ABSTRACT

Title of Document: INSTRUMENT TO EVIDENCE TO ARGUMENT: VISUAL MEDIATION OF INVISIBLE PHENOMENA IN SCIENTIFIC DISCOURSE

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This dissertation examines how scientists and scientific editors have approached specific problems related to visualization and visual argumentation in scientific texts. These problems are related to the following research questions: (1) How are new visualization practices established as scientifically credible? (2) How do scientists modify existing instrument output to make new visual arguments? (3) How do scientists use verbal and visual means to transform problematic data into acceptable support for novel claims? (4) What are the practical and ethical boundaries of modifying visual artifacts for scientific arguments? (5) How do scientists refute established (but incorrect) visualizations that have been widely accepted as accurate representations of reality?

This project considers these issues rhetorically by examining a number of recent and historical cases. The first three case studies explore how scientists created both compelling and uncompelling visual arguments by mediating the visual output

of instruments with rhetorical strategies. These case studies focus on visualizations from physical science: x-ray diffraction photographs, graphics establishing the theory of plate tectonics, and visualizations of atmospheric phenomena. In each case, visualizations articulated invisible phenomena in new ways, transforming unclear or seemingly unremarkable data into convincing knowledge claims. My analysis of these cases explores how scientists integrate visuals into the analogical, causal, transitive, symmetrical, and dissociation arguments that are so essential to the practice of science. The later case studies examine broader concerns regarding ethics, persuasion, and modern scientific visualization. I examine recent issues related to the digital generation and manipulation of scientific images and rhetorical issues related to scientists' increasing dependence on complicated computer algorithms for creating visual arguments.

INSTRUMENT TO EVIDENCE TO ARGUMENT: VISUAL MEDIATION OF INVISIBLE PHENOMENA IN SCIENTIFIC DISCOURSE

By

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Dedication

This dissertation is dedicated to my parents, Susan and Daniel Buehl.

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Many people contributed to the completion of this project, so I have many people to thank. I thank Dr. Lorraine Remer of the NASA Goddard Space Flight Center for discussing her work with me and allowing me to tell her remarkable story. I thank my sister, Dr. Michelle Buehl, for her unflagging support. She has been an invaluable sounding board throughout the dissertation process, and she has been especially supportive toward its end. During the early stages of my research, I workshopped developing material through the visual rhetoric forum at the 2007 RSA Summer Institute. Forum leader Dr. R. Michael Jackson and forum participants Gabriele Bechtel and Christopher Ward provided helpful comments at an important time. I thank Dr. William Fitzgerald for asking provocative questions about my forming work. I am indebted to Professor James Klumpp for the time and attention he has committed to this project. I am also grateful for a dissertation research award from the University of Maryland's Department of English; this award provided precious writing time in the spring of 2008.

I am indebted to each of my mentors. Without their training, I could never have started this project; without their support, I could not have completed it. In addition to supporting this dissertation formally as committee members, they have all contributed to this project's development in ways that should be recognized. Many of my thoughts on dissociation and the rhetoric of science can be traced back to a project on botanical textbooks directed by Professor Shirley Logan in 2005. Professor Michael Marcuse directed me toward and through my work as a corporate science-writing consultant; those projects were also important to the conceptual development of this dissertation. Professor Jane Donawerth provided incisive feedback on earlier drafts of the third chapter and on the developing ideas of the fourth chapter. Finally, I am tremendously grateful for the insight, energy, patience, and advice of Professor Jeanne Fahnestock. Her scholarship has been instrumental to my thinking; her guidance vital to my success.

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Chapter 1: Introduction

Neither the bare hand nor the understanding left to itself are of much use. It is by instruments and other aids that the work gets done, and these are needed as much by the understanding as by the hand. And just as instruments improve or regulate the movement of our hands, so instruments of the mind provide suggestions or cautions to the understanding.

—Francis Bacon, *Novum Organon*, Book I, Aphorism II

In 2005, I was teaching a report-writing course to a group of pharmaceutical scientists at a local biotechnology company. Though most of the course focused on strategies for writing and editing verbal scientific discourse, we discussed the visual components of their texts on the day we discussed the "results and discussion" section of the experimental report. In developing the course, I had access to older drafts of documents from the company's archives, and one of the visual arguments that I included as an example in the course materials had some unusual features.

The graphic was a comparison of two chromatograms— in this case, line graphs indicating the time when specific substances passed a sensor. The report author compared the two chromatograms to demonstrate how minute adjustments to specific settings would affect the runtime and effectiveness of a purification process. This was a routine optimization experiment, but it was unclear to me if some of the imaging practices were routine. The author had adjusted the y-axis scale of one graph; it was in units that were half as large as the other graph. However, the author made no mention of the scale differences in the figure caption or in the report text.

Because of these oddities, it seemed that the figure would be a good prompt for a discussion about working with technical visuals.

While the modification was interesting in its own right, the reactions of my students were even more interesting to me. Some students were appalled that the authors did not document the scale adjustments; others did not care about the adjustment at all; still others thought the adjustment was appropriate and perhaps necessary for the report's argument. For the latter group, it was the x-axis position that was important, not the modified y-axis. Moreover, they argued that the scale adjustment highlighted information in the one graph that they might not have seen otherwise.

Different groups had different opinions, so I had no clear sense of what was "correct" in this institutional context. However, it was clear to me that these simple graphics, produced by a standard instrument, were enmeshed in a network of rhetorical issues. Rhetorical activities were taking place between the instrument recording the data and the visual arguments producing scientific "facts," and different groups of readers were bringing different sets of assumptions to these images. When I looked to rhetorical theory to explain these rhetorical activities, I found some studies on visual rhetoric in science; however, I also found this body of theory insufficient for guiding the study of new situations. There was no comprehensive account of visual argumentation in science. Further research directed my thinking toward a number of issues at the intersection of visual rhetoric and the rhetoric of science. My curiosity also led me to a number of historical and contemporary cases whose visual scientific arguments warranted further rhetorical analysis and explanation. By analyzing these cases, I explored the complexity of rhetorical visualization in scientific discourse.

This dissertation examines how scientists and scientific editors have approached specific problems related to visualization and visual argumentation in scientific texts. These problems are related to the following research questions: (1) How are new visualization practices established as scientifically credible? (2) How do scientists modify existing instrument output to make new visual arguments? (3) How do scientists use verbal and visual means to transform problematic data into acceptable support for novel claims? (4) What are the practical and ethical boundaries of modifying visual artifacts for scientific arguments? (5) How do scientists refute established (but incorrect) visualizations that have been widely accepted as accurate representations of reality? This project considers these issues rhetorically by examining a number of recent and historical cases where visuals were important components of rhetorical situations.

My first three case studies explore how scientists created both compelling and uncompelling visual arguments by mediating the visual output of instruments with rhetorical strategies. These case studies focus on visualizations from physical science: x-ray diffraction photographs, graphics establishing the theory of plate tectonics, and visualizations of atmospheric phenomena. In each case, rhetorical visualizations articulated invisible phenomena in new ways, transforming unclear or seemingly unremarkable data into convincing knowledge claims. My analysis of these cases explores how scientists integrate visuals into the analogical, causal, transitive, symmetrical, and dissociation arguments that are so essential to the practice of science.

The later case studies examine broader concerns regarding ethics, persuasion, and modern scientific visualization. I examine recent issues related to the digital adjustment of scientific images and issues related to scientists' increasing dependence on complicated computer algorithms for creating visual arguments.

This introductory chapter is divided into four parts. First, I discuss what I see as fundamental similarities between the rhetoric of science and visual rhetoric regarding their places in rhetorical studies and the epistemic ground they claim. Second, I review important distinctions between historical, philosophical, sociological, and rhetorical approaches to science and scientific visuals. Third, I describe the theoretical and analytical frameworks that I will use in my study. Fourth, I provide a brief description of each of the chapters.

Foundations and Tensions: Visual Rhetoric, the Rhetoric of Science, and Rhetorical Studies

Current projects studying visual artifacts and scientific texts rhetorically share similar positions in relation to rhetorical studies at large: both of these subdisciplines stretch(ed) the limits of rhetoric's territory. Visual rhetoric extends rhetoric's domain beyond the traditional ground of verbal arguments to encompass visual artifacts of all forms: photographs, paintings, monuments, museum displays, advertisements, etc. The rhetoric of science lays claim to territory long held by philosophy, especially if one considers that, for many, science is the inheritor of dialectic. In this view, science verifies truth(s) that exist outside of social contingencies. In contrast, the rhetoric of science shows that knowledge claims are socially situated through persuasive discourses. In the most extreme formulation of the rhetoric of science, knowledge claims are purely social constructions. More moderate rhetorical approaches make less absolute claims about the role of rhetoric in science, but they still demonstrate that persuasion is a part of establishing knowledge claims. Regardless of the strength of its various incarnations, the rhetoric of science asserts that science is not merely the logical uncovering of an objective reality.

The consequences of both of these rhetorical reclamations are significant; they broadened the scope of rhetoric from obvious sites of persuasion—political discourse, legal argumentation, celebratory occasions—to include endeavors that can be more subtle in their persuasive activity. Visual rhetoric engages the perceived immediacy and "self-evidence" of images that can mask their persuasive qualities. Likewise, the rhetoric of science engages the aura of constructed objectivity in scientific discourse.

These expansions of the realm of rhetoric did not come easily; both subdisciplines faced watershed moments in the mid 1990s when they fended off critiques. Argumentation scholars, such as J. A. Blair (1996) and David Birdsell and Leo Groarke (1996), had to argue for the status of nonverbal arguments, addressing claims that visuals cannot, by definition, be arguments.[1] Similarly, rhetoric of science scholars had to defend their subdiscipline from claims that rhetoric was too "thin" to account productively for scientific discourse. For instance, Dilip Goankar's critique of the field prompted a host of reactions from such rhetoricians as Alan Gross, John

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[1] This was not the first criticism of visual rhetoric. Earlier critiques of the field claimed that that the breadth of visual rhetoric was "reckless" (Bryant qtd. in Olson 11)

Angus Campbell, and Carolyn Miller (see Gross and Keith eds. [1997]).[2] In the cases of both visual rhetoric and the rhetoric of science, rhetoricians broadened definitions of argument and rhetoric, and these discussions enriched both these sub-disciplines and rhetorical studies at large.

As Lester Olson noted in 2007, visual rhetoric is now "flourishing...despite some difficulties, obstacles, and overt resistance" (10). The rhetoric of science has also grown; however, the subfield continues to come under attack from the other branches of science studies. For example, Ceccarelli's 2005 survey of reactions to her own work demonstrates that some historians and philosophers of science are still uncomfortable with rhetorical approaches to knowledge construction. Arguably, the debates regarding both fields are more than just the necessary boundary work that accompanies any revision of critical practice. These growing areas of rhetorical studies were (and continue to be) criticized precisely because of the epistemological ground each can claim.

In the case of the rhetoric of science, rhetoricians push against the rhetoric/dialectic divide, arguing that rhetoric is essential to the creation and dissemination of knowledge: What we know about the material world is (at least partially) the product of addressed argument. The most ambitious articulation of this view—Alan Gross's claim that "science is rhetoric without remainder"—introduces a potentially uncomfortable relativism into the understanding of the foundations of scientific knowledge (Gross 1990). In his revision to that earlier work (*Starring the Text* 2006), Gross himself has backed away from this extreme position, but the

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[2] Harris' introduction to *Landmark Essays in the Rhetoric of Science* also offers a review of Goankar's critique of the rhetoric of science.

“science is only rhetoric” argument still serves as one terminus on the continuum of epistemological positions available to rhetoricians who study science.

In the case of visual rhetoric, assumptions regarding both the epistemic status of visuals and the nature of visual arguments had to be challenged. Throughout the history of rhetoric, visual description has been associated with pre-logical, non-logical, or pathos-driven persuasion. This is not to say that visual arguments and visually vivid verbal descriptions cannot or do not draw from the well of pathos, but, as scholars have demonstrated, it is not their only source of rhetorical force.

Studies of visual arguments that appeared in two special issues of *Argumentation and Advocacy* in 1996 epitomize these demonstrations. These papers refute the “visuals cannot be arguments” claim by extending argumentation theory to account for visual artifacts. For example, Cameron Shelley’s “Rhetorical and Demonstrative Modes of Visual Argument” develops a two-category schema for understanding how viewers process visuals as arguments. Shelly’s *rhetorical* visuals correspond to informal verbal arguments. Her *demonstrative* visuals are the visual equivalent of formal verbal arguments. Each class has specific features. For example, *rhetorical* visuals present premises but not conclusions; *demonstrative* visuals present premises and conclusions. As Shelley notes, “The point of analyzing pictures according to this dichotomy is not to separate them into two distinct classes, but to determine how they are understood to convey a message to the viewer Any picture can make use of either or both modes of argument” (67). Shelley’s work and other contributions—both in and since the *Argumentation and Advocacy* special issues—seem to have settled the “Can visuals be arguments?” question. However, that question is not the only important question addressed by studies of visual rhetoric.

Scholars of visual rhetoric have also challenged the alleged objectivity and epistemic purity of modern "mechanical" visualization. For example, Cara Finnegan's work on photographs and the *naturalistic enthymeme* demonstrates that the medium of a visual argument calls on a matrix of typically unarticulated assumptions held by the audience; these assumptions must be satisfied for an image to function as an argument or evidence for an argument about the real world. According to Finnegan, a photograph is perceived as "natural" so long as the viewer accepts that it has representational, ontological, and mechanical realism. That is, the viewer assumes that the image represents what it claims to represent, that the depicted scene really existed, and that the depicted scene was "…captured by the camera with no intervention from the photographer…" (143). A challenge to any of these assumptions will undermine a photographic argument. Thus, even mechanically produced visual artifacts, like photographs, are not the objective epistemic objects they are often perceived to be. In other words, they are not *arhetorical* in their construction or presentation.

Though there has been significant interest in visual rhetoric and the rhetoric of science separately, there has been very little rhetorical work done on scientific visuals and even less on scientific visual arguments specifically.[3] Much of the study of

[3] Lester Olson's 2006 review of visual rhetoric since 1950 cites eighty-six sources, but only two of these sources are directly or even tangentially related to scientific visual arguments. Of course, additional sources on scientific visuals have been produced over the last fifty years; some are reviewed later in this dissertation's introduction. Moreover, as Olson acknowledges, his review is partial to the studies that are typical in speech and communication departments. Still, the small number of sources on scientific visuals in Olson's review demonstrates that the studies of scientific visual have traditionally been a small portion of the overall corpus of rhetorical scholarship on visuals. Recently,

scientific visuals comes from scholars studying the history of science, philosophy of science, and sociology of science. This is not to say that scholars working in these disciplines do not offer important insights on the history of scientific visualization. As the next section will show, they certainly do. However, what we lack is a comprehensive understanding of scientific visual arguments in primarily rhetorical terms. My project attempts to fill this gap by examining specific cases of rhetorical visualization. Before previewing the methods I apply to these cases, I review other approaches to scientific visuals. Such a review shows the value of these approaches, but it also reveals how a distinctly rhetorical approach can add value to the study of visual arguments in science.

Approaches to Scientific Visuals

To understand the differences between a visual rhetoric of science and other approaches to scientific visuals, it is useful to consider the differences between the rhetoric of science in general and the other general areas of science studies: history of science, philosophy of science, and social studies of science.

Scholars of the history of science are interested in the people and events involved in scientific discoveries and the development of disciplines. Though historians of science do use text as evidence, they approach text as informational content. For example, Bruce Wheaton references the letters and articles of William Henry Bragg, William Lawrence Bragg, and other physicists when describing the

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some rhetoric-of-science scholars have approached visual artifacts (e.g., Gross "Darwin's Diagram" 2007 and Prelli "Visualizing" 2006). These are significant contributions to existing scholarship on scientific visuals; however, the rhetoric-of-science corpus is still predominately focused on textual argumentation.

Braggs' role in the development of twentieth-century physics. Including this material provides details about important events in the development of specific concepts and lines of research. Philosophers of science investigate science from the perspective of how the history of ideas relates to theories of knowledge. That is, they are interested in the logical operations that produce scientific knowledge, and they use episodes in the history of science as evidence. For example, Karl Popper argues that scientific knowledge is based not on whether or not a hypothesis can be verified but rather on the falsifiability of claims; a claim must be falsifiable if it is to count as provisionally scientific. While Popper does draw on specific examples from the history of science (for instance, he discusses Heisenberg's uncertainty principle in *The Logic of Scientific Discovery*), he does so to show how the examples demonstrate specific points about the logic behind scientific knowledge. Not surprisingly, the philosophy of science and the history of science mutually reinforce each other: the history of science is the history of ideas; the philosophy of science draws on this history to explain how ideas work.

Social studies of science approach science from the perspective of sociology, anthropology, organizational psychology, economics, etc. Social studies of science have revealed how science functions as a cyclical economy of credit, funding, and discovery. Resources are deployed to identify phenomena that increase the investigator's prestige and lead to new resources. Scholars who identify with this branch of science studies often use ethnographic methods, such as participant observation, to record and analyze the social activities that produce scientific knowledge. Perhaps the best-known scientific ethnography is Latour and Woolgar's

Laboratory Life. As part of the research for this work, Latour went so far as to take a position as a laboratory technician to get an insider's perspective on the laboratory environment. Latour and Woolgar also analyzed texts, but in light of how text functions in the social construction of facts in laboratory and disciplinary cultures. For example, the modality of claim statements, among other functions, marks the level of investment required to establish or critically engage a claim. Arguing a "fact" into place takes a significant commitment of resources, while a less certain statement can be proposed or challenged by a single experiment or even a reinterpretation of existing data.

In some contrast to historical, philosophical, and social-studies approaches to science, the rhetoric of science focuses on the persuasive activities and rhetorical products of scientists. Texts are the rhetorician's primary objects of study, and his or her methods are the tools of rhetorical analysis: argument analysis, discourse analysis, stylistics, figuration, stasis theory, reception study, and all the other tools of the classical and modern rhetorical traditions. For example, in *A Rhetoric of Science* Lawrence Prelli uses a system of stases and topoi based on the systems of the classical tradition to account for the rhetorical invention of scientific arguments.

Rhetoricians of science will use terms and apply concepts from other areas of science studies, but they do so to make rhetorical claims and observations. For example, the recent collection *Rhetoric and Incommensurability* interrogates Thomas Kuhn's notion of incommensurability. Kuhn, a philosopher of science, argues that major advances in science occur when there are logical incommensurabilities between competing scientific theories. When a new theory can account for a set of phenomena more completely than an older one, a paradigm shift occurs; the older theory is abandoned or incorporated into the structure of the newer one. The essays in *Rhetoric and Incommensurability* show that total incommensurabilities are rare. Rather, in scientific discourse, incommensurabilities are typically either rhetorically negotiable situations or active rhetorical investments.

The content and methods of historical, philosophical, social, and rhetorical studies of science can and do overlap. It could be argued that these disciplinary distinctions needlessly divide the broader field of science studies; however, I find these distinctions useful for describing both the landscape of science studies in general and the scene of science visualization studies in particular. When it comes to visuals, historians of science are interested in how new visualization technologies contribute to new scientific developments and how visualization practices change over time. For example, Christoph Meinel documents the history of chemical modeling techniques and explains how they influenced chemistry instruction and the discovery of new chemical knowledge. Philosophers of science treat images as objects of epistemological study. For example, Robert O'Hara's examination of representations of taxonomic systems shows the epistemological differences between pre- and post-Darwinian mindsets regarding the structure of the "Natural system." Sociologists show how visuals are a part of scientific cultural practice. For example, Michael Lynch shows how data is reduced into normalized forms that can function as standardized tokens of meaning in scientific cultures. Sociological approaches also show how visualization practices contribute to visual cultures of science. For example, Bernike Pasveer's work on x-ray interpretation shows how non-visual tests for diagnosing tuberculosis were transformed through x-rays into medical tests with visual results that could be interpreted by trained practitioners.

Historical, philosophical, and social studies approaches to visuals often converge. For example, Peter Galison's extensive study of nuclear physics, *Image & Logic: A Material Culture of Microphysics*, examines both the historical development of atomic particle visualizations and how these visualizations reflect two parallel traditions of scientific reasoning: the image tradition (where physicists look for individual pictures of "golden events") and the logic tradition (where physicists analyze massive accumulations of instrument counts and other statistically analyzable data). Galison's work also reflects sociological approaches; he is interested in the organization of nuclear physics laboratories and how changes in lab organization influenced and were influenced by the material practices of visualization. (It is interesting that the subtitle of this book is "A Material Culture of Microphysics.") What Galison does not explicitly explore is the distinctly rhetorical dimensions of particle images and how they function as arguments. For example, he explains how "golden event" photographs served as effective representative examples in arguments about particles even though there might have been only one or two images of a particular particle event; however, he does not explain these photographs in rhetorical terms. That is, the "image" tradition of particle physics is not explicitly characterized as reasoning by example.

Recent work by David Kaiser—a student of Galison—offers another example of this kind of interdisciplinary approach. His *Drawing Theories Apart: the Dispersion of Feynman Diagrams in Postwar Physics* (2005) documents how

Feynman's diagrams of subatomic particles were circulated, adapted, and conventionalized in different contexts. Like Galison's work, Kaiser's text is a thorough historical and social analysis, and it offers important insights into the circulation and reification of a visual genre. For example, he documents the changes in the epistemological status of the Feynman diagrams over time and for different audiences. Like Galison, Kaiser does not always raise rhetorical issues explicitly, but his thorough work does offer an interesting case for rhetorical theorists interested in the circulation, adaptation, and adoption of scientific images.

In contrast to other science studies scholars, rhetoricians of science explicitly address how scientists construct visual arguments and how visuals affect the persuasiveness of science texts. For example, regarding ethos and images, Anne Richards shows that the quality and number of visuals in a botany article can build the credibility of the author. Similarly, she shows that otherwise uncompelling or even suspect images will be acceptable if the author is well respected. Thus, there is a reciprocal relationship between images and the authority of knowledge claims. Images can build authority and authority can bolster the persuasiveness of images. While there are several interesting rhetorical studies of scientific visuals (for example, Shelley, Rosner, Dombrowski, Gross "Darwin's Diagram"), the number is noticeably smaller than the number of rhetorical studies focused on the textual features of scientific documents. There has not been sufficient attention paid to the connection between visual form and argumentative effects in the study of scientific visuals. Thus, a study is needed that explores the relationship between the components of visual forms in science and the sophisticated arguments deployed in scientific claims about invisible phenomena made visible. My dissertation addresses this need.

Theoretical Approaches, Analytical Tools, and Methodological Orientations

This project analyzes how scientists visualize specific types of arguments. Two sets of tools are applied to this task: (1) a system of argumentation that accounts for some typical argument types used in science and (2) tools of visual analysis that can account for connections between visual form and intended meaning. This section reviews these tools. The claims enabled by these tools also require empirical support; thus, the last section of this methodological review describes issues related to context, reception, and circulation.

Concepts of Argumentation

Chaim Perelman and Lucy Olbrechts-Tyteca's *The New Rhetoric: A Treatise on Argumentation* is the primary source for the concepts of argumentation applied to each rhetorical case. Perelman and Olbrechts-Tyteca's work is useful for two reasons. First, *The New Rhetoric* is well suited for engaging scientific arguments. Second, this system of argumentation has not been applied extensively to visual arguments.

Though Perelman and Olbrechts-Tyteca are primarily interested in arguments about values in legal, philosophical, and political discourse, these Belgian theorists also specifically address some rhetorical aspects of science.[4] For example, Perelman and Olbrechts-Tyteca discuss issues of audience in scientific rhetorical situations. They note that the scientist communicates to the specialist audience as if the readers are members of the universal audience: “[The scientist] presupposes that everyone with the same training, qualification, and information, would reach the same conclusions” (34). Recent work by Wynn (2007) suggests that scientists do not always make accurate presuppositions about their collegial audiences; still, the Belgians’ observation demonstrates that science is not outside the rhetorical framework of *The New Rhetoric*.

The trajectory of that framework with respect to the techniques of argumentation is particularly pertinent to scientific situations. Perelman and Lucy Olbrechts-Tyteca first define a series of quasi-logical arguments—such as arguments from incompatibility, transitivity, symmetry, and reciprocity. These argument types are used in scientific situations more than one might think. Much of the rest of their survey of the techniques of argumentation is organized according to the various arguments’ relationships with reality. They discuss (1) arguments that depend on the structure of reality, such as causality arguments; (2) arguments that structure reality, such as arguments based on analogy; and (3) arguments that revise the structure of reality, what they call the dissociation of concepts. While many of these arguments—

[4] In later reflections on the circulation of *The New Rhetoric*, Perelman notes that the project was begun as a treatise on the logic of values, but it soon evolved into a study of argumentation in general (“The New Rhetoric and the Rhetoricians” 189-190). He also elaborates on the role of rhetoric in philosophy and the sciences: “Every philosophy is rhetorical to the extent that it is elaborated not by setting out from an intuition of clear and distinct ideas, but setting out from common language, always confused and susceptible to a great number of interpretations. In order to avoid every misunderstanding, every ambiguity, the philosopher is obliged to clarify his intellectual tools, … adapting them each time in such a way as to establish systematic coherence” (194). He then notes, “This way of adjusting the meaning of words to the necessities of coherent discourse is met with again in all the human sciences (perhaps even in all the sciences), but it characterizes above all the controversies of jurists” (194).

such as analogy and causality arguments—correspond to topoi that have been discussed in rhetorical theory since classical times, the arrangement of the Belgians' discussion aligns well with the processes of scientific reasoning and discovery. Scientists develop existing hypotheses from known facts and controlled investigations of correlations and cause; they develop new hypotheses and conceptual models through analogy, example, and metaphor; and they revise and replace established conclusions and theories when those concepts cease to be the best reflection of reality. Regarding the rhetorical restructuring of reality, Alan Gross notes in *The Rhetoric of Science* that dissociation—the separation, negation, and or revaluation of appearance/reality pairs—is a fundamental activity of scientific discourse. Moreover, for Gross, "The persuasive effect of science becomes just its ability to move from term I to term II *as if* moving from appearance to reality" (31). In *Starring the Text,* the 2006 reworking of *The Rhetoric of Science,* Gross advances the claim that "This move is unique to the sciences" (40). If dissociation is important to science and the seamless negotiation of the appearance-reality pair is unique to science, it is also important to understand if and how visuals participate in this powerful argument type.

My second reason for working with *The New Rhetoric* is that many concepts articulated by Perelman and Olbrechts-Tyteca—such as the quasi-logical arguments and dissociation arguments—have not been discussed extensively in relation to visual forms. Thus, in using this text as the basis for analyses of visuals arguments, I am testing the applicability of this rhetoric to the realm of visuals. This test could have important implications for studies of visual rhetoric at large. As Lester Olson observed in 2007, "…while we now have a wide range of conceptually-driven and historically-situated case studies [of visual rhetoric], we do not have a substantive treatise that might accurately be described as a theory of visual rhetoric" (14). As the only complete rhetoric of the twentieth century, *The New Rhetoric* is conceptually rich enough to be the basis for a general theory of visual argumentation. Though this dissertation is a set of "historically situated" case studies of persuasion in science, the consistent application of concepts from *The New Rhetoric* to these cases might yield examples and insights that could translate into a more general theory of visual rhetoric.

Analytical Tools

My analytical framework depends on three interrelated aspects of visualization studies: (1) visual figuration, (2) visual conventions, and (3) visual grammar.

What I refer to as "visual figuration" is the application of argument patterns of verbal rhetorical figures to visualizations. This subject has been articulated before in several contexts. For example, Hanno Ehses shows how various playbill illustrations for productions of Shakespeare's *Macbeth* included visualized rhetorical figures in their designs. These visual figures range from antithesis and irony to synecdoche and periphrasis. While Ehses's examples are useful in demonstrating the existence of visual figures, the playbill illustrations are more ornamental than argumentative instances. Also, the categories that Ehses uses in organizing the figures are drawn from pedagogical works, and these categories are not useful for engaging with figures from the perspective of argumentation. More important for my work is Jeanne

Fahnestock's treatment of visual figures in *Rhetorical Figures in Science*. Fahnestock argues that figures are more than mere ornaments; they are sources of invention that can take both visual and verbal forms. For example, visual *incrementum*—a series whose terms increase (or decrease) in force—is deployed consistently in diagramming the evolution of species across the fossil record. For example, in Figure 1.1 the constituent elements are arranged from bottom to top by increases in size, hoofedness, and time.

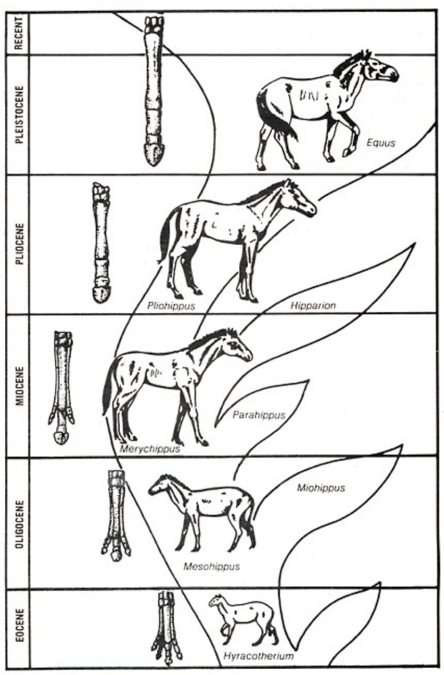

Figure 1.1: An example of visual incrementum. From bottom to top, over time the species increase in body size, the center toes get larger, and the other toes diminish and then disappear. Source: Monroe, James. "Basic Created Kinds and the Fossil Record of Perissodactyls" (11).

The relationships expressed by rhetorical figures can be used to explain how rhetors transform raw and/or previously mediated visual data. I show how rhetorical figures serve as resources that scientists use when constructing and refashioning visual arguments.

I also incorporate notions of visual conventions in my analysis. In *Shaping Information,* Charles Kostelnick and Michael Hassett's demonstrate how visual conventions are mutable forms, changing over time, forms that both constrain rhetorical action and invite inventional creativity. Martin Rudwick's 1976 "A Visual Language for Geology" is a good case study of how particular scientific visual conventions—the conventions of geologic strata diagrams—emerge from other technical and artistic contexts and traditions. Rudwick, a historian, is concerned primarily with tracing the lineage of a visualization tradition. I examine how scientific rhetors use and modify existing conventions to make new arguments.

The third slot in my analytical toolbox contains concepts of visual grammar and semiotics. Though scholars from many fields have developed schemes to describe "visual language," I find the most fully articulated visual grammar in the work of the discourse analysts Gunther Kress and Theo van Leeuwen. Their *Reading Images: A Grammar of Visual Design* uses M.A.K Haliday's functional linguistics as a starting point for the semiotics of images. While Kress and van Leeuwen investigate form-meaning pairings across Haliday's divisions—ideational, interpersonal, and textual—for the purposes of this project, I am most concerned with their treatment of the ideational in what they describe as narrative, classificatory, and analytical representations. For analyzing these classes of images, Kress and van Leeuwen provide a number of useful terms and observations. For example, their taxonomy of analytical structures classifies images into structured and unstructured groups. Structured images are further defined according to spatial and temporal features. Sorting images into these classes helps to identify how formal features create specific conceptual effects. For example, a timeline turns individual elements into a narrative, a "set of successive stages of a temporally unfolding process" (107). Kress and van Leewuen's distinctions between structured and unstructured images are particularly useful for this study, since in most of my cases, scientists transform unstructured (or semi-structured) inscriptions into highly structured visual arguments. In applying these concepts, I identify correspondences across the semiotic and rhetorical approaches to visuals. That is, the study shows how specific concepts from Kress and van Leeuwen's work correspond to specific lines of argument and figural resources.

Accounting for Rhetoric: Context, Reception, and Circulation

As visual rhetoric and the rhetoric of science developed, specific concerns emerged regarding issues of analytical accountability and legitimacy. In responding to criticisms that rhetoric is too textual or too thin to account for visual and scientific artifacts respectively, rhetoricians refined analytical approaches that demonstrated the value of distinctly rhetorical analyses. Part of that refinement involved greater attention to issues of context and reception. In other words, if rhetoric is to offer a legitimate perspective on visual and scientific practice, that rhetorical perspective must be supported with evidence accounting for the context and reception of particular artifacts and arguments. Some scholars of visual rhetoric became particularly attuned to issues of context. As Birdsell and Groarke (1996) note, any analysis of a visual argument must consider the immediate visual context, the immediate verbal context, and the contemporary visual culture of the artifact. Some rhetoric-of-science scholars—for example, Davida Charney (1993); Paul, Charney, and Kendall (2001); Leah Ceccarelli (2001); Randy Harris (2005)—emphasize the importance of critically examining rhetorical context, audience reception, and intertextuality—what Paul, Charney, and Kendall call "moving beyond the moment" of rhetorical production.

For this project on scientific visuals, context and reception are important concerns. *Circulation* is also an important concept to consider because specific types of visuals and specific images circulate and change both within scientific discourse communities and in broader cultural contexts.

For each case, I assemble the best available evidence to support my rhetorical claims. For my older historical cases, there are rich archives to tap for evidence of context and reception; the circulation of visuals within discourse communities can be tracked through citations and historical material. For my more modern cases, the archives are more sparse, but supporting material is available. For example, the case in Chapter Four ("Revealing the 'Twilight Zone'") is quite recent; the first article was published in April 2007, and the few works that cite this article are still in press. However, by interviewing one of the principle authors and contacting the first author to cite the paper, I have collected anecdotal evidence regarding the production and reception of specific visual arguments. I have also been able to track the article's circulation into popular contexts through digital channels.

Finally, my thinking about accountability issues with respect to visual rhetoric in science has directed me toward some concepts from the work of Kenneth Burke, specifically the terministic screen.[5] Burke's chapter on terministic screens intrigues me because both images and science were critical to the development and explanation of the concept. In the following passage, Burke relates the terministic screen to visual artifacts:

> When I speak of terministic screens, I have particularly in mind some photographs I once saw. They were *different* photographs of the *same* objects, the difference being that they were made with different color filters. Here something so "factual" as a photograph revealed notable distinctions in texture, and even in form, depending upon which color filter was used for the documentary description. (115-116)

Science also figures prominently in Burke's discussion of terministic screens. Many of his examples are scientific examples. For example, he discusses the networks of terms used by psychologists John Broadus Watson and John Bowlby; Burke also applies the concept of terministic screens to the work of natural historian Charles Darwin. Moreover, Burke comments explicitly on the capacities and limitations of scientific terminologies (120, 122). It does not take an enormous intellectual move to use Burke's terministic screen to conceptualize the convergence of the visual and the scientific. Arguably, any scientific visualization technology acts like a Burkean terministic screen. In the case of visualization, the variables that are measured by the recording instrument limit what the scientist knows about a phenomenon; therefore,

[5] For Kenneth Burke, "Even if any given terminology is a *reflection* of reality, by its very nature as a terminology it must be a *selection* of reality; and to this extent it must function as a *deflection* of reality"(115).

the inscription constrains the new claims that he or she can make.[6] Thus, a given visualization technology has the potential to be both an asset and a liability in the rhetorical situations of science.

The terministic screen intersects with issues of rhetorical accountability. For example, the terministic screen can help explain rhetorical contexts or reception-based explanations of rhetorical effects. Specifically, thinking about terministic selection can help elucidate both scientific visual cultures and immediate visual contexts. It can also help to explain why specific visuals were received in specific ways.

Chapter Summaries: Cases Studies of Visual Argumentation in Science

My first three cases share a number of traits that make them interesting subjects for rhetorical analysis. Each case explores the rhetorical activity that accompanies a new approach to visualizing otherwise unseeable phenomena. Each case involves documents and events that had or will have revolutionary implications for both the history of science and the trajectories of specific disciplines. In each case, visual arguments were central to the persuasiveness of new knowledge claims. In addition, each case demonstrates the persuasive activities needed to make specific visualization technologies or specific visuals acceptable to scientific audiences.

[6] Burke explicitly addresses the role of terministic selection in scientific instrumentation; that is, terminological selection and deflection are built into instruments. More specifically, he notes that instruments function like any other term system in that they are ultimately based on operators that either connect or divide: "And since all laboratory instruments of measurement and observation are devices invented by the symbol-using animal, they too necessarily give interpretations in terms of either continuity or discontinuity" (120).

Chapter 2, "Articulating Atoms and Examining X-rays: Visual Arguments in the Early Years of X-Ray Crystallography," examines how a new visualization tool was established rhetorically. This chapter discusses rival interpretations of Max von Laue's original x-ray diffraction patterns. These images are symmetrical arrangements of dots; the dots are locations on a photographic plate that had been bombarded by x-rays intensified when diffracted by the atoms of a crystal lattice. Laue was correct in realizing that x-rays are waves that are diffracted by molecular-level structures; however, both his assumptions regarding the geometrical arrangement of the diffracting crystal and his conjectures on the diffracted ray's wavelengths were ultimately not convincing. William Lawrence and William Henry Bragg articulated different methods for interpreting two-dimensional photographs to determine three-dimensional structures. They reinterpreted Laue's original photographs with mathematical and visual arguments based on principles of analogy and causality. Their reinterpretations and later work in x-ray crystallography laid the foundations for some of the most important scientific developments of the twentieth century, including the revelation of the double helical structure of DNA.

Chapter 3, "Showing the Motion under the Ocean: Visualizing Claims about Invisible Geologic Processes," examines visual arguments from the early development of plate tectonic theory. This chapter shows how geophysicists modified existing visualized remote-sensing data to create unprecedented visual arguments that justified the idea that sea-floor spreading is the mechanism of continental motion. They deployed visual figures of thought and adapted visual conventions, first to demonstrate that unusual submarine magnetism patterns exist, and then, to depict a transitive relationship between segments of sea floor, magnetic resonances of those segments, and a timeline of known magnetic anomalies. They argued that distance is related to magnetism and magnetism is related to time; therefore, distance is related to time. Distance divided by time is the definition of velocity, the metric of motion. In other words, these scientists used visual rhetoric to reveal imperceptible magnetism and motion under the ocean. In doing so, they tipped the balance in favor of Wegener's hypothesis that modern continents once formed a single land mass.

Chapter Four, "Revealing the Twilight Zone: Verbal and Visual Articulation of Invisible Atmospheric Properties," examines a 2007 article by NASA scientists who were making novel claims about the nature of the atmosphere. These scientists faced significant scientific and rhetorical challenges because they were using atypical methods to support claims about data that others deemed dubious. These researchers negotiated figural logics and adapted visual artifacts to make dissociation arguments that separated the appearance of a binary relationship between cloudy and cloud-free atmosphere from the reality of an atmosphere that is more complex. They verbally and visually describe a complex invisible region around visible clouds where water vapor and solid atmospheric particles interact, reflect light, and hence affect global temperature. The researchers called this area the "twilight zone" between clouds and aerosols. Despite this evocative name, their discovery was initially rejected. These scientists used an array of dissociation techniques to overcome resistance and argue the "twilight zone" into existence. After demonstrating how their verbal and visual arguments evolved over successive revisions, I show how these arguments were adapted for texts written for popular audiences. Throughout the entire analysis, I draw upon my interviews with one of the NASA researchers who authored the "twilight zone" study. Thus, I document some of the activities surrounding the production and revision of their images and texts.

My last two chapters explore some of the modern complications of using images to argue in scientific contexts. Chapter 5, "(Ir)Responsible Mediation of Scientific Images: Enthymemes, Ethics, and the Visual Rhetoric of Science," explores the ethical problems and rhetorical possibilities of adjusting images with digital tools. Recent cases of high-profile fraud—such as the 2006 Hwang stem-cell scandal—have drawn attention to the role of manipulated images in research misconduct. These scandals and other kinds of cases—such as when honest scientists modify images inappropriately for presentational purposes—have forced editors of scientific journals to think about the lines between rhetorical presentation and dishonest fabrication in the Age of Photoshop. This chapter examines recent editorials by scientific editors and recent revisions to journal submission guidelines to plumb the rhetorical dimensions of image manipulation. By considering these disciplinary discussions of imaging practices in the light of such concepts as *enthymemes*, *ethos*, and *objects of agreement*, I explore the rhetorical and epistemological dynamics of the modern scientific visual. Examining different editorial responses to both fraud and more ethically ambiguous manipulation reveals various assumptions about the status and role of visuals in scientific discourse.

Chapter 6, "Refutations and Revelations: Reflections on the Visual Rhetoric of Science," is my concluding chapter. This chapter examines a recent case in which protein crystallographers had to refute an accepted visual argument produced by an accomplished colleague. I juxtapose the details of this case against the observations I draw about the rhetorical dynamics of the other cases. This summative juxtaposition reveals a skeletal sketch of what could become a comprehensive visual rhetoric of science.

Chapter 2: Articulating Atoms and Examining X-rays—Visual Arguments in the Early Years of X-Ray Crystallography

Even Bertrand Russell is compelled to accept analogy as one of the postulates required to validate the scientific method because it provides the antecedent probability necessary to justify an induction.

— Richard Weaver, "Rhetoric of the Social Sciences," *The Ethics of Rhetoric*

In 1953, James Watson and Francis Crick interpreted a now iconic photograph taken by Rosalind Franklin and Raymond Gosling. Franklin and Gosling had used x-rays to photograph a crystallized sample of deoxyribonucleic acid (DNA), the molecule that stores genetic information. By analyzing the fuzzy two-dimensional crossed-ladder image of the molecule (Figure 2.1), Watson and Crick were able to determine the elegantly complex three-dimensional double-helical structure of DNA (e.g., Figure 2.2). This achievement and other monumental discoveries in molecular biochemistry—such as Kendrew's mapping of myoglobin (1958), Perutz's mapping of hemoglobin (1959), and Hodgkin's determination of insulin's structure (1969)—are part of a tradition of visual interpretation and argumentation that began decades earlier. It was the interpretation and reinterpretation of x-ray photographs produced in 1912 that ushered in this new era of structural chemistry.

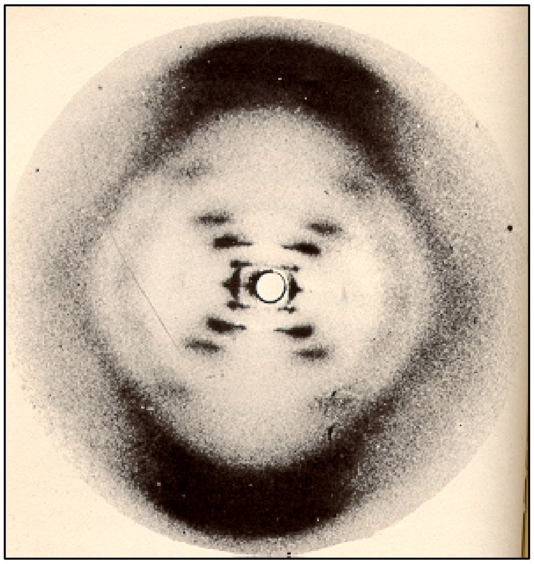

Figure 2.1: X-ray diffraction photograph of DNA. Franklin and Gosling. May 2, 1952. Source: *Linus Pauling and the Race for DNA*. Oregon State University Library.

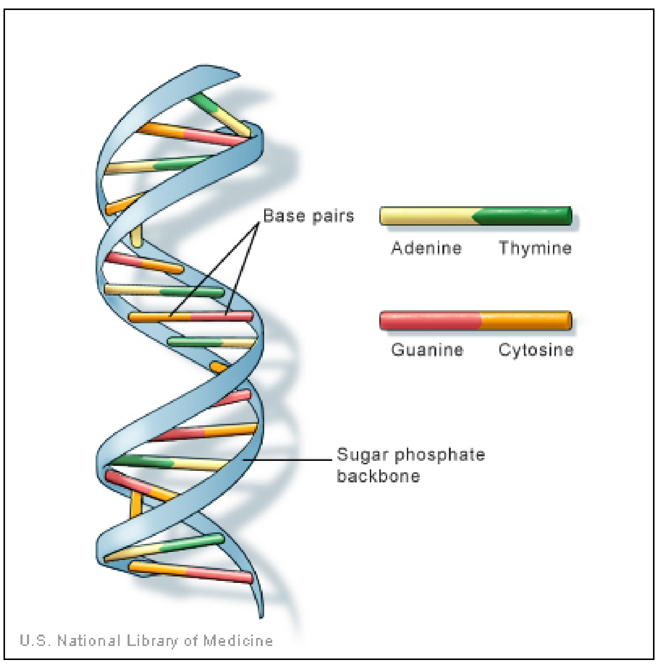

Figure 2.2: Modern representation of DNA's double-helical shape. Source: U.S. National Library of Medicine.

In 1912 German scientists Max von Laue, Walter Friedrich, and Paul Knipping bombarded simple inorganic crystals with x-rays. The atoms in the crystals scattered the x-rays, and the scattered rays left highly symmetrical patterns of spots on photographic plates (e.g., Figure 2.3). With some complicated mathematical analysis and some creative conjectures, Laue was able to make inferences about the wavelengths of the x-rays that made the patterns; he also read the spots as indicators of the relative positions of the atoms that had scattered the x-rays. For the first time in the history of science, scientists had recorded visible traces of the arrangement of atoms in a crystal. However, what they were seeing in the photographs and how the patterns were formed were not quite clear. While the visual evidence itself intrigued many physicists, some scientists were unconvinced by Laue's explanation of the pattern as a diffraction phenomenon.[7] They were also skeptical of his claims about the composition of the formative x-rays.

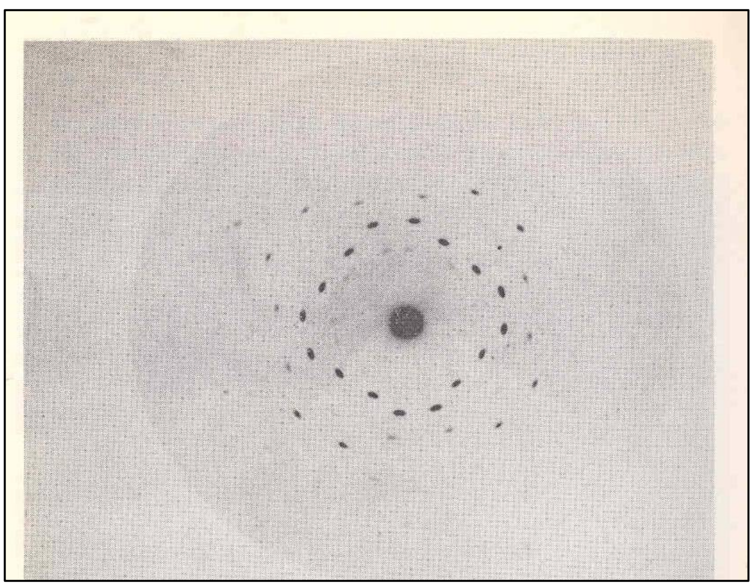

Figure 2.3: Friedrich, Knipping, and Laue. Figure 5. 1912. Reproduced in *X-ray and Neutron Diffraction*. G. E. Bacon. 1966. Unpaginated plate.

[7] In their first paper, experimentalists Friedrich and Knipping described the experiment's methods, while Laue, the theoretical physicist, interpreted the visual evidence. Laue was responsible for interpreting the various results, so in most of this chapter I will refer to Laue singularly, a convention that is consistent with other scholarly accounts of this case.

Laue's interpretations of his images were based on traditional assumptions about the crystal structure of his mineral samples; these assumptions allowed him to make novel claims about the behavior and composition of the x-rays that interacted with these structures to form the spots. He claimed that the photographs indicated that x-rays diffract like other forms of electromagnetic radiation. At the time, it was not clear if x-rays were waves or particles or if x-rays were like or unlike visible light, so this claim to have observed diffraction—a wave phenomenon— represented a significant theoretical development. Laue also conjectured that each spot could be attributed to x-rays with one of five possible wavelengths, and these wavelengths were related to specific kinds of atoms in the irradiated sample. Though some of Laue's initial conjectures would be challenged, his thorough use of visual artifacts to anchor a new visualization regime was remarkable for the time. He used photographs and diagrams to construct a visual account of x-ray behavior; he also used visual evidence to refute counter arguments.

Later in 1912, British father and son William Henry Bragg and William Lawrence Bragg replicated the Laue experiment, and the younger Bragg reinterpreted Laue's photographic results to make different claims about crystal structures and the nature of x-rays.[8] The Braggs used Laue's photographs to support different claims about the molecular structures of the irradiated crystals; they also used the photographs to argue that the formative rays were selected from a heterogeneous bundle of x-rays that resembled heterogeneous white light. For the Braggs, the

[8] William Henry Bragg and William Lawrence Bragg both worked on x-ray diffraction experiments. They published jointly and individually on the topic, and they were jointly awarded the Nobel Prize for their work on x-ray crystallography. In most instances, I refer to the Braggs collectively, though there are parts of the chapter, such as when I analyze individual texts, that I will refer to them individually.

spacing between atoms determined which x-rays were represented in the photographs. Ultimately, the Braggs convinced their contemporaries that this reinterpretation was more plausible; however, their means for describing the behavior of x-rays were based on heuristically efficient but technically imprecise analogies. Their rhetorical and scientific success demonstrates the importance of persuasive analogies in affecting conceptual change in science. Their success also demonstrates the role of incompatibility arguments in scientific debates; the Braggs used a range of argumentative tactics to show that Laue's reading of his photographs was incompatible with the photographs themselves. This line of argument allowed the Braggs to promote their own interpretation.

This chapter examines verbal and visual arguments from the frontier period of the development of x-ray crystallography. Analyses of the early crystallography papers of Laue and the Braggs show how each used the same visual evidence to make novel claims about the structure of reality and the nature of x-ray energy. The exigence for this chapter comes from two sources.

First, these articles and images have been discussed frequently from the perspectives of the history, philosophy, and sociology of science; however, they have not been examined from a rhetorical perspective. Scholars taking other science studies approaches have not examined the range of visual arguments at work in Laue's coauthored and single-authored texts. Though the Braggs' use of analogy is mentioned in some of the secondary literature, the extent and variation of this line of reasoning is not charted across the network of texts where it appears. Moreover, the significance of their analogical reasoning is not elaborated from a rhetorical perspective.

Second, this case is an interesting subject because it exemplifies the use of visuals to link instrumentation and argument. X-rays were an "under known" quantity at the time, and the internal atomic configurations of the crystals were conjectures based only on mathematical hypotheses; consequently, the explanations accounting for these unseen rays and structures were highly speculative. Unlike an instrument whose output allows some visual verification or uses the familiar medium of visible light, such as the microscope, x-ray diffraction photographs in 1912 were doubly ambiguous. Both Laue and the Braggs relied on verbal, visual, and mathematical arguments to make claims with varying degrees of success about these rays and structures; both groups had to argue into place the premises governing the creation and hence the interpretation of the artifact.

Scientific Background

The development of x-ray diffraction crystallography—the science of using x-rays to study crystal structures—was the culmination of three separate lines of research: x-ray research, optics, and crystallography. Each of these research areas is an important component of the scientific context of this case. This section describes the state of each topic in the early twentieth century to clarify the concepts that Laue and the Braggs were arranging, extending, and reinterpreting in their arguments.

X-Rays

In 1895, Wilhelm Conrad Röntgen was experimenting with a cathode ray tube. This device is a vacuum-evacuated glass vessel containing a positive and a negative electrode. When electric current is applied to the system, cathode rays (what we now call electrons) are emitted from the cathode and slam into the positive electrode (the anode). If a phosphorescent screen is inserted within the tube, it will glow when struck by the free electrons. While experimenting with this device, Röntgen noticed that in addition to the expected glowing of a screen within the tube, an unanticipated glowing occurred in phosphorescent screens outside the tube and even some distance away in his darkened laboratory (Mould 22-23). By further investigating this strange phenomenon, Röntgen discovered what we now call x-rays. In homage to Röntgen, some early sources referred to x-rays as Röntgenstrahlen, a word that is sometimes translated as Röntgen-rays or röntgen rays.

We now know that x-rays are forms of electromagnetic radiation whose wavelengths are approximately four hundred times smaller than the smallest visible light waves. Though the information in Figure 2.4 was unknown during the early years of x-ray science, this depiction of the electromagnetic spectrum demonstrates the relationship between x-rays and other forms of electromagnetic radiation, such as visible light, microwaves, and radio waves. The small wavelengths of x-rays allow the rays to pass through matter that is not dense, such as skin and muscle tissue.

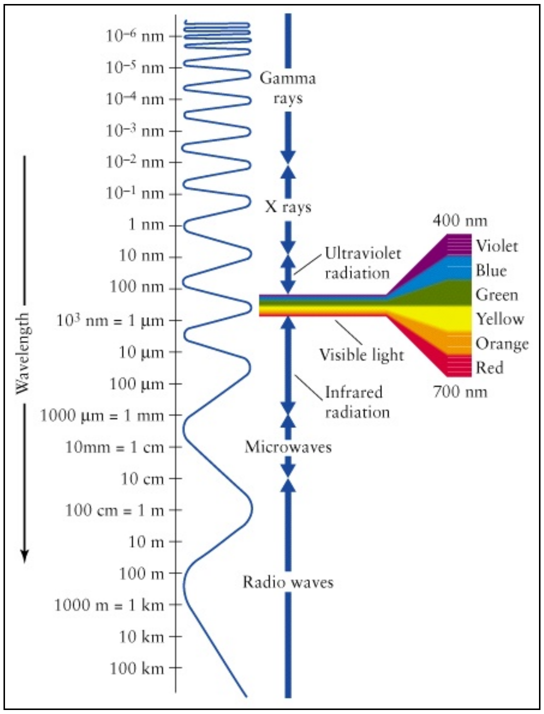

Figure 2.4: The Electromagnetic Spectrum. Source: NASA Langley Atmospheric Science Data Center.

Röntgen's discovery of x-rays was a major contribution to both medicine and physics, but it was also a major cultural phenomenon. The now iconic x-ray photograph of the hand of Röntgen's wife (Figure 2.5) was widely circulated and described in print.

Figure 2.5: X-Ray Image of Anna (Ludwig) Rontgen's Hand. Source: Reynolds Historical Library. University of Alabama at Birmingham.

This photo and other images of "naked" bones unsheathed from surrounding tissue captivated the imaginations of both scientific communities and the public at large. As a contemporary physician noted in 1896, "All the world seems to have gone off on two crazes—bicycles and x-rays" (Thompson qtd. in Pasveer 41).

But in this early period of x-ray science, the phenomenon was not well understood. Medical practitioners did not yet realize that x-rays could have harmful effects on living tissue exposed to strong x-rays for long periods. Novelty applications—such as cosmetic x-ray hair removal and x-ray carnival attractions—seem frivolously reckless by today's standards; however, these applications of x-rays demonstrate how little was known about the rays during the early years of the twentieth century.

The mysterious new x-rays also engaged the imaginations of physicists who wanted to determine the fundamental laws of matter and energy. Initially, x-rays confounded physicists. Some experiments indicated that they were wave phenomena, while other experiments suggested that they were fast moving particles, like alpha particles.[9] At the time, the wave-particle dualism was not yet accepted as a working premise. Many physicists approached the "What is an x-ray?" problem with binary logic: it was either a particle or a wave. Thus, establishing the category into which the x-ray fit was an important research project for many of the most eminent scientific minds of the early twentieth century.

A problem that further complicated the task of classifying the new phenomenon was determining which type of wave x-rays might be, if they were indeed waves. Röntgen believed they were longitudinal waves, like sound waves. Other physicists thought they were transverse electromagnetic waves, like light. In addition, researchers who were invested in the wave hypothesis disagreed about the regularity and composition. Were x-rays periodic continuous streams of waves or aperiodic wave impulses? Were x-rays homogenous radiation consisting of only a few distinct wavelengths? Were they heterogeneous composites like white light? Was there only one kind of x-ray? How did x-rays change when they interacted with matter?

One line of research that was especially important to the development of x-ray crystallography—especially to the projects of Laue and the Braggs—was Charles Barkla's work on x-ray spectra. In 1911, Barkla reported that, when x-rays interact with matter, they excite the atoms and cause them to emit a second (and sometimes

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9 An alpha particle is an ionizing radiation particle consisting of two protons and two neutrons.

third, fourth, etc) “fluorescent” x-ray pulse. This pulse has a wavelength that is unique or “characteristic” of the atoms excited by the primary x-ray beam. This discovery was important to the development of x-ray crystallography because it was unclear if the x-rays forming the earliest spotted patterns were Barkla’s secondary rays or if they were mere variations of the original x-ray beam. Laue and the Braggs disagreed on this important point.

By 1912, scientists had conducted dozens of other experiments to determine the nature of x-rays. For the purposes of this chapter, the specific experiments and developments are not significant for understanding the case; however, it is important to understand both how high the level of interest was in x-rays and how little was known about their basic characteristics. It is also important to note that Max von Laue’s early work was situated in this context of uncertainty and experimentation. He wrote of that time at the University of Munich, “One lived there in an atmosphere saturated with the problems concerning the specific natures of x-rays” (292).

The genesis of Laue’s landmark idea—that atomic crystal lattices could be used to diffract x-rays—is described at greater length later in this chapter; however, this “brainstorm” will make more sense after brief primers on optics and crystallography.

Optics: Reflection, Refraction, Diffraction, and Interference.

Optics is the branch of physics that studies the behavior of light, and it has been a subject of inquiry for philosophers and scientists since ancient times. Newton’s treatise on optics is considered the first modern description of experiments, and the study of light became more sophisticated throughout the eighteenth and nineteenth centuries.

Before the advent of quantum mechanics and the operational acceptance of the wave-particle dualism, theories of light were either particle based (corpuscular) or wave based. By the end of the nineteenth century, most physicists believed that light was a wave phenomenon; the first few decades of the twentieth century brought developments that began to destabilize this view.[10] However, at the turn of the century, the wave model was dominant because it could better explain a wide range of known optical phenomena: reflection, refraction, diffraction, and interference.

Mathematically, a wave is a function that has two properties: a wavelength and amplitude. The wavelength is the distance between the crests of the wave. The amplitude is the height of the wave crest. Figure 2.7 demonstrates the relationship between these parts of a wave.

[10] As Wheaton notes, the period between 1896—the year x-rays were discovered—and 1927—the year Schrödinger published his wave equation—was a tumultuous time for theories of matter and energy. His book *The Tiger and the Shark* is a thorough historical review of developments in physics during this period.

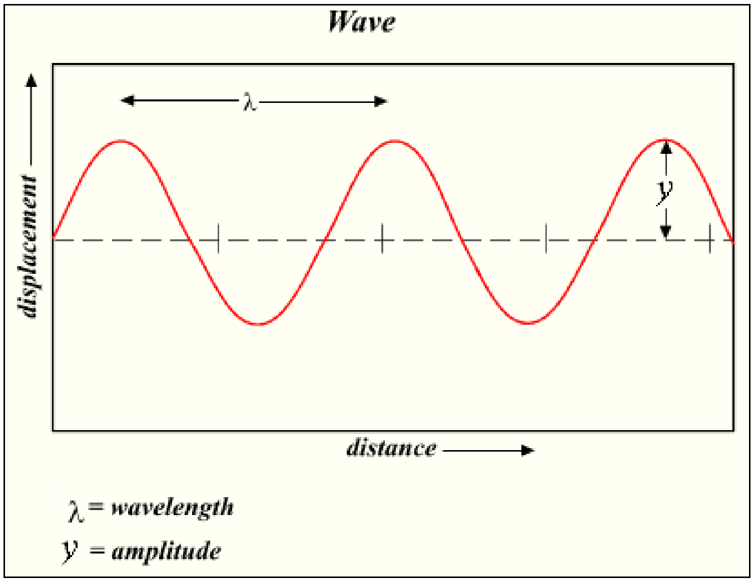

Figure 2.7: A waveform. Source: Oleg Alexandrov. Wikimedia commons.

Phenomena that obey wave functions behave predictably when encountering specific kinds of objects. When a wave changes direction after making contact with a solid object, the wave is *reflected*. This is the behavior exhibited by light bouncing off a mirror or sound bouncing off the walls of a canyon. Figure 2.8 is a diagram of *specular* or mirror reflection. When wave P encounters the mirror, it reflects as wave Q at an angle that is equivalent to the angle of incidence.

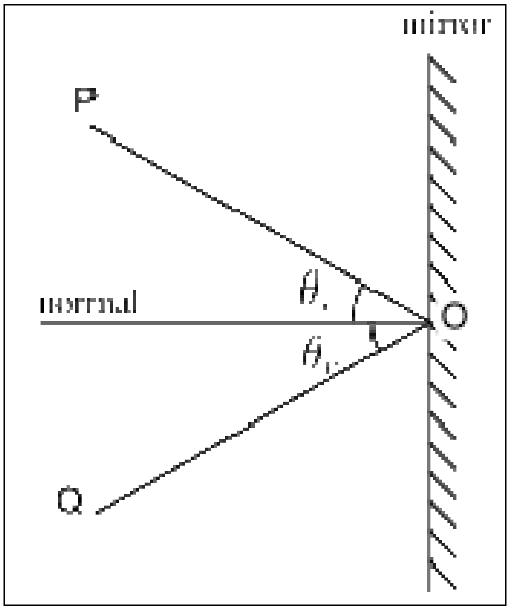

Figure 2.8: Specular (mirror) reflection. The angle of incidence equals the angle of reflection on a mirror. Source: Johan Arvelius. Wikimedia commons.

When a wave changes direction after entering a new medium, it is *refracted*. For example, a straight pole looks bent when partially submerged in water because the light waves behave differently in water than they do in the air. *Diffraction* occurs when waves change direction and interact with each other after encountering a barrier with holes or gaps. Such a barrier is called a transmission grating. When light rays hit the grating obstacle, some rays pass through the slits or holes at new and different angles. Two phenomena can occur when waves interact after they encounter a diffraction grating —destructive and constructive interference. If two of the altered waves meet each other when their wavelengths are out of phase (i.e., the wavelengths of parallel waves do not align), they cancel each other out in a process called *destructive interference,* which is demonstrated in Figure 2.9. If two waves are in phase (i.e., the wave crests and troughs of two waves are aligned), they are constructively amplified, and they produce a wave of greater amplitude. This *constructive interference* is demonstrated in Figure 2.10, and it is sometimes referred to as amplification.

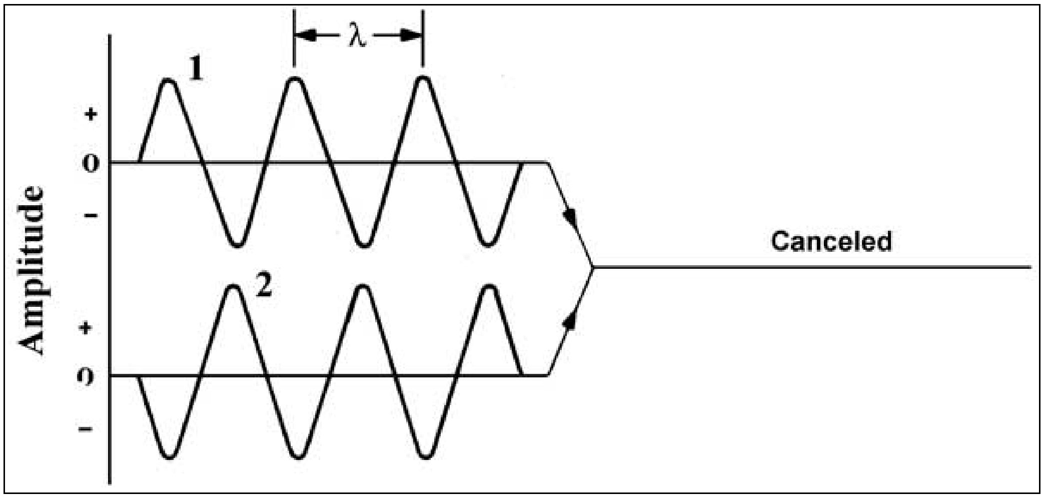

Figure 2.9. Destructive interference.

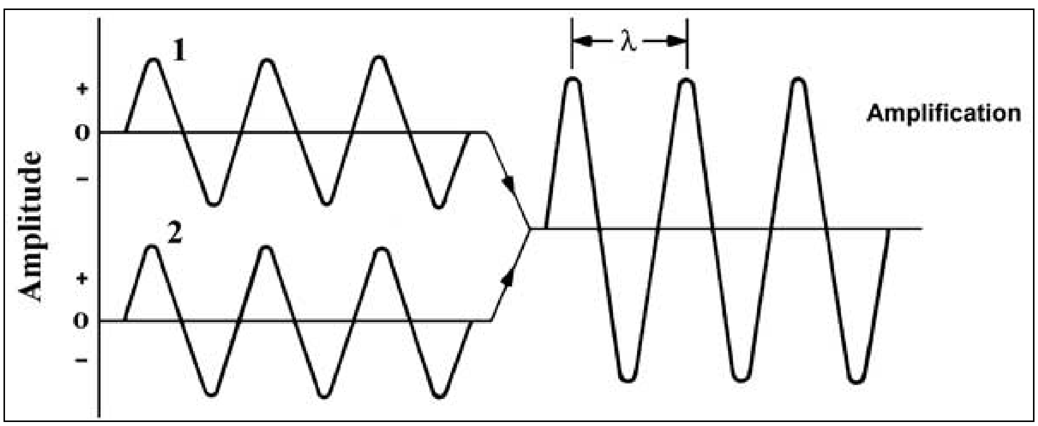

Figure 2.10: Constructive interference.

When visible light waves are diffracted, the combination of constructive and destructive interference creates visible diffraction effects. For example, in the "double slit" experiment (Figure 2.11), a solid obstacle with two slits is placed before a light source. Some waves travel straight through the gaps in the screen, while others emerge at new angles because they strike the edges of the apertures. After the waves emerge from the slits, they interfere with each other. Some waves interfere constructively and others interfere destructively, and these interactions result in a pattern of intense stripes.

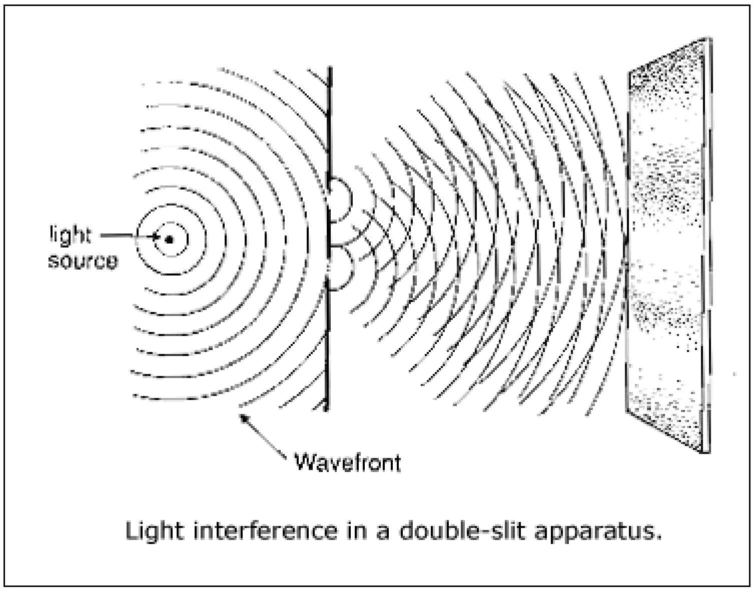

Figure 2.11: The double-slit experiment.

Different gratings—i.e., different patterns of slits or holes—create different interference events resulting in different diffraction patterns. For example, a single hexagonal opening will create a pattern like the one in Figure 2.12. A cross-slit grating will create a pattern like Figure 2.13.

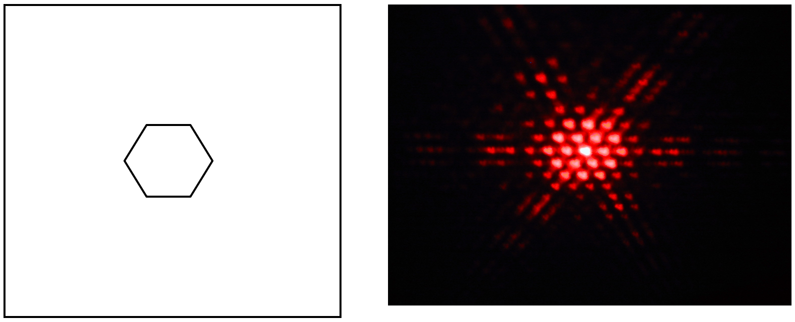

Figure 2.12: A hexagonal diffraction grating produces a pattern with six-fold symmetry.

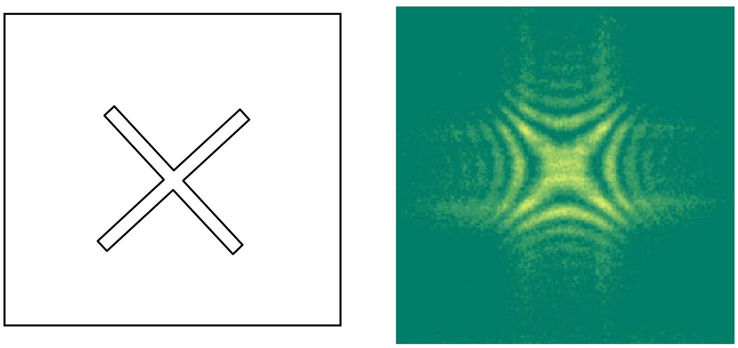

Figure 2.13: A cross-slit diffraction grating produces a pattern with four-fold symmetry.

Basic optical concepts—especially reflection and diffraction—were important premises for researchers studying x-rays. From 1896 through 1912, scientists developed an array of experiments in their attempts to justify or deny that x-rays had the same properties as visible light. If they observed or failed to observe diffraction and reflection under controlled conditions, they could determine if x-rays were particles or waves.[11] Max von Laue's ultimate contribution was his demonstration that x-rays *interfere* and hence *diffract* like visible light waves when they interact with crystal lattices. Though the Braggs ultimately agreed that x-rays are waves that interfere, they mathematically and visually explained the process that formed the Laue photographs in terms of *reflection* from crystal planes. Crystals and crystal

[11] For a thorough review of various successful, failed, and inconclusive x-ray diffraction experiments, see Wheaton's *The Tiger and the Shark*.

lattices are described in the next section to clarify the relationships between the optical phenomena just described and the crystal structures that these scientists claimed to elucidate.

Crystallography

Like optics, philosophical discussions of crystals can be traced back to the ancients. However, the science of crystallography—the formal study of crystal structure—emerged toward the end of the eighteenth century with the work of Roma d'Isle (1736-1790) and Rene-Just Hauy (1743-1822) (Senechal 43). These natural historians developed early laws of crystal structure that were extended and enhanced over the eighteenth and nineteenth centuries. By the end of the nineteenth century, crystallographers had revealed that all solid crystals at the macro-scale are formed by regularly repeating microstructures, now called unit cells. A unit cell can be thought of as the smallest regularly repeatable volume within a solid crystal; they are the invisible "building blocks" of larger visible structures. According to historian and crystallographer José Lima-de-Faria, early conceptions of the microstructures of crystals likely developed from scientists' experiences with cannonball configurations (100). Figure 2.14, a plate from José Lima-de-Faria's *Historical Atlas of Crystallography,* demonstrates various geometry-based visualizations of crystal structures starting with Kepler's work in 1611.

