Theories, Experiments, and Human Agents: The Controversy between Emissionists and Undulationists in Britain, 1827-1859

by

Xiang Chen

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APPROVED:

Peter Barker, Chairman	Roger Ariew
Jed Z. Buchwald	David S. Lux
Albert E. Moyer	Robert A. Paterson

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(ABSTRACT)

This dissertation is an interdisciplinary study of scientific change. The undulatory theory of light replaced the emission theory of light in the early nineteenth century, triggering an "optical revolution" and vigorous debates among physicists in Britain from the 1830s to the 1850s. In this study I give the first full account of this extended episode of scientific change, drawing on methods and concepts from history, sociology and philosophy of science. The interdisciplinary account of the episode provides a basis for criticizing the existing models of scientific change in the philosophy of science.

Previous historical studies of the "optical revolution" pay little attention to the period after the 1830s. Because the cognitive superiority of the undulatory theory had become obvious in the early 1830s, some historians have implicitly assumed that any controversy would soon come to a natural end. I, however, document that intensive debates continued from the 1830s until the end of the 1850s, and that emissionists even enjoyed temporary victories in their fights with undulationists. The narrative reveals the historical complexities of this episode: the debates extended long after the cognitive superiority of the undulatory theory should have become apparent by modern standard, the results of the debates did not necessarily coincide with modern cognitive judgements, and individual agents played decisive roles in determining how long a debate lasted and how it would end.

On the basis of the historical narrative, I provide a philosophical analysis of the practices of theory appraisal and experiment appraisal that constituted the main theme of the controversy. Instead of merely identifying the criteria of evaluation employed in this episode, I pay special attention to how individual agents actually applied these criteria in concrete situations, what kinds of strategies or tactics they employed for the applications of these criteria, and how they created favorable conditions, both cognitive and social, for successfully applying these criteria. Individual agents' efforts in selecting application strategies and in creating favorable conditions made the practices of appraisal complicated, exhibiting various features that are incomprehensible if we limit ourselves merely to studying the criteria of evaluation.

I finally discuss a different approach to scientific change. The existing philosophical models of scientific change merely analyze the final product of science -- scientific theories, and ignore the impact of social factors and the role of individual agents. I suggest we concentrate on the process of knowledge production, and pay attention to individual agents's practices in this process, as well as to the relevant cognitive and social factors that influence individual agents. Following this new approach, scientific change is understood as an evolution that involves interactions among three elements: theory, experiment, and human agent.

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Chapter 1
Literature Review

There were two rival theories of light in Britain during the first half of the nineteenth century: the emission theory of light and the undulatory theory of light. The emission theory of light defined light as a sequence of rapidly moving particles that were subject to the laws of particle mechanics. The undulatory theory of light, however, regarded light as a disturbance in an all-pervading elastic medium called ether. In Britain, the controversy between these two rival theories of light began as early as 1803, when Henry Brougham, an emissionist, criticized Thomas Young's papers that advocated the undulatory theory. The results of this dispute were the defeat of the new-born undulatory theory and the dominance of the emission theory of light in Britain until the late 1820s. At the end of the 1820s, however, a group of young physicists, including John Herschel, George Airy, Baden Powell, and Humphrey Lloyd, adopted the undulatory theory, and, at the beginning of the 1830s, they started to attack their rival. After several heated debates mainly at the meetings of the British Association for the Advancement of Science, undulationists won a preliminary victory and successfully installed their views in the major British scientific societies and educational institutions in the mid-1830s. But emissionists, including David Brewster, Richard Potter, and Henry Brougham, did not immediately give up their objections to the undulatory theory after its preliminary victory. They worked hard to expose the problems of the undulatory theory by conducting a variety of experiments. The counter-actions of emissionists continued almost three decades after the undulatory theory's dominance in the mid-1830s. Finally these emissionists gave up their struggle and resigned from the controversy when a new generation of undulationists emerged. The undulatory theory of light finally replaced the emission theory of light after an extended period of controversy lasting from the early 1830s to the late 1850s.

The primary purpose of this chapter is to review previous studies on the history of optics in early nineteenth century Britain. In the following sections, I first trace the development of the historical works on early nineteenth century optics from Whewell in the last century to Cantor in the 1980s, and briefly outline the problems, both historical and philosophical, of these previous studies. I then sketch a blueprint of the whole dissertation, previewing the main points of each chapter.

1. Previous Studies

William Whewell provided the first historical survey of the emission-undulatory controversy in nineteenth-century Britain in his *History of the Inductive Sciences*, first published in 1837. Whewell's opinion on the history of optics was deeply informed by his philosophical theory. He argued that the development of science involved three different processes. The first step was a period he called the "prelude" when many basic facts were found but no high level generalizations. Following the "prelude" was a period he termed the "inductive epoch" during which "the inductive process by which science is formed, has been exercised in a more energetic and powerful manner" (Whewell 1967, Vol.1, p.12). After the "inductive epoch" was a period of the "sequel" in which the inductively established theory was extended and widely accepted. Using this philosophical model of science, Whewell divided the history of optics into three stages. The stage of "prelude" lay in the early optical researches in the seventeenth and the eighteenth century, including Newton's, Huygens's, and Euler's discoveries. The "inductive epoch" began in the early nineteenth century with the rise of the undulatory theory, particularly with the contributions of Thomas Young and Augustin Fresnel. The "sequel" was the confirmation and extension of the undulatory theory after 1830 conducted by Whewell's contemporaries (*Ibid.*, Vol.2, pp.312-73).

