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A Historical Approach to Understanding Explanatory Proofs Based on Mathematical Practices

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DEDICATION

To Himeko.

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TABLE OF CONTENTS

ABSTRACT

My dissertation focuses on mathematical explanation found in proofs looked at from a historical point of view, while stressing the importance of mathematical practices. Current philosophical theories on explanatory proofs emphasize the structure and content of proofs without any regard to external factors that influence a proof's explanatory power. As a result, the major philosophical views have been shown to be inadequate in capturing general aspects of explanation. I argue that, in addition to form and content, a proof's explanatory power depends on its targeted audience. History is useful here, because from it, we are able to follow the transition from a first-generation proof, which is usually non-explanatory, into its explanatory version. By tracking the similarities and differences between these proofs, we are able to gain a better understanding of what makes a proof explanatory according to mathematicians who have the relevant background to evaluate it as so.

My first chapter discusses why history is important for understanding mathematical practices. I describe two kinds of history: one that presents a narrative of events, which influenced developments in mathematics both directly and indirectly, and another, typically used in mathematical research, which concentrates only on technical developments. I contend that both versions of the past benefit the philosopher. History used in research gives us an idea of what mathematicians desire or find to be important,

while history written by historians shows us what effects these have on mathematical practices.

The next two chapters are about explanatory proofs. My second chapter examines the main theories of mathematical explanation. I argue that these theories are short-sighted as they only consider what appears in a proof without considering the proof's purported audience or background knowledge necessary to understand the proof. In the third chapter, I propose an alternative way of analyzing explanatory proofs. Here, I suggest looking at a theorem's history, which includes its successive proofs, as well as the mathematicians who wrote them. From this, we can better understand how and why mathematicians prove theorems in multiple ways, which depends on the purposes of these theorems.

The last chapter is a case study on the computer proof of the Four Color Theorem by Appel and Haken. Here, I compare and contrast what philosophers and mathematicians have had to say about the proof. I argue that the main philosophical worry regarding the theorem—its unsurveyability—did not make a strong impact on the mathematical community and would have hindered mathematical development in computer-assisted proofs. By studying the history of the theorem, we learn that Appel and Haken relied on the strategy of Kempe's flawed proof from the 1800s (which, obviously, did not involve a computer). Two later proofs, also aided by computer, were developed using similar methods. None of these proofs are explanatory, but not because of their massive lengths. Rather, the methods used in these proofs are a series of calculations that exhaust all possible configurations of maps.

INTRODUCTION

Much of contemporary philosophy of mathematics has been focused on the practices of mathematicians. Contrasted to the traditional philosophies of mathematics of the early to mid-twentieth century, which concentrated on mathematical foundations, ontology, and truth of mathematical propositions, many philosophers now tackle questions concerning what mathematicians do and the historical development of mathematics. This approach requires philosophers to depend on the history of mathematics, because it reflects on why and how mathematics has developed in the way it has. Studying mathematical practices also benefits the philosopher, because the successes of mathematics demonstrate the overall correctness of its practices, thereby suggesting broader epistemological lessons to be learned.

There are still some ties to the traditional philosophical views. For example, the Quine-Putnam Indispensability Argument formulated over forty years ago is still influential today. According to the argument, we are justified in believing in the existence of the mathematical objects found in the parts of mathematics that are used in our best scientific theories. The problem with this view is that it implies that the sciences determine what exists in mathematics. The objects in the applied parts of mathematics exist, while the others have to wait until the sciences have a use for them.[1]

[1] Mathematics is commonly divided into two parts: pure and applied. On the surface, the differences between the two may seem clear—applied mathematics is used outside of mathematics, and pure mathematics is not. However, there are branches of mathematics

However, there is much more to mathematics than its applicability, and most mathematicians are not concerned with how their work is used outside of their discipline. Thus, the indispensability argument ignores mathematical practices. In spite of this problem, philosophers who have written on mathematical practices such as Alan Baker, Mark Colyvan, and Christopher Pincock have their own versions of the indispensability argument to support their views on mathematical realism.

The history of mathematics provides us with a rich source on how mathematicians come to know their subjects and why mathematics has developed in the way it has. When we take historical events into account, we can examine the changes that have occurred within mathematical practices to form a better understanding of what mathematicians do. Philosophers José Ferreirós, Phillip Kitcher, and Penelope Maddy explain that there are different key factors that can change over time, contributing to the development of mathematics. These factors include what language is used, which questions are important, and which methods of reasoning are most salient. Although these philosophers ascribe different degrees of importance to each component, and other factors are also considered, it is generally accepted by them that all three are highly dependent on the history of mathematics.

There are two types of history to consider when thinking about changes in these important factors. The first type is the history that is written by historians. Ideally, sequences of events are presented as accurately as possible, leaving little trace of the present in their narratives. Details external to the technical details of mathematical

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that could belong to both. For instance, harmonic analysis is based on methods from analysis (which tends to be thought of as pure mathematics) but has many applications in the sciences and engineering. The famous number theorist, G. H. Hardy, praised his field as the purest branch of mathematics, but it has many applications in cryptology.

development are vital to understanding how a piece of mathematics came about. Such details include the backgrounds of mathematicians, the locations where developments took place, as well as major concurrent events. The second type of history is the one typically used in mathematical research. It focuses more on the progress of a piece of mathematics. The technical developments are key here, while there is no use for any external details. This type of history tends to be Whiggish; only major developments are highlighted. Developments are presented as if they are compatible with our present mathematics—no changes in language or methods are addressed. Although history used for research purposes is not an accurate presentation of past events, it focuses on the relevant technical details the mathematician uses in her research.

The philosopher benefits from knowing both types of history. The history used in mathematical research reflects the "final products" of mathematical developments. These are papers found in journals, reference books, and other sources that represent polished versions of mathematics. From these sources, philosophers can gain an understanding of what mathematicians consider important in their research. History as told by historians provides the details of what actually occurred; it gives us a narrative of how mathematicians developed their discipline through the twists and turns of the past that are usually ignored in the history used in mathematical research.

Since it is a very difficult (if not impossible) task to write on mathematics and its history in general, I will focus on mathematical proof. Specifically, I will investigate what makes a proof explanatory to mathematicians. A proof is thought to be explanatory if it answers why its corresponding theorem is true, as opposed to only providing justification that it is true. There has been recent philosophical discussion on explanatory proofs. However, it has mostly concentrated on the contents and forms of such proofs without much regard to history, although examples from the distant past are chosen above contemporary ones. Unfortunately, assuming that explanation depends only on the form or content of a proof leaves out the proof's audience. What may be explanatory to one audience may be confusing for another. This can easily be imagined: a research mathematician may find a given proof to be explanatory though it remains very difficult to follow for a first-year undergraduate student. Although philosophical theories on explanation are intended to reflect the practices of research mathematicians, philosophers have chosen case studies that are very simple to follow. This has the consequence of making it seem as if an explanatory proof is accessible to everyone, which is hardly the case.

A different strategy for understanding what makes a proof explanatory is to use the two types of history described above to explore what proofs mathematicians judge to be explanatory and why. Looking at mathematics' past, we are able to determine what mathematicians favor in explanatory proofs. However, this is not an easy task, because mathematicians seldom offer commentary on proofs. I suggest that we study a theorem's multiple proofs starting from its first generation proof to either its textbook presentation or latest generation proof (provided that it is explanatory). By doing so, we are able to track the differences in language, method, and style of proof. These differences are crucial to understanding how mathematicians improve on older proofs. Additionally, it is helpful to know about the mathematicians behind these proofs. Their backgrounds and styles of proving aid us in recognizing what they used the corresponding theorems for and why they proved them in the way they did. I believe that this strategy of looking at multiple proofs for one theorem will illuminate what mathematicians mean when they say that a proof is explanatory and will be more in line with mathematical practices than current philosophical theories.

In line with recent trends, the goal of my dissertation is to emphasize the importance of history and mathematical practices for the philosophy of mathematics. Without these, philosophy of mathematics becomes disconnected with mathematics—the very subject it addresses. But despite increased attention to history and practice, recent work in the philosophy of mathematics sometimes still ignores history and practice in favor of viewing mathematics as a static discipline, especially when addressing specific issues such as explanations found in proofs.

The first chapter of my dissertation discusses the importance of the two types of history described above: history written by historians and history used in mathematical research. While it is not possible to have a "pure" history of mathematics, which is written without any trace of the present, we can still gain insight that is missed in the version of history used by mathematicians. Here, I argue that philosophers should focus on both versions of the past. History used in mathematical research shows what mathematicians desire or find to be important, while history written by historians shows us the evolution of mathematical practices.

Philosophical views that ignore history and mathematical practices have hindered mathematics itself. For example, I present the case of the Axiom of Choice, which initially pitted the mathematical realists against the constructivists at the beginning of the twentieth century.[2] While the realists accepted the axiom, the constructivists rejected it. The main difference between the two was over the definition

[2] The Axiom of Choice states that for any non-empty set, one can choose an object from each of its disjoint nonempty subsets.

of the “existence” of a rule: for the constructivist, a rule exists only if it can be uniquely described; whereas for the realist, the rule does not have to be explicitly stated. The axiom’s eventual acceptance was motivated by specific mathematical objectives; while its delay was due to the constructivists’ worries over what counts as existence.

The second and third chapters cover explanatory proofs. Chapter two is an overview of philosophical theories of explanations found in proofs and their connection to mathematical practices. Here, I show that existing theories of explanation have centered more on form and content of proof rather than the communicative and pedagogical practices of mathematicians. In addition to content and form, whether or not a proof is explanatory depends on its audience. Received accounts do not address how such proofs are understood by a particular audience in possession of the required knowledge to appreciate their explanatory value, making it seem as though they provide explanations to everyone, regardless of mathematical ability. This problem arises because current philosophical theories of explanatory proofs are based on examples presented in elementary textbooks that usually target a level of explanation that is not necessary in research mathematics.

In the third chapter, in order to consider what qualifies as an explanatory proof for research mathematicians, I suggest studying the evolution of a theorem's successive proofs, starting from its first-generation proof—one that often lacks explanatory power—to its recent textbook presentation—one that fills in missing details of previous proofs and provides explanation. Such accounts show how mathematicians were able to prove the theorem using results that were available at the time, keeping in mind that changes in terminology, which have been overlooked in the philosophical literature, also play a role in what is assumed in a proof. I argue that in addition to the contents of the proofs themselves, other factors such as the time period of each proof, the parts of mathematics they originated from, and the intentions of particular mathematicians lend insight to what makes a proof explanatory to a particular audience. The abilities of the audience also play a role in whether a proof is explanatory or not; a mathematician who does not specialize in a certain field may regard a proof from that field as non-explanatory even if those who specialize in the field deem the proof explanatory. As a case study, I use four proofs of Lewy's Theorem from harmonic mapping theory. By tracking the changes and differences between the proofs of Lewy's Theorem, I observe how later proofs fill in the gaps of previous proofs, survey the different ways of showing particular steps of the proofs, and note how mathematicians use methods from different branches of mathematics—namely differential geometry, partial differential equations, and complex analysis—to prove the theorem.

The final chapter is a case study of the proof of the Four Color Theorem. The Four Color Theorem was first proved in 1976 by Kenneth Appel and Wolfgang Haken. The significance of their proof is that it is the first major computer proof. According to both mathematicians and philosophers, their proof lacks explanatory power. However, they give different reasons for this assessment. Here, I compare and contrast what philosophers and mathematicians have had to say about the proof. I argue that the main philosophical worry regarding the theorem—its unsurveyability—did not make a strong impact on the mathematical community and would have hindered mathematical development in computer-assisted proofs. By contrast, the main mathematical worry was that Appel and Haken's proof does not explain why four colors are sufficient for coloring maps, because it only demonstrates that out of all possible configurations, there are no maps that require at least five colors. Mathematicians, who were familiar with the proof, judged its explanatory power based on the methods used rather than asserted that explanatory power is affected by the proof's length. This example illustrates the value of mathematical practices to the philosophical examination of explanatory proofs.

CHAPTER ONE:

THE IMPORTANCE OF HISTORY

In order to understand mathematical practices, philosophers need to pay attention to the history of mathematics. While contemporary philosophers do stress this need, it is a challenging task, because what counts as history varies depending on the purpose for which this history is used. According to some historians, the goal of history is ideally to show how the past is different from the present. The historian starts from a point in time and moves forward to explain what occurred in the past, avoiding any trace of the present. Events that impact mathematics both directly and indirectly are referenced. However, writing history from the point of view of the past is only an ideal, because the historian is only able to view the past from the standpoint of her present. By contrast, history specifically geared toward mathematical research aims to show how similar the past is to the present. This type of history tends to focus on mathematical developments while omitting external factors that do not have a direct impact on mathematics. Here, modern concepts are used to help explain what mathematicians have developed in the past, as anachronisms here are acceptable and encouraged. Although these historiographical methods are at odds with each other, both of them are important for philosophers: accounts of actual past events shed light on how mathematics has developed, and history that is more useful for mathematical research helps the philosopher understand the reasoning behind how and why mathematicians use past results in the way they do.

In this chapter, I will investigate how these two methods of historiography differ from one another and their importance to historians and mathematicians alike. Next, I will argue that most philosophers use one approach to history—the approach to history used in mathematical research—while ignoring the other and that doing so misses certain aspects of mathematical practices. I illustrate this point using the case of the axiom of choice as an example. Last, I will highlight the benefit of considering both to gain a better understanding of mathematical practices. In general, since these different types of history do not fully coincide with each other, it is helpful to use the history used in mathematical research to understand what mathematicians desire out of their work; while using history that goes beyond the technical details of the past to compare actual development with how mathematicians view their discipline.

I. History as Understood by Historians

Although historians disagree on specific details, they have compared and contrasted, in general, two ways of writing the history of mathematics.[3] The first way is to describe past events as accurately as possible through the perspective of the people of the time period. Here, later periods and the present play no role in the explanation of earlier events, because this type of history emphasizes that the present is different from its past. Anachronisms are to be avoided because they work present concepts into past events. These anachronisms would not have made much sense to anyone in the past, so

[3] This is not to claim that there is only one difference, but it is how many historians have divided types of historiography.

they have no purpose here. History written in this way is the study of how a concept developed through time, including both successes and failures, periods of stagnation, and repeated results. Additionally, cultural and social details play a role to help explain the development of mathematics outside of the technical details.

In contrast to this way of writing history, the second version of history is written from the point of view of the present, which is regarded as superior to its past. As such, anachronisms are encouraged, because modern ideas are written into the past to highlight how they came about. External factors outside of mathematical research are excluded, because they are tangential to the technical details found in mathematical research. Similarly, events that do not contribute towards successes tend to get left out, because they get in the way of the overall narrative of progress, which only allows for successful developments and improvements. Usually, the mathematics here is polished. For instance, a mature version of a theorem's proof—one that is best suited to the purposes of mathematical research—will be presented while other versions are ignored. This type of history is commonly described as being "Whiggish" or "present-centered," and has been criticized for presenting inaccurate accounts of the past.

Grattan-Guinness sums up these two methods of historiography neatly. According to him, the first type of history provides the answer to "what happened in the past?" and describes the past as a chronological sequence of events without assuming anything from the present.[4] The second type, which he calls "heritage," "addresses the question 'how did we get here?'"[5] These are two very different questions, but are related through their reference to past events. History and heritage, for Grattan-Guinness, are

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[4] Grattan-Guinness, Ivor. "The Mathematics of the Past: Distinguishing Its History from Our Heritage." *Historia Mathematica*. 31 (2004): 16, 164.
[5] Ibid., 165.

equally acceptable projects.[6] While historians accept their discipline, he acknowledges that heritage is useful for research mathematicians, whose work depends on a condensed version of major developments that directly have an impact on research. Grattan-Guinness warns that the two must not be conflated, because they produce different interpretations of a *notion*.[7] Mistaking heritage for history, he points out, creates a misleading narrative of past events of a notion. For example, we would be led to believe that the sequence of events was determinate, and every event happened specifically for the present notion to come about.[8] Instead, Grattan-Guinness emphasizes that things could have been different—mathematicians in the past could not have anticipated how their work would transform into current notions. Using history as heritage is also problematic. Although Grattan-Guinness does not address this, we can recognize that what should be important through a heritage point of view may be taken as insignificant if historical facts muddle its value.[9] History runs the risk of being too detailed—it is possible to understate major developments as mere events in a sequence of productions.

In describing the differences between these two type of history, Grattan-Guinness and other historians, such as Andrew Cunningham claim that history that attempts to

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[6] Not every historian would agree with Grattan-Guinness. There are many who believe that heritage is to be avoided at all costs. Herbert Butterfield and Andrew Cunningham, for instance, both dismiss it as being inaccurate and harmful to the practice of history. See Butterfield, Herbert. *The Whig Interpretation of History*. London: G. Bell and Sons, 1959. Cunningham, Andrew. "Getting the Game Right: Some Plain Words on the Identity and Invention of Science." *Studies in History and Philosophy of Science Part A* 19.3 (1988): 365-389.
[7] Grattan-Guinness uses 'notion' as a blanket term for a theory, definition, notation, method, or a branch of mathematics (164).
[8] Grattan-Guinness, 171.
[9] For instance, the negative reception immediately following the publication of Gottlob Frege's *Begriffsschrift* in 1879 would make it seem as if his work was not influential; however, this is certainly not the case. For a close examination of the reaction to the *Begriffsschrift*, see Vilkko, Risto. "The Reception of Frege's *Begriffsschrift*." *Historia Mathematica*. 25 (1998): 412-422. Risto argues that despite the poor reviews Frege received immediately after the publication of the *Begriffsschrift*, they were "poorly motivated" rather than "unfair and hostile" (415).

accurately recreate the past attends to both successes and failures in mathematics and the sciences; while history that is strictly used in mathematical and scientific research focuses solely on successes, because they believe that failures only hinder the narrative of progress.[10] This may be true for Whig history which Butterfield describes as one that "praise[s] revolutions provided they have been successful, to emphasize certain principles of progress in the past and to produce a story which is the ratification if not the glorification of the present."[11] However, addressing failures does benefit mathematicians and scientists, because they contribute toward developments in ways that successes do not. For instance, in mathematical research, it can help the mathematician understand the limitations and restrictions of her results, or in extreme cases, overturn a theory completely.[12]

While the differences between history as written from a past point of view and one written from a present point of view are laid out neatly, some historians have challenged the claim that history can be written as if the historian lives in the past. For example, Adrian Wilson and T. G. Ashplant argue that history in general is "constrained by the perceptual and conceptual categories of the present, bound within the framework of the present, deploying a perceptual 'set' derived from the present."[13] The historian

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[10] Grattan-Guinness , 168. Cunningham, 368. Fried, Michael N. "The Discipline of History and the 'Modern Consensus in the Historiography of Mathematics'. *Journal of Humanistic Mathematics* 4.2 (2014): 127.
[11] Butterfield, v.
[12] For example, in the first half of the nineteenth century, mathematicians, such as André-Marie Ampère, Augustin-Louis Cauchy, and Sylvestre Lacroix, believed that a continuous function is always differentiable. It was not until 1872 that this was proven to be false by Karl Weierstrass. The problem was that there was no general definition of function at the time. Weierstrass and some of his contemporary mathematicians aimed to make analysis more rigorous to avoid similar mistakes made by their predecessors. See Gray, Jeremy. *The Real and the Complex: a History of Analysis in the 19th Century*. Cham, Heidelberg, New York, Dordrecht, London: Springer (2015).
[13] Wilson, Adrian, and T. G. Ashplant. "Whig History and Present-Centred History." *The Historical Journal* 31.01 (1988): 11.
Wilson and Ashplant call sources and evidence 'relics'.

must interpret her finding based on a presupposed framework, which "involves the making of assumptions, the using of words, the posing of questions."[14] Wilson and Ashplant point out that the challenge for the historian is to ensure that her framework is indeed appropriate to her research, because it is important for the historian to accurately understand how notional relics were used by their original creators. Additionally, the historian is present-centered even with her choice of relics and while she determines how they fit into her framework. If the historian misinterprets her relics, then she will give an inaccurate representation of the past: either she will try to force her finding to fit into her framework, or if she cannot find anything that agrees with this framework, she will make it seem as if this absence was as important in the past as it is for the contemporary historian.[15] This can also be applied to the omitting of certain details of the past if they are believed to be irrelevant to the historian. It is up to the historian to determine what and why certain relics are important, but these decisions are made from her viewpoint, which is inevitably in the present. Thus, as Wilson and Ashplant explain, there is no escaping present-centeredness as a result.

Nick Jardine agrees with Wilson and Ashplant, but he sets up his argument differently. Taking from anthropology and linguistics, he distinguishes emics from etics. According to Jardine, emics applied to history is described as understanding the past without any influence from the historian. Here, the past is seen through an "insider"

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[14] Ibid.
A. Rupert Hall similarly argues that it is impossible to write history in the way that Grattan-Guinness describes. According to Hall, the historian has to have a structure that helps her determine what is relevant to the particular topic she is writing on; otherwise, she would only have a collection of random data, because being able to piece together her findings into a coherent structure requires hindsight. See Hall, A. Rupert. "On Whiggism." *History of Science* 21.1 (1983): 52.
[15] Wilson and Ashplant., 15.

point of view in which the historian places herself in the past.[16] He describes etics as viewing the past as an observer who lives in the present and is able to know what happens in the time between the time period she is researching and the present.[17] Jardine argues that history should be written using a combination of emics and etics. Emics without etics ignores present knowledge, which could help explain why or how something occurred in the past; while etics without emics risks misinterpreting the historical figures involved.[18] The extreme case of this is Whig history, in which only events that contribute to the narrative of progress are studied. Additionally, Jardine argues that the historian has a different cultural background and language, so when explaining the past, she uses present language and is able to point out cultural differences to communicate her findings.[19] Siding with Jardine, David Alvargonzalez points out, "[A]ny informed analyst cannot honestly ignore the zero or decimal notation when tackling Greek numerals. Conic equations come part and parcel with any modern understanding of Apollonius..."[20] Therefore, in agreement with Wilson and Ashplant, present-centeredness is unavoidable when writing history.