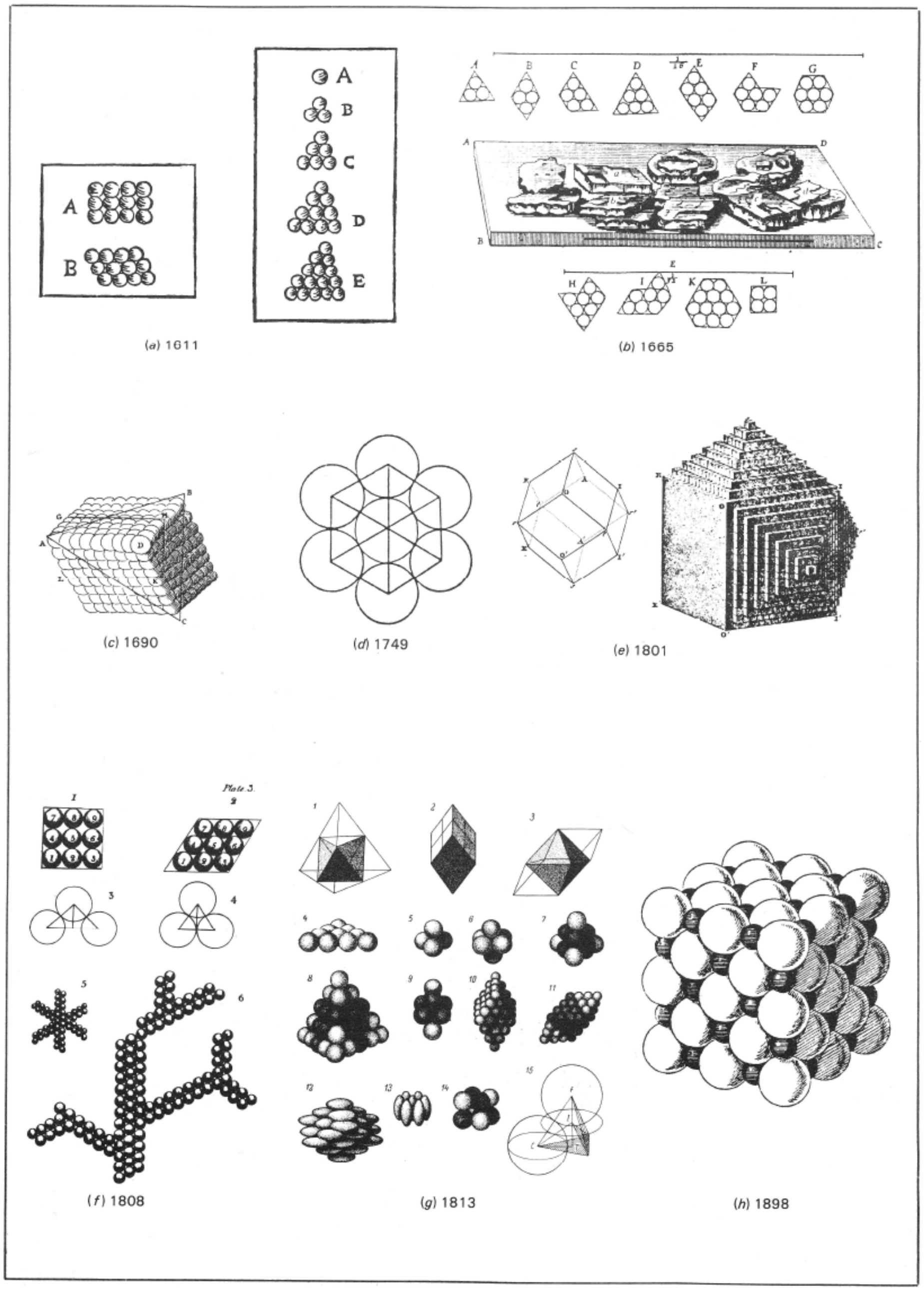

Figure 2.14: Conceptions of how crystals are formed by smaller units. Source: José Lima-de-Faria. *Historical Atlas of Crystallography.* (101).

Of special significance is figure (e) (enlarged in my Figure 2.15), which comes from Hauy's 1801 treatise on crystallography. Unlike his predecessors, Hauy believed that the fundamental microstructures of crystals—what he called *molécules intégrantes*—

were not spheres but angular units—parallelepipeds. As Hauy envisioned them, these polygonal solids were stacked in various configurations that could account for the various crystal faces of visible mineral crystals. Hauy's work would be influential on later theorists who classified and defined the finite set of microstructures that could produce all solid crystals.

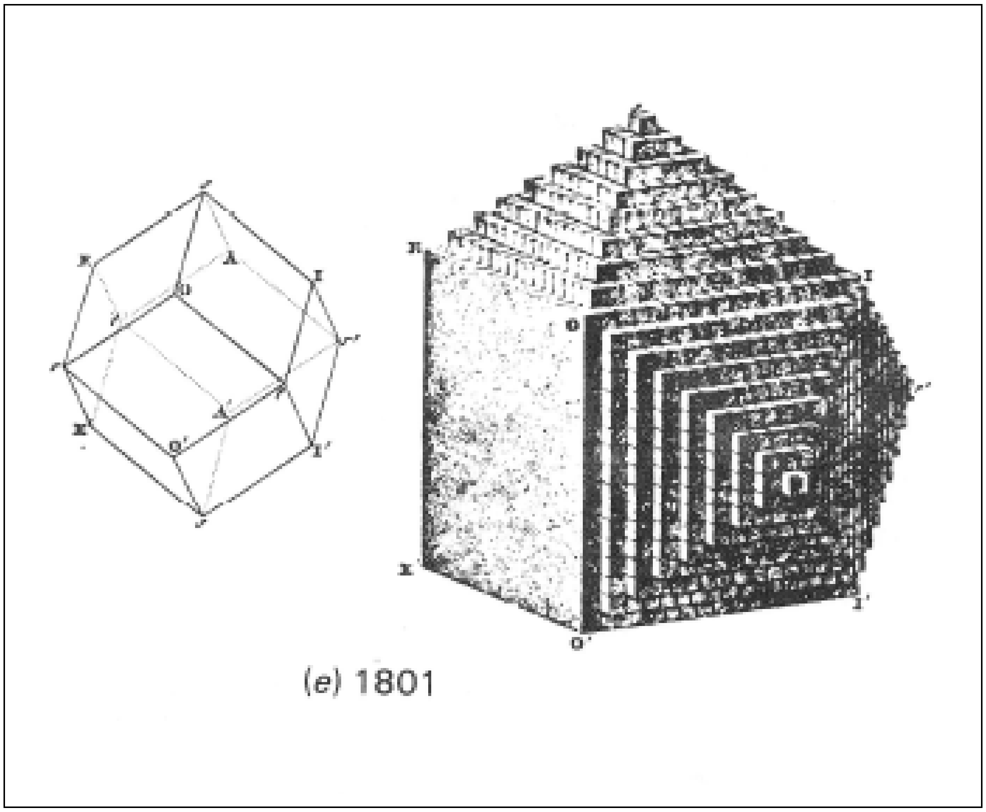

Figure 2.15: Enlarged view of Hauy's drawing of crystal packing. 1801. José Lima-de-Faria. *Historical Atlas of Crystallography.* (101).

In the early nineteenth century, crystallographers began incorporating Dalton's atomic theory into their considerations of crystal structure. That is, they began thinking of the polygonal molecular microstructures of crystal in terms of inter-atomic arrangements. By 1851, using only physical observation and mathematical deduction, crystallographers demonstrated that crystal "building blocks," now called unit cells, could only take one of fourteen atomic arrangements. These forms are now called Bravais lattices after Augustine Bravais (1811-1863), the crystallographer who defined six of the fourteen configurations. In Figure 2.16, the fourteen lattice configurations are sorted into six categories: triclinic, monoclinic, orthorhombic, tetragonal, hexagonal, and cubic. These categories of the unit-cell taxonomy are based on polygonal shapes; i.e., they are based on the angles formed between the atoms that make up the cell and the relative distances between the atoms. For example, in a triclinic cell, each of the angles forming each corner has a different value and none of the axial lengths is equal to any other length. In an orthorhombic unit cell, each vertex angle is a right angle, but none of the axial lengths is equal. The cubic unit cell has right-angled vertices in each direction, and all axial lengths are equal.

Additional unit-cell forms occur when additional atoms are part of the same unit-cell shape. For the purposes of this chapter, I will focus on the cubic unit cells because this class of cells figured prominently in the work of Laue and the Braggs. There are three kinds of cubic unit cell: the primitive (one atom at each corner of the cell), the body-centered (one atom at each corner of the cell and one in the middle of the cell), and face-centered (one atom at each corner and one in the middle of each cubic face). These cubic forms are demonstrated in the bottom row of Figure 2.16. Figure 2.17 demonstrates how the different cubic unit cells “stack” up to form larger lattice structures.

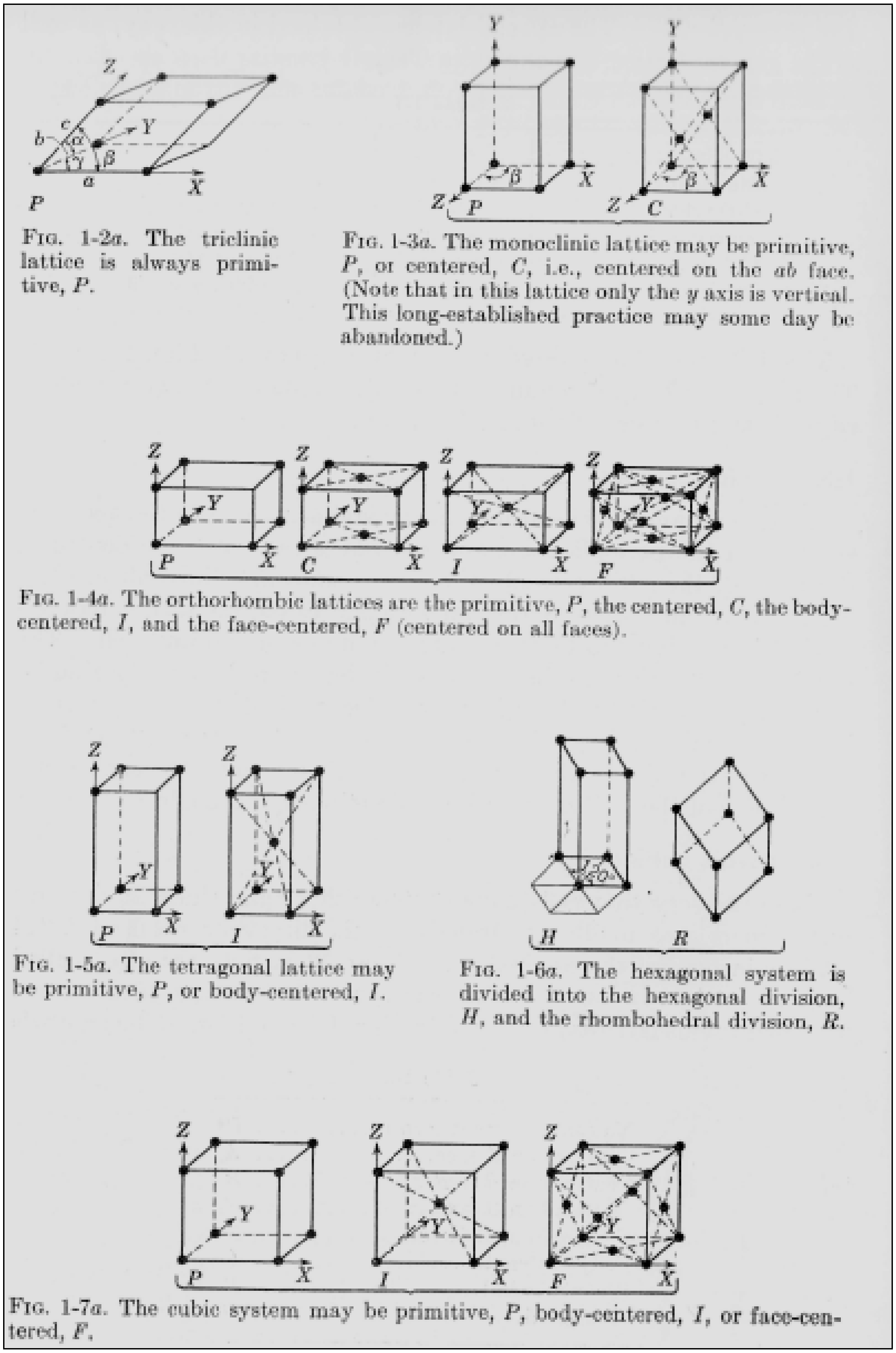

Figure 2.16: The fourteen Bravais lattices. Figure 1-7a of *X-Ray Crystal Structure.* Each point in the polygons represents an atom.

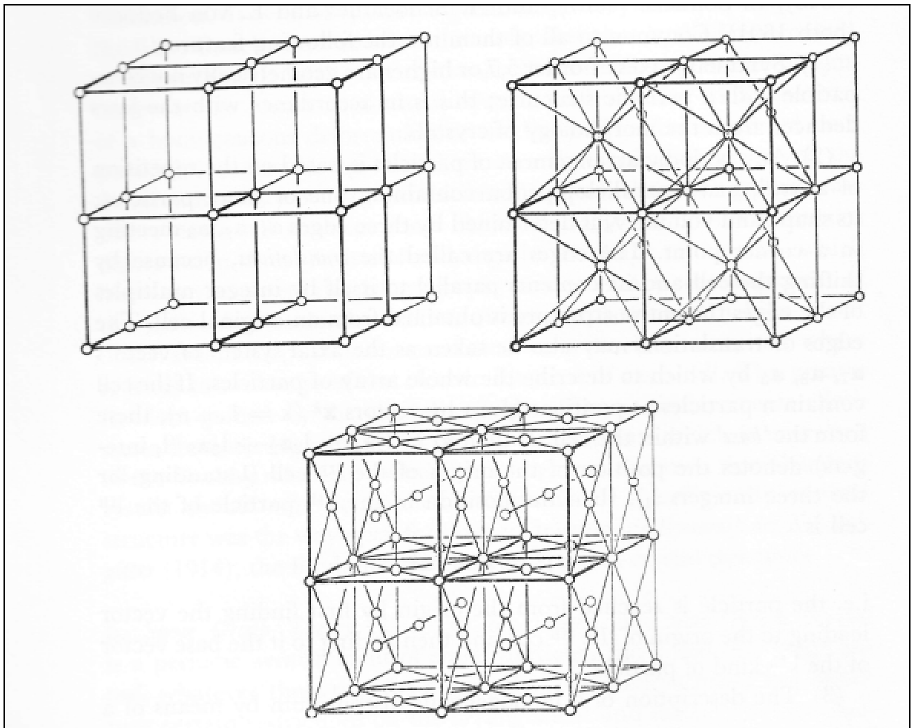

Fig. 3-3(1) a, b, c. The simple, body-centered and face-centered cubic Bravais lattices.

Figure 2.17: The cubic Bravais lattices in eight-cell formations. Figure 3-3(1) from Ewald, *Fifty Years of X-ray Diffraction*.

In any crystal lattice, a unit cell repeats millions of times. Imagine the forms in Figure 2.17 extending in all directions. With so many repeated atoms, there are other ways to conceptualize crystal space. One way is to think of a crystal in terms of different geometric planes. These planes can be defined by the three points where a specific plane intercepts the axes of an individual unit cell. The reciprocals (1/x) of these intercepts are known as Miller indices. Figure 2.18 shows a macro crystal whose faces have been identified by their Miller indices. Each digit in the three-digit code identifies where the plane crosses an axis. The first digit is the x-axis intercept, the second digit is the y intercept, and the third digit is the z intercept. A bar above the number indicates a negative value for the intercept. If a plane does not intercept an axis, it is assigned zero as a value.

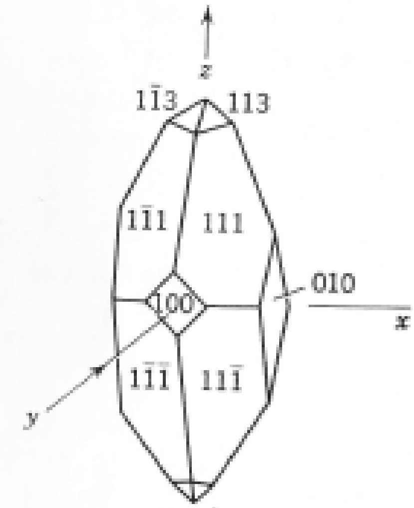

FIG. 1-12. The Miller indices marked upon the faces of a naturally grown crystal.

Figure 2.18: Figure 1-12 of *X-Ray Crystal Structure.*

These indices can be used to describe the geometry of crystals at the macro and micro levels. For example, the 113 plane (read as one-one-three) in Figure 2.18 cuts through a unit cell of dimensions a, b, c at the points a/1, b/1, c/3, while the 010 plane cuts across a set of unit cells at 0, a/1, 0. This relationship is demonstrated for a different crystal at the unit-cell level in Figure 2.19; the 312 and 100 planes of a cubic lattice are diagramed. Figure 2.20 demonstrates how a diagonal plane would intercept different atoms in unit cells in parallel rows.

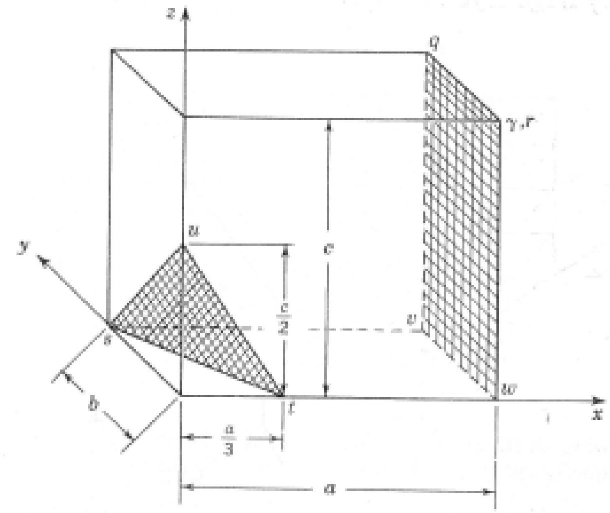

FIG. 1-8. Showing the position of the (312) plane and the (100) plane in the unit cell.

Figure 2.19. Figure 1-8 of *X-Ray Crystal Structure.*

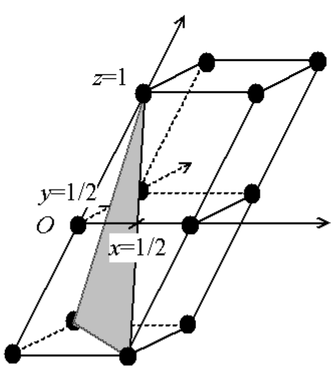

Figure 2.20. A plane with Miller indices of (221). Adapted from Christophe Chan. Wikimedia Commons.

Thinking of crystals as sheets of atoms in sets of parallel planes allowed crystallographers to move from macroscopic observations to inferences about microscopic structures. By studying the different macroscopic forms of crystals (including their visually observable cleavage planes), crystallographers could infer which kind of unit cells formed the lattice.

Understanding the architecture of crystal structure is important because Laue and W.L. Bragg each based their claims on particular unit-cell geometries. Laue based his calculations on the assumption that zinc sulfide (ZnS), the crystal used to produce the symmetrical pattern, is a simple cubic lattice; this assumption was based on the work of Groth, the chief crystallographer in Munich. Bragg thought ZnS formed a face-centered cubic lattice; he cited British crystallographers Pope and Barlow. These small differences in sources and premises had significant consequences for each of their interpretations of the visual evidence of x-ray diffraction.

Before the work of Laue and the Braggs, experimental verification of the specific structures of complex crystals was impossible. Optical microscopes could only resolve the surface features of crystal structures, and these observations were limited by the resolving power of the available microscopes. Though macro-examination of a visible crystal *suggested* the invisible structure of the molecular unit cell, no instrument could verify the arrangement of the invisible atoms; thus, these claims were inferential and in many cases inconclusive. Crystallographers accomplished much with the tools they had, but they could not verify crystal structures at molecular scales until Laue and the Braggs developed the fundamental science for x-ray crystallography. Their work allowed scientists to cross the molecular-resolution threshold in the observation of crystals. However, this was not the purpose of originally applying x-rays to crystals. The earliest work was directed to the task of defining x-rays themselves by using the crystal lattice as a diffraction grating. The inspiration for this project developed during a casual conversation between Max von Laue and an advice-seeking doctoral student.

Munich 1912: The Immediate Context of the Laue Experiments

Max von Laue (1879-1960) was a student of the renowned physicist Max Planck. After completing his doctoral work under Planck in 1909, Laue joined Arnold Sommerfield's Institute for Theoretical Physics at the University of Munich, where he worked primarily on mathematical problems related to traditional optics. In February of 1912, Sommerfield sent his student Paul Ewald to Laue, so Laue could help Ewald with some theoretical problems related to his dissertation on the diffraction of visible light through polarized crystals. Laue could not help Ewald with his specific problems, but during the course of their conversation, Laue suggested, "someone should irradiate crystals with shorter waves, i.e. x-rays" (Laue "My Development" 293). Pursuing this casual suggestion would earn Max von Laue a Nobel Prize (1914) and an esteemed place in the history of physics.

Laue's brainstorm was the coalescence of two ideas. First, it had been conjectured that atoms of solid substances were spaced nanometers apart. Second, some experiments suggested that x-rays were waves of very small wavelengths—10^{-8} to 10^{-9} cm. Combining these two ideas, Laue hypothesized that solid crystal lattices with nano-scale spacing were small enough to interact with the nano-scale wavelengths of x-rays. Presumably, the interaction would cause the x-rays to diffract just as visible light rays diffract when they pass through optical diffraction gratings.

Laue's "suggestion" was taken seriously by Walter Friedrich and Paul Knipping, two young scientists who offered to help Laue design an apparatus to test his idea. Both Friedrich and Knipping had studied x-rays under Röntgen, and their expertise with x-ray equipment allowed the project to proceed quickly. After resolving a few design issues, they developed a method for testing Laue's intuition. Figure 2.21 is a photograph of their device. Figure 2.22 is a schematic diagram.

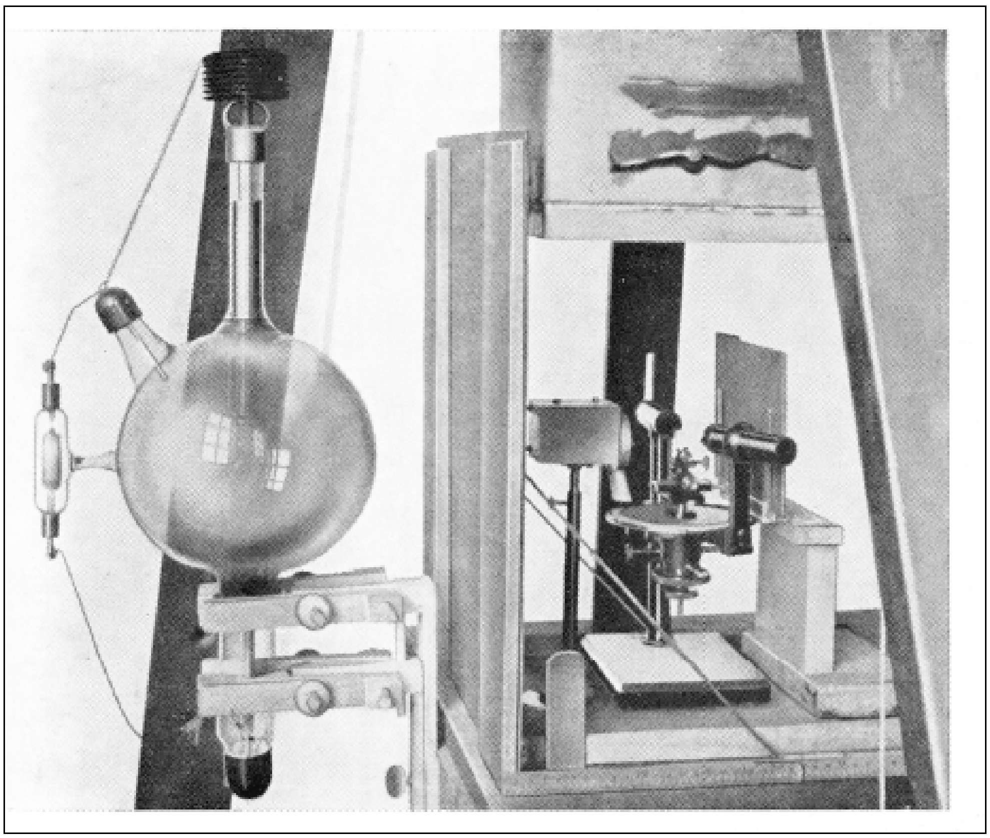

Figure 2.21. Figure 4-4(2) in Ewald, *Fifty Years of X-ray Diffraction*, unpaginated plate.

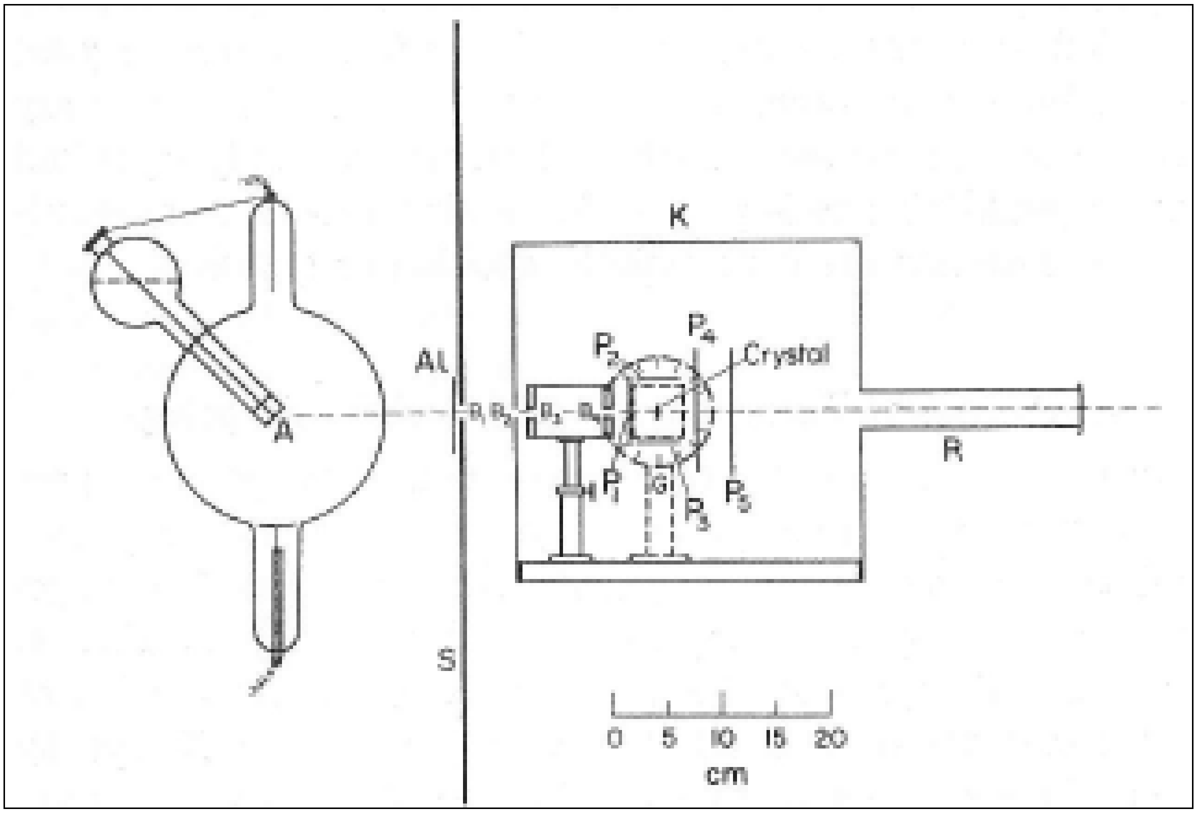

Figure 2.22. Friedrich, Knipping, and Laue. Figure 1. 1912. Reproduced in *X-ray and Neutron Diffraction*. G. E. Bacon. 1966: 98.

As represented in Figure 2.22, the x-ray source (A) emits rays that are focused into thin beams of x-rays by passing through an aluminum screen and a series of lead shutters (B1-B4). These beams strike a crystal sample that is held at a measurable angle with respect to the incident beam. Photographic plates P1 to P5 are positioned around the sample to record the beams that scatter in different directions when they interact with the crystal. Figure 2.23 contains four of the eleven x-ray photographs reproduced in the first Laue article.

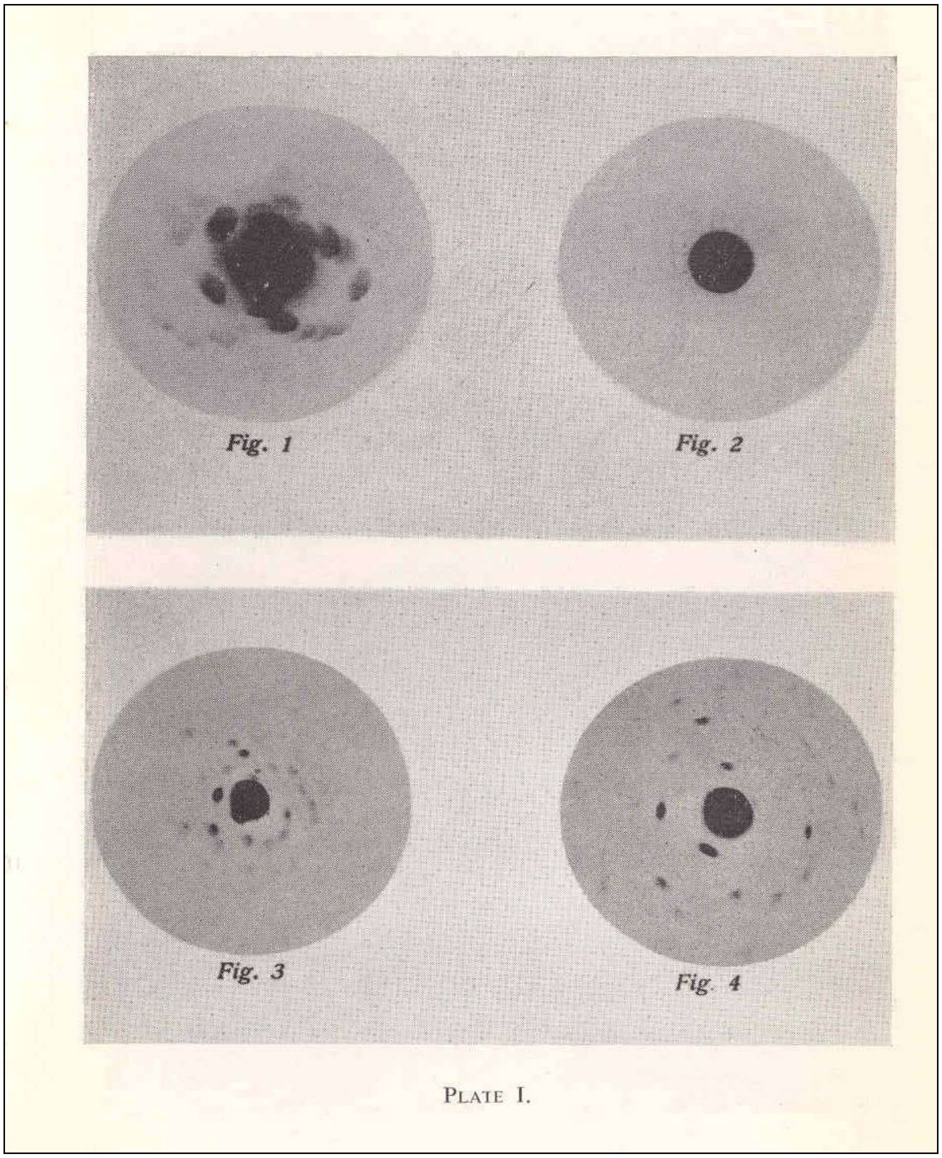

Figure 2.23. Friedrich, Knipping, and Laue. 1912. Plate 1, Figures 1-4. Reproduced in *X-ray and Neutron Diffraction*. G. E. Bacon. 1966. Unpaginated plate.

Though these spotted images might look insignificant, they and others like them were vital for subsequent arguments about the process that formed them. The real "prize" of the early work was Laue's highly symmetrical photograph of the cubic crystal zinc sulfide (ZnS) (Figure 2.24). The four-fold symmetry of the pattern provided the visual evidence Laue needed to validate equations he had been developing to account for earlier images.

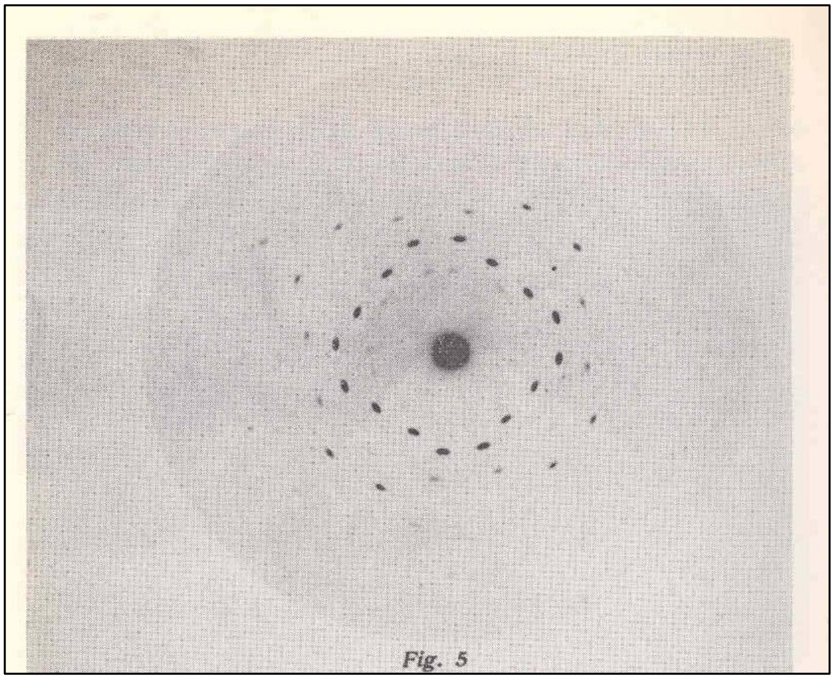

Figure 2.24. Figure 5 of Friedrich, Knipping, and Laue. 1912. Reproduced in *X-ray and Neutron Diffraction*. G. E. Bacon. 1966. Unpaginated plate.

Much of Laue, Friedrich, and Knipping's paper and much of Laue's separate simultaneously published article are dedicated to describing the development and verification of equations that could account for three-dimensional diffractions events. I will not rehearse these mathematical proofs here; however, the three equations in Equations 2.1 indicate the conditions for an intensity spot to appear:

$$I = \frac{|\psi(\alpha,\beta)|^2}{R^2} \frac{\sin^2 MA}{\sin^2 \frac{1}{2}A} \frac{\sin^2 NB}{\sin^2 \frac{1}{2}B} \frac{\sin^2 PC}{\sin^2 \frac{1}{2}C},$$

where

$$A = \frac{2\pi}{\lambda}\{a_{1x}(\alpha - \alpha_0) + a_{1y}(\beta - \beta_0) + a_{1z}(\gamma - \gamma_0)\}$$

$$B = \frac{2\pi}{\lambda}\{a_{2x}(\alpha - \alpha_0) + a_{2y}(\beta - \beta_0) + a_{2z}(\gamma - \gamma_0)\}$$

$$C = \frac{2\pi}{\lambda}\{a_{3x}(\alpha - \alpha_0) + a_{3y}(\beta - \beta_0) + a_{3z}(\gamma - \gamma_0)\}$$

Equations 2.1: Laue Equations. Reproduced in Ewald, *Fifty Years of X-ray Diffraction* (49).

Because zinc sulfide (ZnS)—the target crystal that produced the image in Figure 2.24—was assumed to be a simple cubic lattice—the distances between atoms were assumed to be equal in all directions. This allowed Laue to equalize many of the variables in the general equations, which simplified the math. The Laue equations for the simple cubic lattice could be reduced to the following three equations:

$$\alpha = h_1 * \lambda/a$$

$$\beta = h_2 * \lambda/a$$

$$1 - \gamma = h_3 * \lambda/a$$

In these equations, α, β, and γ are the cosines of the vectors that form the spots; h_1, h_2, and h_3 are integers; λ is the wavelength; and *a* is the length of the unit cell.

Laue's equations explain the spots by articulating the conditions required for multiple waves moving in 3-dimensional space to interfere constructively to produce an amplified wave that could leave the intensity spots. As the elder Bragg later commented in a lecture to the Royal Institution, "[Laue's] mathematical analysis is too complicated to describe now, and indeed it is not in any circumstances easy to handle" (W. H. Bragg 797). This complexity might have directed Laue's decision to support his equation by creating an explicit analogy to optical phenomena:

> The curves $1 - \gamma = h_3 * \lambda/a$, which are due to the periodicity in the direction of the ray, have an analogy in optics, known since the days of Newton, in the so-called Quetelet rings. These are formed in the presence of dust on a smooth glass plate, strongly reflecting on the back…. What occurs is that the incident and reflected light waves at the same dust particle undergo interference with one another. For

> perpendicular incidence, the maxima lie on the curve given by equation (9), but are much flatter than in the case we are considering, since only two waves undergo interference. If the dust particles could be arranged in a regular cross lattice then the analogy with the interference at crystals would undoubtedly be closer (95).

This comparison, though conceptually and mathematically plausible, was not convincing to others. Though internal peer responses to the Laue, Friedrich, and Knipping experiments were positive, some colleagues at Munich were skeptical of the theoretical and mathematical explanation. Ewald's recollection of Röntgen's visit to the lab indicates a mixed reaction: "He was deeply impressed by the photographs, but [Röntgen] held back on the interpretation as diffraction" (Ewald 44).

Laue, Friedrich, and Knipping communicated their results to the Bavarian Academy of Sciences through Sommerfield (a member of the academy) in June of 1912. Laue presented the results at the Berlin Physical Society in June. Reactions there were positive. As Max Planck recalls, "When the first typical Laue diagram became visible…a general, slightly restrained 'ah' propagated through the gathering. Each of us felt that a great achievement had been made" (qtd. in Eckert et al. 50). The symmetry of the ZnS image was immediately obvious and compelling to this scientific audience.

On his way back from Berlin to Munich, Laue visited Wurzburg and discussed his work with Wilhelm Wien's physics group. Among the attendees at the Wurzburg discussion was a visiting Norwegian physicist, Lars Vegard.[12] Vegard, a

[12] There is some inconsistency in the historical scholarship regarding how the Braggs first heard of the experiment; however, the best-supported claims indicate that Vegard was the Braggs source. Jenkin

friend of British physicist William Henry Bragg, requested a copy of one of Laue's photographs, and Laue gave it to him. Vegard sent the image to Bragg with a letter describing Laue's lecture. In this letter dated 26 June 1912, Vegard notes that there are gaps in Laue's explanation of the photographs:

> Regarding the explanation, Laue thinks that the effect is due to diffraction of the röntgen-rays by the regular structure of the crystal…. He is however, at present unable to explain the phenomenon in its details, and there are several difficulties from the diffraction point of view.
>
> On the other hand… it is not easy to see how the corpuscular theory of röntgen-rays can explain the scattering into such sharp bundles of parallel rays…. But whatever the explanation might be, it seems to be an effect of the most fundamental nature. (Vegard qtd. in Jenkin 380)

William Henry Bragg showed the letter and the photograph to his son William Lawrence Bragg. The Braggs replicated the Laue experiment and reinterpreted his results. Their story is described at length in the next section.

While the Braggs were busy thinking about the Laue experiment, Laue and his collaborators were busy publishing. Two papers were published in the *Proceedings of the Bavarian Academy of Science* in 1912: a coauthored paper by all three scientists and a theoretical paper authored solely by Laue. Each of these articles was republished in the *Annalen der Physik* (*Annual of Physics*) in 1913. Laue would

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(2001) included passages from the letter, in which Vegard explicitly states that he sent the photograph to Bragg.

eventually win the Nobel Prize (1914) for this work; however, his papers were not the last word on x-ray diffraction.

<u>Leeds and Cambridge: 1912: The Intellectual Context of the Bragg Reaction</u>

By 1912, William Henry Bragg was an established figure in British science. Beginning his career and starting a family in Adelaide, South Australia, the elder Bragg established his research credentials with his work on alpha particles and his critique of the exponential law of absorption (Wheaton 82). This research program led the elder Bragg to believe that x-rays were particle phenomena, and he developed a hypothesis—the neutral-pair hypothesis—to account for his position.[13] The elder Bragg was one of the staunchest supporters of a corpuscular (or semi-corpuscular) conception of x-rays, and he publicly debated this position with Charles Barkla—a British proponent of wave-based x-rays —in the pages of *Nature* in 1908. Ultimately, Barkla's work and the Braggs' own work with the Laue photograph would undermine the neutral-pair hypothesis, and Bragg eventually accepted that x-rays were at least partially wave phenomena. I mention these earlier events here because the initial response of both Braggs to the Laue photograph was to explain the diffraction pattern in terms of the elder Bragg's corpuscular neutral-pair hypothesis.

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[13] According to Bragg's short-lived neutral-pair hypothesis, x-rays and gamma rays are particle phenomena and the particles are paired elements—a negative electron and a partially positive particle. For Bragg, the neutrality of the particle pair could account for the penetrating power of x-rays and gamma rays, since the small neutral particles would not be deflected or attracted as they interacted with charged matter at the atomic level. Differences in the behavior of x-rays and gamma rays initially confounded Bragg, when he tried to account for both phenomena with the neutral-pair concept.

When this thesis was challenged by evidence that x-rays were waves, Bragg sought a conceptual compromise. In a 1907 paper entitled, "The nature of Röntgen Rays," Bragg proposed that x-rays were a composite phenomenon of fast moving electromagnetic impulses and slower moving neutral-pair particles. In some respects, Bragg's work conceptually prefigures the wave-particle duality that would become solidified by the work of De Broglie, Heisenberg, and others.

William Henry Bragg returned to England in 1909 as Cavendish Professor at the University of Leeds. William Lawrence Bragg followed his father into a career as a research scientist, studying science at Cambridge University. In 1912, the younger Bragg was just twenty-two years old, and he had just finished his first year as a research assistant at the Cavendish laboratory at Cambridge.

The Bragg family was on a summer holiday on the Yorkshire coast, when the elder Bragg shared the letter and image that Lars Vegard sent to him from Wurzburg. Father and son discussed the discovery on this vacation and conducted some initial experiments upon returning to the family home at Leeds. William Lawrence conducted additional experiments upon his return to Cambridge. The younger Bragg's recollection of this important summer describes the corpuscular approach that he brought initially to the case:

> During the summer of 1912 we had discussions on the possibility of explaining Laue's patterns by some other assumption than that of diffraction of waves, and I actually made some unsuccessful experiments to see if I could get evidence of "x-ray corpuscles" shooting down the avenues between the rows of atoms in the crystal. On returning to Cambridge to ponder over Laue's paper, however, I became convinced of the correctness of his deduction that the effect was one of wave diffraction—but also convinced that his analysis of the way it took place was not correct. (W.L. Bragg qtd. in Ewald 60)

William Lawrence Bragg presented a reinterpretation of the Laue photograph before the Cambridge Philosophical Society in November of 1912, and the text of his talk was published in the society's transactions in 1913. Bragg first presented Laue's argument and then replaced it with his own interpretation of the evidence. After the publication of this paper, the two Braggs collaborated on several works, including papers explaining how they used x-ray techniques to determine the crystal structure of salt and diamond. Further individual projects and collaborative endeavors developed new instruments and new methods for determining the structure of crystals.

Though there is an extensive archive of primary material that covers decades of work in x-ray crystallography, this chapter focuses on the earliest documents of both groups. The specific documents are described later. The next section reviews previous studies that have approached this case.

Review of Previous Studies

The significance of early work in x-ray diffraction to later developments in protein crystallography (e.g., the characterization of DNA) and theoretical physics (e.g., the wave-particle dualism) has made this episode in the history of science a popular subject for historical, philosophical, and sociological treatments. This section reviews significant contributions from these disciplines and demonstrates that the diffraction episode has not been the subject of thorough rhetorical consideration.

Historical studies of this case tend to focus on the Braggs, though there is material on Laue. These historical texts range from biographical sketches written by family members to reflective biographies and sketches by former students and colleagues to traditional scientific histories. The most recent biography, Graeme

Hunter's *Light is the Messenger* (2004) is the first "full-length" biography of W.L. Bragg, but this text has had mixed reviews (see Holmes; James; and Thomas).[14] There are also volumes commemorating the anniversaries of both the discovery of x-rays and the discovery of x-ray diffraction specifically. P. P. Ewald's commemorative collection *Fifty Years of X-Ray Diffraction* is especially rich; it contains autobiographical material by Laue, W. L. Bragg, and other early x-ray crystallographers. All of these texts are valuable sources for understanding the context, production, and reception of the arguments about x-ray diffraction.

The Braggs never published complete autobiographies, though some scholars quote from the younger Bragg's unpublished memoirs.[15] Ewald reproduces a brief fragment of W.L. Bragg's recollection of important events. The younger Bragg also documented some of the episode's history in various commemorative addresses. For example, he delivered a speech before the Franklin Institute in 1967 to "reminisce" about fifty years of x-ray diffraction research. Bragg also wrote a posthumously published history of the development of x-ray analysis, though this text is more technical than biographical. Also, both Laue and the younger Bragg delivered Nobel speeches in 1915 and 1922 respectively, where they include some commentary on historical details.

This case has also been included in thematic histories of physics, such as Wheaton's *The Tiger and the Shark: Empirical Roots of Wave-Particle Dualism* and the collection *Out of the Crystal Maze: Chapters from the History of Solid State*

[14] Reviews are particularly critical of Hunter's claim that Bragg "had no great influence" on twentieth century physics. Some reviews criticize Hunter's understanding of the science.

[15] Currently, this text is housed in the archive of the Royal Institution, which is closed for renovations through May 2009. I do not have access to this text.

Physics. Both texts offer additional historical commentary on the significance of the Laue experiment and the Braggs' extensions of it. These texts are rhetorically useful because they include reactions of individual physicists to specific developments.

The most critical analysis of the historical corpus is Forman's 1969 "Discovery of X-Ray Diffraction by Crystals: A Critique of the Myths." Forman argues that the story of Laue's sudden inspiration during his meeting with Ewald is a clever myth perpetuated to give the community of crystallographers an origin narrative. He then asserts that Laue's thinking was directed by the unique intellectual climate in Munich and its legacy of crystallographic work. Critiques of Forman, including L. D. Gasman's "Myths and X-rays" and Ewald's own reply have addressed Forman's argument. For my purposes, the particulars of this intellectual exchange are not significant, since the details of Laue's "eureka" moment are even less verifiable today than they were in 1969. Moreover, for my purposes, the generation of the idea is less important than the means that were used to communicate it persuasively. However, the exchange is interesting because it demonstrates the differences between strictly historical and more socially oriented approaches to science and the "turf wars" that these kinds of orientation differences can inspire.[16]

The early x-ray diffraction photos have also been incorporated into philosophical accounts of the role of images in scientific thought and scientific culture. Philosophical accounts use the case of the first x-ray diffraction photographs

[16] Gasman's critique of Forman's sociological orientation previews some of the same criticisms that would be leveled at later rhetorical approaches to science studies. Gasman defends science from Forman's claim that the problems of studying cultures of science are similar to the problems of studying primitive societies: "Is science then to be regarded as just another belief system, epistemologically on par with the beliefs of primitive societies? Perhaps science as the traditional paradigm of rationality should not be sacrificed with so much alacrity" (55-56).

to make claims about the conceptual mechanics of the scientific inference. For example, David Gooding's "Envisioning Explanations – the Art in Science" applies his dimensional model of visual scientific inference to describe Bragg's reasoning. While Gooding's interpretation of W.L. Bragg's work is interesting, it is oriented toward Bragg's own thought process more than Bragg's communication to and persuasion of others. For example, Gooding describes some pinpricked pages found in Bragg's collected papers to describe how Bragg developed specific inferences about the relationship between the distance of the photographic plate and the shape of the interference maxima spots. Though Gooding's analysis does add to our understanding of Bragg's invention process, it does not explain all of the rhetorically interesting features of Bragg's visualizations.

There are few studies of this case from rhetorical or cultural studies perspectives. Suzanne Black's "Domesticating the Crystal: Sir Lawrence Bragg and the Aesthetics of X-ray Analysis" is a notable exception. Black's work is in dialogue with James Elkins' work on artistic traditions in crystallography. Black notes that few scholars have been "tempted" to follow up on Elkins' observation that "abstract" x-ray diffraction photography and cubism were developing at the same time (257-258).[17] Black contends that a close examination of W.L. Bragg's writing can "reshape existing hypotheses about gender and crystallography and about crystallography and modern art" (258). Her arguments regarding Bragg's support of

[17] Elkins, an art historian, argued that the history of art could be reconfigured by looking to the development of non-art images, such as drawings of crystals. That is, major artistic developments, such as linear perspective or cubism, occur simultaneously in both high art and non-art settings, and specific kinds of images, such as crystallographic images, offer alternative ways to consider artistic traditions. Elkins also argues that the art history of crystallography could be a meaningful alternative narrative of the history of representation. Elkins is focused primarily on crystallographic representations before 1912.

women crystallographers and his “domestic” late-Victorian aesthetic are compelling; however, Black primarily addresses events and texts that were written after the texts that I examine in this chapter. I mention Black’s work because she both frames the critical neglect of x-ray crystallography in cultural studies of science and demonstrates the conceptual richness of the x-ray crystallography texts as subjects for analysis.

Finally, some studies have documented interesting parallel episodes in which x-rays were incorporated into other scientific cultures. Sociologist Bernike Pasveer demonstrates how medical practitioners worked with x-rays to develop new tests for tuberculosis. X-ray photographs of soft tissue, like that of the lungs, were not nearly as clear as images of harder materials, such as bones or bullet fragments. By comparing x-ray images of the lungs with the results of the contemporary tuberculosis tests—such as *percussion* (chest thumping), *ausculation* (stethoscope observation), and bacteriological studies of sputum—practitioners were able to transform ambiguous x-rays into more precise diagnostic instruments. The process that Pasveer documents is an interesting parallel to the development of x-ray crystallography. As mentioned previously, the x-ray photograph that captured the public’s imagination was the skeletal hand (and wedding ring) of Rontgen’s wife. Such an image was easily and immediately acceptable because everyone knew what bones looked like. But few people knew what a tubercular lung looked liked, especially in a live patient. The shadowy images of lungs needed to be argued into place as indications of healthy or diseased lungs. The fixation of this visual regime was possible because there were existing tests that could serve as points of comparison from which an image-reading regime could be built. The visual evidence was even more indirect in the case of the x-ray diffraction images. These photographs are not images of internal structures that people already knew without question, *and* there were no other empirical tests for verifying the structures. Thus, the crystallographers faced significant rhetorical tasks as they established the nature of x-rays and developed them into powerful means for elucidating crystal structures.

Methodological Review

As stated in the introduction, I align concepts from modern rhetorical theory, classical rhetoric, and other sources with semiotic concepts to tease out the relationships between visual forms used by scientists and the underlying structures of their arguments.

In this case, I am interested in arguments from sequential relations—such as causal arguments—and how scientific visuals participate in such arguments. As Perelman and Olbrechts-Tyteca note, causal links enable three types of argumentation:

> (a) argumentation tending to attach two given successive events to each other by means of a causal link;
>
> (b) argumentation tending to reveal the existence of a cause which could have determined a given event;
>
> (c) argumentation tending to show the effect which must result from a given event. (263)

I show how the apparatus of the Laue experiments and the visual artifacts it produced under controlled conditions provided the raw material to establish new causal links through which Laue and the Braggs made larger causal claims.

Cognition theorists Uwe Oestermeier and Friedrich Hesse have explored the possible roles and limitations of visuals in causal arguments. Synthesizing research from cognitive science, philosophy, and argumentations studies, they identify eleven types of positive (pro) causal arguments, ten types of negative (con) causal arguments, and six qualifications to causal arguments. They then compare this taxonomy to a taxonomy of visual representations (e.g., graphs, time-series diagrams, tables, photographs, animations, etc.) to show if and how specific types of visuals can support specific kinds of causal arguments. For example, they claim that photographs can support arguments based on covariation ("A caused B because B changes with A"), but they cannot support arguments based on counterfactuals ("A caused B because B would not have happened if A had not happened"). I introduce this clarification here because, as I will show, the various representations of x-ray spots offer specific advantages and disadvantages in the causal claims made by Laue and the Braggs.

For this case, I also am interested in arguments from analogy and how rhetorical analogies factor into the rhetorical structuring of visual artifacts. Laue and the Braggs both argue from analogies by comparing the ambiguous domain of x-ray optics with the more established domain of visible light optics.[18] These analogies

[18] When examining the application of analogy in this case, it is important to keep a historical perspective. Now we know that x-rays are electromagnetic phenomena like light. The differences between them are differences in quantity (i.e., the sizes of their wavelengths) and not differences in kind. However, in 1912 this relationship was still being established, and thus analogy was still an

generated arguments that were translated to visual artifacts, which constrained the inferences that could be extrapolated from the images.

Many rhetorical theorists have discussed the role of analogy in scientific argumentation. As Alan Gross notes, "Scientific reports and scholarly arguments are alike in the value they place on the heuristic function of analogy and on the rules of inference and evidence with which analogies and the hypotheses they generate must be examined" (*Starring the Text* 39). Belgian theorists Perelman and Olbrechts-Tyteca acknowledge a role for analogy in science; for them, it is a limited role:

> In the natural sciences, analogy—in our conception of the term—does nothing but provide support for creative thought.[19] It is here a question of going beyond analogy in order to infer a resemblance, with the possibility of applying the same concepts to both theme and phoros. By making the same methods applicable to them, the scientist tries to unite theme and phoros in a single field of investigation. (396)

Later in the same section of *The New Rhetoric,* Perelman and Olbrechts-Tyteca further discuss the relationship between scientific analogies and rhetorical invention:

> As a link in the chain of inductive reasoning, analogy finds a place in science, where it serves rather as a means of invention than as a means of proof. If the analogy is a fruitful one, theme and phoros are transformed into examples or illustrations of a more general law, and

---

important rhetorical tool. Laue and the Braggs had to establish an appropriate analogical relationship between the light domain and the x-ray domain to account for the visual evidence in the diffraction photographs.

[19] The Belgians' definition of analogy derives from the generic formulation A is to B as C is to D, where A and B taken together are the *theme*, or conclusion, and C and D are the *phoros,* or supporting terms. According to their definition, the theme and phoros must be from separate domains. If they are from the same domain, or sphere, the relationship is not an analogy but an example or illustration (372-373).

> by their relation to this law there is a unification of the fields of the theme and the phoros. (396-397)

I include these extended passages from the Belgians to demonstrate what they saw as the function of analogy in scientific rhetorical situations. Arguably, the eventual conflation of x-rays and visible light follows the generic move from analogy to identity described in *The New Rhetoric*; however, analogy seems to have played a significant and more argumentative role in the early crystallography papers.

More recently, rhetoricians have complicated and thickened the concept of rhetorical analogy in science. Keith Gibson (2008) has argued that analogy in science is not as limited as theorists have claimed. After surveying various treatments of analogy in science, Gibson shows how an Artificial Intelligence researcher used multiple analogies to establish a persuasive enthymeme; thus, in Gibson's example, analogies were essential components of the argument and not just preliminary tools of invention. In the same issue of *Technical Communication Quarterly*, Joseph Little (2008) notes that rhetoricians often acknowledge the role of analogy in science, but they tend to approach analogies macroscopically and to overemphasize the rhetor's ability to choose the domains of comparison. Such an approach limits the value of analogy as a lens for rhetorical analysis. Little summarizes his criticism in the following passage:

> Put another way, our history of scholarship shows that we tend to leave analogies in their aggregate form, choosing as our unit of analysis the analogy itself rather than the specific points of comparison generated by the analogy and enlisted by the rhetor or the audience. It

> also shows that when we do acknowledge that enlistment, we tend to frame it as a decidedly voluntary act, conferring almost unmitigated agency on the rhetor (220).

Gibson's and Little's recent studies demonstrate the need to study rhetorical analogies in science more closely. My analysis of the roles of analogies in this case contributes to this "thickening" line of scholarship in the rhetorical studies of science.

Different theorists have different views on the role of analogy in science; however, none seems to have addressed issues related to analogy and the scientific visual. In analyzing the visual interpretation done by both Laue and the Braggs, I demonstrate ways in which analogy participates in the process of technical visualization.

The third rhetorical concept highlighted in this chapter is the argument from incompatibility, and I am particularly interested in how visuals participate in incompatibility arguments. Perelman and Olbrechts-Tyteca observe that these quasi-logical arguments are based on the logical reasoning of contradiction; however, incompatibilities are rhetorically negotiable. For example, if a political candidate made two incompatible statements, he or she can resolve the incompatibility by claiming that the statements refer to different times or different contexts. As I show, Laue and the Braggs had to negotiate rhetorically the incompatibilities that threatened to undermine their respective explanations of the x-ray diffraction photographs; they used both verbal and visual means to preserve the consistency of their respective interpretations and to undermine alternatives.

In this chapter, I also consider the classical rhetorical concept of *taxis*, the selective division and arrangement of a subject. In "Visualizing a Bounded Sea: A Case Study in Visual *Taxis*," Lawrence Prelli discusses the persuasive effects of arrangement in visual arguments. He explains that "the Greek word *taxis*, the Latin word *dispositio*, and the English word 'arrangement' all pointed to the same phenomenon: the materials of a speech had to be strategically structured within coherent patterns of thought for maximum persuasive effect with particular target audiences" (92). Prelli argues that rhetorical *taxis* can help explain graphical choices. Laue and the Braggs both make rhetorical choices about arrangement to give coherence to their visual arguments.

Finally, with respect to semiotics, I am interested in how the argument types I just reviewed are coded with specific semiotic markers. Analogies are encoded through graphed equations that are similar to known optical forms. Causality is encoded through the semiotic differentiation across "before and after" photographs and with technical illustrations. Through these markers, Laue and the Braggs transform the naturally structured arrays of diffraction spots into highly structured visual arguments about complex physical phenomena.

<u>*Document Summary*</u>

The documents that mark the birth and early development of x-ray crystallography were published in a number of professional journals and societal transactions. I am primarily interested in the foundational studies of each group because this is where the science is least defined and the rhetorical nature of the activity is most pronounced. These documents are summarized in Table 2.1. The Laue articles and the younger Bragg's first article are the primary objects of analysis.

Date	**Authors**	**Title**	**Publication**
August 1912	Laue, Friedrich, and Knipping	"X-Ray Interference Phenomena"	*Proc. of the Bavarian Academy of Science*
August 1912	Laue	"A Quantitative Test of the Theory of X-Ray Interference Phenomena"	*Proc. of the Bavarian Academy of Science*
1913	Laue[20]	"A Quantitative Test of the Theory of X-Ray Interference Phenomena" (Same as Laue 1912 with additional notes at the end.)	*Annalen der Physik*
November 1913	William Lawrence Bragg	"The Diffraction of Short Electromagnetic Waves by a Crystal"	*Proc. of the Cambridge Phil. Soc.*
July 1913	William Henry Bragg and William Lawrence Bragg	"The Reflection of X-rays by Crystals"	*Phil. Trans. Royal Society*

Table 2.1: Documents

[20] The Laue, Knipping, and Friedrich article and the sole authored Laue article were both republished in the journal *Annalen der Physik* in 1913. Laue's sole authored piece had not been translated into English, so I have had this text translated by a professional scientific translator. My translation of the document is based on the 1913 version because the 1912 version was not available as a base copy. However, sources indicate that the 1913 text is identical to the original 1912 Laue paper with the exception of the *Zusatz* (Additions) section included as a postscript.

Summary of Laue's Arguments

As I show, Laue and his colleagues carefully develop their verbal, visual, and mathematical arguments. They explicitly document the changes they made to their apparatus, and they explain experiments that were conducted to justify specific premises. The primary argument is an argument about the nature of x-rays based on analogical reasoning. This argument is concisely stated in the following passage:

> If X-radiation truly consists of electromagnetic waves, it would be expected that when these atoms [of a crystal] are excited [by being struck with x-rays]…then the spatial lattice structure would give rise to interference phenomena; it would also be expected that these phenomena would be of the same nature as the lattice spectra already known in optics (89).

In other words, if x-rays are *like* visible light waves, by analogy they should interfere as visible light waves interfere. Laue's secondary claim is that the x-rays can be sorted into five distinct wavelength groups. Here I summarize the structure of both arguments as they were articulated in published papers:

Premise:	X-rays are electromagnetic waves.
Claim:	X-rays interfere with each other after interacting with crystal lattices.
Evidence:	X-ray photographs (photograms) with symmetrical spots.
Analysis:	Most of the spots can be mathematically explained by equations similar to equations from optics that are based on

geometry and trigonometry and supported by specific assumptions.

Secondary claim: Secondary x-rays of one of five specific wavelengths form each spot in the photogram; these rays are created when the x-ray beam resonates the atoms in the lattice.

Evidence: Mathematical reconstruction of a photogram.

Supporting assumptions: (1) ZnS forms a simple cubic lattice.

(2) The lattice structure is significant to the pattern.

(3) The x-rays forming the spots are homogenous characteristic radiation determined by the nature of the atoms struck by the x-rays. This assumption is supported by analogy and precedent; i.e., Barkla's work on characteristic x-ray radiation.

The various components of this argument were articulated verbally, mathematically, and visually.

Images to Arguments, Arguments to Images: Visual Rhetoric and the Laue Experiments

Laue's first two articles—the co-authored "X-ray Interference Phenomena" and the sole authored "A Quantitative Test of the Theory of X-Ray Interference Phenomena"–each refers to a common set of six image plates containing twelve figures—eleven photographs and one diagram. Though the forms of the eleven circular photographs are similar, they actually perform a range of rhetorical functions in the first text. The diagrammatic “Figure 6” is only referred to in the second paper. It is a visual rendering of the results of Laue’s equations; its close correspondence to one of the photographs was a rhetorical demonstration of the equations’ ability to explain the photographs. Because the diagram shares a plate with the most important photograph, it is often reproduced in scholarly treatments of this case; however, most sources do not discuss the figure and its complicated rhetorical purpose.

My Figures 2.25, 2.26, 2.27, 2.28 and 2.29 are Laue’s plates 1 through 5. I refer to the individual figures in these plates by putting Laue’s original figure numbers after my figure numbers. For example, “Figure 2.25.1” refers to Laue’s Figure 1 in my Figure 2.25.

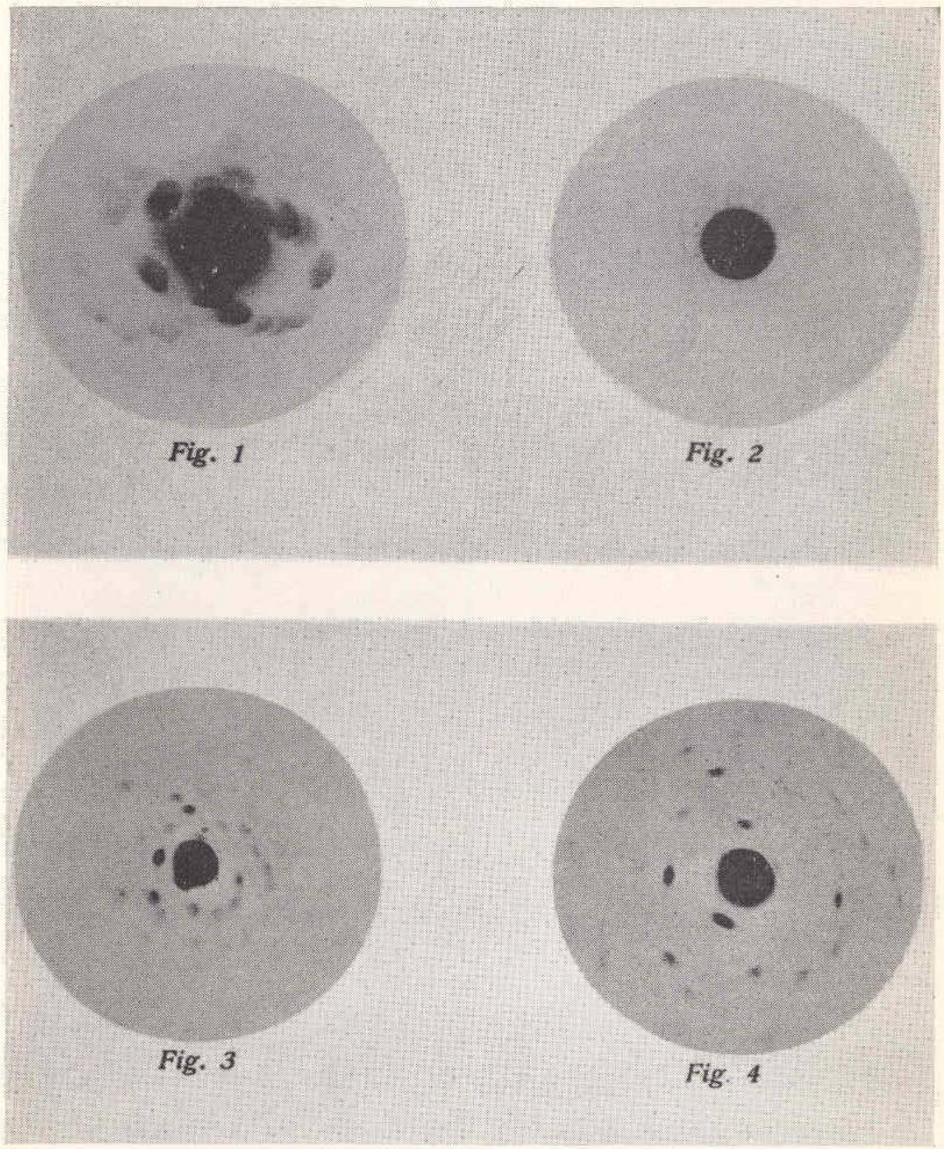

PLATE I.

Figure 2.25: Friedrich, Knipping, and Laue. 1912. Plate 1, Figures 1-4. Reproduced in *X-ray and Neutron Diffraction*. G. E. Bacon. 1966. Unpaginated plate.

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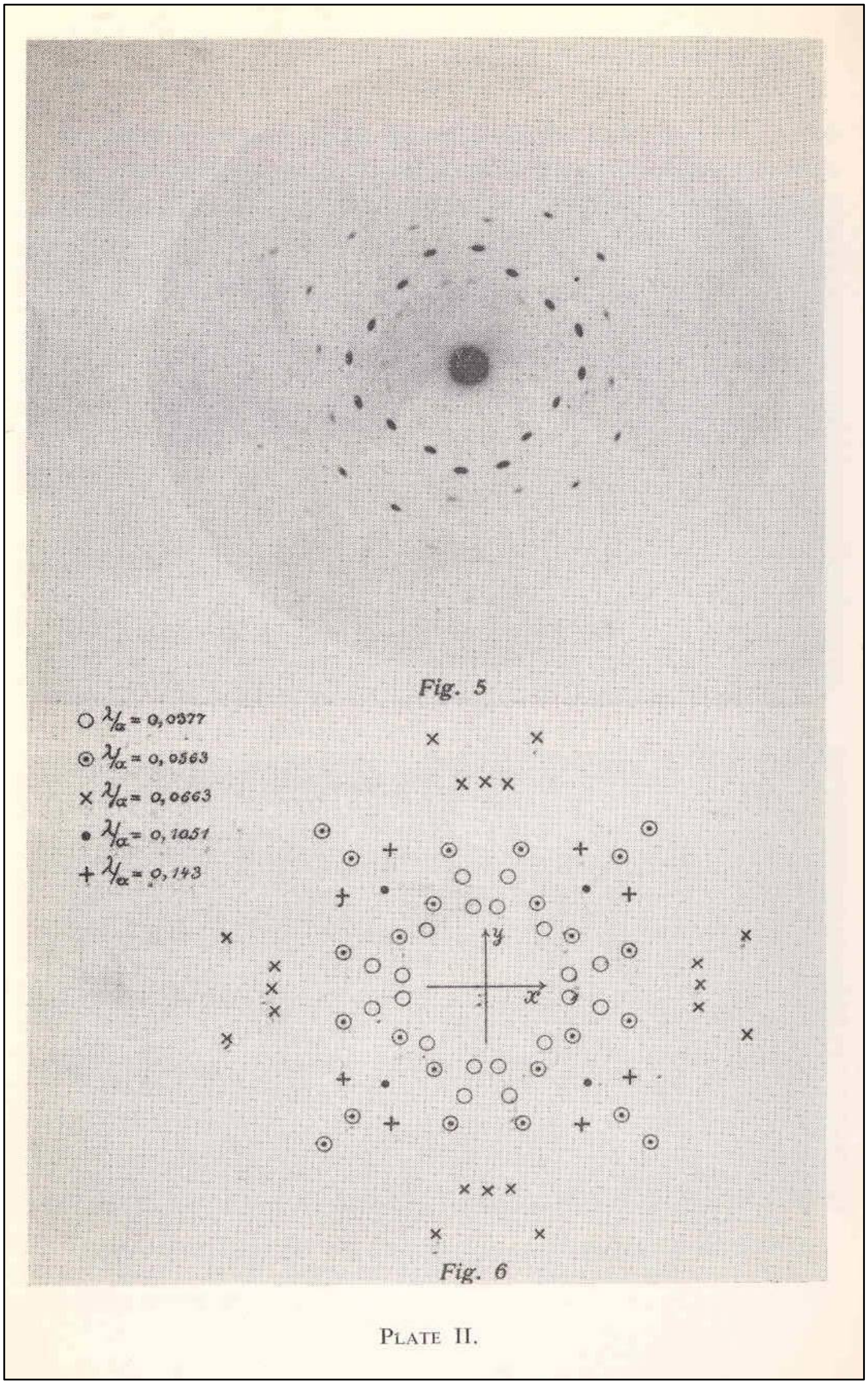

Figure 2.26: Friedrich, Knipping, and Laue. 1912. Plate II, Figures 5 and 6. Reproduced in *X-ray and Neutron Diffraction*. G. E. Bacon. 1966. Unpaginated plate.
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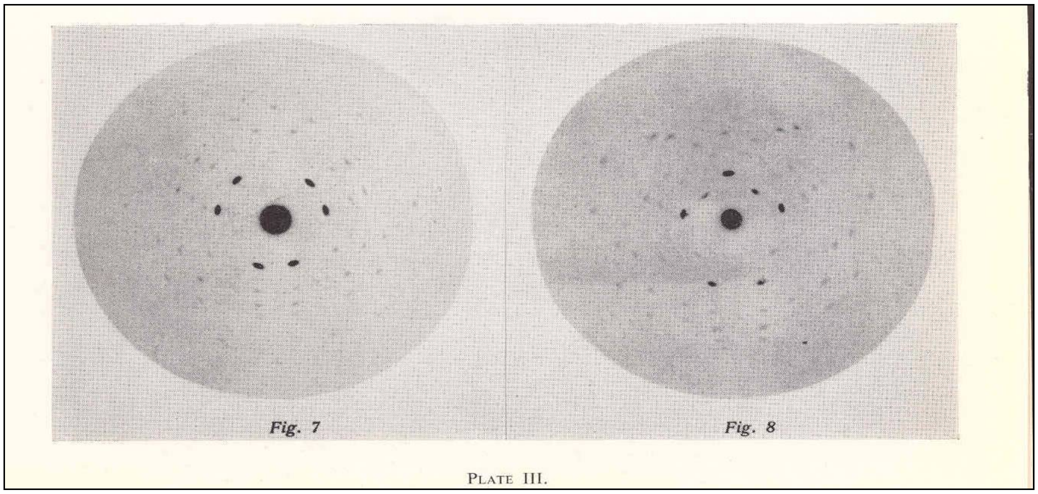

Figure 2.27: Friedrich, Knipping, and Laue. 1912. Plate III, Figures 7 and 8. Reproduced in *X-ray and Neutron Diffraction*. G. E. Bacon. 1966. Unpaginated plate.

