimental versus theoretical values that he suggested was due to the viscosity of the gas in which the pendulum was moving. This prompted Stokes to study "imperfect fluids," contrasting their behavior with that of "perfect fluids."

Stokes initially attempted, in 1843, to study particular cases of perfect fluid motion and compare how the motion of "imperfect fluids" departed from this motion. Stokes, in this case, attempted to avoid any hypothesizing about the molecular constitution of the fluids. One of the cases Stokes considered was that, in direct contradiction with observation, theory predicted that the dampening effect on oscillatory motion should be identical to that on uniform translation motion in a perfect fluid [12, p. 141]. Stokes went on to consider several possible causes of the observed difference in resistance, eventually settling on the explanation utilizing the notion of instability. Stokes wrote

> It appears to me very probable that the spreading out motion of the fluid, which is supposed to take place behind the middle of the sphere or cylinder, though dynamically possible, nay, the only mo-

[23] Quoted in [12, p. 140].

> tion dynamically possible when the conditions which have been supposed are accurately satisfied, is unstable; so that the slightest cause produces a disturbance in the fluid, which accumulates as the solid moves on, till the motion is quite changed. Common observation seems to show that, when a solid moves rapidly through a fluid at some distance below the surface, it leaves behind it a succession of eddies in the fluid.[24]

Stokes proceeded to assert that fluid resistance is due to the "vis viva" of the eddies produced, reflecting the discussions of whirls by Poncelet and Saint-Venant. As an example of this, Stokes noted that ships experience the least resistance when they leave a smaller wake.

Unable to do further analyses of imperfect fluids using this first method, Stokes went on to try the method of including internal fluid friction in the fundamental hydrodynamics equations he used. This new approach, as opposed to his previous, was very explicitly molecular. Interestingly, Darrigol writes that "no more than his French predecessors could he conceive of internal friction without transverse molecular actions." [12, p. 143]

This method led Stokes to the Navier-Stokes equations in his 1845 paper "On the theory of the internal friction of fluids in motion, and of the equilibrium and motion of elastic solids." Stokes then needed to consider the boundary conditions. Stokes already believed some version of the "no-slip" condition, where fluid velocity vanishes at the fluid-solid boundary, when he read this memoir in 1845 [12, p. 144]. However, Stokes knew that this was in contradiction with observations of pipe and channel flow by Du Buat and Bossut, as well as Girard's pipe experiment, which Navier had interpreted as necessitating finite slip. Darrigol writes that

> [Stokes] later adopted the view that the Navier-Stokes condition with the zero-shift boundary condition applied generally, and that the non-linearity of the resistance observed beyond a certain velocity corresponded to an instability of the smooth-flow solution of the equation, leading to energy dissipation through a trail of eddies. This is essentially the modern viewpoint [12, p. 145]

Thus, we arrive at the final form (in a sense) of the Navier-Stokes equations, although it in fact took several more decades for the Navier-Stokes equation to become a common tool in analyzing and modeling fluid flow. In fact, it wasn't until almost two decades later that Navier-Stokes made an appearance in a hydrodynamics treatise, Horace Lamb's 1895 *Hydrodynamics*. The Navier-Stokes equation was left in relative obscurity until it came to prominence in the study of turbulence by Osborne Reynolds and Joseph Boussinesq in the 1880s [12, p. 150].

[24] An excerpt from Stokes' 1843 paper "On some cases of fluid motion," pp. 53-4, quoted in [12, p. 141]

2.2.4 Prandtl's Boundary Layer Solution

Now, as I stated earlier, the Navier-Stokes equation is intractable for the majority of cases, owing in large part to the nonlinearity of the equations. Additionally, the problems of fluid resistance and flow retardation were still largely unsolved, and they were reaching new importance as the problem of powered flight came to the fore, with the Wright brothers achieving their first flight at the end of 1903.

Ludwig Prandtl (1875-1953) was a former engineer who proceed to teach fluid mechanics at the Polytechnic in Hanover [44, p. 161]. In his 1904 presentation to the Third International Congress of Mathematicians, he introduced the idea of treating fluid flow bordering a solid in two parts. Prandtl's method began by treating flow around the body using Euler's hydrodynamics equations, essentially treating the fluid in this region as inviscid. This means that there is finite slip between the fluid and solid. However, it was known that, for a viscous fluid, the velocity of the fluid relative to the solid must vanish at their boundary. Thus, Prandtl "assumed a thin (invisible) layer of intense shear that imitated the finite slide of the Eulerian solution." [13, p. 25] This intense shear allowed Prandtl to use a simplification of Navier-Stokes that was tractable; it could be integrated to derive a velocity profile.

Crucially, there was interaction between the "free fluid" and the "boundary layer." On the one hand, the Euler equations governing the free fluid determine the pressure at the edge of the boundary layer. On the other hand, Prandtl described a process whereby the boundary layer may peel off from the body and thus form a surface of discontinuity in the free fluid flow.

One interesting point to note is that Prandtl demonstrated the effects of the boundary layer using a physical model, a water tank equipped with a paddle wheel, so that Prandtl could generate flow and then immerse objects. Using this, Prandtl demonstrated the formation of vortices by suspending fine, lustrous minerals in the water which shone when in water undergoing strain,[25] vortices predicted by the peeling-off of the boundary layer.

Another interesting point to note from Darrigol's analysis is that

> Prandtl's idea (if we believe his own plausible account) has its theoretical origin in the idea of using solutions to Euler's equations as a guide for solving the Navier-Stokes equation at high Reynolds number. This is only a heuristic, becuase Prandtl had no mathematical proof that the low-viscosity limit of a solution of the Navier-Stokes equation is a solution of Euler's equation. [13, p. 26]

That is, Prandtl's boundary layer solution is not itself rigorously derived from the Navier-Stokes equation. Yet, as Darrigol goes on to point out, Prandtl's solution retains an "intimate connection" to the Navier-Stokes equation, with this connection grounding the legitimacy of the the resulting boundary layer theory, and Prandtl's solution is not an "ad hoc model that owes its simplicity

[25] Accounts of Prandtl's water tank, or "flume," can be found in [41, pp. 230-1], [31, pp. 54-5], and [44, pp.161-2].

to counterfactual assumptions," but rather a "legitimate articulation of the Navier-Stokes theory." [13, p. 27]

In this historical narrative of Prandtl's boundary layer solution to the Navier-Stokes equation, I have shown how considerations of various problems in fluid flow led to the derivation of the Navier-Stokes equation and the various assumptions and physical intuitions that went in to deriving these equations, as well as Prandtl's boundary layer solution eventually. I believe that this historical narrative sheds much more light on how Navier-Stokes and Prandtl contribute to our understanding of fluid flow than the mapping account does. In the next section, I will give a more consolidated philosophical analysis of where the mapping account fails in its analysis of Prandtl and how the actual answer to the specific question should be formulated, principally by centering historical details and the how the physical concept of viscosity evolves and interacts with equation models.

2.3 Philosophical Analysis: Inventing Viscosity

So, what can we extract from the above historical narrative regarding the evolution of the viscosity concept and its role in the derivation of the Navier-Stokes equations and Prandtl's boundary layer solution?

The concept of viscosity did not originally enter into the rational hydrodynamics of the 18^{th} century, as exemplified by Euler's hydrodynamics equations and d'Alembert's work. Even though Euler and d'Alembert recognized the empirical problem posed by the paradox resulting from Eulerian hydrodynamics, neither proposed to resolve the situation with the introduction into hydrodynamics theory of viscosity in some form or another. Rather, d'Alembert avoided the problem as irrelevant to his rational mechanics, and Euler adopted an empirical fix to account for fluid resistance dating back to Marriotte and Newton, which was known to be empirically inexact and theoretically weak.

The concept of viscosity, in Euler's fix, was almost as articulated as empirical conceptions of viscosity used by the engineers. The engineers, through accumulation of experimental data, formulated several proportionalities relating fluid resistance or flow retardation with other properties of flow, such as powers of velocity and the "wetted perimeter" of a cross section of flow. What we have, then, is the beginnings of a concept of viscosity via formation of proportionalities regarding its effects.

At the end of the 18^{th} century, the engineers began proposing molecular explanations for these phenomena, notably Du Buat and Coulomb's proposal that a layer of fluid molecules adhered to the surface of the solid and resistance was due to attractive forces between fluid molecules. This leaning towards a molecular explanation of the effects of viscosity may be regarded as a reflection of the rise of Laplace's molecular program in French mechanics, more generally symptomatic of the spread of atomism.

Thus, around the beginning of the 19^{th} century, the concept of viscosity is accessed via its effects, which are in turn explained via the fundamental physical theory of the day. Here already, there is an uncomfortable tension between two different conceptions of viscosity, which I call the *macro-scale conception* and the *micro-scale conception.* According to the macro-scale conception, viscosity is a simple property of flow, homogeneous throughout to some degree. This conception results from the empirical proportionalities formulated by the engineers in their pursuit of an empirically adequate mathematical model of fluid flow. On the other hand, according to the micro-scale conception, viscosity is the aggregate property of fluid molecules in motion, grounded in intermolecular forces and thus varying on the molecular level throughout the fluid. This conception results from theoretical considerations of fluids under the atomic Laplacian program that tries to rigorously account for physical phenomena through recourse to fundamental physics.

The macro-scale conception dominated attempts by engineers in the 18^{th} century to obtain empirically adequate mathematical representations of various aspects of fluid flow, whereas the micro-scale conception dominated attempts by many of those same thinkers to give a theoretical justification or explanation of the phenomena. However, interestingly, this theoretical narrative from the fundamental ontology did little to constrain the mathematical representations, the proportionalities, constructed by the engineers.[26] And so we see the macro- and micro-scale conceptions of viscosity living in an uncomfortable coexistence.

Navier's derivation of a hydrodynamics equation attempts to bridge the gap between the empirically-derived proportionalities of the engineers and the rigorously-derived equations of the rational mechanics. However, the resulting conception of viscosity that emerges in his hydrodynamics does not fully reconcile the macro- and micro-scale conceptions of viscosity. This is especially apparent in the passage from Darrigol addressing Navier's response to Poisson's objection to the replacement of sums with integrals. Recall that Darrigol characterized Navier as wavering between two possible defenses: denying the universal applicability of fundamental physics treatments, and regarding his approach as an heuristic which approximates a truly fundamental physics approach.

These two strategies have some resonance with the macro-/micro-scale division in conceptions of viscosity. In the defensive strategy where Navier denies the universal applicability of fundamental physics approaches, the macro-scale conception of viscosity may come to have equal or even more importance than the micro-scale conception in deriving hydrodynamics equations. However, in the defenseive strategy where Navier concedes that his approach is merely an approximation of a true fundamental physics approach, the micro-scale conception of viscosity clearly has more importance in deriving hydrodynamics equations.

We can see a similar dynamic arising in Saint-Venant's method, as described by Darrigol. Even though Saint-Venant's process includes finding "molecular definitions for the corresponding macroscopic dynamics," it also involves "black-boxing" the molecular level with adjustable parameters, as well as determining

[26]There is likely an interesting connection here, philosophically and historically, with the interaction between matter theories/theories of bodies in the late 17^{th} and early 18^{th} centuries and the mathematical rules of collisions, as discussed by Katherine Brading and Marius Stan [4].

adjustable parameters through experiment. So, again, we see both the macro- and micro-scale conceptions of viscosity (among other properties) being incorporated in the process of deriving dynamical equations.