Historians also convey the past to a contemporary (or future) audience. Much like for the historian to understand her findings, this requires that she uses a language that her audience can also understand, which does not always coincide with the language of the past. As an example, the Ancient Greeks did not have zero-placeholders for numbers such as 10, 100, and 1,000. Thus, performing arithmetic operations on

[16] Jardine, Nick. "Etics and Emics (not to Mention Anemics and Emetics) in the History of the Sciences." *History of Science* 42.3 (2004): 268.
[17] Ibid., 270.
[18] Ibid., 275.
[19] Ibid., 273.
[20] Alvargonzález, David. "Is the History of Science Essentially Whiggish?" *History of Science* 51.1 (2013): 89.

Greek numerals was a lot more involved than decimal arithmetic today. However, it is the responsibility of the historian to disclose that the language she uses is not the same as the past, but is used so that her audience can understand the mathematics of the past. Otherwise, she misleads her audience into thinking that the mathematics of the present is the same as it was in the past.[21]

Although a "pure" history of mathematics—one that has no trace of the present—is unattainable, historians can and do strive to write history as accurately as possible. However, in addition to living in the present, they are also writing for an audience who also live in the present. In order to make sense of the past, it must be intelligible to us in the present. This goes for both the historians and their audiences. Even claiming that the historian's goal is to show that the past is different from the present requires the historian to understand the past from her present point of view to make the comparison.

II. History Used in Mathematical Research

There are mathematicians who believe that there is no need to understand their work through a historian's point of view. This does not mean that mathematicians do not pay any attention to historical details,[22] but they do not find much value in considering, say, social or cultural factors that have helped guide past developments in mathematics. This way of thinking has had the unfortunate consequence of mathematicians ignoring the work of historians.

For example, the mathematician André Weil, one of the major figures of the Bourbaki group, believes that a genuine historical account of mathematical development

[21] Doing so comes with the consequence of making mathematics seem static and permanent.
[22] It is not possible to completely avoid the past while doing research in mathematics.

is not necessary for mathematical progress, but finds it useful for merely breathing life into mathematical notions through non-mathematical aspects revealed by examining the social and cultural atmosphere of the period. Otherwise, Weil favors an anachronistic version of history, because he finds that this type of history helps mathematicians understand the past in terms of present mathematical language. For example, when he describes Euclid's work as geometric algebra:

> [W]hen quadratic equations, solved algebraically in cuneiform texts, surface again in Euclid, dressed up in geometric garb without any geometric motivation at all, the mathematician will find it appropriate to describe the latter treatment as 'geometric algebra' and will be inclined to assume some connection with Babylon, even in the absence of any concrete 'historical' evidence. [23]

Here, Weil explains that Euclid was able to solve quadratic equations first posed by the Babylonians using geometric methods, and he forces a connection with the Babylonians without regard to how actual events played out.[24]

In a similar vein, Richard Askey believes that all history of mathematics should be written for mathematicians.[25] According to him, historians of mathematics need to

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[23] Weil, André. "History of Mathematics: Why and How." O. Lehto (Ed.), *Proc. International Congress of Mathematicians*, Helsinki 1978, vol. 1, Academia Scientarum Fennica, Helsinki (1980): 204.
[24] This is historically inaccurate for multiple reasons. First of all, there were no 'equations' during the Babylonian time period. Instead, Babylonian mathematicians developed an algorithm that would later give rise to the quadratic equation. Second, there was no such thing as 'algebra' for the Babylonians either; algebra did not exist until Diophantus hit the scene, which was after Euclid's death. So, it is not possible that Euclid was able to disguise his work as geometry as Weil claims. See Unguru, Sabetai. "On the Need to Rewrite the History of Greek Mathematics." *Archive for History of Exact Sciences* 15.1 (1975): 67-114.
Even though Weil read Unguru's essay, he did not consider these facts to be important for mathematicians. Instead, he argues that knowing modern mathematics is needed to understand Euclid's *Elements*, because the purpose is to understand the mathematics in the *Elements*, which does not require someone to understand it in the same way as Euclid presents it. See Weil, Andre. "Who Betrayed Euclid? (Extract from a letter to the Editor)." *Archive for History of Exact Sciences*. 19.2 (1978): 91-93.

have an understanding of mathematics which is on par with professional mathematicians, or else developments that are important to mathematicians may be downplayed or overlooked. Askey uses Morris Kline's work as an example of history skipping over important details. He criticizes Kline for glossing over important mathematical developments in differential equations and uses it to accuse historians of only concentrating on well-known results instead of essential ones that would later further mathematical progress.[26] Another example Askey gives is his own experience. He wrote a short paper on the developments that came out of a specific hypergeometric identity[27] and tried to publish it in *Historia Mathematica*, but was rejected because it was not history by the editor's standards; this paper reads more like a bibliography ordered by date rather than a narrative of events. This is the type of work Askey says should be included as history of mathematics, because, he claims, it is what mathematicians are interested in.[28] However, it is doubtful that any historian would consider it to be history, because it is just a listing of facts.

For mathematicians, history of mathematics emphasizes the development of mathematical notions that are internal to mathematics while ignoring external factors, such as cultural influences. These past notions are explained in terms of present notions in order to explain what they were and how they were used. The goal is to show how modern notions came about from the past, so it is acceptable to use anachronisms, especially so that the audience of these types of narratives can understand the concepts

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[25] Askey, Richard. "How can Mathematicians and Mathematical Historians Help Each Other." *History and Philosophy of modern mathematics (W. Aspray and P. Kitcher, eds.), University of Minnesota Press* (1988): 203.
[26] It is unfortunate of Askey to choose Kline's work as an example, because Kline is a mathematician.
[27] Askey, Richard. "A Note of the History of Series." *Mathematical Research Center Technical Report*. 1532. University of Wisconsin, Madison, 1975.
[28] Askey, "How can Mathematicians and Mathematical Historians Help Each Other," 214.

in terms of what we have today. This type of historiography is useful for mathematical research, because it gives a condensed version of the past for a notion, so that the mathematician is able to understand the past evolution for it.

III. How Philosophers Can Use History

The questions of what should count as history and for what purpose thus remain contentious. While modern-day historians deal with past events to understand the sequence of developments that were made to produce the mathematics we have today, mathematicians favor a more technical point of view, focusing on polished versions of major results, to help with their research. This greatly affects philosophy of mathematics. On the one hand, history that documents events chronologically while avoiding anachronisms as much as possible is important, because it provides a narrative of developments that were influenced by mathematical research, the mathematicians themselves, and by external factors outside of mathematics. Without knowing about past events, certain developments would seem mysterious or out of place.[29] For example, taking the view that mathematics is timeless ignores the changes in terminology and failures that have occurred throughout the history of mathematics and makes it seem as if every mathematical result accepted in the past is still accepted today.[30] This is clearly not the case, though without knowledge of the past, this fact is easily overlooked. On the other hand, philosophers must also have a grasp of the history

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[29] For example, Thomas Tymoczko argues that the proof of the Four Color Theorem is the first empirical mathematical proof, because it was generated by a computer, which produced lengthy results that are too long to be checked by a human mathematician. There are two problems with Tymoczko's claim. First a historical one: there have been previous computer proofs dating a decade before the Four Color proof. Second, although a computer was used for the proof, mathematicians have used other tools in the past to aid with calculations, diagrams, and so on. See Tymoczko, Thomas. "The Four-Color Problem and its Philosophical Significance." *The Journal of Philosophy* 76.2 (1979): 57-83.

[30] If this were so, then non-Euclidean geometry would not exist.

mathematicians use for research, because, generally, this is what mathematicians deem as important for their work. If it were not for mathematicians producing results based on a more technical point of view, mathematics would not develop in the way it has. So, when philosophers investigate mathematical practices, they must recognize that the actual development of theories, concepts and so on do not always match up with the history that guides mathematical research. Grattan-Guinness points out that history is made up of successive heritages:[31] "[t]he historian records developments and events where normally an historical figure inherited knowledge from the past in order to make his own contributions heritage style."[32] Here, the "historical figure" can either be a mathematician or historian who uses a heritage point of view to express past events. It is vital for the philosopher to be aware that heritages have guided mathematicians throughout history, and thus are also part of history as well, even though they may not match up to actual events.

Throughout the history of philosophy of mathematics, there have been different levels of importance placed on mathematical developments, ranging from placing no importance on past events to considering the mathematical details found in the type of history mathematicians prefer. History that goes beyond what is used in mathematical research, however, is rarely used in philosophical discussions of mathematical practices. An extreme instance are the logical positivists, who in the first half of the twentieth century has stripped mathematics of its past, and instead argued that mathematics must be considered atemporally as a set of axioms and deductive rules of inference. This became a widely prevalent view, which has vestiges that are still influential to

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[31] Recall that heritage is the label that Grattan-Guinness gives to research-oriented history.
[32] Grattan-Guinness, 168.

philosophers and mathematicians today.[33] Short episodes of history have been used to put forth philosophical views, but they are usually used as examples to bolster an argument while ignoring other contradicting occurrences in mathematics.[34] When the past is used in this way, it can be manipulated to fit with the expounded philosophical position. There has been an increased interest in mathematical practices, which looks into why mathematics has developed in the way it has.[35] So far, this has involved mostly a research-oriented history that mathematicians rely on rather than the kind of history recommended by historians. This runs the risk of presenting a superficial view of mathematical practices as it skips over some of the salient features of mathematical development. What I would like to argue is that we must be able to consider both the history written by historians and the history used in mathematical research of a notion to see how it fits into mathematical practices. When we compare these two histories, we are able to see that certain views stemming from the history used in mathematical research are influenced by philosophical positions; while history that includes more than the technical details of mathematical development shows us the effects of these positions. Ideally, philosophy should concentrate on the history used in mathematical research, but at the same time, compare it to actual events, because these events reflect mathematical practices, which are influenced by the history used in mathematical research.

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[33] For example, Christopher Pincock investigates the different roles mathematics has to the sciences. To bolster his arguments, he relies on basic examples, while completely ignoring both the history and heritage of mathematical research. As a result, he argues that mathematics, much like any tool of the scientist, contributes toward the successes of the sciences as if all of the mathematics we have today was at the disposal of the scientists who developed the theories Pincock refers to. See Pincock, Christopher. *Mathematics and Scientific Representation*. Oxford University Press, 2011.

[34] In discussing what makes a mathematical proof explanatory, Mark Steiner chooses specific examples that support his theory. However, his theory also allows mathematical inductive proofs, which are considered by the mathematical community to be non-explanatory. See Steiner, Mark. "Mathematical Explanation." *Philosophical Studies* 34.2 (1978): 135-151.

[35] These are philosophers such as Mancosu, Ferreiros, Lange, etc.

IV: Example—The Axiom of Choice

An interesting example in which philosophically-minded mathematicians failed to recognize the importance of either type of history is the case of the axiom of choice, which states that for any non-empty set, one can choose an object from each of its nonempty subsets. The history of this axiom, which spans almost sixty years from its initial formulation to its general acceptance, shows that acceptance did not come very easily. This, in part, has to do with mathematicians' failure to recognize its importance in past mathematical works prior to Ernst Zermelo's explicit formulation of the choice principle (which later becomes the axiom of choice in 1908) in his 1904 paper, "Proof that Every Set Can be Well-Ordered." In this paper, Zermelo states his controversial principle: "[W]ith every subset M' [of a set M], there is associated an arbitrary element m_1' that occurs in M' itself; let m_1' be called the 'distinguished' element of M'."[36] He uses this to claim the existence of a function γ: S → M, such that γ(M') is the distinguished element of M', and S contains all non-empty subsets M' of M. This function is now known as the "choice function," but, at the time, Zermelo called it a "covering." At the end of this paper, anticipating objections to this principle, he writes, "[t]his logical principle cannot, to be sure, be reduced to a still simpler one, but it is applied without hesitation everywhere in mathematical deduction."[37]

The central objection to the choice principle was put forth in a series of five letters between René Baire, Émile Borel, Henri Lebesgue, and Jacques Hadamard. Baire, Borel, and Lebesgue believed that in order to make use of Zermelo's choice function, it must be defined by a rule. Lebesgue writes that "it is impossible to

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[36] Zermelo, Ernst. "Proof that Every Set Can Be Well-Ordered." *Ernst Zermelo: Collected Works = Gesammelte Werke*. Edited by Heinz-Dieter Ebbinghaus and Akihiro Kanamori. Berlin: Springer-Verlag (2010): 115, 117.
[37] Ibid., 119.

demonstrate the existence of an object without defining it."[38] Although when the set M is finite, it is obvious that an element from each of its subsets can be selected, when M is infinite, however, they argue that the choice function must be defined by a rule. For example, Lebesgue writes that since it is not possible to know the distinguished element m' of M', we cannot be sure that we are able to choose each m' without a rule.[39] Specifically, he is worried that we have no way of knowing that the same choice function is always used for each M'. For Lebesgue, as well as Borel and Baire, choosing the distinguished elements is the same as being able to name them.[40] For sets of infinity, this is impossible. Thus, a rule is necessary to define each of the elements.

However, all three mathematicians have relied on the existence of functions without uniquely defining them in their own work. In a letter from Jacques Hadamard to Borel written after Borel wrote an article rejecting Zermelo's proof, Hadamard points out that he only proved existence for functions without uniquely defining them in his work on analytic continuation. Baire was also guilty of using the choice principle implicitly in his work on categorizing real functions (which is now known as the Baire Category Theorem). Lebesgue used the principle indirectly multiple times in his work on measure theory.[41] So, on the one hand, these three mathematicians rejected Zermelo's choice principle because it did not define a rule, but on the other hand, they were unaware that they relied on it in their own research.

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[38] Moore, 314.
[39] Ibid., 316.
[40] Ibid., 315.
[41] Baire, Borel, and Lebesgue continued to unknowingly rely on the choice axiom in their work after Zermelo's 1904 paper. For a detailed summary of their implicit uses of the axiom, see Moore, Gregory H. "Lebesgue's Measure Problem and Zermelo's Axiom of Choice: The Mathematical Effects of a Philosophical Dispute." *Annals of the New York Academy of Sciences* 412.1 (1983): 129-154.

In defense of the principle, Jacques Hadamard maintains that it is not necessary to define a rule to show that it exists. According to him, the requirement for a rule is not a mathematical one, but is psychological instead.[42] Existence for Hadamard "is a fact like any other, or else it does not occur."[43] He also brings up Lebesgue's question about whether any set can be well-ordered. According to Hadamard, what Lebesgue, Baire, and Borel mean by this question is "Can *one* well-order a set?"[44] However, Hadamard regards the question as merely asking whether it is possible. Asking if one is able to well-order a set Hadamard dismisses as subjective, because it would depend on who the "one" refers to.

Zermelo and other supporters of the Axiom of Choice went to great lengths to show that it is a necessary tool in mathematics. In response to his critics, in 1908, Zermelo emphasized that it is not possible to prove his principle, because it is logically independent of other given axioms.[45] Furthermore, he argued that it has always been used in past mathematical proofs, and that no one had ever showed that it leads to contradictions.[46] Zermelo explains that prior to his choice principle, mathematicians have either always relied on it although they unknowingly used it, or he knew of no

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[42] It should be noted that before Zermelo's paper, a variation of the choice principle was recognized by Guiseppe Peano and Rodolfo Bettazzi. In 1890, Peano wrote a paper on differential equations in which he mentions that it is not permissible to use an arbitrary rule an infinite number of times to choose one element each from many classes. In order to avoid making arbitrary choices in this paper, he states a definite rule to solve this problem. Rodolfo Bettazzi, in a paper from 1892 on discontinuous functions, also rejected infinitely arbitrarily many choices. He claimed that choosing infinitely many objects arbitrarily is the same as defining them one by one, which is impossible. Bettazzi went on to say that one can do this for the finite case as long as there is a rule which picks out the objects.
Later on in 1906, Peano replied to Zermelo's paper objecting to his use of the choice principle for the same reasons, which also are in line with the rejection from Baire, Borel and Lebesgue. Although Peano acknowledged that sometimes the choice principle is necessary to produce correct mathematical results, he rejected it based on its lack of rule.
[43] Ibid., 317.
[44] Ibid., 318.
[45] Zermelo, Ernst. "A New Proof of the Possibility of Well-Ordering." *Ernst Zermelo: Collected Works = Gesammelte Werke*. Edited by Heinz-Dieter Ebbinghaus and Akihiro Kanamori. Berlin: Springer-Verlag (2010): 129, 131.
[46] Ibid., 131.

other proof that did not use it implicitly. He presents seven examples that are previous, well-known results found in set theory and analysis dating from the 1880s through the 1900s that implicitly uses the choice principle.

One of these examples, the proof of a theorem of Dedekind's, which states that "a set not equivalent to any segment of his 'number sequence' must have a component equivalent to the entire number sequence,"[47] requires the choice principle. This theorem is found in Dedekind's 1888 book, *What are Numbers and What Should They Be?*, which was dedicated to the theory of finite sets. It is in this book that the differences between finite and infinite sets are brought to light. Generally, Dedekind required that arbitrary choices from subsets of infinite sets be chosen to show that every infinite set that is equivalent to one of its proper subsets has a denumerable subset.

Another example Zermelo offers is what we now call the Partition Principle: "If a set M can be decomposed into disjoint parts, A, B, C..., the set of these parts is equivalent to a subset of M, or, in other words, the set of summands always has a cardinality lower than, or the same as, that of the sum."[48] In order to prove this, Zermelo points out that "we must mentally associate with each of these parts one of its elements."[49] He attributes this principle to Beppo Levi, who states it in a paper critiquing a theorem of Felix Bernstein. Although Zermelo pushes the fact that this involves the choice principle, it turns out that Levi rejected having to choose arbitrary elements from an infinite number of sets.

Although there was hesitation to accept the axiom of choice, it was still used by many mathematicians to produce further mathematical results. In addition to set

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[47] Zermelo, 133
[48] Zermelo, 131.
[49] Ibid.

theory, analysis, and algebra, the axiom was also used in other branches of mathematics.[50] For example, important theorems on compactness and convergence in topology relied on the axiom. As one of its consequences, Tychonoff's Compactness Theorem was shown in 1950 to be equivalent to the Axiom of Choice by John Kelley. It was not until 1937 when Kurt Gödel proved that the axiom of choice is consistent within the common system of axioms (now the Zermelo-Fraenkel axioms) in set theory that the opposition backed down. Later in 1963, Paul Cohen proved that the axiom is independent of Zermelo-Fraenkel set theory. These important results helped secure Zermelo's axiom of choice and his Well-Ordering Theorem.

Mathematicians who rejected the axiom of choice avoided it as much as possible in their future work. However, they were not always successful in their avoidance, as the axiom continued to be crucial for their results. As more and more mathematicians relied on the axiom of choice, it gradually became accepted as a standard axiom in set theory, and over time, mathematicians working in various branches of mathematics found the axiom to be an indispensible tool. As Zermelo explained in his 1908 paper, the axiom is necessary for results in various branches of mathematics, which is why we must accept it. Although some mathematicians still rejected the axiom of choice after his paper, it turns out that Zermelo's position was correct.

It took almost sixty years from when the axiom of choice was formulated until it was generally accepted by mathematicians. In hindsight, one could say that the mathematicians' indifference to history played a part in its delayed acceptance: despite the efforts of Zermelo showing that the axiom is necessary by referring to past

[50] For a survey of propositions from various parts of mathematics that are equivalent to the axiom of choice, see Rubin, Herman, and Jean E. Rubin. *Equivalents of the Axiom of Choice, II*. Vol. 116. Elsevier, 1985.

mathematical results throughout history, his opponents rejected it based on what they believed was the "correct" way of doing mathematics: construction of a rule is the only way to demonstrate the existence of a mathematical object.[51] It is clear that the objections from Baire, Borel, and Lebesgue do not match up with actual mathematical practices. Instead, if mathematics was developed based on their constructivist view that existence of a rule is obtained through description, then much of mathematics would have to be discarded. Their disregard for historical evidence blinded them to the importance of the axiom of choice. Furthermore, they were also guilty of implicitly using the axiom in the own research.

The example of the history of the axiom of choice illustrates the troubles that can occur when there is no regard to history or even mathematical practices. Although the supporters of the axiom tried to persuade their opponents that the choice axiom is necessary using past mathematical results, rejection based on the nonconstructive nature of it remained. In the cases of Borel and Lebesgue, they were unable to realize that even they themselves were using the axiom for their work.[52]

Recall that according to Grattan-Guinness, the aim of history is to show how different the past is from the present. In arguing for the necessity of the choice principle, Zermelo points out various cases in the past of when the principle was used implicitly. Until the choice principle was stated by Zermelo, mathematicians unknowingly relied on the principle for their work. The difference between the cases Zermelo writes about in his 1908 paper and during his time is that mathematicians were now cautious and reluctant to use it. Zermelo's efforts are indeed counted as history used for

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[51] Ernst Steinitz and Wacław Sierpiński also echoed Zermelo's insistence that the axiom of choice is necessary for mathematical developments; however, they avoided it as much as possible. See Moore, 171-175, 197-208.
[52] Moore, 98, 103.

mathematical research, as his work calls attention to the many implicit uses in past mathematical results. It is through hindsight that Zermelo is able to recognize that there have been implicit uses of his axiom.[53] However, through history, we recognize that this was not enough for certain mathematicians to accept the axiom.

Based on their views, Baire, Borel, and Lebesgue argue that Zermelo's choice principle must be rejected based on their belief that one must be able to explicitly state a rule for the choice function to show its existence. However, when we consider the history of their work, they are guilty of implicitly using the principle, both before and after Zermelo's 1904 paper.