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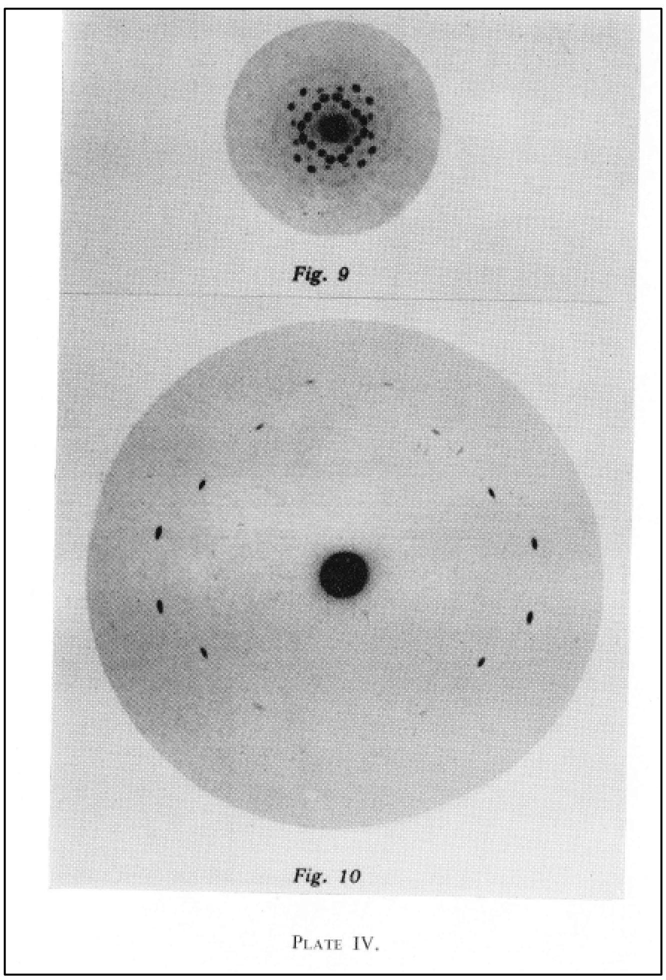


Figure 2.28. Friedrich, Knipping, and Laue. 1912. Plate IV, Figures 9 and 10. Reproduced in *X-ray and Neutron Diffraction*. G. E. Bacon. 1966. Unpaginated plate.

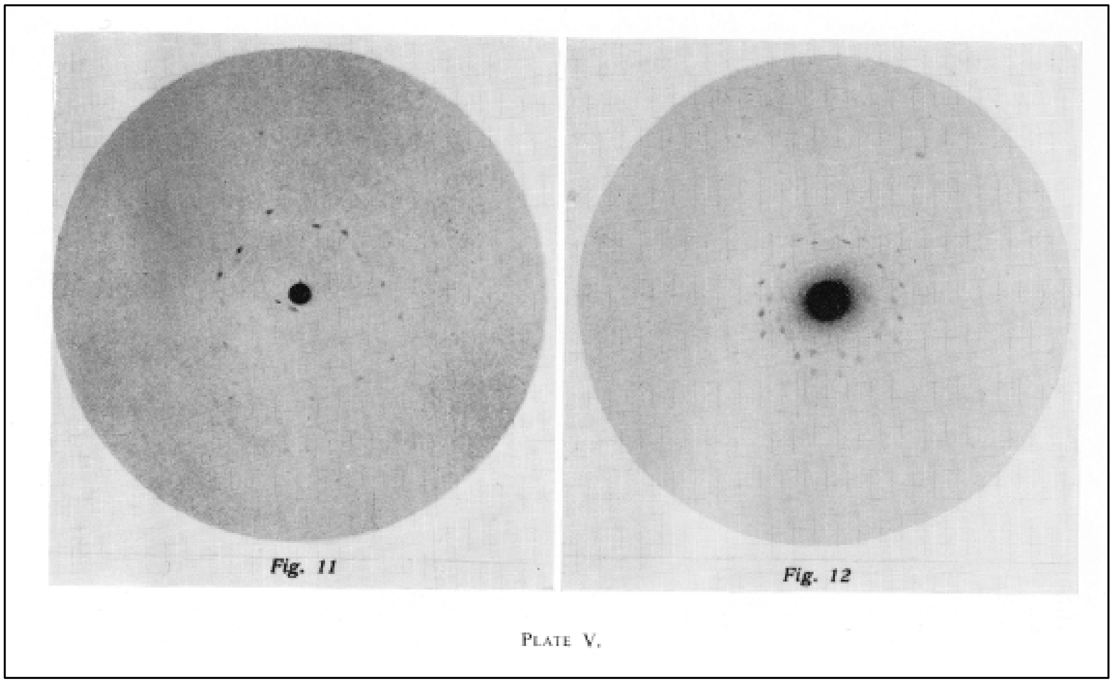


Figure 2.29: Friedrich, Knipping, and Laue. 1912. Plate V, Figures 11 and 12. Reproduced in *X-ray and Neutron Diffraction*. G. E. Bacon. 1966. Unpaginated plate.

Each of the eleven photographs serves one or more functions in the argument. The first four images are from early experiments with copper sulfate crystals. The seven other photographs are images of zinc sulfide.

Figures 2.25.1 to 2.25.4 explain the development of the experimental apparatus and the establishment of some foundational premises through causal reasoning. Each of these images is a photograph of copper sulfate (CuS), the first mineral they imaged with their machine. Copper sulfate is a triclinic crystal (no equal sides, no equal angles), thus it is not symmetrical like the ZnS that the group would latter use in the experiment Figure 2.25.1 is the image of the first exposure taken with the "definitive apparatus." In the text, Friedrich, Knipping, and Laue also refer to other images which are identical to 2.25.1 but which are not reproduced in the text. For example, they moved the crystal laterally without changing its distance from the x-ray source or the angle of incidence; thus, they irradiated a different portion of the crystal. The subsequent image was identical to Figure 2.25.1; thus, they claimed, “the phenomenon is independent of the portion of the crystal which is irradiated” (101). That is, the images are caused by the atomic lattice and not the macrostructure of the crystal.

Figure 2.25.2 demonstrates that the crystal structure is part of the causal chain forming the patterns. This image is of a crystal sample that has been ground into a fine powder. The impressive pattern in 2.25.1 disappears when a box of this powder is the irradiated specimen. There are a few “irregular” spots near the large spot made by the x-ray source; however, these small spots are barely visible. Thus, Figure 2.25.2 shows that the lattice structure is involved in the pattern-forming process because demolishing the lattice destroys the pattern. A difference of effect in the second image supports their claims regarding the mechanism of the cause.

Figures 2.25.3 and 2.25.4 demonstrate improvements to the lead shutters that narrowed and focused the x-ray beam. Each pattern is similar to the pattern in 2.25.1, but the more focused x-rays created smaller spots. Figures 2.25.3 and 2.25.4 are slightly different because the plate (P4) for 2.25.4 was positioned 35 mm from the x-ray source and the plate (P5) for 2.25.3 was set 70 mm from it. Laue compares Figures 2.25.3 and 2.25.4 to conjecture about the nature of crystal-x-ray interaction based on the differences:

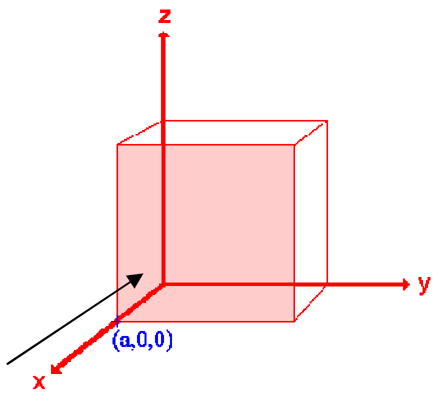
> It is noteworthy that the ratio of the distances crystal-P4 to crystal P5 is the same as the ratio of the sizes of the patterns on P4 and P5, from which it appears that the radiation proceeds in straight lines from the

> crystal. It should also be noted that the size of the individual secondary spots, in spite of the greater distance of Plate P5 from the crystal has remained unchanged. This may well be an indication that the secondary radiation leaves the crystal in the form of a parallel pencil.

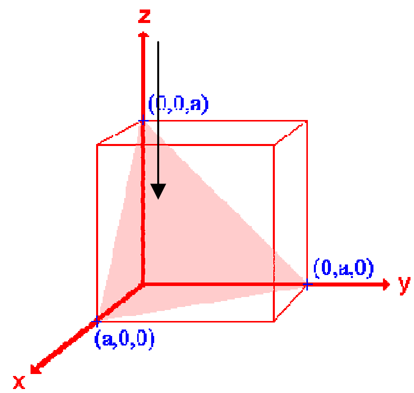
As I will show in the discussion of Figures 2.28.9 and 2.29.10, Laue pays close attention to the positions and sizes of the spots, but he does not note the differences in shape. This would be an important argument for Bragg.

Figure 2.26.5 is now the iconic photograph associated with the Laue experiments. This image is the diffraction pattern of zinc sulfide (ZnS), also known as zinc blende. Zinc blende was chosen as a crystal sample because its crystal lattice was supposed to be formed of simple cubic unit cells. Also, other researchers indicated that heavy zinc atoms should be especially good x-ray resonators. Thus, the experimental setup that produced this image reflects two of the foundational assumptions of the Laue group: (1) the lattice is a simple cube and (2) the pattern is formed by the characteristic radiation emitted by atoms when struck by the x-rays. Both of these premises would be tempered by later work, but at the time of the experiment, they seemed like plausible ideas. The perfect symmetry of the resulting spot pattern seemed to justify the validity of these choices.

Figure 2.27.7 demonstrates how changes in the planar orientation of the crystal change the pattern of spots. While Figure 2.26.5 was set so that the x-rays struck the 100 face of the crystal perpendicularly, Figure 2.27.7 was formed by x-rays impinging vertically on the 111 plane. My Figures 2.30a and 2.30b demonstrate the differences between these two planes. The black arrows demonstrate the direction of the incident radiation to show how the x-rays would strike the crystal differently with adjustments to the plane of exposure.



a.



b.

Figures 2.30a and 2.30b: X-rays (black arrows) striking the 100 plane (a) and 111 plane (b).

Figure 2.29.12 functions in the same way as Figure 2.27.7, except that image is of plane 110. In other words, it is a top down shot of the crystal. The "two fold" symmetry is what one would expect based on the known information about the crystal structure. Figure 2.30c demonstrates how the-x-rays would strike the crystal in this case.

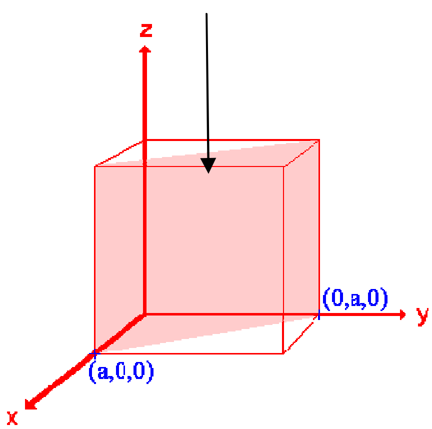


Figure 2.30c: X-rays (black arrow) striking the 110 plane.

Figures 2.27.8 and 2.29.11 demonstrate the effect of changing the angle of the incident radiation by three degrees. Figure 2.27.8 is taken from the same planar orientation as Figure 2.27.7, but the three-degree change results in less than perfect symmetry. Figure 2.29.11 shows the same three-degree shift performed on the 100 plane, i.e., the image in Figure 2.26.5. The images of these slight shifts were taken "to show that an accurate orientation of the crystal is necessary in order to obtain identical pictures on repetition of an experiment" (103). The subsequent unsymmetrical patterns demonstrate the importance of precise orientation.

Figure 2.28.9 and Figure 2.28.10 are only mentioned in a footnote. Together, they demonstrate how the distance of the photo plate from the incident x-ray beam affects the spot pattern. Both images show a formation similar to the pattern in Figure 2.26.5. However, the spots in 2.28.9 are closer together, and in 2.28.10, they are spread further apart. Though all three images are of the same crystal face, the plate for Figure 2.26.5 was 35 mm from the x-ray source, and the plates for Figures 2.28.9 and 2.28.10 plates were set at 10 mm and 70 mm respectively.

The relative neglect of these two figures in historical studies is unfortunate because they demonstrate an important point that Laue overlooked but that Bragg did not. Specifically, these photograms demonstrate that the shape of the spots becomes more elliptical the further the photo plate is from the crystal. In this first paper, Laue is only concerned with the size of the spots, their arrangement, and their intensity (i.e., their relative darkness); thus, he missed this important point.

Kenneth Burke's concept of the terministic screen might offer some insight into why Laue overlooked the differences in the spot shape. Laue was working with an analogy to optical interference in which he extended the concept of a one-dimensional optical diffraction grating to the three dimensional crystal lattice. In such an analogy, shape is not a term of interest, though size and spatial position are. In other words, Laue did not see the shape differences because his attention was directed by a different set of visual terms.

Though much is made of the photogram in Figure 2.26.5, the ten less glamorous photographs are important and understudied aspects of this case. These images provided a visual narrative of the experiment that parallels the verbal narrative. They become significant comparative and causal arguments that visualize the relationships between distance, angle, planar orientation, and lattice structure, and thus they provide important clues about the underlying process. The regularity of the changes provided stability to the case, demonstrated that the process is a reliable method for visualizing crystal structure, and justified that x-rays are waves.

The photographs also provide visual verification of a number of premises essential to Laue's reading of the iconic image. Specifically, they demonstrate that the apparatus is creating images that depend on the structure of the crystal. Ultimately, this portion of Laue's argument would be extremely successful. No one doubted that the group photographically captured traces of x-rays interacting with the crystal. Nor did they doubt that the photos demonstrated that x-rays are waves. However, colleagues did doubt the particulars of his explanation. Specifically, W. L. Bragg and others challenged the mathematically derived claim that the spots are formed by five discrete wavelengths. Laue's rationale for this claim comes from a fascinating mathematical and visual argument that is also an understudied portion of this case.

In Laue's sole-authored "Quantitative Proof of X-Ray Interference," Laue attempts to reconstruct mathematically the image in Figure 2.26.5, and the results of this work are shown in Figure 2.26.6.

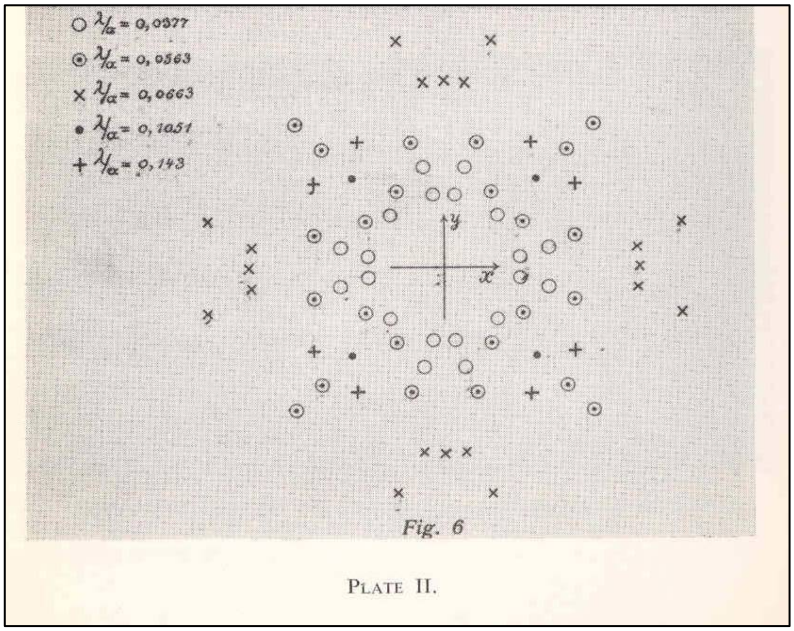


Figure 2.26.6

This image is not (as some have suggested) simply a key to Figure 2.26.5 (the best and most symmetrical x-ray image). It is, instead, a visualization of Laue's equations that corresponds to the photograph. Essentially, Laue "rebuilds" the photograph of Figure 2.26.5 in diagrammatic form through deductive reasoning based on some known premises and his equations. As far as I know, this aspect of the diagram is largely overlooked, though Laue clearly explains how readers should read the diagram:

> To make the comparison with past experience easier, in Figure 6, Plate II [Figure 2.26.6], the photogram 5 on plate II [Figure 2.26.5] was

> reconstructed directly from the numbers given here, h_1, h_2, h_3, according to the equations (13b) and assuming z = 3.56 centimeters. By measuring with the protractor, the reader can check on the agreement of the figure with the photogram. Omitted in the drawing are only the two points whose absence was not explained in the photogram and some points in the outer parts of the photogram, which so far have not been calculated all the way through/ (998)

The visualization's mathematically generated spots coincide with spots in Figure 2.26.5; hence, Laue's explanation is validated. The generation of the same pattern both by the apparatus and by his equations was the critical point for Laue. The similarity of effect indicates a similarity of cause; i.e., the phenomena producing the photographic spots obey the same principles as the mathematically derived spots. He concludes the second paper with the following declaration: "Figure 6 with the Photogram 5 constitutes extensive confirmation of the interference theory on these phenomena" (999). By recreating the image with his equations, Laue argued that his equations—and hence his interpretations of the photographs—were correct.

Interesting from a rhetorical perspective is Laue's invitation to the reader to verify his work by measuring the diagram and the photogram. The reader becomes complicit in affirming the argument. Laue's decision to code the constructed spots with symbols identifying five λ/a values (i.e., their relative wavelengths) is interesting from both semiotic and rhetorical perspectives. Semiotically, it transforms the spot pattern through what Kress and van Leeuwen would call a "classificational process." The spots, which are visually homogenous (or nearly homogenous) in the photogram, have been marked as distinct members of a genus with five species of visual entities—species constructed by Laue's mathematical analysis. In rhetorical terms, Laue is engaged in visual rhetorical *taxis*—the selective division and arrangement of the subject. Though this division of the spots into five categories was compelling to Laue and compelling within the article itself, it was not compelling to others. As later sections show, parts of his argument ultimately fail because of several faulty assumptions, including the assumption that ZnS has a simple cubic lattice. Before examining the reception of Laue's argument, I first examine how Laue attempted to address some perceived incompatibilities in his images and arguments both before and after the debut of his photograms and diagram.

Preemptive and Reactionary Refutation: Constructing and Resolving Incompatibilities

Recall that Laue argues two claims: (1) x-rays are wave phenomena, and (2) the spots are formed by five specific wavelengths. Laue deploys arguments from incompatibility to support both of these claims.

When arguing for the claim that x-rays are waves, Laue uses incompatibility arguments to dismiss the only available alternative hypothesis—the hypothesis that x-rays are corpuscles. Laue refutes this alternative by identifying contradictions that a corpuscular phenomenon would entail:

> Let us imagine that the atoms of the crystal in the cast of Fig. 5, Plate II [Figure 2.25.5], are exited by means of a corpuscular radiation. […] Then only those atoms which were struck by the same corpuscle

> would be brought into coherent oscillations, which would only apply to the series of atoms parallel to the z-direction. Atoms at a definite distance from each other in the x- or y- direction would be excited by different corpuscles, a definite phase difference between their oscillations could therefore not arise. Because of this, in the intensity expression (6) only one sine-quotient would remain; we would have only one condition for an intensity maximum, which as symmetry considerations also indicate, would be fulfilled along circles around the point of impact of the primary ray. The broken character of these circles which is actually observed would be completely incomprehensible. (96)

In other words, a corpuscular cause is incompatible with the photographed pattern. A corpuscular phenomenon would create unbroken circles, rather than the rings of dots that appear in Figure 2.26.5. Figure 2.26.6a demonstrates how the spots in Figures 2.26.5 and 2.26.6 can be viewed as arranged in invisible circles.

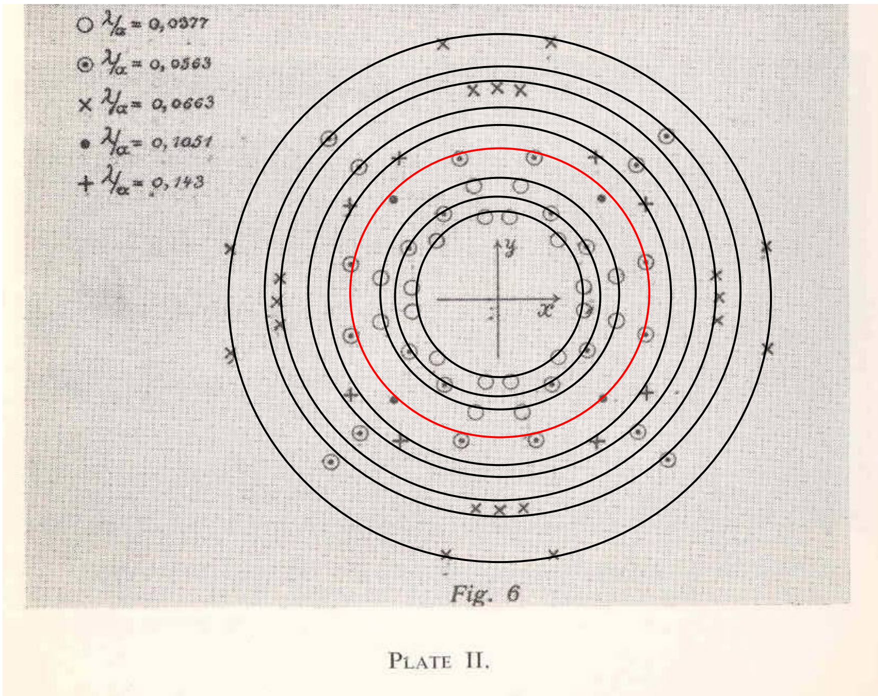


Figure 2.26.6a: Laue's Figure 6, my circles.

Since a corpuscular cause is incompatible with the images as they appeared, the cause must be wave-based. This conclusion was ultimately convincing. Less convincing were Laue's claims about the number and source of the x-rays producing the spots.

Laue believed that when the atoms were struck by the incident beam, they oscillated and released secondary x-rays of specific wavelengths unique to the resonating atomic element. Technically, such a process does occur; however, this was not the process forming the first x-ray diffraction spots. The counter claim eventually forwarded by Bragg (but apparently mentioned by others) was that the spacing of the lattice structure determined the wavelengths of the radiation that ultimately marked the photographic plates. Laue addresses this claim in a note added to his second paper when the paper was reprinted in the 1913 *Annalen der Physik*. He does not respond directly to Bragg, though Laue does note that the counter claim has been expressed “in the course of normal conversation” (1000). In the note, Laue uses tactics similar to his refutation of the corpuscular explanation of x-rays; he invokes an incompatibility argument that depends on what is *not* in the visuals:

> The very strange and as yet unexplained fact, that the spectrally high-grade nonhomogeneous pulses of incident X-ray radiation in the crystal caused oscillations of very precise wavelengths (in my opinion, that is the only way we can explain the accuracy of the observed interference points), in the course of normal conversation occasionally was explained by saying that the selection of these sinusoidal oscillations, from all of those that are present in the pulses, is presumably a function of the spatial grid. In contrast, we want to show that the possible interference maxima, according to [equations] (12) (in the presence of the required oscillations), everywhere lie so densely *that the photographic plate would have to be blackened completely,* unless the properties of the grid elements, stated in the function ψ (α, β), would eliminate a certain number of finite numbers from the infinitely many wavelengths of the original radiation. (1000, my emphasis)

According to Laue, the actual photograms were incompatible with the alternative lattice-filtering hypothesis because that hypothesis would entail either a completely blackened photograph or a spatial lattice of infinite filtering capacity. For Laue, this was a reasonable refutation. For others, this refutation was not convincing.

Laue worked with and against visual evidence to fend off counter arguments; however, there were some inconsistencies he was unable to overcome rhetorically. Specifically, his equations predicted some spots in his photographs that do not appear. He acknowledged these inconsistencies between his images and his math, and he attributed them to the uncertainty involved with the development of a new theory. Laue's peers did not accept this rhetorical mitigation of contradictory evidence, and some of his rivals pointed to these inconsistencies as support for alternative explanations.

Reception

As noted previously, the diffraction photographs intrigued Laue's German colleagues, though some—including Röntgen—were unconvinced that they proved a diffraction/interference phenomenon. British reactions were also a mix of admiration and skepticism. Chemist Oliver Lodge, who had been in Munich around the time of the Laue experiment, commented on the work in a lecture to the Chemical Society:

> This [photographing of atomic positions], if it be a fact, will have to be recognised [sic] as a striking and admirable case of scientific prediction, the various crystalline structures and accuracy of characteristic facets having been indicated by theory long before there was any hope of actually seeing them; so that once more—always assuming that the heralded discovery is substantiated—the theoretical abstraction will have become concrete and visible. (Lodge qtd. in Tutton 309)

After the photographs were published, other physicists in Britain were impressed by the images but uncertain about Laue's explanation. For example, in an October 1912 letter to British physicist Ernest Rutherford, Charles Barkla wrote, "I have had a copy of Laue's paper for some little time and certainly am skeptical of any interference interpretation of the results. A number of features do not point that way" (qtd. in Foreman 55). However, Barkla's skepticism was directed toward the specific results and not the general model of x-rays as waves: "This in no way affects my absolute confidence in the truth of the wave theory of x rays" (qtd. in Foreman 55). In a letter to the journal *Nature* in 1912, the elder Bragg noted the significance of Laue's image, but he was also unconvinced by the explanation: "Until further experimental results are available, it is difficult to distinguish between various explanations which suggest themselves. It is clear, however, that the diagram is an illustration of the arrangement of the atoms in the crystal" ("X-rays and Crystals" 219).[21] H. G. J. Moseley, another British physicist, was entirely unconvinced by the explanation. As historian J. L. Heilbron notes, "When [Moseley] looked more closely at the German work he concluded that Laue and company "entirely failed to understand what it meant and gave an explanation that was entirely wrong"" (Heilbron 70).

One can read this reception evidence as an indication that Laue's work succeeded as an argument for existence, but failed with respect to its arguments of definition and cause. For these readers, the photographs indicated that something regular and predictable happens to x-rays as they interact with the crystals; however, the interpretation of the photographs as the diffraction of x-rays of specific wavelengths was not convincing.

[21] In this letter, Bragg uses the terms photograph and diagram interchangeably.

This was the reaction of both Bragg's to the Laue project, and the younger Bragg's first paper was a detailed replacement of the Laue explanation. His claims rest on a number of visual objects, including a diagram that reinterprets the most essential Laue photogram.

Summary of W. L. Bragg's Argument

As noted previously, both Braggs worked on replicating Laue's experiment and reinterpreting the results, but it was the younger Bragg who first published a revision of Laue's interpretations. A sketch of W. L. Bragg's first argument is as follows.

Claim: X-rays interfere with each other after passing through a crystal; however, it is mathematically and conceptually convenient to describe this process in terms of *reflection.*

Evidence:

(1) Bragg's equation (later referred to as Bragg's law) can account for all the spots on the photograph. This equation is based on optical reflection.

(2) The spots become more elliptical the further the plate is from the crystal, an indication of reflectance phenomena.

(3) Patterns obey the equations of the reflection geometry when offset (e.g., a 3 degree offset makes a 6 degree change to the pattern)

Supporting assumptions: (1) ZnS forms a face-centered cubic lattice.

(2) The x-ray source emits a range of heterogeneous wavelengths; specific wavelengths are "selected" by the distances between crystal planes.

Bragg's rhetorical task was to demonstrate that his basis for interpreting the photographs was superior to Laue's, and he relied heavily on visual means to accomplish this task.

According to historical and autobiographical texts, Bragg was inspired by specific features of the x-ray photograms. In an autobiographical account, Bragg explains the following diagram (Figure 2.31), which he reproduced from his first article, to explain how he came to rethink Laue's work in terms of reflection.

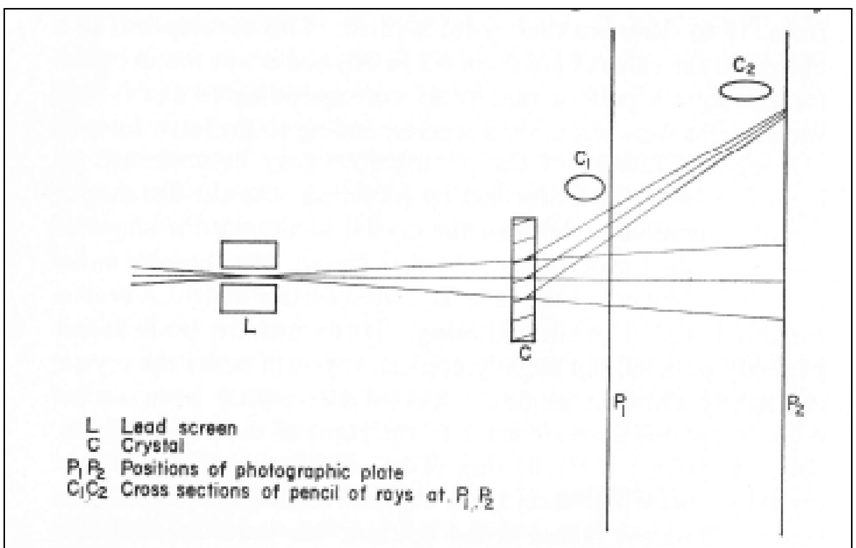


FIG. 2.

Figure 2.31: Figure 2 from W.L. Bragg 1913. Reproduced in *X-ray and Neutron Diffraction*. G. E. Bacon. 1966: 124.

Bragg's description of this graphic is as follows:

> When the plate was placed at P_1 near the crystal the spots were almost circular like C_1, but when placed farther back at P_2 they became very elliptical (C_2). Now Laue had ascribed his pattern to the diffraction of certain specific wave-lengths in the X-ray beam by the regular pattern of the crystal. Given a fixed wave-length, optical theory tells us that the diffraction must take place at a definite angle, and this means that the diffracted rays drawn in the picture should all have been parallel. I had heard J. J. Thomson lecture about Stokes' theory of the X-rays as very short pulses of electromagnetic radiation. I worked out that such pulses of no definite wave-length should not be diffracted only in certain directions, but should be reflected at any angles of incidence by the sheets of atoms in the crystal as if these sheets were mirrors. (qtd. in Ewald 60)

Though the diagram is part of the text of Bragg's article, it is only a supporting argument. Bragg is almost casual in his introduction of the diagram: "A curious feature of the photographs may be explained by regarding the spots as formed by reflection" (123). He then describes why the differences in shape support reflection. However, by the time Bragg gets to this "curious feature" in the text, he had already developed a strong case for treating crystals as sets of mirroring planes and treating the spots as traces of reflection. The analogy with optical reflection was a major component of his case.

<u>Resolving Visual Inconsistencies: "Reflection" as Analogy and Cause</u>

In the text, Bragg first explains and then critiques Laue's interpretation. Bragg bases his critiques on incompatibilities between the predictions of spots made by Laue's math and the real spots in a photogram:

> However, [Laue's] explanation seems unsatisfactory. Several sets of numbers h_1 h_2 h_3 [the integers from the Laue equations] can be found giving values of λ/a approximating very closely to the five values above [that Laue had calculated] and yet no spot in the figure corresponds to these numbers. (Bragg 111)

Laue's math conflicts with the reality of the photographic evidence because the math should produce spots that are not there. The inconsistency reduces the credibility of Laue's account of the spots' formation. Bragg offers an alternative explanation that is a more complete account:

> I think it is possible to explain the formation of the interference pattern without assuming that the incident radiation consists of merely a small number of wavelengths. The explanation that I propose, on the contrary assumes the existence of a continuous spectrum over a wide range in the incident radiation, and the action of the crystal as a diffraction grating will be considered from a different point of view, which leads to some simplification. (Bragg 111)

Bragg's "different point of view" is to consider the diffraction photographs in terms of *reflection* from planes of atoms within the crystal:

> The atoms composing a crystal may be arranged in a great many ways in systems of parallel planes….I propose to regard each interference maximum as due to the *reflection* of the pulses in the incident rays in one of these systems. (W.L. Bragg 112; italics his, underline mine)

Bragg's selection of reflection as an operational analogy is an interesting choice in the light of what rhetorical theorists note about analogical reasoning in controversies. As Perelman observes in the *Realm of Rhetoric*, "In criticizing a thesis illustrated by analogy, we must either adapt the analogy so that it corresponds better to our own conceptions, or replace it by another, thought to be more adequate. The two procedures are found in controversies" (119). In this case, Bragg adapts the analogy of x-rays and traditional optics to argue for a different explanation of the formative waves. Dissatisfied with Laue's optical analogy that the crystal acts like the three-dimensional counterpart of a two-dimensional optical diffraction grating, Bragg describes the photographic output in terms of a different optical phenomenon—reflection. Like Laue, Bragg supports his selection with references to older examples from traditional light optics:

> Regard the incident light as being composed of a number of independent pulses, much as Schuster does in his treatment of the action of an ordinary line grating. If it falls on a number of particles scattered over a plane which are capable of acting as centers of disturbance when struck by the incident pulse, the secondary waves from them will build up a front, exactly as if part of a pulse had been

reflected from the plane, as in Huyghens' construction of a wave front. (112)

It is important to note that Bragg does not disagree with Laue's assessment that the spots represent diffraction phenomena. After all, Bragg titled his article "The *Diffraction* of Short Electromagnetic Waves by a Crystal." However, for Bragg, the process, and hence the math that can explain the process is more *like* reflection. The difference is partially one of the rhetorical arrangement of the crystallographic subject.

Bragg's reinterpretation of the photographs begins by conceptualizing the crystal lattice not as specific cells with atomic corners, but as sheets of atoms formed into planes. This move is another example of visual rhetorical *taxis*, as articulated by Prelli. More specifically, Bragg rhetorically divided and arranged the components of his crystalline subjects in ways that helped explain the interference events; Bragg conceptualizes the crystal as a composite of sheets rather than as composed of atomic points at the corners of individual cubes. As historian Graeme Hunter observes, "Laue viewed the crystal as a three dimensional array of points; Bragg as a one dimensional array of planes" (32). Optical analogs, planar geometry, and trigonometry then led Bragg to an equation that explained the conditions for two parallel waves to be reflected in phase from successive planes. This equation, now known as Bragg's Law, describes the conditions that will result in an amplified wave, i.e., one that would appear as an intensity spot in the diagram. Specifically, crystals will create constructively interfering waves when the source wavelength (λ) is two times the distance (d) between atomic planes, multiplied by the cosine of the incident angle(θ): $\lambda = 2d \cos \theta$. Later, the equations would be conventionalized as $\lambda = 2d \sin \theta$ to reflect preferences in which angle is the angle of interest. Figure 2.32 is a visualization of the equation, though a similar figure did not appear in Bragg's text. While the figure only shows two waves interacting, the relationship applies for an infinite number of equally spaced planes. As historian and scientist Robertson notes, Bragg's Law "…is equivalent to the Laue conditions for diffraction but expressed in a form much easier to visualize" (636).

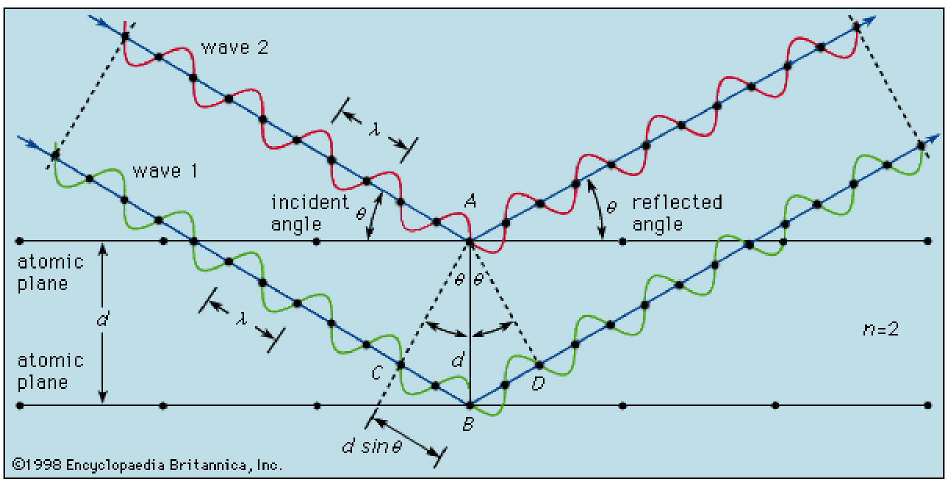


Figure 2.32: Visualization of Bragg's Law. Encyclopedia Britannica.

Historian Graeme Hunter notes that Bragg's law was not new; Schuster developed a similar equation for ordinary line gratings (i.e., those that would be used in slit experiments) three years earlier. That equation— $2e \sin \theta = n\lambda$ —looks quite similar to Bragg's equation; thus, according to Hunter, "The only reason Bragg's name became associated with this relationship was that he showed it could be applied to the diffraction of X-rays by crystals as well as to diffraction of visible light gratings" (36). However, in a review of Hunter's book in *Nature*, Kenneth Holmes criticizes Hunter's dismissal of Bragg's novelty: "Apart from the θ being different, Hunter misses the point first made by von Laue that diffraction from a three-dimensional lattice is subject to constraints not pertinent for a one-dimensional grating. The Bragg law imposes two conditions: specular reflection and the Bragg equation" (1038).[22] Hunter's gaffe and Holmes's response might seem like one historian quibbling over another's understanding of scientific details; however, the exchange offers an interesting comment on the analogical nature of Bragg's argument. That is, the novelty of the idea is not derived from the form of the equation. Rather, the application of the mathematical form to a rhetorically restructured crystal space is the novel development. In other words, it takes both the equation and the analogical transformation of atomic points into mirroring sheets to make sense of the x-ray spots, and the analogy is "built into" the law. In the next section, I show how Bragg develops the reflection analogy with technical visualizations that support and are supported by Bragg's Law.

Tables and Diagrams: Visual Supports for a Productive Analogy

In his first article on x-ray diffraction, Bragg argues that the diffraction of x-rays captured in the Laue photographs can be conveniently described as a reflection phenomenon. To make this case, Bragg had to link the specific spots to specific invisible planes, and he needed to show that this connection explains the photographs more completely than Laue explained them. Bragg would use two kinds of visual

[22] Specular reflection occurs when a wave from a single source is reflected with mirror-like symmetry; e.g., the waves in Figures 2.8 and 2.32.

representation to support his argument: tables and diagrams. These visuals provided a critical bridge between Bragg's analogy (and its consequent mathematical formula) and Laue's original photographs. He used tables to identify planes and to link planes to particular spots. He then recodes a selective reduction of Laue's best photogram with semiotic markers that demonstrate how the various reflections create spots of varying intensity.

Figure 2.33 is the first of four tables included in the text. The first four columns are coordinates for two of the three points that define each invisible reflection plane.[23] Like Laue, Bragg could not determine precise wavelengths or precise inter atomic distances during these early experiments, but he could calculate values that involved both of these unknown quantities based on all of the other known variables. Thus, λ/a is the fifth column. The sixth column indicates the intensity (or invisibility) of the spot formed by a beam reflected by the plane. The last three columns (h_1, h_2, and h_3) are the integers from the Laue equations, which can be used to calculate the coordinates of the spots on Laue's best photograph (my Figure 2.26.5); h_3 is always constant because the plate is a fixed distance from the sample. Bragg includes these variables in his table to show the correspondences between his work and Laue's work.

[23] A plane is defined by three points. In a three dimensional space, each point has three coordinates (x, y, z). By taking the origin (0, 0, 0) as one point in each atomic plane, Bragg only needed to define two other points in the table, and each of those points has a zero coordinate. Only four values (p, q, r, and s) need to be in the table because each plane is defined by only four variables across the three points. Thus the xyz coordinates of the three points defining the plane are [0 0 0], [pa 0 qa], [0 ra sa]), where a is the distance between adjacent atoms.

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reflected train expressed for convenience in the form a/λ, and when in the photograph a spot is visible in the position calculated, its intensity is denoted by star according to an arbitrary scale.

* * * + •

When no spot appears in the calculated position, I have put 'invisible' opposite that plane.

TABLE I. PLANES FOR WHICH $p=1$, $r=1$, $l=2a$, $\lambda=4an^2$

p	q	r	s	a/λ	Intensity	h_1	h_2	h_3
1	1	1	3	2·8	*	1	3	1
1	1	1	5	6·8	*	1	5	1
1	1	1	7	12·8	*	1	7	1
1	1	1	9	20·8	Invisible	1	9	1
1	3	1	1	2·8	*	3	1	1
1	3	1	3	4·8	*	3	3	1
1	3	1	5	8·8	*	3	5	1
1	3	1	7	14·8	+	3	7	1
1	3	1	9	22·8	Invisible	3	9	1
1	5	1	1	6·8	*	5	1	1
1	5	1	3	8·8	*	5	3	1
1	5	1	5	12·8	*	5	5	1
1	5	1	7	18·8	Invisible	5	7	1
1	7	1	1	12·8	*	7	1	1
1	7	1	3	14·8	+	7	3	1
1	7	1	5	18·8	Invisible	7	5	1
1	9	1	1	20·8	Invisible	9	1	1

Range of values of a/λ, all possible up to 15.

Figure 2.33: Table 1 from W.L. Bragg 1913. Reproduced in *X-ray and Neutron Diffraction*. G. E. Bacon. 1966: 117.

This tabular display of data may seem mundane; however, these tables are critical to Bragg's argument, yet they get little explicit attention in other studies of this case. As Fahnestock has observed, the verbal and visual parallelism of tabular forms is highly rhetorical:

> A table reduces each individual instance to its minimal identifying feature …, and it reduces the uniqueness of each individual instance to the one feature considered salient for the case at hand…. Such a mode

> of visual parallelism unproblematizes the data and diminishes occasions for refutation. (Fahnestock "Verbal and Visual Parallelism" 139-140).

In other words, a table is more than just a way to display data.

The following passage demonstrates how the tables participate in Bragg's visual and mathematical argumentation:

> Every spot in the [best Laue] photograph is accounted for in the following tables. I think it is evident that the sets of planes which actually reflect spots can be arranged in a very complete series with few or no gaps. Though at first sight it may appear that in the tables the parameters are selected in a somewhat arbitrary way, they are in reality the simplest possible. For instance, in Table III the first values for *p, q, r, s* considered are 1, 1, 3, 5. This is so because 'r + s' must be positive. If r = 1, s must be odd, 1, 1, 3, 1 and 1, 1, 3, 3 would reflect the beam so as to miss the photographic plate. 1, 1, 3, 5 and 1, 1, 3, 7 are considered. 1,1,3,9 has already been considered as 1,1,1,3 [in Table I], and 1, 1, 3, 11 gives a value for the wavelength outside the 'visible' range. (119)

Unlike Laue's explanation, Bragg's tables account for all of the spots in Laue's most important photograph (my Figure 2.26.5), providing a "complete series" supported by mathematical warrants. Bragg also explains why specific planes are in the tables and why some are left out. He follows this explanation with a diagram (Figure 2.34) that makes visual connections between the data in the tables and the photographic evidence.

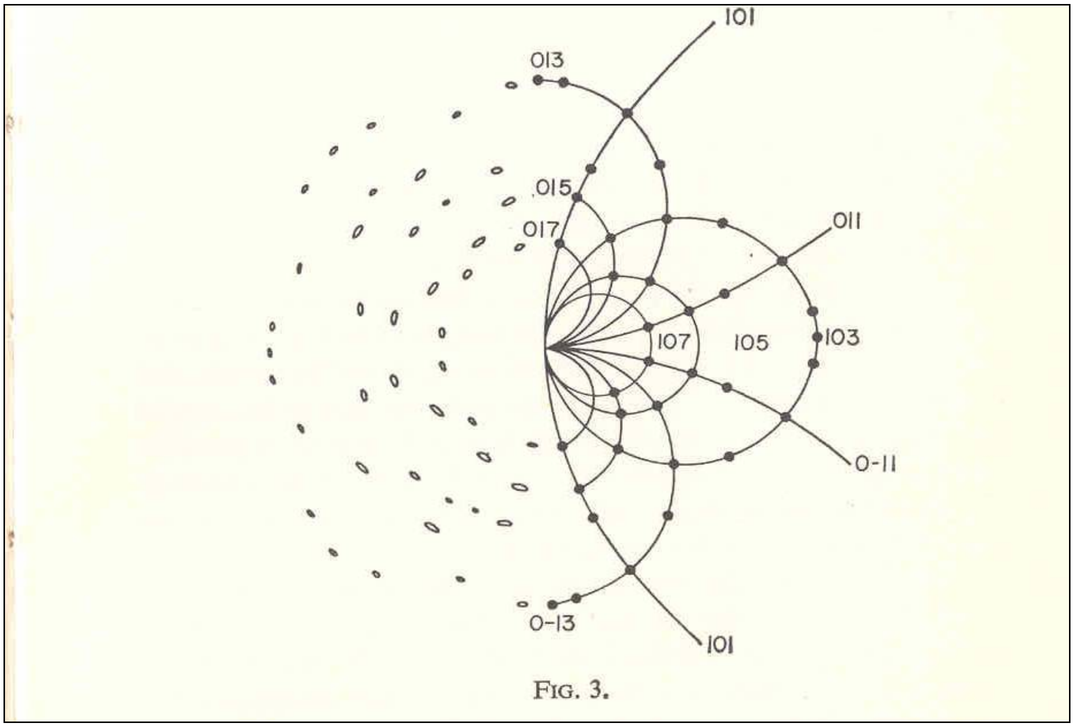


Figure 2.34: Figure 3 from W.L. Bragg 1913. Reproduced in *X-ray and Neutron Diffraction*. G. E. Bacon. 1966: 125. NB: the three-digit labels for the ellipses are not Miller indices. They are specific points in a three-dimensional grid space. Bragg learned the specifics of the Millerian system after the publication of this paper.

The following passage explains how the reader should understand this "key" to the Laue graphic:

> Consider a reflecting plane which passes through the atom at the origin [0, 0, 0] and a neighboring atom, let us suppose the atom whose coordinates are *a, o, a.* As the plane is turned about the line through these two points the reflected beam traces out a circular cone, which has for an axis the line joining the two points and for one of its generators the incident beam. This cone cuts the plate in an ellipse. If

> the atom through which the plane passes is in the *xz* plane as above, the ellipse touches the *y* axis on the photographic plate at the origin. Now take a plane passing through the origin and a point 0, *a, 3a.* The locus of the reflected spot as it turns is again an ellipse, which touches the *x* axis. The intersections of the two ellipses will give the position of a spot reflected by a plane passing through all three points, the origin, the point *a, 0, a,* and the point 0, a, *3a.*

In simple terms, Bragg uses propositions based on geometric principles to explain the appearance of specific spots in the photograph.

This image is rhetorically interesting for several reasons. First, though Bragg had replicated the Laue experiment, he uses Laue's photograph as his example. Laue's photograph was the authoritative image of x-ray interference, and any reconfiguration of the Laue theory would need to explain this photograph. Second, Bragg demonstrates his method selectively; he codes a drawing of only some of the Laue spots, and he only indexes one table's worth of planar data. This illustration was sufficient to demonstrate the sufficiency of Bragg's explanation. Third, Bragg only inscribes half the image; the reader is able to see the relationship between the given information of the photogram and Bragg's new geometric interpretation. Visuals in later documents, such as Figure 2.35, do not have this hybridity; i.e., they are not naturalized drawings of the spots combined with diagrammatic indexing. Rather, the later representations become purely diagrammatic. Taken together, these three rhetorical curiosities demonstrate that Bragg's work is an important intermediate step between Laue's novel experiment and the development of a new field.

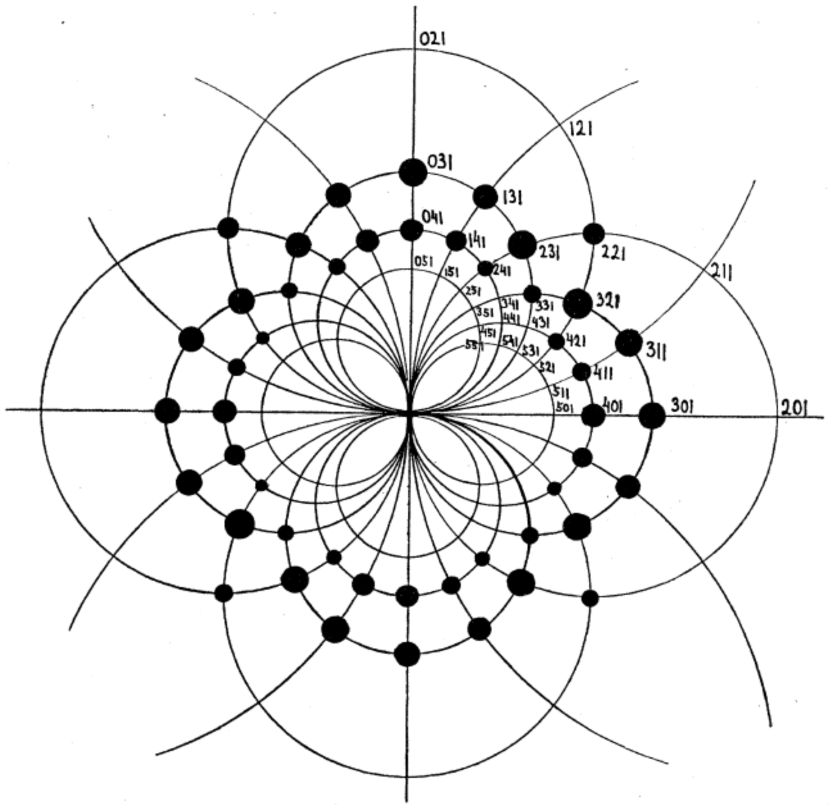


Fig. 3.—Potassium chloride.

Figure 2.35: Diagrammatic rendering of the x-ray diffraction pattern of KCl. NB: In this diagram, the three-digit numbers are Miller indices. Source: Bragg, William Lawrence. "The Structure of Some Crystals as Indicated by Their Diffraction of X-rays." ***Philosophical Transactions of the Royal Society.*** **89 (1913): 253.**

Instrumental Extensions of the "Reflection" Analogy

After the reading and publication of W. L. Bragg's first paper on x-ray crystallography, both Braggs enthusiastically engaged in extending the new science. The younger Bragg quickly published another paper on the simple structures of NaCl, KCl, KBr, and KI crystals, and both Braggs authored a paper on the more complex structure of the diamond. Both Braggs also jointly authored a paper on the development of a new instrument whose conception emerged directly from the younger Bragg's pioneering conceptualization of the simplifying reflection analogy. As the elder Bragg noted in a 1914 lecture to the Royal Institution that was republished in *Science*, "This simpler conception led at once to a simpler procedure. It led to the construction of the X-ray spectrometer" (798). In this section, I briefly discuss the x-ray spectrometer to demonstrate the feedback loop that can develop through the application of rhetorical analogies—instruments produce images that become structured by analogies; analogies lead to interpretations that structure new instruments.

The x-ray spectrometer is a hybrid of the Munich group's x-ray imaging apparatus and an optical spectrometer. As Bragg explained in his 1914 lecture, the x-ray spectrometer "…resembles an ordinary spectrometer in general form, except that the grating or prism is replaced by a crystal and the telescope by an ionization chamber and an electroscope" (198-199). An ionization chamber is a gas-filled vessel whose gaseous atoms produce measurable ions when excited by x-rays; an electroscope measures electric charge in a body, and it could measure the current produced by the ionized gas in the ionization chamber. The combination of these devices with a turnable sample-mount table allowed the Braggs to quantify both specific x-ray wavelengths and interatomic distances for specific crystals. The elder Bragg explained this process in his lecture to the Royal Institution:

> In use a fine pencil of X-rays is directed upon the crystal, which is steadily turned until a reflection leaps out; and the angle of reflection is then measured. If we use different crystals or different faces of the

> same crystal, but keep the rays the same, we can compare the geometrical spacings of the various sets of planes. If we use the same crystal always, but vary the source of X-ray, we can analyze the latter, measuring the relative wave-lengths of the various constituents of the radiation. (W.H. Bragg 798)

The "leaps" to which Bragg refers are spikes in ionization measured by the electroscope. In the next passage, Bragg explains the possibilities of the new device:

> We have thus acquired a double power: (1) We can compare the intervals of spacing of the atoms of a crystal or of different crystals, along various directions within the crystal; in this way we can arrive at the structure of the crystal. (2) We can analyze the radiation of an x-ray bulb; in fact we are in the same position as we should have been in respect to light if our only means of analyzing light had been colored glasses, and we had been presented with a spectrometer or some other means of measuring wavelength exactly. (798)

The elder Bragg supports his explanation of x-ray resolution with a fitting analogy to traditional optics. Even more interesting from a rhetoric-of-science perspective is the embodiment of the reflection analogy in the apparatus itself. Figure 2.36 is a schematic diagram of the x-ray spectrometer. As explained in the original caption text, O is the x-ray bulb slit, P is axis of the sample stage and, PR and PR' are crystal positions, and Q is the opening of the ionization chamber. The geometry of the apparatus clearly demonstrates reflection. What I find interesting is the presence of the "extra lead screen," which gets no mention in the article text. This screen is clearly used to preserve the purity of the reflection phenomenon; without the screen, the chamber would be affected by stray rays from the x-ray source. Thus, the apparatus has been designed to fit the reflection analogy; it only collected *reflected* rays.

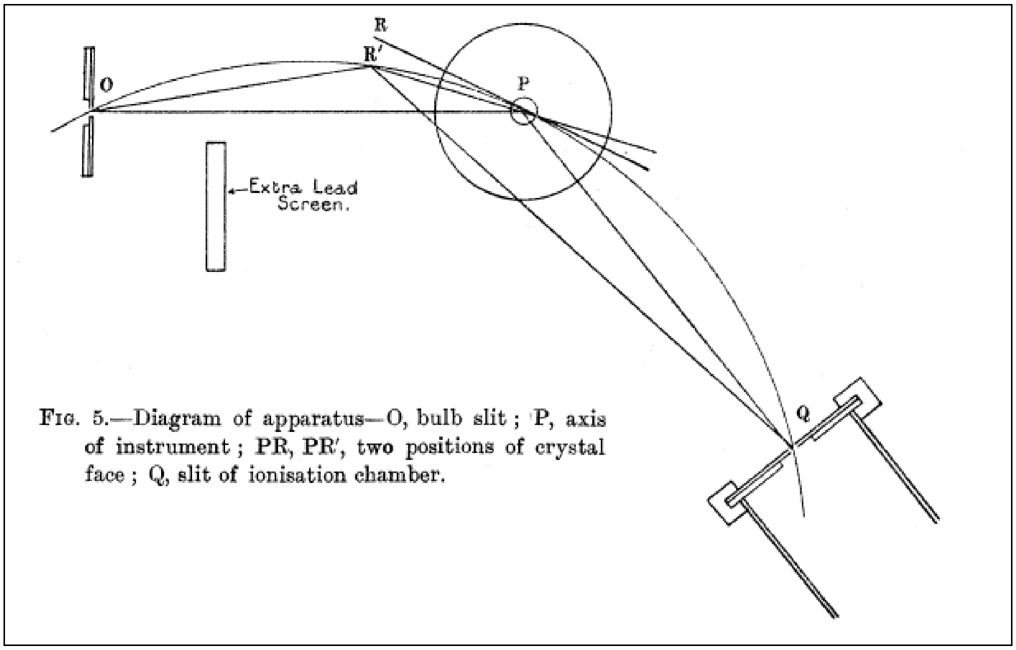


FIG. 5.—Diagram of apparatus—O, bulb slit; P, axis of instrument; PR, PR′, two positions of crystal face; Q, slit of ionisation chamber.

Figure 2.36: Diagram of the x-ray spectrometer. Source: Bragg, William Henry and William Lawrence Bragg. "The Reflection of X-Rays by Crystals." ***Philosophical Transactions of the Royal Society.*** **88 (1913): 433.**

<u>*Reception, Credit, and Scientific Ethos in the Early Twentieth Century*</u>

Like Laue, William Henry and William Lawrence Bragg won the Nobel Prize in Physics (1915) for their work with x-rays. They are the only father and son to win the award for work on the same project. More important for the history of science, their work on x-ray crystallography would revolutionize structural chemistry and lead to some of the most important discoveries in twentieth century biochemistry, such as the structural characterizations of penicillin, DNA, hemoglobin, and insulin. Each of these projects was related technically and personally to the Braggs. W.L. Bragg would become a major figure in the administration of British science, but in the early days of crystallography and until W. H. Bragg's death, the younger Bragg was over-shadowed by his father. Max Perutz explains how the scientific public attributed the credit for the early work:

> It seems hardly believable that the scientific public tended to attribute most of the credit for these discoveries to the father, sometimes with the undertone that the son had cashed in on the father's success. The son must have suffered a great deal from these thoughtless and lazy judgments. Lazy because people could not be bothered to read the literature. (Perutz 187)

Perutz's comment is interesting because of its implications for understanding the rhetoric of science in the early twentieth century. Were people not bothering to read the literature? Or did other rhetorical factors influence perceptions of which Bragg was more responsible for these monumental achievements? The following source-less quotation by W.L. Bragg (reproduced in Perutz's account) hints at some possible complicating factors:

> Inevitably the results with the spectrometer, especially the solution of the diamond structure, were far more striking and far easier to follow than my elaborate analysis of Laue photographs, and it was my father who announced the new results at the Solvay Conference, lectures [sic] up and down the country and in America, while I remained at home. (Bragg qtd. in Perutz 187).

There is not enough evidence to make definitive claims about which specific factors influenced how people received these arguments; however, "laziness" is not the only explanation for unbalanced assessments of each Bragg's contribution. As an established researcher, the elder Bragg possessed greater scientific gravitas. Moreover, as the younger Bragg's comment indicates, the elder Bragg was a more prominent public figure both at important conferences and in scientific circles at large. Finally, the joint work was, according to W.L. Bragg, more "striking" and easier to understand. A scientist could have reasoned that the clearer and seemingly more significant work was performed by both Braggs; it would have been reasonable to assume that the elder's influence made this work better. In other words, the style and significance of specific texts guided public perception. The early reception history of this case indicates that the circulation and perception of scientific texts are complicated and influenced by factors external to the journal publication itself.

Conclusion

This chapter has shown how different scientists used the same visual evidence and different interpretive strategies to make different claims about invisible phenomena. Laue and the Braggs used photographs and diagrams to invoke and support analogical, causal, and incompatibility arguments; these arguments helped define the nature and behavior of x-rays. Their scientific and rhetorical work led to new ways of determining the physical configuration of invisible atoms.

In the next chapter, I show how geophysicists in the middle of the twentieth century modified the visualized output of instruments to make visual claims about invisible geologic processes. Like Max von Laue and the Braggs, these scientists were both constrained and inspired by the visual artifacts derived from invisible forces. They rhetorically reconfigured these artifacts to restructure the history of the Earth. In this next case, reading practices and secondary communication channels, such as conferences, also seem to have influenced the recognition, acceptance, and neglect of important data and developments.

Chapter 3: Showing Motion under the Ocean—Visualizing Claims about Invisible Geologic Processes

And since all laboratory instruments of measurement and observation are devices invented by the symbol-using animal, they too necessarily give interpretations in terms of either continuity or discontinuity.

—Kenneth Burke, *Language as Symbolic Action*

The idea that the seven continents once formed a single land mass is now standard fare even in the science textbooks read by elementary school students. But the acceptance of what we now call plate tectonic theory is a relatively recent development in the history of science. As late as the 1960s, a majority of geophysicists believed in a stabilist position—the continents are where they always have been. Though Thomas Kuhn did not write about this case in *The Structure of Scientific Revolutions*, the intellectual sea change from the stabilist position to a mobilist position—the continents have moved away from each other over time—has all the markings of a paradigm shift. Anomalies that were incommensurable with a stabilist system accumulated, and the strength of this evidence led some scientists to develop revolutionary mobilist hypotheses. New research programs and persuasive activities produced new evidence and new arguments that eventually made it impossible to accept the old stabilist orthodoxy. The mobilist regime became the dominant paradigm for a new period of normal science.

While this case conforms to Kuhn's description of paradigm shifts, the means that persuaded people need to be explained. Though there have been numerous sociological, philosophical, and historical treatments of this case, no thorough rhetorical studies exist. To begin to fill this gap, I analyze the role of visual rhetoric in developing and evangelizing hypotheses supporting continental mobilism. Specifically, I examine how visualized data produced by magnetism detection instruments were adapted to produce world-moving arguments.

Background: 1915-1945

The continental drift hypothesis—an early version of continental mobilism—was proposed by Alfred Wegener in 1915. Though Wegener was not the first person to notice the seeming correspondence between distant continental coastlines (e.g., South America's eastern and Africa's western coast), he was original in overlaying additional evidence—such as the locations of mountain ranges and geological and paleological correspondences—on the reconstructed Pangaea/Gondwanaland image (Giere 281). Figure 3.1 is one of the images from the 1922 German edition of Wegener's *The Origin of Continents and Oceans*. The text was translated into English in 1924.

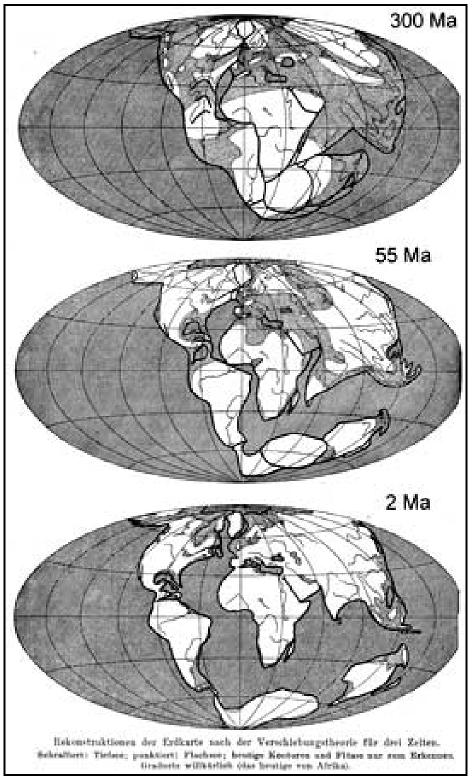
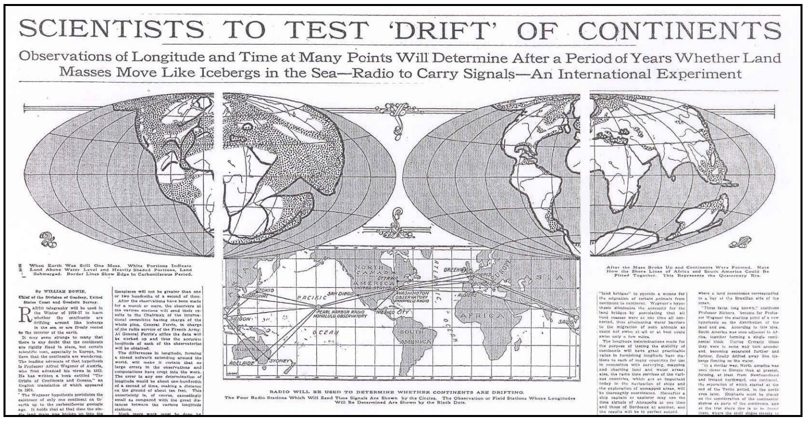


Figure 3.1: Wegener, *The Origin of Continents and Oceans*, 1922.

Though we accept this geologic story today, the “puzzle piece” image sequence was not a satisfying visual narrative for most geologists in the early twentieth century. Scientists rejected mobilist hypotheses because Wegener and later proponents of mobilism could not sufficiently explain the mechanism of motion. If the continents had moved, what moved them? Though centrifugal force, planetary expansion, and planetary contraction were all possibilities, the available evidence did not support the hypothetical physics of these options. Wegener’s belief that the less dense continents “sailed” on top of heavier material did not make sense in light of geologic formations; expansion and contraction hypotheses based on conjectures about the warming or cooling of the Earth’s core were refuted by mathematical and physical arguments (Oreskes *Rejection* 21-53). Moreover, the best contemporary evidence supporting continental drift was refutable. Fossil evidence could be explained by land bridge/isthmus hypotheses, rafting hypotheses, and other "reasonable" accounts of cross-ocean migration (Le Grand 56). Logically, prehistoric plants and animals did not require continental drift to leave similar remains in distant parts of the world. The cartographic evidence was also refutable. Continents could be "reasonably" rearranged according to coastal correspondences that were impossible even under the drift model (Giere 285). Furthermore, Wegener's maps could be challenged as inaccurate representations of the available data. H. E. Le Grand discusses Wegener's "questionable" presentations of geologic and cartographic evidence:

> Wegener got his *fit*, as represented in his famous maps, by massaging the continents. He had distorted the shapes and unfolded the mountains in an arbitrary fashion. Wegener was able to match some patterns of mountains and strata, but with bent and distorted continents, and though he had produced some evidence for matching, there were other features he did not mention which did not seem to have analogues on the opposite side of the Atlantic. Further, some of his data were erroneous or taken out of context or misinterpreted. (56)

Thus, there were numerous grounds for dismissing Wegener's maps as fanciful claims. Finally, elaborate research projects designed to verify drift did not produce convincing evidence. Figure 3.2 is a 1925 *New York Times* article announcing an international collaboration that used a network of radio beacons to determine if distances between "fixed" locations were changing over time.


SCIENTISTS TO TEST 'DRIFT' OF CONTINENTS

Observations of Longitude and Time at Many Points Will Determine After a Period of Years Whether Land Masses Move Like Icebergs in the Sea—Radio to Carry Signals—An International Experiment

Figure 3.2 William Bowie, *New York Times*, 1925.

Ultimately the research did not support drift ("Study Fails" N9). The failure of this experiment can be partially attributed to margins of error and measurement. The project's margins of error were in tens of feet; later work supporting continental mobilism would show that the actual movement is in centimeters per year (Oreskes *Rejection* 236). Regardless of the project's retrospectively identified inadequacies, its failure provided another reason to reject the drift hypothesis.

Despite some support for drift in Europe and discussions of the drift hypothesis in the popular press, the idea was destined to (temporarily) languish. Wegener, the greatest early champion of the drift hypothesis, died in 1930 while on an ill-fated expedition to Greenland. More than thirty years would pass before a revised mobilist hypothesis would become widely accepted in the scientific community.

It is important to note that Wegener's hypothesis did not disappear completely from disciplinary discussions between the time of his death and the development of plate tectonic theory. During the 1930s, 1940s, and 1950s, some scientists did accept versions of the mobilist perspective. These researchers sought new evidence and refined their arguments, as did their stabilist counterparts. For example, "heated debate" at a 1950 meeting of the British Association for the Advancement of Science demonstrated a range of new paleological and geological arguments for both sides ("Britons Weigh" 14). But these new arguments did not settle the debate. Paleomagnetism data collected and interpreted in the late 1950s and early 1960s ultimately transformed "drift" into a viable hypothesis that would become the basis for successful geologic theory.

Finding New Evidence, Establishing New Hypotheses: 1945-1963

The triumph of the mobilist paradigm was the result of a series of conceptual and technical developments: the conceptual developments of mantle convection, sea-floor spreading, and magnetic pole shift; the development of instruments to measure the magnetism in rocks remotely; the development of potassium-argon dating systems; and new discoveries about the topography of the ocean floor. In the 1960s, researchers synthesized all of these developments to produce paradigm-shifting arguments. This section identifies key researchers, discoveries, and publications contributing to this episode in the history of geophysical science.

The end of the Second World War marked the beginning of a productive new chapter for geophysical and oceanographic research. Before the war began, military strategists understood that knowledge of marine topography, temperature dynamics, and magnetic and gravitational anomalies was important for a new era of naval warfare relying on technologies like submarines (Le Grand 171-172). Spurred by its cooling post-war relations with the Soviet Union, the United States poured money into geophysical research. Funding for the Woods Hole Oceanographic Institution in Massachusetts, the Scripps Institution of Oceanography in California, and the Lamont Geological Observatory at Columbia University increased exponentially after the war (Menard 37-39). The U.S. also supported research at Cambridge University in Great Britain, another important site for geophysics research in the post-war period. All of these institutions benefited from a windfall of surplus war material, including submarine-detection equipment that could be modified for geologic sensing and naval vessels that could be refitted for research voyages.

Discoveries from these voyages would reveal key pieces of evidence for solving the puzzles of the Earth's history. The Midpac expedition of 1950 revealed that the oceanic crust of the Pacific was young and, contrary to expectations, topographically rugged. The *U.S.S. Pioneer* survey was launched by Scripps in 1955 to collect bathymetric (depth) data for submarine maps of the eastern Pacific; the survey team also collected data revealing unprecedented magnetic anomaly patterns embedded in the sea floor. Research voyages conducted by the Lamont Geological Observatory throughout the 1950s produced accurate maps confirming the existence and extent of the mid-Atlantic ridge. Scripps' Downwind, Dolphin, and Doldrums expeditions of 1957 and 1958 confirmed the presence of mid-ocean rifted ridges in all of the oceans. All of this new evidence helped develop and support new hypotheses.

Another boon of the post-war period was the opportunity for some scientists to return to ideas without military application that had been set aside during the war. One of these "peace-time" concepts was the sea-floor spreading hypothesis developed separately by Harry Hess, a geology professor at Princeton, and Robert Dietz, a researcher with Scripps. Hess built on the work of Arthur Holmes. Holmes, a British geologist who supported Wegener's continental drift hypothesis, was the first to propose that mantle convection might be the mechanism for continental motion (Le Grand 111-116). The mantle convection hypothesis posits that currents of magma beneath the Earth's crust behave like oceanic currents; hot magma cyclically rises toward the underside of the crust, cools, and sinks back toward the Earth's core. While Holmes was the first to conceive of these convective currents, his assumptions about how they might move the continents were incorrect. He assumed that the continents were carried atop these convective currents. Hess, with new knowledge of the mid-ocean ridge system, asserted that the force moving the continents derives from small quantities of magma that well up and harden inside the rifts of oceanic ridges. Convection pushes hot magma to and through the rifts, and the magma is cooled by seawater. When the cooling material becomes solid rock, the sea floor spreads and the continents move with it. As Le Grand explains, "The crucial difference is that Holmes's continents moved through a rigid seafloor; Hess's, with the seafloor" (199). Dietz's contributions included the role of oceanic trenches in the spreading process and the name of the process itself, sea-floor spreading. Though Hess and Dietz were publishing around the same time in the early 1960s, Dietz acknowledged Hess's priority. I mention both here, since the reception evidence I refer to later—quotations from key researchers—cites both Hess and Dietz as originators of the sea-floor spreading hypothesis.

When Hess and Dietz were publishing in 1960 and 1961, sea-floor spreading could not be observed directly, and the validity of the hypothesis was contested.

Support for sea-floor spreading would come in the form of magnetic data that allowed spreading rates to be measured indirectly. Three concepts—remnant magnetism, magnetic pole reversal, and potassium-argon dating—were important in developing this indirect measurement scheme. The remnant magnetism hypothesis posits that the atoms of solids that are formed with intense heat (such as fired clay or cooling magma) align with the Earth's magnetic field; this alignment is preserved after the material cools. The strength and orientation of the magnetic field is recorded in a solid's structure, even if the material is moved or properties of the Earth's field change. These magnetic signatures can be measured with instruments called magnetometers. The magnetic pole reversal hypothesis posits that the Earth's magnetic pole occasionally reverses over the course of geologic time. Potassium-argon dating is an isotopic dating method, like carbon dating. Measuring quantities of isotopic argon (the byproduct of potassium isotopic decay) allows scientists to extrapolate the age of rock samples. Potassium-argon dating is very accurate for samples between 100,000 and ten million years old (Glen 15). This technique allowed researchers in the 1960s to identify the age of terrestrial rock samples with increasing accuracy. When the ages of these samples were compared with magnetism measurements of the same material, a timeline of magnetic reversals emerged. The timeline of magnetic reversals would be synthesized by Fred Vine, Drummond Matthews, and independently by Lawrence Morley into one of the most important geophysical hypotheses of the twentieth century.