Returning to the Navier-Stokes equation as it is presented in modern notation, recall that the "viscosity term" is given as a constant adjoined to a Laplacian, $\mu\Delta\mathbf{v}$. We may rewrite the Laplacian, or Laplace operator, in the more familiar partial differentiation notation as

$$\mu\Delta\mathbf{v} = \mu\sum_{i=1}^{3}\frac{\partial^2\mathbf{v}}{\partial x_i^2}$$

We have already come across second-order partial derivatives over spatial dimensions, in the one-dimensional heat equation above. In fact, the two- and three-dimensional heat equations use Laplacians. The effect of the Laplacian in the heat equation is the "averaging out" or "dissipation" of heat (or, more accurately, of the values the function $u(x,t)$ takes) over time. Similarly, the presence of the Laplacian in the Navier-Stokes equation results in the effect of the "averaging out" or "dissipation" of the the velocity, or $\mathbf{v}(x,t)$ over time. We then have a constant μ sitting out front, determining the strength of this dissipation.

Setting aside exactly how the mathematics here (namely functions and their partial derivatives) comes to be related to the world, we can see again the macro- and micro-scale conceptions of viscosity both coming into play. The Laplacian considers how the velocity function varies across infinitesimal differences in spatial position, thus essentially realizing the micro-scale conception of viscosity, since it operates at the (sub)molecular level. On the other hand, we have the characteristic constant μ sitting out front, essentially realizing the macro-scale conception of viscosity. Not only is this constant in fact constant throughout the fluid, it is empirically determined for a given fluid.

It is worth recalling that Saint-Venant argued for not assuming the constancy of this parameter; he in fact believed that it was variable. He regarded the parameter as reflecting the effects of local "irregularities of motion" on internal friction, with these irregularities including "mere undulations of molecular paths," as well as the whirls discussed by his forerunners. This seems very much in line with giving the micro-scale conception more significance; however, the Navier-Stokes equations with the parameter constant were still conceived by Saint-Venant as "controlling the average, smoothed out flow with a variable viscosity coefficient." So, again we see this sort of compromise in the Navier-Stokes equations as they are today, with constant μ, between being rigorously derived and accounting for the molecular level versus being empirically adequate, as Saint-Venant says, "controlling the average, smoothed out flow."

These equations were still essentially intractable for a wide variety of practical applications. As much as Navier, Saint-Venant, and the others had made an effort to derive hydrodynamics equations that bridged the practical-theoretical gap, it would seem as though they had not achieved their goal. However, the new hydrodynamics equations were a great improvement over existing treatments, notably Euler's hydrodynamics and the engineers' proportionalities. The new hydrodynamics equations included an account of internal fluid friction, which Saint-Venant had shown was sufficient to resolve the Euler-d'Alembert paradox. Further, the new hydrodynamics equations had reconciled the theoretical fundamental physics explanation of internal fluid friction with a treatment of the macroscopic behavior of fluids, as evidenced in the compression of both the macro- and micro-scale conceptions of viscosity into the new hydrodynamics equation.

Prandtl's innovation was to recognize this tension between the macro- and micro-scale conceptions, and the underlying tension between the aims of empirical adequacy and rigorous derivation, and to tease them apart. It was known to Prandtl that Euler's hydrodynamics, which omitted viscosity, worked relatively well to model the majority of fluid flow. In other words, Euler's equations were empirically adequate for the majority of flow. However, Euler's equations allowed for finite slip between fluid and solid, even though it was known, initially from molecular considerations, that for a viscous fluid, flow velocity must vanish at the fluid-solid boundary. Due to this need for no slip, Prandtl had the boundary layer imitating the finite slip by being a layer of intense shear; the fluid in the boundary layer had velocity zero at the fluid-solid boundary. In this strategy, we see that both the practical considerations of empirical adequacy and tractability and the theoretical considerations having to do with rigorous derivation from fundamental ontology come apart in Prandtl's solution. The theoretical considerations motivate the boundary layer, while the practical considerations motivate the treatment of the rest of flow, and the theoretical and practical considerations compromise to dictate how each region is modeled mathematically.

That is, in Prandtl's boundary layer solution, the macro-scale conception of viscosity is operative in striving for a homogeneous treatment of the majority of flow, and the micro-scale conception of viscosity is operative in contracting the region in which the effects of viscosity are actually modeled. Further, the macro- and micro-scale conceptions dictate the calibration of the intense shear to be imitated by the boundary layer. We then see the resolution, in a way, of the macro- and micro-scale conceptions of viscosity and the underlying aims of fluid modeling in Prandtl's solution.

The above narrative concerning the derivation of the Navier-Stokes equations and Prandtl's boundary layer solution, and the subsequent discussion of how the viscosity concept evolved and interacted with these derivations, begins to provide a much more substantive answer to the questions of how Navier-Stokes and Prandtl's solution contribute to our understanding of fluid flow bordering solids. It is the close attention to historical details and the work that the physical concept of viscosity is doing within each model that underlies this narrative's success, and it is exactly these factors that a mapping account such as Pincock's either fails to center or completely misses.

The narrative that I have provided here begins to answer the specific question by tracing out how the pieces of mathematics in Navier-Stokes and Prandtl's solution are linked to physical phenomena. Especially salient in this narrative is the role that the physical concept of viscosity plays in mediating between physical theory, the aims of modeling, and the various pieces of mathematics. The narrative shows how various observed correlations having to do with the effects of viscosity came to inform the engineers' proportionalities, as well as theoretical explanations that referenced fundamental ontologies. This spawned two distinct conceptions of viscosity associated with different aims of modeling fluids, which we could trace all the way through to their teasing-apart in Prandtl's solution.

The question Pincock had regarding the physical analogue of the "edge" between boundary layer and rest of flow in Prandtl's solution disappears, because we are no longer viewing the mathematics in a way that centers true representation. Rather, the narrative tells us that this edge is the result of compromising between the aims of empirical adequacy and rigorous derivation. The "edge" is a mathematical advance towards tractability that actually falls in with the broader category of modeling known as "multiscale modeling." The boundary layer is motivated by both the macro- and micro-scale conceptions of viscosity. The macro-conception advocates a homogeneous treatment of flow with regard to viscosity, resulting in our treating the majority of flow as inviscid by using Euler's equations. The micro-conception then dictates the creation of the boundary layer, and both conceptions dictate how the intense shear in that layer is to be modeled mathematically.

So, while we are not giving an account of the "edge" that centers true representation, we are also not giving an account that centers pragmatics. The account we have given, the narrative sketched above, is not confined to either category, but rather uses the tools of both, in addition to the tools of historical and cocneptual analysis, in order to provide a more detailed understanding of how Prandtl's solution is able to be successful and how it enables us to understand fluid flow. A similar answer can be given to Pincock's question regarding the reference of predicates across patches.

The mapping account's interpretive failure then—its failure to answer questions about the physical analogue of the "edge," its failure to answer the question about reference of predicates across patches, and, most of all, its failure to answer the specific question—is due to the mapping account's failure to center, or even account for, the rich work that the physical concept of viscosity is doing in mediating between physical theory, modeling aims, and pieces of mathematics. The mapping account may sometimes bring historical details and pieces of physical theory to bear on its analyses of applied mathematics, as noted in the discussions of the enriched, schematic, and genuine contents, but, even when it does this, it does not center these, instead making them secondary to the ideal of true representation.

Now, before moving on to give a general diagnosis of what the mapping account misses and what a replacement account needs to look like, it is worth taking a few steps back. In the philosophical analysis of this section, we had to take for granted what the Laplace operator was doing in the mathematics. In order for our analysis to be satisfactory, it would seem we need to draw on an account of (pure) mathematics in order to not only give us an account of the Laplace operator, but also to secure its grounding. However, as I will argue in the next chapter, it is actually much better for us to develop a unified account of pure and applied mathematics. I will motivate this approach while also arguing for the role of historical analysis in analyzing mathematics, and then I will return to physical concepts and my diagnosis of what, in general, the mapping account misses.

Chapter 3

Building Mathematics: Historically-Motivated Analysis

"God the creator who has bestowed upon man the power to discover the significance of numbers" Muḥammad ibn Mūsā al-Khwārizmī (c. 750-850 CE)[1]

"It must have required many ages to discover that a brace of pheasants and a couple of days are both instances of the number two." Bertrand Russell (1872-1970 CE)[2]

"To those who do not know mathematics it is difficult to get across a real feeling as to the beauty, the deepest beauty, of nature ... If you want to learn about nature, to appreciate nature, it is necessary to understand the language that she speaks in." Richard Feynman (1918-1988 CE)[3]

I have so far focused on accounting for instances of applied mathematics. I have considered the dominant approach, mapping accounts, and shown them to be at best insufficient for answering questions about why specific pieces of applied mathematics are able to aid in our understanding of the specific physical system(s) they are supposed to represent. I have done this by showing how important the history of a piece of applied mathematics is in answering such questions. I will now turn to mathematics side of things, focusing more on the history of mathematics itself. I will argue that the history of mathematics provides equally as much insight into applied mathematics, and I will further argue from history that any satisfactory account of "applied" mathematics must actually be an account of mathematics as a whole. I will argue that, when giving a philosophical account, any hard distinction between "pure" and "applied"

[1] An "opening flourish" Al-Khwārizmī put in his book on equations. See [21, p. 173]
[2] From [42].
[3] From Ch 2 of [15].

mathematics almost completely disappears.

3.1 The View From the History of Mathematics

The history of mathematics traces along many paths with many branches and crossings over. A comprehensive history is hard to come by, partly due to the sheer volume of history, but also due to the Eurocentric tendencies still entrenched in many institutions. Additionally, there is the problem that most histories of mathematics use modern ideas and notations to present historical ideas, which can obscure subtleties of historical development. This lack of appreciation for the context in which mathematics developed is not aided by the "great men/ideas" approach dominant in the history of mathematics.[4] However, there are still many philosophical lessons to be be learned from the history of mathematics we currently have. There is a package of ideas about mathematics floating around that sneaks into philosophizing about mathematics and applied mathematics. One of the main goals of this chapter is to use considerations from the history of mathematics to dismantle this package of views.

A central idea in this package concerns mathematics and its relationship to science, and it goes something like this: "many mathematical developments were motivated by considerations purely internal to mathematics." This makes its way into philosophizing about math when it becomes "a piece of mathematics was developed for purely mathematical reasons but then turned out to be physically significant/useful." It is then mysterious that this should be the case, and suddenly there is an eruption of phrases akin to "mathematics is the language of Nature," phrases already in use due to the ubiquity of math in science, particularly in physics. These can be seen throughout history, as demonstrated by the quotes at the beginning of this chapter. I will refer to this view as the *pure math claim*.