What philosophers can learn from this example is the importance of the history of what mathematicians have done and the history mathematicians rely upon in determining how a philosophical view meshes with mathematical practice. On the one hand, Zermelo uses a condensed version of history to support his claim that the axiom of choice is necessary. The axiom is used implicitly and repeatedly in previously accepted mathematical results, and it is needed for various branches of mathematics. However, when we consider the history of the axiom of choice, we find that Zermelo's efforts were insufficient to persuade other mathematicians. On the other hand, the constructivism of Baire, Borel, and Lebesgue hindered them and other mathematicians from accepting the axiom, even though they were concerned with how mathematics should be done while ignoring how mathematics was done. From our point of view today, we are able to understand what impact this had on mathematics. The fact that the axiom of choice

[53] It could be argued that much like Zermelo, who had a particular heritage view of past uses of the axiom of choice, his opponents also held their own heritage view of how mathematics should be done.

could not be proved delayed the acceptance of it. Existence of a choice function was inadequate, because its description could not be explicitly stated.

If philosophers paid attention to only one side of the controversy (and hence only one version of its history), then the delayed acceptance of the axiom of choice would seem confusing: either it would be strange that it took a long time based on Zermelo's presentation of how mathematicians have always implicitly relied on it, or it could be seen as mysterious that it was even accepted based on the objections of Baire, Borel, and Lebesgue. However, the axiom's history tells us why it took mathematicians a long time to accept the choice principle. Zermelo describes past uses of the choice principle and its contemporary uses to show that it is necessary for mathematical results; however, since some mathematicians required an explicit instance of a choice function, Zermelo's efforts did not amount to much at the time, because he (and his supporters) accepted that existence alone is adequate.

This example shows the importance of both versions of history. Understanding the mathematicians' view of history does not mirror actual events that took place. We observe this from the criticism against Zermelo's work and his reasons for the importance of the axiom of choice as necessary. Although Zermelo relied on the previous usages of the axiom, the history behind these uses shows that mathematicians were not aware that they were using the axiom. Thus, it is clear that the two types of history in this case do not coincide. It is also insufficient to follow the history of the axiom of choice while ignoring mathematicians' idea of its history. The axiom of choice took a very long time for mathematicians to accept. While it is important to understand the reasons as to why it took so long, we also need to consider the reasons why mathematicians who supported the axiom remained loyal, even while the opposition was great. Lastly, ignoring history altogether is problematic, because doing so runs the risk of going against mathematical practices. The constructivist stance of Baire, Borel, and Lebesgue did not rely much on history even though these mathematicians rejected the axiom based on how they believed mathematics should be done.

V: Conclusion

In conclusion, when we look back at the past from the present, it is difficult to separate ourselves out fully from how we understand what occurred in the past. Although historians have compared and contrasted a "pure" history written from the point of view of the past and one that is written from the present point of view, history that avoids any trace of the present is not possible, precisely because we (from the present) are looking back at a different time period. Nonetheless, as historians have pointed out, there is an extreme version of history that is told from the present point of view, which presents an inaccurate and selective version of the past as only a series of successes.

Again, it is not possible to write history without any reliance on the present. This is because the historian, who lives in the present, writes about the past from a different point of view than the figures she is writing about or how notions were understood in the past. Hans-Georg Gadamer argues this very point when he discusses his idea that understanding is a "fusion of horizons."[54] Gadamer describes a horizon as "the range of vision that includes everything that can be seen from a particular vantage point."[55] If we

[54] Gadamer, Hans-Georg. *Truth and Method*. Trans. Weinsheimer, Joel and Donald G. Marshall. London: Continuum (2004): 305.
[55] Ibid., 301.

try to understand the past while ignoring that we live in the present, then we would not be able to make sense of the past, because we would be closed off from it:

> We think we understand when we see the past from a historical standpoint—i.e., transpose ourselves into the historical situation and try to reconstruct the historical horizon. In fact, however, we have given up the claim to find in the past any truth that is valid and intelligible for ourselves.[56]

Instead, when we understand the past, Gadamer explains, it is because our present horizon is fused with the historical horizon; these horizons are not separated from each other to begin with but are constantly in motion together as one great horizon. When we understand the past, it is from our vantage point within our own horizon. Thus, history divorced from the present is not possible to write.

The purposes of referring to the past vary among historians, mathematicians, and philosophers, and so the ways in which history is written and used will vary. However, if philosophers desire to mirror their theories based on mathematical practices, history from the point of view of mathematicians and a study of past events that is not heavily influenced by the present are both necessary tools to formulate philosophical theories of mathematical practices. The former shows what mathematicians desire or find to be important, while the latter shows us the effects it has on mathematical practices.

Unfortunately, compared to the sciences, there is a lack of history written on certain parts of mathematics.[57] Philosophers of mathematics oftentimes have had to develop short histories by themselves. This usually amounts to the philosopher relying only on past mathematical texts, much like the mathematician who uses the same

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[56] Ibid., 302-3.

[57] Although there are many versions of history written on Greek mathematics, for instance, there is not much written on other parts of mathematics such as, say, functional analysis.

materials for research purposes. One should have a good grasp of the mathematics one is writing a history on, but also it is important to know how to write history. From the discussion above about the ways history can be written and for different purposes, this is a difficult task for the philosopher who is not trained in writing history. By referring only to mathematical texts, philosophers run the risk of overlooking the fact that mathematics is an activity and not a discipline that develops on its own. Formulating philosophical theories especially about mathematical practices from this type of history runs into problems, because mathematical texts, such as journal papers, are usually devoid of mathematicians' motivations, inspirations, and conceptions—we lose the human aspect of mathematics. While we may at times be unable to write the history of mathematics that goes beyond mathematical texts, we are still able to find out whether our theories match up to practice by comparing them to present practices.

Similar to Jardine's use of etics and emics in history, perhaps we can also apply it to the philosophy of mathematical practices. The etic point of view corresponds to observing what mathematicians do and formulating philosophical accounts from these observations. This includes knowing what mathematicians consider as history that is useful for their research. The emic point of view corresponds to understanding mathematical texts and understanding why and how they are helpful or contribute to further mathematical developments. Emics is described as "going native,"[58] but the philosopher does not need to be a mathematician. She should, however, be able to understand the perspective of mathematicians, because it will help her develop her philosophical views on mathematical practices. Both etics and emics are beneficial, and, just like for history, there cannot be one without the other. Etics alone misses out on the

[58] Jardine, 268.

point of view of the mathematicians. Since mathematical practices depend on mathematicians, ignoring emics risks developing a philosophical theory that does not match up with what mathematicians do. Emics without etics prevents the philosopher from developing a theory, because she loses the role of being an observer. Thus, both etics and emics work together to help develop a philosophical account of mathematical practices that meshes with what mathematicians do.

CHAPTER TWO:

MATHEMATICAL EXPLANATION REVISITED

In the previous chapter, I have emphasized the value of history when developing philosophical theories based on mathematical practices. By looking through history, we can observe what mathematicians consider to be important to their discipline and how they show this importance through their work. However, the philosopher should not just study the history used by mathematicians for their research, but also history that goes beyond the technical details of mathematical development. The former shows how mathematicians develop their work; while the latter reflects mathematical practices that tend to get excluded from the polished version of history that aids in research.

Another strategy, in addition to looking at history, is to consider current mathematical practices. This is important because mathematics—the discipline, its practices, and so on—changes over time. It would be faulty for philosophers to focus on mathematics of the past to aid in describing what mathematicians currently do. In this chapter, current mathematical practices of researchers are considered. Specifically, philosophical theories of mathematical explanation for proofs are compared to what current mathematicians deem as explanatory proofs.

Mathematical explanation has recently been highlighted as a major concern for philosophers of mathematics. For the most part, there is agreement that explanations exist in mathematics, but there is wide disagreement over what qualifies as an explanation. Some philosophers of mathematics develop theories of explanation based solely on mathematical practices. For explanations found in proofs, they consider ones that mathematicians deem to be explanatory and try to find patterns and features in these proofs that contribute to explanation. Other philosophers apply theories of scientific explanation to mathematical explanations believing that the two are generally compatible while modifying their theories to agree with mathematical practices.[59] Even though there is an emphasis on mathematical practices for theories of mathematical explanation found in proofs, these theories are focused more on either the form or pattern of the explanation or its contents without much consideration for how explanations are understood by or targeted at a particular audience. This is problematic because a heavy concentration on the form and content of proofs emphasizes a polished and static version of mathematics that ignores the fact that mathematics develops gradually not only through new discoveries, but also modifications and various improvements over time.

The focus of this chapter is on mathematical explanations found within proofs.[60] I will first give an overview of the two main accounts of mathematical explanation for proofs, one by Mark Steiner and the other by Philip Kitcher. Next, I will consider arguments against the existence of mathematical explanations and point out why these arguments are inadequate. Last, I will argue that current theories of mathematical explanation should focus more on mathematical practices—what mathematicians actually do—in addition to the content and form of proofs—to develop a more

[59] While there have been discussions of mathematical explanations in the sciences. Most of these are in support of mathematical realism and the indispensability arguments. They do not contribute much to explanations within mathematics.

[60] Mathematical explanations go beyond proofs. Diagrams, tables, theories, and so on can be used to explain various mathematical results.

compatible account that helps explain how and why mathematicians consider certain proofs explanatory.

I. Explanatory Proofs

The discussion over explanatory proofs has been concentrated on how certain proofs show why their corresponding theorems are true. They go beyond proofs that only show that their theorems are true. Although proofs that only verify their theorems are acceptable, mathematicians favor explanatory proofs more because they aid in understanding. There have been two main philosophical camps regarding mathematical explanation. The first consists of philosophers who want to develop a theory based on mathematics without appealing to theories of scientific explanation. According to this group, because of the differences between mathematics and the sciences, mathematical and scientific explanations are fundamentally dissimilar. The strategy here is to build a theory of mathematical explanation based on proofs that are regarded as explanatory by mathematicians. The second group believes that a theory of mathematical explanation can be developed out of existing theories of scientific explanation. Although there are obvious differences—for example, the fact that there are no causal relations in mathematics as there are in the sciences—the general outlines of theories of scientific explanation are thought to be compatible with explanations found in mathematics. Most contemporary philosophers favor a combination of the two—they side with the first group when it comes to focusing on the details of specific proofs; however, when they consider mathematical explanation in broader terms, they also side with the second group.

Mark Steiner is perhaps the first analytic philosopher who sets the stage for an autonomous theory of mathematical explanation.[61] He does not rely on any past theories of scientific explanation, but instead proposes a theory for mathematical explanation based on the inner-workings of a proof. For Steiner, there are two criteria which proofs must satisfy in order to be explanations. First, within a proof there must be a reference to an entity's or structure's "characterizing property," which he describes as "a property unique to a given entity or structure within a family or domain of such entities or structures."[62] He leaves "family" as undefined, but insists that it be taken as broadly as possible.[63] Moreover, he requires that "an explanatory proof makes reference to a characterizing property of an entity or structure mentioned in the theorem, such that from the proof it is evident that the result depends on the property."[64] Second, he requires a generalizability criterion, by which he means that if we choose different characterizing properties of some object and through a series of "deformations" of the original proof, but using the same "proof idea" or method, we are able to come up with multiple related proofs.[65] Unfortunately, we are left in the dark by what Steiner means by "deformations," except that they involve more than just substitutions of characterizing properties.[66]

It seems as though Steiner requires an explanatory proof to be malleable enough to generate other proofs by exchanging the properties found in the original proof for

[61] Steiner, Mark. "Mathematical Explanation." *Philosophical Studies* 34.2 (1978): 135-151.
[62] Ibid., 143.
[63] In an attempt to show what he means by "characterizing property," Steiner gives an example of an entity with two different characterizing properties: he describes the number 18 as i) the successor of 17, and ii) as the product of $2 \cdot 3^2$ (143).
[64] Ibid.
[65] Ibid., 143, 146.
[66] Ibid., 147. He acknowledges that the terms he describe are vague, but insists that his examples will offer clarification.

ones that belong to a different family or structure. It is not enough to require that new properties take the place of old ones using substitutions, but that a proof is able to be modified into a more general one while retaining much of the same method as the original proof. This is the generalizability requirement, though it is difficult to say how much modification is acceptable to stay within the boundaries of a proof's "proof idea," which is taken to be the structure of a proof.

Steiner uses the well-known Pythagorean proof of the irrationality of the square root of two as an example of an explanatory proof.[67] This proof starts off by assuming a contradiction—supposing that 2 has a rational square root. We can represent this as $2 = (\frac{a}{b})^2$, such that a and b are coprime.[68] By multiplying both sides of the equation by b^2, we obtain $a^2 = 2b^2$. From this, we observe that a^2 is even, which means that a is even too. This also implies that $4 \mid a^2$. Since $a^2 = 2b^2$, we also have $4 \mid 2b^2$, and this reduces to $2 \mid b^2$, which shows that b is even as well. However, because we assumed that a and b are coprime, we reach a contradiction, because a and b are both divisible by 2; therefore, it is false that the square root of two is rational.

According to Steiner, this proof satisfies his two criteria for a proof to count as one that explains. First of all, since a and b are coprime, the characterizing property that this proof relies on is the unique prime power factorization of a and b.[69] Steiner's second requirement is that the proof is generalizable. This is indeed the case, because we can extend this method to show for any positive integer n, $\sqrt{n}$ is either rational or

67 Ibid., 137-8.
68 Recall that two integers are coprime if their greatest common divisor is 1.
69 This is known as the Fundamental Theorem of Arithmetic.

irrational. It is further possible to generalize to *m*th roots of *n*. Thus, through Steiner's account, the proof of the irrationality of $\sqrt{2}$ is explanatory.[70]

Steiner's account of mathematical explanation has been heavily criticized. The main reason for these criticisms is because Steiner is too vague with his terms. In his essay, he purposefully leaves "family," "deformation," and "proof idea" undefined, and relies on his examples to describe them. However, in each example, these terms are used in different contexts making it difficult to understand what he would include or exclude in his account. For instance, he believes that mathematical induction is non-explanatory because it is not a property of any entity that occurs in a given theorem: "Induction, it is true, characterizes that *set* of all natural numbers; but this *set* is not mentioned in the theorem."[71] However, Hafner and Mancosu argue that induction does fall under Steiner's view of explanation, mainly because induction is a property of the number system being used—regardless of being explicitly mentioned in the theorem or not.[72] Nonetheless, Steiner's account of mathematical explanation paved the way for philosophers to investigate the various modes of explanation found in proofs.

Philip Kitcher emphasizes the value of unification found within both mathematics and the sciences.[73] According to him, explanations, which are deductive arguments, unify parts of mathematics (and science) together through their forms.

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[70] However, this goes against the argument that proofs by contradiction are non-explanatory, because these proofs only show that using a false assumption leads to an impossibility. Reactions against proofs by contradiction have existed for centuries. Although mathematicians generally do not question the validity of such proofs (besides intuitionists, who reject the Law of the Excluded Middle), they regard them as lacking explanation. See Mancosu, Paolo. "On the Status of Proofs by Contradiction in the Seventeenth Century." *Synthese* 88.1 (1991): 15-41. For more recent criticisms against proofs by contradiction, see Novaes, Catarina Dutilh. "Reductio ad absurdum from a Dialogical Perspective." *Philosophical Studies* 173.10 (2016): 2605-2628.
[71] Ibid., 145.
[72] See Hafner, J. and P. Mancosu. "The Varieties of Mathematical Explanation", in P. Mancosu et al. (eds.), *Visualization, Explanation and Reasoning Styles in Mathematics*, Berlin: Springer, 2005: 234-237.
[73] Kitcher, Philip. "Explanatory Unification and the Causal Structure of the World." *Scientific Explanation*. Eds. P. Kitcher and W. Salmon. Minneapolis: University of Minnesota Press, 1989: 410-505.

Kitcher holds that the degree of unification depends on the number of deductive arguments that are made using the least number of premises that generate the most number of conclusions: "Science advances our understanding of nature by showing us how to derive descriptions of many phenomena, using the same patterns of derivation again and again, and, in demonstrating this, it teaches us how to reduce the number of facts we have to accept as ultimate (or brute)."[74] The same goes for mathematics. The optimal set of arguments that satisfy this requirement is what he calls the "explanatory store," which is "the set of derivations that best unifies K,"[75] where, K is "the set of statements endorsed by the [mathematical] community."[76] In order for an argument to count as an explanation, it must be included in the explanatory store. By using the same argument forms repeatedly, Kitcher argues, we are able to increase our understanding, because we would be able to derive different things using these schemes and become aware of different connections and patterns found within a theory or between theories. Contrasted to Steiner's theory of mathematical explanation, which focuses on the contents of individual proofs, Kitcher provides us with a global view of explanation as he considers multiple proofs found in the explanatory store: an argument counts as an explanation if it is contained in a set of arguments that contribute towards unification.

Kitcher is mostly concerned with scientific explanation, but he claims that his theory is compatible with mathematical explanation. The mathematical explanatory store is made up of proofs. The basic premises or brute facts contained in the proofs are the axioms; however, Kitcher does not require that every explanatory proof start from axioms, but that the axioms are included in the explanatory store as facts that do not

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[74] Ibid., 432.
[75] Ibid., 431.
[76] Ibid.

require justification. This turns out to be a problem for Kitcher, because his theory does not line up with mathematical practices. Mathematicians coming up with various independent proofs for one theorem becomes a mystery under Kitcher's account—especially when a theorem is proved multiple times using different axiom systems or using concepts from different branches of mathematics.[77] Additionally, producing multiple proofs for one theorem goes against Kitcher's claim that unification depends on the number of proofs. Furthermore, his theory of explanation, being a global theory, does not explain why a proof is explanatory independently from the theory (or theories) it comes out of. Oftentimes, a proof is created from different branches of mathematics. However, for Kitcher, if the proof is included in the explanatory store, it contributes to unification.

According to Hafner and Mancosu, there are two approaches to formulating a theory of explanation: one is called "top-down" and the other is "bottom-up." Both Steiner and Kitcher developed their theories of explanation first and then compared them to various examples found. Hafner and Mancosu call this the "top-down" approach.[78] The strategy is to come up with a theory and then pick out examples that support it. This method runs the risk of having an account that is not in line with mathematical practices. Although Steiner believes that mathematical induction is non-explanatory, through his account, it sneaks in as being explanatory; while for Kitcher, placing emphasis on the quantity of arguments to qualify as explanatory ignores the fact that mathematicians seek out multiple proofs for one theorem. Contrasted to this is the "bottom-up" approach, which Hafner and Mancosu believe will do better justice to

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[77] This is not to say that the desire for explanation is the only reason mathematicians seek out multiple proofs. There are a variety of reasons why they prove theorems in more than one way including, for instance, seeking out new methods and showing a connection between different parts of mathematics.
[78] Hafner and Mancosu, 221.

mathematical practices. This involves observing what mathematicians do and then formulating a theory based on these observations. They favor this strategy, because they think that it avoids the pitfalls of Steiner's and Kitcher's accounts of not matching up to mathematical practices.

While it is clear that the top-down approach is used by Steiner and Kitcher, a purely bottom-up approach is only an ideal situation. Hafner and Mancosu do not supply their own theory of explanation, but they examine examples that go against existing theories of explanation. They hope that by doing so, philosophers will gain a better understanding of explanations in mathematics, but at the same time, they want to continue looking at other proofs that have been deemed explanatory by mathematicians before coming up with an account that covers these cases. However, this is the same as taking the top-down approach, because in order to come up with a theory, they need to either generalize or find similarities from their observations that they consider to count as explanation from the start. All this means is that the bottom-up approach is not enough to create a theory of explanation, but both approaches are necessary. The bottom-up approach is useful for gathering up examples of what mathematicians deem as explanatory proofs; however, creating a theory of mathematical explanation utilizes the top-down approach, because this theory is based on these examples. It is difficult to determine what connects these examples together, but a bottom-up approach is helpful in testing theories against other examples much in the way Hafner and Mancosu have done.

Using both the top-down and bottom-up strategies, Marc Lange develops a theory of mathematical explanation by incorporating the views of Steiner and Kitcher, as well as considering some characteristics he observes in proofs that mathematicians believe are explanatory. According to him, generally, explanatory proofs exploit specific features—symmetry, simplicity, or unification—that stand out in their corresponding theorems.[79] Lange does not give detailed descriptions of these characteristics, but tries to describe them through a handful of examples. Symmetry contributes to explanation if there is some sort of similarity between a theorem (problem) and its proof (its result). Symmetry is established on a case by case basis—there does not seem to be a fixed group of properties that turn out to be symmetric, but if there is a "striking" symmetry—that is, a symmetry that seems to play a heavy role in illuminating the reasons why a theorem holds, then this symmetry is said to be explanatory. Similarly, Lange holds that simplicity counts as explanation when a theorem is simple, and its proof exploits this simplicity and is equally simple (as opposed to including many steps or making use of complicated mathematics that go beyond what is stated in the proof). Lastly, unification is similar to how Kitcher describes it; however, a proof counts as explanation if there is a striking feature of it that contributes to unification.[80]

Lange's account of mathematical explanation is similar to Steiner's and Kitcher's as they are focused on the content and methods of proofs, but, additionally, Lange pays attention to what mathematicians say about particular proofs beyond their form and content. He gives examples of proof—comparing ones that are considered by mathematicians to be non-explanatory to their explanatory counterparts by highlighting the differences and pointing out how the latter contain explanations.

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[79] Lange, Marc. *Because without Cause: Non-causal Explanations in Science and Mathematics*. New York: Oxford UP (2017): 232-3.
[80] Ibid., 309.

II. Against Explanatory Proofs

In opposition to all such accounts of mathematical explanation, there has been opposition to the claim that mathematical explanations exist in the first place. Michael Resnik and David Kushner deny that there are explanatory proofs mainly for two reasons.[81] First, they claim (without any justification) that explanatory proofs are not significant in mathematical practices; at the time of their writing, Steiner's essay was the only one that addressed mathematical explanation, so they believe that it would be difficult to test his account.[82] They give two examples[83] of proofs that they grant could qualify as explanatory, but they neither discuss why they seem to be explanatory nor why they ultimately fail to persuade them that there are explanatory proofs in mathematics.