When twenty-two-year-old graduate student Fred Vine arrived at Cambridge in 1962, his future collaborator, Drummond Matthews, was on an expedition in the

Indian Ocean surveying the Carlsberg Ridge. Vine's first assignment was to review the existing studies on marine magnetism surveys, a project which led him to computer-based methods for analyzing and modeling magnetic data. Matthews returned from his expedition with striking new data; a profile of the Carlsberg Ridge revealed magnetic anomalies that were not always correlated with topographical features, as was expected. While he was analyzing this data, Vine formulated what would become his greatest intellectual achievement—the Vine-Matthews hypothesis.

As Vine and Matthews note in their first publication, their hypothesis is a corollary of the sea-floor spreading hypothesis. According to the Vine-Matthews hypothesis, the ocean floor spreads as volcanic material wells up and cools within oceanic ridges; this spreading process is recorded in the remnant magnetism of the sea floor. When the new rock hardens, it acquires a magnetic signature aligned with the Earth's magnetic pole. As noted earlier, this pole occasionally reverses over the course of geologic time; after a reversal, newly formed oceanic rock will have a reversed magnetic signature in relation to adjacent rock. According to the Vine-Matthews hypothesis, measurement of the remnant magnetism should reveal distinct, symmetrically arranged stripes of "normal" and "reverse" magnetism that run parallel to the ridge axis. These stripes should correspond to timelines of specific magnetic reversals. The young rock at the center of the ridge should correspond to our current magnetic epoch, and older rock farther from the ridge center should correspond to older magnetic epochs. Thus, magnetic signatures record a slow but consistent motion under the ocean. Fred Vine summarized this process in a 1966 *Science* review article. His description conflates three metaphors—the conveyor belt, the fossil, and the tape recorder:

> The [sea-floor spreading] hypothesis [of Hess] invokes slow convection within the upper mantle by creep processes, drift being initiated above an upwelling, and continental fragments riding passively away from such a rift on a *conveyor belt* of upper-mantle material; movements of the order of a few centimeters per annum are required.
>
> ****
>
> Vine and Matthews have suggested that variations in the intensity and polarity of the Earth's magnetic field may be *"fossilized"* in the oceanic crust, and that this condition in turn should be manifest in the resulting short-wavelength disturbances in "anomalies" in the Earth's magnetic field observed at or above the Earth's surface. *Thus the conveyor belt can also be thought of as a tape recorder.* (Vine "Sea-floor" 1405, emphasis mine)

Vine and Matthews' first research report appeared in *Nature* in 1963, making them the first to publish this hypothesis; however, another scientist developed the same idea at the same time.

Lawrence Morley, a geologist with the Geological Survey of Canada, was not associated with an institute making grand research voyages, but he did read widely in the geophysics literature. Morley's inspiration for his version of the magnetically tracked sea-floor spreading hypothesis came from the magnetic anomaly maps produced by Arthur Raff and Ron Mason, two of the Scripps researchers on the 1955

U.S.S. Pioneer survey of the Pacific. Morley also submitted a description of what historians now call the Vine-Matthews-Morley hypothesis to *Nature* in 1963, but his paper was rejected. His subsequent submission to the more specialized *Journal of Geophysical Research* also met with rejection. The paper was never published by contemporary journals, though the manuscript has been reprinted in historical treatments of this case.

There are numerous possible—and obviously rhetorical—reasons for the success of Vine and Matthews and the failure of Morley in publishing their respective works. Historian Naomi Oreskes notes a few possibilities:

> In retrospect, Vine and Matthews' presentation was much more developed, including a sophisticated analysis of existing sea-floor magnetic data. While many people believe that ideas are the key to science, the difference in the treatment of the two papers shows that good ideas are not enough; you need good data, too, and you need to show how the data fit with the idea. And perhaps Vine and Matthews' Cambridge credentials carried weight at *Nature* that Morley's Canadian ones did not. (*Plate* xx)

Though Vine and Matthews were far more influential in changing the course of geophysical research and geological thought, I mention Morley for two reasons. First, one of the objects I analyze is the magnetic survey map of Raff and Mason. Morley's reaction to and citation of this map are important in understanding its persuasive effects. Vine incorporates Raff and Mason's work into later arguments, but Vine and Matthews do not mention the Raff and Mason map in their original

1963 paper. Second, in fairness, Morley did conceive of the same idea, and historians have subsequently recognized his activity, even if it was not recognized at the time. I follow several historians in referring to this hypothesis as the Vine-Matthews-Morley hypothesis.

Evangelizing the Vine-Matthews-Morley Hypothesis: 1963-1968

Vine and Matthews' 1963 article initially met with perhaps the worst possible reaction for scientists—silence. Vine recalls that only one researcher—Scripps' Victor Vacquier—even mentioned the article at the 1964 Royal Society Discussion Meeting on Continental Drift; Vacquier's comments dismissed the hypothesis ("Reversals" 58). Matthews recalls that "the paper dropped into a sort of vacuum" and that "American labs wouldn't hear anything of it—thought it was all nonsense" (qtd. in Glen 303). Vine describes 1964 as a "fallow year" for their hypothesis ("Reversal" 59). Drift-related papers published through 1965 either ignored or opposed it, but interest spread, and other researchers would eventually support the idea.

In 1965, Canadian geologist J. Tuzo Wilson and Princeton professor Harry Hess both came to Cambridge on sabbaticals. Both professors supported the hypothesis, and the exchanges between them and Vine helped to refine it. Specific discussions between Vine and Wilson influenced Wilson's conception of transform faults. A transform fault occurs when plates move horizontally in relation to each other (Wilson 482). These tectonic features could account for a range of topographic and magnetic phenomena within the sea-floor spreading construct. Figure 3.3 is a graphic published in Wilson's first paper on transform faults; it demonstrates four variations of transform faulting. The doubled vertical lines represent ridge sections, the horizontal lines represent the fault, and the arrows represent the direction of motion.

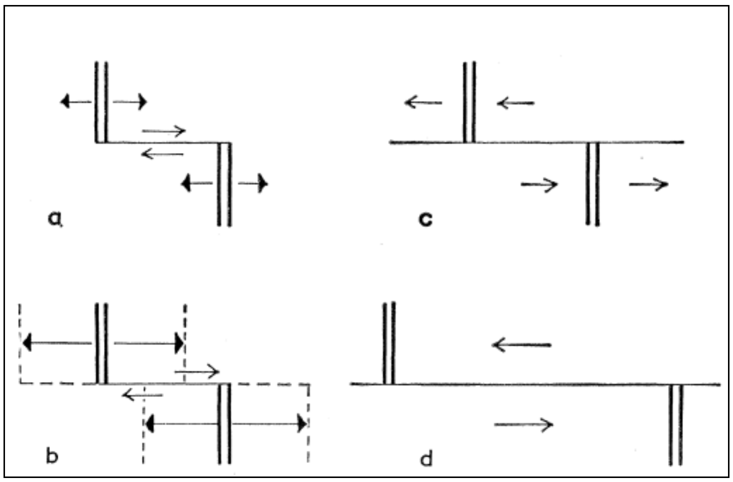


Figure 3.3: Wilson, *Science*, 1965, Figure 1.

These fault types were useful in explaining why magnetic patterns were offset in specific ways.

Another important development of the 1963-1966 period was the consistent refinement of the time scale of magnetic-polarity epochs originally described in 1963 by U.S. Geological Survey researchers Allan Cox, Richard Doell, and Brent Dalrymple. Figure 3.4 is a graphic from Allan Cox's 1969 review article that describes the scale's development. The black bands are positive polarity epochs; the white bands are negative polarity epochs.

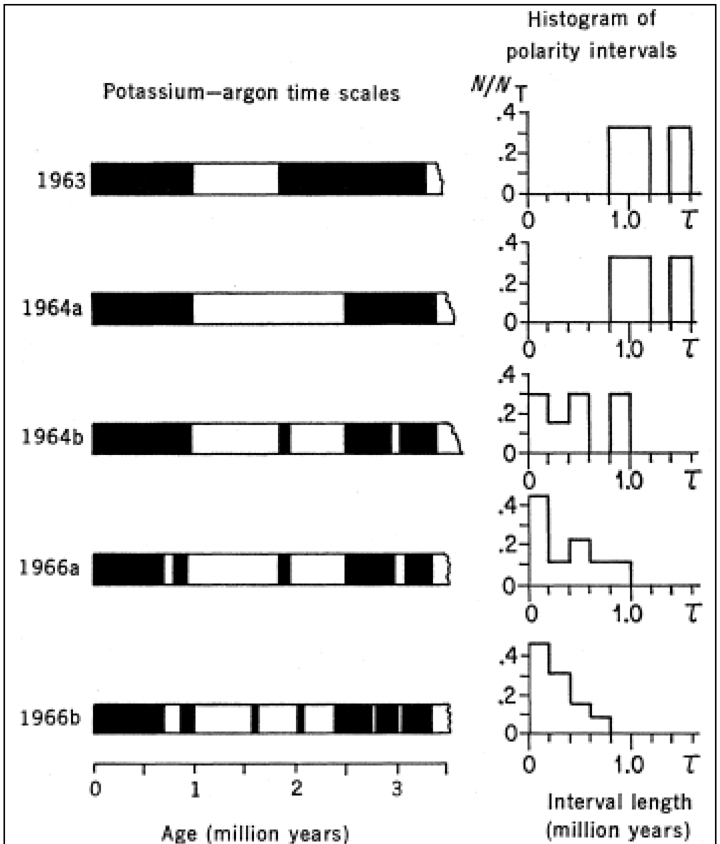


Fig. 3. Successive versions of the radiometric time scale for reversals, showing how the discovery of polarity events changed the apparent distribution of polarity intervals (*16, 18, 19*). In the corresponding histograms, N_T is the total number of polarity intervals and N is the number in each class interval of the histogram.

Figure 3.4: Cox, *Science*, 1969, Figure 3.

Of special significance was the Jaramillo event, the thin black stripe at one million years on the 1966a and 1966b scales. This event would provide a clear marker for calibrating distance and time with magnetic measurements. When researchers from the Lamont Geological Observatory applied the refined reversal timeline to new visualized data, they created the most significant visual argument in support of the Vine-Matthews-Morley hypothesis.

Initially, researchers at Lamont, including the director of marine magnetics Jim Hiertzler, were stabilist in orientation. They opposed the implications of the Vine-Matthews-Morley hypothesis, and Matthews recalls that Hiertzler's 1965 articles were among those that "continued to flay it forever" (qtd. in Glen 303). The position of the Lamont team changed after a research voyage to the Pacific-Antarctic ridge off the southern tip of South America. Magnetometers aboard the *U.S.S. Eltanin* collected a revolutionary set of data that both confirmed the Vine-Matthews-Morley hypothesis and swayed previously skeptical researchers at Lamont and elsewhere. Making a number of passes over the ridge, the crew of the *Eltanin*, which included Hiertzler's protégé Walter Pitman, collected magnetic data. When these data were later analyzed and rendered as magnetic profiles, the results were astounding. One of the profiles was an almost perfectly symmetrical curve of positive and reverse magnetism extending for hundreds of kilometers perpendicular to the ridge segment. This highly symmetrical data fulfilled the predictions of Vine and Matthews. Pitman and Hiertzler published their findings in *Science* in 1966; two weeks later Vine published a new review article that also incorporated a version of their graphic. Vine, Pitman, and Hiertzler would use this same graphic in various presentations throughout 1966, including presentations delivered at the April meeting of the American Geophysical Union and at a November conference hosted by the Goddard Institute for Space Studies. Researchers attending these conferences included the world's most eminent geophysicists. After these publications and meetings, support for the hypothesis was nearly universal. As I will show, this conversion was directly linked to visual arguments; therefore, there is exigence to study the rhetorical construction, distribution, and reception of these essential artifacts.

<u>Review of Previous Studies</u>

Because this episode in the history of science occurred relatively recently, the available archive of primary sources is extensive, and there are numerous scholarly accounts of it. However, some accounts contain inaccuracies that are revealed when these accounts are verified against the primary sources. This case has been the subject of traditional scientific histories (such as William Glen's *Road to Jaramillo*), retrospective autobiographies (such as the essays collected in Naomi Oreskes' *Plate Tectonics*), and philosophical and sociological analyses (such as H. E. Le Grand's *Drifting Continents, Shifting Paradigms*). This episode has also been used as a test case for philosophical models of scientific decision making, and it has been included in sociological accounts of the role of visualization in constructing knowledge. While many of these studies incorporate or mention visual artifacts, typically visuals are not the primary focus. The most explicitly visual-oriented analyses are Giere's essay "Visual Models and Scientific Judgment" and Gooding's "Visualization, Inference and Explanation in the Sciences." Each of these essays discusses a few of the images that I analyze, but they do not examine invention, circulation, and reception issues extensively. For example, Giere's philosophical approach traces the development of ideas at the macro level, but this approach neglects important intra-textual and intra-graphical elements. Similarly, Gooding makes incorrect assumptions about the influence of texts based solely on their publication dates; reception evidence indicates that the patterns of influence are more complex.[24]

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[24] The limitations of Giere's and Gooding's readings of these images might demonstrate the differences in orientation between rhetoric of science and the other branches of science studies. Giere and Gooding are interested in broad issues of cognition, inference, and decision making with regards to science, but these processes cannot always be adequately recaptured and reconstructed from all

To support my conclusions, I draw on several of the biographical and autobiographical sources that document the reactions of geophysicists to new visual arguments. William Glen's *Road to Jaramillo* (1982) incorporates material from thousands of hours of interviews with researchers. A more recent collection, Naomi Oreskes' *Plate Tectonics: An Insider's History of the Modern Theory of the Earth* (2001), offers a series of retrospective essays written by the researchers themselves. Mason, Vine, Morley, and Pitman each contributed to the volume, as did other researchers who extended and refined the early research. Though these retrospective accounts were written forty years after the key events, they are the best reception evidence available for this case. Time could have affected the memories of these individuals, but the fact that so many of them recall when and how they reacted to specific images is evidence for the persuasive significance of these maps and profiles.

Document Summary

As is the case with many other developments in modern science, the documents that articulated the Vine-Matthews-Morley hypothesis were distributed across multiple journals and conferences. Some of these venues had a broad scientific audience, while others were more specialized. Table 3.1 lists the sources I discuss throughout my analysis.

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networks of texts. What texts enable is the rhetorical analysis of arguments constructed and received at specific cultural moments. The rhetorician is not as interested in how a communicator arrived at the idea; rather, he or she wants to know what strategies were used to articulate the idea, and if these strategies were successful in persuading others to accept the new knowledge claim. As I will show in the rest of this chapter, this kind of information is recoverable from texts and from reception evidence.

Year	Authors	Publication
1961	Raff and Mason	*GSA Bulletin*
1963	Vine and Matthews	*Nature*
1966	Pitman and Hiertzler	*Science*
1966	Vine	*Science*
1966	Vine	*Goddard Institute of Space Studies Conference*
1968	Pitman, Herron, and Hiertzler	*Journal of Geophysical Research*

Table 3.1: Document list.

I analyze two sets of visuals culled from these texts in terms of the graphics' features, the visual arguments they encode, and the circumstances of their circulation and reception. In both sets of images, the invisible magnetic properties of the sea floor are made visible and incorporated into an array of evolving arguments.

The first set of images is composed of three iterations of the same magnetic anomaly map. Affectionately named "zebra stripe" maps in some retrospective accounts, these images visualize a pattern of magnetic anomalies off the coast of Vancouver Island in the Pacific. Though each version presents the same visual data, variations in mediation strategies change the knowledge claims presented in each map. This study is the first comparative analysis of these three figures.

The second set of images uses magnetic anomaly profiles to develop the visual arguments that enabled the Vine-Matthews-Morley hypothesis to become a viable model for explaining and asserting continental motion. The key feature of this visual data is the bilateral symmetry of some anomaly profiles. Authors use a number of strategies to amplify this symmetry, and these amplification strategies become conventionalized rhetorical actions.

Methodological Review

As outlined in Chapter 1, my methods include a number of analytical techniques and rhetorical concepts: semiotics, argument analysis, visual figuration, terministic screens, convention analysis, and circulation and reception study.

This chapter considers how Burke's terministic screens can account for the successes and failures of the rhetorical activities of this episode in science. As Burke notes, "Even if any given terminology is a *reflection* of reality, by its very nature as a terminology it must be a *selection* of reality; and to this extent it must function as a *deflection* of reality" (115). I argue that the rhetorical history of specific magnetic anomaly maps demonstrates this reflection-selection-deflection dynamic.

This chapter also considers Perelman and Olbrechts-Tyteca's comments on rhetorical presence in light of the relationships between presence, selection, and visual figuration. Like the terministic screen, the concept of presence can help to explain the successes and failures of specific visual arguments. As Perelman and Olbrechts-Tyteca explain, "By the very fact of selecting certain elements and presenting them to the audience, their importance and pertinency to the discussion are implied. Indeed, such a choice endows these elements with a *presence*, which is an essential factor in argumentation and one that is far too much neglected in rationalistic conceptions of reasoning" (116). They also note, "*Presence* acts directly on our sensibility" (116). In this case, rhetorical choices created data graphics that acted directly on the sensibilities of geophysics researchers.

Finally, I am interested in the visualization of transitivity arguments and in the relationship between visualization, symmetry, and conceptual change. According to

Perelman and Olbrechts-Tyteca, "The use of transitive relations is valuable in cases where it is a matter of ordering beings and events which cannot be directly compared with each other" (229). In this case, scientists used graphed magnetism data to connect distance and time visually when they could not compare those variables directly. Scientists also relied on the visual presentation of symmetry to separate the appearance of stability from the reality of a mobile sea floor.

Though the magnetic survey maps and the magnetic profile graphics appear in the literature of this case across the same span of time (1961-1968) and sometimes in the same document, the discussion is divided by image type. I first discuss the changing rhetorical landscape encoded in the magnetic maps. I then discuss the strategies used to turn magnetic profiles into compelling knowledge claims supporting sea-floor spreading and the Vine-Matthews-Morley hypothesis.

Zebra Stripes: The Iterations and Arguments of Magnetic Anomaly Maps

The "zebra stripe" image set is comprised of three images (Figures 3.5, 3.6, and 3.7). Figure 3.5 is the original map that appeared in Raff and Mason's 1961 article in the *Geological Society of America Bulletin*. Figure 3.6 appeared in Vine's seminal 1966 review article in *Science*. Figure 3.7 is a colorized version; colors correspond to a magnetic reversal time scale that appears beside the map. Vine presented this image during his November 1966 presentation at a conference hosted in New York by the Goddard Institute for Space Studies. It was republished in a 1968 volume, *The History of the Earth's Crust*, which records the proceedings of the

1966 conference.[25] Figures 3.5, 3.6, and 3.7 appear after this paragraph. Other supporting figures (Figures 3.8-3.16) are distributed throughout the discussion.

[25] Though technically Figure 3.7 was presented before Figure 3.6, Figure 3.6 was developed first. Figure 3.7 was presented November 12, 1966; Figure 3.7 appeared in print December 16, 1966. However, Figure 3.6 was submitted in September of 1966, thus it is conceptually prior to Figure 3.7.

Figure 1. Index anomaly map of the total magnetic field. The positive area of the anomalies is shown in black.

Figure 3.5: Raff and Mason, *GSA Bulletin*, 1961, Figure 1.

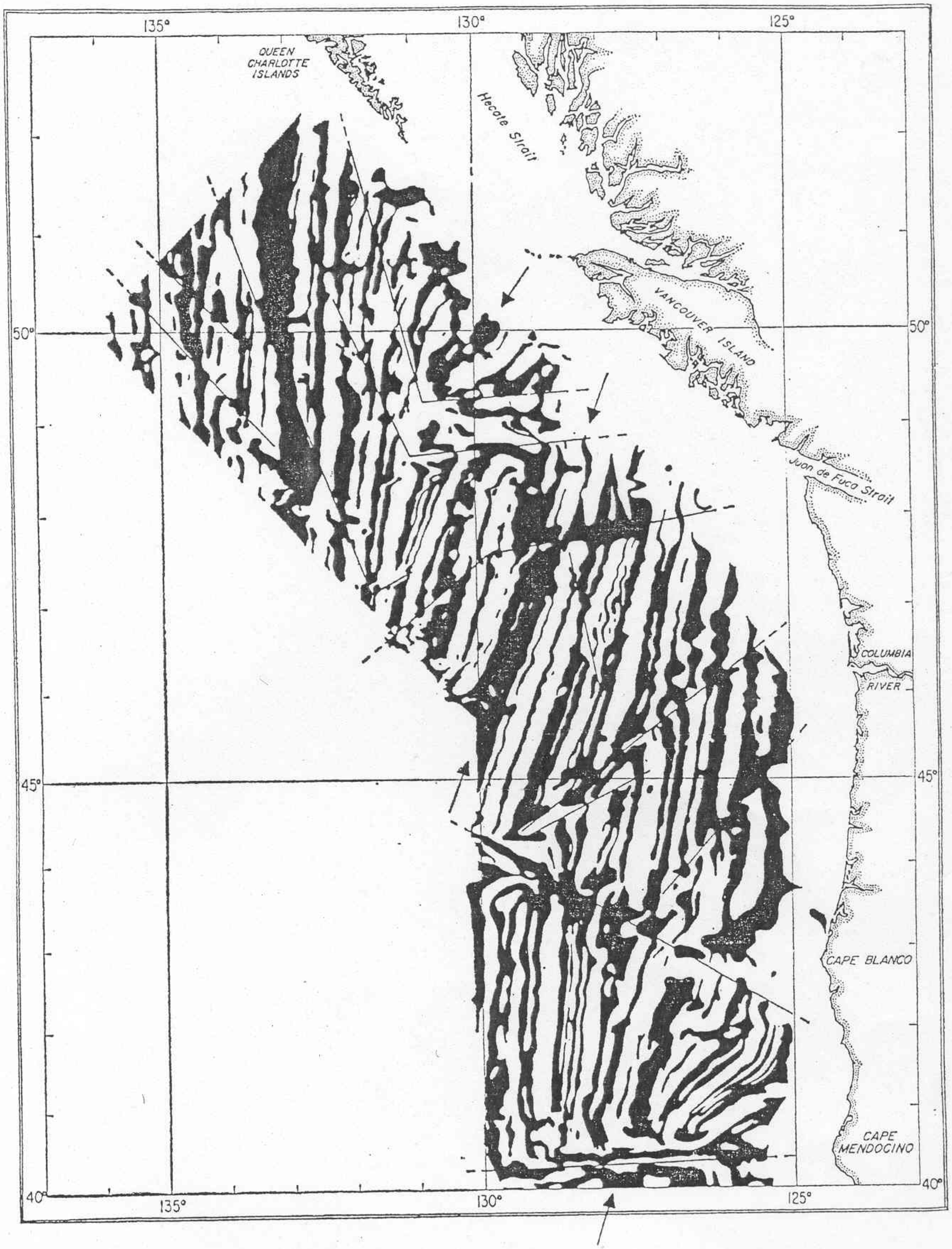

Fig. 1. Summary diagram of total magnetic-field anomalies southwest of Vancouver Island. Areas of positive anomaly are shown in black. Straight lines indicate faults offsetting the anomaly pattern; arrows, the axes of the three short ridge lengths within this area—from north to south, Explorer, Juan de Fuca, and Gorda ridges. See also Fig. 15. [Based on fig. 1 of Raff and Mason (27); courtesy Geol. Soc. Amer.]

Figure 3.6: Vine, *Science*, 1966, Figure 1.

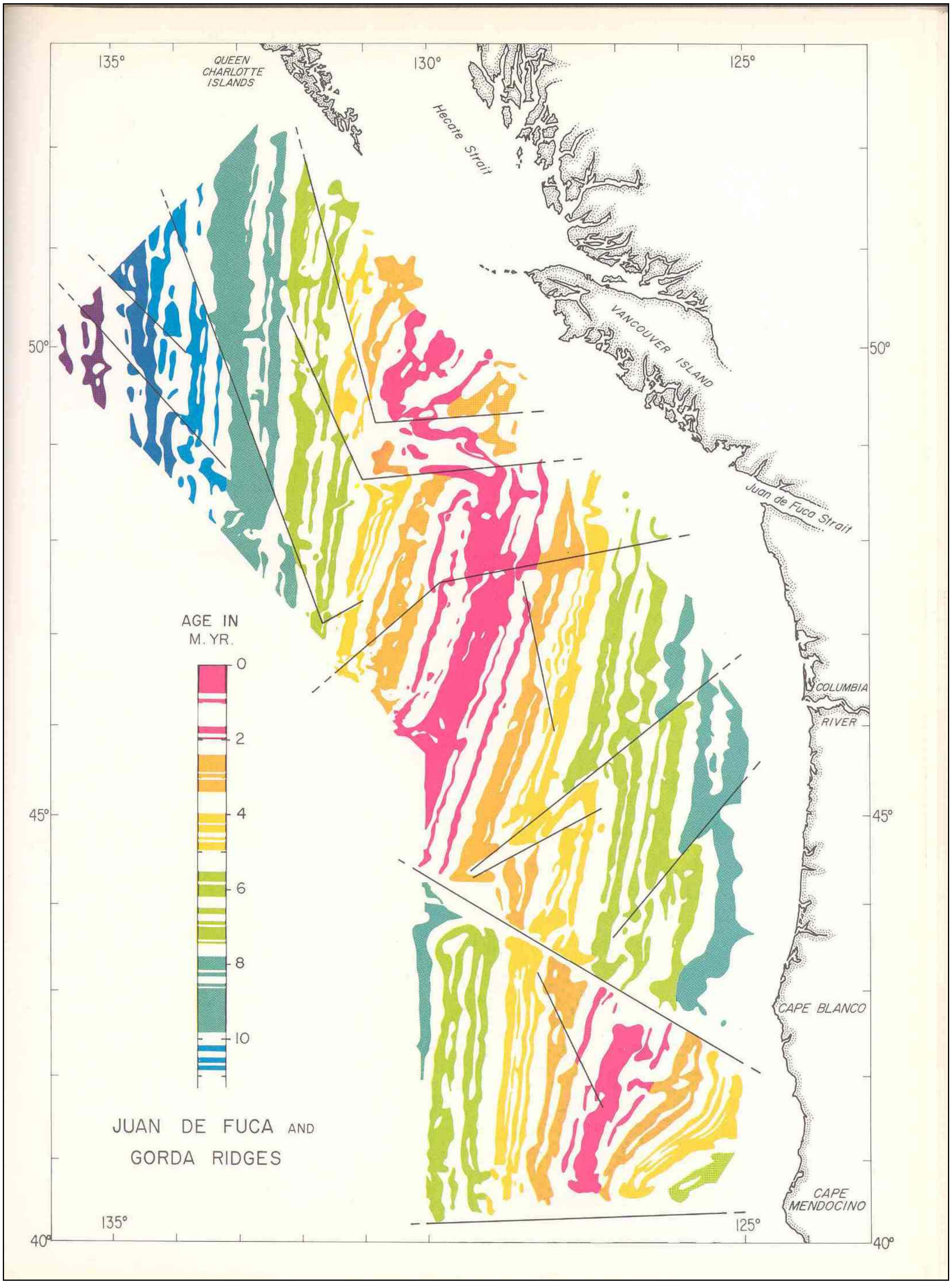

Figure 3.7: Vine, Goddard Conference, 1966, Figure 4.

Raff and Mason's most referenced magnetic anomaly map (Figure 3.5) depicts a pattern of magnetic anomalies in the oceanic rock off the coast of Vancouver Island. This map is an adaptation of another magnetic anomaly map of the same area; Raff and Mason's Plate 1, my Figure 3.8.[26]

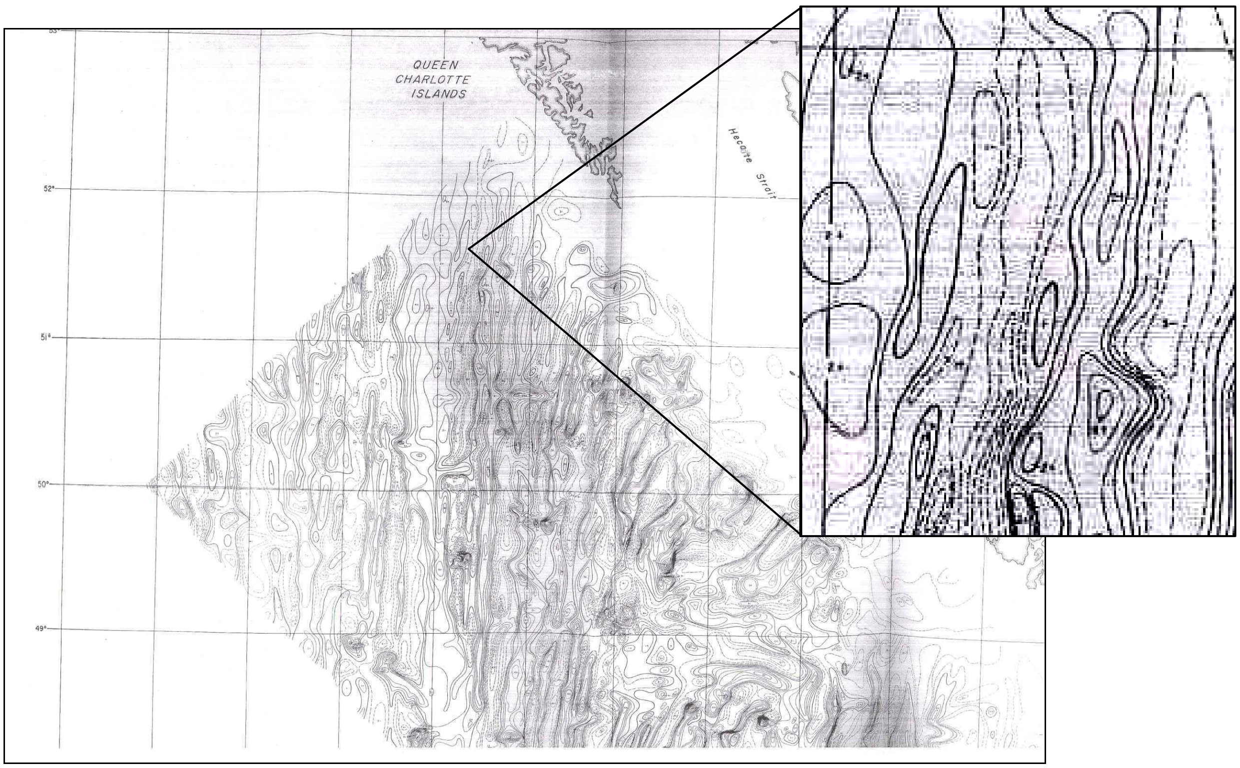

Figure 3.8: Raff and Mason, *GSA Bulletin*, 1961, Plate 1.

Figure 3.8, like Figure 3.5, presents magnetic data in what Kress and van Leeuwen would call a topographic structure. That is, the visualization of the data accurately reflects the real spatial relationships between constituent elements (101-102). However, Figure 3.8 records specific magnetic values in addition to polarity and shape. As the enlarged section of Figure 3.8 shows, these features are encoded

[26] Raff and Mason's Plate 1 covers the same area as the summary map; however, it is in the form of a large-format foldout. My Figure 3.8 shows approximately half of the plate.

through solid and dashed lines with values and polarity signs near the lines. Though a thorough examination of this map might indicate linear magnetic anomalies, these patterns are not as immediately visible as they are in Figure 3.5. To give additional presence to distinctions between the anomalies, Raff and Mason used an antithetical black-and-white color scheme to encode only for positive or negative polarity in the summary map. As Fahnestock notes, the figural move from graded series to binary distinctions is a basic conceptual process that can be rendered both visually and verbally (*Rhetorical Figures* 45-85). In Figure 3.5, positive magnetic anomalies are shown in black, and negative anomalies are shown in white. The form of the anomalies will not change much in later iterations of this map. What does change are the mediating visual elements that make specific features of the mapped data more or less salient. Early arguments rely primarily on the antithetical logic of two-tone coloration; later graphics apply different strategies for conceptually reorganizing the "terms" that emerge from the data.

In this first iteration (Figure 3.5), Raff and Mason use alphabetically labeled lines to identify specific features in the anomaly pattern. Though the labels are not explained in textual descriptions on the image or in the caption, they are explained in the text of the article. Table 3.2 summarizes the textual descriptions of the labels. Noteworthy from a semiotic perspective is the artificiality of the group created through the labels. Beyond the common carrier of the alphabetically labeled lines, the identified elements do not share a common feature to define them as a class. As Table 3.2 summarizes, some of the lines are magnetic features, some are topographic features, and others are "suggestions." Thus in Figure 3.5, the lines are ambiguous as carriers of specific attributes; it is not immediately clear when scanning the visual what each line encodes.

Label	Identified Feature
A	Linear structures bend smoothly
B	No topographic counterpart
C	Boundary with a gradient density similar to continental shelf
D	Row of three sea-mounts
E, G, and H	Magnetic gradient density change not indicated by the topography
J and K	Faults in the magnetic linear trend
L, M, and N	All the same feature with the center section having slipped to the east
Q	Trough 800 fathoms deeper than the surrounding area
R	500 fathom ridge with no magnetic counterpart
S	Suggestion of a fault, but nothing indicated from the topography
T	Area of magnetic depression
U, V, and W	Partial breaks in the linear pattern

Table 3.2: Textual explanation of alphabetic labels summarized from Raff and Mason (1961). The map does not identify features with the labels F, I, O, P, X or Z.

The primary argument of this image is one of identification, what classical stasis theory would call an An Sit ("It is") argument. According to Prelli's stasis grid for scientific arguments, the image is an evidential/conjectural argument: "Is there evidence for the claims that X exists?" (*A Rhetoric of Science* 146). In other words, the image and the article only claim that a phenomenon exists; the extant phenomenon is an anomaly pattern with a roughly linear organization. As Mason explains in an autobiographical essay, "The discovery of sea-floor magnetic stripes was serendipitous: We were not looking for them, nor could we have been, because no one knew they existed!" ("Stripes" 43). Thus, the rhetorical task of this article and its primary image is to show that this newly discovered phenomenon exists. Visualization indicates existence.

Though the authors do identify additional attributes with the labeled lines, these graphical features lack a common trait that would allow the taxonomy to encode any additional visual arguments beyond an "it is"/"they are" identification. Moreover, the authors themselves admit that the cause and meaning of the pattern is unclear: "There is as yet no satisfactory explanation of what sort of material bodies or physical configurations exist to give the very long magnetic anomalies" (1269). This statement indicates that the conceptual separation of magnetic and physical topography is not yet in operation for Raff and Mason. That is, they expect existing topographic features, such as seamounts, ridges, and troughs, or physical features, like rock type or sediment density, to account for *all* of the magnetic differences. Later arguments developing the Vine-Matthews-Morley hypothesis rhetorically negotiate the relationship between magnetic and physical geography.

The New Rhetoric's concept of presence helps to explain the authors' decision to code the image with only two signifying tones—one for positive and one for negative magnetic anomalies. This design feature clearly marks the linear anomaly pattern in a way that the more traditional contour-style map (Figure 3.8) does not. By making antithetical distinctions rather than graded ones, which would reflect the data values more closely, the linear pattern of polarity differences is readily visible. The clearly presented pattern might account for the intense reaction other scientists had when seeing the image (Figure 3.5) for the first time. Lawrence Morley recalled his initial reaction in his retrospective essay "Zebra Stripes": "I literally freaked out when I saw it!"(80). Fred Vine shared a similar experience while viewing the map with Harry Hess and J. Tuzo Wilson in 1965 after retrieving the 1961 article from the library to corroborate some of their coalescing ideas: "All three of us stared at it in amazement" (60). The contrasting tones allowed patterns in the data to be recognized immediately by these scientists whose new ideas were guiding their vision.

On the other hand, Burke's notion of the terministic screen might help explain why the map received little attention from 1961 until more than two years after its publication. As noted earlier, Burke asserts that the terms we choose to reflect reality must also deflect other possibilities. Raff and Mason effectively foreground the magnetic pattern through terministic selection, but other variables are overwhelmed by the pattern. For example, seamounts, troughs and other physical features cannot be represented effectively on the map because the graphical space is filled with the visualized magnetic data. The arbitrarily labeled lines do not help the viewer to see significant patterns in the relationship between magnetism and topography. Though Vine and Wilson were able to "see" the underlying correspondence between some of Raff and Mason's linear anomalies and the crests of oceanic ridges, this was possible only after they anticipated the existence of the physical ridges in the same area that Raff and Mason had mapped.

Further complicating the situation is the relationship between science, international politics, and military affairs during the middle of the twentieth century. As Vine notes, "The irony of the discovery of the Juan de Fuca Ridge [the largest ridge in the Raff and Mason survey area], or rather its non-discovery at an earlier date, is that because the survey was undertaken for military purposes during the Cold War—detailed maps of bathymetry and gravity field were required for the nuclear submarine deterrent—only the full details of the magnetic data were declassified" (60). The selection of data by Raff and Mason reflected the magnetic attributes of the northeastern Pacific sea floor, but at the same time, the realities of military-funded science forced them to exclude clear details about the physical topography. Vine claims that the absence of these details slowed the development of the Vine-Matthews-Morley hypothesis: "Had the detailed bathymetry been released at the same time as the magnetics, this would have been a very different story" (60).

Alternatively, Raff and Mason's nebulous conclusions, as encoded in their graphic, contributed to the curiosity of Morley. He notes the effect of this underdetermined work on his thinking:

> In retrospect, what stands out for me is how Mason and Raff frankly admitted to having no plausible explanation for the "zebra stripes," which they had spent so much time and effort to acquire. If they had suggested that one of their interpretations was indeed correct, I would probably have accepted it, thinking that they had the situation in hand. Instead, their forthright approach created a space for a young scientist like myself to attempt an explanation of their data. (85)

Ambiguity provided an opening for Morley's correct but unpublished conjectures.

Raff and Mason's map can be interpreted as a vetted artifact of Cold War science or as an example of an honest but unassertive scientific claim. Regardless of the motivation for selecting or not selecting data to present, the rhetorical effect of these choices was the same. Raff and Mason's choice of visual terms affected the inferences about reality that the map could generate on its own for its viewers. The Vine-Matthews-Morley hypothesis developed because later scientists could bring new concepts to their theory-laden readings of this image.

<u>Circulation and Iterations of the "Zebra Stripes"</u>

From a circulation and reception perspective, Raff and Mason's map (Figure 3.5) is fascinating. Not only was the map significant for Morley and Vine in the development of their thinking, but it also has an interesting though sometimes troubling circulation history in the broader science studies literature. Specifically, in several works (for example, Oreskes' *Plate* and Henry Frankel's "From Continental") Vine's 1966 version (Figure 3.6) is falsely reproduced as Raff and Mason's original map. My analysis of the 1966 image might help explain why this false attribution occurs.

Vine's 1966 review article was a watershed argument in the establishment of sea-floor spreading. (When *Current Contents* dubbed it a "Citation Classic" in 1986, it had been cited in more than 440 articles.) Though Vine and Matthews articulated their hypothesis in 1963, they did not have sufficient evidence to convince many geologists that the history of sea-floor spreading was "recorded" in the remnant magnetism of oceanic rock. Vine's 1966 review article marshaled a host of new visualized data, including magnetic profiles and magnetic anomaly maps. An iteration of Raff and Mason's map was an important supporting argument. As noted earlier, the version of the map appearing in the Vine article is often cited as the Raff and Mason map; however, there are subtle differences between the two, and these differences have significant rhetorical consequences.

Like Raff and Mason's map, the Vine version (Figure 3.6) is what Kress and van Leeuwen would call a topographical-conceptual system. System features are represented in relation to one another according to a scale that is accurate to these relationships in physical space (101). However, in his version, Vine has replaced Raff and Mason's relatively uninformative alphabetically labeled lines with more revealing carrier/attribute pairs. Specifically, he uses thin lines to represent exclusively the faults that have affected the anomaly pattern; he uses arrows to point to the axes of oceanic ridges associated with specific magnetic anomalies. Essentially, Vine layers Wilson's identification of transform faults and sea-floor ridges off Vancouver Island (Figure 3.9) onto the visually arresting magnetic visualization of Raff and Mason. The new details do more than just identify the geologic features of the area; the changes direct the viewer's attention. The arrows above and below the thick black band in the center of the map pull the viewer's eye to this large positive anomaly; the other arrows make smaller anomalies more salient as well. These additions change the mapped data from a loosely interpreted mass into a more structured composition that communicates a different kind of argument. The arrows lead the viewer to accept that this image is comprised of symmetrically arranged phenomena running parallel to the graphically identified ridge centers; the fault lines explain why the map is not perfectly symmetrical. These new distinctions—with some textual and inter-graphical anchoring— also change the stasis of the argument from the conjectural to the causal. In this review article on sea-floor spreading, ridges are the generative engines of geologic motion. By visually identifying these loci of activity, Vine identifies the primary cause of this distinctive pattern. Thus, the original map (Figure 3.5 on page 139) has been adapted in Figure 3.6 (page 140) to be consistent with the sea-floor spreading activity argued for at large in Vine's 1966 article.

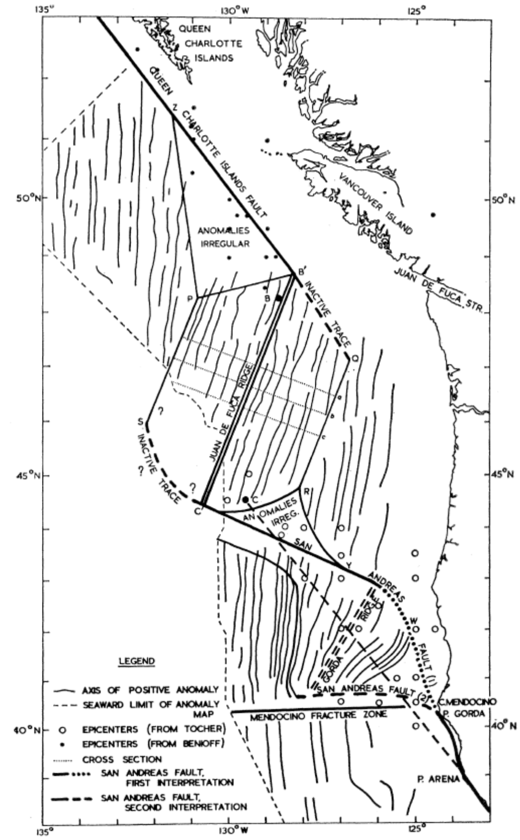

Fig. 3. Sketch map showing the proposed interpretation of the Juan de Fuca Ridge with its relation to other faults and the magnetic anomalies. It should be compared with the map of anomalies in Raff and Mason (7).

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Figure 3.9: Wilson, *Science*, 1965, Figure 3.

The consistent misidentification of the first Vine map (Figure 3.6) for the Raff and Mason map (Figure 3.5) could be attributed to sloppy fact checking; however, science-studies scholars might unconsciously (or consciously) select Vine's version because it is a better theory-laden representation of the Vine-Matthews-Morley hypothesis. This intuition is supported by graphics from other historical and philosophical treatments of "continental drift" that include (and correctly identify) the original Raff and Mason map but modify the map with explanatory devices that are similar in function to Vine's arrows. For example, historian H. E. Le Grand adds a clear label of the Juan de Fuca Ridge axis (Figure 3.10).

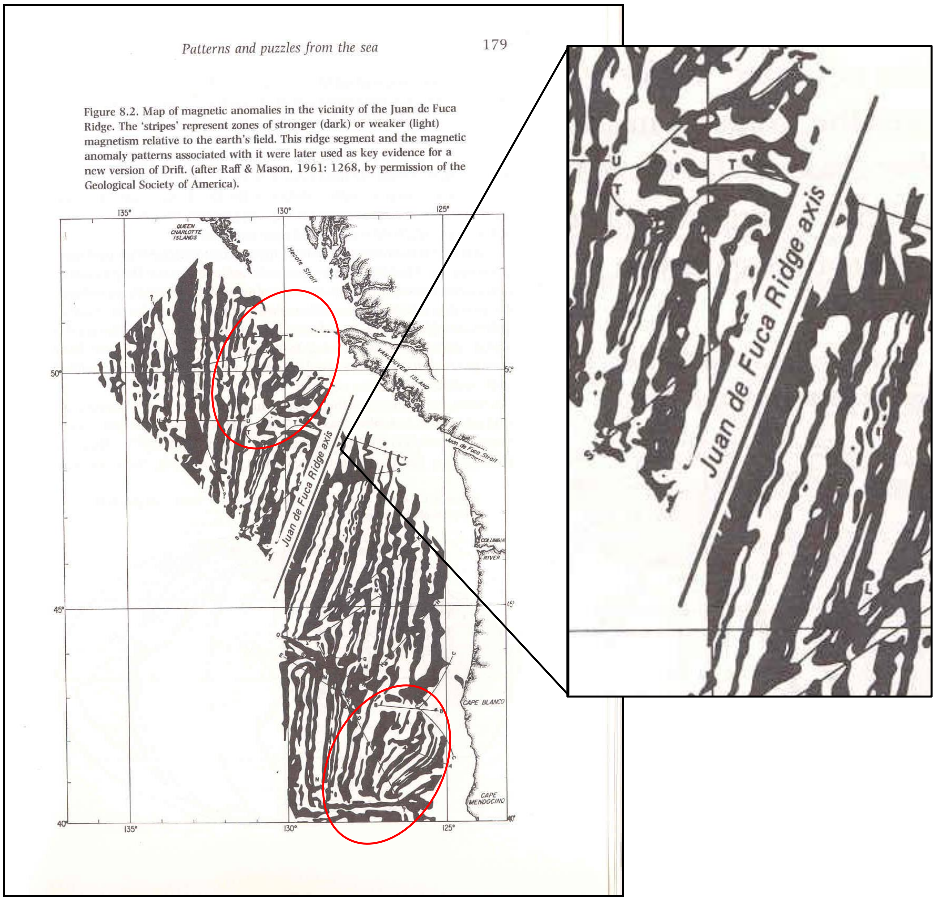

Figure 3.10: Le Grand, *Drifting Continents: Colliding Paradigms*, Figure 8.2, my red circles. Enlarged section shows the label details.

Though this identification is helpful, it is also potentially misleading. By marking off the Juan de Fuca Ridge, but not the Gorda or Explorer Ridges, Le Grand makes it seem as if the Juan de Fuca Ridge is the organizing feature of all the magnetic anomalies in the entire region, which is not the case. A more accurate rendering would also identify the other ridges, whose locations Vine identified with other arrows. I have identified them with red circles on Figure 3.10.

The third iteration of the map (Figure 3.7) is also from 1966. Vine included it in his Goddard conference presentation, and it was reprinted in the 1968 volume *The History of the Earth's Crust*. There are several significant differences between this map and its predecessors. Most obviously, this version is in color; the color allows additional information to be layered onto the map.[27] Specifically, color is a carrier for the age of the positive magnetic anomaly bands. The color gradient follows the traditional arrangement of the color spectrum; red/pink represents the youngest anomalies, and blue hues represent the oldest ones. By adding color and also adding the color-coded time scale, Vine has transformed the graphic into a complete transitivity argument: magnetism is related to distance; magnetism is related to time; therefore, distance is related to time. Thus, concepts that cannot be compared directly are connected with a middle relational term. As my later analysis of the profile data will show, this map is not the first complete visualization of this transitivity argument, nor is it the most effective. But this graphic is novel for this case in using multiple colors to present a visual transitivity argument.

The addition of color also adds another layer of figural logic to the graphic. Specifically, Vine has transformed the graphic from a simple black vs. white antithesis into a pair of color-coded *antimetabole*. Colored elements are repeated in reversed order: blue-teal-green-yellow-orange-red-orange-yellow-green-teal-blue. The red stripes mark the two ridges that are the pivot points of each *antimetabole*. The symmetrical arrangements of colors represent the symmetrical arrangement of time on either side of each ridge axis. The rationale behind this figural

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[27] There is clear evidence from the conference proceedings that this image was presented in color at the Goddard conference. During the Q&A of Menard's presentation, Vine refers explicitly to the color slide: "With regard to the Juan de Fuca area I think it is clear from the color slide [Fig. 4, Vine, this volume] that there is no single process going on" (Vine qtd. in Phinney 117). However, it is not clear if the coloring of the slide was identical to the reproduced figure. For example, in Vine's caption text he explains, "Central red anomalies coincide with ridge crests" (Vine qtd. in Phinney 80). In my Figure 3.7, a scanned true-color facsimile of the figure from the Phinney volume, these crests are an intense shade of pink. If there was a difference in color (red vs. pink), it affects neither Vine's arguments nor my analysis.

transformation will become clearer after my explication of the magnetic profiles in the next section.

The addition of color is not the only change Vine makes. He also modifies the form of some of the mapped magnetic anomalies. A comparison of the color map with the previous black-and-white maps reveals that Vine has removed magnetism data from the diagonal fault off Cape Blanco. I have marked this redaction of magnetic data with a rectangle on Figure 3.11. Figure 3.12 compares enlarged portions of Figures 3.6 and 3.7 to demonstrate the data redaction.

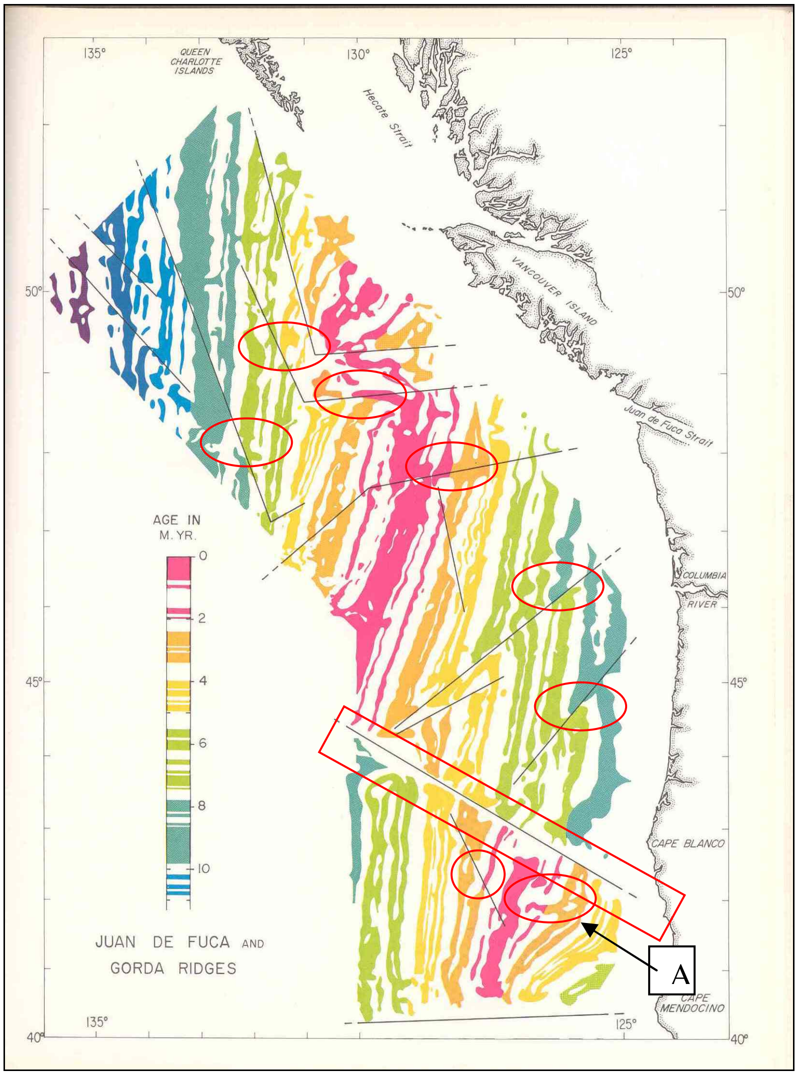


Figure 3.11: Vine, Goddard Conference, 1966, Figure 4. The rectangle marks the area where data were redacted. Circle A marks a magnetic feature that was divided into two colors but is not associated with a fault. Other circles identify areas where color is used to divide magnetic features on either side of a fault.

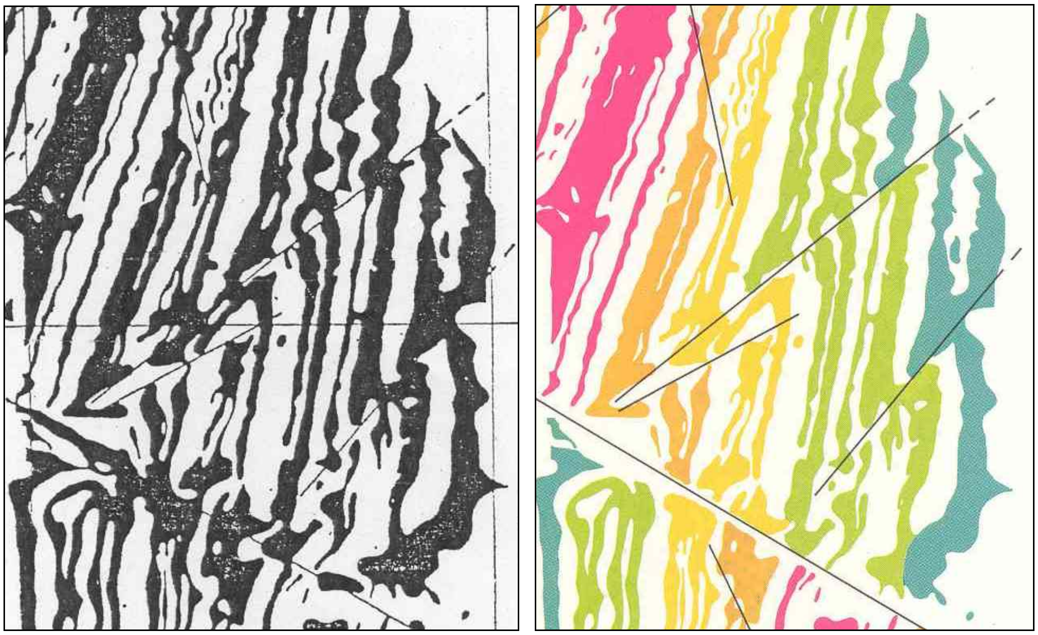


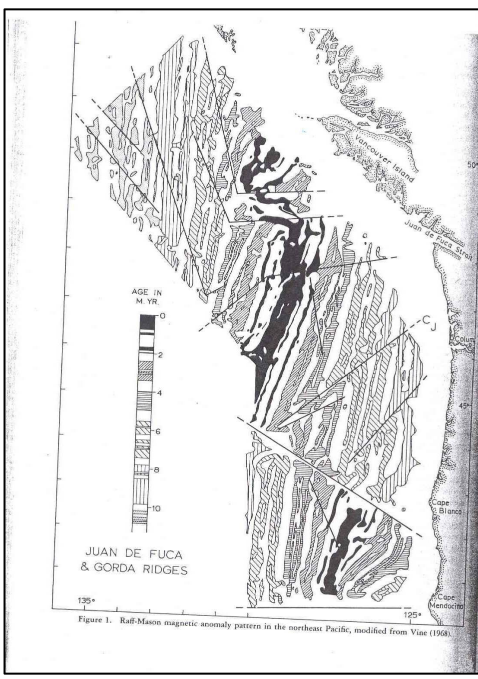
Figure 3.12: Comparison of enlarged sections of Figures 3.6 and 3.7. In the color map, data has been removed along the long diagonal line that forms a triangle with the bottom-left corner.

These changes are not mentioned in the transcribed text of Vine's talk. Vine notes that his interpretation is influenced by Wilson's 1965 article, but this article by itself does not necessarily justify removing the material. If Wilson's map of the area (Figure 3.9) authorized changes to Vine's color map, there should also be material removed at other places than the region between the two ridges. Also, as Wilson indicates, his map is conjectural and should be compared to Raff and Mason's map. Moreover, the removal in Figure 3.7 was not featured in Vine's other 1966 map (Figure 3.6), even though Wilson's map was also available to inform this graphic. Thus, the decision to remove material from this large fault seems more rhetorical than evidential. Specifically, he removes the magnetic data along the fault so he can more clearly arrange the features on either side of the fault with the color-coded time scale. Such a move is consistent with mapping practices observed by rhetorical theorists studying the rhetorically motivated "arrangements" of data maps serving as important visual arguments.

As mentioned in Chapter Two, Lawrence Prelli has discussed the persuasive effects of arrangement in visual arguments. Specifically, he has shown how the concept of rhetorical *taxis* can help explain graphical choices. In "Visualizing a Bounded Sea: A Case Study in Visual *Taxis*," Prelli explains how Canadian and American trade representatives organized oceanic maps to "literalize" land-based metaphors that supported their respective arguments for fishing rights. For example, the Georges Bank—a productive fishery coveted by both sides— was portrayed as a submarine "peninsula" extending from the American coast on the U.S. map; it was portrayed as an independent "island" on the Canadian map (96-98). Each side adjusted contour lines so that the maps invoked familiar conceptual structures. Vine's map (Figure 3.7) does not invoke metaphoric associations, but it does demonstrate strategic visual *taxis*. Specifically, Vine taps the *topos* of division to separate the whole of the mapped pattern into two parts; he then uses color to arrange each part into a symmetrically organized series. The complexities of this visual *taxis* warrant further explication of it.

Vine's first map (Figure 3.6) notes the existence of three ridge sections, but his revised map (Figure 3.7)—by conflating ridge features with the red coloration—only visually identifies two ridges, which are separated by the new "empty space" along the long diagonal fault. Similar but less noticeable divisions are created in locations where bands that were solid entities on the previous map must be divided based on time data. Unlabeled circles on Figure 3.11 identify places where lines originally placed to identify faults are now also separating two differently colored areas of what was formerly represented as a unified magnetic-anomaly feature. Figure 3.12 (page 156) compares an enlarged section of Vine's colorized map with the same section of his black-and-white map to demonstrate how Vine divided his visualized data. The color divisions within the positive anomaly bands help to explain the removal of material along the fault. Without the removal, there would have been the possibility for multiple interpretations of the colorized data. That is, the removed materials could have been colorized and thus time coded in multiple ways. In the places where Vine has made color distinctions in what were uniformly black-coded anomalies, the divisions are small and visually consistent. They mostly occur in places where there is a clearly identified fault or in places where two large features touch. Even the small "peninsula" (Circle A on Figure 3.11) that is divided into pink and orange makes "visual sense," because the division lines up with the small but distinct anomalies north and south of it. However, carving up the material that Vine removed from the diagonal fault would have been difficult, since the color patterns of the two ridge systems are offset in relation to each other. Removing the potentially ambiguous magnetic data creates a clear distinction between the two separate sets of time-graded terms, which are conceptually organized around two separate physical ridges. The clean break also supports Vine's argument that sea-floor magnetism is less complicated than terrestrial magnetism: "The strikingly simple structure of the oceanic crust implied by the 'geologic map' presented in Figure 3.4 contrasts with the complexity of the continental crust" (80). Vine reinforces this "simplicity" with graphical choices affecting a specific visual *taxis*.

Vine's Goddard Conference map is an important graphical development. Later studies would use this map instead of the original Raff and Mason map as the basis for new visual arguments. For example, Silver (1971) as well as Hey and D. Wilson (1982) use a grayscale version of Vine's Goddard map when making new arguments about the Juan de Fuca area (Figures 3.13 and 3.14). Regardless of the accuracy of the adjustments Vine made to Figure 3.7, the image has become, in the terms of *The New Rhetoric*, a "truth." The account presented in Vine's color map, as such a "truth," provides a network of provisionally accepted claims that serves as a cohesive set of agreements on which new arguments can be built. The articles that present Figures 3.13 and 3.14 each elaborate "normal science" refinements to the geologic processes that created this distinctive magnetic pattern in this region of the northern Pacific.



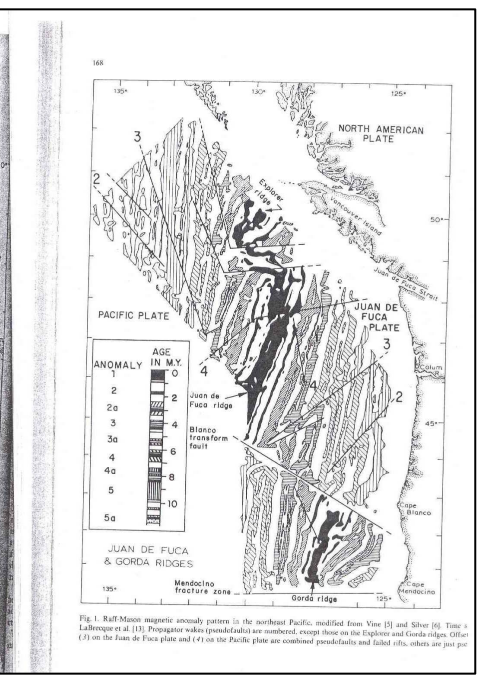


Figure 3.13: Silver, 1971, Figure 1. **Figure 3.14: Hey and D. Wilson, 1982, Figure 1.**

Circulation and Peirce's Semiotic Functions: "Zebra Stripes" as Icon, Index, and Symbol

In "The Verbal and the Visual in Science: A Heideggerian Perspective," Alan Gross reads Heidegger's philosophy of science against C. S. Peirce's semiotic system to account for the semiotic functions of scientific images. In Peirce's system, an image is an icon when it represents the depicted thing itself; it is an index when it represents a related phenomenon, such as the trace of an effect; and it is a symbol when it becomes the representation of a broad set of ideas and/or theory by convention. As Gross notes, scientific visuals, though typically indexical, can and often do perform all three semiotic functions (451). The history of the "zebra stripes" map demonstrates how a single image performs different functions when it appears in different texts.

Raff and Mason's original map functions as an icon. It shows a thing that has not been seen before; the representation is the thing itself. In this case, Raff and Mason show the remnant magnetism of a specific area. When the image appears in Vine's texts (Figures 3.6 and 3.7), the map functions as an index—the magnetic pattern exemplifies patterns that exist around the world. In another graphic from the Goddard presentation (Figure 3.15), Vine selects a small portion of the Raff and Mason map to demonstrate relationships between actual data and idealized models. This small area of the "zebra stripes" map also functions as an index; the selection demonstrates a process.

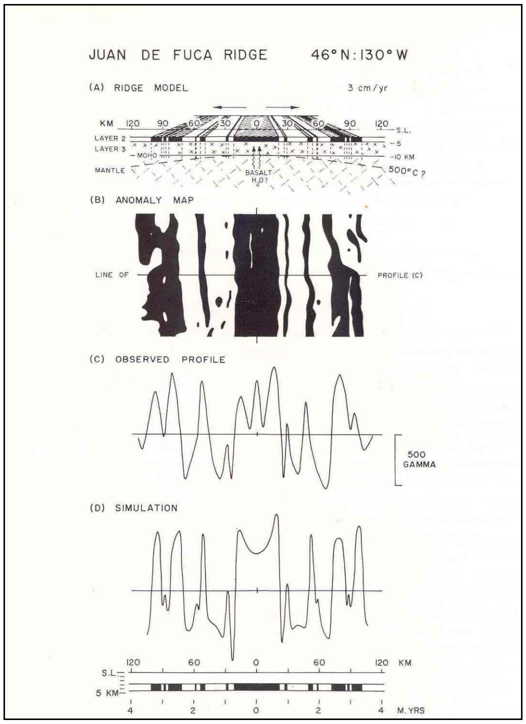


Figure 3.15: Vine, Goddard Conference, 1966, Figure 1.

Finally, the "stripes" image becomes an aestheticized symbol of this episode of the history of science. For example, the cover art (Figure 3.16) of Glen's *The Road to Jaramillo*—a history of the development of magnetic dating and its role in the development of plate tectonics—incorporates a portion of the Raff and Mason map. The color of this portion of the map was adjusted for the cover; the negative anomalies were colored blue.

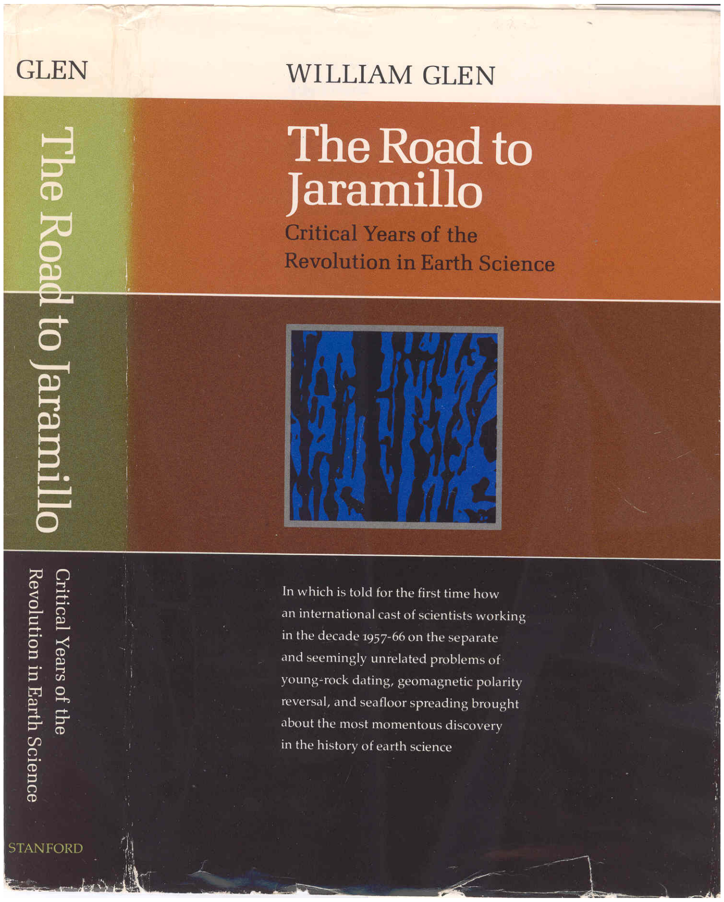


Figure 3.16: Cover of Glen, The Road to Jaramillo, 1982.

Interestingly, the aestheticized area is not the most essential data for justifying the Vine-Matthews-Morley hypothesis. It is, however, a visually interesting segment of the "zebra stripe" map. The section contains part of the largest anomaly stripe on the map. The salience of this particular stripe was progressively reduced by the vectors and colors of subsequent versions that emphasized other magnetic features.

Transitivity, Symmetry, and Magnetic Anomaly Profiles

As noted previously, the Vine-Matthews-Morley hypothesis was not persuasive initially. Visual arguments based on specific magnetic-anomaly profiles were essential in convincing the geophysical community that the hypothesis was viable.

In 1965 researchers from the Lamont Geological Observatory aboard the *U.S.S. Eltanin* collected magnetic data from the sea floor of the Pacific-Antarctic Ocean. These data were digitally processed and inscribed in the form of magnetic anomaly profiles. A magnetic profile is a line graph that demonstrates the remnant magnetism recorded by a magnetometer towed by a ship or plane traversing a specific path. Magnetic profiles are essentially cross sections of magnetic anomaly maps; this relationship is clearly demonstrated in Figure 3.15. One of the Lamont groups magnetic profiles—Eltanin 19—would become the most important piece of geophysical evidence supporting the Vine-Matthews-Morley hypothesis. But this significance is not immediately revealed by the visualized data alone. Rhetorical mediation transformed this curve into a functional visual proposition in an array of visual arguments.

The saw-toothed line of Eltanin 19 does not look like much in its unmediated form (Figure 3.17); however, when it is enhanced with rhetorical strategies, the profile articulates 10 million years of geophysical activity.

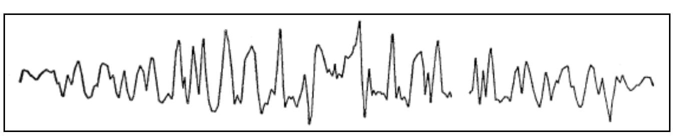


Figure 3.17: Raw profile of Eltanin 19.

The "symmetry" of the profile was the key aspect of this evidence; its symmetry fulfilled the predictions of the Vine-Matthews-Morley hypothesis by correlating the magnetic data extending outward from a ridge center with a newly revised timeline of magnetic polarity epochs. The rhetorical history of the Eltanin-19 profile begins with its use as primary evidence in Pitman and Hiertzler's 1966 research article. It was later circulated as a supporting figure in Vine's review article and as a component in graphics for other publications. This section will document the rhetorical life of this crucial curve.

<u>*Training the Viewer to See Symmetry: The Local Context of the Eltanin-19 Profile*</u>

Pitman and Hiertzler's 1966 article is only four pages long, but it contains five remarkable images. The first three images train the reader to identify symmetrical patterns that he or she might not otherwise see. Their fourth figure compares the Eltanin-19 profile from the Pacific-Antarctic Ridge to magnetic profiles from the North Atlantic. Their fifth graphic is comprised of diagrams similar to Tuzo Wilson's diagrams of transform faults (Figure 3.3). These diagrams explain away slight deviations in symmetry by offering multiple explanations for asymmetrical incongruities. Graphically maintaining symmetry is important because the ideal prediction of the Vine-Matthews-Morley hypothesis is the existence of identical patterns on either side of the ridge.

Figure 3.18 provides geographical and conceptual context for the argument. This map is calibrated to lines of latitude and longitude. Unbroken lines mark the *U.S.S. Eltanin*'s collection paths across the Pacific-Antarctic ridge. These collection runs produced the profiles articulated in Figure 3.19. However, Figure 3.18 does more than mark the survey area. The figure is already arguing that there are symmetrically spaced anomalies on either side of the ridge crest. These anomalies are marked with dashed lines; numbers (N) and prime-marked numbers (N') identify allegedly corresponding phenomena. These labels train viewers to recognize a phenomenon patterned on the logic of the rhetorical figure *chiasmus*.

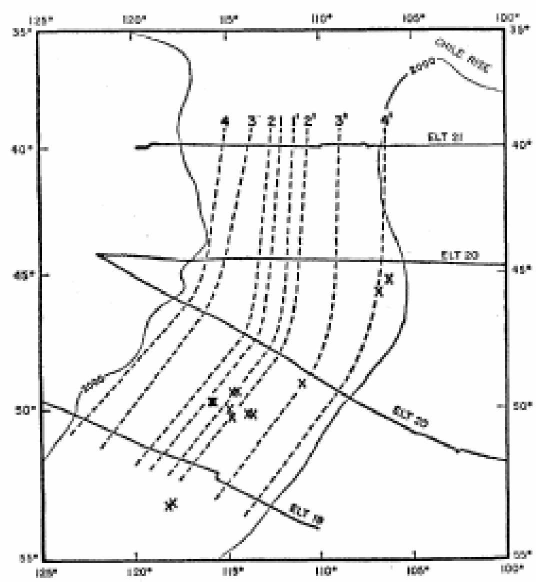


Fig. 1. Ship's tracks in the Pacific-Antarctic Ocean; bathymetric contours in fathoms. The numbers refer to specific magnetic anomalies, and the dashed lines connect similar anomalies. The crosses indicate positions of earthquake epicenters south of 40°S between 1 June 1954 and 31 May 1965 (*18*). *Elt, Eltanin.*

Figure 3.18: Pitman and Hiertzler, *Science*, 1966, Figure 1.

Chiasmus (XY:Y'X') is similar to the figure *antimetabole* (XY:YX), which also repeats terms in reversed order; however, with chiasmus similar but not identical constructs can be substituted for the original terms. For example, Kennedy's "Ask not what your county can do for you, but what you can do for your country" is an antimetabole, since the terms "your country" and "you" are repeated in reverse order.

In the following example of chiasmus, similar but not identical elements are in reversed conceptual order: Napoleon was defeated by a Russian winter and the snows of Leningrad destroyed Hitler.[28] As Fahnestock notes, across the history of rhetorical criticism, the study of antimetabole and chiasmus has focused on their textual manifestations, especially in Biblical passages and oratory; however, these figures possess argumentative power often developed in scientific contexts: "This pattern is clearly discoverable in nature. The image is found, but also constructed, by manipulation and selection, to approximate a familiar conceptual pattern" (*Rhetorical Figures* 137). Fahnestock shows that the patterns of these figures are often visually invoked, a rhetorical phenomenon that is occurring within the Eltanin graphics.