Another, related part of this package of views about the relation between mathematics and science goes something like this: "science and mathematics are necessarily intertwined as knowledge-seeking enterprises." This notion is often expressed in calls for fields to be "rigorized" by becoming "mathematized," and by philosophical views that mathematics is "indispensable" to scientific theorizing. The latter of these is prominently displayed in so-called indispensability arguments, which argue that mathematics is indispensable to our scientific theorizing in order to show that the objects of mathematics are "real" in some sense.[5] I will refer to this view that math and science are necessarily intertwined as the *necessary connection claim*. This idea also tends to support the "mathematics is the language of Nature" slogan.

[4] This approach is complained about in as recent a work as mathematician/historian of mathematics Jeremy Gray's preface to [23], although he does acknowledge recent progress in this area, for example Grattan-Guinness's work documenting early 19^{th} century French mathematics in [20].

[5] Indispensability arguments are often thought to originate with Quine and Putnam. One of the main contemporary proponents of such arguments is Mark Colyvan; see [10].

The other main notion in this package that I will consider is what has come to be called the "warehouse view" of applied mathematics, the idea that pure mathematics develops a storehouse of tools that applied mathematicians and scientists can then sift through in order to find the right tool for a specific job. This sort of view is discussed, for example, by philosopher of mathematics Penelope Maddy. In describing the turn geometry took after the confirmation of general relativity, Maddy states that

> At that point, it became natural to regard mathematicians as providing a well-stocked warehouse of abstract structures from which the natural scientist is free to select whichever tool best suits his needs in representing the world. [29, p. 20]

This sort of view supports both the necessary connection claim and the pure math claim; it clearly undergirds the pure math claim by isolating the tool-devloping work of pure mathematicians from the tool-selecting work of applied mathematicians. And it lends viability to the necessary connection claim by separating the epistemologies of pure math and applied math and characterizing the epistemology of applied math as drawing the appropriate (necessary) connections between pieces of mathematics and physical phenomena/theory. I will, as per convention, refer to this as the *warehouse claim*.

In this chapter, I will use various cases from the history of mathematics in order to argue that this package of views is untenable. I will argue that it is rarely, if ever, possible to make sense of the claim that developments in mathematics were "motivated by purely mathematical considerations." I will first discuss the case of the *Qibla* problem in medieval Arabic mathematics, where developments in planar and spherical trigonometry were clearly motivated by a problem in the physical world. I will then use another historical case to argue that, even in one of the most-discussed examples of "math developed due to purely mathematical considerations," the complex numbers, it is unintelligible at best what could be meant by the pure math claim. After dismantling the pure math claim, I will begin to sketch out a way to view the relationship between math and world based on an analogy with language that seriously undermines both the necessary connection claim and the warehouse claim. I will then return to the question of physical concepts, using this sketch of mathematics, and give a diagnosis of where the mapping account goes wrong.

A few brief remarks concerning the historiography of mathematics are in order. "Comprehensive" histories of mathematics often begin with a brief discussion of the Babylonians and the Egyptians, take a long and detailed tour of Greek mathematics, then maybe take a brief pitstop in the "Middle Ages" Near East before arriving at the Renaissance[6] and spending the majority of their time there. This is then usually followed by a more or less detailed recounting

[6]Following [21] and much contemporary historical work on this time period, I will briefly note that the distinction between the "Middle Ages" and the "Renaissance" is blurry at best, that there was not an intellectual "gap" during the "Middle Ages," and that these terms were actually Italian inventions of the 15^{th} century, with "Middle Ages" being "put forward in a derogatory sense to contrast with both the period of ancient glory and the current 'rebirth.'" [21, p. 135]

of how modern mathematics evolved from Renaissance mathematics. There are several justifications for this type of trajectory. Eurocentrism figures prominently in space allocations, but it is also important to note that mathematical notation that is "recognizable" for the modern reader emerged during, or, more often, after the Renaissance. Thus, in considering the reader, the historian generally speeds through all previous mathematics that would be unrecognizable without either lengthy explanations, prior knowledge, or considerable work to anachronistically translate such mathematics into modern notation/thought.

An additional difficulty confronting the historian of mathematics is the fact that "pure mathematics," as such, did not emerge as a distinct discipline until around the end of the 19^{th} century.[7] Thus, histories of mathematics often become entangled with histories of the sciences, particularly physics, a fact which may for many lend credence to the necessary connection claim.

One last point to note about the historiography of mathematics is the degree to which it has tended in the recent past to consist of what may be called the "great ideas" or "great men" approach. As late as 1999, Jeremy Gray writes that "[i]n providing a satisfactory context for the work of scientists, and in broadening the focus away from the 'great men,' (and occasional 'great woman') historians of science have generally led historians of mathematics." [23, p. 3] That is, there has been a tendency in the historiography of mathematics to focus on so-called big ideas and major figures without paying attention to the context in which the ideas were developed and in which these figures worked. Although there has been some progress in this area in the intervening decades, there remains work to be done.

All of these facets of current historiography of mathematics affect my own project. The focus on mathematics after the Renaissance and anachronistic presentations of older mathematics obscure connections that even a purely historical, much less aphilosophical, account of how pieces of mathematics were actually connected and actually motivated needs in order to be accurate. The lack of attention to the wider context mathematicians work in and the "minor" figures in history of mathematics further obscures details that such an account needs. The problem (even impossibility) of disentangling history of math from history of science has direct bearing on my project, as shown by its connection to the necessary connection claim, and so we will return to this point later.

With these considerations in mind, I will now turn to the *Qibla* problem in Arabic mathematics as a clear example of mathematics motivated by worldly (physical) problems.

3.1.1 The *Qibla* Problem

By the 800s CE, the Islamic people had spread from the modern Middle East across the coast of Northern Africa and into the Iberian peninsula. One of the major problems for the Islamic people in that time was to figure out, depending

[7] Jeremy Gray writes that "The 19^{th} century is the century in which it became first possible and then necessary to speak of mathematics and physics," that "[t]hey emerged as separate subjects with separate institutional bases in this period." [23, p. 3]

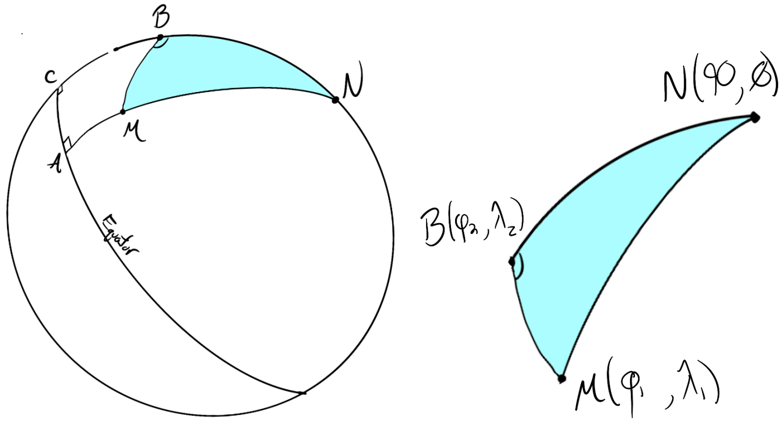

Figure 3.1: The *Qibla* problem, worshipper at B, Mecca at M, North Pole at N, equator as indicated. (Author illustration, partial copy with alterations of Fig 1 from [32].)

on their location on the globe, in what direction to face in order to be praying towards Mecca. The problem, then, was to consider some fixed point on the globe, usually the North Pole, such that the worshiper at point B, the Mecca at point M, and the North Pole at point N formed a triangle ΔBMN on the surface of the globe, a triangle whose sides were on great circles on the sphere. (See Fig 3.1 (a))[8] Then, it remained to calculate $\angle NBM$ [32]. This involved significant developments not only in planar and spherical trigonometry, but also in the measurement of locations and distances on the globe.

Arab mathematician Abū al-Wafāʾ produced one of the more remarkable solutions to the *Qibla* problem in his *Almagest*. It is a solution very close to one we might use today, which goes as follows.[9] Suppose we know point M, at Mecca, has latitude ϕ_1 and longitudeλ_1 and that point B, where the worshiper is, has latitude ϕ_2 and longitude λ_2. From this, we know that $\angle N = \Delta\lambda = \lambda_2 - \lambda_1$, the difference in longitudes. Further, we know that the arcs NB and NM are equal to $90 - \phi_2$ and $90 - \phi_1$ respectively. We can then use a formula known as the "four-parts formula," according to which

$$\cos(90 - \phi_2)\cos(\Delta\lambda) = \sin(90 - \phi_2)\cot(90 - \phi_1) - \sin(\Delta\lambda)\cot B$$

From this, we can extract a formula for $\angle B$ by solving for $\cot B$ and then taking the inverse. Although this is not exactly Abū al-Wafāʾ's method, contemporary mathematician Ali Moussa writes that this formula is "implicitly one of the three methods of Abū al-Wafāʾ, and he calls it 'a method without using the distance.'" [32, p. 3]

[8] A "great circle" on a sphere is a circle on the surface of the sphere whose radius is equal to the radius of the sphere. In other words, it is a circle that results from the intersection of the sphere and a plane that goes through the center of the sphere.

[9] This is the treatment of *Qibla* problem as described in [32, pp. 2-3].

Moussa writes that the mathematical developments in al-Wafāʾ's solution included new definitions of trigonometric functions, proofs of trigonometric formulae, and proofs of theorems in spherical trigonometry.[10] Prior to the *Qibla* problem, Arabic mathematics had received detailed studies of chords in circles as well as some spherical trigonometry from the Greeks and tables of sines from Indian mathematics. Moussa specifically notes Euclid's *Elements*, Ptolemy's *Almagest*, and Menelaus's *Sphaerica* as possible influences on al-Wafāʾ.

What should interest us in this particular episode is the clear and direct causal connection between a worldly problem, the Qibla problem, and a development in mathematics, in this case several in planar and spherical trigonometry. There is a clear motivation to further developments in the analysis of triangles and angles on the sphere, given that the worldly problem involves relations between locations on the globe, which is (roughly) a sphere. And so, in this case, both the pure math claim and the warehouse claim flatly fail to pan out. While it may be true that the Arab mathematicians drew on previous work in spherical trigonometry and other areas, it is clear that these applied mathematicians (an anachronistic name considering that there was no such distinction) developed their own pieces of mathematics.

Now, it may be objected by the proponent of the warehouse claim that, given the lack of distinction between pure and applied mathematics/mathematicians in that time period, it is irrelevant as a piece of evidence for or against the warehouse claim. They might further object that the warehouse claim is only meant to apply to more contemporary mathematics and science. And it likewise may be objected by the proponent of the pure math claim that, additionally, this is just one instance of mathematics motivated by a worldly problem. However, there are two points of reply.

First of all, despite that this is an episode earlier on in the history of mathematics and only one such episode, this is not by itself a reason to disregard it. This is a representative example of how mathematics grew early on in its development, alongside similar episodes in fields such as optics, painting, and seafaring. As such, it is part of the overall narrative of mathematics, and it is indicative of how mathematics was conceived early on in its development as a discipline. We cannot discount such early developments if we lack evidence that early conceptions of mathematics either disappeared with its further development or had no effect on subsequent development.