Their second reason is that there is no clear-cut way of determining what qualifies as explanatory or non-explanatory proof, because this all depends on individual mathematicians, their mathematical communities, and their training. This objection carries some weight against Steiner's, Kitcher's and Lange's accounts since whatever is to count as a characterizing property, what belongs in the explanatory store, or if a proof contains a striking feature depend on much more than what is contained in a proof, relying as it does on the reactions of individuals or groups of mathematicians. Resnik and Kushner write, "Whether or not something is evident from a proof is relative to subgroups of the mathematical community, at best."[84] While Lange is careful to include the mathematical community's considerations throughout most of his examples,

[81] Resnik, Michael D., and David Kushner. "Explanation, Independence and Realism in Mathematics." *The British Journal for the Philosophy of Science* 38.2 (1987): 141-158.
[82] Ibid., 151
[83] These are a proof of the Intermediate Value Theorem and Henkin's proof for completeness of first order logic.
[84] Ibid., 146.

he is still in charge of deciding what the important feature is in each of his presented proofs. In other words, he gives the content of proofs priority over mathematical practices to fit his theory of explanation.

For instance, to support his argument that differences between cases could be a salient feature of a proof, Lange presents a proof that, he believes, explains why the derivative of an infinite sum is not always equal to the infinite sum of derivatives. He does this in three steps. The first step consists of showing that the derivative of the sum of two functions is equal to the sum of its derivatives, i.e., $(f+g)'(x) = f'(x) + g'(x)$. Lange shows this by a direct calculation, relying on the definitions of the derivative and of the limit using δs and εs. This routine calculation is what makes up the majority of the proof.

In the second step, Lange generalizes to finite sums. Finally, the last step is to show the infinite case, but Lange only provides us with the following: "[W]ith infinitely many functions and hence infinitely many δ_is, there is no guarantee that some positive number is less than or equal to every δ_i. Rather, the δ_is may approach arbitrarily near to 0."[85] Lange then concludes with:

> This difference between finite and infinite sums is responsible for the difference between the two results. Thus we can "explain why it is that sometimes you can differentiate an infinite series by differentiating each term, and sometimes you cannot."[86]

Here, Lange is quoting the mathematician David Bressoud, whose textbook Lange refers to for this example.

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[85] Lange, 264.
[86] Ibid.

There are two problems with Lange's example. First of all, it is difficult to see why this proof counts as being explanatory. While I agree that the third step of this proof explains why we are not guaranteed a δ smaller than any of the δ_is, most of the details for this proof lies in the calculation of the first step, which is then extended in the other two steps, making this proof more of a brute force effort than an explanatory one. A proof that uses brute force tests every possible case without connecting the cases together. Here, three cases are used: the case of two functions, a finite number of functions, and infinitely many functions. Lange acknowledges that brute force type proofs are regarded by mathematicians as non-explanatory;[87] yet, he pushes this example as one that explains the difference between derivatives of finite and infinite sums.

Second, we are still left clueless as to why the finite and infinite cases are not always the same—the statement Lange attempts to prove. Instead, Lange only proves that the cases *could be* different, but this merely depends on the δ_is, which we are unable to keep track of in the infinite case (because there are infinitely many of them). Lange even quotes Bressoud making it seem as if the proof really does explain why we can sometimes interchange summation with differentiation for infinite series. However, this quote appears in the introduction of the chapter of Bressoud's textbook that contains the proof Lange uses and not as commentary on the proof. Bressoud provides a different proof for what Lange was supposed to prove, which is a lot more involved than Lange's presentation.[88]

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[87] Ibid., 241, 309.
[88] Specifically, he proves that if the sum of the derivatives converge uniformly, then the two derivatives will be the same. Lange relies only on point-wise convergence, which is insufficient, because, as he recognizes, there are an infinite number of δ_is.

Mark Zelcer argues that there are no explanations in mathematics, mostly because there is nothing that is similar to explanations found in the sciences.[89] His main argument comes from attempting to apply theories of explanation in the sciences to mathematics. Nothing similar to what is (or has been) accepted as scientific explanation occurs in mathematics, because, according to him, mathematics contains no predictions, surprises, or, in agreement with Resnik and Kushner, any reason to desire explanations in general.[90] Zelcer uses explanatory proof and explanation interchangeably, which suggests that he believes that nothing in mathematics could provide explanations.

Using Hempel's D-N model of explanation, Zelcer argues that there are no explanations in mathematics, because there are no predictions. He writes that the only statements that could be predictions would be about mathematical facts, but the justifications of these predictions are adequate enough to be proofs of these facts, which turns any potential prediction into a mathematical fact. Zelcer does not give any description of what a prediction is except that it is not merely a guess. He acknowledges that mathematicians make guesses all the time in practice, but they never make any predictions. However, it could be argued that there is something analogous to a prediction in mathematics, namely a conjecture, which is a mathematical statement which has yet to be proved. Not all conjectures are true, but similar to predictions they can be later confirmed or disconfirmed. Furthermore, even if they are speculations, many conjectures are based off of prior mathematical work, or, as in the case of the Four

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See Bressoud, David. *A Radical Approach to Real Analysis*. Mathematical Association of America (1994): 195-6.

[89] Zelcer, Mark. "Against Mathematical Explanation." *Journal for General Philosophy of Science* 44.1 (2013): 173-192.

[90] He also includes the usual objections: math is non-causal, it's deductive, not about nature, but of something abstract.

Color Conjecture, there was a reasonable starting point to guide mathematicians in the right direction to prove it.

Zelcer goes on to say that "the failure of a scientific prediction to be correct potentially has serious repercussions for the theory. The discovery of a proof that contradicts a mathematical conjecture only speaks to the poor intuitions of the conjecturing mathematician, not to any mathematical theory."[91] Zelcer makes it seem as if a mathematical theory is flawless, and any error that occurs is due to the fault of the mathematician. This is because mathematical statements are necessary truths, and it is up to the mathematician to uncover these truths. The way to get to these truths is through proof, which, according to Zelcer, only shows *that* a statement is true.

Zelcer seems to be under the impression that once a piece of mathematics is accepted, it will be accepted forever; there is no need for any modifications. This is a rather naïve view of mathematics that is reminiscent of A. J. Ayer's view that mathematics is made up of tautologies, where the axioms are counted as definitions, and theorems are the logical consequences of these definitions.[92] Ayer holds that mathematical statements are necessary truths that are independent of our experience. If there are changes in mathematics, it is only because, he believes, mathematicians are correcting mistakes they have made in the past. For example, he mentions that past mathematicians were "mistaken" in believing that geometry is the study of physical space, and became aware of this mistake through the creation of non-Euclidean geometries.[93] Zelcer does not go as far as Ayer in claiming that mathematics consists of

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[91] Ibid., 181.
[92] Ayer, A. J. "The A Priori." *Philosophy of Mathematics: Selected Readings*. Eds. Paul Benacerraf and Hilary Putnam. Cambridge UP (1987): 315 - 328.
[93] Ibid., 324.

tautologies, but similar to Ayer, he believes that changes in mathematics are only due to errors made by mathematicians.

This view goes against actual development and history of mathematics. Both Zelcer and Ayer do not realize that mathematical theories are developed by mathematicians themselves, and that theories can and do change over time, not only to fix past errors, but through changes in language, methods, types of questions, and so on. This is clear when we observe how definitions for terms such as "line" and "function" have changed over time and what their effects have had on mathematics throughout history.

In agreement with Resnik and Kushner, Zelcer claims that explanatory proofs are not important to mathematicians. He believes that mathematicians are more focused on just being able to prove that a mathematical theorem is true rather than attempt to give an explanation as to why something is the case. He further argues that nothing resembling explanation in mathematics exists, because mathematicians are satisfied by proofs that only show that the mathematical statement proved is true. However, though it is not necessary to always provide explanatory proofs, mathematicians frequently desire explanations of certain theorems—especially ones that are proved through heavy calculations or by brute force. For example, many mathematicians find the proof of the Four Color Theorem lacking an explanation and are holding out for a better proof, because its current computer proof tests every possible mapping by brute force methods. If Zelcer were correct that no mathematicians desire explanations, then the fact that some theorems have multiple proofs using different methods or coming out of different branches of mathematics would seem mysterious, as it does on Kitcher's account.[94] There would be no reason why mathematicians would bother proving the same theorem multiple times when one proof that shows that the theorem is true does the job.

However, it is plausible that philosophers have placed too heavy an emphasis on explanatory proof. Juan Pablo Mejia-Ramos and Matthew Inglis conducted a study on how often "explain" words are found in papers from ArXiv, an online repository that holds preprints of scientific and mathematical papers.[95] They used almost seven thousand mathematics papers and roughly fifteen thousand papers from the sciences in their study. According to their results, there was a higher frequency of words such as "show," "solution," and "prove," than "explain" in the mathematical papers. There was far more use of "explain" words in the science papers compared to the mathematical papers. The authors conclude that their "analysis of 'explanatory' talk in a large sample of mathematics papers does not offer support for a claim made in the philosophy of mathematics: that this type of talk is prevalent in mathematical discourse."[96] Instead, according to them, mathematicians are more interested in explanations that answer how-questions rather than why-questions. As a result, the authors believe that philosophers of mathematics have exaggerated what is desired in mathematical practices.[97] However, they acknowledge that their findings may not reflect mathematical proofs in general considering that they only looked at papers through ArXiv searching for words that contain the string "explain," such as "explain," "explains,"

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[94] Again, there are more reasons than just lacking explanation that motivates mathematicians to seek out multiple proofs for a theorem.

[95] Mejia-Ramos, J. P., and M. Inglis. 'Explanatory' Talk in Mathematics Research Papers. *Proceedings of the 20th Conference for Research in Undergraduate Mathematics Education* (2017): 1-7.

[96] Ibid., 6.

[97]Ibid.

and "explained," while it is possible that mathematicians use different words that are related to explanation.[98]

III. Explanation and Understanding

Philosophers should look beyond the form and content of mathematical proofs to have a better grasp of how proofs contribute to understanding. This is important for a theory of mathematical explanation, because to understand what makes a proof explanatory depends on a community of mathematicians and not solely on a collection of mathematical proofs. It is difficult to find patterns in proofs that will always guarantee an explanation in the ways Kitcher, Steiner, and Lange desire. Hafner and Mancosu suggest that philosophers must test their theories on the activities of mathematicians, and so far, they have found that the major theories are inadequate. Additionally, what is explanatory in one mathematical community might not explain anything in another community; it is dependent on the specific community addressed. For instance, it is not difficult to imagine that a proof might be considered explanatory to a group of trained mathematicians but may not explain anything to a group of mathematics students. The level of knowledge and experience are different between the two. Research mathematicians have a better understanding of the terminology, what methods are used, and so on compared to students who are still learning these things.

Contrary to Steiner, Kitcher and Lange, it is not enough to consider just either the form or content of a proof. The form of a proof is merely the steps required to get from the hypotheses to its conclusion; it contains no context of what the proof is about. There is no reason to believe that a proof is explanatory simply because it follows a specific

[98] Ibid., 5.

pattern of reasoning, because then anything that followed the same pattern would be thought of as explanatory regardless of what the proof is about. This is a problem for Kitcher, because he favors only the quantity of arguments in his “explanatory store” to supply explanations. If we evaluate the explanatory power of a proof based on its contents, then we end up judging that a proof is explanatory just because it contains something specific. Lange and Steiner, for instance, claims that as long as a proof contains and exploits the same salient feature in its theorem, then the proof is explanatory. The problem with this is that what counts as a salient feature could be anything as long as it is also used in a proof.[99] For Lange and Steiner, how a feature is used in a proof is not as important as its existence within a theorem and its proof.[100] If something is not mentioned in theorem, its use in a proof will not contribute toward explanation. This goes against what mathematicians expect out of a proof. The vast majority of theorems and proofs do not contain every single possible detail; otherwise, they will be needlessly lengthy, and, in many instances, unsurveyable. Certain details are omitted because they are obvious to the proof’s purported audience, or they can be easily obtained. It is also doubtful that what one mathematician or community deems as a striking feature of a theorem will be the ultimate feature that constitutes explanation *tout court*, independent of audience. Multiple proofs for one theorem may exploit different features and still be counted as explanatory, but their explanatory power will vary among different mathematicians. Again, there is no reason to assume that a proof that is considered to be explanatory by one community will be explanatory for all mathematicians.

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[99] Additionally, a theorem could be worded in a way where the salient feature is not explicitly mentioned.
[100] This is not all that Lange requires, but he emphasizes that these features must be in the proof without stating why they are important for explanation besides being able to avoid coincidences and exploiting some sort of symmetry between a theorem and its proof.

Perhaps the reason why it may seem to philosophers that once a proof is judged as explanatory, then it will be explanatory for all communities of mathematicians is in part because of the various case studies philosophers have been using. Although it is helpful to use simple and short examples, they are misleading because either they are too simple to cover general cases or they come from elementary textbooks that attempt to make the material as clear as possible, which often includes explanation. Explanations that occur in the classroom setting with these types of sources are important for the training of mathematicians to understand the material, but in mathematical research, there is less focus placed on explanation of why something is the case and more on methods of how a result is obtained, which is not part of some fixed set of patterns. In addition, using easy examples gives off the impression that if a proof is considered to be explanatory, it is explanatory for all mathematicians. However, at the research level, not every mathematician is capable of following every proof in mathematics—especially parts of mathematics that are outside of a mathematician's expertise. As William Thurston observed during a topology workshop, what may be clear to mathematicians from one branch may be difficult for another:

> It became dramatically clear how much proofs depend on the audience. We prove things in a social context and address them to a certain audience. Parts of this proof I could communicate in two minutes to the topologists, but the analysts would need an hour lecture before they would begin to understand it. Similarly, there were some things that could be said in two minutes to the analysts that would take an hour before the topologists would begin to get it.[101]

[101] Thurston, William. "On Proof and Progress in Mathematics." *Bulletin of the American Mathematical Society* 30.2 (1994): 175.

So, even though the case studies presented by philosophers match up with their theories, it does not guarantee that these theories will describe explanatory proofs covering every branch of mathematics.

If we regard explanations as being generated through a specific pattern reserved for explanatory proofs, then it would seem plausible that mathematicians would use these patterns for the purpose of making their proofs explanatory. Unfortunately, this is not how mathematicians go about their work. They may mimic a method found in a previous proof if the method is relevant to their work, but it is uncertain that they would use the same method with the hopes to create an explanatory proof. In other words, it is doubtful that mathematicians would use the accounts of philosophers to create explanatory proofs. This is not to say that philosophers' theories should be used in this way, but these theories suggest that if a pattern is followed or if a property is tweaked, then another proof can be produced, which will also be explanatory. Unfortunately, this just does not conform to mathematical practices.

To regard a proof as explanatory, one has to understand it first. How a mathematician understands a proof depends on her training. Although the form and content of a proof contribute toward explanation in proofs, the present philosophical theories exclude any mention of the role understanding plays or anything beyond what is already present in a proof. These theories only rely on what is stated in the proof, disregarding any required background information or omitted steps—what you see is all you get and nothing more.

For example, although Steiner rejects mathematical induction as explanatory, his reason is inadequate. He claims that mathematical induction is not a property of any entity that appears in a theorem—specifically, the fact that a proof is performing induction over the integers does not appear in the theorem, so Steiner reasons that there is no entity with a characterizing property. This is unsatisfactory, because any mathematician who understands how mathematical induction works will know that it is over the integers without needing it to be stated in the theorem or proof. In fact, centuries before its logical formulation (and coinage) by Augustus De Morgan in 1838, Gersonides is credited to be the first mathematician to use induction and to recognize it as a general method in his *Maasei Hoshev* written in 1321.[102] The way he uses induction is similar as we do today—using a base case (1 possesses property P) and an induction case (*n +1* possesses P, assuming *n* has P) to prove that any natural number *n* has property P. However, he called this method "Hadraga," which Rabinovitch translates as "rising step-by-step" and he used different notation than what we use now.[103] Obviously, Gersonides, as well as other mathematicians predating the nineteenth century who used induction in their work, did not know about the properties of the integers as how we know them today or considered induction as an axiom[104]—Gersonides was alive centuries before these mathematical developments.

If every detail of a proof must be explicit, we run into the problem of having lengthy proofs that end up being too detailed to follow. Steiner does not require this, but if mathematicians are supposed to pick out the characterizing property of the relevant entity in a theorem, then all the details and steps of the explanatory proof must be stated, because what may be considered as a characterizing property for one mathematician may not be for another who may judge a different property to be the characterizing one instead. Theorems and proofs do not contain only one entity with *the*

[102] See Rabinovitch, Nachum L. "Rabbi Levi Ben Gershon and the Origins of Mathematical Induction." *Archive for History of Exact Sciences*, 6.3, (1970): 237–248.
[103] Ibid., 245.
[104] Mathematical induction is Peano's fifth axiom.

essential characterizing property. Mathematicians prove theorems in multiple ways relying on different parts of the theorem and using multiple methods from different branches of mathematics. One example of this is the proofs for the Fundamental Theorem of Algebra, which can be proved using methods in algebra, topology, complex analysis, and others.

When we focus on just form and content of a proof, we risk falling into formalism: mathematical proofs are just deductive arguments that prove their theorems true; they may or may not be explanatory, but that just depends on logic and how their contents are manipulated. Even on an intuitive level, it is easy to see that this has nothing to do with how mathematicians come to understand the proofs to claim that they are explanatory. If form and content were really all that is needed, then proofs such as the Four Color Theorem ought to count as being explanatory: every deductive step is included in its proof, every case is considered, and it could be argued that the symmetry of the different mappings is the salient feature that helps explain why the theorem is true. Nonetheless, the proof of the Four Color Theorem is notorious for being non-explanatory. This is due to its massive length, such that no human being is capable of surveying it. Also, the proof is regarded by most mathematicians as using brute force methods, which are viewed as non-explanatory, because the majority of the proof is made up of calculations performed by the computer.

In response to criticism that their proof to the Four Color Theorem is non-explanatory, Kenneth Appel and Wolfgang Haken hold that their work explains how only four colors are needed to color a planar map. Their proof structure is easy to follow, and the code written for the computer program is also straight-forward. However, they agree that their proof does not explain *why* four colors is the minimum.

In support for formalism in connection to lengthy computer proofs, John Harrison argues that if a proof contained every one of its deductive steps, it will aid in the theorem's explanation.[105] This is because—as long as the proof is logically correct—starting from the axioms, and working its way to show that the conclusion holds, all of the details are included. This cuts down on errors—at least compared to informal proofs made by human beings, because there are no skipped steps.[106] If it is too lengthy for a human to look over, Harrison suggests that the level of detail can be controlled, which is determined by the mathematician's needs. He clearly favors a theory of explanation that focuses on form and content. However, even if all the details are included in a proof, it may still lack explanatory power to answer why-questions. Perhaps Harrison is more interested in how proofs can answer how-questions, and computer proofs are capable of showing this.

IV: Conclusion

In conclusion, although philosophers have presented convincing theories of mathematical explanation, they are still focused on the internal parts of a proof without giving much thought to the targeted audience of these proofs: the mathematicians. By ignoring how mathematicians regard their work, philosophers create theories that do not match up to mathematical practice. It does not seem possible to only rely on the "bottom-up" approach, where philosophers formulate their explanatory accounts according to mathematical practices, because what counts as explanatory will ultimately depend on the philosopher's conception of explanation. When they only look over

[105] Harrison, John. "Formal Proof—Theory and Practice." *Notices of the AMS* 55.11 (2008): 1400.
[106] Ibid., 1399.

simple proofs or proofs found in textbooks, philosophers base their theories on the ideal situation where mathematicians always desire explanatory proofs, because these proofs are intended to train the mathematician and do not necessarily reflect what happens at the level of research. As a result, philosophical accounts of explanation will continuously be criticized because they fail to represent actual mathematical practices. However, if philosophers look beyond these proofs and inquire about why mathematicians consider certain proofs explanatory while continuing to pay attention to their non-explanatory counterparts, then it is possible to come up with a general theory of mathematical explanation.

CHAPTER THREE:

A HISTORICAL APPROACH TO MATHEMATICAL EXPLANATION

There is no doubt that proofs contribute towards mathematical understanding. After all, they are used to show that a mathematical proposition is true. However, as discussed in chapter two, this does not mean that all proofs are guaranteed to provide understanding. It depends on the contents of a proof, as well as the proof's audience. Some proofs only verify that a proposition is true. For example, the proof of the Four Color Theorem is said to only show that the theorem is true using brute force. In addition to verification, there are proofs that also offer explanation or introduce methods that could aid in generating further results. It is this latter kind of proof that philosophers have been focused on when discussing explanatory proofs; however, they stop short of exploring how they contribute to mathematical understanding.

Explanatory proofs play an important role in mathematical understanding. Not only does one convince the reader that a theorem is true, but also why or how it is true.[107] In the previous chapter, I have argued that although philosophers have formulated theories of explanation for mathematical proof, they do not match up with mathematical practices. Either philosophers selectively ignore instances in which mathematicians consider a proof explanatory, or they include proofs that are not

[107] Philosophers have concentrated on proofs that demonstrate why a theorem is true, while ignoring proofs that show how a theorem is true. Roughly, a proof that explains why a theorem is true presents reasons and purposes for the theorem to hold; whereas a proof the explains how a theorem is true focus on the methods involved and the conditions needed for the theorem to be true.

considered explanatory by mathematicians. The reason for this is that philosophers have based their theories on a small sample of basic proofs. This is not necessarily a bad strategy to start with; however, basing a theory on examples from elementary textbooks only represents the material that is used for the training of the mathematician, which for its part fails to represent research practices.

History also plays an important role for mathematical explanation. Mathematics goes through a number of changes over time. This includes its language, methods, questions, and so on. We need to be aware of these changes so that we avoid dangerous anachronisms—such as assuming that a definition of a mathematical term used in the past is the same as its present one—and it will give us an idea of how and why mathematicians developed their work in the way they have.