The numerical chiasmus of Figure 3.18 (4-3-2-1:1'-2'-3'-4') is reproduced by Pitman and Hiertzler in Figure 3.19, but the rhetorical presence of this chiasmus is visually complicated by the inclusion of magnetic and bathymetric data.

[28] Example from Fahnestock's *Rhetorical Figures in Science* (123).

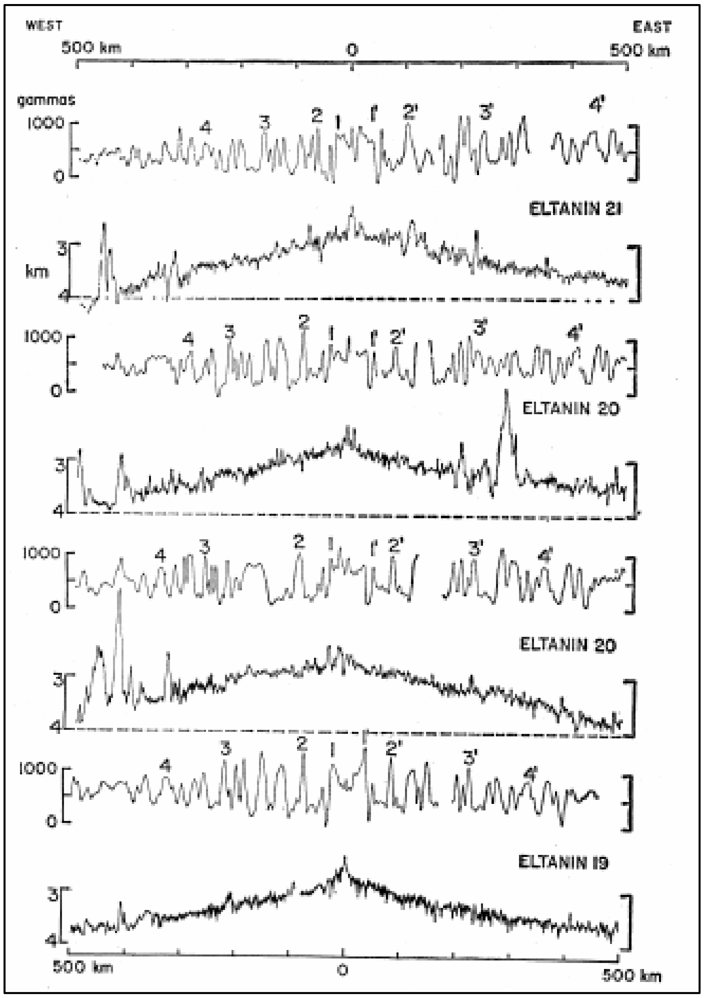


Figure 3.19: Pitman and Hiertzler, *Science*, 1966, Figure 2.

The chiasmus works in Figure 3.18 (at least provisionally) because nothing challenges the symmetry claim. The authors present equidistantly spaced lines, which they label with equidistantly spaced numbers and prime-marked numbers. Because the terms have clear (though simplified) symmetrical form, the viewer accepts (or at least tentatively accepts) the similarity of the juxtaposed phenomena. In Figure 3.19, however, the credibility of this chiastic reasoning is stretched. There are numbers in reversed order equally spaced along the profile, but the underlying symmetry is not as precise; some profiles seem asymmetrical at first glance. This is especially the case for the Eltanin 20 and Eltanin 21 profiles; it takes several seconds of scanning the image to determine which features of the contrapuntal terms are symmetrical. For example, on the bottom run of Eltanin 20, 3 and 3' are not symmetrical at first glance, since the 3 peak is taller and more compressed than 3' peak. Once the viewer recognizes that it is the relative horizontal (peak-to-peak) positions that are symmetrical, the assertion is clearer; however, it is not necessarily visually convincing on this graphic. Eltanin 19 is the most symmetrical of the four profiles, but when the profile is stripped of numerical markers (as I presented in Figure 3.17), this symmetry is also difficult to see. Pitman and Hiertzler use Figure 3.20 to amplify the symmetry of Eltanin 19. The amplification transforms the figural logic of the inscription from the logic of chiasmus to the logic of antimetabole—from comparisons claiming *similarity* to comparisons claiming *sameness*. The amplified Eltanin-19 profile, visually situated between distance and time scales, also encodes the transitivity argument at the heart of the Vine-Matthews-Morley hypothesis. The rhetorical complexities of this graphic warrant a closer rhetorical analysis.

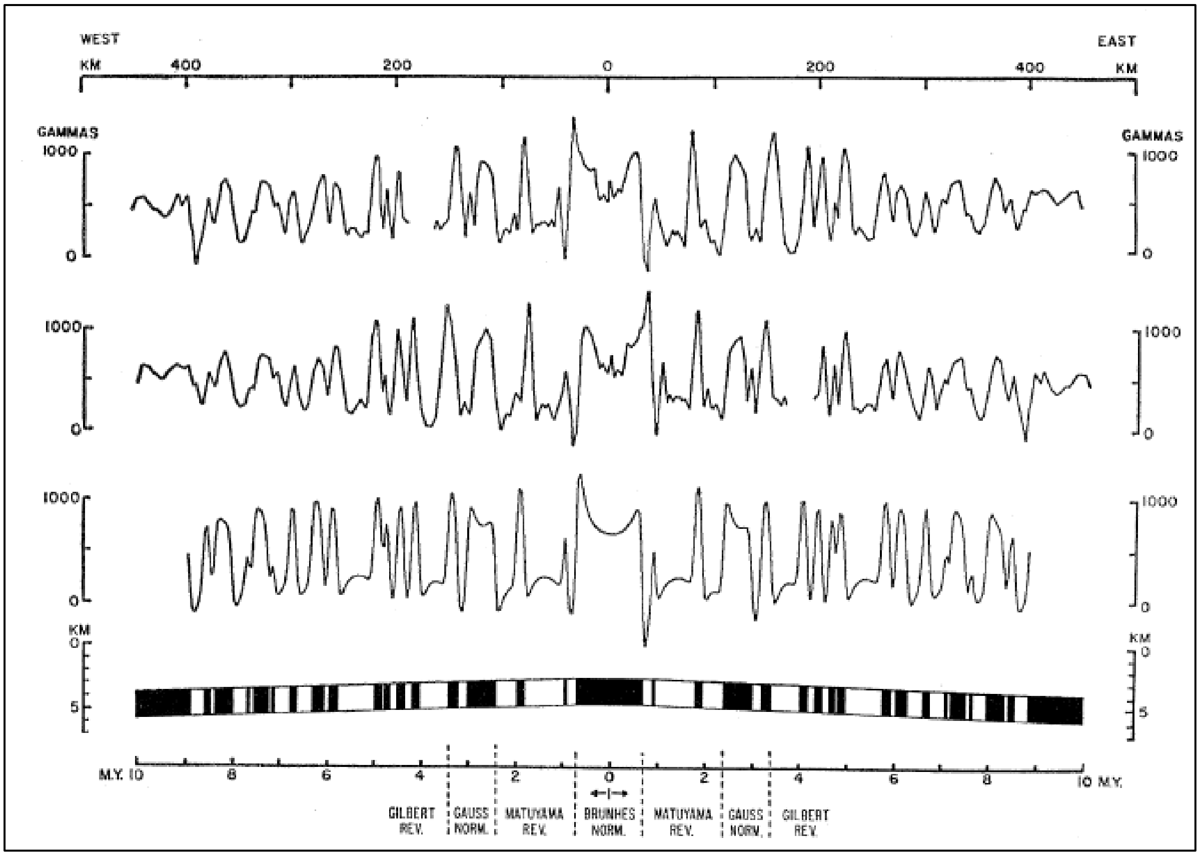


Figure 3.20: The amplified Eltanin-19 profile. Pitman and Hiertzler, *Science*, 1966, Figure 3.

As noted previously, it is difficult to see the symmetry of the unmediated profile (Figure 3.17). In Figure 3.20, several maneuvers amplify and emphasize the symmetry. First, the top curve is the reversal of the original inscription. This action is noted in the text and the caption, but it is not emphasized on the graphic itself; that is, the authors did not add a reversed scale to indicate that east and west are reversed for the top curve. This move changes the figural logic from chiasmus to antimetabole by asserting that elements on either side of the profile are not merely similar—they are the same. The X:X’ relationship is transformed into an X=X relationship. The bottom curve, a computed model, reinforces this visual argument. This curve smoothes out the bumps of the processed inscriptions, a tidying action that makes the symmetry more apparent. Elements on either side of this curve mirror each other in height, area, and configuration. Moreover, three instances of the "same" curve create a striking visual parallelism. The viewer's eye is almost forced to move along the vectors created by the aligned minima and maxima (the relative high and low points on the curves). These vectors also create implicit visual connections between the scale at the top of the diagram and the elements at the bottom.

Below the three iterations of the curve is a representation of the sea floor. Magnetic anomalies are mapped onto this thick line, and the relative minima and maxima of the curves are transferred to mark magnetic boundaries onto physical space. As was the case with the Raff and Mason map (Figure 3.5), invisible features of rock gain visual presence through contrasting tones. The caption text preserves this visual antithesis in verbal form: "Shaded areas are normally magnetized material; unshaded areas, reversely magnetized material" (1166). This strategy for visually segmenting the oceanic crust into distinct sections according to the points of a magnetic anomaly profile is not new. Vine and Wilson made similar maneuvers in a 1965 publication. However, Figure 3.20 is novel because these segments align with a reliable scale of known periods of magnetic reversals. In addition, the length of the Eltanin-19 profile allows the viewer to recognize an overwhelming number of correspondences between the marked crustal blocks and known (and previously unidentified) magnetic epochs. The "blocking" of segments indexed against time draws on the visual tradition of the geologic column diagram. Martin Rudwick documents the development of this tradition in "The Emergence of a Visual Language for Geological Science" (164-172). Moreover, as Kress and Van Leeuwen note in

Reading Images, timelines turn phenomena into a narrative of gradual unfolding stages (95-97). By making antithetical distinctions and indexing them against the timeline of known anomalies, Pitman and Hiertzler transform the Eltanin-19 data into an episodically segmented history of the spreading sea floor.

The persuasive force of this story relies on the transitivity argument encapsulated in the entire graphic. As mentioned previously, Perelman and Olbrechts-Tyteca explain that "the use of transitive relations is valuable in cases where it is a matter of ordering beings and events which cannot be directly compared with each other" (229). In the case of the developing Vine-Matthews-Morley hypothesis, it was impossible to relate distance and time directly (i.e., to determine a rate of motion) because the spreading of the sea floor was too slow and (in the 1960s) too deep to observe directly. Measurement of remnant magnetism provided the connecting variable; magnetism could be correlated with both distance and time. Magnetism and distance can be correlated through the magnetometer data; potassium-argon dating provided the timeline that allowed magnetism to be related to time.

Several features of the graphic enhance the expression of this transitivity argument. As noted previously, the aligned points of three iterations of the same curve create directional vectors that guide the viewer's eye. The authors encourage this process by not adding extraneous framing elements or scales that might disrupt the visual flow. In similar images, this graphical parsimony is not applied. For example in Figure 3.21 (Figure 3 of Vine and Wilson 1965), x-axis lines strike through each compared profile.

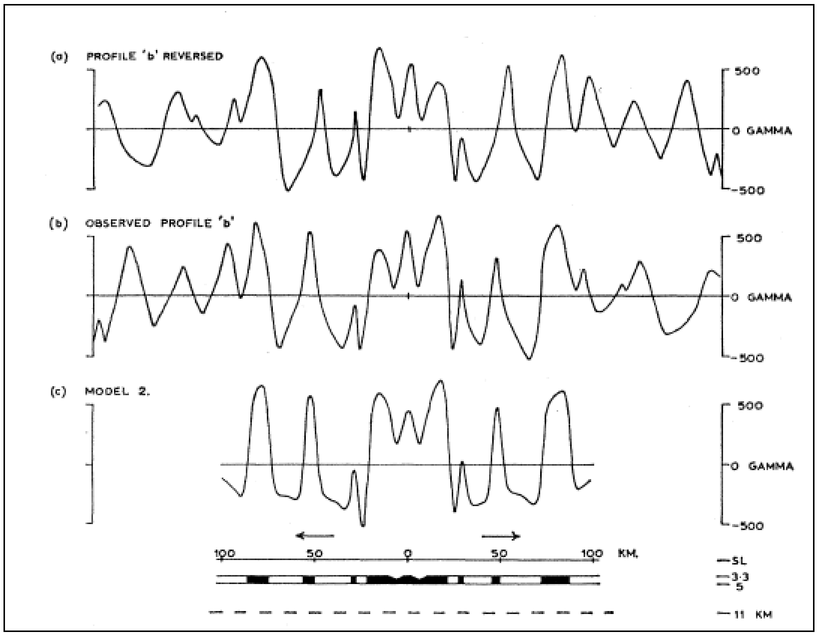


Figure 3.21: Vine and Wilson, *Science*, 1965, Figure 3.

Though I do not want to make too much of what Pitman and Hiertzler did not include in Figure 3.20, I contend that the addition of lines would have reduced the effectiveness of the transitivity argument. Figures 3.20a and 3.20b test this intuition by adding dividing lines and x-axes, respectively. By removing the reversed and ideal curves in Figure 3.20c, I test my intuition that the repetition of parallel curves is also important for presenting the transitivity argument.

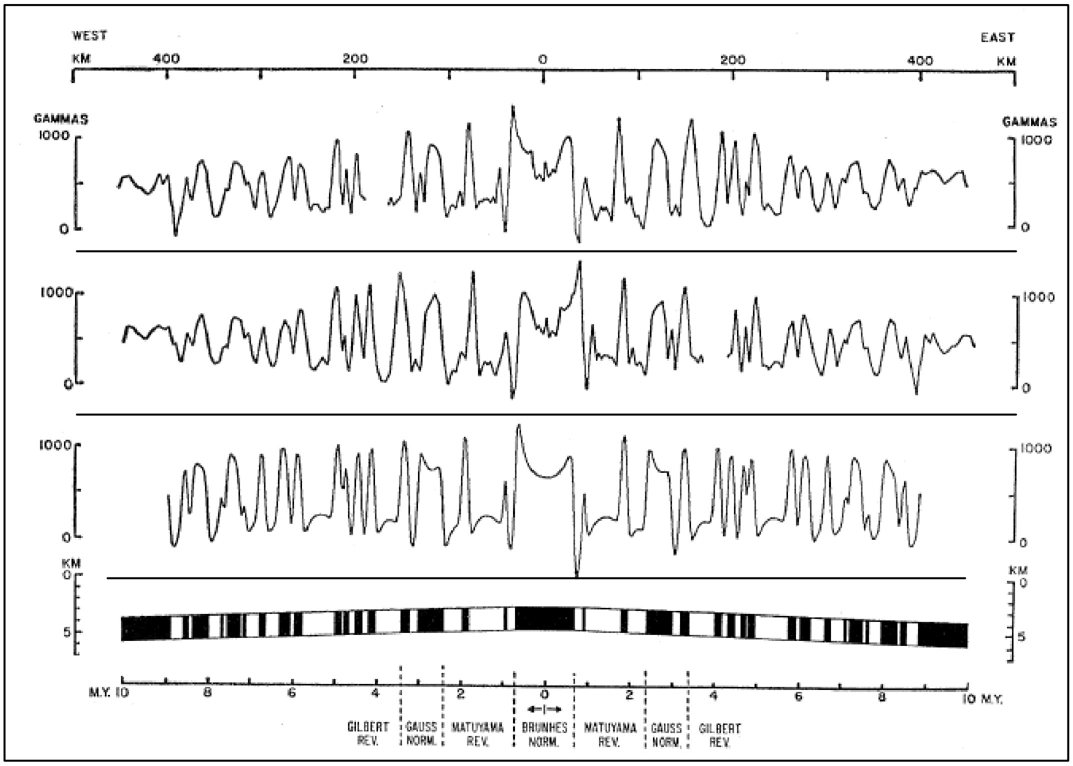


Figure 3.20a: Amplified Eltanin 19 with dividing lines.

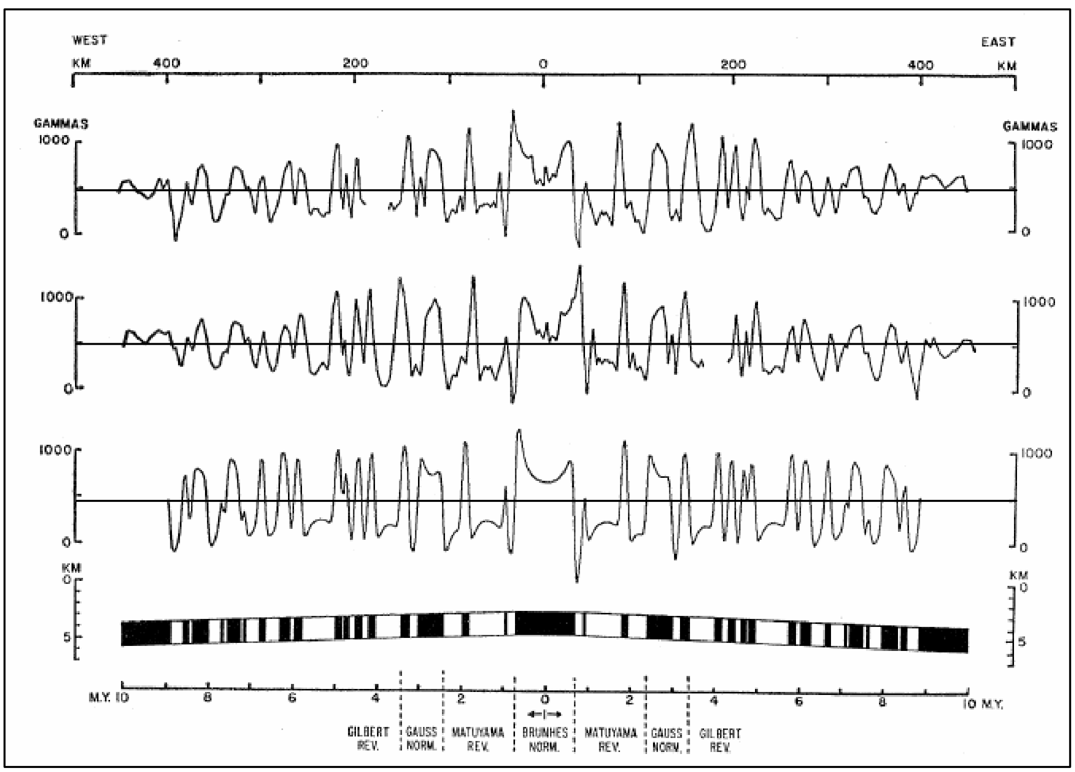


Figure 3.20b: Amplified Eltanin 19 with axis strikes.

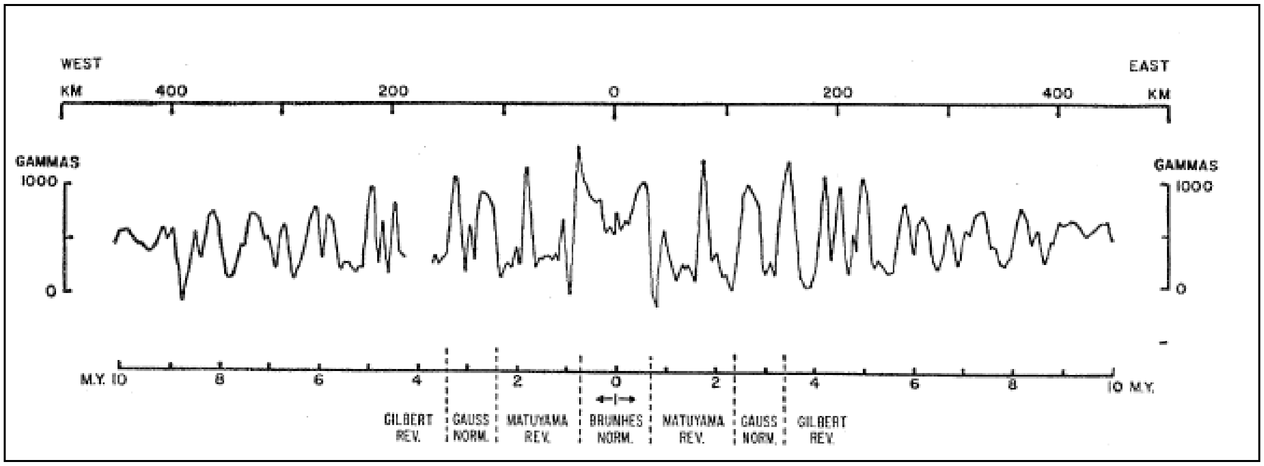


Figure 3.20c: Eltanin 19 without the sea-floor diagram, reversed curve, and model curve.

Without the additional curves and the black-and-white representation of the sea floor, the symmetry of the data is less clear, and the transitive relationship between distance, magnetism, and time lacks presence. As Figure 3.20c suggests, merely including the requisite elements of this transitivity series is less successful than the complete graphic as it was presented in Figure 3.20. Pitman and Hiertzler's visual argument was effective because it provided rhetorical amplification of the connections between magnetism, distance, and time.

The overall construction of the Eltanin-19 amplification provided a transitive pattern that the authors used to test other data from the Reykanjes ridge in the Atlantic Ocean (Figure 3.22).

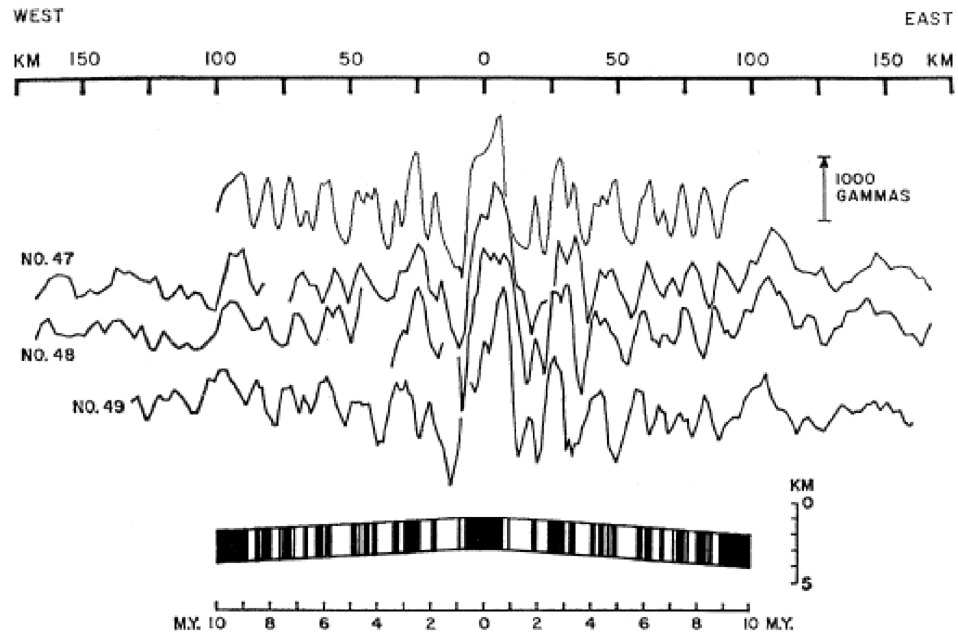


Fig. 4. Bottom shows the Pacific-Antarctic model applied to Reykjanes Ridge, with a time scale related to distance by a spreading rate of 1 cm/yr. The top profile is the one computed from the model. The bottom three profiles are from an aeromagnetic survey of Reykjanes Ridge (*6*); they are projected at right angles to the ridge axis.

Figure 3.22: Pitman and Hiertzler, *Science*, 1966, Figure 4.

Though the authors do not reverse any of the profiled data from the Reykanjes, they do use a highly symmetrical model curve, based on the Eltanin-19 model, to aid the reader in recognizing the symmetry of the Reykanjes data. The visual antimetabole is preserved in the seabed diagram situated between the curves and the time scale. Graphics in later articles will incorporate the figural logic of antimetabole to assimilate evidence from elsewhere in the world; these assimilations show that the Vine-Matthews-Morley hypothesis was a global phenomenon. A discussion of these rhetorical extensions of Eltanin 19 follows the next section, which describes its immediate reception.

Reception and Conversion

The effectiveness of the amplified Eltanin-19 graphic (Figure 3.20) can be measured in part by the reactions of other scientists. Some of William Glen's interviews document reactions to the graphic at the 1966 meeting of the American Geophysical Union, the first public display of the figure.[29] Allan Cox described the experience of seeing it as "truly extraordinary" (qtd. in Glen 337). According to Neil Opdyke, Dick Doell was "stunned" and later remarked, "It's so good it can't possibly be true, but it is" (Opdyke qtd. in Glen 339). Similarly, Xavier Le Pichon, a Lamont researcher who had been skeptical of sea-floor spreading but was not in attendance at the AGU meeting, recalls when he was "converted" by the Eltanin-19 graphic:

> Walter Pitman showed me the "magic" magnetic anomaly profile obtained over the South Pacific ridge crest, the Eltanin-19 profile that had been presented by Jim Hiertzler at the American Geophysical Union meeting in Washington D.C., on April 27. My wife still remembers that on my way back from the laboratory, I asked her to get me a drink and told her: "The conclusions of my thesis are wrong: Hess is right." ("My Conversion" 212)

Once the symmetry of the Eltanin-19 profile and its correspondence to the magnetic time scale were established, the curve itself and the mediation strategies used to articulate its symmetry would be used in other arguments that solidified the Vine-Matthews-Morley hypothesis.

[29] Though I do not have access to the display that was presented at the 1966 AGU meeting, descriptions of the graphic correspond exactly with the version that appeared in the December 2, 1966 article. For the complete description, see Glen (337).

In Support of Figural Logics: The Circulation and Extensions of Eltanin 19

In this section I test the validity of my claims regarding the symmetrical antimetaboletic logic of the Eltanin-19 profile by examining how the profile was incorporated into other visual arguments. As Fahnestock notes, "To the extent that a writer can preserve parallelism as well as repetition in the two cola, the antimetabole gains predictive power in the invitation it offers readers to participate in how the figure should be completed. The strength of this predictive power…comes across in the ease with which an antimetabole can sustain an ellipsis" (*Rhetorical Figures* 124). I discuss two examples where Eltanin 19 sustains visual ellipses.

Figure 3.23 is another graphic presented by Vine at the Goddard Conference. In this figure, the form of the Eltanin-19 profile is used to validate less symmetrical data from the northern Pacific.

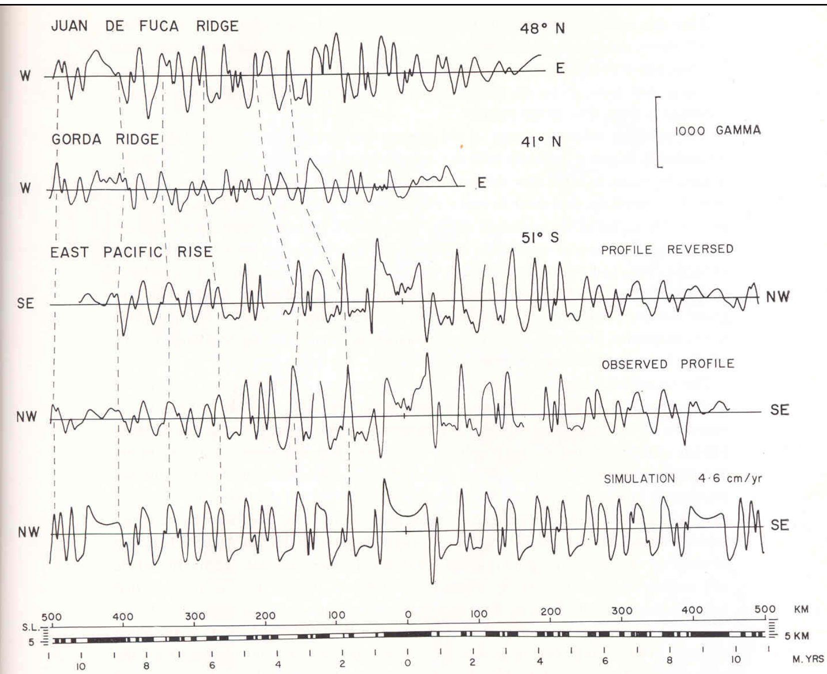


Figure 3. The *Eltanin* 19 profile across the East Pacific Rise (Pitman and Heirtzler, 1966) together with the profile reversed about its midpoint to demonstrate its symmetry, and a computed profile assuming the reversal time scale for the past 11 m.yrs. listed in Table 1, a total field intensity and dip of 48,700 gamma and —62.6° respectively, and a magnetic bearing of 102° for the profile. The profile is also compared with a composite profile across and to the northwest of the Juan de Fuca Ridge, and a profile normal to the strike of the anomalies across and to the west of the Gorda Ridge (Raff and Mason, 1961; Vacquier et al., 1961).

Figure 3.23: Vine, Goddard Conference, 1966, Figure 3.

The bottom three curves are adapted from Pitman and Hiertzler's Eltanin-19 graphic. The other curves are of profiles of ridges off the coast of Vancouver Island, which is the same region mapped by Raff and Mason in Figure 3.5. Though the Juan de Fuca and Gorda profiles are symmetrical around their centers, this symmetry breaks down toward the eastern edges that abut the North American continental mass. These shorter profiles do not cover enough submarine longitude to demonstrate complete bilateral symmetry; however, the comparison of these profiles to the Eltanin-19 profiles invites the viewer to infer the existence or previous existence of such symmetry in these other curves. Dotted lines identify similar terms across the left half of each of the profiled areas; the existence of similar terms on the right half can be accepted, even for short profiles—like the Gorda Ridge profile—that cannot present perfect extended symmetry.

A similar process is at work in Figure 3.24. This graphic is from a 1968 article by Pitman, Herron, and Hiertzler that appeared in *The Journal of Geophysical Research*. Eltanin 19 and its reversal are situated at the center of the graphic, and the symmetry of these profiles is extended to other profiles from throughout the Pacific Ocean. Also noteworthy, the figure is topped with a numerical antimetabole and not the numerical chiasmus Pitman and Hiertzler used in 1966.

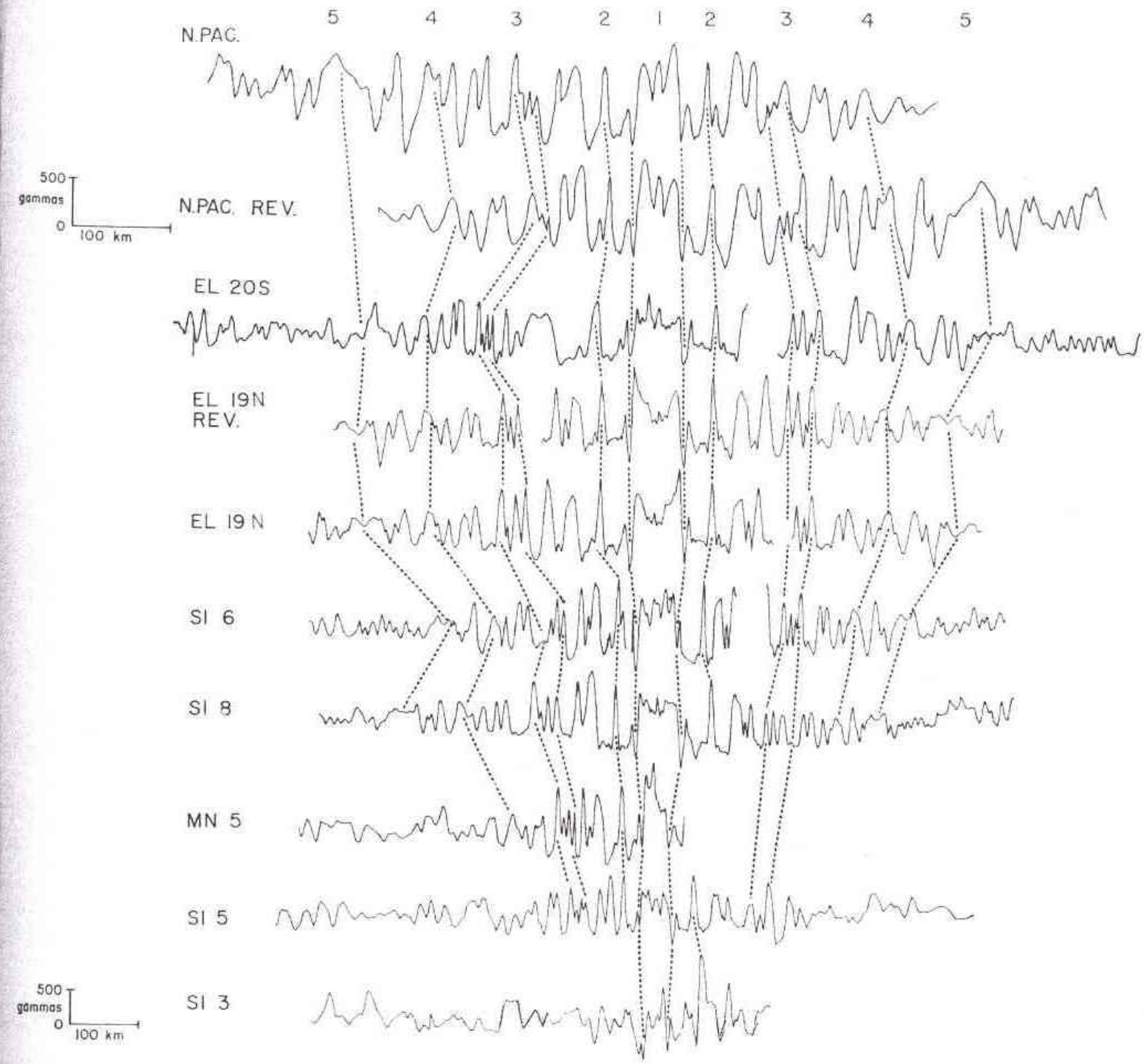


Fig. 4. Observed magnetic anomaly profiles from Figure 3 along tracks shown in Figure 1 and from the North Pacific (see text). The numbers refer to specific anomalies; the dashed lines show the correlation. The solid vertical lines outline to the ridge axis (anomaly number 1).

Figure 3.24: Pitman, Herron, and Hiertzler; *Journal of Geophysical Research*; 1968; Figure 4.

Of particular interest in Figure 3.24 is the presentation of the N. PAC. curve, an extended version of the Juan de Fuca profile. By presenting the observed and reversed iterations of this curve, the authors assert the existence of bilateral symmetry for N. PAC., even though the curve is not long enough to verify the symmetry of all the recorded data. Also interesting is the presence of the MN 5 and SI 3 profiles. These profiles only cover half of a ridge flank, but bilateral symmetry can still be inferred because of their placement within the series. Thus, this graphic demonstrates two ways to argue symmetry into a curve: compare a profile to part of a known standard or compare a curve to its reverse. In the case of N. PAC., both methods are used. Figures 3.24a, 3.24b, and 3.24c test if the presence of Eltanin 19 is essential for demonstrating symmetry. Figure 3.24d tests whether the N. PAC. curve and its reversal could also be used as symmetrical standards.

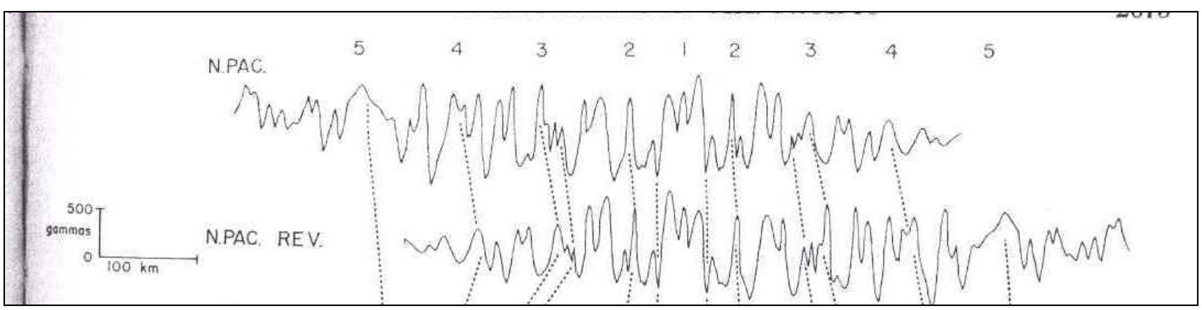


Figure 3.24a: Graphic with N. PAC. isolated.

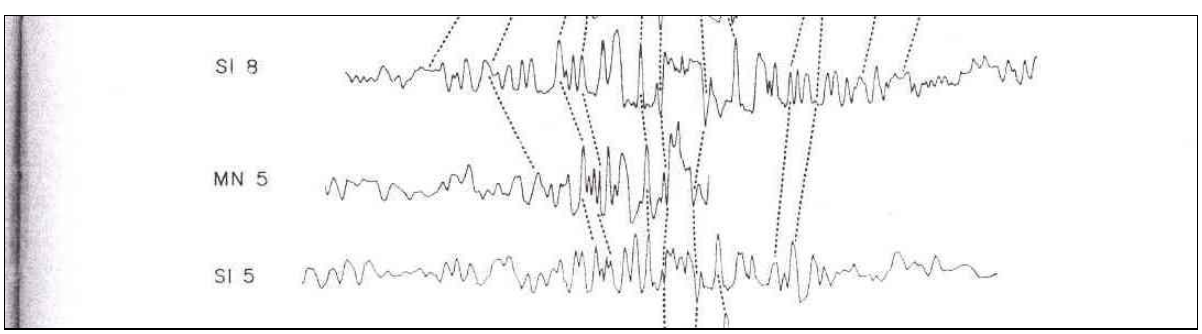


Figure 3.24b: Graphic with SI 8, MN 5, and SI 5 isolated.

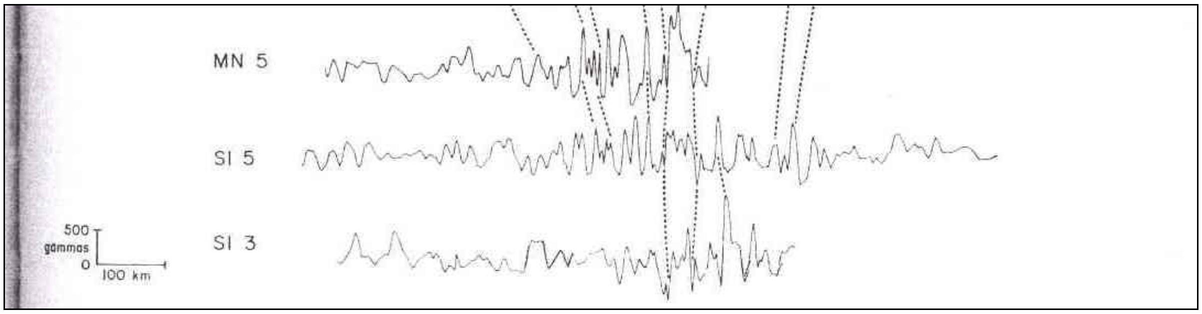


Figure 3.24c: Graphic with MN 5, SI 5, and SI 3 isolated.

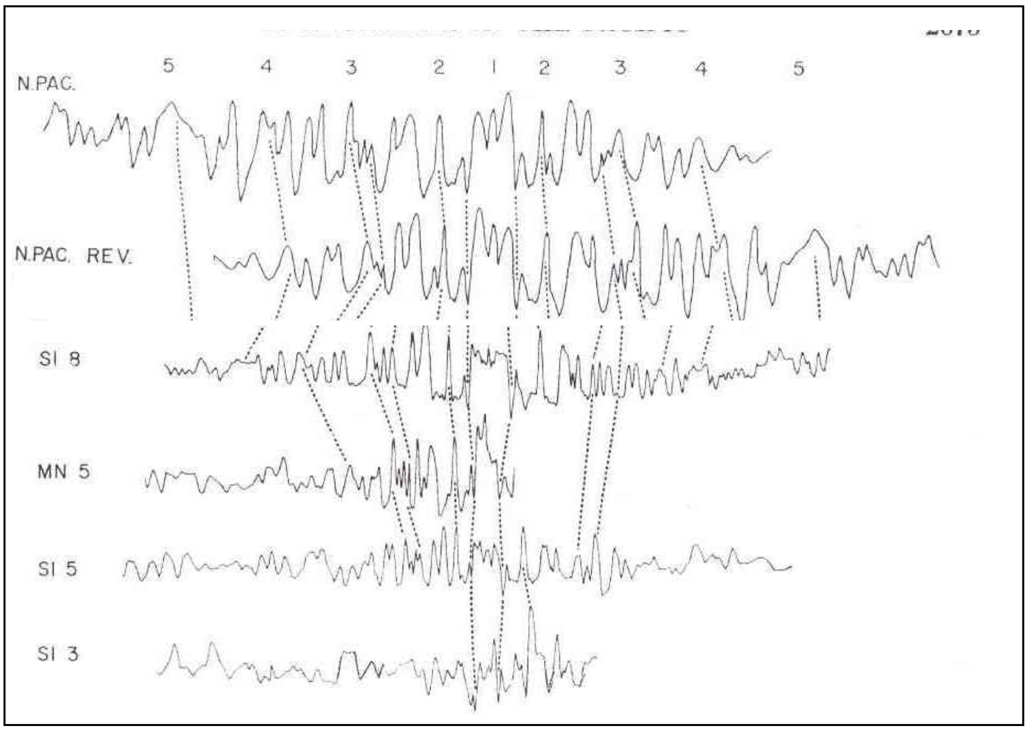


Figure 3.24d: Graphic with the N. PAC. profile spliced in for the Eltanin profile.

As its absence in Figures 3.24b and 3.24c indicates, Eltanin 19 is more significant for encouraging symmetrical readings of the half curves. Even without the Eltanin-19 standard, the N. PAC. profile (Figure 3.24a) looks symmetrical when compared to its mirror; however, as Figure 3.24d suggests, N. PAC. is less effective than Eltanin 19 in functioning as a symmetrical standard for visually organizing the set of curves. Again, I do not want to make large claims based on what did not happen; however, these graphical experiments suggest that wise graphical choices can affect the acceptability of less than ideal visual data.

Conclusion

In this chapter I have shown how geophysicists in the 1960s mediated the output of magnetometers to solve a scientific puzzle that had thwarted scientists for decades. I have also shown how this mediation is a rhetorically driven process. The visual arguments presented in the “zebra stripe” maps changed as scientists used different graphical elements to make specific features more or less salient. Similarly, the magnetic profiles of the Pacific-Antarctic Ridge were rhetorically mediated constructions. Computers may have produced the curves, but scientific rhetors manipulated figural logics and graphical traditions to amplify symmetry and to connect unrelated variables. The “zebra stripe” maps and the Eltanin-19 graphics demonstrate the persuasive power of data transformed into the standard visual patterns of rhetorical figures. These graphics argued motion into the sea floor by visually reorganizing known relationships between magnetism, topography, and time.

In the next chapter, I show how contemporary scientists at NASA used more sophisticated instruments and visualization practices to make explicit dissociative distinctions. These scientists also faced the challenge of converging spatial and temporal domains while tracking the motion and properties of clouds. They deployed unclear satellite data, unconventional data from robotic instruments, digitally enhanced photographs, and verbal rhetorical figures to argue new atmospheric phenomena into existence.

Chapter 4: Revealing the "Twilight Zone"—Verbal and Visual Arguments about Invisible Atmospheric Phenomena

Visualization is an aid to seeing relationships, and there are rhetorical situations which demand some kind of picturation. Many skilled expositors will follow an abstract proposition with some easy figure which lets us down to earth or enables us to get a bearing. There is some value then in the "incarnation" of concepts.

— Richard Weaver, "The Rhetoric of Social Science," *The Ethics of Rhetoric*

In 2006, a group of scientists working for NASA's Goddard Space Flight Center described a previously unquantified atmospheric phenomenon that could significantly change climate models. They hypothesized that invisible particles in areas of the sky around visible clouds reflected much more light from the sun than most researchers acknowledged. More specifically, they believed that data-processing schemes applied to reflectance data recorded by satellite instruments did not correctly account for the reflectivity of near-cloud atmospheric aerosols. Aerosols are suspended particles, such as dust, sea salt, volcanic ash, disorganized water vapor, and anthropogenic pollutants. Processing algorithms were trained to make distinctions between cloudy and clear atmosphere and to measure reflectance for clouds and aerosols separately, but because some aerosol measurements taken of the areas near clouds were considered unreliable, the algorithms that processed that data treated any area that was not obviously "cloudy" as "clear." The researchers at Goddard believed that the light reflected by these operationally "clear" areas should be measured and modeled differently to account for the additional reflectivity caused by aerosols near clouds. They named these near-cloud areas "twilight zones." These regions of invisible water vapor and solid aerosols could extend for tens of kilometers around visible clouds and constitute a significant portion of the atmosphere.

When these scientists articulated their *twilight zone* hypothesis, they supported their claims with data collected by several sophisticated instruments; however, their peers did not accept their arguments. Colleagues were skeptical; journals rejected the work. This chapter describes the means by which these scientists overcame significant rhetorical challenges to argue the *twilight zone* between clouds and aerosols into existence. Specifically, I examine a series of drafts of the *twilight zone* manuscript to demonstrate how verbal and visual components were modified to separate rhetorically the appearance of a clear divide between cloudy and cloud-free atmosphere from the reality of gradually diminishing reflectance phenomena.

Background: Satellites over the Sky – 1957 – 2006

On 4 October 1957, much of the world paused and listened to a stream of beeps chirping over a static-filled radio signal. Those beeps were produced by transmitters on the Soviet Union's Sputnik craft—the Earth's first artificial satellite. Sputnik's debut and the launches of the subsequent Sputnik II and Sputnik III are generally remembered for their social and political implications; the Sputnik satellites were the first substantial legs of the Space Race, and they were significant Cold War victories that accelerated aerospace development on both sides of the Iron Curtain. However, it is often forgotten that the Soviet Union's Sputnik project and the early American satellite projects, such as the Vanguard and Explorer programs, were missions of international scientific discovery. Officially, Sputnik was a contribution to the International Geophysical Year (IGY)—a period of global scientific cooperation that spanned from July of 1957 to December of 1958 (NASA Sputnik). At least on paper, Sputnik was a scientific satellite launched to support the IGY. Its launch may have marked the beginning of an escalating Space Race, but it also marked the beginning of a new era of scientific research based on streams of satellite data.

Satellite observations were first trained on celestial phenomena. For example, in 1958 the instruments of the Explorer satellites recorded the data that allowed an American team lead by James Van Allen to discover and map features of the magnetosphere—phenomena now called the Van Allen belts (NASA-NSSDC "Explorer 1"). Later satellites carried instruments that looked down at the Earth. In 1960, the National Oceanic and Atmospheric Administration (NOAA) launched the first of a series of new weather satellites. These Television Infrared Observation Satellites (TIROS) were used to map cloud cover. As NOAA documents indicate, the sky-high view revealed new phenomena:

> TIROS showed clouds banded and clustered in unexpected ways. Sightings from the surface had not prepared meteorologists for the interpretation of the cloud patterns that the view from an orbiting satellite would show. (NOAA)

Satellite observations provided novel perspectives on atmospheric phenomena and changed the way that atmospheric scientists worked; satellites also created new research challenges. Increasingly sophisticated remote-sensing instruments increased the need for computational resources to make sense of the data. But reliance on computers can and did lead to scientific conflicts.

In 1985, researchers with the British Antarctic Survey argued that ozone concentrations over Antarctica were dropping to lower than expected levels at specific times of the year (Farman, Gardiner, and Shanklin 207-10). These findings were based on data collected by ground-based instruments, but these data conflicted with satellite measurements. According to Sparling, "The authors had been somewhat hesitant about publishing, because Nimbus-7 satellite data had shown no such drop during the Antarctic spring." In the popular account of this case, the programs processing the satellite data "threw out" data points lower than a specific threshold because such levels were "impossibly" low.[30] Once the algorithms were adjusted, it was indeed clear that the seasonal reduction in ozone concentration was real and that lower spring levels could be identified as far back as 1976. Though the "ozone hole" case is not the primary focus of this chapter, it demonstrates the complexity and potential problems of modern satellite instrumentation and the tension between satellites, ground-based instruments, and the expectations of scientists arguing about atmospheric phenomena. Nor is it a unique case.

In 1995, multiple studies claimed that clouds absorbed more energy from the sun than contemporary atmospheric models recognized (Li et al). In the jargon of atmospheric science, clouds were "darker" than models acknowledged. The technical

[30] In conflicting accounts of the discovery of the "ozone hole," historians and scientists disagree over the characterization of events. Some claim that the algorithms processing satellite-measured ozone data were "throwing out" ozone data below a certain threshold because such low levels were impossible. Others claim that this is a fabricated myth, a nice story that is inconsistent with the real science as understood by NASA insiders (Sparling). Regardless, there was enough of a mismatch between what was known about the satellite data by the BAS team that they were cautious in offering potentially contradictory evidence.

details of what would be named the cloud absorption anomaly (CAA) are less significant to this chapter than the persuasive dynamics of the scientific situation. Specifically, some researchers suggested that models should be revised to account for CAA, others disagreed. Critiques of instrumentation were central to dismissing CAA proponents' main claims about discrepancies between models and the observed "realities" that were not in fact real (Li et al; Kerr). Ultimately, the CAA was not a productive concept for thinking about the atmosphere; however, the CAA case offers another example of the kinds of activities that are central to the practice of atmospheric science. Scientists use instruments to collect data that are used to challenge or support existing models.

Tensions between models and observations and satellite and ground-based measurements would be successfully negotiated by the Goddard researchers in 2006. In this case, ground-based instruments were used to validate satellite data that were deemed unreliable and hence ignored by some atmospheric scientists and in some atmospheric models. In other words, unlike the CAA supporters, the Goddard group was successful in arguing for the existence of a phenomenon defined by a new concept—the *twilight zone* between clouds and aerosols.

Challenging Assumptions: Revealing the Twilight Zone: 2006-2007

In 2006, Yoram Kaufman, an atmospheric scientist at NASA's Goddard Space Flight Center, and Ilan Koren, then a postdoctoral fellow at Goddard, were investigating some unusual data coming back from the Moderate-resolution Imaging Spectroradiometer (MODIS). MODIS is a satellite-mounted instrument that uses a spinning parabolic mirror and several sets of sensors to collect reflected radiation in both visible and invisible wavelengths. Figure 4.1 is an illustration of MODIS from the cover of its press brochure. The gold foil (partially removed in the cutaway view) protects the components of the device from extraneous solar radiation, so radiation reflected from the Earth that passes through the round-cornered rectangular aperture is the only light to interact with the sensors. (The large circular element on the front of the device is part of the cooling system.)

Figure 4.1: Modified from cover page of the MODIS promotional brochure. Source: NASA-Goddard Space Flight Center. ***MODIS: Moderate-Resolution Imaging Spectroradiometer*****.**

Essentially, MODIS is a super-sophisticated digital camera. Using algorithms to combine and compare wavelengths from the 36 different "bands," scientists can transform MODIS data into 44 different data products. Table 4.1 demonstrates the relationship between some of these bands and specific geographic, atmospheric, and oceanic features.

Primary Use	Band	Bandwidth[1]	Spectral Radiance[2]	Required SNR[3]
Land/Cloud Boundaries	1	620-670	21.8	128
	2	841-876	24.7	201
Land/Cloud Properties	3	459-479	35.3	243
	4	545-565	29.0	228
	5	1230-1250	5.4	74
	6	1628-1652	7.3	275
	7	2105-2155	1.0	110
Ocean color/ Phytoplankton/ Biogeochemistry	8	405-420	44.9	880
	9	438-448	41.9	838
	10	483-493	32.1	802
	11	526-536	27.9	754
	12	546-556	21.0	750
	13	662-672	9.5	910
	14	673-683	8.7	1087
	15	743-753	10.2	586
	16	862-877	6.2	516
Atmospheric Water Vapor	17	890-920	10.0	167
	18	931-941	3.6	57
	19	915-965	15.0	250

Table 4.1: The first nineteen MODIS data bands. Modified from MODIS brochure. Inside back cover. Source: NASA-Goddard Space Flight Center. *MODIS: Moderate-Resolution Imaging Spectroradiometer.*

The different data that MODIS simultaneously collects allow researchers to create visualizations of phenomena ranging from plankton concentrations (Figure 4.2) to pollution events and sand storms (Figure 4.3). The green bands in Figure 4.2 show areas of high plankton levels. The yellow regions in Figure 4.3 show aerosol events. In the upper map of Figure 4.3, the spot over the Atlantic Ocean is sand blowing off the Sahara Dessert; that sand will ultimately seed the beaches of the Caribbean. The brighter regions in the lower map are smoke events. The particularly large swaths of color over South America and Africa reveal the fine aerosols released by the burning of biomass.

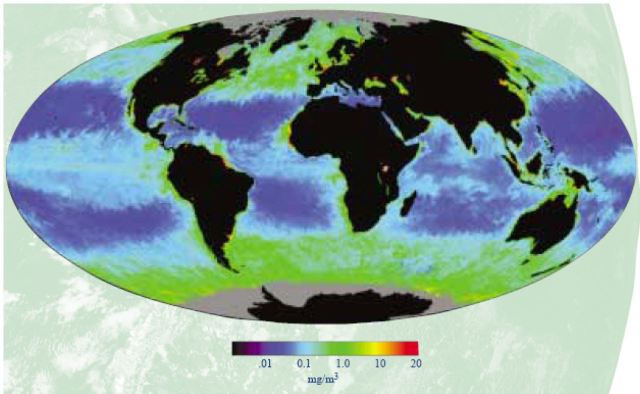


Figure 4.2: A visualization of ocean chlorophyll concentrations. Chlorophyll is an index of plankton. Source: NASA-Goddard Space Flight Center. *MODIS: Moderate-Resolution Imaging Spectroradiometer* (13).

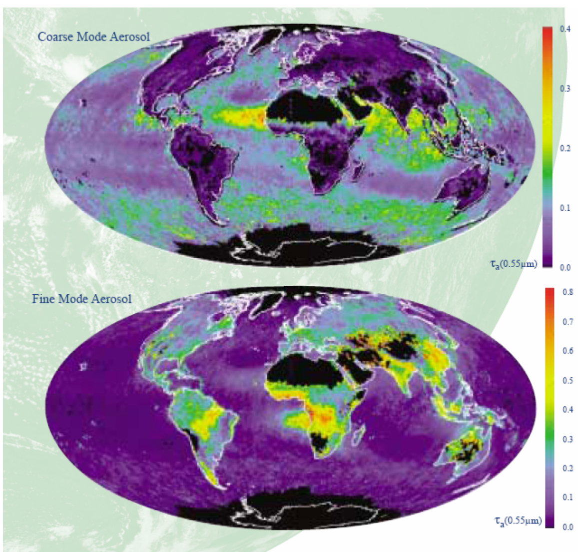


Figure 4.3: Visualizations of aerosol events. Source: NASA-Goddard Space Flight Center. *MODIS: Moderate-Resolution Imaging Spectroradiometer* (17).

The MODIS viewing field is enormous—it can image half of the continental United States in one orbital pass, and NASA's two MODIS-equipped satellites circle the Earth so frequently that the entire planet is scanned every two days. In contrast, the next best satellite only images a given area once every 16 days; a rate that is too infrequent to capture many rapid biological and meteorological changes—changes that can be recorded with MODIS. MODIS also allows scientists to track climate trends, such as deforestation and temperature change. In fact, studying climate change was the purpose for which MODIS was designed in the first place. It was part of the U.S. Global Change Research Program, a research initiative mandated by Congress in 1990 (NASA-GSFC "MODIS" 3).

Kaufman and Koren were examining MODIS cloud and aerosol data to better understand how these two elements of the atmosphere reflect light and hence contribute to global temperature and temperature change. As climate scientist Hans Graf noted in 2004, "[A]erosol-cloud interactions are seen as one of the most important single forces that drive climate change, but there are big uncertainties in the current understanding of these processes" (1309). Kaufman and Koren had been investigating these processes, and they were particularly interested in using MODIS aerosol data from regions around clouds to refine their understanding of cloud-aerosol interactions. For example, Koren and Kaufman (with co-authors Lorraine Remer and J. Vanderlai Martin) published a study in 2004 indicating that aerosols released from burning rainforests prevent the formation of specific kinds of clouds in Amazonia. This is just one example of the dozens of studies conducted by this atmospheric aerosol group at Goddard.

The inspiration for what Koren and Kaufman would later call "the *twilight zone* between clouds and aerosols" was not a sophisticated visualization. Rather, they believed that curves emerging from averaged MODIS data—like the one in Figure 4.4—indicated that atmospheric reflectance did not drop off precipitously at the edges of visible clouds. Instead, reflectance declined gradually. This contradicted current practices that applied “clearing” algorithms to make sharp distinctions between cloudy and clear atmosphere.

At the time, it was common practice for atmospheric scientists to ignore MODIS aerosol measurements of near-cloud areas because of the corruption caused by sunlight scattering out from the sides of the cloud. Many researchers considered MODIS aerosol data unreliable for an entire kilometer around the visible cloud. But neglecting this data affected models of atmospheric phenomena. Measurement schemes cut clear (but arbitrary and idiosyncratic) distinctions between cloudy and cloud-free areas despite a less than complete understanding of the region directly surrounding a cloud. Ignoring the non-visible cloud and aerosol particles in this region reduced the accuracy of temperature models because these particles also reflect light and affect global temperatures.

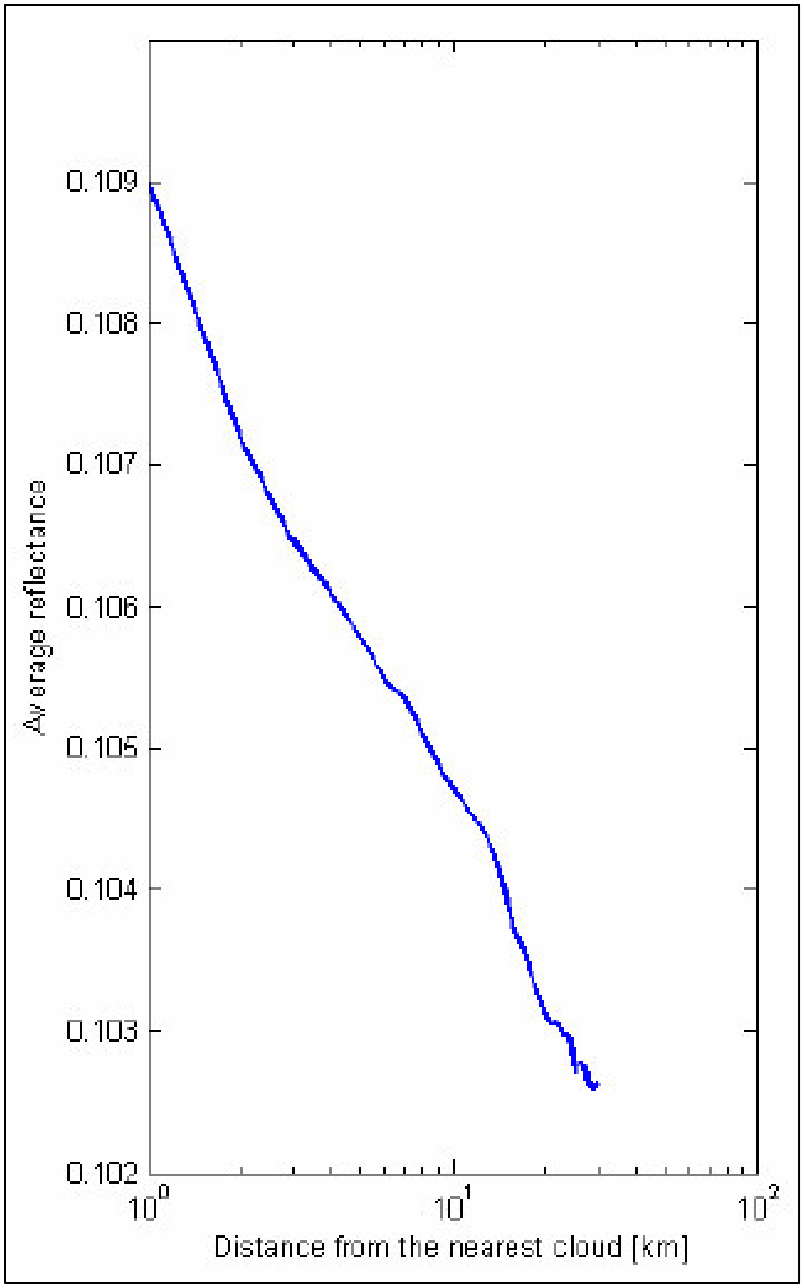


Figure 4.4: Atmospheric reflectance as a function of distance from the nearest cloud based on averaged MODIS data. Modified from Koren et al, Draft 12 of "On the Twilight Zone between Clouds and Aerosols." Unpublished manuscript, 2006.

Kaufman and Koren thought that a quantifiable aerosol phenomenon around clouds was represented in MODIS data within and beyond the 1 km threshold. Specifically, atmospheric reflectance did not drop off precipitously at the edges of clouds; it gradually declined, which suggested that the clear-cut distinctions between cloudy and clear atmosphere were not so clear-cut. Kaufman and Koren named this region in between the cloudy and the clear atmosphere the *twilight zone* between clouds and aerosols.

According to the *twilight zone* hypothesis, there is a region of invisible aerosol particles and disorganized water vapor extending for huge distances around visible clouds. The wetter and hence larger particles near clouds reflect more light than the drier particles further from clouds. The reflectivity of the *twilight zone* gradually decreases, though the "twilight zones" of several clouds can and often do overlap. According to the Goddard team's estimates, the *twilight zone* could affect 30% to 60% of the "clear" atmosphere, yet because these regions were not well defined or appropriately measured, they were not accounted for in contemporary atmospheric models. It was clear to Kaufman and Koren that the quantification of the twilight zone was important to climate studies and atmospheric science. However, they faced significant scientific and rhetorical challenges.

Kaufman and Koren knew that MODIS data—their primary evidence for the *twilight zone*—were not going to be persuasive because of the alleged data corruption caused by light scattering out through the sides of visible clouds. They began looking for other ways to corroborate the existence and extent of the "twilight zone," and they thought they had found the answer in AERONET—the Aerosol Robotic Network. AERONET is a global, ground-based array of robotic instruments that detect aerosols in the atmosphere. Each instrument in this network measures the aerosol content in the column of air between the instrument and the sun, a data collection technique described by one researcher as “a clean shot” (Remer).[31] Figure 4.5 is a picture of an AERONET installation; there are dozens of similar installations at sites around the world.

Figure 4.5: Picture of an AERONET sun-photometer installation. Source: “AERONET Mission Follow Up.” NASA Langley Research Center.

As the sun moves, sensors signal the robotic arm to track it, so the measuring instruments stay aligned with the solar disk. When the sun shines, a sun-photometer records precise measurements of aerosol particle size and aerosol optical depth—a measurement of the light passing through the column of airborne particles. Solid particles are larger when they are wet and smaller when they are drier, and some particles are larger than other particles. For example, airborne sand is larger than the solid particles in smoke; damp smoke particles are larger than dry smoke particles. AERONET data can be interpreted to distinguish these differences.

[31] I interviewed Dr. Lorraine Remer in her office on the Goddard campus on August 29, 2007. All quotations from Remer are from this interview.

If a cloud completely blocks out the sun, the photometer does not record data, which is why Koren and Kaufman were interested AERONET data. By locating the times in the data records when these instruments were inactive, they could infer the presence of a cloud and then zero in on the aerosol measurements recorded after the inferred cloud passed and the instrument resumed recording. Also, AERONET's design and observation position meant that the scattering effect that allegedly corrupted MODIS data did not cause similar corruption in AERONET data. The brightness of the solar disk overwhelms the scattering effect. Thus, AERONET seemed to be the instrument that could validate what Koren and Kaufman thought they saw in the MODIS data.

Interpreting AERONET data to suggest that aerosol density and particle size gradually decrease as clouds move away from the instrument, Koren and Kaufman believed they had the core arguments for what they thought was a publishable idea. They enlisted the aid of Lorraine Remer, also with the MODIS aerosol project, to help analyze the data and produce a publishable manuscript. Like Kaufman, Remer had worked with MODIS from the instrument's inception, and she was especially good at supporting ideas with clear, methodical written analysis (Remer).

Tragically, on the afternoon of 26 May 2006, hours after Remer met with her colleagues about the project for the first time, Yoram Kaufman was struck by a car while riding his bicycle through the Goddard campus. His injuries were fatal; the team was devastated. The *twilight zone* project was put on hold while Remer, Koren, and other members of the team mourned the loss of their friend and mentor.

It would be several months before Remer and Koren returned to work on the *twilight zone* paper. Koren accepted a permanent position at the Weizmann Institute in Israel, and Remer moved on to other projects at Goddard. But as Remer comments, the *twilight zone* paper was "too good" to neglect. Resuming work in July 2006, Remer and Koren revised Koren and Kaufman's original paper and circulated it amongst colleagues at Goddard. They were surprised to learn that what they thought was a clear scientific argument was not acceptable to many of their peers. Some colleagues at Goddard even told them to retract the paper after they heard that Koren and Remer had submitted a manuscript for publication.

Retraction was not necessary. The article was rejected by the journal *Nature* and then by the journal *Science* and then by *Geophysical Research Letters*. According to Remer, "Everyone was highly skeptical, [and they thought] that it was all artifice." A specific complaint was the paper's casual assumption that MODIS and AERONET observations were equivalent. MODIS measurements account for aerosol reflectance in spatial terms. AERONET measurements record aerosol properties as functions of time. The researchers were using time as a rough proxy for distance, which had not been done with AERONET before. Also, they were using AERONET data that was not normally used to make arguments. Other aerosol researchers ignored the "negative data" from dormant periods, but the Goddard team was interpreting these moments when the AERONET instruments did not collect data as an index of the presence of a cloud. This creative interpretation is logical considering the instrument's design, but AERONET data had not been used in this way before.

The novel aspects of the study were considered suspect by reviewers; this skepticism can be partially attributed to the way the authors presented their rationale for connecting MODIS and AERONET observations. Remer explained that in the earlier submitted version of the paper they "had made a lot of hand waving assumptions to translate time from the nearest cloud into distance from the nearest cloud and those are wild assumptions. People really objected." These objections are exemplified in the following comment from a reviewer for *Geophysical Research Letters:*

> There are also occasional examples of what I would characterize as incomplete science and or methodology (for example the simplistic discussion of near-cloud scattering effects on AERONET optical depths or the assumptions employed in using missing measurements in AERONET data as a means of crudely estimating distance to the nearest cloud). (*Geophysical Research Letters* Correspondence)

To overcome the doubts of their peers, Remer and Koren refined the description of their methodology and clarified their discussion of the data, but they also introduced new visual arguments to support their case. A revision was ultimately accepted and published by *Geophysical Research Letters*, but there are significant differences between this last version and its predecessor drafts.

This chapter traces the evolution of these drafts to investigate the rhetorical activities of these scientists. Specifically, it describes the development of dissociation arguments that transformed the *twilight zone* concept from a dubious "artifact" of a single instrument into a radical and accepted revision to atmospheric models essential to studies of climate change.

Before examining the verbal and visual arguments of this case, I first summarize the drafts that I analyze and the documents I use to support this analysis. I then review the concept of dissociation, as explained in *The New Rhetoric,* and I discuss some observations that rhetorical critics have made regarding the relationship between dissociation and the rhetoric of science.

Document Summary

The primary text analyzed in this chapter is the published version of "On the Twilight Zone between Clouds and Aerosols," which appeared in *Geophysics Research Letters* on 18 April 2007. This analysis also incorporates earlier drafts of the article, which were obtained from and are used with the permission of Dr. Lorraine Remer. The authors' original draft numbers have been retained. Draft 7 is the first version that Koren and Kaufman sent to Remer when she became a co-author on the paper; it is an incomplete draft. Draft 9 is a significant revision that also includes some electronically tracked comments. Draft 12 is the version that was submitted to *Science* and *Nature* and to *Geophysical Research Letters* the first time. While Drafts 7 and 9 were preliminary drafts not submitted for publication, they show how specific arguments were evolving as the authors revised the manuscript and prepared it for submission.

Supporting this analysis are the comments of Lorraine Remer; she was interviewed by the author on 29 August 2007. Other supporting documents include the comments from the reviewers for *Geophysical Research Letters* who rejected the initial submission to that journal.

I also examine several popularizations of the twilight zone story to track modifications to key images circulating into popular contexts.

<u>*Dissociation Arguments*</u>

As Perelman and Olbrechts-Tyteca explain, the process of dissociation represents a rhetor's attempt to revise an assumed structure of reality—a belief about a state of affairs held by an audience—by rhetorically transforming a unitary concept into two entities: one which retains the value associated with the original concept and one which is cast away as false, artificial, or mere appearance (411-415). Dissociations resolve conceptual incompatibilities and paradoxes, and they are often invoked in definition arguments through which the "real" meaning of a term is established. For example, Schiappa identifies how different rhetorical theorists have used dissociation techniques when arguing about the "real" definitions of rhetorical concepts, such as "argument fields" (75-76). Indeed, the term "rhetoric" itself has been the subject of dissociative discourse since ancient times. For example, the entire text of the *Phaedrus* clarifies "true" rhetoric from "false" rhetoric. More recent discussions about the "globalization" of rhetoric have also incorporated dissociation arguments. For example, one can read Gaonkar's critique of modern rhetoric's interpretive turn as a dissociation argument in which rhetoric's "essence"—its productive capacity—is separated from what he sees as the "non-essential" interpretive purposes to which rhetoric is now applied (25-85).

At the core of a rhetorical dissociation is the identification and juxtaposition of paired terms. In the generic formulation “term I” is that which is marked as artificial and purged; “term II” is the authentic remainder. Perelman and Olbrechts-Tyteca provide several examples of common term I/ term II bundles that they call “philosophical pairs”: appearance/ reality; artificial/ natural; letter/ spirit; opinion/ fact; name/thing etc.

Dissociations are marked by specific argumentative activities and stylistic moves. As argumentation theorist M.A. van Rees observes, dissociations are indicated by separation, negation, and/or revaluation of concepts. One of van Rees’s examples concerning jury sports (such as ice skating and gymnastics) exemplifies each of these operations:

> Jury sports must go back to the circus, ice show, or freak show. Everything is all right, as long as we are delivered from them during the real sports events. Sports are sports except jury sports, another word for unfair. Jury sports are sometimes quite nice to watch, but they shouldn’t be made into competitive games. (56)

In this passage sports are separated into jury sports and non-jury sports; jury sports are marked as not sports; “real” sports are given a higher value than jury sports.

In *The New Rhetoric* Perelman and Olbrechts-Tyteca identify some of the stylistic markers of dissociation. These include adjectival forms of the term I/term II labels (e.g., real, apparent, true, illusory, etc.), dismissive prefixes (pseudo-, quasi-, non, etc), and typographic indicators, such as capitalization changes (e.g., Being vs. being) and the use of dismissive quotation marks (436-442).

No critical work explicitly discusses the use of visuals in dissociation arguments. However, there are related concepts described by some visual discourse analysts that are useful to consider in the light of dissociation. Kress and van Leeuwen describe how ideal/real distinctions and given/new relationships are coded visually through top-bottom and left-right compositions respectively (181-202). These form-meaning relationships could potentially be used in dissociation arguments. Kress and van Leeuwen also describe how color saturation can signify modality, though this modality marking is context dependent (168-171). These linguists are using the term *modality* in the linguistic sense, the extent to which a set of signs is marked as real or unreal, possible or impossible. In written text, the modality of a claim can be marked with the modal auxiliaries: may, might, must, can, etc. Regarding context-specific visual modality, Kress and van Leeuwen note that in technical and scientific discourse, black-and-white images often have a higher modality, or "reality," than images that are naturally colored or super-saturated in color. In glossy magazine ads, supersaturated colors have the highest modality (170). (Recent preferences for super-saturated "false" color visualizations in some scientific imaging regimes might challenge the claim that monochromatic images have the highest modality in technical contexts, but even if such a shift occurs, it would not dismiss Kress and van Leeuwen's observation that specific color schemes connote a higher "truth value" than other schemes in specific contexts). Kress and van Leeuwen do not consider modality marking by color saturation in terms of dissociation, though modality shifts could be used to make a term I / term II distinction, if images of different modality levels are compared to each other to separate, negate, or revalue.