However, there is a further point: the fact that these developments in planar and spherical trigonometry were motivated by worldly problems may have had an amplified effect on later work furthering and utilizing planar and spherical trigonometry. It is quite possible that this effect rippled forward to contemporary math and science, and so it would take an actual analysis of more modern uses of and developments in planar and spherical trigonometry in order to truly be able to discount this episode with regard to the warehouse and pure math claims.

[10] See the abstract of [32].

3.2 The Development of Mathematical Concepts: Complex Numbers

Next, I will use the history of usage of complex numbers to argue that it is at least viable to reject the pure math claim with regards to this specific case. That is, I will argue that it is not necessarily true, or really intelligible, that complex numbers came about for so-called purely mathematical reasons.

The first point to address is how we might make intelligible the claim that the complex numbers were motivated by purely mathematical considerations. What is it that is being claimed here? I suggest that there is a bundle of beliefs about complex numbers that underlies such a claim, which can be cashed out under the slogan "complex numbers are purely mathematical inventions." While such a claim may be trivially true (seeing as complex numbers are inventions of mathematics, as are all numbers in some sense), there is a connoted meaning about what complex numbers cannot be: something physical, something "real," something that counts as a factor in the causal structure of the world. And it is this notion that makes it "surprising" that we see complex numbers in mathematical models of physical phenemona. So, what are the beliefs underlying this slogan?

The simplest belief has to due with the nature of complex numbers. A complex number involves a term including $\sqrt{-1}$, and this square root seemingly has no physical meaning in the way that the integers do. An integer can be associated with some amount of things that you count out. Even the negative integers can be taken to have some physical meaning derivative from the integers, perhaps as debts. And rational numbers, numbers of the form $\frac{p}{q}$ where p and q are integers, can be thought of as ratios between integers and so still have a physical meaning derivative from the integers. Even the square root of a positive number can have a physical meaning, the side length of a square whose area is equal to the number under the square root. All of these can be thought of as lengths. But complex numbers seem to have no such physical interpretation. We cannot use lengths or counting or ratios or side length of a square with negative area. Complex numbers, then, do seem very much like pure mathematical inventions.

Another belief has to do with the way mathematics is taught in contemporary college-level mathematics programs; the belief that complex numbers were introduced "to round out the real numbers." The story goes something like this. There is a natural progression of larger and larger sets of numbers, each subsuming its predecessor. We begin with the set of natural numbers, denoted $\mathbb{N}$, which consist of counting numbers:[11]

$$\mathbb{N} = \{1, 2, 3, \dots\}$$

Next comes the set of integers, denoted $\mathbb{Z}$, which includes the natural numbers and allows them to be negative as well (and includes 0):

$$\mathbb{Z} = \{\dots, -2, -1, 0, 1, 2, \dots\}$$

[11] The inclusion of 0 in the set of natural numbers can depend on the textbook author. I opt for excluding 0 from the natural numbers here.

After the integers, we have the set of rational numbers, denoted $\mathbb{Q}$, which consists of all ratios of integers:

$$\mathbb{Q} = \left\{ \frac{p}{q} \,\middle|\, p, q \in \mathbb{Z} \right\}$$

This set includes all of the integers, when we take $q = 1$ and let p be any integer. But, it further includes many numbers that sit between integers. However, this set is not exhaustive of all of the numbers that sit between the integers. For example, there is a classic proof that $\sqrt{2}$ is not a rational. Therefore, the set of rational numbers needs to be extended to capture all of the numbers on the number line. The resulting set is the set of real numbers, denoted $\mathbb{R}$, which cannot be given an easy definition like the former three.

Now, we may think we're done extending the set of numbers. After all, the set of real numbers captures all of the numbers on the number line. However, a problem occurs when we consider polynomials.

Suppose we have a polynomial of degree n, that is, a polynomial $p(x)$ where the highest power of x is n. Further, let us begin by supposing that the polynomial has all integer coefficients. So, we have

$$p(x) = a_n x^n + a_{n-1} x^{n-1} + \cdots + a_1 x + a_0$$

where $a_n \neq 0$ and $a_i \in \mathbb{Z}$ for all i. Let's make a first observation, that such a polynomial can have at most n integer roots.[12] One way of showing this is to note that, if a polynomial $p(x)$ of degree n has a root α, then we can rewrite the polynomial as $p(x) = (x - \alpha)q(x)$, where $q(x)$ is a polynomial of degree $n - 1$. It follows from this that we can find at most n integer roots of our given polynomial. Now, the question is, can we always find exactly n integer roots, not necessarily distinct, for a polynomial of degree n with all integer coefficients? That is, can we always rewrite $p(x)$ as

$$p(x) = a_n(x - \alpha_1)(x - \alpha_2) \cdots (x - \alpha_n)$$

where all of the α_i are integers?

It is immediately clear that the answer is no. We can even see that a polynomial may have *no* integer roots. For example, the simple polynomial

$$p(x) = 3x - 4$$

has no roots in $\mathbb{Z}$. In fact, the only root of this polynomial is $\frac{4}{3}$, which is not an integer. We then have a reason to extend our number system from integers to rationals, in searching for a more elegant theory of polynomials.

A natural next question is whether letting all of the coefficients be rational implies that we can always get exactly n rational roots. However, the answer is again no. Consider the polynomial

$$p(x) = x^2 - 2$$

[12]For those unfamiliar with "roots," a root of a polynomial $p(x)$ is a solution for x of the equation $p(x) = 0$.

This polynomial has no rational roots, but it does have two roots, $\pm\sqrt{2}$, which have been proven not to be rational. We now have a reason to further extend our number system to the reals.

We again ask the question whether letting all of the coefficients be real implies that we get exactly n real roots. And again the answer is no. Consider the polynomial

$$p(x) = x^2 + 1$$

Here, the coefficients are real, integer even, but no real roots exist. But we're using the reals, which cover the entire number line. What can we do? The answer is to keep going, extending the number system as before. If we use the formal operations we did in the previous cases to find roots, we arrive at the equation

$$x = \pm\sqrt{-1}$$

Why not extend our number system to allow this?

This extension of the reals gives us the set of complex numbers, denoted $\mathbb{C}$, and defined as

$$\mathbb{C} = \{a + bi \mid i = \sqrt{-1}; a, b \in \mathbb{R}\}$$

And it so happens that, if we let the coefficients of our degree n polynomial be complex, then we will *always* be able to find n complex roots, not necessarily distinct. This is encapsulated in what is called the Fundamental Theorem of Algebra, which can take several forms, the most prevalent form stating that a polynomial with real coefficients can always be broken down into linear and quadratic factors. This then implies as a corollary that a polynomial with complex coefficients can always be broken down into linear factors.[13]

So, extending our number system to the complex numbers gives us a very elegant theory of polynomials, epitomized by the Fundamental Theorem of Algebra.

The narrative I have just given is often how complex numbers are taught via modern/abstract algebra classes for undergraduate mathematics majors. This is often the first discussion of complex numbers that undergraduates are exposed to aside from the minimal overviews of complex numbers in introductory classes, where students are just handed complex numbers for calculation without any discussion. The resulting belief that complex numbers are introduced to round out the real numbers, combined with the belief about the nature of complex numbers, creates a perfect storm for coming to the conclusion that complex numbers are purely mathematical inventions.

However, even a quick overview of the history of complex numbers completely undermines the belief that complex numbers were introduced to round out the real numbers. It is to such an overview that I now turn.

[13]As a historical note, the Fundamental Theorem of Algebra first appeared (in the form "the number of roots of a polynomial equation [is] equal to its degree") without proof in 1629 in the *Invention Nouvelle d'Algèbre* of Albert Girard (1595-1632), followed by two efforts at proofs of the prevalent form in the 1740s by Euler and d'Alembert. Karl Friedrich Gauss (1777-1855) was the first to give a proof of the Fundamental Theorem of Algebra, his first proof appearing in 1799, two more in 1816, and a final one in 1850. See [21, pp. 224, 281-2, 412].

3.2.1 History of the Complex Numbers

A common misconception is that the complex numbers came into use due to the discovery of quadratic equations without real roots. This is not historically accurate. Before complex numbers came to be widely used, if a quadratic equation appeared to have no real roots, then it simply had no solution. For example, the quadratic equation

$$x^2 + 1 = 0$$

discussed above would have been regarded as having no solutions. How, then, did complex numbers come into usage, if not through quadratic equations like this? The answer is through the solving of the cubic.

By the late 14^{th} century Italian mathematics, solving equations of low degree like quadratic equations was a problem in large part solved. For example, there were formulae for finding roots of quadratic equations; Grattan-Guinness [21, p. 145] gives the example that

$$ax^2 + bx = c \text{ has only } \sqrt{\left(\frac{b}{2a}\right)^2 + \frac{c}{a}} - \frac{b}{2a}$$

Grattan-Guinness notes that "the formulae for roots covered the linear and quadratic equations, but the equations were classified after al-Khwarizmi ... so that all coefficients, quantities under the square root and the roots themselves were positive." [21, p. 145] Note that any quadratic equation can be turned into an equation with all positive coefficients by moving negative terms to the other side. Thus, there are multiple classes of quadratic equations, and that is the spirit of the al-Khwarizmi classification that Grattan-Guinness is talking about. No negative numbers under square roots, and no complex numbers to worry about. The next big problem, though, was solving the general cubic equation.

The cubic was eventually solved by Scipione del Ferro (1465-1526) around 1500 and then by Niccoló Tartaglia (1499/1500-1557) in 1535, eventually publicized by Gerolamo Cardano (1501-1576) in his 1545 *Ars magna*.[14] Cubic equations were treated just as quadratic and linear equations were, classified so that each form has only positive coefficients, again avoiding negative numbers. This, combined with the use of a linear transformation that enabled ignoring the quadratic term, led to there being several different forms of the cubic equation, such as:

$$ax^3 + bx + c = 0, \quad ax^3 + c = bx, \quad ax^3 = c + bx$$

[14] I must follow tradition here and briefly tell the story of these three; I base this on many discussions in math classrooms and [5]. It is thought that del Ferro was the first to find and prove the formula for the roots of the depressed cubic, $x^3 = a + bx$, a,b rational. However, he kept it a secret, releasing it on his deathbed to his assistant, Antonio Maria Fior. Fior then challenged Tartaglia to a mathematics duel, where each gave the other a list of problems, and Fior gave Tartaglia depressed cubics to solve. Tartaglia had already figured out how to solve the cubic of the form $x^3 = ax^2 + b$ with a, b rational, and he figured out how to solve the depressed cubic just in time. Tartaglia himself kept this solution secret, in fact encoding it in a poem. However, Cardano, perhaps in underhanded ways (this is still a matter of contention), obtained the general solution to the cubic from Tartaglia and went on to actually publicize this work.

where $a, b, c \geq 0$ [21, pp.186-187]. Cardano then gave a general formula for each form. For example, Grattan-Guinness [21, p. 187] writes the del Ferro/Tartaglia/ Cardano formula for a root of the first of these forms with $a = 1$ as

$$x = \sqrt[3]{-\frac{c}{2} + \sqrt{\frac{c^2}{4} + \frac{b^3}{27}}} + \sqrt[3]{-\frac{c}{2} - \sqrt{\frac{c^2}{4} + \frac{b^3}{27}}}$$

This will not yield negative numbers under square roots.