In this chapter, my goal is to highlight the importance of history, which was discussed in chapter one, remaining focused on explanatory proofs for mathematical researchers, which are different from proofs found in textbooks. I will first discuss the importance of distinguishing between the training of a mathematician and research done outside the classroom setting with regards to mathematical proof. Because of these differences, theories of mathematical explanation or understanding developed from material found in the classroom do not extend to how a research mathematician comes to understand a proof. As an alternative to looking at basic mathematical proofs found in textbooks, I suggest that we should look at a series of proofs for one theorem, starting with the earliest proof and ending with what is now considered to be the standard proof. My example will be Lewy's Theorem and four of its proofs that were written in different time periods, spanning a total of seventy-eight years. After that, I will consider what we can gain through looking at proofs in this way. Specifically, I will argue that the history of a theorem and its successive proofs give us an idea of how mathematicians prove theorems in multiple ways, which depends on the purposes of these theorems. With this, we are able to recognize that explanations in proofs depend not only on their form and content, but also on their audience.

I. Proofs in Training and Research

A general difference between proofs that are studied in the classroom and those used in research is that there is more emphasis on explanatory proofs in the classroom setting. Proofs found in textbooks are written with details that help students understand why a theorem is true while making clear the methods and definitions used. Additionally, textbooks are written with the aim of presenting the material clearly to guide the reader. Compared to a theorem's first generation proof, which tends to depend on calculations and limited insight, proofs of later generations are more polished and can rely on generalizations or abstractions that can contribute to their explanatory power. Textbooks contain these later generation proofs, while journals present first generation proofs.

In an effort to distinguish the differences in purpose between proofs used in training and research, the mathematician Reuben Hersh writes, "[T]he purpose of proof [in the classroom] is understanding" [108] while for research, the purpose is to convince its audience that its corresponding theorem holds.[109] He believes that the main goal of a mathematics course is to explain new concepts to students; the teacher is to provide full explanation when necessary with the help of proofs to aid the students in grasping the

[108] Hersh, Reuben. "Proving is Convincing and Explaining." *Educational Studies in Mathematics* 24.4 (1993): 398.
[109] Ibid., 396.

material.[110] The proofs found in research, however, Hersh claims, are mostly to verify a mathematical result and do not focus on explanation as much as in textbooks.[111] He believes that the goal for proofs in research mathematics is to be able to convince other mathematicians that a result is correct.[112]

The difference between training and research practice is important when formulating a theory of mathematical explanation as well as understanding. Although whether or not a proof is explanatory or just convincing is partially dependent upon its audience, we are able to extract certain details from proofs to determine how they fit within the overall scheme of mathematical practice. However, this variation takes some careful analysis because there are a variety of ways proofs are created. This detail is overlooked by philosophers, but it is important when developing a general theory that attempts to mesh with mathematical practices. Most philosophers who have introduced theories of mathematical explanation, for example, claim that they apply to research practices, but then rely on material for students.[113] Perhaps this is to keep their examples simple for their audience, but it comes with the disadvantage of mistaking training material for what is found in research, largely ignoring the transformation that takes place between primary mathematical research and the textbook presentation of results. The process of how results were obtained through research is very different

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[110] Ibid., 397.

[111] This is not to say that there are no explanatory proofs in mathematical research, but only that they are not emphasized as much.

[112] Ibid., 391.

[113] Mark Steiner consults Hardy and Wright's well-known *An Introduction to the Theory of Numbers* as well as George Pólya's *Induction and Analogy in Mathematics*, which is written for students as a guide to mathematical reasoning (beyond deductive reasoning). In response to Steiner's essay, Resnik and Kushner depend on *What is Mathematics* (a reference book of basic mathematics) and Rudin's *Principles of Mathematical Analysis*, an undergraduate textbook. More recently, Christopher Pincock relies heavily on *Galois Theory*, by Ian Stewart, an undergraduate textbook. Marc Lange presents a problem given at a high school mathematics event as one of his main examples.

from how they are given in textbooks, which provide the reader with polished and often anachronistic versions of developments while ignoring their historical development.

II. Genealogy of a Theorem

Instead of developing theories according to simple examples from various branches of mathematics, a different strategy is to look at the evolution of one proof from its origins to its recent standard presentation to have a better grasp of what mathematicians look for in proofs. This would shed some light also onto how a proof aids in understanding. At the same time, we need to look beyond just the proofs themselves and consider how and why they came about, as well as the intention behind each iteration. The backgrounds of the mathematicians who have worked on these proofs and the time and place at which they were written will also give us a better understanding of why a theorem's proofs developed in the ways they have.

Some mathematicians have explained that proofs are improved over time, becoming clearer and easier to understand. For instance, endorsing the stance that simplicity is tied to understanding, Gian-Carlo Rota writes,

> The first proof of a great many theorems is needlessly complicated... It takes a long time, ranging from a few decades to entire centuries, before the facts that are hidden in the first proof are understood, as mathematicians informally say. This gradual bringing out of the significance of a new discovery takes the appearance of a succession of proofs, each one simpler than the preceding. New and simpler versions of a theorem will stop appearing when the facts are finally understood.[114]

Similarly, from Michael Aschbacher:

[114] Rota, Gian-Carlo. "The Phenomenology of Mathematical Proof." *Synthese* 111.2 (1997): 192-3.

> The first proof of a theorem is usually relatively complicated and unpleasant. But if the result is sufficiently important, new approaches replace and refine the original proof, usually by embedding it in a more sophisticated conceptual context, until the theorem eventually comes to be viewed as an obvious corollary of a larger theoretical construct. Thus proofs are a means for establishing what is real and what is not, but also a vehicle for arriving at a deeper understanding of mathematical reality.[115]

This highlights the fact that even if a proof is available for a theorem, mathematicians still look for other proofs if they find it confusing, lacking explanation, or it does not lead to further developments.[116]

By surveying the evolution of successive proofs, we are able to track developments through changes in language, method, and context. What contemporary philosophers tend to forget when developing their theories is that mathematics is not a static discipline; mathematics changes over time. According to some philosophers,[117] the explanatory value of a proof depends only on what is presented in that proof; anything external (such as prior mathematical knowledge that helps mathematicians understand a proof, omitted steps, and the impact it may have on existing and future results) does not contribute to explanatory value. Simply put, there is no more to a mathematical proof than what appears in print. It is my contention that, looking

[115] Aschbacher, Michael. "Highly Complex Proofs and Implications of Such Proofs." *Philosophical Transactions of the Royal Society of London A: Mathematical, Physical and Engineering Sciences* 363.1835 (2005): 2403.
[116] The mathematician, Paul Erdös, believed that mathematicians should strive to write a proof that belongs in "The Book," which is filled with the best proofs for theorems.
[117] Here, I am referring to Mark Steiner and Philip Kitcher, who developed the main accounts of mathematical explanation.

beyond a single proof of a theorem, we can come to a better understanding of what mathematicians desire from proofs.

Tracking the changes made through several proofs has the added advantage of highlighting that a piece of mathematics is neither static nor permanent. Because of this, we are able to observe that mathematics is historically situated—the desires of mathematicians change over time due to further developments in techniques, terminology, and so on. This can help us determine what types of questions mathematicians seek to answer and what was acceptable for them in a particular time period and place. What was relevant in one period and place may not be so relevant in another. For instance, during the second half of the nineteenth century, there was a growing emphasis on mathematical rigor in proofs in Germany to avoid the ambiguities found in older methods of mathematical reasoning; however, in France, the focus was on the applications of mathematics as a tool, but after 1880, the French also embraced the style of German mathematics.[118] Nowadays, the amount of rigor required in a proof has lessened in both countries (and in general).

Mathematicians are aware of some of the history of their specific fields and are familiar with the changes that take place over time.[119] This helps them understand the common methods, main theorems, and concepts that have helped shape their fields and open the door to further developments. Knowing the background of their fields also helps the mathematician avoid past errors and confusions made by other mathematicians, as well as aid in clarifying certain components of the field.

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[118] Schubring, Gert. *Conflicts between Generalization, Rigor, and Intuition: Number Concepts Underlying the Development of Analysis in 17-19th Century France and Germany*. New York, NY: Springer Science+Business Media, Inc. (2005): 606-9.
[119] Their version of history, however, may differ from an accurate sequence of historical events emphasizing major developments.

Philosophers should also be aware of the history behind theorems and proofs when developing theories of mathematical explanation. This would help their theories match up with the practices of mathematicians instead of solely relying on the contents of individual proofs. Knowing the history of a theorem and its multiple proofs will add to the philosopher's understanding of why a theorem was proved in these different ways. Additionally, it would provide an idea of what mathematicians desire out of proofs.

III. Example: Lewy's Theorem

I will now turn to an example to highlight what can be gained from following the developments made through a series of proofs for a theorem.[120] Here, Lewy's Theorem and four of its proofs will be presented. Today, mathematicians consider this theorem as one of the basic theorems of harmonic mappings. Harmonic mapping theory is the study of complex-valued harmonic functions. It is a branch of mathematics that developed out of the study of minimal surfaces in the 1920s and gained interest among complex analysts later on in the 1980s through a famous paper by James Clunie and Terence Sheil-Small that presents some similarities between univalent harmonic mappings and conformal mappings, which are univalent, holomorphic functions.[121] This work and other developments suggest that harmonic mappings are generalizations of conformal mappings.

Although Lewy's Theorem is named after Hans Lewy from his work in "On the Non-Vanishing of the Jacobian on Certain One-to-One Mappings" from 1936, the result can be traced back to a problem posed by Tibor Radó in the German Mathematical

[120] Readers may skim over this and the next four sections without missing relevant information.
[121] One difference between the two is that while a conformal function's real and imaginary parts are harmonic conjugates, the components of a harmonic mapping need not be. All conformal mappings are harmonic, but the converse is false.

Society's annual report of 1926—a decade before Lewy's paper—regarding the continuity of a harmonic mapping inside a convex curve and answered by Hellmuth Kneser, who offered a general sketch of a proof. Part of Kneser's solution relies on the same result that Lewy proves. Later on in 1951, in his paper, "Isolated Singularities of Minimal Surfaces," Lipman Bers came up with a more simplified and clearer proof than Lewy's and is based on Kneser's work. The last proof that will be presented is by Peter Duren. This proof appears in his reference book titled, *Harmonic Mappings in the Plane*. The level of detail in his proof is the standard that usually appears in other texts.[122]

Before jumping into the theorem and its proofs, a few technical details are in order. A real harmonic function $u: \mathbb{R}^2 \to \mathbb{R}$ is one that satisfies Laplace's equation:

$$\Delta u = \frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} = 0.$$

A complex harmonic mapping f is made up of two harmonic functions u and v with the form

$$f(x, y) = u(x, y) + iv(x, y).$$ [123]

Using complex notation, we have for $f: D \to \Omega$, where $D, \Omega \subseteq \mathbb{C}$, $f(z) = u(z) + iv(z)$. Here, $z = x + iy$.

Lastly, the Jacobian of a function is the determinant of its partial derivatives. It is calculated as

$$J_f = \begin{vmatrix} \frac{\partial u}{\partial x} & \frac{\partial v}{\partial y} \\ \frac{\partial u}{\partial y} & \frac{\partial v}{\partial y} \end{vmatrix} = \frac{\partial u}{\partial x}\frac{\partial v}{\partial y} - \frac{\partial u}{\partial y}\frac{\partial v}{\partial y}.$$

[122] See for example Dierkes, Ulrich, Stefan Hildebrandt, Friedrich Sauvigny, and Anthony Tromba. *Minimal Surfaces*. Heidelberg: Springer (2010): 78-79.
[123] The main difference between a harmonic mapping and a holomorphic mapping is that a holomorphic mapping must satisfy the Cauchy-Riemann equations ($u_x = v_y$ and $u_y = -v_x$).

If $J_f > 0$ at some point $z_0 \epsilon D$, the function f is sense-preserving, and if $J_f < 0$, the function is sense-reversing.

Lewy's theorem states that the Jacobian determinant of a complex harmonic function in a neighborhood of $z_0 \epsilon D$ is never equal to zero. In other words, either this function's orientation is sense-preserving or sense-reversing at z_0 depending on whether its Jacobian determinant results in a positive or negative quantity. This is a well-known result for holomorphic functions, which are sense-preserving.[124] Lewy's Theorem extends this result to the harmonic case.

III.a. Kneser's Proof of Lewy's Theorem

In 1926, Tibor Radó posed a problem in the German Mathematical Society's annual report, a collection of mathematical works, reviews, and a section devoted to a listing of problems presented by mathematicians with their solutions written by other mathematicians.[125] Radó set up the problem as follows. Let B = ∂D be a simple and closed curve in the xy-plane and C = $\partial \Omega$ be another simple and closed curve in the uv-plane.[126] Suppose that the harmonic mapping $u(x, y), v(x, y)$ is a continuous mapping from B onto C. If C is convex, then, the mapping is also univalent and continuous from D onto Ω. This is now known as the Radó-Kneser-Choquet Theorem and is a well-known theorem in harmonic mapping theory.[127] This theorem is connected to Lewy's

[124] See Ahlfors, Lars V. *Complex Analysis: an Introduction to the Theory of Analytic Functions of One Complex Variable*. New York: McGraw-Hill (1953): 71.
[125] Kneser, Hellmuth. Loesung der Aufgabe 41., *Jahresbericht der Deutschen Mathematiker-Vereinigung*. 35 (1926): 123-4.
Radó, Tibor. Aufgabe 41., *Jahresbericht der Deutschen Mathematiker-Vereinigung*. 35 (1926): 49.
[126] Simple and closed curves are known as Jordan curves, which are homeomorphic to the unit circle.
[127] Gustave Choquet's name is included in this theorem, because he proved a similar result twenty years after Kneser. It is likely that he did not know of Kneser's proof. The first part of his proof (showing that the mapping is locally univalent) is similar as Kneser's, so it is omitted here. Choquet's version of the

Theorem, because to prove part of it requires showing that the mapping is locally univalent in a neighborhood in D. This involves showing that the Jacobian in this neighborhood does not vanish, which is precisely Lewy's Theorem.

There are two steps in Kneser's proof. The first step is to show that the mapping is locally univalent in D. Although Kneser does not mention that this is done by showing that the Jacobian determinant is never zero, this is what he essentially shows. The second step involves using Cauchy's Argument Principle modified for harmonic functions to show that the mapping is globally univalent and maps D onto Ω. The first part of the proof is the main result and is relevant for our purposes.

Kneser proves the first step by contradiction. He first supposes that $au(x, y) + bv(x, y) = c$, where c is a constant. The points (x, y) in D that satisfy the level curve $au(x, y) + bv(x, y) = c$ are, according to him, points with different tangents. Thus, he observes that the level curve is made up of simple closed curves and lines ending on B, which can only occur in at most two points whose images are in the intersections of C with the level curve. Since this can occur for at most two such lines on C, on B, it is constant, which, Kneser concludes, is a contradiction, because of the convexity of C.

Without knowing what level sets have to do with univalence or the Jacobian determinant, it is difficult to understand how this proof proves the result. There are many details left out, and there is even no mention of the Jacobian in the theorem or in this part of the proof. As a first proof that shows that harmonic mappings are locally univalent, it is difficult to follow without unpacking the details from the beginning of the proof.

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theorem only requires that the function is a homeomorphism and not a harmonic mapping. His result involves the Poisson integral, which is used to extend the homeomorphism into a harmonic function.

III.b. Lewy's Proof

A decade after Kneser's result, Hans Lewy published his version of the theorem, which now bears his name. He states his theorem as:

> If $u(x,y)$ and $v(x,y)$ are harmonic, $u(0,0) = v(0,0) = 0$, and if there exists a neighborhood N_1 of the origin of the xy-plane and a neighborhood N_2 of the origin of the uv-plane such that $u(x,y)$ and $v(x,y)$ establish a mapping of N_1 onto N_2 which is one-to-one both ways, then the Jacobian $\partial(u,v)/\partial(x,y)$ does not vanish at the origin. [128]

His proof is a bit more involved and very different from Kneser's. First, Lewy converts u and v to polar coordinates. He is able to do this because univalent harmonic functions are invariant under linear transformations:[129]

$$u = \sum_{n=j}^{\infty} a_n r^n \cos(n\theta) + b_n r^n \sin(n\theta), \qquad \text{where } a_j^2 + b_j^2 \neq 0,$$

$$v = \sum_{n=k}^{\infty} A_n r^n \cos(n\theta) + B_n r^n \sin(n\theta), \qquad \text{where } A_k^2 + B_k^2 \neq 0.$$

Lewy then considers two situations: when $j = k$ and when $j < k$.[130] He first tackles the case when $j = k$. He makes two assumptions. The first one is that $a_k B_k - A_k b_k \neq 0$,[131] and second, because of the invariance of these functions, he assumes that

$$a_k = B_k = 1 \text{ and } b_k = A_k = 0.$$

Applying these restrictions, he obtains

$$\bar{u} = r^k \cos(k\theta) \text{ and } \bar{v} = r^k \sin(k\theta).$$

[128] Lewy, Hans. "On the Non-vanishing of the Jacobian in Certain One-to-One Mappings." *Bulletin of the American Mathematical Society.* 42.10 (1936): 689-692.
By "one-to-one both ways," Lewy means that the functions and their inverses are univalent.
[129] See Ahlfors, 175.
[130] There is no need to look at the case when $j > k$, because it is the same as $k < j$.
[131] Lewy is able to make this assumption, because otherwise $\bar{u}$ and $\bar{v}$ below would not be univalent.

Lewy adds these to the rest of the series resuming with $k + 1$, with $0 \leq t \leq 1$:

$$u_t = \bar{u} + t \sum_{k+1}^{\infty} a_n r^n \cos(n\theta) + b_n r^n \sin(n\theta),$$

$$v_t = \bar{v} + t \sum_{k+1}^{\infty} A_n r^n \cos(n\theta) + B_n r^n \sin(n\theta).$$

The winding number is the same for $(\overline{u}, \overline{v})$ and (u_t, v_t), and is either ± 1, because the mapping is univalent.[132] Lewy concludes that from this, $j = k = 1$, and the theorem is proved for this case.

Next, for $j < k$, Lewy calculates the Jacobian determinant,

$$J_{(u,v)} = \frac{\partial(u,v)}{\partial(x,y)} = \frac{\partial(u,v)}{\partial(r,\theta)} = \begin{vmatrix} \partial u/\partial r & \partial v/\partial r \\ \partial u/\partial \theta & \partial v/\partial \theta \end{vmatrix}$$

of the first term of the mapping to obtain

$$kjr^{j+k}\left[\left(A_k a_j + B_k b_j\right)\sin\left((j-k)\theta\right) + (a_j B_k + b_k A_j)\cos((j-k)\theta)\right].$$

From this, Lewy observes that the Jacobian determinant is either positive or negative. This is a contradiction, because the value must either be 1 everywhere or -1 everywhere (and not both) for a univalent mapping. Therefore, he concludes that it is not the case that $j < k$.

It is clear that Lewy uses a different method to prove the theorem from Kneser. While Kneser uses level sets, Lewy's proof depends on the coefficients of the series representations for the harmonic functions. In the second part of Lewy's proof, he makes use of the Jacobian determinant, which was not referenced in Kneser's proof at all.

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[132] This follows from the Argument Principle. The positive or negative value depends on orientation. See Markushevich, A. I. *Theory of Functions of a Complex Variable: Pts. II & III. Trans. Richard Silverman*. (Providence: AMS Chelsea, 2005): Pt. 2, 48.

III.c. Bers' Proof

Next, we will look over Lipman Bers' 1951 proof of Lewy's theorem.[133] His proof is based on Kneser's proof as it also involves level sets. Bers' version of Lewy's Theorem depends on the holomorphicity of two complex functions. He creates a harmonic mapping from the real parts of two holomorphic functions, f_1 and f_2:

$$u = u(x, y) = Re\ f_1(z) \ \text{ and } \ v = v(x, y) = Re\ f_2(z).$$

Bers proves that if the harmonic mapping (u, v) is univalent in a neighborhood of z_0, then the Jacobian determinant of the mapping (u, v) is never zero. This is different from Kneser's and Lewy's versions, because neither of them made use of holomorphic functions. In Radó's formulation of his problem answered by Kneser, he brings up univalent holomorphic functions; however it is to separate them from the harmonic case.

Bers proves Lewy's Theorem by contradiction. He sets z_0 as the origin, and supposes that the Jacobian determinant of the mapping is zero there. He then separates the problem into three cases: when $f_1'(0) = 0,$ when $f_2'(0) = 0$, and setting $u = ax + by + \cdots$ and $v = \frac{1}{c}(ax + by) + \cdots$ in the neighborhood of the origin, with $a, b, c \in \mathbb{R}$, $a^2 + b^2 \neq 0$, and $c \neq 0$. For the first case, since $f_1'(0) = 0$, there are at least two lines $u = Re\ f_1(0)$ that go through $z_0 = 0$, but the mapping was assumed to be univalent, so we reach a contradiction. Lewy observes that the same goes for the second case of $f_2'(0) = 0$. Lastly, to show that the last case fails to be univalent, he sets $g_1(z) = f_1(z) - cf_2(z)$ and $g_2(z) = f_1(z) + cf_z(z)$. Then $u_g = Re\ g_1(z)$ and $v_g = Re\ g_2(z)$. Since $g_1'(0) = 0$ and $g_2(0) = 0$, the mapping (u_g, v_g) is not univalent.

[133] Bers, Lipman. "Isolated Singularities of Minimal Surfaces." *Annals of Mathematics* (1951): 369-370.

III.d. Duren's Proof

The final proof appears in Peter Duren's book on harmonic mappings from 2004.[134] His proof is based on Bers' proof (and Kneser's), but he fills in details and breaks down the steps. Unlike the proofs above, Duren's proof appears in a book that is used as a reference to the theory of harmonic mappings. What makes his proof the easiest to follow is that the material in his book before Lewy's theorem appears prepares the reader with the basics of harmonic functions, important theorems, and definitions. For Duren, Lewy's Theorem is a stepping stone to the Radó-Kneser-Choquet Theorem, and thereby makes the latter theorem easy to understand as well, even though the Radó-Kneser-Choquet Theorem came chronologically before Lewy's Theorem.