An analysis of key images from the *twilight zone* case reveals how both arrangement and color saturation participate in the process of separating the appearance of a divided sky from the reality of a graded system.

Dissociation and the Rhetoric of Science

The dissociative pair that is particularly interesting for the rhetoric of science is the appearance-reality dyad.[32] As Alan Gross notes, dissociations negotiating this dyad are the fundamental activities of science, and scientific dissociations are a unique class of dissociation arguments. Specifically, through science the "appearances that [lead] to diverse and contradictory speculation" are distinguished from a reality "that is the product of a privileged class of observations and statements derived from agreed-upon practices" (*Starring* 40). He further explains, "The persuasive effect of science becomes just its ability to move from term I to term II *as if* moving from appearance to reality" (40, emphasis his). Thus, science effaces the rhetorical nature of dissociation to enable the constant emergence of a more refined understanding of reality—an emergence that, once articulated, makes the new "term

[32] The appearance-reality pair is not the only pair that has been considered in light of scientific dissociations. Barnes (1982) explains that the accident/essence pair is commonly invoked. Schiappa (1985) discusses the theory/observation pair in relation to the philosophy of science, but he explains that this distinction has been rejected as the fundamental activity of science: "The distinction between observational and theoretical language has been rejected by many contemporary philosophers. It has proved quite difficult to establish that there is a clear basis by which to distinguish between observable and unobservable objects—a necessity if the distinction between languages is to be based on ontological grounds" (76). I mention Barnes and Schiappa here to recognize that dissociation has been considered in light of the operations of science, though I do not find these articulations as productive as Gross's commentary on the appearance/reality pair.

II" world seem self-evident and immediately acceptable. Gross's example of Crick's immediate acceptance of a rival's "correct" interpretation of a biochemical structure demonstrates this seamless dissociative activity (Gross 39-40). Such immediate acceptance would not take place in an argument about the authenticity of jury sports, which would not be supported by the argumentative machinery available to and expected of scientific discourse.

As compelling as Gross's description of dissociation in science is—and I do find it compelling—there are some aspects of it that might be extended through the close analysis of specific atypical situations. For example, what dissociation techniques are employed when a scientific argument rests on observations and statements that are not provisionally agreed upon? How do scientists invent, prepare, and revise arguments to accomplish a seamless dissociation under less than ideal conditions? How do images, especially rhetorically mediated images, participate in the dissociation process?

"On the Twilight Zone between Clouds and Aerosols" offers an opportunity to investigate such questions. This article explicitly invokes dissociative distinctions to argue that the real structure of the atmosphere is different from accepted models; however, the best evidence the authors had to articulate this new reality was, at first, less than acceptable. Koren and colleagues responded to these issues by invoking dissociative distinctions both visually and verbally throughout the text. The authors use rhetorical figures, refine figurative language, and deploy a range of visual objects to make the *twilight zone* real. The visuals—data graphics and photographs taken with a basic digital camera—served both evidential and demonstrative functions that were essential to their dissociative restructuring of reality.[33]

My analysis of the *twilight zone* case might also clarify some comments on dissociation and science made by Alan Gross and Ray Dearin in *Chaim Perelman.* In that book they remark, "It is not unusual for the dissociation of science to be plainly visible, even on the surface of its prose" (89). I acknowledge that the markers of dissociation *can* be plainly visible in scientific texts; however, the examples chosen by Gross and Dearin—passages from revolutionary texts by Darwin and Einstein—might not be the best support for the claim that explicit markers of dissociation are *typical*; i.e., "not unusual." While this observation may seem to be mere quibbling over the words "not unusual," I think there is an interesting and under-theorized point here. Specifically, a rhetorical dissociation can have lesser and greater degrees of salience depending on the terms the author used to mark it, and these markings will be more transparent in cases in which the persuasive stakes are higher, such as when the audience is likely to be resistant to the claim. As both the Gross and Dearin examples and the example of the twilight zone case demonstrate, explicit dissociative language in science is the product of *atypical* rhetorical situations. Though dissociation is always at work in scientific arguments, rhetorical dissociations are more overtly expressed when an argument challenges established scientific traditions or practices. During periods of normal science, dissociation is still taking place as the fundamental persuasive activity, but it is less explicit. When subtle dissociative activity is unsuccessful, rhetors take steps to make it more visible. This extension and

[33] Evidential images serve as records of specific traits or events. Demonstrative images operate at a higher level of abstraction; they present visual information that is generalizable for multiple situations and contexts.

clarification of Gross and Dearin's comments is supported by the examples of verbal and visual dissociative activities of Koren et al. became increasingly blatant as they revised their work in response to rejection and disbelief.

Analysis Summary

This chapter analyzes five facets of this rhetorical case. First, I describe how the reasoning patterns of specific rhetorical figures were used to create conceptual distinctions and empower a dissociation argument; an "apparent" antithesis is replaced by what is "actually" a gradual phenomenon. This replacement of one conception with another becomes verbally and visually clarified over successive drafts. Second, I show how definitions were negotiated to make the label of this concept—"the twilight zone"—palatable to all readers. Creating non-dissociative distinctions between definitions of the word "twilight" was important for clarifying the meaning of the label. Third, I explore the atypical scientific imaging practices that were essential arguments supporting the existence of the twilight zone. Fourth, I trace the circulation of specific *twilight zone* images in public spaces—such as NASA's Web site, news sites, and popular science blogs. Finally, I examine the composition and alteration of the Goddard team's data graphics and describe how these visuals participated in the making the case for the "twilight zone."

From Antithesis to Continuum: Grading the Sky, Revealing the Twilight Zone

In *Rhetorical Figures in Science*, Jeanne Fahnestock surveys the range of strategies that rhetors use to reconnect terms that have been pried apart by the rhetorical figure *antithesis* (86-95).[34] These strategies include bracketing (for example, Catholics and Protestants are bracketed as Christians), adding a flanking term (for example, the human-ape dichotomy becomes the graded human-ape-monkey triad), inserting a middle term (for instance, Huxley incorrectly filled in the inorganic-organic antithesis with the protoplasmic substance *bathybius*), and using more elaborate serial logics such as the figures *incrementum* and *gradatio*.[35] The shifts from binary distinctions to either a unitary or a graduated idea are common but effective patterns of conceptual change. Figure 4.6 demonstrates how various strategies for rejoining terms separated by antithetical reasoning—bracketing, flanking, bridging, serial figures—can be rendered visually with color. Shape, size, proximity, or other gradable visual variables might also be used to rhetorically (re)connect binary terms.

[34] Antithesis is the juxtaposition of contrasting ideas; for example, Lincoln's "It has been my experience that folks who have no vices have very few virtues."

[35] *Incrementum* is the arrangement of words or clauses in a sequence of increasing force; for example, "Marcus was a captain, prince, king and emperor." *Gradatio*, also know as *climax*, is a series of clauses that increase in significance; each clause repeats at its beginning the last word from the previous clause. For instance, the following example occurs in St. Paul's letter to the Romans: "But we glory also in tribulations, knowing that tribulation worketh patience; and patience trial; and trial hope; and hope confoundeth not, because the charity of God is poured forth in our hearts, by the Holy Ghost, who is given to us."

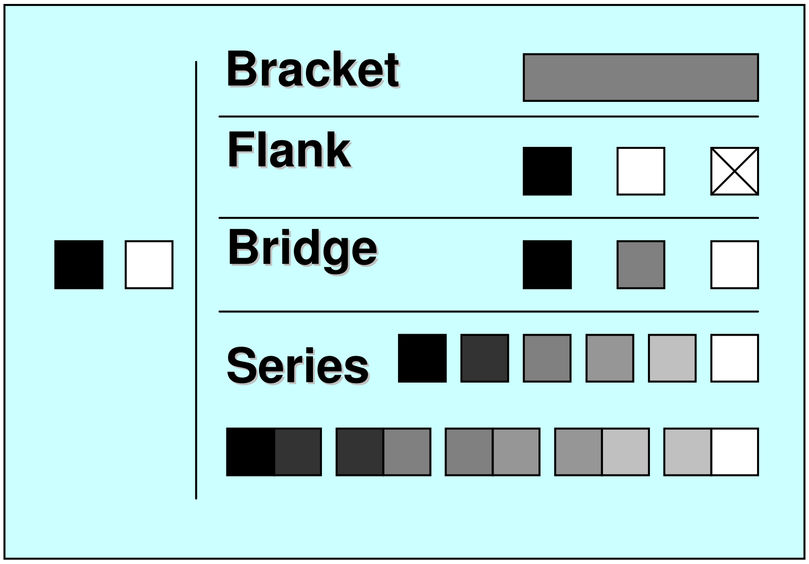


Figure 4.6: Visual analogs for the verbal strategies that can undo antitheses.

The rhetorical negotiation of antitheses is common in all argument fields, and as Fahnestock shows throughout *Rhetorical Figures in Science*, such maneuvers are widely used in scientific arguments. The rhetorical development of the *twilight zone* offers a new and rich case for exploring the negotiation of antithetical and serial logics in both verbal and visual modes.

The primary argument of Koren and colleagues is a definition argument that replaces a structure defined as an antithesis with a gradual phenomenon. The *twilight zone* functions as a term that bridges the cloudy and cloud-free, but it also resonates both verbally and visually with incremental logic. By its definition, the "twilight zone" is a graded phenomenon. Its gradual nature is articulated in the authors' primary scientific evidence, visual displays of MODIS (Figure 4.7) and AERONET (Figure 4.8) data that indicate graded patterns when aerosol variables (reflectance in

Figure 4.7 and density and particle size in Figure 4.8) are compared to distance or time, respectively.[36]

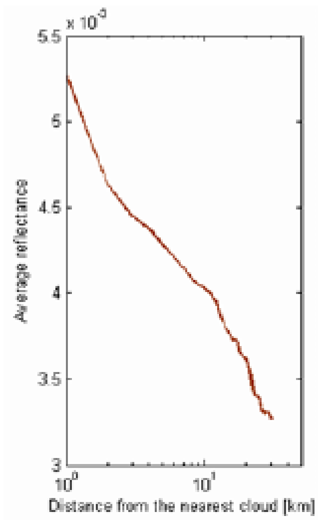


Figure 4.7: Visualized MODIS data From Figure 2 of Koren et al 2007.

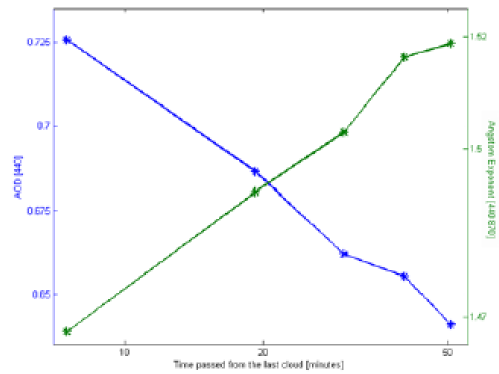


Figure 4.8: Visualized AERONET Data. From Figure 3 of Koren et al 2007.

These visuals are discussed at length in later sections. This section traces the verbal evolution of the authors' figural logic and shows how this logic contributes to the overall dissociative activity of the paper. That is, the appearance of antithetical distinctions between cloudy and cloud free atmosphere are replaced with an incremental reality—a gradually dissipating field of diminishing aerosol particles and unorganized cloud droplets. Rhetorical figures are negotiated in both the abstract and the introduction of the article; it is in these compressed spaces where the argument is most succinctly epitomized. A comparison of successive revisions shows how the

[36] As mentioned previously, early drafts displayed AERONET data in terms of distance, which was a point of contention for the reviewers. However, regardless of the x-axis domain, the graph serves as verification of gradual declination of aerosol properties.

negotiation of figural patterns becomes more refined over time to foreground the appearance/reality separation that the authors wished to assert.

Across revisions of the abstract, the shift from antithesis to graded series moves toward a version that best articulates the *twilight zone* as reality and dismisses absolute distinctions between “cloudy” and “cloud free” as artificial, binary, and instrumentally idiosyncratic. Table 4.2 includes the first four sentences of the abstract. Changes in the text are described sentence by sentence.

Draft	Sentence 1	Sentence 2	Sentence 3	Sentence 4
7	**The mutual interactions between clouds and aerosols <u>generate</u> a complex system** leading to significant uncertainties in understanding cloud's climatic effects in particular and future climatic changes, in general.	To simplify the understanding of this non-linear system **<u>we usually separate</u> <u>cloudy</u> and <u>cloud-free</u> areas and measure various clouds' and aerosols' properties separately**.	However, we find that clouds are surrounded by a "twilight zone" **– a "halo" (or region) composed of** a **mixture of forming and evaporating thin clouds and aerosols** extending kilometers **from the clouds into the area that is <u>usually considered</u> as "cloud-free zone."**	The optical depth (OD - a measure of the aerosol column concentration) of **<u>these twilight clouds</u>** is proportional to the concentration of smoke or pollution aerosols, suggesting that anthropogenic emissions may influence this zone.
9	Cloud and aerosols **affect each other and <u>form</u> a complex system** leading to high uncertainty in understanding fundamental processes in Earth's climate in general and climate change in particular.	To simplify this non-linear system **we usually <u>distinguish</u>** between **<u>"cloudy"</u> and <u>"cloud-free"</u>** areas and measure them separately.	However, we find that clouds are surrounded by a "twilight zone" – **a belt of forming and evaporating cloud fragments and hydrated aerosols** extending tens of kilometers from the clouds into the **<u>so-called</u> "cloud-free zone."**	The optical depth of these twilight clouds is proportional to the concentration of smoke or pollution aerosols, suggesting the **<u>twilight zone is an *aerosol* phenomenon</u>** that takes place in the vicinity of clouds.
12	Cloud and aerosols affect each other and form a complex system leading to high uncertainty in understanding climate change.	To simplify this non-linear system we usually distinguish between "cloudy" and "cloud-free" areas and measure them separately.	However, we find that clouds are surrounded by a "twilight zone" **–** a belt of forming and evaporating cloud fragments and hydrated aerosols extending tens of kilometers from the clouds into the so-called "cloud-free zone."	The optical depth of **the twilight zone** is proportional to the concentration of smoke or pollution, suggesting an anthropogenic contribution.
Final	Cloud and aerosols **<u>interact</u> and form a complex system** leading to high uncertainty in understanding climate change.	To simplify this non-linear system **<u>it is customary</u> to distinguish** between ''cloudy'' and ''cloud-free'' areas and measure them separately.	However, we find that clouds are surrounded by a ''twilight zone'' – a belt of forming and evaporating cloud fragments and hydrated aerosols extending tens of kilometers from the clouds into the **so-called cloud-free zone.**	**The <u>gradual transition from cloudy to dry atmosphere</u> is proportional to the aerosol loading**, suggesting an additional aerosol effect on the composition and radiation fluxes of the atmosphere.

Table 4.2: The first four sentences of the abstracts of four drafts of the "twilight zone" paper. My emphasis.

Over successive drafts, diction shifts clarify the actions and interactions of clouds and aerosols. In Draft 7, mutual interactions between clouds and aerosols are "generating a complex system." In Draft 9, clouds and aerosols "affect each other and form a complex system." By the final version, they "interact and form a complex system." The diction differences are slight, but not insignificant. In the earliest version, the actions of clouds and aerosols create a system; by Draft 9 and through to the final draft they are the components of the system; they are part of a whole rather than generating agents.

In the second sentence of each draft, the authors establish the antithetical distinction between cloudy and cloud free, which they later challenge. Several interesting changes occur across revisions that indicate the authors want to affect a dissociative distinction. The introduction of qualifying quotation marks to the terms "cloudy" and "cloud-free" in Draft 9 emphasizes the arbitrariness of the binary distinction. Moreover, "usually separate" is replaced by "it is customary to distinguish" in the final draft. "Usually" just connotes typical, but "customary" can connote typical by means of tradition, which is not necessarily an acceptable warrant in scientific discourse. As Perelman notes, the word "customary" can be invoked as an effective term-I marker in situations where the customary and the novel are marked as false and true, respectively (*The Realm of Rhetoric* 136).[37] The separation of the current work from traditional practice is further exemplified in the change from the collective "we" in "we usually separate" (Draft 7 and 9) and "we usually distinguish" (Draft 12) to the empty subject of "it is customary" in the final version.

[37] Perelman notes that "all the definitions which Charles Leslie Stevenson, in a thought-provoking article, called "persuasive"—and which contrast a novel, "true" meaning to a customary but specious one—set up a dissociation of a refined idea" (*The Realm of Rhetoric* 136).

In sentence three of each draft, the authors explicitly introduce a "fuzzy middle" to bridge the antithesis. The authors explain that their *twilight zone* is situated between the cloudy and cloud-free terms, thereby arguing that the atmosphere around clouds is graded with respect to humidity and aerosol concentrations. Changes in the description of the twilight zone over successive drafts reflect the development of a more precise figural logic—a more nuanced bridging of the cloudy/cloud free dichotomy. In Draft 7, the authors describe a "mixture of forming and evaporating thin clouds and aerosols." By Draft 9, there are no longer two things being mixed, but rather a continuous entity, "a belt of forming and evaporating clouds and hydrated aerosols." The belt serves as a bracketing term.

The addition of the word "hydrated" is also significant; it acts as a descriptive counterpart to the word "evaporating," emphasizing the bidirectional process of this graded phenomenon. As formations of water vapor, clouds are inherently wet. Aerosols can be completely dry. For example, windstorms blow huge quantities of dry sand from the Sahara desert to the beaches of the Caribbean. As the authors articulate it, the twilight zone is a liminal space where drying clouds and moist aerosols coexist proportionally. Articulating the presence of this space changes the structure of reality by replacing a stark "cut" distinction with this complex intermediate space. The dissociative dynamics of the relationship are outlined in Diagram 4.1.

Appearance - Binary

Cloudy (Wet)	Cloud free (Dry)
Clouds	Aerosols

Reality - Continuum

Cloudy	Twilight zone	Cloud free
Clouds	Forming clouds Evaporating clouds Hydrated aerosols	Aerosols

Diagram 4.1: Appearance/reality distinctions in the *twilight zone* article.

Significant textual markers of the evolving dissociation include the change from "usually considered as 'cloud free zone'" to "'so called' cloud free zone" in the third sentence. Like the shift from "usually" to "customary" in the second sentence, this change emphasizes the arbitrariness of the antithetical distinctions. The qualifying quotation marks disappear in the last version, but the term is still marked as an artificial construct by the "so-called" modifier.

In the fourth sentence of each version, the authors further define the *twilight zone* concept. A comparison of how the term is described demonstrates how the concept changed with revision to generate a more effective dissociative distinction. In Draft 7, the *twilight zone* is paraphrased as "these twilight clouds," a phrase which suggests that the zone is more cloud than aerosol or that the zone is part of the cloud. In Draft 9, the *twilight zone* is described as an "aerosol phenomenon that takes place in the vicinity of clouds." This definition implies it is more aerosol than cloud. In Draft 12 of the unpublished manuscript, the term *twilight zone* is repeated in the fourth sentence, though the qualifying quotation marks disappear, suggesting that the authors want to promote the "reality" of the concept; the zone itself is not redefined, but the authors emphasize a potential anthropogenic contribution. By the final iteration, the "twilight zone" is simply paraphrased as a "gradual transition from cloudy to dry atmosphere."

The bridging of the cloudy/cloud-free dichotomy is stated in more technical language in the body of the paper. It is best exemplified by the following passage from the introduction of the final draft:

> Clouds are defined as clusters of condensed droplets formed when aerosol particles are activated to droplets in a super saturated environment. However it is not clear what is the minimal amount of condensed water that could be considered a cloud. Discriminating between aerosols and clouds and the demarcation of cloud boundaries have been exceedingly difficult independent of the measurement system employed. We suggest that the shift between clouds to cloud-free atmosphere contains an additional component, a ''Twilight Zone'' or a gradual transition zone that depends on both the presence of nearby clouds and on the aerosol loading. (Koren et al. 1)

By questioning the "absolute" threshold between cloudy and cloud free, the authors open a space for their twilight zone. Interestingly, the final version is more subtle in its evaluation of the practice of making clear cuts between cloudy and cloud-free than previous versions. For example, the same passage from Draft 12 (the submitted and rejected manuscript) is a more blunt criticism of current practice:

> Commonly, cloud identification is forced to be binary: either there is a cloud or there is not. This decision can be made by an automatic algorithm that applies an absolute threshold to an observed parameter such as droplet concentrations measured *in situ* or reflectance measured by satellite. However, developers of such algorithms are often hard pressed to define a single threshold, because the identification is seldom truly binary. For example, in the MODerate resolution Imaging Spectroradiometer (MODIS) aerosol procedure, clouds are determined from a threshold based on variability of observed reflectances, because there appears to be a clear separation between cloud and cloud-free scenes in this variable (Martins et al., 2002). Even so, closer examination shows a range of reflectance variability between distinctly identified clouds and distinctly identified non-cloudy scenes. (Draft 12, 1-2)

Communicated in both versions is the notion that current practices are arbitrarily and incorrectly bifurcating what is actually a scalar phenomenon. The conceptual shift from antithesis to series is part of a scheme of distinctions contributing to the overall dissociation argument. These distinctions are summarized in Diagram 4.2.

Appearance	Cloudy	Cloud Free	Antithesis	Customary
Reality	Cloudy—Twilight Zone—Cloud Free		Series	New/Correct

Diagram 4.2: Scheme of dissociative distinctions.

From the abstract and introduction, it is clear that the authors are working with and against figural logics to elicit a dissociative definition. But their articulation of this phenomenon faced two rhetorical hurdles: (1) a name with multiple technical and popular resonances and (2) what some considered dubious evidence based on novel methods. Some clever definition work solved the first problem; clever imaging solved the second.

The name *twilight zone* was, according to Remer, the label for the phenomenon from the very beginning. However, many people, including Remer herself, were initially uncomfortable with Kaufman and Koren's term. While *twilight zone* is an evocative term, it has multiple meanings, some of which carry problematic cultural significance. It is, after all, the same name as a science-fiction television series and several classic rock songs from the 1970s and 1980s. Each of these "twilight zones" connotes a separation from reality, rather than a graduated phenomenon. A scan through the *Oxford English Dictionary* reveals even more definitions of the term. It can connote aging, imperfect perception, earlier times, and degradation, among other meanings that would infuse inappropriate connotations for Koren's phenomenon (*OED*). Moreover, "twilight" has a very specific definition in meteorological science: "the light from the sky between full night and sunrise or between sunset and full night produced by diffusion of sunlight through the atmosphere and its dust" (*American Heritage*).

Ilan Koren named the *twilight zone* with Yoram Kaufman during the project's early stages, and he insisted that the name stick. Obliging her colleague, Remer proceeded to make the term work by making some conceptual distinctions within the text. Specifically, she needed to distinguish the team's understanding of "twilight" as an intermediate state from other technical and popular understandings of twilight. To make this distinction, Remer tapped a source that is not usually cited in a scientific paper: *Merriam-Webster's Collegiate Dictionary*. Webster's second definition of twilight is "an intermediate state that is not clearly defined." This was precisely the connotation they wanted to express; thus, the dictionary entered the paper's bibliography.

While the citation of the dictionary to clarify the meaning of twilight is interesting, it is not a dissociative activity. As argumentation theorist M.A. van Rees notes, "Through dissociation a number of aspects is [sic] placed *outside* a given domain, while through a non-dissociative distinction they are kept *within* a given domain" (54, van Rees emphasis). In this case, an operational definition of *twilight* is established but it does not supersede all other definitions of *twilight*. Still, it was an important distinction to make, since the rhetorical creation of the phenomenon requires that readers understand it as an intermediate state. The dissociation work is accomplished verbally through the management of figural logic (as shown previously) and visually through an array of graphics and images.

Imag(in)ing the Twilight Zone

For the authors, images were essential tools used to produce a dissociative distinction between an artificially separated atmosphere and a gradual reality. Images were also important elements of popular accounts of the twilight zone story. This section examines the imaging practices of the authors and describes how their images were changed by other authors for different audiences. First, I look at the major visual addition between Drafts 12 and publication—a sequence of photographs taken with a basic digital camera and modified with over-the-counter software. I then examine how these photographs were changed when they circulated to non-specialist audiences. The chapter concludes with a discussion of the other visuals of the twilight zone article—a series of more technical visualizations and data graphics. Though these other graphics are largely the same in both the initially submitted manuscript and the final accepted one, there are rhetorically motivated differences that warrant further explication.

The Little Cloud that Could: The Rhetorical Power of Demonstrative Images

Disappointed but undaunted by the initial rejection of their manuscript, Koren and Remer proceeded to rewrite the paper. Remer explains that Koren was especially frustrated that others did not recognize the data patterns that were so clear and significant to him. During our interview, Remer paraphrased a statement from Koren:

> People aren't seeing the patterns that I see in this MODIS imagery or the AERONET data, but it's right there, and I can see it every day when the sun goes behind the clouds. (Koren qtd. by Remer)

Koren decided to reproduce his perceptual experience through a visual artifact. One afternoon, he walked out of his office and took a picture of a cloud with a simple digital camera. Using basic photo-adjusting software, Koren subtracted out the background reflectance. In other words, he took the blue out of the photographed sky. He then blotted out the obvious cloud pixels (i.e., the bright white pixels within the cloud's edges) and extended the dynamic range to draw out elements reflecting less light than the formerly bright cloud. Each step of this digital transformation was presented as a photograph in the final paper (Figure 4.9).

This set of pictures was displayed on a poster at the winter meeting of the American Geophysical Union and on slides for other conference presentations. It was also included in the revised manuscript submitted to *Geophysical Research Letters* in January 2007. While the addition of these pictures is not the only change between Draft 12 and the final document, Remer's comments on the reactions of others to the image set indicate that it was a significant contribution to the argument. In the following passage, Remer comments on the reception of the photographs at conference talks:

> People saw that picture and they finally understood what we were talking about— that yes there is something around this cloud that we can see. It's not a MODIS artifact, and that it's not something weird we're doing with AERONET, but there's something real. And we finally managed to convince them.

This image is rhetorically interesting because of its participation in the rhetorical dissociation process. As Remer points out, it was the picture that convinced people that the twilight zone was real. This reaction is interesting to consider in the light of Richard Weaver's observation on the rhetorical role of visualization:

> Visualization is an aid to seeing relationships, and there are rhetorical situations which demand some kind of picturation. Many skilled expositors will follow an abstract proposition with some easy figure which lets us down to earth or enables us to get a bearing. There is some value then in the "incarnation" of concepts. (203)

In this case, the cloud photo series grounds the abstract concept of the twilight zone with a picturation of the twilight zone surrounding a specific cloud. A closer reading of the graphic can illuminate why it was so effective.

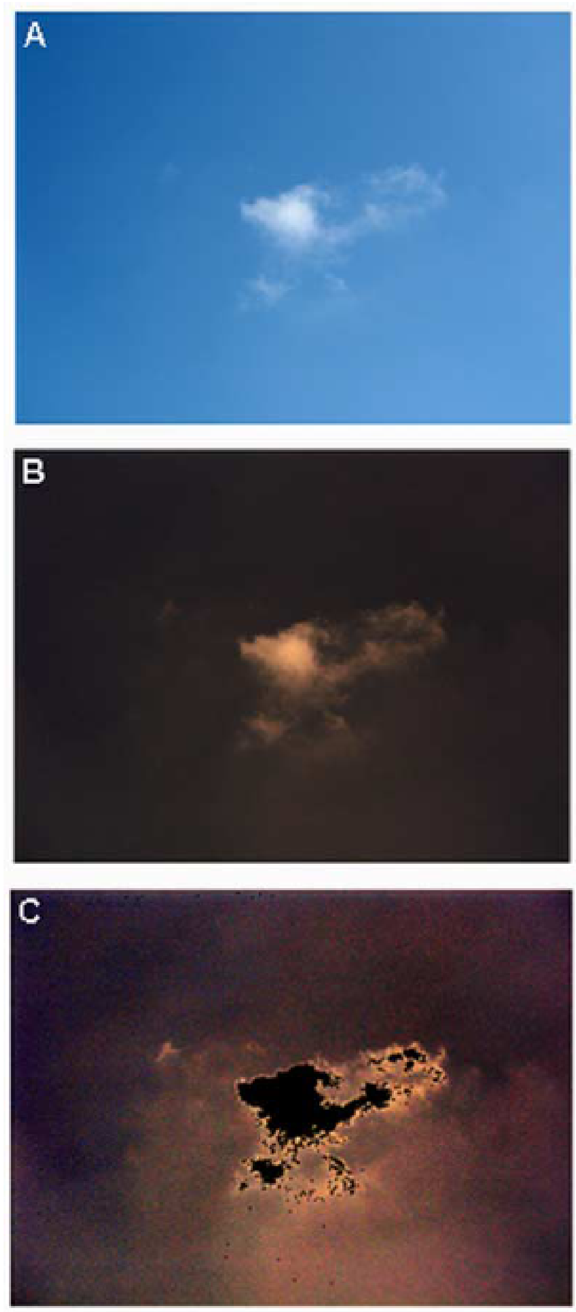


Figure 1. An image of a cloud and the "twilight zone" taken from the ground using a digital camera: (a) true color image of an isolated dissipating cumulus cloud; (b) background gradients caused mostly by molecular scattering were removed; and (c) by masking out the obvious cloud pixels the new dynamic range allows to see the extent of the twilight zone and how the clear sky is not so clear.

Figure 4.9: Figure 1 from Koren et al. 2007. Their caption text.

As noted previously, Kress and van Leeuwen assert that an ideal-real relationship can be carried by placing the constituents of an image into a top-bottom relationship (193-202). They also note that a vertical triptych arrangement can encode an ideal/real distinction; in this kind of composition, the middle term serves as a mediator. These relationships are demonstrated in Diagram 4.3. The diagram also shows how a horizontal arrangement can encode given-new relationships in either dyadic or triptych forms.

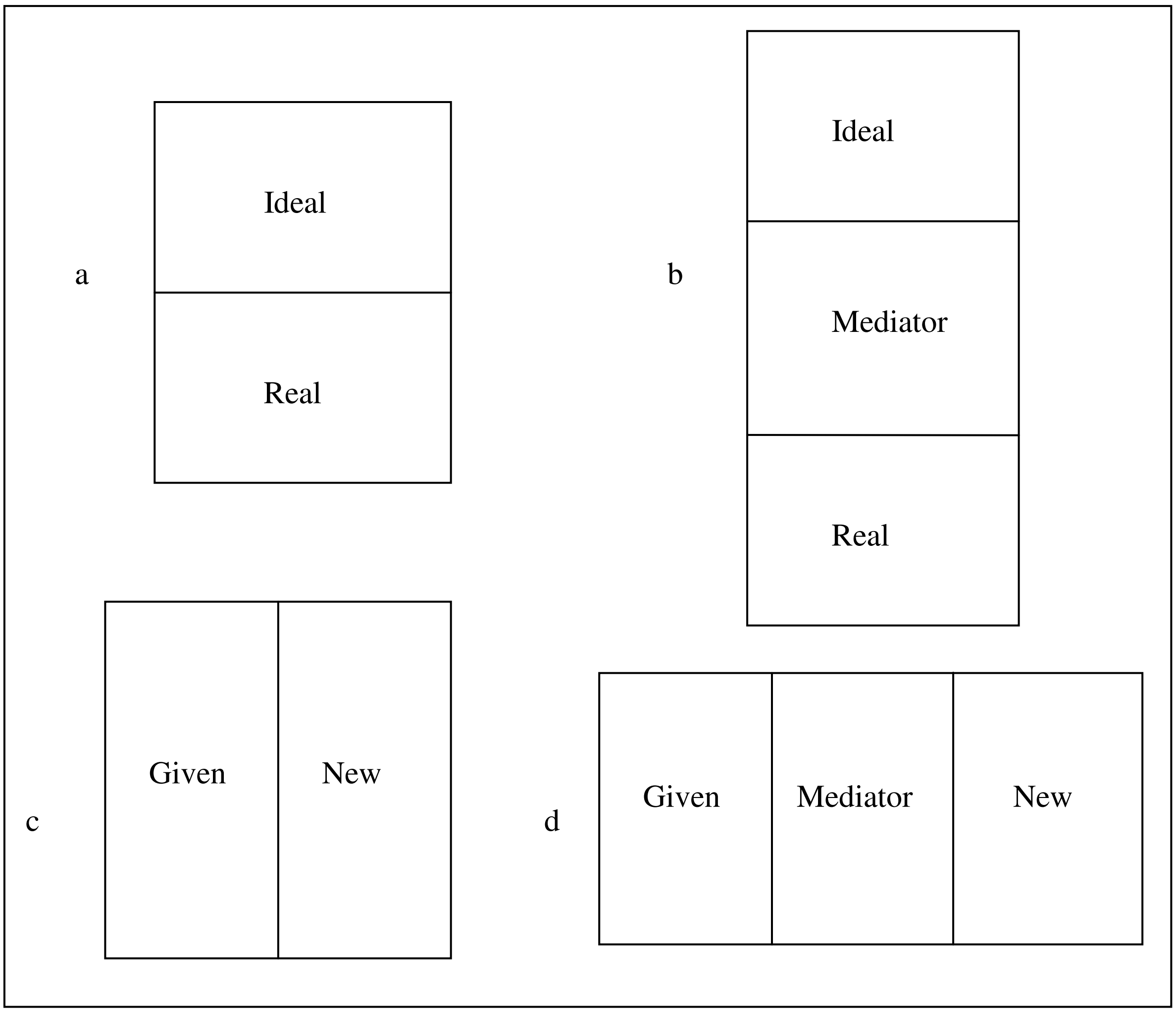


Diagram 4.3. The relationships between image composition and modality. Top-bottom compositions connote ideal-real relationships (a and b). Left-right compositions connote given-new relationships (c and d). The triptych figures are based on Figure 6.24 of Kress and van Leeuwen.

If the ideal-real/top-bottom relationships can be generalized as visual renderings of term I/ term II pairs, they could also be applied to other dissociation arguments, such as appearance-reality distinctions. The cloud photo series seems to support this extension; the "real" twilight zone is placed in the bottom position and the typical "appearance" of a white cloud on blue sky is in the top position. The middle image—the cloud photo with only the blue removed—mediates between the illusory blue-sky image and the "real" *twilight zone*. This "mediator" function is supported by Remer's comments on this visual argument:

> You needed A [the first image] because you needed to connect people directly to the real world. You might have done it with just A and B [the second image], but to stretch [from A to C] is a big leap. You have to make two steps there.

Another factor that could have contributed to the power of this series is the adjustment of color saturation. As Kress and van Leeuwen also note, for scientific contexts monochromatic images often have a higher modality or truth value. Thus, the top-bottom/ideal-real juxtaposition is reinforced by the natural color of the top cloud and the higher-modality black and white coloration of the other two photographs.[38]

Also important to consider when reading this image set are notions of visual syllogisms and visual enthymemes. As Finnegan describes in "The Naturalistic Enthymeme," viewers bring specific assumptions about the truth of photographs to the

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[38] Though false color can also have high modality in scientific contexts, it would likely not have been as effective in this particular instance. False color is deployed to show readers that which they cannot see but that an instrument and the subsequent false color visualization can reveal. The purpose of the cloud photo was to reveal to readers that the twilight zone is part of a reality they already know—a reality they see but overlook because of the false binaries of measurement schemes.

viewing experience. Specifically, a photograph is accepted as "natural" based on unstated propositions of ontological, representational, and mechanical realism. An image's claim to represent nature depends on the viewer's belief that the depicted scene really existed, that the image represents what it claims to represent, and that it was not unusually altered by people either before or after the photographic rendering. If a reader has any reason to doubt these unstated propositions, the argumentative value of the image as "natural" can be compromised. In the case of the cloud series, mechanical adjustments were made for purposes of demonstration, but the authors prevent challenges to the realism of their images by explaining premises governing the adjustments to the photograph. These explanations appear in both the figure caption and the body text. The body text is as follows:

> Figure 1a shows a small dissipating cloud. Subtracting the background reflectance, mostly Rayleigh scattering, causes the background to become black (Figure 1b).[39] Then by masking out the obvious cloud pixels and stretching the dynamic range (to be sensitive to low reflectance), it is clearly shown that the cloud's optical influence extends far beyond the borders of the cloud (Figure 1c). Other cloud masks could be defined, but there is no perfect mask that will unambiguously determine the cloud. (1)

The authors demonstrate how they are drawing out elements that are in the photograph but not seen by the naked eye. Thus, they maintain the mechanical integrity of the image while making effective rhetorical adjustments, adjustments that reveal the previously

[39] In this case, Rayleigh scattering is the scattering of sunlight by molecules in the atmosphere; this phenomenon is what makes the sky blue (Nave "Rayleigh Scattering").

invisible twilight zone. Thus, the adjustments are to be read as informative and not as deceptive.

Though the photo series is an effective conceptual demonstration, it is not the primary evidence for the twilight zone. It epitomizes the appearance-reality distinction, a distinction that is supported inductively with the bulk data of instruments that were reduced into the other visual arguments presented in the article. Though photographs can function as evidence in scientific arguments, this particular graphic is not functioning in this way. As Remer explained, the qualitative aspect of the photo series was understood and even appreciated by her peers:

> People liked it. It was just accepted that this is an image that explains. It's not the thing that you are building the quantitative case from, but it's what explains what that quantitative data means.

The graphic was not seen as evidence but as an illustration. The rhetorical influence of this graphic in this case suggests that visual rhetoric in scientific contexts can be more than the presentation of quantitative data or evidentiary images.

Later sections show how the argument epitomized in the photo series is built through quantitative data graphics. Before commencing with that discussion, I describe and comment on the adaptation of the cloud series for non-expert audiences.

The Cloud that Could in Popular Contexts

After the article's publication in *Geophysical Research Letters*, news of the *twilight zone* spread quickly. NASA produced a popular summary on its Web site, which is a typical practice for the agency (Remer). However, other online news outlets also picked up the story, presumably because of the *twilight zone*'s potential to revise climate models and climate predictions. These online news outlets included *Fox News* and other lesser-known Web sites, such as the environmental news site *Terra Daily*. The selection and adaptation of the cloud images in these popular accommodations demonstrate how the same images were modified for different contexts.

In the NASA accommodation, the entire cloud sequence is presented, but the arrangement is different. Rather than a vertical arrangement, the series is organized horizontally. Figure 4.10 is the photo series as presented on the Web site.

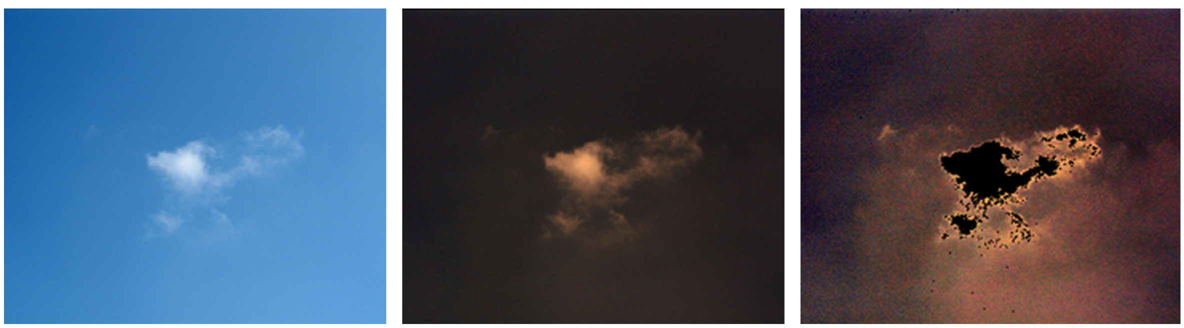

Figure 4.10: The cloud photo series as reproduced in the accommodation on the NASA Website. Source: Cole, Stephen. "Widespread 'Twilight Zone' Detected around Clouds." National Aeronautics and Space Administration. 3 May 2007.

The choice to change from a vertical to a horizontal arrangement could have been motivated by multiple factors. First, a horizontal arrangement is more suitable for a Web text; the entire sequence can be viewed at one time. A vertical arrangement could require a viewer to scroll down to see the final image, disrupting the reading process. Second, as Kress and van Leeuwen's work indicates, the more narrative structure of left to right composition can encode a given to new relationship. The "novelty" argument could be a more powerful argument for a lay audience than the rhetorical dissociation coded in a top-bottom/ideal-real structure. Testing this possibility requires further study of other images and contexts where vertical and horizontal compositions are substituted when presenting the same information to different audiences and in different media.

Regardless of the motivation for the arrangement change, the text still presents a dissociation argument through the caption text:

> What *appears* as clear sky around a cloud as seen from the ground through a digital camera (left) *actually* has a twilight zone of light-reflecting particles around it (right). To see this, the blue light from the atmosphere in the original image is first subtracted (middle). The twilight zone is revealed after the darker parts of the image are enhanced (right). (Cole, my emphasis)

The circulation of the cloud image is not uniform across the Web sites reporting on the story. For example, *Fox News* only included the final image in the sequence—the image revealing the full extent of the cloud's twilight zone (Figure 4.11). *Terra Daily* included a modified comparison that removes the middle image (Figure 4.12).

Figure 4.11: Image of the *twilight zone,* FoxNews.com. Source: Thompson, Andrea. "Atmospheric 'Twilight Zone' of Particles Could Skew Global-Warming Models." FoxNews.com. 4 May 2007.

Figure 4.12: Image of the *twilight zone,* Terra Daily. Source: "Widespread Twilight Zone Detected Around Clouds." TerraDaily.com. 5 May 2007.

Figures 4.11 and 4.12 suggest that for popular contexts the entire visual argument is not needed. In the case of *Fox News*, only the visual conclusion is presented. For *Terra Daily*, only the major premise and conclusion are presented. These adaptations raise interesting questions about visual reasoning as it moves from specialist to lay contexts. Specifically, do popular audiences accept obvious rhetorical mediation if that mediation is backed by credible scientific authorities (e.g., NASA scientists)? Does the visual burden of proof change when the argument moves from the forensic setting of the peer-reviewed journal to the epideictic setting of popular news outlets? Plausible interpretations of such issues require additional comparisons with other cases.

Another issue raised by the cloud series and its circulation is the relationship between demonstration, example, and induction in various contexts. As Aristotle notes, the use of examples is the rhetorical equivalent of inductive reasoning in dialectic (*Rhetoric* i. 2. 1356b). But how is the cloud series functioning? In the technical research report, the photo series is an illustration of a phenomenon otherwise argued into place by the data. But in popular contexts, the cloud series stands alone or with contextual images, such as pictures of an AERONET instrument (e.g., Figure 4.5). Thus, in these texts, the modified cloud image serves as the primary visual evidence and not as a mere supporting demonstration. The next section examines the relationship between example and induction in the data graphics of Koren and Remer's research report.

Visual Reasoning in the Twilight Zone Data Graphics

Though the data graphics did not circulate into the popular press, they were essential to the success of the research report. This section discusses how the series of graphics follows the paper's overall argument, and it describes the differences between the graphics of the initially submitted version and the final published version.

Table 4.3 is a side-by-side comparison of all the data graphics in Draft 12 (the rejected manuscript) and the published manuscript. Larger versions of each graphic appear after Table 4.3. Figures 4.13a and 4.13b are enlarged versions of the first row of Table 4.3. Figures 4.14a and 14.4b are the figures from the second row. Figures 4.15a and 4.15b are larger versions of the third row. While there are differences between them, the roles of the specific graphics are the same in both texts.

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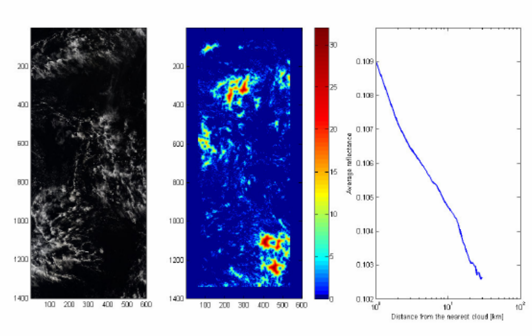
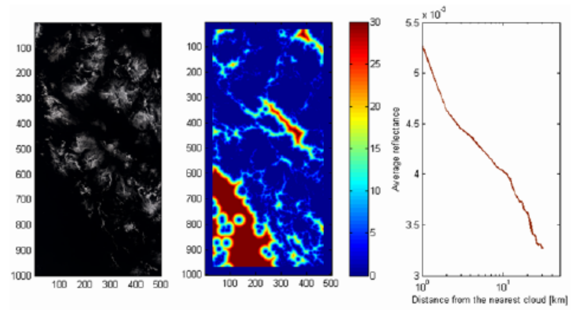
<tr><td>
</td><td>
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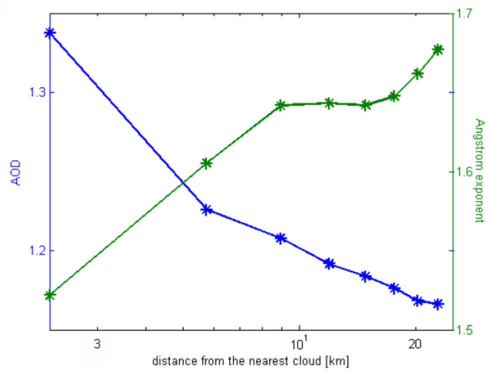
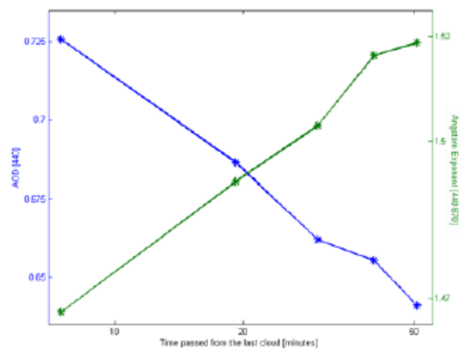
<tr><td>
</td><td>
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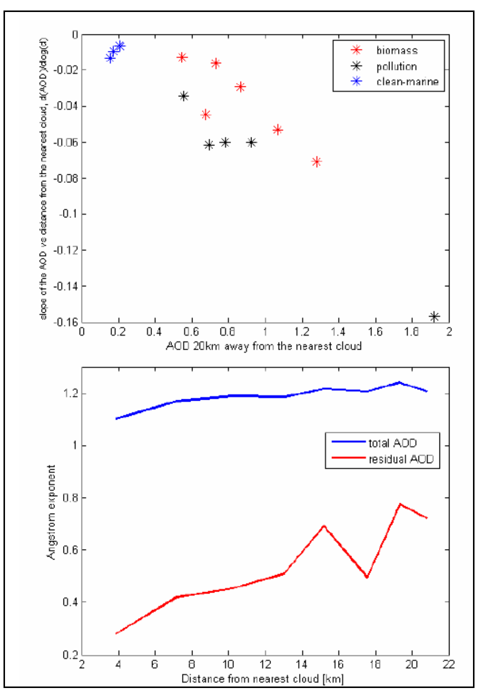
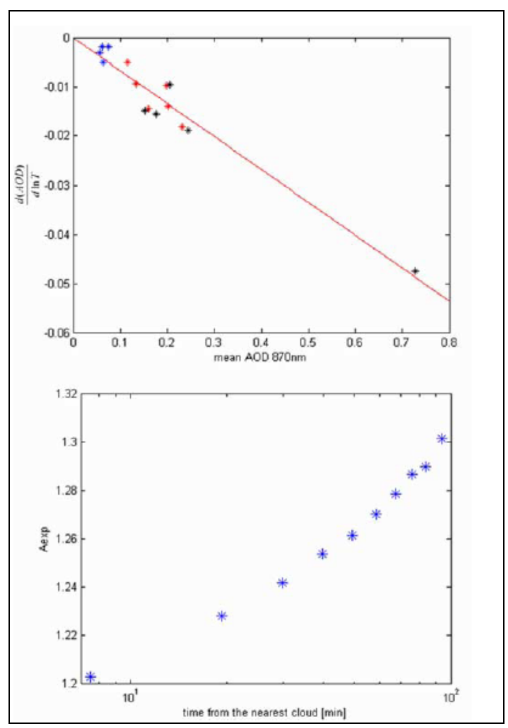
<tr><td>
</td><td>
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Table 4.3: Comparison of data graphics from the rejected draft and the final manuscript.

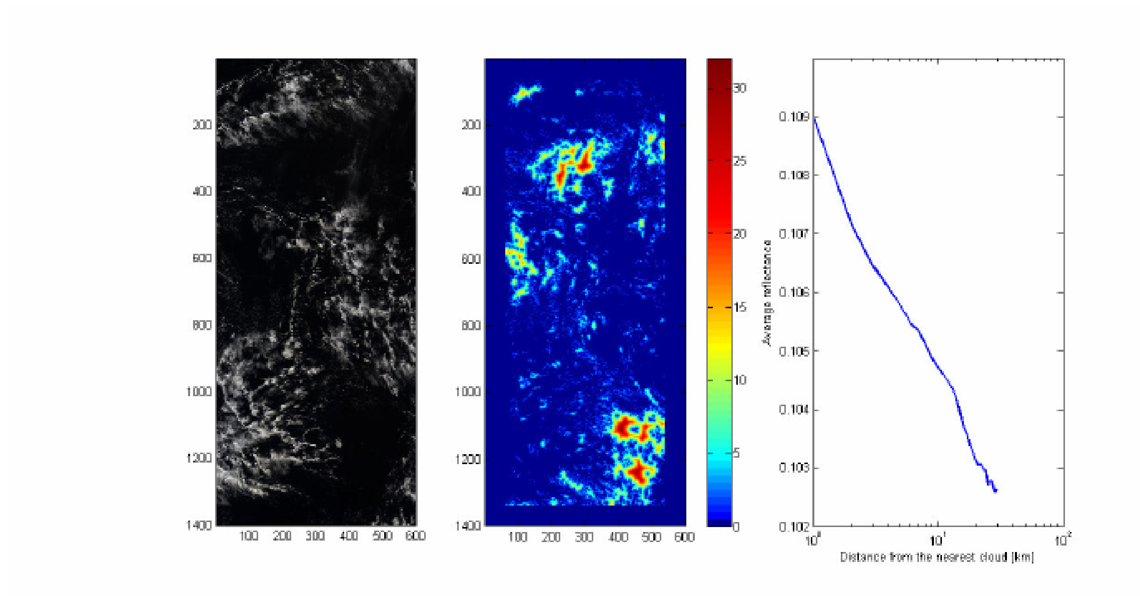

Figure 1- Reflectances as a function of the distance from the nearest cloud. Left - True color image of cumulus cloud field over the Indian Ocean. Middle – Distance to the nearest cloud map of the field. Right - average reflectance of MODIS blue channel (469nm) of the free atmosphere as a function of the distance to the nearest cloud.

Figure 4.13a: Figure 1 from Draft 12.

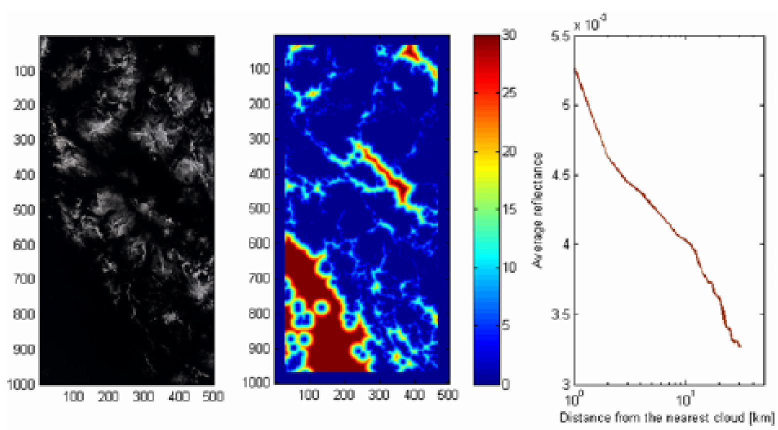

Figure 2. Reflectances as a function of the distance from the nearest cloud: (left) true color image of a cumulus cloud field over the Atlantic Ocean; (middle) distance to the nearest cloud in km (color scale saturated on distance larger than 30 km); and (right) average reflectance of MODIS NIR channel (870 nm) of the cloud free atmosphere as a function of the distance to the nearest cloud in km.

Figure 4.13b: Figure 2 from the final manuscript.

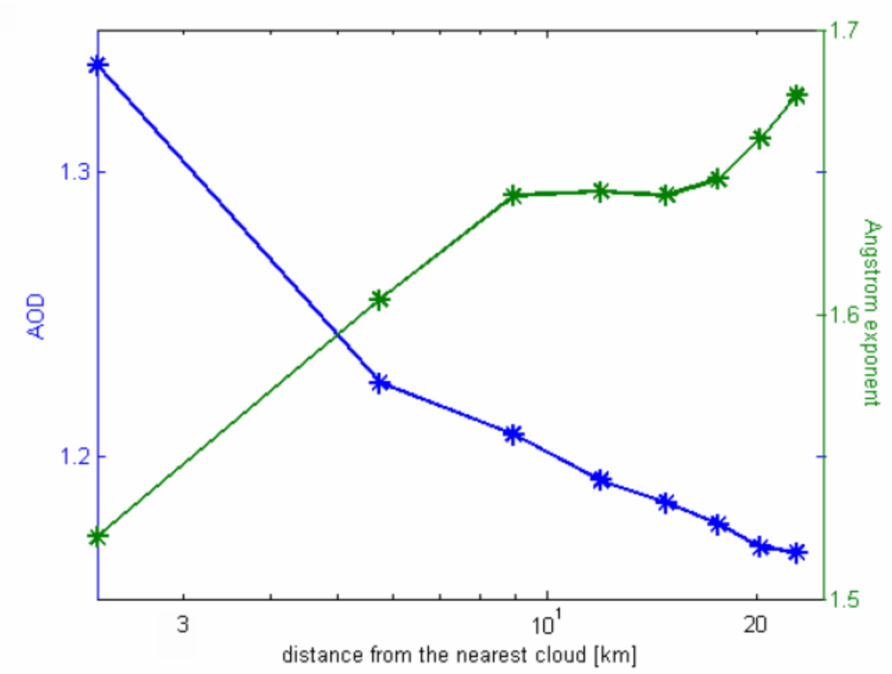

Figure 4.14a: Figure 2 from Draft 12.

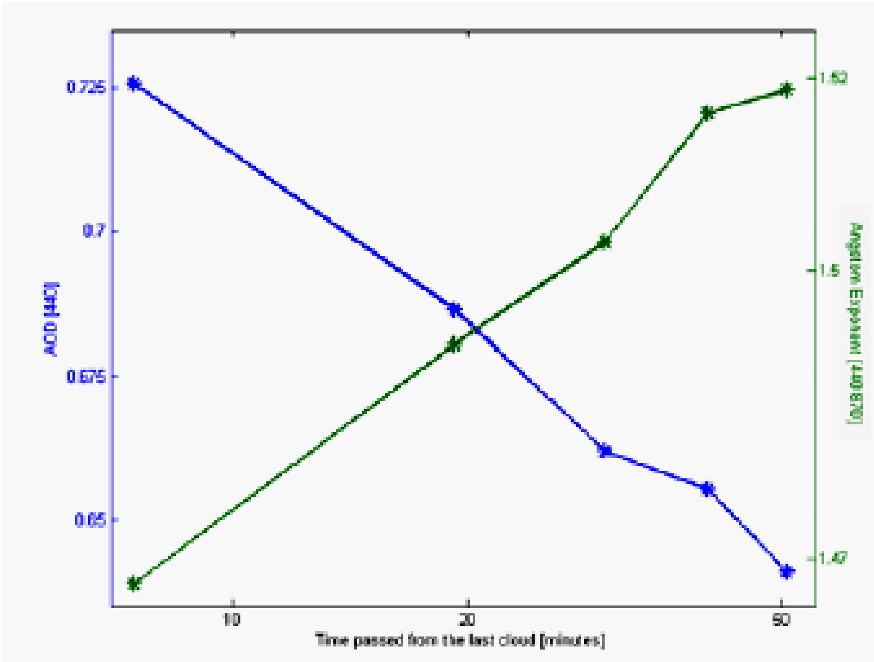

Figure 4.14b: Figure 3 from the final manuscript.

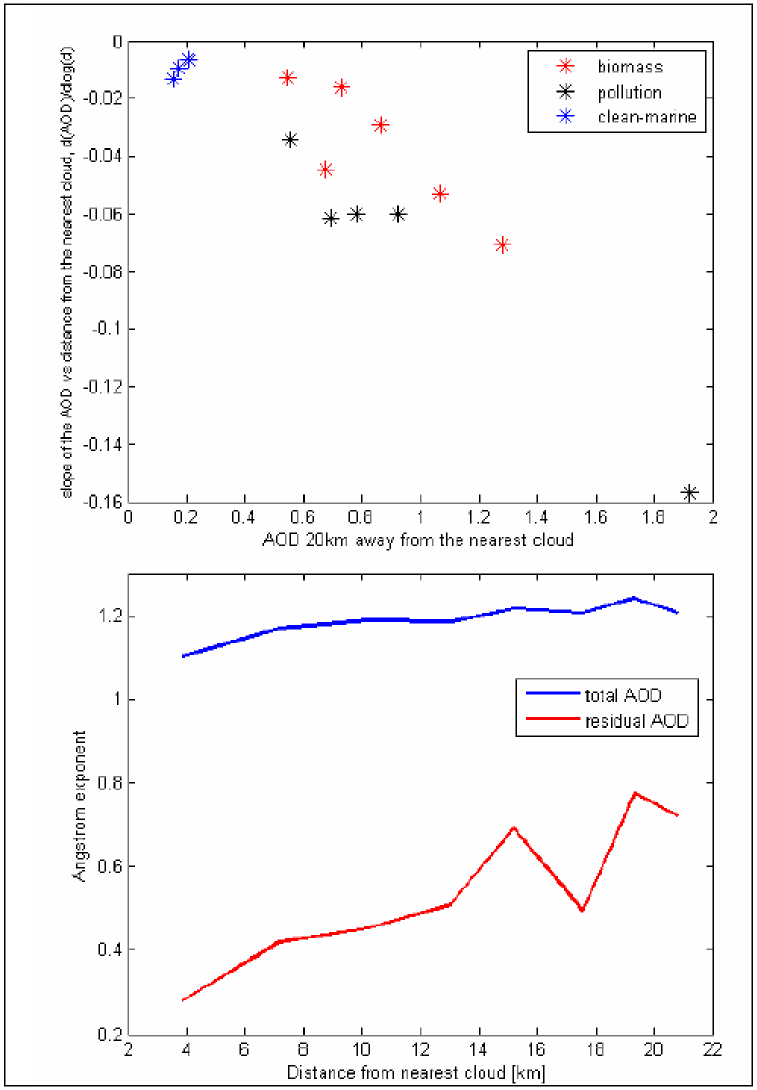

Figure 4.15a: Figure 3 from Draft 12.

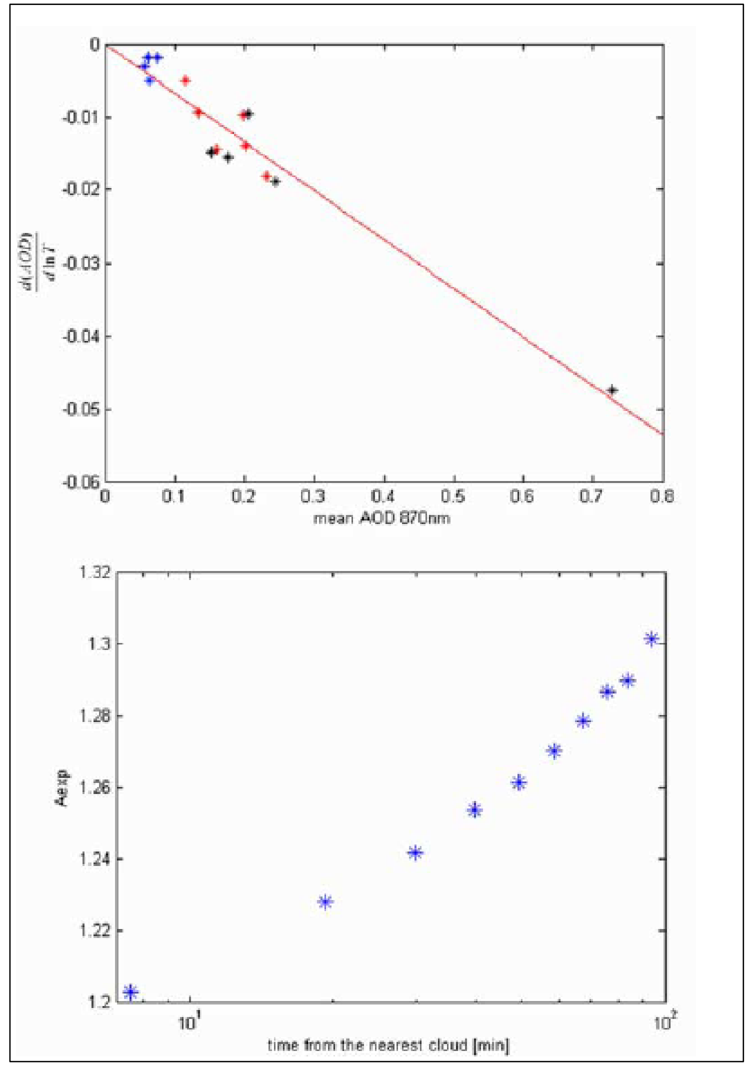

Figure 4.15b: Figure 4 from final manuscript.

The first row of Table 4.3 reproduces the first data graphic of both the rejected and accepted manuscripts (i.e., my Figures 4.13a and 4.13b). (The figure numbers are different because the cloud photo series [my Figure 4.9] was Figure 1 in the published version; thus, Figure 1 of Draft 12 became Figure 2 in the published text.)

The purpose of the graphics in the first row of Table 4.3 is to identify the twilight zone in the MODIS satellite data. Each version presented a triptych that contains three panels: (1) a MODIS image based on reflectance data, (2) a color-coded visualization of the spatial distribution of a cloud field, and (3) a graph of reflectance as a function of distance. The third panel in each triptych—the line graph—is actually an aggregate of thirty MODIS images and not just a graph of data from the images presented in the first two panels. This third panel is the most important from an evidentiary perspective. The first two panels visually describe the two axes of the third panel; i.e., the first image is the reflectance of a cloud field and the second is a visualization of distance from the nearest cloud. Like the cloud photo series, the first two panels of the triptych are not primary evidence. They merely prime the reader's understanding, so the significance of the third pane is more readily apparent.

The center panel of the triptych—the hyper-saturated color visualization of each pixel's distance from the nearest cloud—also demonstrates Koren's skill in programming data visualization software. Koren wrote a special program that separates cloud pixels from non-cloud pixels and indexes each pixel against a color-coded distance scale; the bluest pixels (0 km) are the cloud pixels, and the reddest pixels are as much as thirty kilometers from a cloud. Remer explained that developing a compact algorithm for visualizing pixel-distance from the nearest cloud was “hard to do.” Koren wanted to “show off” this accomplishment.

The graphics in row two of Table 4.3 present data collected from a single AERONET instrument in Brazil. Though the x-axis variable changes from “distance” to “time” between versions, the function of the graphic is the same. It identifies the twilight zone in terms of AERONET data, i.e., aerosol density and particle size. The two curves are inversely related since particle size is measured by a variable called the Angstrom exponent, which is a negative exponent.[40] Larger exponents indicate finer particles; hence, the upward slope of the Angstrom exponent measurement indicates a decrease in size. Significant differences between these similar graphics include the change in the x-axis domain: the first uses distance, the second time. There is also a change from standard to logarithmic scales; this change allows a greater range of time values to be included into the graph.

The final figure in each version is composed of two graphs that fill similar functions. The first graph plots data from several AERONET stations to demonstrate that the rate of tapering in the twilight zone for any given cloud is dependent on the overall aerosol content of the area. For example, changes in the twilight zone are more pronounced in areas with higher aerosol concentrations (for example, regions where deforestation by burning is prevalent) than they would be in an area of low concentration (for example, a “clean” marine environment). The purpose of this graphic is to show that the twilight zone is at least partially dependent on aerosol concentrations; the graphic also shows that the twilight zone is a global phenomenon by including AERONET data from

[40] A negative exponent represents the reciprocal of its positive counterpart. For example, x^{-2} equals $1/x^2$. Therefore, in equations involving negative exponents, a larger negative exponent results in smaller quantity. For example, 5^{-2} equals $1/5^2$ or 1/25, where as 5^{-3} equals $1/5^3$ or 1/125.

points around the globe. The second graph presents particle-size data in relation to cloud position, which is plotted as distance in Draft 12 and as time in the final version. Differences in the graphic from version to version represent differences in the variables and calculations. Also, the first version is more complicated because the authors were making additional claims. As Remer notes, "There was a lot in there that did not have to be there."

As a group, the series of images accomplishes what Alan Gross would call a complete rhetorical description. In *The Rhetoric of Science* and its revision *Starring the Text,* Gross shows how a new species of hummingbird is brought into existence through converging vectors of description: detailed verbal description, statistical analysis, illustration, etc. Each of these elements is used to synthesize the identification of the new species within the framework of evolutionary biology (*Starring* 49-62). Arguably, a similar activity is happening with the *twilight zone*. Multiple instruments are used to measure and visualize the same phenomenon; in addition, visualizations of individual clouds, cloud fields, and the data from single-instrument installations are supported with graphics that aggregate data from multiple points around the world. The series of data graphics (especially in the final version) emphasizes the gradual nature of the twilight zone through the multiple iterations of gradual linear relationships. These graphical descriptions are supported with the article text. Moreover, reading these graphics as rhetorical "over-description" is supported by comments from Lorraine Remer. In the following passage, she describes the conclusion to her many presentations on the twilight zone research:

> I would finish the talk with, "The twilight zone, we measure it in MODIS, we measure it in AERONET, this other group we know has measured it with LIDAR, we see it with our eyes. It's real." And it was that kind of statement that people took with them. And you never know who your reviewers are, and I'm sure some of them were sitting in the audiences of these talks.

In these concluding comments, Remer explains that several different instruments converge to support the existence of the twilight zone. Thus, she emphasizes the significant corroboration for this novel, high-stakes claim. Remer's reference to "seeing it with our eyes" refers to the cloud picture series. The presence of the photo series also supports my claim that this article offers a "complete rhetorical description," similar to the hummingbird speciation described by Gross.

Reading down the table of images also reveals an interesting pattern of argument. Specifically, for each instrument the authors move from individual examples to larger collections of data. In the case of the MODIS triptych, a single cloud field is presented in the first two panes, and an aggregate is presented in the third pane. For the AERONET data, the first graphic presents averages for a single station and the later graphs present data of multiple stations either as individual data points or as aggregates. Thus, for both instruments the authors move the view from the local to the global and from specific exemplars to broader patterns. In other words, the authors use both example and induction to demonstrate that the twilight zone is a global atmospheric phenomenon.

Reviewer Comments and Visual Revisions

Changes between the graphics of Draft 12 and the published version also reflect the comments of reviewers. As noted previously, colleagues and reviewers objected to the presentation of AERONET data in terms of distance. That instrument measures in *time*, so a distance-indexed graphic seemed deceptive. In the revised version, the AERONET graphics plot aerosol values as functions of time. This aspect of the paper and its production is rhetorically interesting. The authors initially felt they had to present MODIS and AERONET data in the same domains of measurement to demonstrate the similarities between MODIS and AERONET data. Thus, they used a calculation to convert time to distance in the early draft: "Assuming a mean transport wind-speed of 10m/s (Archer and Jacobson, 2005) we translate a time interval into a distance interval" (6). But this simplifying assumption was not acceptable to their peers, and thus the graphic—a visual argument resting on this premise—was not acceptable either. In their attempts to make their argument clearer, the twilight zone authors created an untrustworthy visual argument. Moreover, this mathematical simplification was unnecessary for the case.

The twilight zone paper presents an argument about the existence of a phenomenon and not an argument about its precise measurable extent. The AERONET data only needed to corroborate the observations recorded by MODIS by showing that the MODIS data was not faulty as some researchers thought it was. Such corroboration does not require measuring in the same domain. The revised time-scaled AERONET graphic presents data that cannot be directly compared on a numerical basis with the MODIS distance-scaled data; however, these graphics support one another via the visual similarity of the data patterns. Both instruments are generating curves with similar patterns even though they measure aerosol variables in different domains.

Modifications to the first data graphic—the MODIS triptych (Figures 4.13a to 4.13b)—also seem motivated by reviewer comments. One reviewer objected to the configuration of the middle pane of Figure 4.8 (Figure 1 of Draft 12):

> One should at least come away from Figure 1 with an appreciation of the qualitative relationship between the left most figure and the middle figure. There are two larger red spots in the top half of the image for which the nearest cloud is ~ 20 km according to the color scale. A very short distance below and a little to the right of the rightmost red spot (maybe 15 km distance using a crude scaling) the distance to the nearest cloud drops to something like 1 or 2 km. How could there be such large changes in the distance to the nearest cloud across such a short distance if there doesn't appear to be a cloud in the vicinity of these red spots?

In the revised version, the authors present a similar triptych, but they select a portion of the sky that more clearly shows distance relationships within a cloud field. Specifically, they choose an example that includes a larger region of cloud-free atmosphere. In the revised graphic, the distance relationships are less ambiguous, which averts the visual incommensurability of the previous draft.

Publication and Reception; Resources and Acceptance

As noted previously, "On the Twilight Zone between Clouds and Aerosols" was ultimately accepted by *Geophysical Research Letters*, and the *twilight zone* concept circulated into popular culture through both mainstream and niche online publications.

Though it is still too early to tell how influential the *twilight zone* will be for atmospheric science, there are indications of the authors' extended rhetorical success within several communities. First, their colleagues at Goddard who initially wanted Koren and Remer to retract their paper are now supporting it. These former skeptics have even offered to help collect higher resolution AERONET measurements to refine the research. Second, the *twilight zone* paper is beginning to be cited in other articles. As of 21 April 2008, Google Scholar searches revealed that only one paper had cited the Koren et al piece, though this is not unusual, considering the time it takes for a new concept to percolate into new projects whose manuscripts then need to be reviewed and revised before publication. Though the first report to cite the *twilight zone* paper (Zuidema et al, in press) does not extend or challenge the concept, it does acknowledge it:

> *In reality*, particle growth varies as a function of relative humidity so that there is a continuum from dry aerosol, to humidified haze particles and finally cloud droplets as the relative humidity increases and eventually reaches saturation (Charlson et al. 2007; Koren et al. 2007). We made no attempt to account for these effects. (Zuidema et al, 5, in press, my emphasis)

Though Zuidema and her colleagues do not account for the twilight zone in their study, the reference suggests that it is a concept that the atmospheric scientists who study clouds and aerosols need to consider. That is, the reference is evidence of the legitimacy of the concept. The opening prepositional phrase—"in reality"—suggests that the dissociation argument was convincing to this group of authors. As I extend my work on the verbal and visual articulation of atmospheric phenomena, I plan to continue to track the circulation and extensions of the twilight zone.

Conclusion

This chapter has shown how an invisible atmospheric structure was argued into existence through verbal and visual strategies. The authors negotiated verbal rhetorical figures and a range of visual artifacts to demonstrate that the *twilight zone* is a real phenomenon and that the atmosphere cannot be neatly sorted into cloud and cloud-free categories. These rhetorical activities demonstrate the importance of dissociation arguments to the rhetoric of science. They also demonstrate that the visual rhetoric of science is more than just data graphs and the regurgitation of instrument output. These scientists altered images and developed lines of visual reasoning to change the structure of reality. And they were persuasive. The same colleagues who had told them to retract the paper are now offering to help further substantiate the twilight zone.