However, for the cubic equation of the third form, with $a = 1$, the del Ferro/Tartaglia/Cardano formula is given by[15]

$$x = \sqrt[3]{-\frac{c}{2} + \sqrt{\frac{c^2}{4} - \frac{b^3}{27}}} + \sqrt[3]{-\frac{c}{2} - \sqrt{\frac{c^2}{4} - \frac{b^3}{27}}}$$

And it is here that we see the possibility of getting a negative number under the square root, namely if $\frac{c^2}{4} < \frac{b^3}{27}$. For example, consider the case, discussed by Cardano, of the cubic $x^3 = 15x + 4$. Then the del Ferro/Tartaglia/Cardano formula yields

$$x = \sqrt[3]{2 + \sqrt{-121}} + \sqrt[3]{2 - \sqrt{-121}}$$

Cardano insisted that his general formula for cubics was just inapplicable in this case, as he did not use square roots of negative numbers.[16]

However, it was not as easy to brush off square roots of negative numbers here as it was to brush them off in the case of quadratics. The reason is that *every cubic has at least one real root*. Through the modern lens, this can be seen by noting that the graph of any cubic must cross the x-axis at least once, meaning that it must have at least one real root. But, as a real root $x = 4$ to the above equation was found,[17] the mathematicians were now confronted with the task of "reconcil[ing] the formal and "meaningless" solution $x = \sqrt[3]{2 + \sqrt{-121}} + \sqrt[3]{2 - \sqrt{-121}}$ of $x^3 = 15x + 4$, found by using Cardano's formula, with the solution $x = 4$, found by inspection." [1, p. 322]

This problem was to be taken up by Italian engineer Rafael Bombelli (1526-1572), in his 1572 treatise, *l'Alegebra*. Bombelli had the idea that the cube roots in the above use of the del Ferro/Tartaglia/Cardano formula could reduce to linear forms. That is,

$$\sqrt[3]{2 + \sqrt{-121}} \quad \text{and} \quad \sqrt[3]{2 - \sqrt{121}}$$

[15]See [1, p. 321]. I've changed the variable names to be consistent with Grattan-Guinness's presentation which I use above.

[16]Agarwal [1, p. 321] cites that Cardano considered the problem of dividing 10 into two parts whose product is 40 impossible. This amounts to solving the quadratic equation $x^2 - 10x + 40 = 0$, or $x^2 + 40 = 10x$. Cardano found the two roots, $5 + \sqrt{-15}$ and $5 - \sqrt{-15}$ and even multiplied them, finding that there product was 40. However, according to Agarwal, Cardano "did not pursue the matter but concluded that the result was 'as subtle as it is useless.'" Agarwal also notes that "it was the first time the square root of a negative number had been explicitly written down." [1, p. 321]

[17]In fact, the equation $x^3 = 15x + 4$ has three real roots, 4 and $-2 \pm \sqrt{3}$.

could reduce to the forms

$$a + \sqrt{-b} \quad \text{and} \quad a - \sqrt{-b}$$

respectively. Bombelli then worked with the equations

$$\sqrt[3]{2 + \sqrt{-121}} = a + \sqrt{-b} \quad \text{and} \quad \sqrt[3]{2 - \sqrt{121}} = a - \sqrt{-b}$$

and used the formal operations used with real variables to find that $a = 2$ and $b = 1$. This then meant that the del Ferro/Tartaglia/Cardano formula could be rewritten as

$$\sqrt[3]{2 + \sqrt{-121}} + \sqrt[3]{2 - \sqrt{121}} = (2 + \sqrt{-1}) + (2 - \sqrt{-1}) = 4$$

thereby recovering the real solution $x = 4$. And so began the use of complex numbers.[18]

It is worth taking a brief look at how Bombelli actually presented his thought about complex numbers. Here is an extract from page 169 of the 1579 edition of *l'Algebra* in Italian, English translation below:

Più uia più di meno, fà più di meno.
Meno uia più di meno, fà meno di meno.
Più uia meno di meno, fà meno di meno .
Meno uia meno di meno, fà più di meno.
Più di meno uia più di meno, fà meno.
Più di meno uia men di meno, fà più.
Meno di meno uia più di meno, fà più.
Meno di meno uia men di meno fà meno.

A positive times a positive square root of a negative gives a positive square root of a negative.
A negative times a positive square root of a negative gives a negative square root of a negative.
A positive times a negative square root of a negative gives a negative square root of a negative.
A negative times a negative square root of a negative gives a positive square root of a negative.
A positive square root of a negative times a positive square root of a negative gives a negative.
A positive square root of a negative times a negative square root of a negative gives a positive.
A negative square root of a negative times a positive square root of a negative gives a positive.
A negative square root of a negative times a negative square root of a negative gives a negative.[19]

[18]This paragraph closely follows the presentation in [1, p. 322].
[19]Scan of page 169 of *l'Algebra* and the accompanying English translation of this section are from [24].

In (somewhat) modern notation, we would write these rules as

$$\begin{aligned} &+\cdot +i = +i && +i\cdot +i = - \\ &-\cdot +i = -i && +i\cdot -i = + \\ &+\cdot -i = -i && -i\cdot +i = + \\ &-\cdot -i = +i && -i\cdot -i = - \end{aligned}$$

Looking at the excerpt from *l'Algebra* should underscore how differently mathematics was done back then as opposed to now. Algebra was still undergoing a transformation, part of which involved the introduction of symbols as opposed to words. It should be noted that this transformation took place over several centuries.

However, this was not the end of the story for complex numbers. Agarwal cites several figures in mathematics, from the late 16^{th} century to the early 19^{th} century, who still doubted the legitimacy of the complex numbers. For example, John Wallis (1616-1703) wrote that "These Imaginary Quantities (as they are commonly called) arising from the Supposed Root of a Negative Square (when they happen) are reputed to imply that the Case proposed is Impossible" [1, p. 323] while Christiaan Huygens (1629-1695) wrote in a letter to Gottfried Liebniz (1646-1716) that "One would never have believed that $\sqrt{1+\sqrt{-3}}+\sqrt{1-\sqrt{-3}}=\sqrt{6}$ and there is something hidden in this which is incomprehensible to us." [1, p. 323]

However, as Agarwal notes, complex numbers began to rack up theoretical and applicational uses in that same period. For example, new trigonometric identities were found that made use of complex numbers, and complex numbers came to be used in applications such as d'Alembert's hydrodynamics.

The press for a logical foundation or explanation of complex numbers became acute in the latter part of the 18^{th} century, as Agarwal notes,[20] due to mathematics being thought of as the model of rational thought. The complex numbers' stowaway of irrationality needed to be confronted, and this project found its first successes with the work of Gauss. Gauss showed that complex numbers can be thought of geometrically as points in the plane, publishing this thought in work on number theory in 1831. Further developments by Casper Wessel (1745-1818), Jean-Robert Argand (1768-1822), Karl Theodor Wilhelm Weierstrass (1815-1897), Cauchy, and Niels Henrik Abel (1789-1857) reinforced the place of complex numbers in a rational mathematics as justified by Gauss.

Even still, uncertainty about complex numbers continued. Even after William Rowan Hamilton (1805-1865)'s more rigorous 1831 algebraic definition of complex numbers as pairs of real numbers, George Airy (1801-1892) stated, "I have not the smallest confidence in any result which is essentially obtained by the use of imaginary symbols." [1, p. 324] However, by the end of the 19^{th} century, this doubt and mistrust seems to have vanished, with French mathematician Jacques Salomon Hadamard (1865-1963) declaring that "[t]he shortest route

[20] The next couple paragraphs closely follow [1, p. 323-4].

between two truths in the real domain passes through the complex domain." [3, p. 110]

3.2.2 Philosophical Analysis: "Inventing" Complex Numbers?

The above historical narrative flatly refutes the narrative from undergraduate mathematics concerning the introduction of complex numbers for a more elegant theory of polynomials.[21] It also draws the resulting belief that "complex numbers were introduced to round out the real numbers" into significant doubt. It then becomes unclear if we can render the thought that "complex numbers were developed for purely mathematical reasons" intelligible in a way that fits with this historical narrative. It will be the argument of this section that it is at least viable to answer "no." In the following, I will call the claim that we cannot find such an intelligible rendering of that idea the *impurity claim*.

What may be initially discouraging about pursuing the impurity claim is that there is no clear influence of a physical problem here, as there was in the case of the *Qibla* problem. However, all that this means is that the answer is not as simple as it was there. One observation that should really motivate us to pursue the impurity claim is that, as mentioned above, mathematics and physics did not split into distinct disciplines until sometime during the 19^{th} century. So, those same people grappling with square roots of negative numbers in the 1500s were not "pure mathematicians;" they worked across the board, in what we would call applications of mathematics as well as mathematics itself. As such, we should be hesitant about calling anything "purely mathematical" during this period.

However, there is further motivation for pursuing the impurity claim in light of the context of the historical narrative above. Grattan-Guinness notes that "[Cardano's] conception of algebra was oriented around its applications to arithmetic and geometry, and the subject was still largely concerned with calculating or approximating to numerical values." [21, p. 188] That is to say, complex numbers arose from a problem in algebra, the goal of which, at that point in history, was calculation for other disciplines.

We may then wonder what motivated the problems of the disciplines in which algebra was applied, namely arithmetic, geometry, and trigonometry. In the early 16^{th} century, arithmetic was still primarily motivated by worldly problems, and trigonometry and geometry were similarly motivated, especially by problems in navigation, astronomy, perspective, and optics.[22]

[21] Although I should note here that it is not the intention of undergraduate mathematics classes to teach that narrative as actual history. Rather, that narrative provides a natural structuring of courses in modern algebra, and it would seem as though many who learn about complex numbers in this way without learning the actual history come to that belief that complex numbers were introduced to round out the real numbers.

[22] See [21, pp. 175-85]. With regard to arithmetic and its practical motivations, see Grattan-Guinness's discussion of British mathematician Robert Recorde (1510?-1558), especially the discussion of the problem of proportions in coin-making. On trigonometry and navigation, see in particular §4.8.

As far as appropriately recognizing historical context goes, it is also important to note the heritage of the problem of solving equations. The problem of solving equations as inherited by the Italian mathematicians discussed in the historical narrative can be traced back through to Arabic mathematics. Solving equations was a problem in arithmetic for the Arabic mathematicians, where arithmetic encompassed problems we would now define as belonging to algebra, seeing as algebra had not yet grown distinct from arithmetic. Grattan-Guinness notes that, regarding Arab arithmetic, "texts in these branches were prepared to teach administrators, tax collectors, and similar functionaries." [21, p. 116] The problem of solving polynomials was initiated by al-Khwarizmi in a book on equations written sometime in the 9^{th} century, where, as indicated in the historical narrative above, al-Khwarizmi would rewrite quadratic equations to avoid negative numbers.[23]

After the 12^{th} century began in Europe, throughout the next couple centuries, Greek and Arabic texts in mathematics became more familiar to Europeans, due to increased availability of translations and increased contact with the Middle East. Grattan-Guinness notes that, of the Arab mathematicians, al-Khwarizmi in particular garnered much attention in Europe, especially for translations of his book on equations. Diophantos's work on "indeterminate analysis" was also eventually translated and thus available for the European mathematicians to draw from. Many advancements were made in both arithmetic and the gradual rise of algebra as a distinct subject in those centuries, although it seems as though work was still motivated by worldly problems.