Before Duren presents Lewy's Theorem, he supplies an additional theorem about the level set of a harmonic function that goes through a critical point. According to this theorem, there are at least two arcs that emanate through this point at equal angles.[135] This theorem is used later in the proof of Lewy's Theorem.

Lewy's Theorem in Duren's book is straight-forward: If a harmonic function f is locally univalent in a domain $D \subset \mathbb{C}$, then its Jacobian determinant never vanishes for every $z \in D$.[136]

Duren begins his proof with assuming that the Jacobian determinant is equal to zero. This is the same strategy that Kneser and Bers use. Here, Duren fills in the details. He sets $f = u + iv$. Since he assumes that the Jacobian determinant is zero at z_0, i.e.,

$J_f = u_x v_y - u_y v_x = 0$,

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[134] Duren, Peter. *Harmonic Mappings in the Plane*. Cambridge Tracts in Math. 156, Cambridge UP, Cambridge (2004): 20.
[135] Ibid., 19.
[136] Ibid., 20.

$$\begin{cases} au_x + bv_x = 0 \\ au_y + bv_y = 0 \end{cases}$$

is a system of linear equations where a and b are not both zero, which means that z_0 is a critical point of $au + bv$. Without loss of generality, Duren assumes that $f(z_0) = 0$ and near z_0, he considers the level set $\{z \in D: au + bv = 0\}$. From the theorem stated above, through z_0, there are at least two arcs that cross each other with equal angles. Duren applies f to this level set and observes that these arcs are mapped into $au + bv = 0$, which is linear. He reaches a contradiction because of the hypothesis that f is locally univalent. Therefore, the Jacobian determinant is non-zero, and the theorem is proved.

IV. Recapitulation and Analysis

As we can observe, formulations of Lewy's Theorem and its various proofs have changed over time. The theorem's first version comes from a result needed to answer Radó's problem, which Kneser provides in a short sketch of a proof. Instead of being its own theorem, the local univalence of a harmonic mapping is needed to prove a further result, which later becomes an important theorem in harmonic mappings.[137] Showing local univalence amounts to showing that the Jacobian determinant of a harmonic mapping does not vanish. Kneser proves this by contradiction by assuming that there is a critical point, which implies that the Jacobian determinant is zero at this point. Lewy, ten years later, proves that if a harmonic mapping is a homeomorphism, then its Jacobian determinant does not vanish. Lewy proves this very differently from Kneser, using the properties of the harmonic functions' trigonometric series representation. Bers and Duren follow Kneser in their proofs, proving the theorem by contradiction by

[137] This is the Radó-Kneser-Choquet Theorem.

assuming that a level set exists for their mappings. Bers describes the theorem differently from Kneser and Lewy, because he uses methods from complex analysis, which was how he approached much of his work. Duren's proof spells out the missing details of Kneser's and Bers' proofs. [138]

The purpose of the theorem varies among these mathematicians. Radó uses the result in his work on minimal surfaces. At the time, he was at the University of Szeged. His advisor was Frigyes Riesz, who made significant contributions to functional analysis. With the guidance of Riesz, Radó mostly concentrated on complex analysis and was interested in topics such as boundary problems for analytic functions and subharmonic functions.[139] Four years into his studies, around 1925, he switched gears and turned to the study of minimal surfaces investigating their analytic properties.[140] In 1930, he published his famous solution to the Plateau Problem which includes Kneser's proof to aid in solving a boundary value problem to determine a minimal surface bounded by an analytic curve.

Lewy used his theorem for his work on the Monge-Ampère equations from partial differential equations. In 1936, the year his theorem was published, he was a professor at the University of California. By then, he had already started working on analytic solutions to the Monge-Ampère equations and the Minkowski Problem.

Bers briefly references this theorem in his well-known paper on isolated singularities found on minimal surfaces. He presents Lewy's Theorem in the beginning

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[138] Interestingly, the method of using level sets to prove Lewy's Theorem fails for dimensions of $\mathbb{R}$ greater than two. See Szulkin, Andrzej. "An Example Concerning the Topological Character of the Zero-Set of a Harmonic Function." *Mathematica Scandinavica* 43.1 (1979): 60-62.
[139] Kreyszig, Erwin. "Remarks on the Mathematical Work of Tibor Radó." *The Problem of Plateau: A Tribute to Jesse Douglas and Tibor Radó*. Ed. Themistocles Rassias. River Edge: World Scientific Publishing (1992): 21-2.
[140] Ibid., 23.

of his section on harmonic mappings to use in a proof of a lemma. His paper was praised for using techniques from complex analysis.[141] Even in his proof of Lewy's Theorem, he makes use of holomorphic functions.

Lastly, as mentioned above, Duren presents his proof in his reference book on harmonic mappings. Duren is an analyst who has done work in harmonic mappings, function theory, as well as other areas of analysis. Since he aims to show the similarities between harmonic mappings and conformal mappings, many of the results he presents in the beginning of his book uses tools from complex analysis.

Lewy's proof stands out as being very different from the proofs of Kneser, Bers, and Duren. He was apparently unaware of Radó's and Kneser's work. The paper in which his theorem appears only contains two theorems and their corresponding proofs without any sort of explanation as to why he presented these results. However, when we look beyond his paper and consider what he needed these results for, we find that Lewy's main focus at the time (and throughout his career) was on partial differential equations. This explains the style of his proof; to prove his theorem, he manipulates the coefficients of the series representations that make up the mapping (u, v). Looking at his published papers a few years before and after the paper in which he proves his theorem, it is clear that he was working on analytic solutions to the Monge-Ampère equations used in differential geometry as well as an analytic solution to the Weyl and Minkowski problem, which is closely connected to the Monge-Ampère equations.[142] A vital step for both of these results is his theorem on the univalence of harmonic

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[141] Abikoff, W., C. Corillon, I. Kra, T. Weinstein and J. Gilman. "Remembering Lipman Bers." *Notices* of the *American Mathematical Society.* 42 (1995), 11.
[142] See Lewy, Hans. "A Priori Limitations for Solutions of Monge-Ampère Equations. II." *Transactions of the American Mathematical Society* 41.3 (1937): 365-374.
Lewy, Hans. "On the Existence of a Closed Convex Surface Realizing a Given Riemannian Metric." Proceedings of the National Academy of Sciences 24.2 (1938): 104-106.

mappings, which he uses to prove a compactness theorem, which is one of his main contributions to the study of Monge-Ampère equations.[143]

Kneser's style of proof comes from topology. At the time of his proof, the study of topology was largely dependent on the properties of homeomorphisms, which, unfortunately, were not yet fully developed. What was considered as a homeomorphism varied among mathematicians. Either it was considered what mathematicians now call a diffeomorphism,[144] a univalent continuous function, or a continuous bijection with a continuous inverse, which is the modern definition.[145] It was not until the 1930s that the modern definition of it became the standard.[146] This is relevant for our case because Radó poses his question originally in German. His description of the mapping in German is "eine umkehrbar eindeutige Abbildung," which at the time mathematicians regarded as a univalent mapping, and not a unique, invertible mapping (which would be a modern-day homeomorphism as long as the mapping is continuous).[147] In a paper from 1930, "The Problem of the Least Area and the Problem of Plateau," where Radó mention's Kneser's proof, he only requires that the mapping is univalent on the boundary.[148] Even if Radó required that the mapping be a modern-day homeomorphism, then Kneser's proof will still work, because it does not depend on the invertibility of the mapping.

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143 Heinz, Erhard. "Commentary on Lewy's Papers." *Hans Lewy Selecta. Vol 1.* Ed. David Kinderlehrer. Boston: Birkhauser (2002): xxxvi.

144 This is a differentiable bijective map with a differentiable inverse. Henri Poincaré originally coined the term homeomorphism to refer to these types of maps in 1895. See Moore, Gregory. "The Evolution of the Concept of Homeomorphism." *Historia Mathematica.* 34 (2007): 335

145 Ibid., 333-4.

146 Ibid., 342.

147 Ibid., 336n4.

148 Radó, Tibor. "The Problem of the Least Area and the Problem of Plateau." *Mathematische Zeitschrift* 32.1 (1930): 796.

Bers and Duren based their proofs on Kneser's proof. In his paper that contains the proof, Bers mentions only Lewy when he presents the theorem; however, elsewhere in his paper, he refers to Radó's book on the Plateau Problem, which contains Kneser's proof. It is clear that Bers knew about Kneser's 1926 proof as his proof is very similar. His proof is more straight-forward, but also contains a section covering how minimal surfaces relates to harmonic mappings, so there is a little background information about the properties of the holomorphic functions that are used in the theorem. In general, although Bers' paper is on minimal surfaces and partial differential equations, his work is in the style of complex analysis, using properties of holomorphic functions, which are made up of real harmonic functions. After he proves Lewy's Theorem in his paper, Bers notes that "Lewy's [Theorem] implies that a one-to-one mapping by harmonic functions is a homeomorphism, and that the inverse mapping is analytic."[149] He is the only mathematician of the four who notices this, because the rest are interested on the properties of real harmonic functions and not their corresponding holomorphic extensions.

Duren expands on Bers' proof by filling in more details, specifically on how the level sets play a role in the beginning of the proof. Unlike Bers, he does not assume holomorphicity anywhere in his proof. In the theorem he gives, he only assumes that a function is complex and harmonic. Other than that, he only uses its harmonic parts in the proof. He avoids using holomorphic functions in the way Bers uses them, because

[149] Bers, 370. Analytic here can be taken to mean holomorphic.
In fact, we can take it further: by the Inverse Mapping Theorem, the mapping is a diffeomorphism—a homeomorphism which is differentiable and also has a differentiable inverse. However, it would not be until 1956 that John Milnor coins the term "diffeomorphism." Some papers present Lewy's Theorem as referring to a diffeomorphism. See, for example Martin, Gaven. "Harmonic Degree 1 Maps are Diffeomorphisms: Lewy's theorem for Curved Metrics." *Transactions of the American Mathematical Society* 368.1 (2016): 647.

he defines a harmonic mapping to be a univalent complex-valued function, which he compares and contrasts to conformal mappings throughout his book. This is different from how the other mathematicians above use "mappings."

V. From Demonstration to Explanation

Lewy's Theorem and the different versions of its proof demonstrate what Rota and Ashenbacher explained about the evolution of successive proofs: the first proof usually lacks explanation and is difficult to follow; while later proofs are easier to follow. In our example, Kneser's proof is part of another proof in which many of the details are left out. His result was in response to Radó's problem, which is also brief. It is not easy to recognize that Kneser's proof is related to Lewy's Theorem as Lewy, Bers, and Duren formulate it. For example, there is no mention of the Jacobian determinant in the first part of the proof. Additionally, the proofs of Kneser and Lewy are very different from each other. These two proofs come from different branches of mathematics—minimal surfaces and partial differential equations. Kneser mostly focuses on the properties of the level sets of univalent mappings. Lewy's proof is full of calculations and rarely makes use of the harmonic properties of the functions he is using. The paper in which Lewy proves his theorem consists only of two theorems and their proofs, so it is difficult to understand why Lewy proves his theorem in the way that he does unless we look at his other papers where he uses his theorem. The same goes for Radó's problem presented in the *Jahresberichte*. We have to look at other sources written a few years later to find out that he used Kneser's result to solve the Plateau Problem.

Returning to the style of Kneser's proof, Bers supplies some of the missing details and uses the properties of holomorphic functions. Although these properties do not play much of a role in the theorem or proof, they are important for the rest of his paper. However, having these properties pointed out, we are able to recognize the different connections between holomorphic functions and harmonic functions. Leading up to the theorem, in Bers' paper, we are also able to see how harmonic mappings are tied to minimal surfaces. We also get a glimpse of how Bers uses methods from complex analysis to tackle problems in partial differential equations and harmonic mappings. This is important, because complex analysts now work on problems in harmonic mapping theory.

Supplying even more details is Duren, who proves Lewy's Theorem in the same way as Bers and Kneser. Unlike Bers, Duren states the theorem in much the same way Lewy presents it. In his book, he comments that he chose Bers' proof over Lewy's because it is "simpler,"[150] and it is closer to how mathematicians now study harmonic mappings, which is by using tools from analysis. Duren spells out many details that are left out of Bers' proof. Again, he begins the proof with the Jacobian determinant and showing how having a critical point implies that the determinant is zero. From Duren's proof, we are able to understand why the Jacobian does not vanish for harmonic mappings. He does not only prove *that* the result is correct, but he also gives a more intuitive understanding about *why* it is correct.

Unless Lewy's paper is cited, a variation of Duren's proof is presented in papers and books. Lewy's Theorem is now thought to be a basic result in harmonic mappings. The theorem is often presented before the Radó-Kneser-Choquet Theorem even though Lewy's proof came a decade after it. Perhaps this is because it is seen as part of the Radó-Kneser-Choquet Theorem due to its standard proof; however, the latter theorem is

[150] Duren, 20.

often individually proved without using Lewy's Theorem. For instance, both Bers and Duren present both theorems and their proofs. Bers proves the Radó-Kneser-Choquet Theorem in much the same brief manner as Kneser after proving Lewy's Theorem; while Duren proves Lewy's Theorem and then goes on to repeat the same line of reasoning to prove the Radó-Kneser-Choquet Theorem, but his version of the theorem involves using the Poisson kernel, which comes from Choquet's result.

In agreement with Rota, mathematicians are not seeking more proofs to Lewy's Theorem. This is because Duren's proof, or those similar to it, is very clear, because he fills in the gaps missing from previous versions of the proof. Instead, mathematicians have moved on to finding other results that are related to the theorem. Further developments after Lewy's Theorem took off in different directions. In the theory of harmonic mappings, there have been attempts to generalize the theorem to higher dimensions. In 1963, Lewy proved that if the Hessian of a real harmonic function in $\mathbb{R}^3$ is zero at some point, then it will take positive and negative values in the neighborhood of this point.[151] Stewart Gleason and Thomas Wolff show that this holds for $\mathbb{R}^n$ in general. However, John Wood proved that Lewy's Theorem fails in dimensions greater than two.[152] In the study of partial differential equations, Erhard Heinz and Friedmar Schulz prove Lewy's Theorem for nonanalytic functions.[153]

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[151] Lewy, Hans. "On the Non-Vanishing of the Jacobian of a Homeomorphism by Harmonic Gradients." *Annals of Mathematics* (1968): 518-529.
[152] Wood, John C. "Lewy's Theorem Fails in Higher Dimensions." *Mathematica Scandinavica* 69.2 (1991): 166-166.
[153] Heinz, Erhard. "On Elliptic Monge-Ampere Equations and Weyl's Embedding Problem." *Journal D'Analyse Mathematique*7.1 (1959): 1-52.
Schulz, Friedmar. *Regularity theory for quasilinear elliptic systems and Monge-Ampere equations in two dimensions*. Vol. 1445. Springer, 2006.

VI. What Can We Gain from Looking at Successive Proofs?

As we follow the evolution of a series of proofs for a theorem, we can observe a few things. The most obvious is that mathematicians prove theorems multiple times. There are a variety of reasons for this,[154] but as more proofs are presented, mathematicians are able to uncover details they have not noticed before or use them to connect different parts of mathematics together. Over time, proofs are improved and become clearer and easier to understand because missing steps are filled in, complicated steps are simplified or broken further down into parts, the language is clearer, and so on.

By going through various proofs, we can track the differences between them. For instance, we have to be aware of changes in language. Often times, definitions are modified during the evolution of successive proofs. Mathematicians apply these changes without warning, so we must be careful when analyzing a proof to know how a mathematical term is used. It is not only the proofs that we must be careful of, but also their corresponding theorems. It is challenging to determine whether or not different proofs belong to the same theorem if there is a long time period between proofs because of the changes in language. However, knowing the background or research of the mathematician whose proof we are considering will give us some clues. The same goes for the different methods used in multiple proofs. The methods in which mathematicians prove theorems have partly to do with what they will use the theorems for. Sometimes, the purpose is clear, but there are also times when only results are

[154] For an interesting overview of why mathematicians prove theorems multiple times, see Dawson, John W. "Why do Mathematicians Re-prove Theorems?" *Philosophia Mathematica* 14.3 (2006): 269-286.

published without much explanation.[155] We have to look beyond these theorems and proofs to compare the differences between them.

This is in contrast to what philosophers have done to form their accounts of mathematical explanation for proofs. Using textbooks as a guide has the advantage of analyzing mathematical proofs that are clear and easy to follow without having to worry about differences in language, because textbooks are designed to be more or less self-contained. However, it has the disadvantage of ignoring how the proofs went through various changes. It also misses the reasons why a mathematician would prove a theorem in a certain way, which in turn overlooks questions mathematicians ask to help with their research. In short, using textbooks as a source to base an account of mathematical explanation is inadequate, because it ignores the practices of the research mathematician.

The methods mathematicians prove theorems with depend on what they need the theorem for. The purposes of using a particular proof vary among mathematicians and the branch of mathematics they are working in. In the example above, we have observed that Radó and Bers used the theorem for their work on minimal surfaces, while Lewy used it for his research in partial differential equations. The styles of their proofs differ from each other according to their background and research. As a consequence, each of their proofs reveals something different about the theorem. The point is that one theorem will not always serve the same function. Because of this, the proofs are also not restricted to one purpose. When philosophers look at multiple proofs to compare their explanatory power, usually the proofs they use are within the same branch of

[155] Lewy's paper that contains his theorem is an example of a paper that is devoid of commentary. His paper consists only of two theorems with their proofs.

mathematics. This gives off the impression that the only way to compare proofs is if they come from the same part of mathematics. However, this is not always the case—especially at the research level. Parts of mathematics are not self-contained; instead, these parts blend together. The research mathematician is free to use theorems outside her area of research if it is relevant for her work. The mathematician's ability to recognize connections between different parts of mathematics and her insight of how they can be applied to her research area plays a role in a proof's explanatory power.[156] This is neglected when we only pay attention to proofs without considering how they might impact a mathematician's understanding.

VII. Conclusion

History plays an important role for understanding how mathematicians go about proving theorems and why some theorems are proved multiple times. As we have observed, looking through a series of successive proofs, we are able to see the changes between them and contexts in which they were produced. Mathematical language—like any language—is not static. Mathematical terms used centuries ago—or even only decades ago—have different meanings than they do today, because definitions are modified and are dependent on mathematical development. When we look at the history of proofs for a theorem, we come to recognize that whether or not a proof is

[156] This goes against the view that proofs should be "pure"—as in the contents of proofs should not stray away from its theorem's content. For example, a theorem of geometry should be proved using geometric methods and not, say, algebraic ones. For many centuries, mathematicians have believed that mathematicians can gain a better understanding through pure proofs rather than impure ones. Since the proofs of Lewy's Theorem come from different parts of mathematics, each proof is considered to be impure, because there is no "correct" branch this theorem belongs to. This is especially so, because although Lewy's Theorem is now regarded as part of harmonic mappings, before it was part of minimal surfaces and partial differential equations. For an interesting analysis of purity in mathematics, see Detlefsen, Michael, and Andrew Arana. "Purity of Methods." *Philosopher's Imprint*. 11.2 (2011): 1-20.

explanatory is dependent on its audience, and not just on its content or form. For example, on the one hand, a student, say, learning about Lewy's Theorem for the first time may find Duren's proof much easier to understand than Kneser's proof, because Duren fills in missing steps and uses modern terminology. On the other hand, a mathematician who is familiar with partial differential equations may prefer Lewy's proof. Thus, philosophers must go beyond theorems and their proofs and consider the mathematicians who are, after all, the ones who are developing their discipline.

CHAPTER FOUR:
CASE STUDY—THE FOUR COLOR THEOREM

So far, I have stressed the importance of history and current mathematical practices of explanatory proofs to better understand what mathematicians desire in their proofs of theorems. Knowing the history of mathematics involves following both actual historical events as well as history used in mathematical research, which differs from an accurate view of the past as it concentrates more on technical developments. The past gives us a view of how mathematicians have developed their discipline based on their version of history. Recognizing current mathematical practices helps philosophers formulate their theories that are in agreement with what mathematicians do. Regarding mathematical explanation, we find that theories that focus only on form and content of proofs are inadequate, because they do not consider external factors such as a proof's purported audience. The case studies that have been used for theories of mathematical explanation come from textbooks, which typically emphasize explanation. This gives off the impression that if a proof is considered to be explanatory, it will be explanatory for everyone, which is certainly not the case.

In this chapter, I will use the Four Color Theorem as a case study to investigate the differences between how philosophers and mathematicians have reacted to its proof. First I will briefly sketch the Four Color Theorem Proof. I will then focus on what philosophers have said about the proof of the Four Color Theorem. Next, I will turn to the worries of mathematicians. After that, I will consider the responses from philosophical and mathematical communities to each other's concerns. I will argue that the main philosophical worry regarding the theorem did not make a strong impact on the mathematical community and would have hindered mathematical development in computer-assisted proofs. Last, by looking at other proofs of the Four Color Theorem, I will highlight the differences between them and Appel and Haken's proof. This will give us an idea of what mathematicians have improved from the original proof.

I. A very brief sketch of the proof

In 1976, Kenneth Appel, Wolfgang Haken, and John Koch proved the Four Color Theorem. The theorem states that no more than four colors are required to color a planar map such that adjacent countries are not the same colors. The significance of the proof is that it is regarded as the first major proof that relies on the use of a computer. The computer was used for a lemma to generate and check 1,482 maps—an undertaking too large for any human mathematician to accomplish by hand.

The proof of the Four Color Theorem is quite lengthy due to the large number of different configurations that have been verified. However, there are only a few steps for the proof. Besides the numerous test cases, Appel and Haken use Kempe's results from 1879. Although flawed, it contained the general ideas of how to go about proving the theorem. In this section, I will present a sketch of Appel and Haken's proof.