I end this chapter with two quotations from Remer, both of which relate to the reading of science as a dissociative activity and the relationship between rhetoric, science, and knowledge:

> We get criticism that we haven't done anything new: "We've known about this since eternity." But no one had sat down to quantify it.
>
> * * *
>
> This is not new, people see this all the time. They inherently know this is true, but no one had quantified it, no one had put into print that "this is real," that it's important, and that we have to think about it.

The first quotation can be read as indicative of the scientific dissociation that Gross describes in *Starring the Text.* That is, in hindsight the *twilight zone* is obvious to everyone, even former critics. However, no one saw it before, or if they did see it, they did not act on the knowledge. Such a reading supports my argument, but is it the whole story? Was the *twilight zone* always known but not quantified? Or is this evidence of a dissociation that was so successful that it asserts itself on the reading of previous understandings? Regardless, I am intrigued by the relationship between quantification, rhetoric, and reality that is expressed in the quotations. We might intuit the existence of phenomena, but in modern science it is data described in print that create a reality we can act on, and thus the visual rhetoric of science has "real" consequences. It is through images, the ultimate expression of quantifying instruments, that things are "really" made real.

Scientific images can be powerful arguments, but such power must be used responsibly. When instruments and images are literally recasting our world, unethical imagining practices are more than deception; they are assaults on reality itself. The next chapter examines the ethical implications of mediating images in scientific contexts. By examining recent science scandals, ethics violation reports, and editorial responses to unethical imaging, I consider how scientists can be rhetorically innovative without being ethically questionable.

Chapter 5: (Ir) Responsible Mediation of Scientific Images—Enthymemes, Ethics, and the Visual Rhetoric of Science

What was observed by us in the third place is the nature of the Milky Way itself, which, with the aid of the spyglass, may be observed so well that all disputes that for so many generations have vexed philosophers are destroyed by visible certainty, and we are liberated from wordy arguments.

—Galileo Galilei, *The Sidereal Messenger*

However, to say that "the camera cannot lie" is merely to underline the multiple deceits that are now practiced in its name.

—Marshal McLuhan, *Understanding Media*

It thus appears that rhetoric is an offshoot of dialectic and also of ethical studies.[41]

—Aristotle, *Rhetoric*

The last three chapters examined the persuasive dynamics of individual historical and contemporary cases in which visual arguments were essential for rhetorical success. This chapter examines issues of visual rhetoric in science more broadly. Specifically, it examines responses to both the proliferation of digital imaging technologies and the escalation of digital fraud in scientific settings. Analyses of new image-preparation guidelines, journal feature stories on visualization practices, and editorial responses to imaging misconduct reveal otherwise tacit beliefs about the rhetorical functions and

[41] I am not the first person to include these passages from Aristotle and Galileo as epigrams. Richard Weaver uses the Aristotle quotation as the epigram to *The Ethics of Rhetoric* (1985). Edward Tufte includes a modified version of the Galileo passage as an epigram to *Beautiful Evidence* (2006). Regardless, as far as I know, these two passages have not been paired as epigrams, nor have they ever been included alongside the passage from media theorist Marshall McLuhan.

epistemic value of scientific visuals. Such an analysis also reveals that these assumptions are unfixed, evolving, and context dependent.

A rhetorical approach to scientific images and scientific ethics can plumb the line between an impossible ideal of universally transparent visual certainty and an equally impossible default skepticism that assumes all images are deceptive. In other words, a rhetorically prepared and rhetorically situated image can be effective and ethical in scientific contexts. Rhetorical concepts including *ethos* and the *enthymeme* are productive terms for framing a discussion of the ethical manipulation of visual artifacts in science. Before describing how these terms can be applied productively, I first describe recent cases that demonstrate the problems and possibilities of digital mediation in science.

Fakes, Frauds, and Ethical Enhancements: Digital Visuals and the Rhetoric of Science

A 2005 investigation by the U.S. Department of Health and Human Services' Office of Research Integrity (ORI) revealed that Xiaowu Li, a postdoctoral fellow at UC–San Francisco, falsified three digital images in a paper on pancreatic cancer cells. Li claimed that the images were human pancreatic cancer cells plated with a specific compound of interest. In fact, the images were of mouse melanoma cells plated with an entirely different chemical. The ORI case summary describes the ramifications of Li's misconduct:

> The misconduct was significant because pancreatic cancer has a poor prognosis for patients, since it tends to invade other tissues and to metastasize early in its course. Knowledge of the factors that facilitate

> cancer cell invasion and metastasis, which was the focus of the questioned figure and paper, is crucial to attempts to develop better treatments for pancreatic and other cancers. Thus, the falsified figure could have misled other investigators in this important area of medical research. (ORI "Li")

Li was excluded from working on federally supported projects for three years; however, this damage to his career is far less severe than the damage his deception might have done to the field and to pancreatic-cancer patients anxiously awaiting new therapies.

In May 2006, ORI banned renowned cancer-cell researcher Steven Leadon from federally funded projects for a period of five years (ORI "Leadon").[42] The ORI investigation revealed that Leadon had falsified DNA samples and constructed falsified figures in eight articles and four grant applications over a seven-year period. For example, the autoradiogram in "A" of Figure 5.1 was a convincing falsification.

[42] Technically, Leadon and Li were not "banned," but they did sign voluntary exclusion agreements. This situation is equivalent to an employer asking an employee to resign instead of firing the person. However, Leadon accepted the terms of his agreement under protest. He never admitted to the charges of misconduct, and he claimed he only accepted the voluntary exclusion agreement because he could not afford the legal expenses that a formal defense would require (ORI "Leadon").

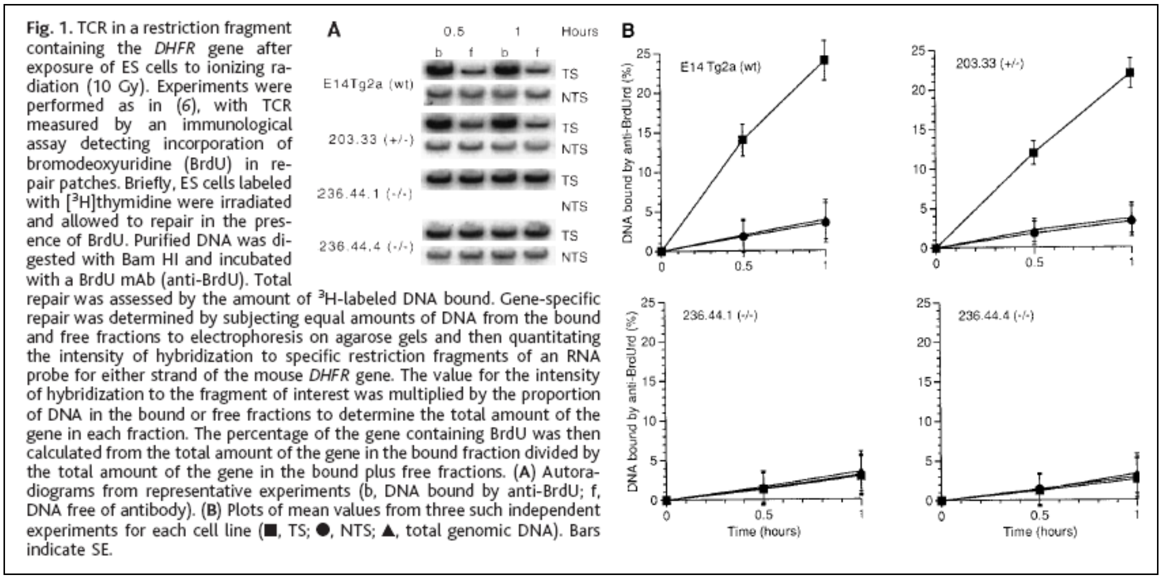

Figure 5.1. Figure 1 from Gowen et al. 1998. Leadon was the fifth author on the paper, but he was responsible for the graphics, and he was the author of correspondence. The autoradiogram (part A) is a falsification.

Leadon's work on cellular repair of oxidative damage to DNA—such as the damage caused by the sun—had enormous influence on cancer research and investigations of the rare but debilitating Cockayne syndrome. The fraud devastated both those with whom Leadon collaborated and others who built research programs around his "groundbreaking" work. According to Leadon's former collaborator Priscilla Cooper, "The subfield that is working on oxidative damage is really having to redo everything from scratch" (qtd. in Check 1015).

In January 2006, after months of rumor, accusation, and investigation, the editors of the journal *Science* retracted two papers on embryonic stem cell research. The papers, authored by the research group of celebrity scientist Woo Suk Hwang, documented remarkable advances. The first paper (2004) claimed to have extracted viable pluripotent (multipurpose) stem cells from cloned blastocysts (early-stage human embryos).

The second (2005) documented the creation of patient-specific stem cells by inserting cellular material from adult subjects into "hollowed out" egg cells—a process called Somatic Cell Nuclear Transfer (SCNT). SCNT promised to revolutionize stem-cell research by removing the need to destroy fertilized embryos to develop stem cells and by allowing stem cells to be cultivated from the patients' own cells. Thus, Hwang's research seemed to skirt many of the ethical quagmires that have made stem-cell research a hot-button political issue in the United States.

Unfortunately, Hwang's claims were based on fraudulent data presented in fraudulent images. For example, in the SCNT paper, Hwang provided photographic evidence of eleven distinct lines of stem cells allegedly derived through SCNT, but nine of these photo sets were complete fabrications. These nine photo sets were modified versions of just two photo sets. The cells of these two sets of "original" digital photographs were in fact stem cells, but they were derived from another cloning process, not SCNT. Figure 5.2 and Figure 5.3 are two different demonstrations of Hwang's fraudulent practices. Figure 5.2 shows how multiple images were layered to create the illusion of a distinct "new" cell. Figure 5.3 shows a more blatant display of using the same image to represent two different cell lines. The duplication was masked by the medium of "negative staining," a microscopy technique in which cells are plated with dark ink and thus appear brighter than the dark background. In Figure 5.3, forensic contrast adjustments reveal the fraud.

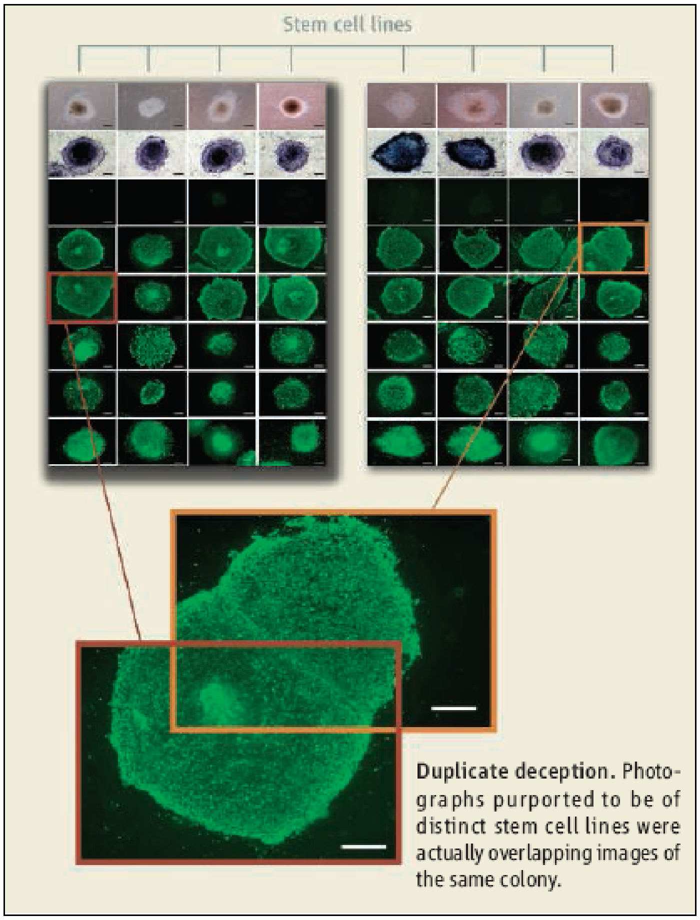

Figure 5.2. From Couzin "…And How." ***Science.*** **6 January 2006: 24.**

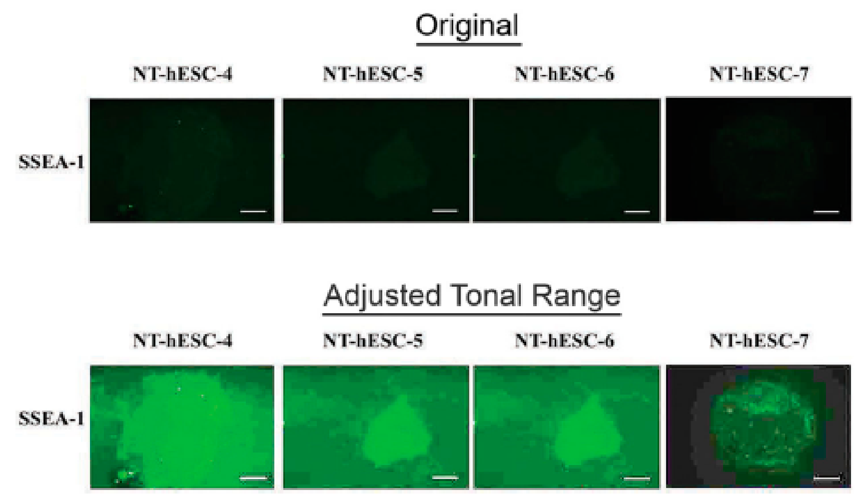

Figure 1. **Detecting image manipulation in the Hwang et al. stem cell paper** (9). The image in the top row is from the third row of Supplemental Figure S1B in that paper. It purports to show negative staining for a particular cell surface marker in four different cell lines. A simple adjustment of tonal range in Photoshop clearly shows that the two middle images are identical. The minor differences in pixel structure are due to image compression. Detecting this duplication would have led us to request the original data from the authors. We may still have published the paper if the authors dishonestly claimed a clerical error, but at least we would have started asking questions. Clearly someone in the Hwang lab was desperate to blow the whistle on the case—eventually approaching the media. If that person had learned that the journal editors were questioning the data before publication, perhaps the whistle might have been blown in time to prevent publication. Reprinted with permission from *The Scientist.*

Figure 5.3. Figure 1 from Rossner. "Hwang Case Review Committee Misses the Mark." ***Journal of Cell Biology*** **2007. NT-hESC-5 and NT-hESC-6 are identical images and do not represent what they claim to represent.**

Before the revelation of the fraud, Hwang was lauded as a national hero in South Korea for his intrepid work on stem cell research and animal and human cloning. His work was so convincing that other researchers had abandoned stem-cell projects because Hwang seemed to have control of the field (Wade and Sang-Hun).

Digital photographs may seem especially vulnerable to unethical adjustments; however, they are not the only visuals implicated in technology-enabled misconduct. For example, post doc Jason Lilly—in addition to claiming that figures represented replicated studies that he had not actually replicated—manipulated the color of visualized mRNA accumulation data (ORI "Lilly"). Figure 5.3 is one of Lilly's falsified visuals from an article that appeared in the journal *The Plant Cell.* In the enlarged segment in Lilly's graphic, the visualization for the Sac1 gene has been modified to represent higher levels of mRNA; a closer look reveals that two of the Sac1 blocks that are black in the longer sequence are shades of red in the enlarged portion. As punishment for his misconduct, Lilly was debarred for a two-year period, which ended in March 2007.

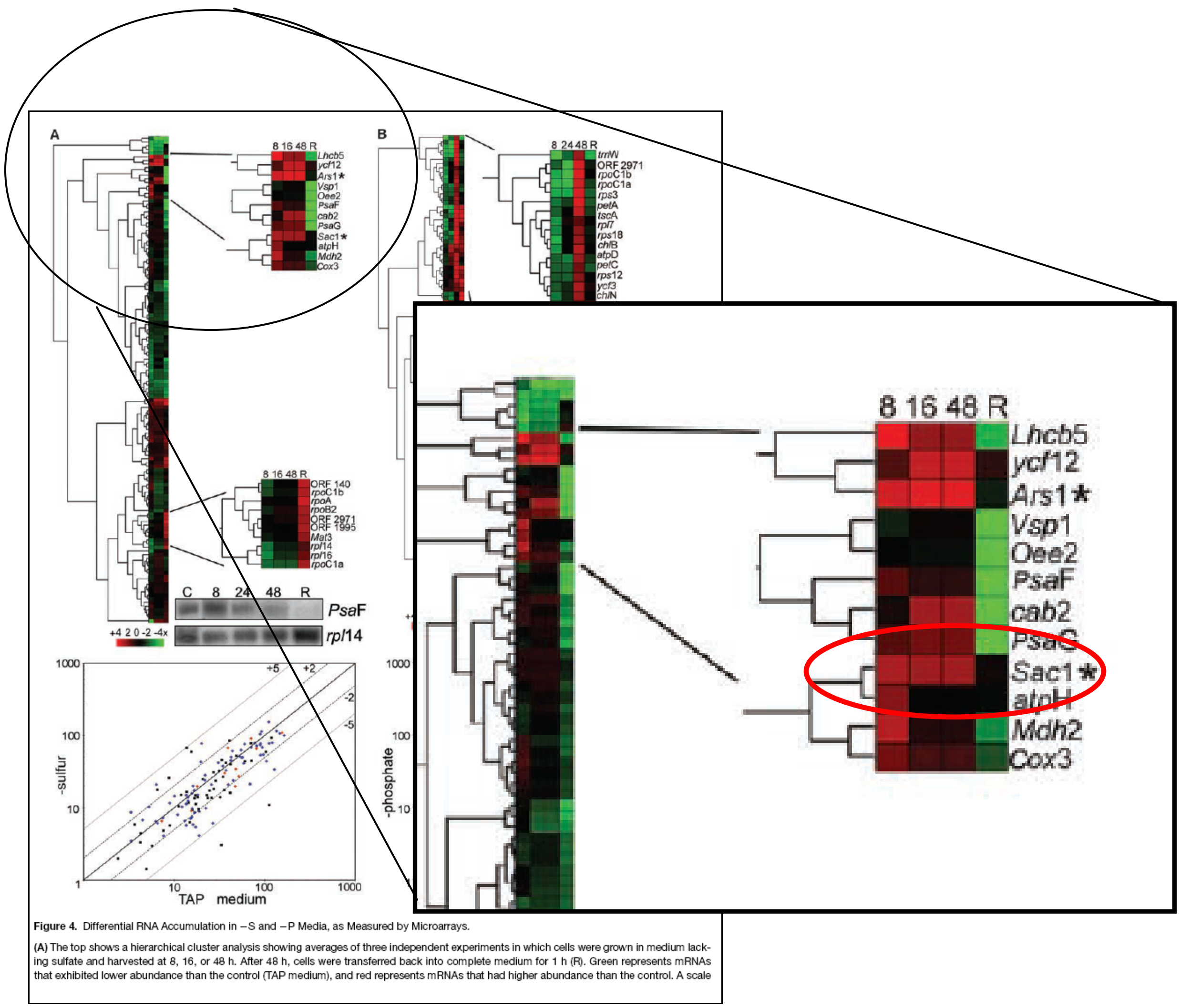

Figure 5.4. Figure 4 of Lilly, Maul and Stern. *The Plant Cell.* 2002. My red circle in the enlarged portion marks the modified portion of the data visualization. Squares 16 and 48 of Sac1 were modified.

Each of these cases of scientific misconduct demonstrates that unethical research practices in science are often based on visual components and that such visual misconduct can have significant scientific, social, and professional consequences.

Arguably, new visualizing technologies and their digital processing have increased the prevalence of imaging misconduct by providing expedient means for committing fraud. According to one analysis of ORI reports, less than 3% of allegations of research misconduct in 1990 involved images; by 2001 more than 25% of ORI investigations involved images (Kreuger qtd. in Pearson 952). As the editors of *Nature* noted in 2006, such a trend is more a matter of opportunity than motive: "It is doubtful that scientists were more angelic then than now. It is more likely that, when it came to image manipulation, they wouldn't because they couldn't" ("Not Picture-Perfect" 891-892). According to others, the increase in imaging misconduct suggests generational differences. An editorial in *Nature Cell Biology* laments "a whole generation of scientists has known nothing but the magical world of Photoshop and regularly use [sic] tools with fashionable names such as clone and healing" ("Appreciating Data" 203). Figure 5.5 is an example of "cloning." The "rubber stamp" tool, which is also known as the "clone stamp," was used to tidy up the background of the photographed gel; however, this tidying also removed real data—faint lines indicating the presence of molecules (Rossner and Yamada 13).

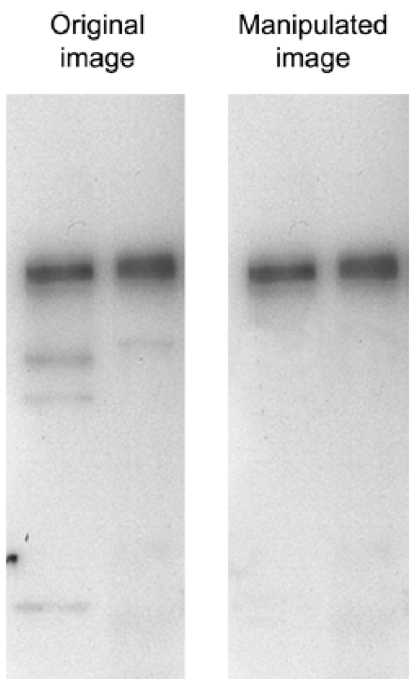

Figure 4. **Manipulation of blots: cleaning up background.** The Photoshop "Rubber Stamp" tool has been used in the manipulated image to clean up the background in the original data. Close inspection of the image reveals a repeating pattern in the left lane of the manipulated image, indicating that such a tool has been used.

Figure 5.5. Figure 4 from Rossner and Yamada. *Journal of Cell Biology*. 2004.

But should younger generations and new technologies take all of the blame? After all, there were frauds involving scientific images long before the digital revolutions of the latter half of the twentieth century, and there were image-enabled frauds long before photographs were even a major mode of science communication. For example, Ernst Haeckel's 1866 drawings of embryonic development (Figure 5.6) are now infamous both for their strategic inaccuracy and for the significant ramifications of the fraud (see commentary by Paul Dombrowski; Michael Richardson and Gerhard Keuck; Stephen Jay Gould; and Elizabeth Pennisi). Haeckel made strategic selections to highlight the similarity of embryonic forms, and he left out or masked important differences in his drawings. Haeckel's "ontogeny recapitulates phylogeny" argument—the idea that a developing embryo "relives" evolutionary history—was visualized better with drawings that left out real differences between the embryos of different species. Opponents of evolutionary theory consistently point to Haeckel's fraud when attacking the credibility of the Darwinian paradigm (Dombrowski 317).

Moreover, even in the Age of Photoshop, scientists can use low tech means to fabricate results. Xiaowu Li—the researcher who presented images of mouse cells as human cells—could have perpetrated that fraud without digital technology. In another ORI investigation, a medical student "created" visual results by modifying autoradiographic films with nothing more than a black marker (ORI Zhao).

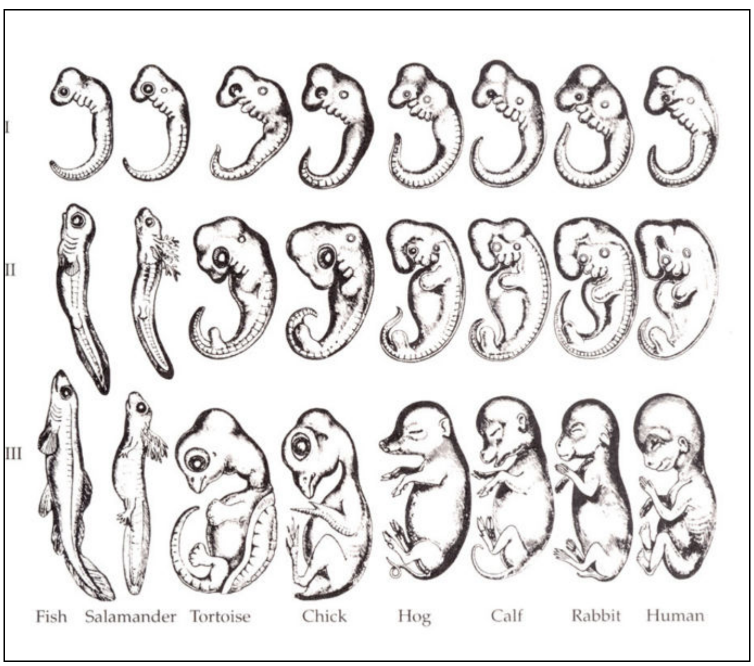

Figure 5.6: Haeckel's embryo drawings as reproduced in Richardson and Keuck (144). These images are gross simplifications of embryos across species. Creationists continue to use Haeckel's fraudulent drawings to cast doubt on evolutionary theory.

Digital adjustments do not have to be unethical; they can also improve scientific practice. For example, in 1997 Richard Levenson and Daniel Farkas developed a system to "spectrally classify" the pixels in digital micrographs of cells. The images in Figure 5.7 demonstrate the utility of this digital imaging procedure. The top image is a contemporary micrograph of cells from a pap smear; the bottom image has been spectrally classified by the Levenson and Farkas system. *Science* writer Gary Taubes reported the details of the differences:

> The cells [in the top image] have been stained to bring out the contrast between different types: Mature epithelial cells are pink-orange, while younger cells stain blue-green, as does the precancerous dysplastic cell in the middle. A pathologist would identify it by its abnormally large nucleus, but it wouldn't be hard to miss.
>
> On the bottom is the same image, spectrally classified. […] The [SpectraCube] microscope divides light from each pixel into beams that travel along paths of varying lengths, then are recombined and allowed to interfere. Mathematical analysis of the resulting interference patterns yields a spectrum. […] By comparing each pixel's spectrum to those of reference pixels (boxes on original micrograph), Levenson and Farkas's system identifies groups of pixels with similar spectra and assigns them distinctive colors, making them much easier to tell apart than they are in the original stained micrograph. The nucleus of the dysplastic cell, only subtly different in color from that of a normal cell in the traditional

micrograph, is here colored a unique and fiery red, befitting its threatening nature. (1990)

In the spectrally classified image, the red-colored nucleus of the precancerous cell is much more salient. Thus, this digital enhancement could help pathologists identify precancerous cells more quickly and with greater accuracy.

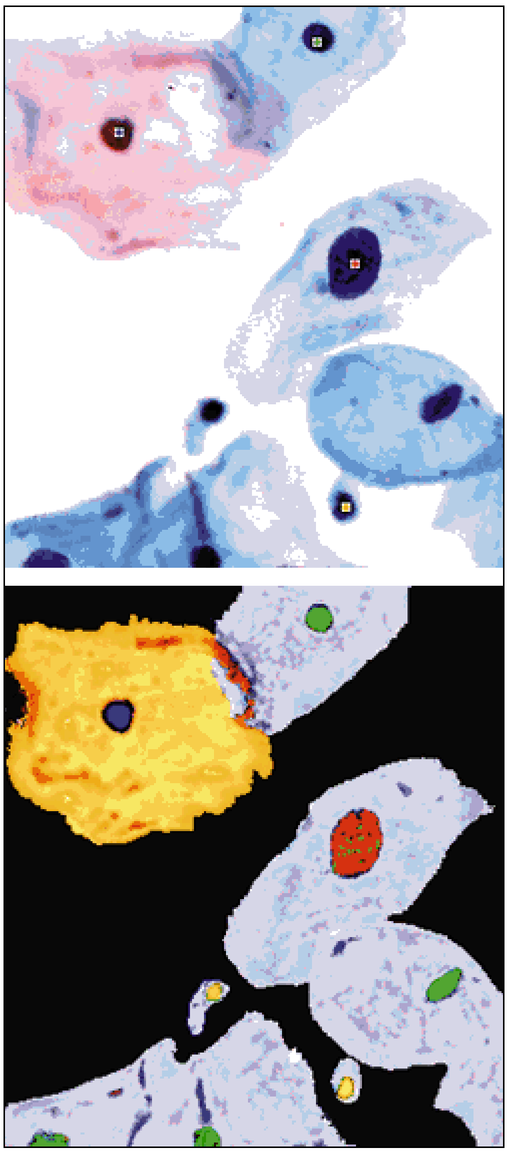

Figure 5.7: The top image is a micrograph of stained epithelial cells. The bottom micrograph is the same image with spectrographic classification applied. Source: Taubes, Gary. "Spectral Technique Paints Cells in Vivid New Colors." ***Science*** **276. (1997): 1990**

The first sentence in Taubes' report on the Levenson-Farkas system is also interesting in the light of the history of digital rhetorical mediation. Taubes wrote, "You can think of image enhancement as the art of helping the eye do what it does naturally" (1990). After a decade of high-profile scandals involving "enhanced" images, Taubes' 1998 statement might seem quaintly naïve. However, many would agree that digitally enhanced images are useful for a range of purposes in a variety of contexts. For example, science photographer Felice Frankel often uses digital tools to enhance technical science images and to create inspiring science photographs. Figure 5.8 and Figure 5.9 are reproduced from Frankel's *Envisioning Science: The Art and Craft of the Scientific Image*, a handbook on creating scientific images. In Figure 5.9, Frankel has adjusted the color of a scanning electron micrograph (SEM) of nanowires (Figure 5.8). The color adjustment makes the specific feature of the nanowires and their stringers more visible.

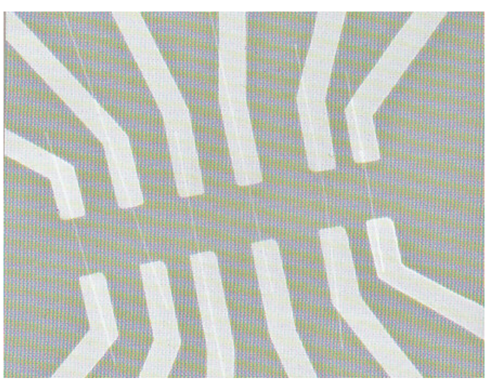

Figure 5.8: Figure 8.28 from Frankel (270).

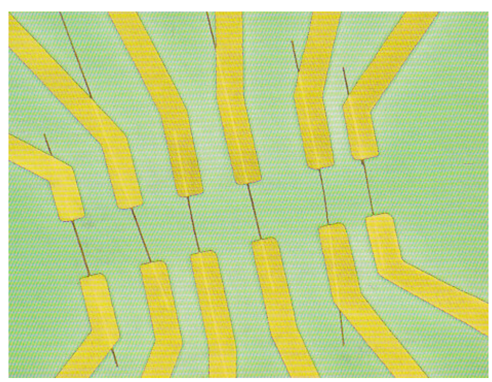

Figure 5.9. Frankel (back cover). A larger version appears as Figure 8.33 of her text (272).

Some of Frankel's comments suggest that she has a highly developed rhetorical sensibility regarding image enhancement. In the following passage from her chapter on digitally altering images, Frankel suggests that the decision to apply artificial color to a SEM should depend on the rhetorical situation:

> Although many researchers digitally color their SEMs, I am not convinced that all images benefit from adding color. If coloring a SEM clarifies the structure of the image or if it helps make the image more attractive, thereby making the work more accessible, then you should consider doing so. (269)

"More accessible" can be read as "more persuasive." Frankel also recognizes the ethical problems that can accompany image manipulation:

> Think seriously about whether the change, or "enhancement," you make to an image is appropriate. What might be a well-intentioned adjustment to clarify the structures and forms could actually create a significant change in the data showing something new in the image that is not there. (268)

She follows this cautionary advice with several examples of image enhancements that could be ethically problematic. However, Frankel's text seems to skirt many of these ethical issues because, as she acknowledges, her work focuses primarily on digital alterations to "communicative" photographs; i.e., photographs for popular accommodations of science, museum installations, journal covers, etc (268). But as the cases described in my previous chapters show, creative visual enhancements are often vital to the success of scientific persuasion in technical settings. For example, the Goddard researchers creatively enhanced digital photographs to create a conceptual epitome of the twilight zone between clouds and aerosols. This enhancement was a legitimate and thoroughly explained digital alteration that helped convince skeptical audiences.

Still, many scientists struggle to locate the line between legitimate and fraudulent enhancement. *The Journal of Cell Biology* and the journal *Blood*—two journals that are especially vigilant in verifying the integrity of submitted images—have separately reported that more than 20% of submissions include inappropriate figures, though only 1% of all submissions are truly fraudulent (Rossner qtd. in Pearson 953; Shattil 2275). For the 19% of submissions that are not purposeful frauds, the adjustments intended to make good data more persuasive actually compromised their credibility. Thus, there appears to be a significant disconnect between the expectations of journals and scientists' understandings of those expectations; this disconnect is indicated in editorials and reports on digital enhancement in 2005 and 2006. As *Nature* reporter Helen Pearson observed in 2005, "[S]cientists say they feel under pressure to produce faultless images to present convincing experiments that reviewers and editors want to publish" (953). By early 2006, the editors of *Nature* were calling "to end the fetish of the perfect image" (892). My research has not revealed an extensive written record supporting the extent of such a "fetish," though some journal guidelines allude to the clarifying power of digital adjustments. For example, the 1993 guidelines of *Molecular and Cellular Biology* state "computer-generated content can be manipulated for better clarity," but this statement is far from an explicit expectation of "perfect" images ("Instructions" v). However, the *Nature* call to end the perfect-image fetish as well as other statements highlighting the value of "warts and all" images indicate that there were—and perhaps still are—significant misconceptions regarding the ethical rhetoric of scientific images.[43]

This chapter takes a rhetorical approach to ethical problems of arguing with images in science. I argue that rhetorical concepts—such as *enthymemes, ethos, presumptions, values,* and *hierarchies*—can clarify the often tacit relationships between instruments, images, authority, expectations, and ethical disciplinary practices. In examining these relationships, I develop a conceptual framework for approaching scientific visual cultures; such a framework can be useful for future rhetorical analyses and for advising scientists on ethical practices for visual argumentation.

<u>*Document Summary*</u>

This chapter discusses evidence from two sets of documents: (1) editorials and feature stories on visual ethics and (2) journal guidelines on appropriate images. In some cases, these documents are combined; for example, an editor might include selections from new guidelines as part of a commentary on ethical imaging. These kinds of documents were chosen because they make tacit assumptions explicit and thus reveal the expectations of specific scientific visual cultures.

The dates of these documents range from 1993 to 2006. I begin with 1993 because that is the year that *Molecular and Cellular Biology* released new submission

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[43] The following editorials are just a few of the calls for more ethical digital imaging practices: "Gel Slicing and Dicing: a Recipe for Disaster," *Nature Cell Biology,* 2004; "What's in a Picture? The Temptation of Image Manipulation," *The Journal of Cell Biology,* 2004; "Not Picture-Perfect," *Nature,* 2006; "Beautification and Fraud," *Nature Cell Biology,* 2006; "Appreciating Data: Wrinkles, Warts and All," *Nature Cell Biology,* 2006; "Don't Pretty Up that Picture Just Yet," *Science,* 2006.

guidelines on using digital enhancement software. These guidelines were developed during the journal's 1992 editorial board meeting, and they appear to be the first mention of the digital editing of scientific images. I end with 2006, a year when scientific editors were reacting to the wake of the Hwang stem-cell scandal. This scandal made digital visual fraud a salient issue, and hence there was keen interest in (re)establishing clear ethical boundaries with respect to image manipulation.

Unlike the previous chapters, which examined particular cases, this chapter explicates relationships between rhetorical concepts, ethical standards, and technological changes. Though the chapter ends with a brief chronological survey of evolving editorial policies on imaging, most of the chapter draws on textual evidence in the editorial documents to explicate usually unstated conceptual relationships that were brought to the surface during this period of technological change.

Images, Ethos, Ethics, and Enthymemes: An Examination of Interrelated Concepts

This chapter examines explicit statements about imaging ethics to reveal typically unstated assumptions about images, ethics and, argumentation. My starting premise is that the explicit descriptions of the relationship between images and ethics rests on four other conceptual relationships: (1) the relationship between the enthymeme and the scientific visual, (2) the relationship between the scientific visual and the ethos of the author, (3) the relationship between ethos and ethical norms, and (4) the relationship between ethics and enthymemes. Diagram 5.1 is a visualization of these relationships. In the diagram, each circle represents a rhetorical concept of interest; each dotted line represents the relationship between two concepts. As indicated in the diagram, these relationships have been elaborated separately by rhetorical theorists. Some of these rhetorical critics approach the concepts in the light of broadly defined rhetorical situations whiles other approach scientific situations specifically. Each of these treatments is summarized or elaborated in the subsections of this review.

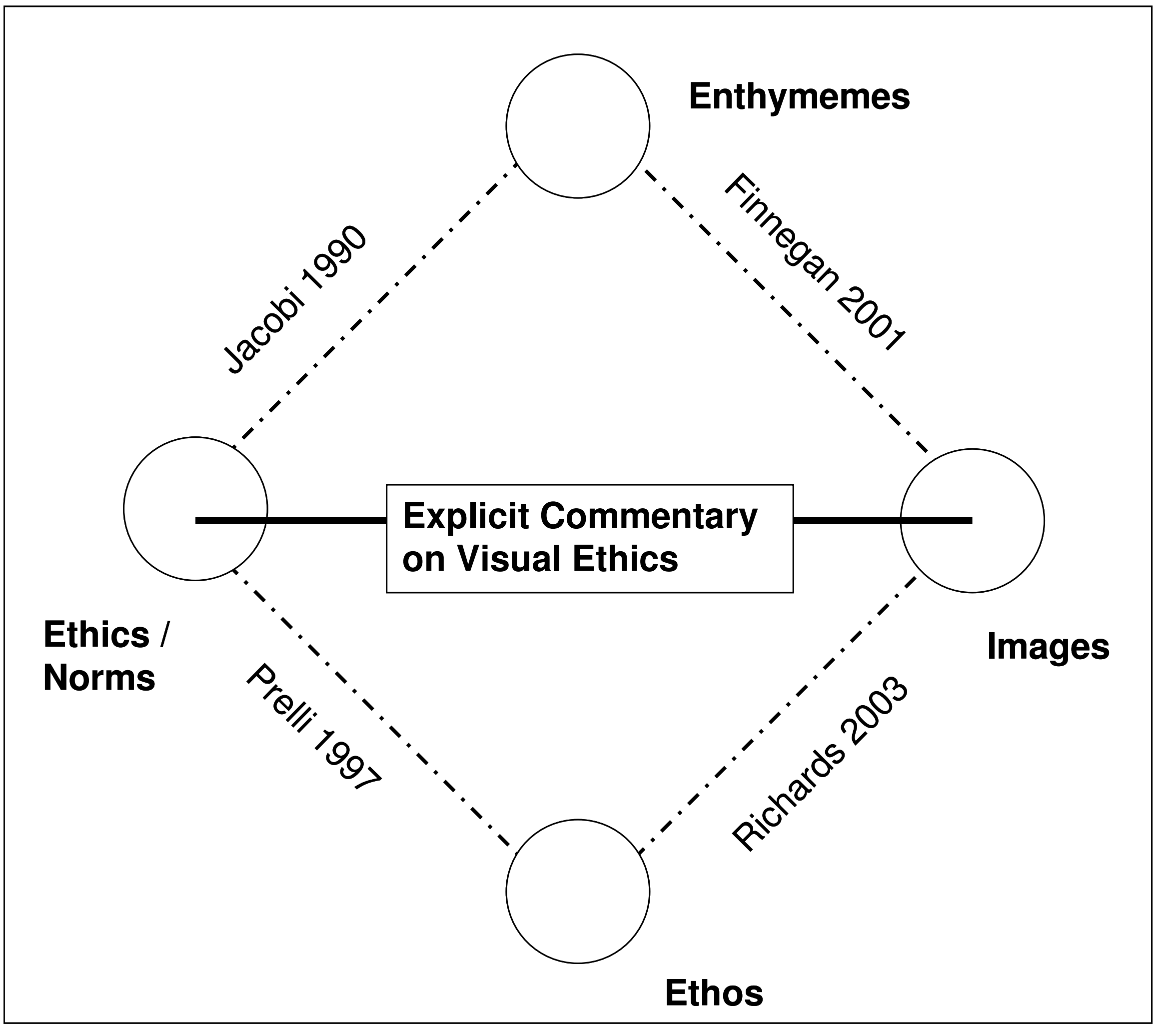

Diagram 5.1: A visual representation of the argument for this chapter: Explicitly stated expectations about images and ethics reveal a number of relationships that are often implicit.

Enthymemes and Images

As noted elsewhere in this dissertation, the enthymeme can be a productive concept for explaining visual arguments. The enthymeme, as Aristotle describes it, is a rhetorical syllogism—an informally logical argument supported by unstated, uncertain, or probabilistically acceptable premises (i. 2. 1357a-b). Rhetors use enthymemes to argue in situations in which audiences do not need or want every premise stated explicitly. Enthymemes can be powerful tools of argument because audiences activate implicit assumptions. As James Raymond observes, "Because enthymemes presume upon what an audience already knows or believes, they can express in a condensed or elliptical manner chains of logical connections that would be complex indeed if the assumptions themselves had to be demonstrated" (144). Thus, enthymemes are economical as well as powerful.

Cara Finnegan applies the concept of the enthymeme to documentary photographic arguments.[44] She explains that photographs succeed as "naturalistic" arguments—as representations of the real world—when three unstated premises are acceptably satisfied for the audience. Finnegan labels these premises *ontological*, *representational*, and *mechanical* realism. That is, these arguments succeed when viewers have no reason to doubt that (1) the photograph depicts a scene that really existed, (2) the depicted scene is of the subject that the author claims it depicts, and (3)

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[44] Finnegan is not the only scholar to note the role of enthymemes in visual processes. Keith Niall notes that geometrical figures also rest on networks of unstated propositions. Specifically, he notes that for centuries Euclid's first theorem was not visually "proven" by the graphic used to represent it; the viewer had to fill in a number of unstated propositions to derive meaning from it. Niall asserts, "The original proof of Euclid's first theorem is incomplete, and this gap in logic is undetected by visual imagination. While cognition involves truth values, vision does not: the notions of inference and proof are foreign to vision" (202). Niall is not a rhetorician, and he is looking at these issues from the perspective of cognitive science. Nonetheless, his more philosophical account of the enthymeme in visual perception and cognitive processing suggests interesting connections between rhetoric, perception, and the workings of the brain.

human intervention has not interfered with the "normal" production of the image ("The Naturalistic Enthymeme" 143). As Finnegan demonstrates through her account of the infamous Farm Security Administration (FSA) "skull" photographs,[45] an explicit challenge to any of these propositions can refute a photograph's claim to represent the real.

In later work, Finnegan extends her enthymemic conceptualization of photographs through the term *image vernaculars*—"…the enthymematic modes of reasoning employed by audiences in the context of specific practices of reading and viewing in visual cultures" ("Recognizing Lincoln" 34). Finnegan uses the concept of the image vernacular to account for the ways in which letter writers responded to a rediscovered photograph of Abraham Lincoln published in 1895. These texts reveal tacit assumptions regarding facial physiognomy, moral character, ethnicity, and national identity at the end of the nineteenth century. Viewers brought a set of assumptions to the viewing of the photograph, and they reveal this enthymemic viewing when articulating equally enthymemic verbal arguments.

Finnegan's terms and methods are conceptually rich; however, she focuses specifically on photographs and specifically on broad instantiations of visual culture. Considering the theoretical machinery of visual enthymemes and image vernaculars in the light of other kinds of images and more narrowly defined visual cultures raises some interesting questions. If the enthymeme underlies the structure of "naturalistic" photographic arguments in documentary situations, can it structure similar visual arguments making claims about reality, such as scientific visuals? Can an enthymemic

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[45] In 1936, FSA photographer Arthur Rothstein took a set of photographs of a sun-bleached cow skull in the South Dakota badlands. The photographs were used in newspaper articles as evidence of the effects of drought, but the images were later discredited. Finnegan's analysis (2001) of this case is superb.

account apply to other kinds of scientific visual artifacts that reference reality indirectly through physical and mathematical tools that transcribe features of the natural world? How can we account for the tacit parameters of the image vernaculars of scientific visual cultures to teach future scientists how to produce both creative and ethical visual arguments?

Starting from Finnegan's work, I offer a generic schema for an *instrumental enthymeme* for scientific visuals that can begin to approach these questions. Such an enthymemic frame must be generic since the matrix of unstated propositions will be context dependent, field dependent, and instrument dependent. That is, the unstated and provisionally acceptable propositions for a given visualization depend on the physical, mathematical, and graphical conditions by which data are collected, processed, and reproduced; the success of the argument also depends on the audience's conscious or unconscious acceptance of those conditions. For example, a magnetic resonance map of the sea floor rests on tentatively established premises about magnetism and its capacity to be measured through the proton-precession process. The unstated and accepted propositions of this instrument and the mathematical premises that convert magnetic data into a visualization are different from the tentatively accepted premises governing the collection and processing of sun photometer data about atmospheric aerosols. However, despite such differences, understanding the "instrumental" enthymeme at a generic level can provide a framework for explaining the successes, failures, and frauds of scientific visuals.

Like Finnegan's *naturalistic enthymeme*, what I call the *instrumental enthymeme* is dependent on assumptions of (1) ontological, (2) representational, and (3) mechanical integrity. (I substitute the word integrity for realism because I think it better reflects the expectations of science.) The reader assumes that the image or visualized data (1) refers to a phenomenon that really existed and (2) that the graphic represents what it claims to represent. Though mechanical integrity (3) is also a component, this concept becomes more complicated in scientific imaging situations. Finnegan's mechanical realism refers specifically to the material circumstances of staging and producing photographs; however, scientific photographs are usually staged, especially in experimental contexts. Thus, the mechanical integrity of the "real" scientific image is dependent on accepted assumptions of the integrity of the imaging process. In contrast, the documentary photograph's mechanical realism largely rests on the assumption that the camera captured a candid moment. Moreover, the "reality" claims of some scientific images depend on additional components, such as mathematical transformations. Thus, viewers expect that instrument "data"—whether they are patterns of light encoded chemically (or digitally) in a photograph, indices of particles counted by a scintillation counter, or signs of some other phenomena—are collected and transformed into visual form according to accepted principles. Mechanical integrity could also consider the principles of graphical integrity articulated by Tufte (1983).[46] For example, Tufte notes, "The representation of numbers, as physically measured on the surface of the graphic itself, should be directly

[46] In Visualizing Quantitative Information, Edward Tufte outlines six principles of graphical integrity that should be considered when making honest representations of numerical data:

> (1)The representation of numbers, as physically measured on the surface of the graphic itself, should be directly proportional to the numerical quantities represented. Clear, detailed, and thorough labeling should be used to defeat graphical distortion and ambiguity.
> (2) Write out explanations of the data on the graphic itself. Label important events in the data.
> (3) Show data variation, not design variation.
> (4) In time-series displays of money, deflated and standardized units of monetary measurement are nearly always better than nominal units.
> (5) The number of information-carrying (variable) dimensions depicted should not exceed the number of dimensions in the data.
> (6) Graphics must not quote data out of context. (77, my enumeration)

proportional to the numerical quantities represented" (77). While Tufte presents his "principles of graphical integrity" as prescriptions, they (or contextual variations of them) can also be considered as expectations—components of enthymemes. That is, scientists expect that represented data were not deceptively distorted, even if the data are mathematically altered either by necessity or for presentational/argumentative purposes.

A plausible distinction between Finnegan's *naturalistic enthymeme* and what I call the *instrumental enthymeme* concerns the epistemological relationship between images, time, and novelty. The documentary photograph, whose persuasive "naturalness" rests on the *naturalistic enthymeme*, can only represent a singular moment in time. The scientific visual can reflect a specific moment, but it can also reflect multiple moments, as is the case with visualized data measured in the time domain. More importantly, most scientific images tacitly promise to be reproducible. For most experimentally derived visuals and many observationally derived visuals, viewers assume that the same visual artifact would be produced were the experiment to be repeated under the same controlled conditions or if the data were collected under similar field conditions. In other words, unlike the image supported *naturalistic enthymeme*, which must be unique to be real, the scientific image cannot be unique if it is to be received as real.

Two good examples that demonstrate the possibility of specific *instrumental enthymemes* come from the *twilight zone* case described in Chapter Four. In both cases, the inscribing instruments are observational rather than experimental. First, Koren et al. initially failed to persuade their peers because their work seemed to defy the expectations of representational and mechanical integrity. Reviewers and local colleagues were skeptical of the visualized AERONET data because it was presented in terms of variables that were not measured by that instrument. The aerosol data in the graph were presented in terms of distance from the nearest cloud; AERONET only measures in terms of time. The authors were making a simplifying assumption about rate of movement for argumentative purposes; however, reviews and local reactions indicated that this transformation was not acceptable, even though the premise governing the data transformation was explained in the text. Thus, even though these authors were presenting valid data, the means of the presentation pushed against the expectations of the audience. As Lorraine Remer explained, this small change was significant for her colleagues at Goddard who worked closely with AERONET:

> The AERONET people who are up here [at Goddard] were very critical of our first draft, and a lot of the changes we made were because of their comments because (1) they're our friends and we want to remain friends and (2) they know their data a lot better than we do. But we went over that hump, and *it was as simple as staying in the time domain*, and everybody was friends again, and now they are supporting our paper. (My emphasis)

Remer's comment indicates that graphical conventions and graphical choices have significant rhetorical, personal, and political implications.

A second possible example of a visual enthymeme in the *twilight zone* case is suggested by the authors' awareness of the unstated premises governing how others would view their MODIS data. Specifically, an understood "fact" of the MODIS device was that its aerosol reflectance data were unreliable near clouds. Koren and Kaufman knew that any expert viewer considering a visual argument based on MODIS data near clouds would dismiss the argument. That data did not have ontological integrity; therefore, any data pattern would be considered an "artifact" of the instrument. In the contemporary image vernacular of atmospheric-aerosol scientists, that data did not "really" exist. Koren, Kaufman, and Remer sought out ways to give that data ontological status through independent instrumental verification (i.e., the AERONET data) and conceptual priming (i.e., the cloud photo series).

Ethos, Norms, and the Scientific Image

In "The Rhetorical Construction of Scientific Ethos," Lawrence Prelli juxtaposes the "norms" of scientific ethos proposed by Merton—universalism, community, disinterestedness, organized skepticism, originality, and humility—against the identified counter-norms identified by Mitroff and others—particularism, solitariness, interestedness, and organized dogmatism (87-89). This juxtaposition allows Prelli to show that the scientific ethos is constructed contextually; scientists activate specific topoi to project an appropriate and effective ethos for specific situations. When a norm-based ethos is not rhetorically successful or expedient, a scientist can project a more revolutionary ethos based on counter-norms.

I summarize Prelli's work on scientific ethos in general as a starting place for thinking more specifically about ethos, scientific visuals, and image enhancement. In an idealized conception of science, the scientific image should reflect the norms identified by Merton. A scientific visual should be an original but universally reproducible artifact created by communally sanctioned means that limit the biases of the scientists producing a falsifiable claim for a skeptical audience. Conceivably, the "enhanced" image could align with Mitroff's counter-norms if the enhancements are undocumented (and hence irreproducible), inconsistent with community standards, or biased toward preformed conclusions even in light of plausible counterarguments.

As one might expect, editorial reactions to image manipulation indicate a strong affinity for the Mertonian norms, and their characterizations of unethical practices correspond to the counter-norms. However, as other rhetorical theorists have observed, the relationship between ethos and images in science is more complicated than just consistent conformity to norms. Some of these complicating factors can explain why scientists are motivated to manipulate their images.

As Anne Richards has shown, there is a reciprocal relationship between images and the authority of scientific knowledge claims; images can build authority, and authority can bolster the persuasiveness of images. Richards shows that the quality and number of visuals in a botany article can build the credibility of the author. She also shows that otherwise uncompelling or even potentially questionable images will be acceptable if the author is well respected. In one of the samples read by the subject in her think-aloud protocol, a botanical drawing was inserted but not identified in a collage of botanical photographs.[47] Such a collage is a typical graphic practice for this discipline, but mixing media was not typical. The expert reader participating in Richard's study did not find the insertion to be problematic because the authors were respected botanists (200-202). Thus, the scientists' ethos preserved the "truth value" of the image even though it was a drawn construction.

[47] In this context, a collage is a composite of multiple images and not a glued assemblage of ephemera.

Tacit assumptions about the relationship between image quality and scientific ethos are sometimes explicated in editorial commentaries on imaging ethics. In 2004, two editors of *The Journal of Cell Biology* allude to these implicit relationships:

> The quality of an image has implications about the care with which it was made, and a frequent assumption (though not necessarily true) is that in order to obtain a presentation-quality image you had to carefully repeat an experiment multiple times. (Rossner and Yamada 11)

Essentially, these editors invoke two concepts that are well explained in *The New Rhetoric*: *presumptions* and the rhetorical *index*.[48] In this case, a common presumption identified by Perelman and Olbrechts-Tyteca— "the quality of the act reveals the quality of the person responsible for it"—clearly pertains (70). That is, the quality of the image reflects on the quality of the science and hence the scientist. A "clear" or "faultless" image entails clear science and hence a conscientious scientist; a sloppy image entails sloppy science and hence a careless scientist. Regarding the rhetorical index, a "presentation-quality" image can serve as an index not just of a single experiment but also of the multiple replications of it. Thus, the "clear" image can indicate the thoroughness once required to obtain a presentation-quality image. Though Rossner and Yamada note that this assumption is not always true, in the light of their comment it is clear why scientists are tempted to "beautify" their visual arguments despite calls for

[48] In the system of Perelman and Olbrechts-Tyteca, *presumptions* are universal objects of agreement like facts and truths, but adherence to presumptions is "less than maximum" (70). A presumption obtains so long as there is no evidence to the contrary. For example, we presume that a speaker is telling the truth as long as we are given no reason to doubt his or her honesty (71).

The rhetorical *index* is like a *sign* in that it is a "phenomenon capable of evoking another phenomenon"; however, the index "makes it possible to evoke another phenomenon in what we may call an objective manner, independently of any intentionality" (122-123).

"warts and all" artifacts. So much is riding on their images—including the perception of credibility.

The observations of Richards and the comments from the editors of *The Journal of Cell Biology* suggest that there are multiple values associated with scientific images and that perhaps digital technology has affected the relationship between those values. With digitally generated and manipulated images, values of verisimilitude and clarity now compete for hierarchical supremacy. Presumably, before the advent of digital manipulation, these two values were inseparable or at least coincident in the minds of the scientists. The clear or "faultless" scientific image accurately recorded a scene as it existed. As one editorial observes, this verisimilitude was a product of available technology:

> In days gone by, whether we liked it or not, data acquired at the bench were not much different from what was published. In a biomedical lab, for example, samples that had been radio-labeled and separated on a gel were recorded on X-ray film. Composite figures were assembled, with lettering carefully placed around the mounted film. If a control was forgotten or a gel was uneven, the graduate student or postdoc was sent back into the lab to get it right "for publication." If a speck of dust on the film obscured data in the original photograph, another picture was taken. Slicing films to rearrange the order of samples, or to splice in a control group that was actually part of another gel, was not common because it took almost as much skill to do that as to rerun the experiment. ("Not Picture-Perfect" 891)

Digital tools decoupled the value of verisimilitude from the value of clarity, which allows these values to be arranged (consciously or unconsciously) into a hierarchy. In ranking these values, some scientists rank clarity above verisimilitude. They remove "artifacts" not contributing to their arguments even if these features were legitimate, or perhaps, contradictory parts of the scene. The rhetorical project of many scientific editors has been to re-establish verisimilitude as the preferable value in the hierarchy. Such an activity is demonstrated in the following passage from an editorial *Nature Cell Biology*:

> There is a myth that editors only like clean data that show striking effects.[49] What we actually like is solid data that provides striking conceptual advances. Effects may be small, but statistics and controls are needed to make them believable. Data should be clearly presented and concise, but not at the expense of important information. Let's celebrate real data — wrinkles, warts and all. We want to publish gritty documentary movies, not digitally beautified yarns!
> ("Appreciating Data" 203)

This passage is representative of a host of similar arguments made in journal editorials. These documents have become more prevalent in recent years (2002–2006). It is unclear if such arguments are changing the visual rhetorical practices of many scientists, though there is some anecdotal evidence of change. For example, in the following passage, a scientist interviewed for a 2006 feature on image manipulation describes changes in his rhetorical behavior:

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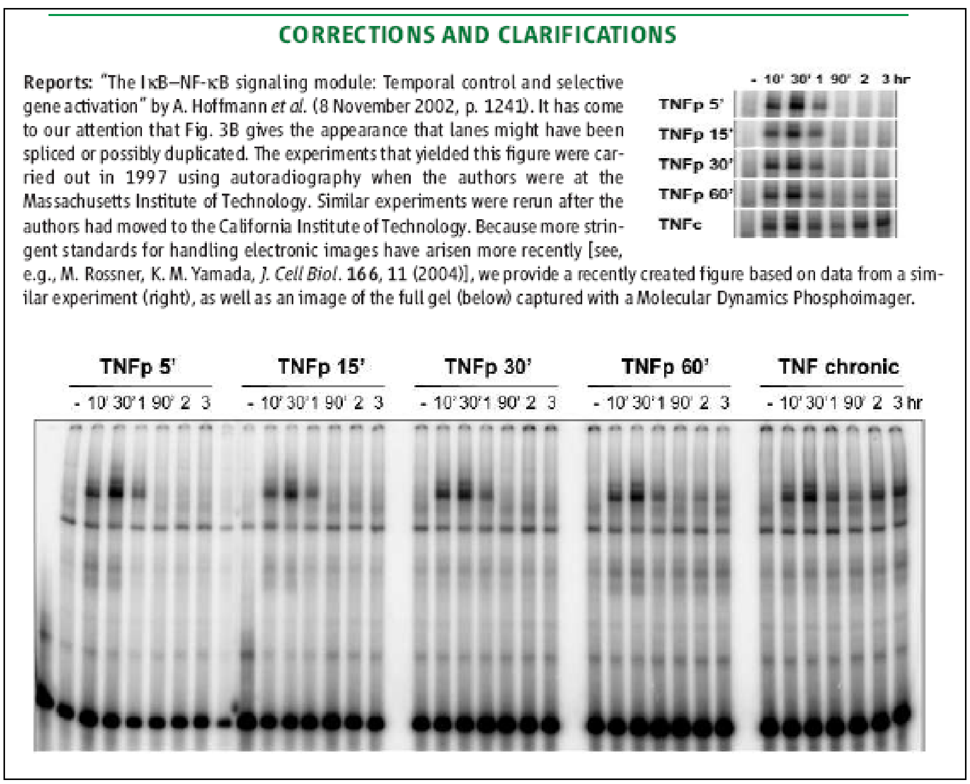
[49] In this context, data includes images. For example, the following sentence appears in the paragraph preceding the passage that is excerpted above: "For now, we have focused on two types of *data* that are subject to manipulation most often: gels and immunoblots, and light microscopy" ("Appreciating Data" 203, my emphasis).

> Hayden… wrestles with the fine line between appropriate and misleading image alterations. "We've all seen gels that look like a complete disaster," with "splotches" everywhere from artifacts related to processing. "In the past, I would take those out," says Hayden. "I wouldn't do that now." (Couzin 1866)

Some scientists are even correcting much older images in light of newer standards. The following passage from a 2007 correction of a 2002 *Science* article describes why the authors needed to update an image from 1997:

> It has come to our attention that Fig. 3B gives the appearance that lanes might have been spliced or possibly duplicated. The experiments that yielded this figure were carried out in 1997 using autoradiography when the authors were at the Massachusetts Institute of Technology. Similar experiments were rerun after the authors had moved to the California Institute of Technology. Because more stringent standards for handling electronic images have arisen more recently [see, e.g., M. Rossner, K. M. Yamada, J. Cell Biol. 166, 11 (2004)], we provide a recently created figure based on data from a similar experiment (right), as well as an image of the full gel (below) captured with a Molecular Dynamics Phosphoimager.
>
> ("Corrections and Clarifications," 1550, bracketed phrase in original)

This passage is interesting with respect to the relationship between ethos and images. In this case, there is no suggestion of fraud or misconduct; the correction does not change the results described in the report. However, the authors still felt compelled (1) to correct the image and (2) to insert additional markers of their credibility into the correction by identifying the names of their institutions, two premiere American research universities. Moreover, they attempt to restrain the act-person association that a sloppy image equals a sloppy scientists by emphasizing (1) the age of the image, (2) the fact that the experiment had been reproduced, and (3) the circumstance that imaging standards are now different than they were when the article was published. Finally, they reproduce a new version of the original figure, but they also include an image of the entire gel; thus, they provide more visual evidence than was initially published to demonstrated that the original results are credible (Figure 5.10).


CORRECTIONS AND CLARIFICATIONS

Reports: "The IκB–NF-κB signaling module: Temporal control and selective gene activation" by A. Hoffmann *et al.* (8 November 2002, p. 1241). It has come to our attention that Fig. 3B gives the appearance that lanes might have been spliced or possibly duplicated. The experiments that yielded this figure were carried out in 1997 using autoradiography when the authors were at the Massachusetts Institute of Technology. Similar experiments were rerun after the authors had moved to the California Institute of Technology. Because more stringent standards for handling electronic images have arisen more recently [see, e.g., M. Rossner, K. M. Yamada, *J. Cell Biol.* **166**, 11 (2004)], we provide a recently created figure based on data from a similar experiment (right), as well as an image of the full gel (below) captured with a Molecular Dynamics Phosphoimager.

Figure 5.10. From "Corrections and Clarifications." *Science*. 7 December 2007: 1550

This retraction and Hayden's comments suggest that editorial responses to image manipulation have been successful. With time, more extensive reception evidence should become available. This evidence should allow rhetorical scholars to verify the long-term effects of the editorial messages that have attempted to negotiate the value hierarchy of digital images.

Ethics and Enthymemes

Martin Jacobi (1990) elaborates on the connection between the enthymeme and ethics in the context of professional writing instruction. Though he is interested in professional discourse in general and not in visual arguments in science specifically, Jacobi's comments offer insight into the use of enthymemes in the ethical practices of professional discourse communities. Specifically, Jacobi advocates using the enthymeme to "reintegrate ethics" into professional writing instruction. He uses the concept of the enthymeme to guide students through the process of determining the shared assumptions of professional audiences. His students then use knowledge of those shared assumptions in creating arguments based on enthymemes. Jacobi explains the benefit of this approach:

> The crucial benefit of the enthymeme…is that it avoids the opportunity, available when rhetoric is perceived as a technology, of having no investment in what is said. Rather, it requires writers to think about values. Its success as a means of revitalizing ethics rests in its requirement

> for coductive truths and values, truths and values that are—and ought to be—shared by the writer and the audience.[50] (285)

Visual arguments in science rest upon shared truths and values. Scientists expect visual arguments that accurately reflect the situation described in the text, and they expect that those visuals are generated by reliable means of representation. What counts as an ethically appropriate visual mediation will vary by discipline, but the general descriptors of scientific misconduct—fabrication, falsification, and plagiarism—provide a good baseline. The ethically produced scientific text presents original, *real* results that have not been modified inappropriately. Appropriateness, however, is a relative term, and its value depends on the assumptions about "appropriate" practices shared by members of the discourse community. But these shared assumptions are often unstated, and this lack of explicit guidelines can lead to ethically ambiguous situations. The following anecdote, included in a *Science* feature story on imaging ethics, demonstrates the ambiguity that can emerge from the enthymemic nature of visual arguments in science:

> A member of [a] biologist's lab had created a collage of images. Editors of the journal in which it was published, tipped off by a reader, deemed the collage unethical because it was presented as a single picture. A correction was subsequently published, noting that the modification, although inappropriate, did not affect the paper's conclusions. "We thought it was obvious that it was a collage," says the biologist, adding that he vehemently opposes altering images. (Couzin, "Don't Pretty" 1867)

[50] Jacobi uses Wayne Booth's definition of coduction, which Jacobi paraphrases as "the comparing of one's experiences and beliefs with those of other qualified people" (281).

This anecdote suggests that a range of stakeholders approach visuals with different assumptions. The image was peer reviewed and published, so at least three people saw and "approved" the collage.[51] One cannot know if each of these readers thought the figure was a collage or a single image. That is, did they share the authors' assumption that the collage was intuitively obvious, or did they just not notice or not care? However, some readers of the published piece found the image inappropriate; it needed elements to divide the visualized objects that were presented together. The biologist and his team are portrayed in the narrative as ethical and responsible scientists who failed to consider how their visual would be received. They failed to situate the image in a way that was consistent with the visual enthymeme governing some readers' expectations about a specific class of visual artifact. This failure created an awkward situation; they had to publish a correction to their paper with an updated figure (Couzin 1867)

Clearly, scientists need rhetorical guidance when planning and executing visual arguments. Individual journals often provide specific advice, and some journal guidelines are quite specific about what is acceptable for different image types and data visualizations. For example, a 2004 editorial in *The Journal of Cell Biology* reiterated guidelines that would apply to the "collage" situation discussed in the previous paragraph. After demonstrating an unethically spliced micrographic collage (see Figure 5.11), the editors explain how images can be combined appropriately and within rhetorical constraints: "You may want to combine images from several fields into a single micrograph to save space, but this assembly should be clearly indicated by thin

[51] In this context, a collage is a digital image in which portions of several images are combined in a single field; i.e., it is a composite photograph. The top image in Figure 5.10 is an example of an unethical collage. If there were clear demarcations between the cells from different images, it would be an ethical and acceptable image.

lines between the different pieces"(Rossner and Yamada 14). This statement is a less formal wording of the journal's submission guidelines regarding composite images: "The grouping of images from different parts of the same gel, or from different gels, fields, or exposures must be made explicit by the arrangement of the figure (e.g., dividing lines) and in the text of the figure legend" (Rossner 837). Following these guidelines would preserve the ontological and representational integrity of the image; the marked image would reflect the "reality" of what was actually observed and depict what the author claims that it depicts. The image would be an explicitly marked micrographic collage.

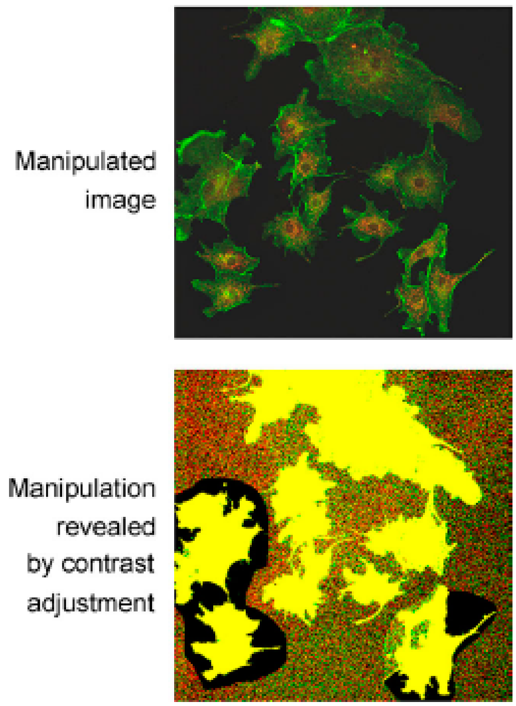


Figure 6. **Misrepresentation of image data.** Cells from various fields have been juxtaposed in a single image, giving the impression that they were present in the same microscope field. A manipulated panel is shown at the top. The same panel, with the contrast adjusted by us to reveal the manipulation, is shown at the bottom.

Figure 5.11: Figure 6 from Rossner and Yamada, 2004. The figure demonstrates how misrepresentations of image data can be revealed through digital forensics.

Similarly, the editorial policies of the journal *Nature* now stipulate specific and extensive requirements for visual artifacts of microscopy and electrophoretic gels and blots (*Nature* "Image Integrity"). I will discuss these and other guidelines later in this chapter, but first I examine how an understanding of enthymemes can help rhetoricians and scientists to think broadly about ethical visual conduct in science.

Enthymemes and Standards of Ethical Conduct in Science

As mentioned previously, there are three main categories of scientific misconduct: fabrication, falsification, and plagiarism. The National Science Foundation defines fabrication as "making up data or results and recording or reporting them." It defines falsification as "manipulating research materials, equipment, or processes, or changing or omitting data or results such that the research is not accurately represented in the research record." Plagiarism is "the appropriation of another person's ideas, processes, results or words without giving appropriate credit" (NSF). These categories are used by ORI and other authorities that investigate research misconduct. Juxtaposing these terms with the components of the visual *instrumental enthymeme* is one way to conceptualize the ethical boundaries of visual argumentation in science.

Table 5.1 demonstrates the relationships between a violated assumption of the *instrumental enthymeme*—ontological, representational, or mechanical integrity—and the specific types of ethical misconduct. Fabrication contradicts the proposition of ontological integrity; a visual artifact refers to something that did not exist. Falsification contradicts the proposition of mechanical integrity; a visual has been modified in an excessive way through embellishment or omission; thus, it is not an accurate depiction of the visualized object or data as indexed by the instrument. Both fabrication and falsification can also contradict representational integrity; a falsified or fabricated visual does not necessarily represent what the author claims it represents.[52] Plagiarism

[52] It is possible that a falsified or fabricated image could maintain representational integrity. For example, an ornithologist could claim that a fabricated photograph depicts a new species of bird. Technically the photograph does not violate representational integrity because indeed it does *represent* a new species; however, such an argument would lack ontological integrity, and a photographic argument without ontological warrant is still a refuted argument.

contradicts representational integrity; a figure plagiarized from the work of another researcher could not possibly represent what the author claims it represents if the graphic is not cited appropriately.

	Ontological Integrity	Representational Integrity	Mechanical/Mathematical/ Graphical Integrity
Fabrication	X	X	
Falsification		X	X
Plagiarism		X	

Table 5.1: Ethics violations and the *instrumental enthymeme*.

Considering the ethical facets of each component of the *instrumental enthymeme* can help clarify the line between what can only be outright fraud and modifications that are liminally ethical and thus rhetorically interesting. Situations in which ontological or representational integrity are compromised are clearly fraudulent in scientific contexts. This is the case with each of the most egregious frauds mentioned previously in this chapter. Li presented mouse cells as human cells, violating the assumption of representational integrity. Leadon and Hwang each presented visuals that did not represent ontologically real phenomena. What Leadon presented in his autoradiographic films never existed in that form. Hwang's composite images of cells were never "real." However, in cases involving only mechanical "misconduct" the line between appropriate and inappropriate modification is less clear. For example, Figure 5.12 shows how contrast adjustments can be either ethical clarifications or mechanical misconduct depending on the extent and results of the adjustment. As Rossner and Yamada observe,

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It is interesting that representational integrity is a component of each kind of misconduct. The assumption that an image is what the author says it is speaks directly to issues of credibility and ethos.

the change in contrast from B1 to B2 is acceptable because no data is obscured; however, the change from B2 to B3 is not acceptable because specific features are obliterated. Hence, the image in B3 has lost ontological integrity because removing data has created an image that no longer depicts what really existed. The image has become a falsification.