Thus, by the time the problem of solving equations reached del Ferro, Tartaglia, and Cardano, it had a history rife with worldly motivations. This alone should give pause to anyone prepared to discount the impurity claim immediately.

I now want to argue specifically from the historical narrative above. Broadly speaking, the development of complex numbers is neither simple nor clean. Not only did it take several centuries after their appearance for them to become widely accepted, even the work of Cardano displays a hesitance about them. Had they been conceived as purely mathematical constructs, a natural thought is that there should not have been a problem. However, what we see in the historical narrative is mathematician after mathematician fretting over the meaning of the square root of a negative number. And, even though eventually Bombelli accepted complex numbers, it was not because they provided for an elegant theory of polynomials, or that they made finding the roots of the general cubic easy. It was because the mathematicians had no choice if they wanted to reconcile a del Ferro/Tartaglia/Cardano formula that had square roots of negative numbers with the cubic in question having a real root. Then, even after Bombelli, even after numerous seemingly successful applications of complex numbers, the doubt remained.

What we really see in the historical narrative is a slow, gradual alteration in the language of mathematics that involves admitting a new set of vocabulary

[23] As an interesting and oft-left out historical note, al-Khwarizmi called this operation *al-jabr*, and this is where the name "algebra" comes from.

and rules for their use. This change is not simple and takes several centuries to find its final form and stick, and so we see an analogue here with how language changes.

There are a few quick objections that I would like to address and so further bolster the legitimacy of the impurity claim. The first objection is that it still leaves the worry about the *physical meaning* of complex numbers untouched. If complex numbers are not purely mathematical inventions, we are left with having to answer the question of what they are, what meaning they have, given that the natural numbers, negative integers, rationals, and reals all seem to have such meanings. This objection can also take a historical angle by pointing to the persisting doubt about the legitimacy of complex numbers.

There are two ways to reply to this objection, addressing the ahistorical and historical parts respectively. To respond to the ahistorical portion of the objection, I would note that the impurity claim does not claim that complex numbers are not purely mathematical inventions; it claims that we cannot render the idea that "complex numbers were developed for purely mathematical reasons" intelligible in a way that squares with the history of complex numbers. The impurity claim concerns the circumstances of the development of complex numbers, claiming that we cannot reduce any narrative of their development to "purely mathematical motivations." Nothing about this constrains the nature of complex numbers. Even if they did not have any physical meaning, this is consistent with insisting that their development was more nuanced.

We can also reply to the historical portion of the objection by noting that we should have similar worries about negative numbers. Recall that negative numbers were also widely distrusted and avoided by Cardano and his contemporaries, as evidenced in their use of the al-Khwarizmi classification.[24] Thus, if the objection is that our impurity claim should be troubled by the persisting doubt in the legitimacy of complex numbers, then it would seem that the same should apply to negative numbers, even though we do not naturally have similar doubts concerning their meaning. Although, I will admit, I do not believe that this is something we should take seriously. Rather, I intend this response to motivate us to think more critically about historically-based arguments and guard ourselves against retroactively projecting our own beliefs onto historical figures.

While I do not believe that I have definitively proven the impurity claim to be true, this section has given some credence to the impurity claim, elevating it to a hypothesis deserving of empirical investigation. And empirical investigation is what is needed here. While the brief historical narrative that I have provided here suggests the possibility that the impurity claim is not outrageous, further and more detailed historical work will be able to go a great distance in providing (or removing) credence for the impurity claim. In particular, historical work going through the primary sources in this history and analyzing not just *what* was being talked about, but *how* and in what *context*.

[24] Although, it should be noted that Fibonacci allowed negative integer solutions to arithmetic problems as early as 1202. (See [43, p.116])

3.3 The Language Metaphor

It will be of use at this point to briefly pivot to a discussion of linguistic development, the processes by which language evolves over time, as well as the subtleties of linguistic use. Mark Wilson provides good discussions of these sorts of phenomena that pay careful attention to details. For example, in his book *Wandering Significance*, Wilson discusses the evolution and complexities of several predicates, including the predicate of "being red," or "redness."

In this specific case, Wilson is concerned largely with claims of the nature "grasping the concept of red" and "understanding redness." In this vein, Wilson talks about what he calls the "directivities" that guide the acceptable usage of a concept, that is, the rules regarding the usage of the concept. Wilson first discusses the change that the concept of redness underwent after science demonstrated that "colors are not a part of the objective world," or rather that there is no direct correspondence between the classification of colors humans have and properties science recognizes as actually being possessed by things in the world. Wilson states that this led to a "radical subjectivization of color," whereby color was regarded as something in the mind of the observer, rather than something in the world [47, p. 74]. Even in this initial step, though, there are complexities, for then such questions arise as whether color is not an objective property in the world, or rather whether it is only so within the purposes of science.

Wilson goes on, though, to describe another complexity underlying the concept of redness. While for centuries philosophers and scientists had asserted that understanding the concept of redness requires direct perceptual experience, Helen Keller, who was born both blind and deaf, challenges this assumption in her autobiography by asserting that she does understand the concept of redness [47, pp. 105-6]. Wilson discusses this as a different type of directivity guiding the use of the concept *red*. Wilson argues that there may not exist, and often does not exist, a simple directivity guiding the use of a concept. Rather, there may be a constellation of directivities, guiding the use of the predicate in different domains and in different contexts.

This discussion of application of concepts to different domains, or patches in Wilson's parlance, is exemplified by considering how to apply the "redness" concept in extreme circumstances, such as under irregular lighting or in a Plutonian mine. Certainly, objects appear to be different colors when under different colored lights, and the response that "the true color is what color it is under regular light" begs the question of what regular light is and why it is privileged. Wilson discusses the example of Plutonian rubies in more detail, stating that

> Suppose we have constructed our explorer to hunt exclusively for Plutonian rubies. Pluto, however, is both a cold and ill-lit spot, well outside the range of earthly variation. The hues of beryls like rubies and sapphires depend sensitively upon scattered color center impurities in their matrix (the pure mineral is colorless). It is within the realm of possibility that the intemperate Plutonian conditions may induce a subtle shift in the crystal array, causing the local stones to

> unexpectedly reflect the dim sunlight strongly in the green. Likewise, beryls we would consider to be of poor quality reflect preferentially in the red in the Plutonic conditions. Even if we visit Pluto, we won't be able to see these effects, because our color vision will not be active in the low illumination; however, the altered spectral reflectances will be apparent in a time exposure photograph. Should such greenish, frozen stones qualify as rubies, for if we merely subject them to stronger light, the radiant heat will shift their delicate structure sufficiently to reflect strongly in the red as normal rubies do? Or should we say that terrestrial stones stop being rubies within Pluto's bitter climate? [47, pp. 231-2]

This is a more extreme example of the complexities that result from extending predicates to new domains. Indeed, there does not seem to be a "right" choice on how to proceed, and certainly there is no way to proceed in accordance with what Wilson calls the "classical picture," according to which predicates like "redness" have fixed core contents, core directivities guiding their usage.

Wilson's discussion of redness demonstrates that language is more complex than we take it to be, including and especially how language manages to mesh on to the world. The picture that Wilson paints is one whereby language gradually evolves to expand into new domains, changing in order to suit our purposes in navigating and describing the world around us.

We can then compare this narrative of language and the language-world connection with how I have above described mathematical developments and the math-world connection. It should be clear from the episodes in the history of mathematics discussed above that there is not an easy answer to any question about developments in mathematics and connections with the world, just as in the case of language. What should also be clear from the above historical episodes is the close relationship between mathematics and what we now refer to as the mathematical sciences, that is, mathematics applied to the world. Even when the development of a piece of mathematics is not explicitly and clearly connected to a worldly problem, successful application in worldly problems can often play a role in lending credence to the piece of mathematics in question, as with the case of the complex numbers.

Now, I want to sketch a way to view mathematics that makes an analogy with language. I mentioned in the previous section that the development of algebra was a slow, gradual process, and that the introduction of complex numbers and the rules guiding their usage into the standard mathematician's toolbox also took quite awhile. This naturally suggests an analogue with the way in which language changes gradually to adapt to new circumstances, for example the processes which Wilson discusses in relation to the concept of "redness" in the above discussion.

We might generally conceive of mathematics as another sort of language, which we gradually alter in order to suit navigation, description, and prediction in the environment we find ourselves in, just as we do with ordinary language. The long and rich process of admitting complex numbers into the language of mathematics may be in many ways akin to the long and rich process that color words underwent as scientific theories of light evolved. Indeed, both processes involve significant interaction between the piece of "language" evolving and scientific theory.

It is worth taking a second and focusing on the "interaction with scientific theory" part of the language analogy. We express statements of theory both in natural language (supplied with theoretical terms) and the language of mathematics. On the one hand, preconceived notions tied to words in ordinary language may be carried into scientific theorizing that utilizes such words. On the other hand, scientific inquiry may revise the meaning of such words. Similarly, pieces of mathematics may enter into scientific theories carrying the meanings formed before their introduction into theory, and they may also be altered as a result of scientific theorizing.

Numerous examples provided for the warehouse claim support the notion that mathematics entering into scientific theorizing carries previous meaning; consider, for example, the continuity assumptions carried into continuum mechanics by the use of infinitesimals. However, it is also true that a piece of mathematics may be altered as a result of scientific theorizing. A simple example of this is the Qibla problem's motivation of changes, or developments, in spherical and planar trigonometry. We also see, in the case of the complex numbers, successful usage in scientific theories (such as d'Alembert's hydrodynamics) lending credence to the legitimacy of complex numbers.

Indeed, just as we can conceive of the concept of redness being extended into new domains, such as in Plutonian mines, with no clear "right" direction forwards, so we can similarly conceive of how the number system evolved to encompass complex numbers. It was not an arbitrary process on the one hand, just as deciding the Plutonian ruby question is by no means arbitrary. But, on the other hand, there is no clear right answer as to whether to include complex numbers in the number system, just as there is no clear answer how to classify rubies on Pluto. In the case of the complex numbers, an accumulating mass of usage and success in application lent credence to taking the path of accepting complex numbers into our number system. And this, in a way, resembles the way the color concept changed as a result of scientific theories of light, and we might imagine similar usage and success experiences guiding how we end up classifying Plutonian rubies.

Now, this two-way interaction between science and mathematics should lead us to doubt the viability of the warehouse claim. And the language analogy as a whole should lead us to doubt the legitimacy of the necessary connection claim. If this is indeed a strong analogy, then the fact that we don't take our specific natural language to be fundamentally intertwined with the scientific enterprise should lead us to doubt that mathematics is fundamentally intertwined with the scientific enterprise. Rather, the appropriate view of mathematics is that it provides a (not necessarily unique) way to articulate scientific theories. That is, mathematics is *a* language *that we have developed to describe* Nature, rather than *the* language *of* Nature.

We may actually conceive of the ubiquity of mathematics in science in the following way. We take as a central value in many parts of science the notion of *precision*. We have found that mathematics provides a particularly fruitful way to pursue this value of precision. And, as such, mathematics has become a primary way to articulate our scientific theories so that they are precise.