A few definitions will be helpful to understand the details of the proof. A planar graph is a collection of vertices and edges that lie on one plane. Vertices are points in the graph that are connected by non-intersecting lines, which are called edges. We are able to transform a normal map into its corresponding graph, where each country is represented by a vertex, and each edge shows which countries are adjacent. A normal map is one that has no country surrounding another (so it is simply-connected) and has at most three countries meeting at a point[157]. Transforming a normal map into its corresponding planar graph is called triangulation (since the faces that are formed by the vertices and edges are triangles). The degree of a vertex is the number of edges that end at the vertex, and this represents the number of neighbors a country has. Lastly, a map is maximal if it contains the highest possible number of non-intersecting edges.[158] The Four Color Theorem, using these terms, can be stated as "In a maximal planar graph, we need only four colors to color each vertex such that every adjacent vertex is a different color."

Much of the theoretical ideas of the proof come from the work of Alfred Kempe and Heinrich Heesh.[159] Although Kempe's proof is flawed, he was able to show that in a normal graph, each vertex can have no more than five edges. From this fact, Appel and Haken were able to build up a set of unavoidable configurations. This unavoidable set is a collection of graphs such that at least one of its graphs appears in every maximal planar graph of five degrees or lower.

In order to create this set, Appel and Haken use their "discharging procedure." This method comes from the work of Heesch, who initially had the idea of assigning positive and negative charges to each vertex of the graph and redistributing them

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[157] For instance, Florida, Georgia, and Alabama share a point. Two examples of what is not normal are Michigan, which consists of two separate regions, and the four states Utah, Colorado, Arizona and Mexico, which meet at a point. Although these are states and not countries, the idea is still the same.
[158] Here, more edges are added as long as none of them intersect. For example, although California and Texas are not neighboring states, they can be connected with an edge that does not intersect any other edge. Any planar graph is contained in a maximal planar graph.
[159] See Kempe, Alfred B. "On the Geographical Problem of the Four Colours." *American Journal of Mathematics* 2.3 (1879): 193-200, and Heesch, Heinrich. *Untersuchungen zum Vierfarbenproblem*. Vol. 810. Bibliographisches Institut, 1969.

without changing the overall sum of charges. Charges are assigned to each vertex using the formula: $6 - \deg(v)$. From this, Appel and Haken then either discharge or overcharge some of the vertices in the graphs according to a list of 487 rules, which make up the discharging procedure, while keeping the sum total of charges constant. Since the total sum of charges is positive,[160] there will always be vertices that are positive. From these vertices with positive charges, they were able to generate a list of 1,482 unavoidable configurations.

The second step to the proof is reducibility, which basically shows that a configuration in the unavoidable set cannot be part of a graph that can be colored with at least five colors. Kempe correctly proved that this further implies that there are no graphs that require five colors. So, once reducibility is shown, the proof is done. This part of the proof relies heavily on a computer program made by John Koch, who tested reducibility on small ring sizes. [161] Appel was able to modify his program to work with up to 14 ring size. This part of the proof took up approximately 1,200 hours of computer time.[162]

II. Philosophical Issues with the Proof

The work of Appel, Haken and Koch gave rise to a discussion within the philosophy of mathematics. The traditional idea of what counts as a mathematical proof

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[160] This result comes from Euler's polyhedral formula: $v - e + f = 2$, where v is the number of verticies, e is the number of edges, and f is the number of faces. In this case, through triangulation of normal graphs, since for every edge, there are two faces and for every face, there are three edges, we obtain $2e = 3f$. Summing over all vertices, we then have $\sum_i^M (6 - i)v_i$ = 12, where M is the maximum degree of the vertices. In other words, the sum of the charges is 12, which, of course, is positive.

[161] Ring size corresponds to the number of neighboring countries, or the number of neighboring vertices. Appel and Haken prove that at least a 14-ring size is necessary to check. Thus, there is no need to check cases with larger ring sizes. See Appel, Kenneth, and Wolfgang Haken. "Every Planar Map is Four Colorable. Part I: Discharging." Illinois Journal of Mathematics 21.3 (1977):478.

[162] Appel, Kenneth and Wolfgang Haken. "The Solution of the Four-Color-Map Problem." *Scientific American* 237 (1977): 121.

was thought by some to be insufficient to cover cases such as the Four Color Theorem, and led some philosophers to question the a priori status of mathematics. Mathematicians also had some concerns about the Four Color Proof; however, they concentrated on its reliability and lack of explanatory power. Although philosophers and mathematicians acknowledged each other's main concerns, there seems to be a disconnection between what philosophers have said versus what mathematicians have said about computer-assisted proofs in general.

In 1979, Thomas Tymoczko wrote "The Four Color Problem and Its Philosophical Significance."[163] This was the first major philosophical essay on Appel, Haken, and Koch's proof of the Four Color Theorem. He set the stage for philosophers and mathematicians to consider whether or not the traditional concept of proof conflicted with mathematical practices. In his essay, Tymockzo argues that the Four Color Proof is an empirical proof, because the work done by computer is unsurveyably long—no mathematician is able to check its results by hand. Since the proof is unsurveyable, in order to verify the correctness of the proof, he argues that we must make sure that there are no computer hardware or software defects. In other words, we have to consider the physical workings of the computer and confirm that the findings of the computer do not contain any errors. Such empirical considerations lie outside of what Tymoczko believes to be traditional mathematical methods. Thus, he claims that the proof of the Four Color Theorem is "a traditional proof with a…gap, which is filled by the results of a well-

[163] Tymoczko, Thomas. "The Four-Color Problem and its Philosophical Significance." *The Journal of Philosophy* 76.2 (1979): 57-83.

thought-out experiment."[164] From this view, mathematics is seen as an empirical science, contrary to the traditional belief that mathematics is an a priori discipline.

According to Tymoczko, a traditional mathematical proof is convincing, formalizable, and surveyable. The reason he gives as to why proofs are convincing (to any mathematician) is that they are both formalizable and surveyable. A proof is formalizable if it is possible to construct a formal deduction from the axioms to the conclusion. Tymoczko defines a surveyable proof as "a construction that can be looked over, reviewed, verified by a rational agent."[165] Surveyability is important for mathematicians, because

> [t]he proof relates the mathematical known to the mathematical knower, and the surveyability of the proof enables it to be comprehended by the pure power of the intellect—surveyed by the mind's eye, as it were."[166]

This means that there is nothing external to the proof that is needed for a mathematician to understand what is being proved. Because of this, Tymoczko claims that we have a priori knowledge of (proved) mathematical propositions.

Many philosophers and mathematicians have written against Tymoczko's claim that the proof of the Four Color Theorem is empirical because it is unsurveyably long. Although they agree that presently no mathematician is capable of looking over the proof, they disagree that either lacking human surveyability renders the proof empirical, or it must be surveyed by a mathematician in order to count as a proof. While some philosophers agree with Tymoczko that the proof is empirical, there are others who claim that it is a priori despite its reliance on a computer. For them, the Four Color

[164] Ibid., 58.
[165] Ibid., 59.
[166] Ibid., 60.

Proof is a traditional proof. Here, I will present some of the main arguments against Tymoczko's views.

Since the Four Color Proof cannot be checked by a human being, Tymoczko states that we must look into how the computer works to ensure that there are no errors in the implemented computer program. This requirement is what is supposed to make the Four Color Theorem novel: it is the first empirical proof.[167] However, Michael Detlefsen and Mark Luker challenge this claim.[168] Although they agree with Tymoczko that the Four Color Proof is empirical, they deny that its unsurveyability is the cause. Instead, they argue that empirical methods in mathematics are common. Like Tymoczko, they consider surveyability as part of a proof, and any kind of checking—by hand or by computer—is an empirical task. If Tymoczko is correct to say that a proof "needs nothing outside of itself to be convincing,"[169] then one must survey the proof as a whole to understand and be convinced by it—checking must be considered as part of the proof.[170] For instance, when proofs contain computations, one must check that the computed results are correct. Specifically, we need to make sure "that the computing agent correctly executes the program," and "that the reported result was actually obtained."[171] (Here, the computing agent can either be a mathematician or a computer.)

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[167] Tymoczko's claim that the Four Color Proof is the first computer-assisted proof is false. The first known computer proof was in 1954 by Martin Davis, who proved that the sum of two even numbers is even. In the same decade, mathematicians started to use computers to find Mersenne primes. See O'Leary, Daniel J. "Principia Mathematica and the Development of Automated Theorem Proving." *Perspectives on the History of Mathematical Logic*. Birkhäuser Boston, 2008. 47-53. Robinson, Raphael. "Mersenne and Fermat Numbers." *Proceedings of the American Mathematical Society* 5.5 (1954): 842-846.
[168] Detlefsen, Michael, and Mark Luker. "The Four-Color Theorem and Mathematical Proof." *The Journal of Philosophy* 77.12 (1980): 803-20.
[169] Tymozcko, 59.
[170] Detlefsen and Luker, 810.
[171] Ibid., 808.
Resnik is also on board with their view regarding the empirical status of the Four Color Theorem. See Resnik, Michael D. "Computation and Mathematical Empiricism." *Philosophical Topics* 17.2 (1989): 130.

Therefore, Detlefsen and Luker conclude that traditional proofs rely on empirical considerations even though they are surveyable.

Some philosophers deny that surveyability by a mathematician is a requirement for proof. There is no reason to require that only *one* mathematician surveys the proof. Margarita Levin suggests that since all proofs have a finite number of steps, a lengthy one could be divided up into parts and distributed among a group of mathematicians.[172] However, for a proof such as the Four Color Proof, it seems highly unlikely that a group of mathematicians would be willing to spend its time checking the proof by hand. According to Appel, the computer report for the reducibility lemma consists of 30,000 pages.[173]

Instead of relying on a mathematician to hand-check unsurveyable proofs, computers can and are used to check for errors. The computer program can be run multiple times and on different computers to compare the findings of each instance. Different programs can also be written to test against the results. Since Appel, Haken, and Koch wrote their program in assembly language, they were not able to use computer verification methods. To check their work, they compared the computer reducibility results of other mathematicians,[174] who wrote their own programs on different computers, yielding the same results.

According to Paul Teller and Mark McEvoy, surveyability has no bearing on whether or not something is a proof. Teller writes, "Surveyability is needed, not because without it a proof is in any sense not a proof (or only a proof in some new sense), but

[172] Levin, Margarita R. "On Tymoczko's Argument for Mathematical Empiricism." *Philosophical Studies* 39.1 (1981): 84
[173] Appel, Kenneth. "The Use of the Computer in the Proof of the Four Color Theorem." *Proceedings of the American Philosophical Society* 128.1 (1984): 39.
[174] Frank Allaire, E. R. Swart, Heinrich Heesch and Karl Durre. Ibid., 39.

because without surveyability we seem not to be able to verify that a proof is correct. So surveyability is not part of what it is to be a proof in our accustomed sense." (798).[175] McEvoy, in agreement with Teller, writes that if we are unable to survey a proof, then it is because "*we are [unable]* to tell whether a proof is error-free…" [emphasis in the original].[176] Instead of being part of the proof, surveyability then only applies to the checking process, which is independent of the proof itself. Teller points out that the ability to review a proof is not an all-or-nothing proposition—there is a sliding scale of surveyability: while there are proofs that anyone can follow, there are also proofs that only a few mathematicians can understand. It is then no large jump to say that there are proofs that are beyond the understanding of the best mathematicians. Mathematicians use tools like pencil and paper and calculators to check a proof. Teller writes that these tools are important, because "the limits of any one mathematician's powers of surveying depend among other things on which tools he or she uses."[177] He and James Fetzer explain that the computer extends the way that we are able to perform and check calculations much in the same way a microscope extends our vision capabilities.[178] If pencil and paper are acceptable to use under Tymoczko's requirements, then, Teller

[175] Teller, Paul. "Computer Proof." *Journal of Philosophy* 77 (1980): 798.
Arkoudas and Bringsjord also point out the difference between a proof and proof-checking. See Arkoudas, Konstantine, and Selmer Bringsjord. "Computers, Justification, and Mathematical Knowledge." *Minds and Machines: Journal for Artificial Intelligence, Philosophy, and Cognitive Science* 17.2 (2007): 189.
[176] McEvoy, Mark. "The Epistemological Status of Computer-Assisted Proofs." *Philosophia Mathematica* 16.3 (2008): 381.
[177] Ibid.
[178] Teller focuses on the testing of the computer as a machine, while Fetzer claims that this holds for general programing in mathematics. However, this is strictly concerning calculations and not using a computer for diagramming and visualization purposes.
See Teller, 86 and Fetzer, James H. "Program Verification: the Very Idea." *Communications of the ACM* 31.9 (1988): 1062.

concludes, a computer should also be permissible, because the method of checking—as long as it is reliable—has nothing to do with the contents of the proof.[179]

Levin considers that Tymoczko is not bothered by the lack of surveyability as much as the disconnection between the computer and mathematician. According to Tymoczko, no human being is able to understand how the computer generated its results for the Four Color Theorem. However, this is false. Mathematicians—specifically Appel and Koch—wrote the computer program used in the reducibility lemma. They obviously had an idea of what the computer was expected to do in executing their program. Computers are often used to quickly perform difficult or numerous calculations. While there is a difference in the quantity of calculations, Levin concludes that there is no qualitative difference between using a pencil and paper and using a computer. Edward Swart adds that the same rules of logic are used for both work done by pencil and paper and by computer.[180] Thus, the computer should be seen as a tool, much like pencil and paper. Consistent with Teller's stance that any reliable tool may be used to check a proof, the tools used in proofs are independent from the proofs themselves.

In order to combat the claim that the use of computers is just a shortcut for obtaining results, Tymoczko likens this appeal to computer to a situation in which a

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[179] He does not distinguish between using computer results within a proof and using the results for checking purposes. However, he mentions that the proof was dependent on the computer survey of different combinations.

[180] Swart, Edward R. "The Philosophical Implications of the Four-Color Problem." *The American Mathematical Monthly* 87.9 (1980): 703.
Arkoudas and Bringsjord also hold that the choice of tool is irrelevant: "Indeed, the particular hardware platform on which an algorithm is implemented is entirely immaterial, as long as we have good reason to believe that the underlying physical mechanism is reliable" (193).
Van Theemat agrees that the choice of tools has nothing to do with the proof itself. See Van Themaat, W. A. Verloren. "The Own Character of Mathematics Discussed with Consideration of the Proof of the Four-Color Theorem." *Zeitschrift für allgemeine Wissenschaftstheorie* 20.2 (1989): 347.

Martian society relies on a method called “Simon says,” which is an appeal to authority. Simon is a mathematically-inclined alien who is able to produce mathematical results. The Martians, believing that Simon is always correct, would justify his results based only on his authority. They even use “Simon says” for results that Simon did not offer.

According to Tymoczko, appeal to Simon is the same as appeal to computer: mathematicians are unable to hand-check lengthy computer generated results, but these results are used anyway. So, Tymoczko says that either mathematicians accept the computer’s output much like the Martians’ “Simon says” method, or they must provide evidence for the reliability of the computer's work in their proofs. Clearly, it would be bizarre for mathematicians to blindly accept a computer's results, so concerns about the reliability of the computer must be addressed. Such empirical considerations, Tymoczko says, introduces a new method to mathematics that is external to the traditional concept of mathematical proof.

It is easy to see that Tymoczko’s analogy does not match up with what mathematicians do; there is just no equivalent in mathematics to “Simon says,” because the utility of computers does not introduce a new method into mathematics, let alone an appeal to authority.[181] Since the computer is a machine that has been made by human beings based on the mathematics that we have developed, we are able to find out how it obtained its results. Whereas Simon’s inner workings remain mysterious, we are able to understand why the computer generates the output it does. Teller writes that “the theory, practical execution, and reliability of the known methods of computation

[181] One mathematician agrees with this analogy. In describing computer-assisted proofs, Joseph Auslander says that since testing a program does not always reveal errors, “[a]t some point in the proof, a result is true because the computer ‘said so.’” See Auslander, Joseph. "On the Roles of Proof in Mathematics." In B. Gold & R. A. Simons (Eds.), *Proof and Other Dilemmas: Mathematics and Philosophy*. Washington: Mathematical Association of America (2008): 70.

executed by the computer are all open for inspection in as much detail as desired by anyone who cares to investigate."[182] He emphasizes that what is more important than results calculated by the computer are the methods involved in obtaining these findings. The mathematician William Thurston brings up this same point:

> [I]t is common for people first starting to grapple with computers to make large-scale computations of things they might have done on a smaller scale by hand. They might print out a table of the first 10,000 primes, only to find that their printout isn't something they really wanted after all. They discover by this kind of experience that what they really want is usually not some collection of 'answers'—what they want is *understanding* [emphasis in the original].[183]

Instead of the unsurveyability of the computer results, the focus should be placed on the mathematical reasoning that produced these results.

While philosophers and mathematicians in general believe that the Four Color Proof is either a traditional or empirical proof, Stuart Shanker argues that it is not a proof at all. He believes that the worries Tymoczko raises and the objections which followed are irrelevant to what counts as a proof. Shanker agrees with Tymoczko that the reducibility lemma is unsurveyable, but his idea of surveyability differs from Tymoczko's. Following Wittgenstein, Shanker holds that surveyability applies to the laws that are used in composing the proof and not the large quantity of calculations done by the computer. In other words, the proof's form is what is important for surveyability and not its content. Thus Tymoczko's argument that because the

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[182] Teller, 799.
[183] Thurston, William. "On Proof and Progress in Mathematics." *Bulletin of the American Mathematical Society* 30.2 (1994): 162.

reducibility lemma is too long to be looked over by any mathematician has nothing to do with the question of whether or not Appel, Haken and Koch's work counts as a traditional or empirical proof.[184]

Against Teller and agreeing with Tymoczko, Shanker claims that surveyability must be part of a proof. In order to understand a proposition, he argues, we must be able to survey it, but the point is not "*how a proof is checked*, but rather *how a proposition is used*."[185] Without knowing how a proposition is used, according to Wittgenstein, we are unable to understand what is being proved; hence, there would be no proof without surveyability in this Wittgensteinean sense.

The problem with the Four Color Proof, as Shanker sees it, is that we are unable to understand how the computer is able to generate the unavoidable set of reducible configurations.[186] The distinction between experimentation and mathematical proof, according to him, is blurred.[187] While a proof is supposed to be perspicuous, we have to rely on a computer for the unavoidable set without being provided any rule for how the computer generated its results. Shanker points out that Appel and Haken were sometimes amazed at the computer's findings. Two years into working on their results with a computer, they were surprised that the computer

> would work out compound strategies based on all the tricks it had been 'taught' and often the approaches were far more clever than those we would have tried. Thus it began to teach us things about how to proceed

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[184] Empirical proofs are not proofs under Shanker and Wittenstein's views.
[185] Shanker, Stuart. *Wittgenstein and the Turning Point in the Philosophy of Mathematics*. Albany: State University of New York Press, (1987): 142.
[186] This is the set of normal maps such that every triangulation of a sufficiently large map contains at least one of these configurations.
[187] Shanker, 143.

> that we never expected. In a sense it had surpassed its creators in some aspects of the 'intellectual' as well as the mechanical parts of the task.[188]

Since there is no rule provided on how to create the unavoidable set, the rule that was used remains a mystery. As a result, Shanker claims, "I cannot understand a mathematical construction as a *lemma* unless I understand the proof underlying it."[189] Since the computer only provides a description of the unavoidable set, the lemma is more of an experiment than a proof. So, Shanker concludes that Appel, Haken, and Koch's work is an experiment and not a mathematical proof.

It is too strict of a requirement to need to know the rule used in the lemma's proof. Shanker (and Tymoczko) makes it seem as if the computer created the unavoidable set on its own without any assistance from Appel, Haken, and Koch. However, they wrote the code to be used by the computer. In other words, they gave it a set of instructions to follow in order to generate the set. Although the program is lengthy, it is still surveyable—its creators looked over the code. They may not have been able to come up with the same results by hand, but the rule used by the computer is no different from the one that would be used if the unavoidable set had to be generated by hand. Despite the fact that the specific rule for the proof of the lemma cannot be grasped, Appel, Haken, and Koch are still able to describe what restrictions they imposed on the computer.

This means that mathematicians have to settle for a sketch of a proof to understand it at all. In regard to the reducibility lemma for the Four Color Theorem, Haken observes, "Our proof is logically quite simple but combinatorially complicated

[188] Appel, Kenneth and Wolfgang Haken. "The Solution of the Four-Color-Map Problem," 117.
[189] Shanker, 153.

through a very large number of case-distinctions."[190] So, on the one hand, the methods used for the proof are surveyable, but on the other hand, the physical output of the computer results make the proof impossible to directly follow. O. Bradley Bassler says that there are two types of surveyability going on here: global and local.[191] He defines global surveyability as requiring "the surveying of the entire proof as a comprehensible whole; while local surveyability "requires the surveying of each of the individual steps in a proof in some order."[192] He argues that philosophers conflate global surveyability with local surveyability. Tymoczko is clearly guilty of doing this. He believes that a mathematical proof must be surveyable at the local level in order to understand the theorem. However, this is not always the case. There are instances in which one can follow each step of a proof but not be able to know how it is connected to its theorem. The reason Tymoczko focuses on local surveyability is because for him proofs must also be formalizable—the conclusion of a proof is obtained through use of deductive rules and a set of axioms. In other words, we must be able to construct a proof without missing any logical steps. This implies that we survey each step of the proof. Undoubtedly, this cannot be done with the Four Color Proof.

Yet, it is easy to follow Appel, Haken, and Koch's proof. We are able to understand the reasoning behind their reducibility lemma. If we consider the computer's work as routine calculations, combined with the fact that the computer is just a tool—no different from pencil and paper—then we *can* survey the proof. This shows that it is possible to have global surveyability without requiring local surveyability.

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[190] Haken, Wolfgang. "An Attempt to Understand the Four Color Problem." *Journal of Graph Theory* 1.3 (1977): 193.
[191] Bassler, O. Bradley. "The Surveyability of Mathematical Proof: A Historical Perspective." *Synthese* 148.1 (2006): 99-133.
[192] Ibid., 100.