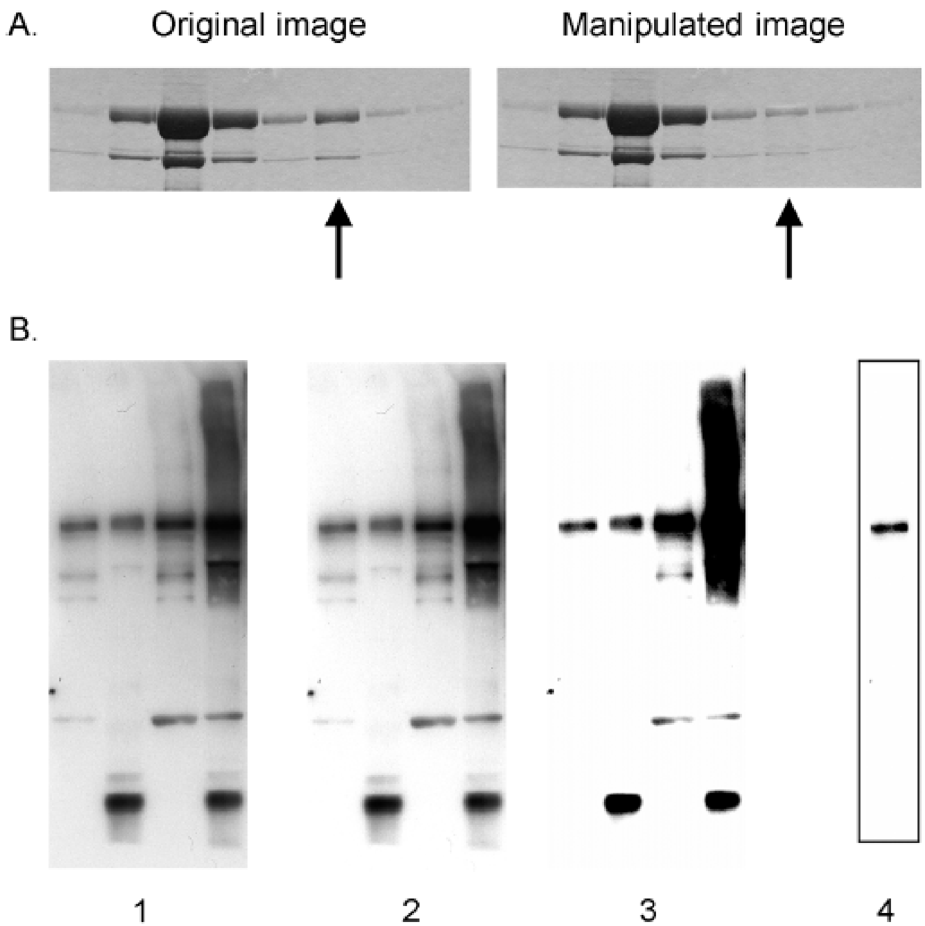

Figure 3. **Manipulation of blots: brightness and contrast adjustments.** (A) Adjusting the intensity of a single band (arrow). B) Adjustments of contrast. Images 1, 2, and 3 show sequentially more severe adjustments of contrast. Although the adjustment from 1 to 2 is acceptable because it does not obscure any of the bands, the adjustment from 2 to 3 is unacceptable because several bands are eliminated. Cutting out a strip of a blot with the contrast adjusted provides the false impression of a very clean result (image 4 was derived from a heavily adjusted version of the left lane of image 1). For a more detailed discussion of "gel slicing and dicing," see *Nature Cell Biology* editorial (2).

Figure 5.12: Demonstrations of brightness and contrast adjustments. Source: Rossner, Michael and Kenneth M. Yamada. "What's in a Picture? The Temptation of Image Manipulation." ***The Journal of Cell Biology.*** **166 (2004): 11-15.**

Images, Ethics, and the Objects of Agreement

In *The New Rhetoric* Perelman and Olbrechts-Tyteca explain that all arguments are based on common starting places, what they call the objects of agreement. These objects of agreement include facts, truths, presumptions, values, hierarchies, and loci. I have already used some of these terms to account for ethical issues in the visual rhetoric of science. For example, the notion that the quality of an image is indicative of the quality of a scientist rests on a common presumption: the nature of an act reflects on the nature of the person who performed the act. One could argue that the various digital-imaging policies of science journals are attempts to maintain the status of specific premises as objects of agreement. I use the case of digital photography as an example.

Before the development of digital imaging and image manipulation software, photographs had a relatively stable epistemic status. Of course, there were plenty of "retouched" photographs before digital imaging, and as many critics have observed, the photograph has never been a transparent window to the real. However, as previously cited comments indicate, for science, the photographic process (not necessarily any specific photograph) was a *truth*—an uncontested reference to the real based on a network of facts that would be accepted by any member of the universal audience. In this case, the *truth* of photography is based on an interlocking network of physical and chemical facts. Despite McLuhan's comments about photographic deceit, the traditional photograph was and is epistemically reliable (though not infallible) from a rhetorical perspective, as long as the premises of Finnegan's *naturalistic enthymeme* are not challenged. In a scientific context, this reliability also rested on the beliefs that modifying traditional photographs is relatively difficult and that scientists are relatively honest.

Enter digital imaging. Digitally produced images look like regular photographs; however, with the exception of some shared physical facts (for example, they both inscribe reflected light), each medium rests on different networks of facts and activates different sets of probabilities. For instance, one could assume digital photographs will be modified because it is easy to modify them or because they can be modified unconsciously. Images can also be modified with far greater subtlety than regular photographs, making the adjustments harder to spot with mere visual inspection. A fraud that is difficult to detect is more likely to occur.

When the two media shared the same space, it was necessary to differentiate them. As the next section explains, in the 1990s the first response of journals was to require that information about software and hardware be added to the figure captions of digital images. Separating digital from analog marked the digital as different and preserved the epistemic status of the analog image.

The development of complete digital workflows and increased evidence of unethical image adjustments led to more extensive and more specific requirements. These new guidelines can be interpreted as requirements that help authors, editors, and reviewers separate fact from claim. For example, a small contrast adjustment made to an entire image is a fact, especially if it is documented in some way. This fact interacts with the network of facts that support the reception of an image as the record of a "real" event. A selective contrast adjustment—one that is applied to only part of an image—is an argument. It gives greater or lesser visual weight to specific features of the image; it might minimize or obliterate important information. Essentially, the adjustment applies an additional terministic screen to the image. If a selective contrast adjustment is made but not acknowledged in the text, the author is passing off an arguable premise as a fact. Thinking about visual arguments in relation to the objects of agreement helps to explain why some editors were compelled to promote ethical digital imaging practices so explicitly.

Enthymemes, Ethos, and Editorial Reactions in the Age of Photoshop

Adobe's Photoshop was released for the Apple Macintosh operating system in 1990, and soon thereafter it was available for Microsoft Windows (1992) and Sun Microsystems' Solaris operating system (1993) (Story "From Darkroom to Desktop"). By 1993, the popular image-editing program and others like it were already influencing scientific editorial policy. During their 1992 meeting, the members of the publications board of *Molecular and Cellular Biology* decided to include language on digital adjustments in their submission guidelines. These guidelines debuted in January of 1993. Though much of the text is concerned with technical details for submitting reproducible digital images, the guidance document also recognized the positive benefits of digital enhancement:

> Since the contents of computer-generated images can be manipulated for better clarity, the Publications Board at its May 1992 meeting decreed that a description of the software/hardware used should be put in the figure legend(s). ("Instructions for Authors" v).

Compared to later editorial language on image manipulation, this statement is highly positive. Digital manipulation is characterized as a means to achieve greater clarity and not as a means for fraud. But what are the rhetorical effects of meeting the editors' request?

Figure 5.13 is a group of images from a paper that also appeared in the January 1993 issue of *Molecular and Cellular Biology*. The caption follows the 1992 guidelines and frankly states that panels A and B were composed in Adobe Photoshop and Adobe Illustrator. This mere mention does not provide any significant details about any "enhancements," nor did the editors or reviewers expect such details. Presumably, if readers knew that an image was digitally produced, such knowledge could change the modality of the photograph for them; i.e., they might be less certain of the images' claims than if the images were traditional photographs. Yet this does not appear to be the case for *Molecular and Cellular Biology* in 1993; that is, I have not found any direct evidence to indicate that these first digital images were any less persuasive than their chemically produced counterparts. However, some scientists, editors, and other stakeholders were becoming increasingly anxious of the impending age of the digital image, and this anxiety surfaced in the contemporary descriptions of image manipulation.

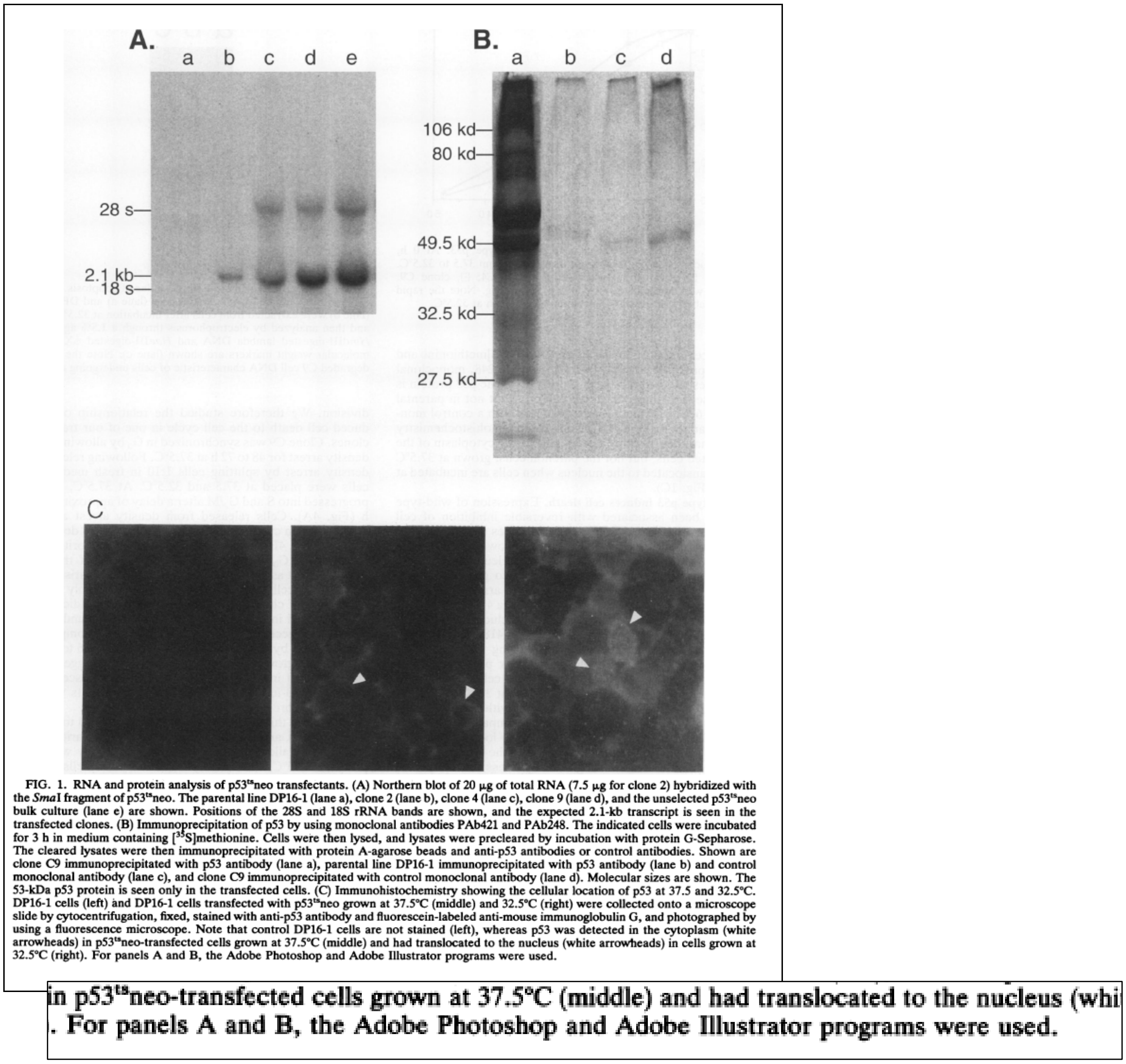

Figure 5.13: Figure 1 from Ryan et al. *Molecular and Cellular Biology,* 1993. Selected text enlarged. NB: This is not an unethical image.

A 1994 "News and Comment" piece in *Science* summarized the debate over digital imaging in the early 1990s. Some scientists were increasingly anxious about digital manipulation; others were not concerned (Anderson 317). At that time, scientific digital fraud was still hypothetical. Major cases of digital fraud had not yet occurred, and many journals had not taken a position on the issue. *Science* reporter Christopher Anderson described the situation:

> So far scientific journals have received only a trickle of digital photographs (as opposed to computer-generated digital images of molecular structures, which are ubiquitous). As a result, only a few journals have set polices for handling digital photographs. *Nature* does not have such a policy, but its two spin-off journals, *Nature Genetics* and *Nature Structural Biology*, have both adopted the requirement that authors submitting a digital image list what software and hardware they used in the methodology section and in the caption. *Science* has discussed but not adopted such a policy…. (Anderson 317)

This passage indicates that there were different tacit standards for different kinds of digitally supported graphics. Digitally rendered models of invisible molecules were "ubiquitous" and ethically unproblematic, whereas even the potential emergence of digital photographs as a visualization technology prompted discussions of ethics and policy. The comment in context also demonstrates that editors were anxious about digital imaging but unclear on how to accommodate it rhetorically; they were not sure how to preserve the epistemic value of scientific photographic arguments with digital imaging as part of the equation. Some journals and associations raised the possibility of more rigorous auditing of digital images. For example, the Council of Biology Editors (CBE) had discussed approaches to ethical issues of digital imaging in 1993; possibilities included the maintenance of electronic histories of the changes made to an image (Anderson 317). However, by 1998 CBE still did not have a written policy on the publication of digital images. A 1998 "CBE Views" article recognized this lack of clear policy, but CBE only suggested that journals request that "… at least the name of the application that was used to create the image should be included in the figure legend" (Rossner et al 189). This suggestion is almost identical to the suggestion in the *Molecular and Cellular Biology* guidelines developed in 1992. As noted previously, merely knowing the program name is not very helpful for replicating an experiment or verifying the integrity of an image, though it could potentially affect the viewer's perception of the image. As time passed, it became clear that mere software identification was insufficient. Two developments drew attention to digital manipulation and encouraged changes to imaging standards: the proliferation of digital manuscript submission and the Hwang scandal.

In December 2001, *The Journal of Cell Biology* began requiring that all manuscripts and image files be submitted electronically. By September of 2002, the journal's editor, Mike Rossner, had begun what would become a veritable crusade for more ethical digital imaging. In response to a deluge of questionable images in submitted manuscripts, Rossner's journal and its publisher Rockefeller University Press developed new guidelines on image manipulation. Rossner's *Journal of Cell Biology* also implemented a new policy—all digital images are forensically analyzed for improper manipulation. The submission guidelines for *The Journal of Cell Biology* list four points:

> No specific feature within an image may be enhanced, obscured, moved, removed, or introduced.
>
> Adjustments of brightness, contrast, or color balance are acceptable if they are applied to the whole image and as long as they do not obscure, eliminate, or misrepresent any information present in the original.

> The grouping of images from different parts of the same gel, or from different gels, fields, or exposures must be made explicit by the arrangement of the figure (e.g., dividing lines) and in the text of the figure legend.
>
> If the original data cannot be produced by an author when asked to provide it, the acceptance of the manuscript may be revoked.
>
> (Rossner 837)

A 2003 policy update added requirements regarding the submission of specific information about image acquisition and modification. The required information includes such details as the makes and models of microscopes, microscopy settings, temperature measurements, acquisition software, modification software, and all digital adjustments (Rossner 837). In essence, *The Journal of Cell Biology* demanded explicit documentation of typically unstated features of an image's production.

Guidelines similar to those of *The Journal of Cell Biology* are now part of the editorial polices of *Science* ("Preparing Efficient Figures").[53] Essentially identical to the guidelines of Rossner's *The Journal of Cell Biology*, the guidelines of *Science* address concerns about selective adjustment of images (a change may be made only if it is made to the entire image) and issues related to false continuity (composite images must be clearly marked as composites). *Science* also reserves the right to have authors supply

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[53] The *Science* guidelines are as follows:

> Modification of figures. *Science* does not allow certain electronic enhancements or manipulations of micrographs, gels, or other digital images. Figures assembled from multiple photographs or images must indicate the separate parts with lines between them. Linear adjustment of contrast, brightness, or color must be applied to an entire image or plate equally. Nonlinear adjustments must be specified in the figure legend. Selective enhancement or alteration of one part of an image in not acceptable. In addition, *Science* may ask authors of papers returned for revision to provide additional documentation of their primary data. (Information for Authors; "Preparing Your Art and Figures")

primary data to support specific visual arguments. However, unlike *The Journal of Cell Biology*, the editorial staff of *Science* does not forensically analyze submitted digital images. Some critics, including Mike Rossner, blame the Hwang stem-cell scandal on insufficient editorial oversight by *Science* (Rossner "Hwang Case").

In the wake of the Hwang scandal of 2005–2006, the Nature Publishing Group published even more specific guidelines regarding image integrity. The guidelines of *Nature* are similar to but more extensive than those of both *Science* and *The Journal of Cell Biology*, but they also reflect the need for flexibility: "A certain degree of image processing is acceptable for publication (and for some experiments, fields and techniques is unavoidable), but the final image must correctly represent the original data and conform to community standards" ("Guide for Digital Images"). In other words, *Nature* recognizes that there are technical and rhetorical reasons to modify an image; the journal also recognizes that there are field specific differences regarding imaging practices. That is, the boundaries between ethical modification and unethical manipulations are context dependent. Including such language was necessary for *Nature* because it publishes articles on topics in a wide range of scientific fields. (One wonders why *Science* does not have a similar statement in its guidelines.)

The guidelines of *Nature* also stipulate that additional descriptions of visualization methods must be included with a submission: "Manuscripts should include a single Supplementary Methods file (or be part of a larger Methods section) labeled 'equipment and settings' that describes for each figure the pertinent instrument settings, acquisition conditions and processing changes..."("Image Integrity"). Though these descriptions might not be part of the final published account of an experiment, they are used to evaluate and validate the veracity of evidence—the basis of the visual argument. Thus, the journal's reputation also certifies the credibility of the images that ultimately appear.

This brief survey of evolving journal requirements demonstrates significant changes in approaches to scientific visuals over a thirteen-year period. The guidelines vary over time and from journal to journal; however, all of these policies can be read as reactions to technological changes that threatened to undermine specific kinds of visual arguments because they could undermine the objects of agreement at the heart of these visual arguments.

Conclusion

Though rhetoricians have been studying science in earnest for the last twenty years, and though ethicists and moral philosophers have been examining the ethics of science for decades, there has not been a concerted effort by rhetorical scholars to think about the confluence of rhetorical and ethical issues with respect to scientific visuals.[54] This chapter has attempted to account for the ethics of visual mediation in scientific contexts by identifying typically tacit relationships between visual artifacts, individual credibility, scientific cultures, and epistemic rhetoric. Rhetorical concepts such as the enthymeme, ethos, facts, values, hierarchies, and presumptions help to explain the causes of liminally ethical visualization and the motivations for editorial responses to image

[54] A noteworthy exception is the work of Mary Rosner who rightfully criticized rhetoric and composition scholars for taking an overly simple position on technical visuals. She notes how scientific imaging reinforces ideologies, specifically the ideology of patriarchy. My project does not engage issues regarding the ideologies supporting scientific visuality; however, the intersections of ideology, enthymemes, scientific image vernaculars, and visual arguments could be a productive site for future research.

manipulation. The crises of confidence that resulted in the advent and abuses of digital imaging caused editors and commentators to create texts that explicated competing assumptions about images as arguments. An examination of these texts shows how scientific editors attempted to reestablish the “real” over the “clear” in the hierarchy of values attached to scientific images.

Chapter 6: Refutations and Revelations: Reflections on the Visual Rhetoric of Science

Any refutation—whether it be of an accepted proposition, of one's opponent's argument, of an unexpressed argument, or of an objection to an argument—implies an attribution to what is refuted of a certain force deserving attention and effort.

—Chaim Perelman and Lucy Olbrechts-Tyteca, *The New Rhetoric*

This dissertation has considered the rhetorical dynamics of a number of cases in order to approach five questions: (1) How are new visualization practices established as scientifically credible? (2) How do scientists modify existing instrument output to make new visual arguments? (3) How do scientists use verbal and visual means to transform problematic data into acceptable support for novel claims? (4) What are the practical and ethical boundaries of modifying visual artifacts for scientific arguments? (5) How do scientists refute established (but incorrect) visualizations that have been widely accepted as accurate representations of reality?

I have considered each of the first four questions in some detail in previous chapters. This concluding chapter will review significant observations from those chapters, but it also considers issues of *refutation* in greater detail. It presents a recent case in which an established visual argument required visual refutation. The case also engages rhetorical issues related to automatic and algorithmic visualization in science. Aspects of this case are then discussed along with points from previous chapters to frame the broader implications of this dissertation and to mitigate possible counter arguments.

Visual Refutations of Visual Arguments in Science

In 2006, protein crystallographer Geoffrey Chang, a researcher at the Scripps Research Institute, had to retract five papers that described the structures of complex cellular proteins. At a basic level, Chang's methods were the same as those used by Max von Laue and the Braggs in the early days of x-ray crystallography. Chang used x-ray diffraction to generate data about the positions of atoms in crystallized proteins. But almost one hundred years of technological and scientific development have changed many aspects of the process. Indeed, even the x-ray photographs that Watson and Crick used to determine the structure of DNA seem antiquated when compared to the digital tools for data collection, analysis, and visualization that are available today. But as the circumstances of Chang's retraction demonstrate, these tools bring their own problems—even when scientists are using them ethically and with good intentions.

In the first and most influential of the retracted papers (2001), Chang and his colleagues described the structure of MsbA, a protein of the bacterium *E. coli*. MsbA is an ABC transporter protein—a biochemical mechanism that moves other molecules between the layers of a cell's membrane. ABC transporters are proteins of interest because pathogen cells might use these mechanisms to "pump" antibiotic molecules out of themselves. Thus, this class of proteins could be a crucial factor in the development of antibiotic resistance in bacteria strains. Chang's paper presented the results of his crystallographic study through elegant and convincing visuals. These visuals include visualizations of MsbA electron density generated from x-ray diffraction data (Figure 6.1).

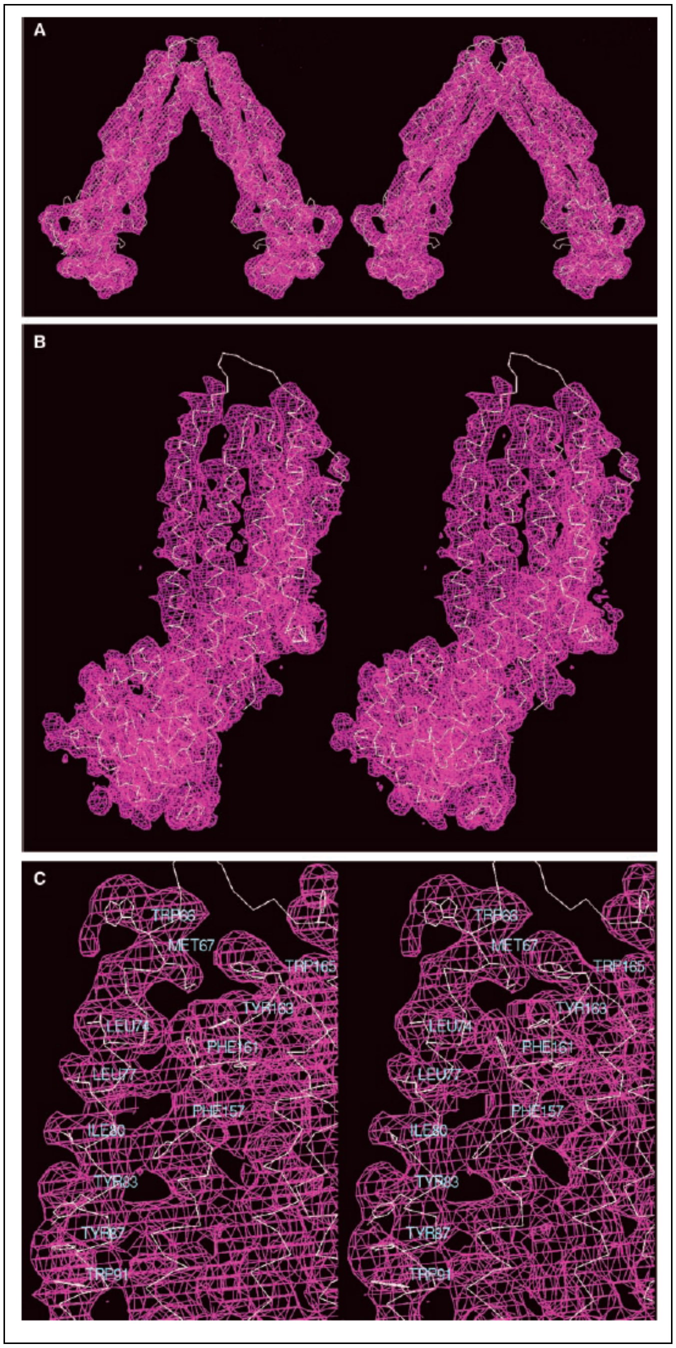


Figure 6.1: Figure 2 of Chang and Roth 2001. Stereo views of electron density maps of MsbA for *E. coli.*

This breakthrough work appeared in *Science* in 2001; it was the first determination of an ABC-transporter structure. Chang used this structure as the basis for other work, including the "structural determinations" of MsbA for *Vibrio cholera*

(*Molecular Biology,* 2003) and *Salmonella typhimurium* (*Science,* 2005). Unfortunately, the base structure presented in 2001 is inaccurate, and hence the other structures are also inaccurate. As *Science* journalist Greg Miller reported, a blip in the code of an in-house data analysis program "…flipped two columns of data, inverting the electron density-map from which his team had derived the final protein structure" (1856). This small error propagated through the visualization process, resulting in a faulty "determination" of the protein's structure. This error was not intentional, but it had serious consequences for the scientific community studying ABC transporters. MsbA was the first ABC transporter to be mapped successfully, and other crystallographers studying this class of proteins used Chang's work as the basis for new research on homologous structures. In light of Chang's retraction, they had to reconsider their work (Greg Miller 1857). But Chang's work had significant consequences even before the error was revealed. Researchers working with different protein characterization methods also faced hurdles. Greg Miller notes that biochemist David Clarke "…had a hard time persuading journals to accept [his group's] biochemical studies that contradicted Chang's MsbA structure..."; and grant applications that did not agree with Chang's structure were, in Clarke's words, "given a hard time"(Greg Miller 1857).

The errors in Chang's MsbA structure were not formally recognized until Kaspar Locher, a researcher working on a similar bacterial protein (Sav1866), compared his work with Chang's structure. According to Miller, "after pulling up Sav1866 and Chang's MsbA from *S. typhimurium* on a computer screen, Locher says he realized in minutes that the MsbA structure was inverted" (1856). Locher and co-author Roger Dawson included the following graphic to compare the two proteins in the *Nature* report on Sav1866. The MsbA structure is presented as a purple wire structure; the Sav1866 structure is presented in green.

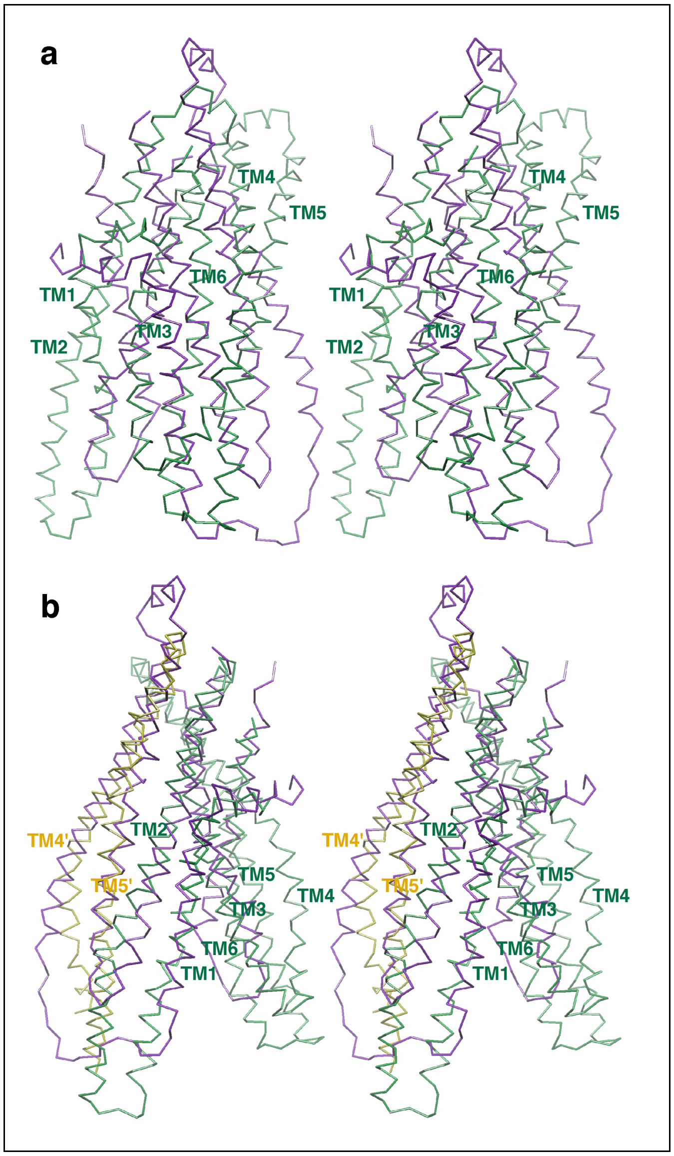


Figure 6.2: Figure 4S in Dawson and Locher (*Nature* 2006). The original caption text is as follows: Superposition of Sav1866 and MsbA. a, Stereo views of protein backbones of the transmembrane domains of a single subunit of *Salmonella typhimurium* MsbA (pdb entry 1Z2R, purple) and Sav1866 (green) after manual superpositions of the entire transporters. No agreement in the helix arrangement is apparent. b, The structure of MsbA was inverted using the coordinate manipulation program pdbset[5], and the resulting molecule was superimposed onto Sav1866. The transmembrane domain of a single monomer of inverted MsbA is shown in purple, while that of subunit A of Sav1866 is shown in green and transmembrane helices TM1 and TM2 of subunit B are shown in yellow. The approximate superposition of TM helices is evident. The differences in helix directionality and subunit assignment are detailed in Table S2.

Part a of Figure 6.2 demonstrates the significant but unexpected differences between the two structures; part b demonstrates how similar the two structures could be if the MsbA structure is inverted. Both part a and part b are *stereo views*; if you stare at them long enough a three-dimensional images should appear to hover in between the doubled images.

The purpose of Dawson and Locher's paper was to define the Sav1886 structure and not to contradict Chang's MsbA structure specifically, so the visual was included in a supplemental document and not in the body of the paper. Still, the visual comparison serves three purposes: (1) it demonstrates the incompatibilities between the structures as they are defined, (2) it preempts the rejection of the Sav1886 structure on the grounds that it does not agree with Chang's structure, and (3) it shows that Chang's structure can be compatible with Sav1886 if the underlying data of the MsbA structure is corrected. The article text presents a verbal refutation:

> The arrangement of transmembrane helices observed for Sav1866 is consistent with recent cross-linking data that identified neighbouring [sic] helices in human MDR1. In contrast, the reported X-ray analyses of another ABC exporter, the bacterial lipid flippase MsbA, show an arrangement distinctly different from the one we describe here (Supplementary Fig. S4 and Supplementary Table S2). With respect to the nucleotide-binding domains, the structure of Sav1866 exhibits close structural agreement with that of MJ0796, whereas in MsbA of *Salmonella typhimurium*, the analogous domains appear laterally offset and at a greater distance. The observed architectures of MsbA and Sav1866

> remain incompatible, even when considering that the proteins may have been trapped in distinct states, and the differences—if real—would indicate a convergent evolution of the two proteins. (182)

An explicit refutation was clearly necessary given the authority that the Chang structure had accrued, and Dawson and Locher's refutation is brief but thorough. They present their structure as consistent with similar proteins; hence, it is more accurate than the Chang structure, which is inconsistent with these other findings. Moreover, inverting the MsbA structure brings it in line with previous expectation for both it and analogous proteins. Thus, Locher and Dawson do not dismiss the entirety of Chang's work, just the faulty premise that led to a faulty structure.

In the passage, the implausibility of the visual incompatibility between the MsbA and Sav1886 structures is reinforced verbally. The authors point out what such differences would logically entail if they were "real." Specifically, significant structural differences would suggest convergent evolution. In other words, if the incompatibility is "real," then Sav1886 and MsbA independently evolved to perform similar functions. Such convergent evolution is unlikely in this case.

Dawson and Locher were rhetorically successful. Not only did they persuasively define the Sav1866 structure, but they also revealed the errors in the Chang structure, a revelation that resulted in Chang's retraction and revision of the five ABC-transporter papers. The case of the Chang retractions suggests that in some cases it can take a visual to refute a visual. The case also reveals contemporary problems of visuals and visuality within the protein studies community. In a letter to the editor entitled "Pretty Structures, But What about the Data?" biochemist Chris Miller reacted to the Chang incident by elaborating on what he sees as systemic problems in the culture of protein-structure scientists:

> The mistake so clearly illustrates two lessons that we aging baby boomer professors ram down the throats of our proteomically aroused graduate students: (i) that those lovely colored ribbons festooning the covers and pages of journals are just models, not data, and (ii) that you invite disaster if you don't know what your software is actually doing down there in the computational trenches. Students have a hard time subsuming these dicta into their souls for two reasons: the tyranny of authority (the vanity journals occupying the vanguard) and the inherent beauty of the macromolecular models that emerge, as if by magic, from the user-friendly crystallographic software accumulated over decades through the generous labor of the field's talented reciprocal space-cadets.[55]

Miller's comment raises some interesting points about the status and power of visuals in constructing the "real" and the unstated premises that can support or sink a crystallographic argument. These points will be revisited throughout the rest of this chapter as I put the details of the Chang/Locher case in dialogue with the general observations and broader implications of this dissertation.

Rhetorical Themes, Discursive Evidence, and Considerations of Counter Arguments

This project has applied a range of tools for rhetorical analysis to specific cases in which scientific visuals were important or problematic in arguments. While the analysis

[55] The final phrase is a play on words. The concept of "reciprocal space" is a crystallographic concept developed by P.P. Ewald in the 1930s.

described in each chapter develops specific conclusions for its specific case, taken together, the analyses also demonstrate three common conceptual threads: (1) the applicability of *The New Rhetoric* to scientific arguments that depend on visuals; (2) the importance of considering scientific visual arguments in the light of context, reception, and circulation evidence; and (3) the distinct value of a *rhetorical* approach to scientific visuals. The remainder of this dissertation considers and then mitigates arguments which would refute the validity of these threads. I elaborate on the most striking evidence supporting each theme. For the first two threads, I also elaborate on a special case or "fiber" of each. Specifically, the section on *The New Rhetoric* is followed by an amplified consideration of dissociation and presence; the context and reception section includes an amplified consideration of the terministic screen. Each elaboration follows the sequence of research questions outlined at the start of this dissertation.[56] Thus, this process of refutation and summary both defends and frames the foundations of a comprehensive visual rhetoric of science.

The Applicability of The New Rhetoric *to Visual Argumentation*

There are several grounds on which one could dismiss Perelman and Olbrechts-Tyteca's *The New Rhetoric* as an insufficient system for explaining scientific visuals. Its application faces the same challenges that rhetoric has encountered previously when approaching science and visuals separately. That is, one could claim that *The New*

[56]Five research questions were listed at the beginning of this dissertation: (1) How are new visualization practices established as scientifically credible? (2) How do scientists modify existing instrument output to make new visual arguments? (3) How do scientists use verbal and visual means to transform problematic data into acceptable support for novel claims? (4) What are the practical and ethical boundaries of modifying visual artifacts for scientific arguments? (5) How do scientists refute established (but incorrect) visualizations that have been widely accepted as accurate representations of reality?

Rhetoric provides tools meant only to guide the production of *texts* and/or that these tools were never intended for analyzing scientific situations. The latter objection has been sufficiently refuted by the many textual studies of scientific rhetoric that deploy the this rhetorical theory convincingly; Perelman's own admission that the scope of *The New Rhetoric* has extended beyond its initial intent as a treatise on legal and philosophical argumentation also supports its applicability to scientific discourse ("The New Rhetoric and the Rhetoricians" 189-194). The former objection is not as clearly dismissed by precedent. *The New Rhetoric* has been far less popular in studies of visual rhetoric than other rhetorical texts have been. For example, the index of the 2006 collection, *Rhetorics of Display,* lists only two lines of references for both Perelman and Olbrechts-Tyteca respectively, and all but three of the nine references in those lines point to brief mentions in endnotes. In comparison, Aristotle has more than eight lines of index references, and Kenneth Burke has more than twenty lines of references. Similarly, the ten-essay collection *Ways of Seeing, Ways of Speaking: The Integration of Rhetoric and Vision in Constructing the Real* (2007) has no references to Perelman or Olbrechts-Tyteca in its index. They are mentioned only three times in the thirty essays of Carolyn Handa's anthology *Visual Rhetoric in a Digital World* (2002). Of course, the extent of index references in a few essay collections is not definitive evidence of the critical neglect of specific texts or theorists; however, this anecdotal evidence at least suggests that *The New Rhetoric* has not been wildly popular with scholars of visual rhetoric. But does lacking a groundswell of citations mean that the text cannot be productively applied to visual arguments? The cases examined in this project demonstrate the opposite.

Though this project has not justified the applicability of every concept in *The New Rhetoric* to visual rhetoric in science, it has successfully covered large swaths of conceptual ground. The concepts of *presence* and *dissociation* are discussed at length in the next section. Other terms, such as the Perelman and Olbrechts-Tyteca's argument classes and the "objects of agreement," have also been conceptually productive for explaining the rhetoric of scientific visualization.

In attempting to establish the new visualization regime of x-ray diffraction photography, Laue and the Braggs each worked with and against visual artifacts when developing arguments establishing the structure of reality, arguments based on the structure of reality, and quasi-logical arguments. Specifically, Laue's analogy between x-ray diffraction and traditional diffraction was supported both with causal arguments based on the comparison of photographs and with visually supported incompatibility arguments meant to fend off refutation. The Braggs used a similar combination of incompatibility, analogy, and causality arguments to establish a different interpretation of the x-ray images; and each of their claims required visual articulation or visual support. The Braggs ultimately instantiated their reflection analogy in the physical design of a new instrument—the x-ray spectrometer.

The analyses of the other cases also benefit from the conceptual richness of *The New Rhetoric*. The visuals supporting the Vine-Matthews-Morley hypothesis show how existing visualized data were adapted to fit specific quasi-logical argument forms. Specifically, the Eltanin-19 profile was mediated in ways that both enhanced the symmetry of the profile and reinforced the transitive relationship between distance, magnetism, and time. The dissociation of concepts—the rhetorical revision of reality—is the primary argument at work in the *twilight zone* case. (Dissociation is described at length in the next sub-section as a special case of the visualized *New Rhetoric.*) The editorial responses to the ethics of digital-image manipulation demonstrate how the starting points of argumentation—what the Perelman and Olbrechts-Tyteca call the "objects of agreement"—operate visually. A visual's ability to function as an argument about reality depends on networks of facts, truths, presumptions, loci, and hierarchically arranged values. In some contexts, the proliferation of digital imaging destabilized formerly reliable objects of agreement, and scientific editors responded with revised guidelines, new editorial practices, and candid discussion of the ethics of image adjustment.

The Chang case demonstrates concepts from section in *The New Rhetoric* on the interaction of arguments. Locher's refutation of Chang's protein structure exemplifies refutation strategies described by Perelman and Olbrechts-Tyteca, such as the following passage:

> To make the refutation of consequence and deserving of consideration, one has to make a sufficiently high estimate of what one is attacking: This is necessary not only for purposes of prestige, but in order to better gain the attention of the audience and secure certain strength for the future for the arguments one uses. And one has to make a sufficiently low estimate of what one is attacking, so that the refutation is strong enough. (470)

Locher and Dawson make a "sufficiently high estimate" of Chang's structure by recognizing it in the text and addressing the incompatibility of his established structure with their new one. However, they also minimize the salience of Chang's argument by

(1) refuting Chang's arguments briefly and (2) placing the visual refutation in a supplemental section. Though the visual refutation was necessary, it did not need to have the enhanced visual presence that placement in the main text could entail. Those positions were filled with Dawson and Locher's positive visual arguments that establish the structure of their protein, Sav1886.

Clearly, the rhetoric of Perelman and Olbrechts-Tyteca is comprehensive enough to account for a range of persuasive situations involving visuals. As the next section will show, specific concepts from *The New Rhetoric* are also help to explain the role of visual artifacts in the fundamental epistemic processes of science.

Presence, Dissociation, and the Rhetoric of the Real

Presence and *dissociation* are two terms from *The New Rhetoric* that are especially productive for thinking about visual rhetoric and science. Regarding *presence*, Perelman and Olbrechts-Tyteca note, "…by the very fact of selecting certain elements and presenting them to the audience, their importance and pertinency to the discussion are implied. Indeed, such a choice endows these elements with a *presence*, which is an essential factor in argumentation and one that is far too much neglected in rationalistic conceptions of reasoning" (116). Recall that the process of *dissociation* revises the structure of reality by rhetorically transforming a unitary concept into two entities: one that retains the value associated with the original concept and one that is cast away as false, artificial, or mere appearance (411-415). As Alan Gross notes, both of these processes are essential to the practice of science. For example, Gross's examination of ornithologists' verbal and visual construction of a new hummingbird species shows how multiple discursive vectors bring a species into existence through rhetorical presence. As mentioned previously, Gross also remarks that dissociation—the process of separating appearance from reality—is the fundamental activity of science. The speciation of the bonobo offers a parallel case to Gross's example of hummingbird speciation. Until 1933, bonobos were considered chimpanzee variants. They were sometimes referred to colloquially as pygmy chimpanzees, dwarf chimpanzees, or gracile chimpanzees, but taxonomically they were treated as identical to the common chimpanzee, *Pan troglodytes*. The work of Ernst Schwarz (1929) and Harold Coolidge (1933) separated the appearance of a common group of visually similar animals from the reality of two distinct species. Consequently, the bonobo received its own Linnaean taxonomic label, *Pan paniscus* (de Waal 82-88). As was shown through the explication of the *twilight zone* case, the salience of dissociation markers in a scientific text is proportional to the challenge of the rhetorical situation. Higher rhetorical stakes evoke more emphatic dissociations. Koren et al. made their dissociation argument more salient verbally over successive drafts of a text that did not initially make convincing claims about reality. Though Gross notes the role of the visual in creating rhetorical presence, he does not mention how visual elements participate in dissociation specifically. This is not a criticism, just a clarification. The cases from this dissertation clearly demonstrate that visual arguments are involved with rhetorical processes related to both dissociation and presence. These cases also suggest that there might be a consistent link between the two concepts in visual scientific argumentation.

In the x-ray diffraction case, the interplay of presence and dissociation was essential to establishing a new visualization tool. Laue used his array of photographs and his mathematical arguments to create visual traces of two invisible phenomena: x-rays and atoms. Though Laue's diagrammatic proof was a mathematical "reconstruction" of the photograph's pattern and not an analysis of individual spots, his coded graphic directed the attention of his readers to specific aspects of the spots—the five wavelengths that he believed accounted for them completely. To replace Laue's interpretation of the photographs, the younger Bragg directed the attention of readers in different ways. He used visual arguments that stressed aspects of the photographs that were more compatible with his interpretation of the photographs and less compatible with Laue's. Specifically, he showed how a reading based on "reflection" could account for the more elliptical spots that formed when the crystal was set further from the x-ray source. Bragg invokes a rhetorical dissociation by separating the "appearance" of Laue's simple cubic structure revealed by five discrete wavelengths from the reality of a face-centered cube diffracting a heterogeneous x-ray beam. Bragg's means: arguments from incompatibility and compatibility backed by mathematical and visual arguments supported by geometry. He used math to cast doubt on the veracity of Laue's reading because if Laue's reading were correct, additional spots would have been in the photograph, and they were not there. Bragg uses the "complete series" of his tables to demonstrate the mathematical completeness of his reading, and he uses the circular inscriptions on his key to the photograph to visualize why the invisible "reflected" waves selectively darken particular parts of the image.

In considering the developing Vine-Matthews-Morley Hypothesis, dissociation and presence help explain the rhetorical process of adjusting existing visualized data. Raff and Mason's figural transformation from series to antithesis created visualized stripes of magnetism on magnetic maps; those stripes were eventually read as distinct periods of magnetic time. Pitman and Hiertzler's visualization of the Eltanin-19 profile enhanced the presence of the curve's symmetry. In both graphics, invisible features of the sea floor were given visual presence, and then those salient features could participate in arguments that separated the appearance of a static sea floor from the reality of continents in motion.

With the cloud case, the outputs of multiple instruments (MODIS, AERONET, and a normal digital camera) were combined to bring the *twilight zone* into rhetorical existence. Just as the ornithologists described by Gross "constructed" a new hummingbird species through verbal and visual means, the Goddard scientists rhetorically constructed a new atmospheric phenomenon through a multi-vectored description that gave their *twilight zone* visual presence. Moreover, the cloud photo series epitomized their dissociation argument visually; the appearance of a distinctly bound cloud is replaced with the visible reality of a complex reflectance phenomenon.

To summarize, presence can rhetorically construct new phenomena by directing the attention to specific aspects of reality; dissociation is used to revise the structure of reality by rhetorically separating the apparent from the genuinely real. Clearly, visualization participates in these rhetorical processes, but advances in visualization technology—either imaging technology or information visualization technology—can cause significant scientific and rhetorical concerns.

The editorial reactions to digital images reveal ethical and epistemological tensions related to presence and dissociation in scientific argumentation. To make arguments, scientists must often present visualized data. These visuals direct the attention of the reader and hence add visual presence to the arguments expressed textually. These representations are always motivated rhetorical selections. Even the "unmodified" photograph is chosen and situated in a text for persuasive ends. However, digital tools allow scientists to adjust the rhetorical presence of particular features in ways that were formerly difficult; for example, a few mouse clicks on a contrast button can enhance or obliterate the faint trace of a molecule in an autoradiogram. Editorial responses to the proliferation of both digital technologies and digital misconduct indicate how visuals participate in rhetorical dissociations. Specifically, the texts spawned by editorial anxiety suggest that scientific visuals act as term II markers—the "real" term in an appearance-reality dyad. Technological developments threatened to undermine the dissociation process at the heart of scientific persuasion because images were no longer the "self-evident" products of accepted objects of agreement. New imaging guidelines are attempts to reaffirm the visual starting points essential to scientific argumentation.

The Chang case demonstrates the power and problems of the data visualizations that both guide scientists working with invisible phenomena and influence those who read and believe their work. As Chris Miller noted, protein visualizations can be perceived *as* reality and not as models of data. Thus, this class of visuals can possess significant ontological status, a status they do not automatically deserve. Chang's interpretation of the MsbA protein obtained an ontological status it did not deserve based on the rhetorical presence established through the visual model. This inaccurate visualization had more authority than the accurate biochemical evidence that contradicted it. Refuting the Chang structure required a replacement visualization that could distinguish appearance from reality. This dissociation shares some interesting similarities with the rhetorical dissociations analyzed in Chapters 2, 3, and 4.

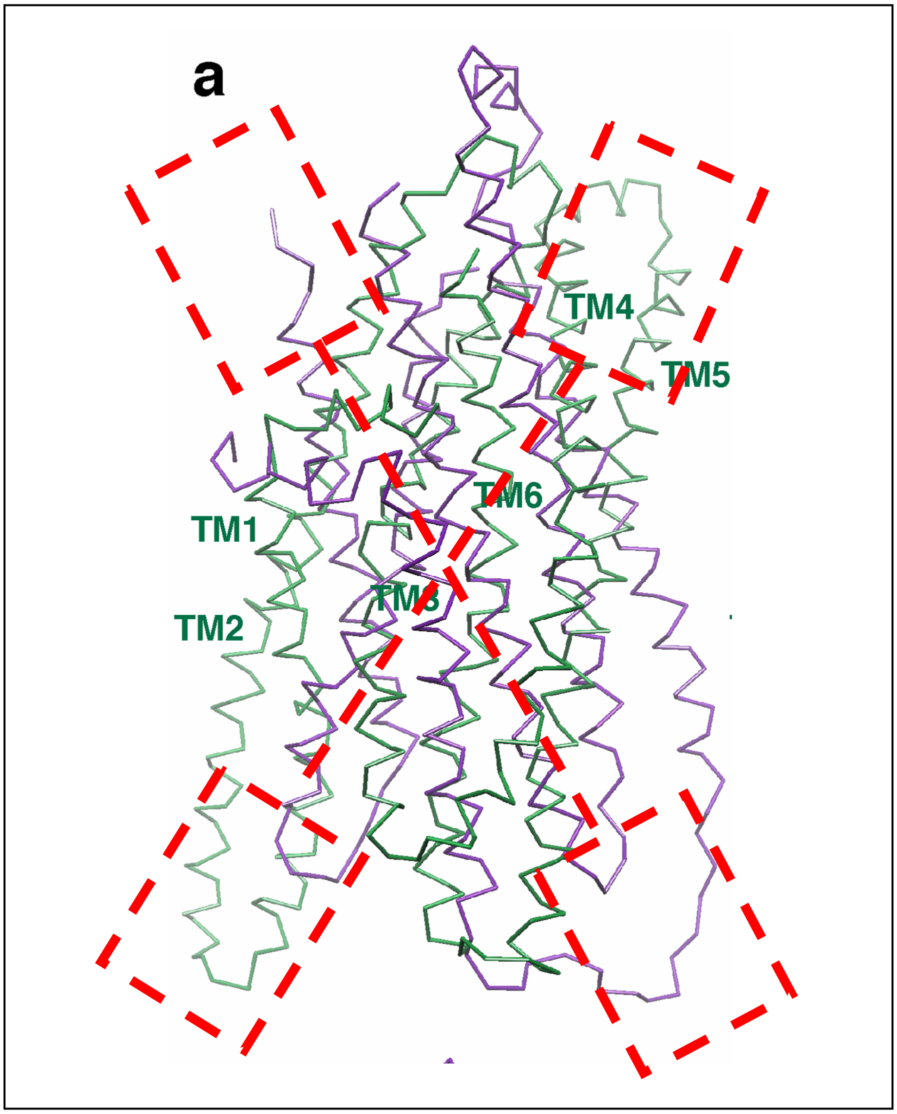
Like the x-ray case, a lack of understanding of the underlying premises caused the creation of an inaccurate reading in the first place. In the Chang case, computational premises were the problem rather than physical premises; however, the results of these faulty premises still needed to be considered and reinterpreted in the refutation and replacement. By inverting the structure visually and supporting that inversion with data in a table, Dawson and Locher demonstrated why their reading of Sav1886 and their proposed interpretation of MsbA are more compatible with reality.

Like the visual arguments of the cloud case, this graphic (Figure 6.2) takes advantage of top-bottom/term I-term II image arrangement to indicate the dissociative distinction. The more "real" reading is placed in the lower position; the false pretender is placed in the upper position.

Figural logics participate in the presence-dissociation dynamic of Figure 6.2, as they did in visuals from the tectonics and cloud cases. However, instead of a chiasmus-to-antimetabole transformation or an antithesis-to-series transformation, this graphic offers a chiasmus-to-analogy transformation. The top structure (part a) can be read as a chiasmus (XY:Y'X') if color is read as marking recurring terms and shape as marking cognates.[57] That is, from left to right it is a purple to green sequence across the upper portion of the structure (with overlap in the middle); across the bottom, it is a green to purple sequence (with overlap in the middle). In other words, purple - green: green' -

[57] In the following example from Fahnestock's *Rhetorical Figures in Science* the recurring concepts of "invader" and "winter" are arranged in a chiasmus; historically distinct figures and weather events as the cognate terms: "Napoleon was defeated by a Russian winter and the snows of Leningrad destroyed Hitler"(123).

purple'. As my red markings in Figure 6.3 suggest, the graphic is visually X-like; X is the Greek letter *chi*, the etymological root of the rhetorical figure *chiasmus*. The false contrastive symmetry in part a of Figure 6.2 (page 301) is contrasted with the visual analogy in part b. Though structures are not identical, they are visually *like* each other.[58] This similarity is consistent with all the other supporting evidence, including the biochemical studies that were once dismissed because they were inconsistent with Chang's interpretation of MsbA.



[58] A textual analogy equivalent to the chiasmus in the previous note is as follows: "The Russian winter destroyed Hitler's army just like it destroyed Napoleon's army."

Figure 6.3: Modified version of Figure 4S (part a) of Dawson and Locher 2006. My red markings.

<u>*Context, Reception, and Circulation*</u>

As noted in the introduction, scholars of visual rhetoric and the rhetoric of science have raised important concerns about the accountability of their rhetorical analyses. Wanting to be sure that their claims accurately reflect the rhetorical situations in which the artifacts they analyze are operating, rhetoricians have emphasized the importance of understanding the complexities of context and reception. Given the validity of successful approaches, I can invent no plausible counter argument that would not be a "straw man." Understanding context and reception is important; this point is born out in my analyses of the previous cases. Studying the context and reception of scientific arguments dependent on visuals helps tease out the issues and implications of establishing new visual practices, adapting existing images, validating suspect data, and guiding ethical practices.

In the Chang case, reception evidence is limited but revealing. For example, even cursory citation analysis demonstrates the impact of Chang's work. As Greg Miller reported, a 2006 Google Scholar search showed that more than 350 publications had cited Chang's first MsbA paper before the retraction. Clearly, it was persuasive for some readers. On the other hand, anecdotal evidence reveals that some scientists were not convinced by Chang's visual arguments. As British biochemist Christopher Higgins recalls, "When the first structure came out, we and others said, 'We really don't quite believe this is right'. [...] It was inconsistent with a lot of things" (qtd. in Miller 1857). Juxtaposing these pieces of reception evidence reveals the rhetorical effects of the Chang paper beyond the moment of initial publication. Though published in the prestigious journal *Science*, the paper and its structure were not accepted in all quarters, and thus there was room for refutation and replacement.

While context and reception have been important components in the analyses in my previous chapters, I have also examined the *circulation* of specific artifacts and arguments. The concept of circulation is related to issues of context and reception because studying the circulation and adaptation of visuals reveals (1) aspects of scientific cultures, (2) the evolution of arguments, and (3) the conceptual traction of specific ideas in different contexts. While one could argue that tracing circulation is frivolous, the explication of these cases shows otherwise.

Max von Laue's photographs of x-ray diffraction circulated through formal and informal channels. Recall that the image was first revealed through Laue's presentations at various symposia; the Braggs first saw that artifact after a copy of it was mailed to the elder Bragg by Lars Vegard. Both Laue and the Braggs mediated that image to instantiate their differing arguments about x-rays and crystal structures. The now iconic image of ZnS served different rhetorical functions when it circulated within Laue's own work. It first served as evidence supporting a wave interpretation of x-rays. It then served as the logical conclusion to Laue's mathematical reconstruction. W. L. Bragg's revision of the ZnS photogram demonstrates that foundational propositions and assumptions are malleable, especially in the early period of a new visualization technology.

Visual arguments from the case of the Vine-Matthews-Morley hypothesis demonstrate how subtle changes in visual form can connote significant conceptual changes. For example, Raff and Mason's "zebra stripe" map was initially ignored, but it was later incorporated as evidence in a review article and transformed into a colorized representation of a hypothesis for a conference slide. The argument of that color slide—that magnetism is an index of time for the areas near the Juan de Fuca ridge—ultimately became transformed into a visual *truth* replicated in new claims about other geophysical activity. Tracing the circulation of this map across these contexts revealed important connections between visual signifiers and evolving scientific concepts. In this case, visual cues such as vectors, color, and selective redaction were used to rearrange the same static magnetic data for different rhetorical purposes.

Chapter 4 offers an example of an argument's circulation from specialist to popular contexts. The dissociation argument epitomized in the cloud photo series was maintained as the *twilight zone* circulated into non-specialist contexts; however, the composition of that epitome changes from a vertical triptych to a horizontal triptych and from a horizontal triptych to both a side-by-side comparison and a single representative image. Thus, tracing the photographs' circulation reveals a range of strategies used to convey the same argument for different audiences.

The Chang case offers an interesting comment on the circulation of refutation arguments. In their paper, Locher and Dawson minimized the salience of their visual refutation of Chang by placing it in a supplemental document; however, when the visual was incorporated into a feature story on the case, it was literally the center of attention (Figure 6.4). As in the cloud case, what had been a vertical comparison was transformed into horizontal comparison. The chiastic mismatch of molecules is in the left/given position; the visual analogy is in the right/new position. The formal similarity between these cases seems to support Kress and van Leeuwen's observations about graphic composition and modality, or "truth value" in images. Also, the *stereo views* that were conventional for the specialist audience were removed in this news piece written for the broadest readership of the journal *Science.*

SCIENTIFIC PUBLISHING

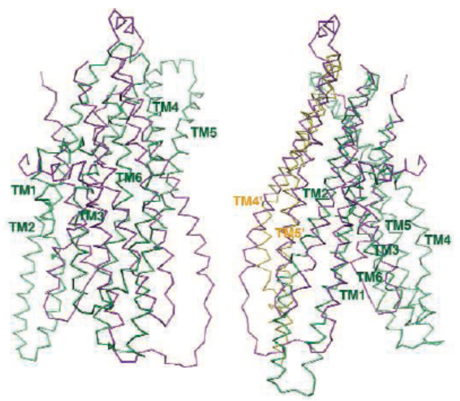
A Scientist's Nightmare: Software Problem Leads to Five Retractions

Until recently, Geoffrey Chang's career was on a trajectory most young scientists only dream about. In 1999, at the age of 28, the protein crystallographer landed a faculty position at the prestigious Scripps Research Institute in San Diego, California. The next year, in a ceremony at the White House, Chang received a Presidential Early Career Award for Scientists and Engineers, the country's highest honor for young researchers. His lab generated a stream of high-profile papers detailing the molecular structures of important proteins embedded in cell membranes.

Then the dream turned into a nightmare. In September, Swiss researchers published a paper in *Nature* that cast serious doubt on a protein structure Chang's group had described in a 2001 *Science* paper. When he investigated, Chang was horrified to discover that a homemade data-analysis program had flipped two columns of data, inverting the electron-density map from which his team had derived the final protein structure. Unfortunately, his group had used the program to analyze data for other proteins. As a result, on page 1875, Chang and his colleagues retract three *Science* papers and report that two papers in other journals also contain erroneous structures.

"I've been devastated," Chang says. "I hope people will understand that it was a mistake, and I'm very sorry for it." Other researchers don't doubt that the error was unintentional, and although some say it has cost them time and effort, many praise Chang for setting the record straight promptly and forthrightly. "I'm very pleased he's done this because there has been some confusion" about the original structures, says Christopher Higgins, a biochemist at Imperial College London. "Now the field can really move forward."

The most influential of Chang's retracted publications, other researchers say, was the 2001 *Science* paper, which described the structure of a protein called MsbA, isolated from the bacterium *Escherichia coli*. MsbA belongs to a huge and ancient family of molecules that use energy from adenosine triphosphate to transport molecules across cell membranes. These so-called ABC transporters perform many essential biological duties and are of great clinical interest because of their roles in drug resistance. Some pump antibiotics out of bacterial cells, for example; others clear chemotherapy drugs from cancer cells. Chang's MsbA structure was the first molecular portrait of an entire ABC transporter, and many researchers saw it as a major contribution toward figuring out how these crucial proteins do their jobs. That paper alone has been cited by 364 publications, according to Google Scholar.



Flipping fiasco. The structures of MsbA (purple) and Sav1866 (green) overlap little (*left*) until MsbA is inverted (*right*).

CREDIT: R. J. P. DAWSON AND K. P. LOCHER, NATURE 443, 180 (2006)

Two subsequent papers, both now being retracted, describe the structure of MsbA from other bacteria, *Vibrio cholera* (published in *Molecular Biology* in 2003) and *Salmonella typhimurium* (published in *Science* in 2005). The other retractions, a 2004 paper in the *Proceedings of the National Academy of Sciences* and a 2005 *Science* paper, described EmrE, a different type of transporter protein.

Crystallizing and obtaining structures of five membrane proteins in just over 5 years was an incredible feat, says Chang's former postdoc adviser Douglas Rees of the California Institute of Technology in Pasadena. Such proteins are a challenge for crystallographers because they are large, unwieldy, and notoriously difficult to coax into the crystals needed for x-ray crystallography. Rees says determination was at the root of Chang's success: "He has an incredible drive and work ethic. He really pushed the field in the sense of getting things to crystallize that no one else had been able to do." Chang's data are good, Rees says, but the faulty software threw everything off.

Ironically, another former postdoc in Rees's lab, Kaspar Locher, exposed the mistake. In the 14 September issue of *Nature*, Locher, now at the Swiss Federal Institute of Technology in Zurich, described the structure of an ABC transporter called Sav1866 from *Staphylococcus aureus*. The structure was dramatically—and unexpectedly—different from that of MsbA. After pulling up Sav1866 and Chang's MsbA from *S. typhimurium* on a computer screen, Locher says he realized in minutes that the MsbA structure was inverted. Interpreting the "hand" of a molecule is always a challenge for crystallographers, Locher notes, and many mistakes can lead to an incorrect mirror-image structure. Getting the wrong hand is "in the category of monumental blunders," Locher says.

On reading the *Nature* paper, Chang quickly traced the mix-up back to the analysis program, which he says he inherited from another lab. Locher suspects that Chang would have caught the mistake if he'd taken more time to obtain a higher resolution structure. "I think he was under immense pressure to get the first structure, and that's what made him push the limits of his data," he says. Others suggest that Chang might have caught the problem if he'd paid closer attention to biochemical findings that didn't jibe well with the MsbA structure. "When the first structure came out, we and others said, 'We really ▸

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Figure 6.4: The first page of Greg Miller's "A Scientist's Nightmare: Software Problem Leads to Five Retractions." *Science*. 314 (22 December 2006): 1856.

Using Burke's Terministic Screen to Explain Visual Arguments in Science

Kenneth Burke had both science and visualization in mind when developing the terministic screen. It was a series of photographs taken through different filters that brought Burke to think of the concept, and his scientific examples of verbal terministic screens include both Darwin and various flavors of early twentieth-century psychology (120; 122). As Burke himself observed, the necessarily partial selection of terminological systems also applies to scientific instruments (115; 120).[59] The variables that are measured by a recording instrument and the ways those measurements are reported limit what a scientist can know about a phenomenon. Any inscription can constrain the new claims that scientists can make based on collected evidence, but scientists also apply additional terministic screens in creating visual arguments. Scientists and their algorithms select (and hence limit) the data that becomes visualized; in many cases scientists can then determine which data will be highlighted or diminished visually. Thus, a given visualization technology has the potential to be both an asset and a liability because it can focus and divert the attention of both the scientists developing new knowledge and those who receive rhetorically selective images.

Burke's terministic screen is a useful notion for thinking about the general operation of science visuals, but it is also a powerful concept for recovering and explaining context and reception for specific scientific rhetorical situations. In the cases examined in this study, both perceptual and instrumental terministic screens affected the interpretation, mediation, and reception of visuals. In the case of the x-ray diffraction photographs, preconceived notions of x-rays, optics, crystal structure, and geometry

[59] Burke remarked, "And since all laboratory instruments of measurement and observation are devices invented by the symbol-using animal, they too necessarily give interpretations in terms of either continuity or discontinuity" (120).

skewed the interpretations of both Laue and Bragg, and hence it skewed the representations they used to direct the attention of their audiences. Differences show the assumptions each brought to “reading” the visuals, but assumptions also limited the kinds of claims that each could make about the structure. In this case, equations derived from analogical processes created the screens, and those equations were developed in light of both certain and uncertain premises about molecules and crystal formations. Once a given screen was established, seeing the evidence differently became difficult. For example, historians point to Laue’s overlooking of specific features of the photographs as “signs of exhaustion,” and they explain specific misinterpretations in his 1913 addendum as “indications of confusion.” Thus, in hindsight parts of Laue’s reading do not make sense, so they are dismissed. However, one can also interpret Laue’s commitment to his hypothesis through the concept of the terministic screen. Once he accepted an analogy between the x-ray effects and traditional diffraction, his vision and his reasoning were guided by the constraints of that analogy.

The case of the sea-floor spreading hypothesis shows how terministic selection can deflect attention. The “terms” that Raff and Mason selected to highlight data in the first map of the Juan de Fuca area deflected attention from other aspects of the topography. Indeed, the paper sat largely ignored for more than two years; it could not be interpreted as support for the Vine-Matthews-Morley hypothesis until it could be read in the light of more detailed topographic data and more developed geophysical premises.

Some of the images of the *twilight zone* case—had they been made decades earlier—could have been Burke’s inspiration for the very idea of terministic screens. The cloud photo series is filtered as are Burke’s inspirational photographs; however, Ilan

Koren's "filters" were digital computational transformations. Such transformations selected some details of the image and ignored others to draw the twilight zone out of the digital shadows and into existence. More important from the perspective of context is the role of terministic screens in the more standard rhetorical activities of atmospheric science. Satellite instruments like MODIS collect so much data that selection must involve digital processing. Atmospheric scientists, such as those at Goddard, are constantly refining these data "masks," so they can create better models of the atmosphere. However, as was the case with the "twilight zone," sometimes the settings of those algorithmic screens can deflect attention away from important phenomena. Correcting such deflections after they have become conventionalized can take tenacity and rhetorical skill.

The ethical problems of digital imaging are also illuminated by considering the terministic screen; the proliferation of digital manipulation brought issues of selection and deflection to the editorial pages of major journals. Editorial accounts raised concerns about fabrication of data, but they were frequently more worried about selective omission; for example, removing seemingly artifactual objects that are actually real data. These concerns are as rhetorical and epistemological as they are ethical. A scientific image is received as the product of acceptable terministic selection that reflects reality in sanctioned ways. These visual selections are the visual equivalent of discipline-specific vocabularies. However, the digitally manipulated image can be a screen that is idiosyncratic. An individual who is motivated to change an image to highlight one aspect of it might not see what is deflected by the potentially "biased" selection presented in the altered visual.

The Chang case demonstrates that the visualization tools of modern crystallography act as powerful terministic screens that can produce artifacts that eclipse other data. As Chris Miller's letter to the editor suggests, the practice of visualization has become so regularized, implicit, and uncritical that some scientists cannot see when the visualization created by the terministic screen is inaccurate. With the MsbA structure, the reflection of reality encoded in Chang's graphic was more powerful than the terministic screens applied by other scientists in different sub-disciplines. For five years, Chang's x-ray diffraction reality was more real than other scientists' biochemical realities.

The Value of a Rhetorical Approach to Scientific Visuals

Though rhetorical scholars have been studying the discourses of science for decades, they are still considered the "new kids" on the block of science studies. As Leah Ceccarelli observed in 2005, some historians, philosophers, and sociologists of science continue to resist the claims of rhetoricians studying science. In her analysis of the responses of historians of science to her scholarship, Ceccarelli identifies two specific complaints: (1) her rhetorical reading misrepresents the history of science literature, and (2) her rhetorical analysis presumptuously assumes that historians are not already doing rhetorical scholarship (i.e., close textual analysis) ("A Hard Look" 262-263). Though Ceccarelli deftly and fairly mitigates these criticisms, refutation is not the primary exigence for her analysis. Her purpose is to examine "how audiences from various science studies disciplines are responding to the arguments for academic significance that we rhetoricians have been making, [so] we might be able to improve our future arguments for interdisciplinary relevance" (258). This is indeed a goal that any rhetorician of science needs to consider, and I consider myself a rhetorician of science.

Rhetoricians of science need to recognize the contributions to their cases by scholars of allied fields, but they also need to demonstrate the value of a rhetorical approach to science. For the purposes of this dissertation, the first of these tasks has been addressed. For each of my historical cases, I have examined the valuable contributions of historians, philosophers, and sociologists. My most recent cases are too new to have significant support from allied fields. For example, I am the first person to write about the *twilight zone* case from any science studies perspective.

To demonstrate rhetoric's ability to make a distinct contribution to science studies, I summarize observations made in this dissertation that have been unrecognized by other scholars because of their disciplinary orientations and commitments. Regarding the x-ray diffraction case, important elements of Laue's visual arguments had never been explicated in detail. Specifically, the entire series of photographs is typically ignored even though these images reveal how Laue developed his larger claims. Similarly, the nuances of his diagram had been overlooked. Often, Laue is portrayed as blinded by preconceived assumptions; however, close attention to his texts demonstrate that he was merely persuaded by provisionally acceptable arguments that even he recognized would need to be refined.

In the case of the Vine-Matthews-Morely hypothesis, many studies do not cite specific visuals appropriately. For example, the different iterations of the Raff and Mason map are either intentionally treated as equivalent or the differences are merely unnoticed; the frequency of this oversight suggests the latter. A close reading of these visuals informed by their rhetorical situations reveals subtle semiotic differences. For example, when Fred Vine first appropriated the image, he removed Raff and Mason's unhelpful labels and added new features that could better explain his hypothesis. A rhetorical approach that emphasized the importance of circulation and reception revealed these significant differences.

The *twilight zone* case is perhaps my best evidence for the distinct value of a rhetorical approach to science studies. This is a new case that historians and philosophers would not choose to analyze because it is so new. Sociologists would not have found it unless they were already in place to observe the culture of the Goddard group. While I found this case accidentally, a rhetorical approach led me to capture this fascinating story. When a popular accommodation of the *twilight zone* case appeared in my daily news feed, I was intrigued by the accommodated visual arguments. I traced the circulation of the *twilight zone* back to the original article. This article is a remarkable rhetorical artifact on its own, and an analysis of it is only enhanced by parallel analyses of preliminary drafts and the illuminating comments provided by Dr. Lorraine Remer. Taking a rhetorical approach to these artifacts revealed important details about how modern scientists develop and refine arguments of critical contemporary importance.

Rhetoric also helps to explain the ethics of digital image manipulation by providing terms for the typically tacit understandings at the heart of most argumentation. Scientists are sometimes tempted to modify images digitally because clear and striking images have traditionally enhanced a scientist's *ethos,* and because they want to reveal patterns that are ultimately persuasive. However, digital technologies have enabled modifications that can be inconsistent with the enthymemic structures of a scientific visual culture. Rhetorical readings of new image submission guidelines and editorial comments on ethics reveal that the scientific visual is not an ethically or epistemologically "pure" object. That is, scientific visuals and visualization technologies are not good or bad, right or wrong, true or false at any essential level. Instead, they are socially situated objects whose status as facts, truths, and premises depends on networks of tacit assumptions. When it was clear that such assumptions were not understood uniformly by members of their discourse communities, journals responded to (re)establish expectations for the ethical use of images. A rhetorical approach provided the vocabulary to explain these processes.

The Visual Rhetoric of Science: More than the Regurgitation of Images and Data

The visual rhetoric of science is more than just stark photographs, data plots, and the regurgitation of instrument output. Instruments collect traces of phenomena, but for traces to become evidence, and for evidence to become argument, some rhetorical processing is always necessary. Scientific visuals are always contextually situated and they are often actively mediated to make convincing knowledge claims. The following comment from a response to the Chang retraction demonstrates that scientists themselves recognize the rhetorical nature of their visual artifacts:

> Inherent in structural analysis is a degree of subjectivity, which is particularly relevant in low-resolution studies such as those made by Chang and co-workers. Essentially correct structures have been built at 4.5 Å resolution, but it is not surprising that some of them turn out to be wrong upon further scrutiny. For this scrutiny to take place, however,

> readers must be provided with the original experimental data, not only the derived atomic coordinates. Only armed with these data can an investigator conduct an independent evaluation that may result in a reinterpretation of the published structure.
> (Jones and Kleyget 194)

Even with the advances of modern technology, crystal structure determinations are initially unfixed and probabilistic arguments; hence, they reside in the realm of rhetoric.

Science is an inter-subjective rhetorical activity that now relies on visual arguments more than ever. Rhetoricians need a comprehensive visual rhetoric of science to enable the interpretation of visual scientific arguments and to support science-writing instruction. This project has moved toward this goal by applying concepts from the most thorough modern system of argumentation to an array of visual rhetorical activities that scientists encounter. This analysis has yielded important insights on the relationships between visuals, rhetoric, reality, and knowledge; however, there is still much work to be done.

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