However, there is nothing inherent in the notion of precision that makes it necessary as a guiding value of science, science broadly conceived of as an enterprise for understanding the world. The idea of precision, I suspect, is tied up with Enlightenment ideals tied to manipulation of the world, and so once science is conceived of as "the pursuit of the ability to manipulate our surroundings," precision becomes a guiding value. But this just means then that precision, and by extension mathematics, is useful for science *in its pursuit of manipulability*. And even then only in certain contexts. Thus, there may be some explanation of the ubiquity of mathematics in science that does not reference the necessary connection claim.

3.4 The View From the History of *the Mathematical Sciences*

I have demonstrated how interrelated mathematics and the mathematical sciences have been throughout their histories and the difficulty, sometimes even impossibility, of separating out developments in one or the other. From this vantage point, it should seem at least viable to posit that a successful account of applied mathematics must also be an account of mathematics (or, a successful account of mathematics must also be an account of applied mathematics). I will now list several reasons, some from the above analysis, some external, for pursuing such an approach.

First, from the various histories presented above, I hope it has been clear that it is difficult, if not sometimes impossible, to distinguish between developments in one or the other of applied and pure mathematics. Certainly, there are mathematical developments that are motivated by worldly problems, as in the case of the Qibla problem motivating developments in spherical and planar trigonometry. Such cases already present some difficulty for an account that wishes to deal only in either applied mathematics or pure mathematics. However, an account of pure mathematics may still seek refuge in more "pure" developments, such as the case of the complex numbers, while an account of applied mathematics may seek to avoid such cases. But, even though there is no worldly problem directly motivating the development there, successes in worldly application still motivated giving credence to complex numbers, helping to solidify their place in mathematics. And so an account of pure mathematics is not sufficient here, nor is an account of applied mathematics. Rather, in order to give a sufficient account of complex numbers, we must take a new approach that does not lie within the pure-applied division.

Second, there is a more general reason to prefer a combined account of pure and applied mathematics; even if they could be separate, each must still draw on the other, and so it would be beneficial for them to be developed side by side in order to better work in tandem. An account of applied mathematics must be able to tell a story about the pieces of mathematical machinery that appear in a given mathematical model, which requires an account of pure mathematics. On the other hand, an account of pure mathematics, in dealing with the epistemic and metaphysical status of the objects of mathematical study, cannot ignore the application of mathematics to resolve worldly problems, and thus must draw on the resources of an account of applied mathematics. So, even if we were to grant a division between pure and applied mathematics and develop distinct accounts for each, they must both draw on the resources of the other. Thus, given that they must work in tandem anyways, it seems beneficial to develop them in tandem.

Finally, there is a more sociological reason for pursuing a unified account of pure and applied mathematics, and that is simply that such a distinction is always hard to draw and will always be blurry at best. It is true that there are separate publication avenues for pure and applied mathematics, that they may even occupy different departments and programs in academic settings. But it would be wrong to take this to imply two distinct subjects with distinct methodologies and subject matter. While it may be possible to identify certain people and projects that fall certainly in one or the other camp by any standard of demarcation, there is continuity in method and subject matter between the two camps, and there are certainly nontrivially many people and projects lying on various parts of this continuum in between. Not to mention the distinction between pure and applied mathematics did not arise, as mentioned above, until sometime in the 19^{th} century, forcing any account of (pure or applied) mathematics which considers the history of mathematics to recognize this blurriness between the two.

Having motivated the notion of a unified account of mathematics spanning the spectrum from pure to applied, we must now revisit the question of physical concepts. I have argued that they work with mathematics in order to make for a successful mathematical representation, and thus the question must be addressed from two ends: the mathematics side and the physical side. In the remainder of this chapter, I will focus on the physical side, as this is the side which I have so far developed to a point where I can provide fruitful discussion. The mathematics side must be addressed, and I will sketch out some thoughts on that subject in the conclusion.

3.5 Concepts As Mediators: Conceptual Infrastructure

In the discussion of the development of the Navier-Stokes equations and Prandtl's boundary layer solution, I showed that conceptions of viscosity mediated between physical theory, the aims of modeling, and the pieces of mathematical machinery. A natural question is whether we might be able generalize this work that a physical concept does in mediating these connections and find it in other pieces of applied mathematics. I believe that this is not only possible, but in fact an integral part of any satisfactory account of applied mathematics.

The salient point in my discussion of viscosity was that the concept of viscosity had a complex internal structure that allowed it to perform many different roles for modelers, depending on the aims of the modeler. I will refer to this complex internal structure of a physical concept as the *conceptual infrastructure* of the concept. The notion of infrastructure calls to mind networks of roads, highways, bridges, railroads, maybe even airways and waterways. These all provide a variety of ways to get from Point A to Point B, taking different routes that serve different purposes, with different modes of transportation operative in connecting different regions. It is in this way that a physical concept may be said to have "infrastructure." It is true that the concept as a whole may have a guiding role, or guiding purpose in physical theory, much like transportation networks may be unified by their purpose and character (such as the more general aim of connecting Paris and Baghdad, or the more specific aim of delivering Amazon packages). However, at the same time, these concepts take on different sets of qualities, different interpretations, different directivities (in Wilson's parlance) in different contexts.

For example, the viscosity concept can be taken in either of the micro- or macro-conceptions mentioned in the previous chapter, depending on the context. It was Prandtl's innovation that these be separated out through what I have identified as a multi-scale modeling procedure. Likewise, the concept of viscosity has inroads connecting different empirical phenomena through which we access viscosity in observation, such as the flow retardation and fluid resistance observed and measured by the engineers.

These different internal components of the viscosity concept get used in different permutations depending on the purpose for which the concept is being used. In models aimed at empirical adequacy, the macro-scale "mode of transportation" delivers us between the empirical phenomena of flow retardation and fluid resistance, with some rough paths carved out towards other concepts such as pressure and velocity. This naturally runs on the fuel of proportionalities and simple approximation techniques. On the other hand, models aimed at fundamental ontology and rigorous derivation opt for the micro-level modality to traverse regions far from the macro empirical phenomena of resistance and retardation, rather providing quick transport to the distant lands of molecules and intermolecular forces on the stronger fuel of analysis.

This sort of work that the viscosity concept does, that many physical concepts do, may be generally characterized as mediating between physical theory, the aims of modeling, and the mathematical machinery of the given model. In fact, it is necessary that physical concepts perform this role for mathematical models to be successful at all. There must be some connection between physical theory, the aims of modeling, and mathematics in order for a piece of applied mathematics to model some given phenomenon adequately. If the physical theory is not appropriately moderated by the aims of modeling, it would be unclear from what physical theory to draw, what physical theory is appropriate for the modeling task at hand. The aims of modeling must also moderate the mathematics used to articulate the physical theory.

Physical concepts are uniquely situated to play this mediating role. Many physical concepts, such as space, time, and energy, have been developed in such a way that they can be associated with numbers through measurement. Further, the relations between physical concepts in physical theory can be articulated through mathematics by making the physical concepts functions of one another.

However, it is also true that physical concepts have a complex structure of usage, with it often being true that there are many conceptions of a given physical concept guiding the usage of that physical concept in different domains. In the viscosity example, we saw different scale-dependent conceptions of viscosity. There was one conception that guided the usage of the viscosity concept in the molecular domain and one conception that guided is usage in the macroscale domain.

Another example of a physical concept with different scale-dependent conceptions is temperature. Closely paralleling viscosity, there is a macro-scale conception of temperature according to which it is a homogeneous property of some substance, and there is a micro-scale conception of temperature according to which it is an aggregate property of molecules in motion. However, what is interesting about temperature is that these two different conceptions (generally) wound up in being studied in different subfields of physics; thermodynamics is for the most part concerned with the macro-scale conception of temperature, whereas statistical mechanics is largely concerned with the micro-scale conception. Hasok Chang's narrative charting the development of thermometry [7] gives a good part of the story behind this division and the evolution of these two conceptions of temperature, alongside other conceptions, as well as demonstrating how the various conceptions of temperature functioned in the various mathematical models involving temperature.

This flexibility, this differing in conception of concepts across different domains, allows them to adapt to various aims of modeling. Again, as in the viscosity case, we see one conception particularly suited to the aim of empirical adequacy and the other particularly suited to rigorous derivation (and true representation), with an admixture of them allowing us to generate a representation that served several aims of modeling.

This way in which physical concepts function as mediators in a particular mathematical model is what is meant to be encapsulated by the notion of *conceptual infrastructure*. It is exactly this conceptual infrastructure that is necessary to answering the specific question for a piece of applied mathematics, and it is exactly this conceptual infrastructure that the mapping account passes over. Since the mapping account centers true representation and focuses on the structural aspects of the mathematics, it will not "see" the delicate and rich mediation being performed by the involved physical concepts, and so will miss this entire story.

Chapter 4

Conclusion: Integrating Mathematical and Physical Concepts

"[The mathematician is] constantly occupied with his formulae and blinded by their abstract perfection, often mistakenly assuming that the inner relations he had found reflected processes in the real world."
Ludwig Boltzmann (1844-1906)[1]

Now that I have argued for a unified account of pure and applied mathematics and have argued for the centrality of conceptual infrastructure in accounting for pieces of applied mathematics, it is time to sketch out the general outline of what an account of mathematics should look like. In fact, I will present in this conclusion the idea that a satisfactory account of how mathematics functions cannot be given a general, schematic, axiomatic form. Rather, the appropriate approach to mathematics treats each case individually. What the account does is provide a set of tools for analysis. I will not be able to enumerate all of the tools here but can speak to the utility of those that I have used in my above analyses.

In what follows, I will first in general discuss how a unified account of pure and applied mathematics may take shape, essentially from the notion that we understand mathematics through modeling. I will then summarize the tools of analysis that I used in the previous chapters and their utility in such an account of mathematics, ending the chapter with a discussion of several possible farther-reaching implications of such an an account of mathematics, particularly ramifications in the interpretation of physical theories.

[1]From his 1890 inaugural address when he became the Professor of Physics at Munich, quotation from [23, p. 4].

4.1 Understanding Mathematics Through Mathematical Modeling

In the previous chapter, I argued that the best philosophical approach to pure and applied mathematics is to develop an account that does not make any clear distinction between the two. However, in my discussions in the first two chapters, I was focused solely on accounting for given pieces of applied mathematics. Naturally, there is a question as to how the treatment I have given, for example, to boundary layer theory may be extended not only to other instances of applied mathematics, but to mathematics in general. I will argue that the main facets of that analysis which need to be extended are (i) the case-specific style of analysis, and (ii) the analysis of the mathematics *as it is being used*.

One of the reasons that my analysis of Navier-Stokes and Prandtl's boundary layer solution was able to wander through various subtleties of the viscosity concept and their role in the mathematics is that I was not consciously presupposing a general form of method or answer.[2] Rather, though I used several tools of analysis in conducting my analysis of Navier-Stokes and Prandtl, I for the most part let the details of the case drive the direction that the analysis took. Rather than presupposing that the narrative would take a specific form or that a particular pattern of connecting math to world would emerge, I followed the historical narrative of the study of fluids and noted subtle details along the way to the best of my ability. While it is true that out of these I drew out one specific strand of narrative, this choice was not guided as much by preconceived notions as by the case itself.