Haken's assertion that the proof is easy to understand yet difficult to do line by line reflects this reasoning.

The Four Color Theorem is an example of a proof that is globally surveyable, while being locally unsurveyable. On the surface, it seems as though we can easily separate the two. Bassler believes that we can, but also says that global surveyability comes in different degrees. We can compare Appel and Haken's published proof in the *Illinois Journal of Mathematics* with their discussion about it in an article in *Scientific American*. Their proof in the journal is technical and difficult to follow without a background in graph theory. The article, however, caters to a general audience—much of the technical detail is suppressed, but Appel and Haken outline the entire proof in a way that can be easily followed. Bassler would say that the journal publication has a "finer level of grain" than the popular article, but it is still only a global survey of the proof.[193]

Local surveyability demands that each line of a proof be surveyed. In order to do this, the proof must be rigorously formalized. This is to show that there are no slips in logic and that the proof is a valid one. Further, a formalized proof is transparent because every deductive step is explicitly stated. Tymoczko says that formalizability contributes to the convincingness of a proof—the mathematician is certain that the proof's theorem is true (or at least follows deductively from the axioms). However, in practice, most mathematicians do not bother with formalizing their proofs. Some of them point out that doing so makes them longer and harder to understand, because they

[193] Ibid., 125.

become cluttered with details that are usually left out of informal proofs.[194] Formalizing a proof is not an easy process either—especially because no details can be left out. Many theorems are built from previous mathematical development and not straight from the axioms, making their formalized proofs extremely long and tedious. It would be very easy to get lost in the details of such proofs, preventing one from being convinced that a proof is correct. Thus, traditional proofs are not usually formalized. So it seems that Bassler's local surveyability does not apply to most traditional proofs. Instead, the vast majority of traditional proofs can only be surveyed globally.

Tymoczko believes that traditional proofs are more reliable than computer-assisted proofs, because they can be checked by hand: "The reliability of the Four Color Theorem…is not of the same degree as that guaranteed by traditional proofs, for this reliability rests on the assessment of a complex set of empirical factors."[195] However, a proof that has been surveyed by a mathematician or even a group of them is not guaranteed to be correct; mathematicians are fallible.[196] Mathematicians who have worked with computer-assisted proofs believe that the computer is at least as reliable—if not more reliable—than a human being.

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[194] For example, Robinson says that "too much detail causes difficulty in viewing the big picture: one cannot discern the forest for the trees." See Robinson, J. A. "Proof = Guarantee + Explanation," in S. Holldobler (ed.), *Intellectics and Computational Logic: Papers in Honor of Wolfgang Bibel*. Kluwer Academic Publishers, Dordrect and Boston (2000): 279.
DeMillo, Lipton, and Perlis write, "We often use 'Let us assume, without loss of generality…' or 'Therefore, by renumbering if necessary… to replace enormous amounts of formal detail. To insist on the formal detail would be a silly waste of resources." See De Millo, Richard A., Richard J. Lipton, and Alan J. Perlis. "Social Processes and Proofs of Theorems and Programs." *Communications of the ACM* 22.5 (1979): 278.
[195] Tymoczko, 74.
[196] Even the history of the Four Color Proof shows that mistakes can go undetected through hand-checking for a long period of time. In 1879, Alfred Kempe presented a "proof" of the Four Color Theorem, which was accepted until 1890, when it was shown to be incorrect.

III. Mathematicians' Concerns with the Proof

Ten years before Tymoczko's essay, and seven before the publication of Appel, Haken, and Koch's proof, Elsie Cerutti and P. J. Davis raised the same worry as Tymoczko regarding the reliability of their own computer-assisted proof of Pappus' Theorem.[197] They acknowledge that there may be errors in proofs using computers, but traditional proofs may also contain errors. "Human processing is subject to such things a fatigue, limited knowledge or memory, and to the psychological desire to force a particular result to 'come out.'"[198] To highlight his point that mathematicians often make errors, Davis, in another essay, presents a long list of published materials from 1860 through 1970 all of which contain mathematical mistakes.[199] In the discussion of their Four Color Proof, Appel and Haken note that

> even when hand-checking is possible, if proofs are long and highly computational, it is hard to believe that hand-checking will exhaust all the possibilities of error. Furthermore, when computations are sufficiently routine, as they are in our proof, it is probably more efficient to check machine programs than to check hand computations.[200]

Lastly, in response to Tymoczko's essay, Swart, who has himself worked with computer-assisted proofs, maintains that computers are more reliable than human beings. Compared to human beings, "[c]omputers do not get tired and almost never introduce

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[197] Cerutti, Elsie, and Philip J. Davis. "Formac Meets Pappus: Some Observations on Elementary Analytic Geometry by Computer." *The American Mathematical Monthly* 76.8 (1969): 895-905.
[198] Ibid., 903.
[199] Davis, Philip J. "Fidelity in Mathematical Discourse: Is One and One Really Two?" *The American Mathematical Monthly* 79.3 (1972): 260-3.
Also see: Frans, Joachim, and Laszlo Kosolosky. "Mathematical Proofs in Practice: Revisiting the Reliability of Published Mathematical Proofs." *Theoria: An International Journal for Theory, History and Foundations of Science* (2014): 345-360.
[200] Appel, Kenneth and Wolfgang Haken, 121.

errors into a valid implementation of a logically impeccable algorithm."[201] He believes that Appel, Haken and Koch's reducibility lemma proved by computer is more reliable than their discharging procedure, which was all done by hand.[202] However, he points out that Frank Allaire independently proved the Four Color Theorem, also by computer, with the same results as Appel, Haken and Koch. This gives further support to the claim that the Four Color Theorem is true. From this, it is evident that the mathematicians who work with computer-assisted proofs believe that the computer is a reliable tool to do mathematics.[203]

Despite being (locally) unsurveyable by a mathematician, the Four Color Proof was accepted as a proof by the mathematical community without much hesitation. Different computers were used to confirm that the proof is correct, and there are now multiple proofs for the Four Color Theorem, which have all been done by computer. The latest was by Georges Gonthier and Benjamin Werner (2005), who produced a fully formalized proof, which was checked using a proof assistant called Coq. It is evident that the Four Color Theorem is indeed true.

Mathematicians are not as worried about human surveyability of proofs as they are with reliability of computer results. Tymoczko claims that since the Four Color Proof is not surveyable by a human being, we should not be confident that it is reliable either. However, given that mathematicians believe that the computer is a reliable tool, surveyability by computers is sufficient (as well as necessary, since hand-checking is not currently possible). In order to check results, programs are executed multiple times on

[201] Swart, 700.
[202] Appel, Haken and their families hand-checked approximately 480 pages for errors.
[203] Robin Thomas, one of the mathematicians who worked on a later proof of the Four Color Theorem, also agrees that a computer is more reliable than a human—especially considering the enormous amount of checking involved with the Four Color Proof. See Thomas, Robin. "An Update on the Four-Color Theorem." *Notices of the AMS* 45.7 (1998): 848-859.

multiple computers. Also, different programs are written to compare results. In fact, the referees of Appel, Haken, and Koch's work used their own computers and programs to check the results for the reducibility lemma. While there are concerns about computer hardware and software defects, repeatability of programs increases confidence that the computer output is correct.

This seems to be a new method for checking mathematical proofs—but repeatability has been used in traditional mathematics as well. Mathematicians do not often repeat a traditional proof multiple times to check if it is correct. Instead, repeatability here refers to having different proofs for the same theorem.[204] Usually one proof is enough to convince mathematicians that a theorem is true, but multiple proofs provide further ways to show why or how it is correct. It may be argued that this kind of repeatability is different from repeatability for computer proofs. On the one hand, the results of a computer proof are checked against a different program's output; these results are internal to the proof. On the other hand, having multiple traditional proofs is an external "check" for any one particular proof. However, I am only concerned with confidence in reliability here. The fact that mathematicians are able to replicate the results of a proof is the same in both cases.

The bigger issue for mathematicians is that lengthy computer-assisted proofs appear to lack explanatory power. They cannot be humanly surveyed, at least not in the sense of Bassler's local surveyability, so it is obviously not possible to understand them directly as with traditional shorter proofs. One of the ideal goals of a proof is to not only convince someone that its conclusion is true, but also to show why it is true. A computer

[204] For instance, there are hundreds of proofs for the Pythagorean Theorem. Elisha Loomis gives 370 different proofs. See Loomis, Elisha S. *The Pythagorean Proposition: Its Demonstrations Analyzed and Classified and Bibliography of Sources for Data of the Four Kinds of "Proofs."* Washington: National Council of Teachers of Mathematics. 1968.

proof tends to only provide an answer that a mathematical theorem is true, providing no explanation of why. A "good" traditional proof contains enough steps to guide the mathematician. There are often gaps found in proofs for a variety of reason—to save space, because the gap can easily be filled without much effort, it was overlooked, and so on—and it is up to the mathematician to fill in these gaps. A computer proof, which tends to be lengthy, can be considered as one large gap that is not easily filled in by mathematicians. This leaves some of them unsatisfied with computer proofs.[205]

However, the proof of the Four Color Theorem in particular is not a completely computerized proof. The first part of their proof, which collects the unavoidable configurations, was done by hand. It is only the testing for reducibility that required a computer. While it is true that the results of the computer do not explain why these configurations are reducible, the method used to generate the set of unavoidable configurations also lacks explanatory power, because the discharging process is based on a large set of rules that must be satisfied. In other words, it is not that Appel and Haken's proof is nonexplanatory because it was done by computer but because of the methods involved in the proof. The general two-step strategy of first finding an unavoidable set and then checking for reducibility came from Kempe's flawed proof from 1879. Unlike Kempe's list of three major cases to consider, Appel and Haken found close to 1,500 cases.

Despite their massive lengths, computers proofs are still being written, in part because they have fewer errors than traditional proofs. Now, some traditional proofs can be formalized, especially with current computer technology. It is still a challenging task, but developments have been made to construct proofs using computer proof

[205] However, they do not outright deny that these are not proofs.

assistants and proof checkers. Coupled with the fact that computers are very reliable, mathematicians are able to successfully construct and check formal proofs in a reasonable amount of time.[206] An advantage of computer proofs, according to John Harrison, is that there are fewer opportunities for error than in hand-checked proofs. The code for proof checking software is short enough that it is easy to discover and get rid of bugs. Because of this, Harrison claims that since a proof is supposed to show that a theorem is true, as well as explain why it is true, we can focus on the explanation without being troubled by errors. As for the length of these proofs, he says that "a computer program can offer views of the same proof at different level of detail to the differing needs of readers."[207]

IV. Two Later Proofs of the Theorem

Although there was much criticism against the computer proof of Appel and Haken, two more computer proofs of the Four Color Theorem have been created. The second generation proof written in 1997 by Neil Robertson, Daniel Sanders, Paul Seymour and Robin Thomas is very similar to the one created by Appel and Haken. Specifically, it uses a reducible set of unavoidable configurations that were generated using a discharging procedure. However, relying on 633 configurations, their set is much smaller than Appel and Haken's 1,482 configurations, and the number of rules for discharging has decreased from 487 to 32.[208] According to Robertson, Sanders,

[206] It took Gonthier and Werner five years to create a fully formal proof for the Four Color Theorem.
[207] Harrison, John. "Formal Proof—Theory and Practice." *Notices of the AMS* 55.11 (2008): 1400. Thomas Hales agrees with Harrison that the number of errors found in formal computer proofs is smaller than in traditional roofs, but adds that they are much harder to follow. See Hales, Thomas. "Formal Proof." *Notices of the AMS* 55 (2008): 1371.
[208] Robertson, N., Sanders, D., Seymour, P., and Thomas, R. "The Four-Color Theorem." *Journal of Combinatorial Theory, Series B* 70.1 (1997): 2-44.

Seymour, and Thomas, their proof is much simpler and easier to check than that of Appel and Haken. While Appel and Haken requires hand-checking the unavoidable sets, the second-generation proof formalizes these sets so that they may be checked quickly with a computer.[209] The third proof is by Georges Gonthier with the aid of Benjamin Werner in 2005.[210] What sets his proof apart from the other two is that it is a formal proof. In the first two proofs, the theoretical parts were not formalized, and only the cases were tested by computer. Gonthier's entire proof is able to be checked by the proof checker software, Coq—there is no need to check any part of the proof by hand. His work relies on Robertson et al., but his proof does not use planar graphs, because Coq is unable to "read" diagrams. Instead, Gonthier "translates" the topological definition of planar graph into one from combinatorics called a hypergraph, which is compatible with Euler's formula, but uses permutations. By using these hypergraphs, he is able to formalize the unavoidable set of the second proof.

These two later computer proofs did not face the same criticisms as Appel and Haken's proof. Instead, according the community of mathematicians, they provide further justification that the Four Color Theorem is true. According to Thomas Hales, "[a]s a result of Gonthier's formalization, the proof of the four-color theorem has become one of the most meticulously verified proofs in history."[211] This is despite the fact that Gonthier's computer proof is checked by computer; however, due to its formalization, there are no missing steps. Even though the computer does much of the heavy lifting in these proofs Gonthier and John Harrison (a computer scientist) believe that formal proofs provide explanation to answer why a theorem is true, because every

[209] Ibid., 4.
[210] See Gonthier, George. A Computer-Checked Proof of the Four Colour Theorem. Technical Report, Microsoft Research Cambridge (2005): 1–57.
[211] Hales, 1372.

step of the proof is stated.[212] This makes proofs much longer than necessary and could even be unsurveyably long.

The two groups of mathematicians who have proved the Four Color Theorem after Appel and Haken acknowledge that their proofs are different from traditional proofs, because they rely on the computer. They do not seem bothered by the unsurveyability aspect of their proofs as they do on their reliance. Robin Thomas stresses that the parts of the proof that utilized the computer have been independently checked at least twice.[213] He gives two suggestions to help persuade the reader of the proof that it is correct: the first is to write a computer program to check it against their work, and the second option is to download the programs and supplemental documents to verify that they work correctly.[214] As for the fact that their work is not humanly surveyable and could contain errors, Thomas argues that

> [T]he chance of a computer error that appears consistently in exactly the same way on all runs of our programs on all the compilers under all the operating systems that our programs run on is infinitesimally small compared to the likelihood of a human error during the same amount of case checking.[215]

Thus, the fact that part of the proof is not able to be checked by a human being is not a huge issue for Thomas, because the proofs' results have been checked independently and have been able to be reproduced through multiple runs. Gonthier also addresses computer error, but is confident that the proof checker is reliable. The original purpose of his proof was to test how advanced Coq was, using the data from Robertson et al. as

[212] See Gonthier, Georges. "Formal Proof–The Four-Color Theorem." *Notices of the AMS* 55.11 (2008): 1382-1393. Harrison, John. "Formal Proof—Theory and Practice." *Notices of the AMS* 55.11 (2008): 1395-1406.
[213] Thomas, Robin. "An Update on the Four-Color Theorem." *Notices of the AMS* 45.7 (1998): 852-3.
[214] Ibid., 853.
[215] Ibid.

much as possible. Since his results matched with the results of the second generation proof, he believes that his proof is reliable.

V. Conclusion

Tymoczko's claim that the Four Color Proof is a traditional proof with a gap because of its lack of surveyability received many responses from philosophers and mathematicians. His requirement that a human being be able to look over a proof has been rejected, due to the general agreement that computers are reliable machines that are no different from tools such as pencil and paper. If we were to imagine that a proof must indeed be surveyable by a human being, the Four Color Theorem would still be a conjecture; even today, mathematicians are unable to create the unavoidable set of reducible configurations by hand. This is because, as Shanker rightly stresses, there is no rule that is known to generate this set. Additionally, there would be no further developments in computer-assisted proofs, because proofs would be all done by hand.

The other two characteristics Tymoczko claims to be tied to a proof are convincingness and formalizability. For him, in order to be convincing, a proof must be both formalizable and surveyable. Considering that Appel, Haken, and Koch's proof are neither of these, on Tymoczko's view it is difficult to understand why mathematicians found it to be convincing.[216] Given that the proof is accepted by mathematicians, these three characteristics are not necessary conditions for proof. This is made evident by the fact that after Appel, Haken, and Koch's proof, other mathematicians have come up with different proofs which all rely on computers. If surveyablility were a requirement that

[216] Tymoczko says that the Four Color Proof is formalizable, but at the same time, he says that there is an unsurveyable gap in the proof. According to him, the proof is convincing simply because he knows of no mathematician who rejects the proof (59).

mathematicians took seriously, no one would have bothered to come up with different computer proofs of the Four Color Theorem or any other computer-assisted proof.

Mathematicians who work on computer-assisted proofs do not seem to be bothered by the claim that their work can be regarded as experimental. Cerutti, Davis, Appel and Haken have themselves compared their work to experiments. Perhaps this is not the consensus view of mathematicians, but it did not hinder them from continuing their work. This is in contrast to the philosophers who maintain either that computer-assisted proofs fall under traditional standards, or, as Tymoczko, see them as a threat to tradition.

This is not to say that Tymoczko's worries are irrelevant to mathematical practice. The issues he raised did make it clear to mathematicians and philosophers that the concept of proof is not static, and must match up with current mathematical practices, which now include computer use. As Shanker and Wittgenstein argue, the rule of a proposition is what is essential and not each deductive step of its proof nor its content; however, they further hold that we must be able to survey the form of the proof in order to understand the proposition.

Both philosophers and mathematicians have had much to say about the proof of the Four Color Theorem. Starting with Tymoczko, philosophers focused on the issue of the proof's surveyability. The proof is globally, but not locally, surveyable. We can only understand an informal sketch of the proof, without the details of the computer's work. However, this distinction between global and local surveyability is not very useful. Most mathematicians provide only informal proofs, while avoiding writing out fully formal versions. So, with the exception of Gonthier's proof, the Four Color Theorem is much like a traditional theorem. Mathematicians do not seem to be bothered by the fact that they are unable to look over the proof, but accept Appel, Haken, and Koch's work. This could be for either one of two reasons. First, the theorem is not a very interesting one. Although it is connected to other theorems in graph theory, it is not a deep theorem. Second, mathematicians believed that the Four Color Conjecture was true for a long time before 1976. Since the proof was done using brute force methods, the proof is not interesting either. At best, the significance of the Four Color Theorem is that it paved the way for other computer proofs.

Computer proofs in general lack some of the explanatory power, but the advantage is that they have fewer errors than traditional proofs. Although having fewer errors is a positive, mathematicians are more concerned with understanding the reasoning in a proof. Gonthier and Harrison claim that a computer proof can provide the reader with an explanation of why a theorem holds; however, as I have argued in the second chapter, a formal proof will not guarantee an explanation. Explanatory power depends on the proof's audience. While the proofs of the Four Color Theorem are not explanatory to a number of mathematicians because of their massive lengths as it is easy to get lost in the details, the mathematicians and computer scientists who are familiar with the proofs and their histories have been successful in developing proofs that explain how a result is obtained. It may seem as if these computer proofs are novel, but the strategies and details come from the late nineteenth and early twentieth centuries.

CONCLUSION

My dissertation has examined the role of history and mathematical practices for explanatory proofs. I began by considering two types of history: one that attempts to show that the past is different from the present and another that shows how similar the past is. The former version is what typically is written by historians. The latter is used by mathematicians for their research. I argued that both versions of history benefit the philosopher: history used in research reflects what mathematicians desire out of their work; while the version of history written by historians shows the effects of using history for research on mathematical practices.

In the second chapter, I surveyed theories of explanatory proof and compared them to mathematical practices. I argued that philosophers need to consider more than what appears in a proof, because factors such as the proof's audience play a role in determining the explanatory power of a proof. The current theories of explanation make it seem as if explanatory proofs are explanatory to everyone regardless of mathematical ability. Clearly, this is false, but without looking further than what is stated in a proof, it is difficult to determine the explanatory power of a proof.

I suggested an alternative method of evaluating a proof's explanatory power in chapter three: looking at the evolution of successive proofs of a theorem can help us determine how its explanatory version came to be for a particular audience. Here, I used the two versions of history I described in chapter two to explain the changes and developments found in four proofs of Lewy's Theorem.

The contents of chapter four is a case study of the Four Color Theorem Proof, which is the first major computer proof. The proof does not explain why only four colors are needed to color a map. Instead, through a number of cases, the proof exhausts the all possible reducible configurations in the unavoidable set. In addition to furnishing an example that lacks explanatory power, the discussion surrounding the proof between philosophers and mathematicians was of interest, showing the concerns of philosophers were different from those of mathematicians. While philosophers worried over the proof's unsurveyability, mathematicians were skeptical about the computer's reliability and the proof's lack of explanatory power. These concerns are related to each other, but mathematicians dismissed the philosophers' issues with the proof as they focused more on the methods used in the proof rather than its massive length and the use of computer. From this case study, I showed how philosophers can benefit from understanding what mathematicians consider to be some factors that contribute to explanatory proofs.

As more philosophers of mathematics continue to regard understanding mathematical practices as vital to the philosophy of mathematics, the more it will be in sync with what mathematicians care about. The same goes with recognizing how important history is to formulating these philosophical theories. Presently, there is research being done on topics such as mathematical depth and purity of methods. These two characteristics of proofs highly depend upon their audience. The philosophers who have done work in these areas have noted the difficulty of determining what mathematicians mean when they use these terms. Like mathematical explanation, depth and purity may mean different things in different branches of mathematics. I think that it would be interesting to compare the differences from these branches, because there is no one-size-fits-all when it comes to proof, even though current theories make it seem that way.

Further research should also be done on contemporary mathematics. Presently, philosophers have been using simple mathematical examples found mostly in undergraduate textbooks to advance their theories of explanation. For the purpose of training, the level of explanation and detail found in textbooks tends to be higher than in research. It would be beneficial to look at more recent mathematics, because the proofs that are currently analyzed in the philosophical literature are very old. Contemporary mathematics reflects the mathematical practices we have today, so we should try to look into present mathematical research.

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