Now, it is true, of course, that some preconceived notions played a role in the particular subtleties I drew out of the narrative; this is just a facet of any analysis. The guiding notions in my analysis, however, rested in a conviction not to gloss over the complexity of the phenomenon being studied and the historical narrative. So, while I was certainly focused on the viscosity concept and the details having to do with viscosity in the historical narrative, I like to think that the tenet of remaining true to the developmental history of the phenomenon afforded the ability to balance out any illegitimate influence these preconceived notions might have had.

This leads to a first methodological principle of a satisfactory account of mathematics, the case-specific approach:

> **Methodological Principle 1 (No Axioms):** Analyses of how mathematics enhances our understanding of the world should be case-specific, not presuming a general method or answer form.

That is, we should be wary of any account of mathematics that gives a general, axiomatic form to either methods for asking about mathematics or answers to questions about mathematics. For example, the mapping account does, in a sense, assert a general form for the question of how mathematics is successful in aiding in our understanding of the world: the so-called "mapping" relation.

[2]Of course, there will always be biases of one sort or another. Still, there is some amount of control the analyst may exert over conscious presuppositions.

It is claims like this that the "'No Axioms' Principle" tells us to be wary of.

Now, my analysis also benefited from paying attention to the mathematics *insofar as the mathematics was being used in that instance.* That is, my focus was on how various models of fluid flow were used in the given historical period and how they were understood by thinkers in that period, rather than considering the models in the abstract, apart from how they were used or understood in their period of development. This was in fact crucial to my analysis, as this is why the historical narrative was so important to trace. It was important to understand the evolving usage of fluid flow models and the evolving understanding of their aims and why they worked (or not).

Since the size and complexity of pieces of mathematics vary in size, there will be some degree of drawing on understanding of pieces of mathematics external to the case being considered. For example, I did rely on an understanding of the Laplace operator as, for example, applied in the various dimensional heat equations in my analysis of how it was used in Navier-Stokes. However, I also connected this back to the macro- and micro-scale conceptions of viscosity that emerged from the historical narrative. And still, in both cases, it was an understanding of the Laplace operator *in use* rather than abstracted from usage.

From this, we can generalize a second methodological principle for a satisfactory account of mathematics regarding this "in use" style of analysis:

> **Methodological Principle 2 (In-Use Analysis):** A given piece of mathematics should be analyzed as it is used rather than in the abstract.

This principle encapsulates the idea that, in analyzing some piece of mathematics, the analysis should focus on how the piece of mathematics gets used; this includes tracing out development as well as subsequent usage. It is important to trace out the development of the usage of the piece of mathematics, similarly to how I traced out the usage of the viscosity concept in various models of fluid flow.

Importantly, these principles generalize beyond accounting for pieces of applied mathematics. The first principle simply states that there should be no general method of analysis or general form of answers to questions, and this applies to analyses of pieces of "pure" mathematics in addition to pieces of "applied" mathematics. I also did not presume a general form of answer or a general method for my analysis of complex numbers and questions regarding their motivation. Rather, paralleling my analysis of Navier-Stokes and Prandtl, I largely let the details of the case guide the analysis. So, again, the first principle applies.

The second principle also generalizes to all mathematics; whether it's understanding how a piece of mathematics functions in a given model of a physical system or how a piece of mathematics functions in a proof or theoretical framework, pieces of mathematics should be understood as they are used. This principle is part of a larger trend in philosophy of science and philosophy of mathematics that focuses on the *practice* of scientists and mathematicians, and it has proven a fruitful mode of inquiry.[3]

[3] See, for example, the work coming out of the Society for the Philosophy of Science in

Before turning to a discussion of several of the analytical tools I used in the above analyses, I would like to briefly discuss how the above two principles come to bear on more mainstream approaches to the foundations of mathematics.

The three traditional approaches to the foundations of mathematics are logicism, intuitionism, and formalism. I will focus here on logicism and formalism. Logicism is mainly associated with Frege, Russell, and Whitehead, notably Russell and Whitehead's *Principia Mathematica*, in which they attempt to reduce mathematics to logic. This is the basic tenet of logicism, that mathematics reduces to logic. Although it took a major hit after Gödel's incompleteness theorems, logicism remains alive with several contemporary advocates. Logicism, however, violates both principles laid out above; in attempting to reduce all mathematics to logic, logicism assumes a general form for the answer to the question of why mathematics seems a priori necessary (violating Principle 1) and only considers mathematics in abstract (violating Principle 2).

Formalism, of which Hilbert's program is quite representative, adopts a similar approach. However, the general formalist strategy is to assert that mathematics is something like a useful fiction, a game with a specific set of rules. Or, to put it another way, mathematics is a formal system with rules for manipulating the symbols but no associated content. However, formalism, like logicism, violates both principles. It again assumes a general for an answer to a question, this time the question of what mathematical knowledge consists in. And, again, the focus is on mathematics abstracted from any particular usage.

4.2 The Analyst's Toolkit

Now, I would like to quickly discuss the general tools of anlaysis that I used in the various cases I discussed in this thesis, mainly the Navier-Stokes/Prandtl case and the complex numbers case. The three tools I want to highlight are: historical analysis, conceptual analysis, and mapping analysis. All three are useful, and, although they may superficially disagree, it is in this tension that an especially insightful and valuable analysis can emerge.

Historical analysis clearly played a significant role in my discussions of fluid mechanics and complex numbers. The key role of historical analysis is to center how various ideas *actually* developed and evolved, including not only intellectual motivations for such evolution, but also social and other external motivations. As such, historical analysis must be careful, able to pick up on the nuances and subtleties of the historical narrative, which is often messy and takes several paths that diverge and intertwine in strange ways. History is especially important once the attitude that scientific progress is "purely cumulative" is discarded (as Thomas Kuhn demonstrated in [25]).

Conceptual analysis also played a significant role in my analyses, largely in my analysis of Navier-Stokes and Prandtl through analyses of the viscosity concept at various stages in its development. The value of conceptual analysis

Practice for examples of the utility of this sort of inquiry. On the mathematical side, see [30] for several essays in this vein.

lies in its ability to pick apart subtleties lying within a particular concept, differentiating between the concept as it is used in different contexts. This sort of conceptual analysis has both a historical and an ahistorical bent to it; I not only laid out how viscosity was understood by various thinkers throughout the evolution of fluid dynamics, but also provided my own analysis of this evolution and the varying uses of the viscosity concept. Crucially, this style of conceptual analysis also relies on the previously-mentioned insights from Wilson and Chang concerning how complex concepts are in use. And I must again emphasize the role that recognizing conceptual infrastructure plays in understanding a given mathematical model.

Finally, I want to briefly remark that the mapping intuition should not be completely discarded; it does do valuable work for us. For example, it does seem to work in the simple arithmetic case and in the Bridges of Königsberg case, at least to some degree. The identification of a mapping, should one exist, plays an important role in answering questions about how that piece of mathematics works, even if the mapping is not the entire answer. Additionally, the mapping intuition is just that, an intuition that we have about how mathematics works. And it is important to at least follow up on our intuitions, so long as we leave them at critical distance and balance out their influence with our other tools of analysis.

These tools of analysis, and the others that may be used, may indeed need to balance one another out. They may seem to contradict one another when it comes to answering specific questions about how a given piece of mathematics works. In line with the first principle listed above, there is no a priori guideline for arbitrating between the analyses; what should guide such conflicts is the case at hand. The details of the case should guide the analysis, and these will usually be able to aid in resolving conflicts between the various tools of analysis.

4.3 Implications of the Account

I would like to wrap up this thesis with quick discussion of some of the more far-reaching implications this account has: the metaphysics and epistemology of mathematics, the reading of metaphysics off of the mathematics of a physical theory, and the use of mathematical consistency as a theoretical virtue.

Many philosophical accounts of mathematics get stuck on the thorny question of the metaphysical status of mathematical objects. However, I think that the sort of account sketched above suggests that this question is ill-formed; it focuses on mathematics abstracted from usage and so violates the second principle. Rather, the metaphysical question we should be asking concerns mathematics as it is used. That is, in a particular mathematical model or proof, we may be permitted to ask about the metaphysical relation between the various parts of the model or proof and their relations to, for example, the world or the larger theoretical framework. But it does not make sense to ask about the metaphysical status of a piece of mathematics in the abstract. Additionally, it should be clear from the account sketched above that, not only are the epistemologies of "pure" and "applied" mathematics contiguous, but the epistemology of physical concepts should be such that it meshes well with the epistemology of mathematics.

Now, there is a trend in modern physical theory to try and read metaphysics off of the mathematics of a given theory. This is especially common with fundamental physical theory, as seen in the proliferation of interpretations of quantum mechanics and quantum gravity. However, once we lose the mapping intuition as a surefire guide to relations between mathematics and world, we lose any automatic license to read metaphysics off of mathematics in this way. If there is not necessarily a structural relation holding between pieces of mathematics and world, if there is nuanced work being performed by physical concepts under the surface, if there were various social and pragmatic factors playing a significant role in the development of various models, this all suggests extreme caution in developing interpretations of physical theory, and other metaphysical views, based solely on the mathematics.

Rather, the account sketched above suggests that, in order to develop a well-grounded interpretation of a given physical theory, an analysis of the sort described must be performed for the mathematical model of the physical theory. There must be detailed historical analysis tracing the development of the mathematical model and any conceptual, pragmatic, and social influences throughout this process. And a detailed conceptual analysis may reveal important aspects of conceptual infrastructure that block a direct mapping, such as the macro-/micro- distinction in the viscosity case does for Navier-Stokes.

Finally, there is a tendency in modern physical theory to use "mathematical consistency" as a theoretical virtue, that is, as reason to prefer one theory over another. This has often replaced other theoretical virtues such as empirical confirmation in realms such as quantum gravity, where empirical confirmation is hard to come by. However, the account sketched above casts doubt on the notion that mathematical consistency can be used as a theoretical virtue. Treating mathematical consistency as a theoretical virtue relies on the underlying intuition that, if the mathematics were inconsistent, this would suggest that the physical theory is inconsistent. However, this is not necessarily so on the account sketched above.

Since the various pieces of a given mathematical model may have evolved from different origins in different ways and may be recombined in various ways, there is no reason that a successful model should not contain mathematical inconsistencies. These may simply be the result of pragmatic factors' influence in the course of development, such as simplifications and alterations motivated by tractability. Or perhaps some physical concept has several varying usages in different contexts that eventually got compressed into a single model, as happened with the viscosity concept in Navier-Stokes. These are all possible and do not a priori signal that the model under consideration cannot be successful. Rather, they suggest that further analysis is warranted in order to judge the theory's adequacy.

These are just a few of the implications of an account of the sort I have described above. Although I have only given cursory indications of how such an account would support these conclusions, filling out a more detailed description of the account would allow for these philosophical positions to be given solid arguments. I leave such work to future endeavors, though. For now, I maintain the importance of the two principles outlined above; a satisfactory account must be case-specific and must consider mathematics in use. And I hope that my previous analyses support the success of these principles.

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