Whewell's opinion on the history of optics was contaminated by his strong commitment of the undulatory theory. Whewell was not a neutral historian, but an advocate of the undulatory theory, closely associated with such chief proponents of the theory as Airy and Herschel. He used the undulatory theory he advocated as a criterion to judge all other earlier and contemporary researches in optics. According to Whewell, the most important and significant researches in the development of optics were achieved in the "inductive epoch", exemplified by the works of Young and Fresnel. The undulatory theory of light had reached its peak by the late 1820s. While he claimed that the undulatory theory was the true theory of light, Whewell played down the significance and value of the emission theory in every historical period. After the undulatory theory was established through "the inductive epoch," he claimed that "we cannot properly say that there ever was an Emission Theory of Light which was the *rival* of the Undulatory Theory of Light" (*Ibid.*, Vol.2, p.482, original emphasis).[1] In his *History of Inductive Sciences*, Whewell mentioned almost nothing about the emission-undulatory controversy that occurred after 1830. His contemptuous attitude toward the emission theory led to a hostility to all emissionists. For instance, he implicitly blamed all seventeenth- and eighteenth-century emissionists, including Newton, for retardating the development of the new-born undulatory theory by Huygens and Euler. He also condemned Brougham, charging his attack on Young in 1803 to be "ignorance as well as prejudice" (*Ibid.*, Vol.2, p.347).

In 1916, Ernst Mach published *The Principles of Physical Optics: An Historical and Philosophical Treatment*. Heavily influenced by the positivist philosophy, Mach

[1] According to Whewell, the last struggle in favor of the emission theory happened in the early 1820s when Biot attacked Fresnel on the subject of polarized colors (Whewell 1967, Vol.2, pp.351-2).

aimed his book at revealing "the origin of the general concepts of optics and the historical threads in their development, extricated from metaphysical ballast" (1916, p.vii). Following this principle, he organized his book according to the conceptual structure of physical optics, which by the time he wrote had stabilized to include six major categories: linear propagation, reflection and refraction, colors, diffraction, periodicity, and polarization. He tried to show how these concepts had been formulated at the hands of prominent individuals and what transformations they had undergone in order to explain new facts. Except for internal developments of conceptions in optics, however, Mach paid very little attention to the emission-undulatory controversy.[2] In his few references to the controversy, Mach clearly expressed his hostile attitude toward emissionists. When he reviewed the Brougham-Young debate in 1803, for example, he called Brougham "a scientific reactionary" who upheld the Newtonian theory even to an iota (1916, p.275). Like Whewell, Mach in his book used the undulatory theory as a standard to assess the emission-undulatory controversy and did not believe that the critiques and challenges from the emission camp in the nineteenth century had contributed anything positive to the development of physical optics.

In later studies of the history of optics, the emission-undulatory controversy began to draw historians' attention. In Whittaker's *A History of the Theories of Aether and Electricity* (1960), and Ronchi's *The Nature of Light: An Historical Survey* (1970),[3] both authors provided detailed descriptions of the controversy before 1830, including such events as the debate between Brougham and Young in the early 1810s and the debate between Young and Laplace on double refraction in the early 1820s. Although Whittaker

[2] In the Preface of the book Mach promised that he would deal with such topics as the decline of the emission theory in a subsequent part of the book, but he never did so.

[3] Ronchi's book was originally published in Italian in 1939. The English version with some new material was translated and published in 1970.

and Ronchi have different ideas of the specific time of the final downfall of the emission theory -- Whittaker suggests 1832 when Airy applied Fresnel's theory in explaining Newton's rings experiment (1960, p.126), while Ronchi pinpoints 1819 when Fresnel won the prize from the *Acadèmiè des Sciences* (1970, p.251) -- both of them agree that the works of Fresnel were decisive in closing the controversy. Both Whittaker and Ronchi realize that, after the initial victory of the undulatory theory in the late 1820s and the early 1830s, emissionists "did not surrender nor did they lose hope of reinstating the concepts they had learnt in their youth" (Ronchi 1970, p.259). But both authors only trace the controversy up to early 1830s, implicitly assuming that nothing important occurred in the later controversy.

In the last few decades, more and more historians of science have concentrated on the episode of the emission-undulatory controversy in nineteenth-century Britain. They provide not only detailed descriptions of the episode but also analyses of the underlying causes for the victory of the undulatory theory. In his unpublished Ph. D dissertation, titled "The Reception of the Wave Theory of Light by Cambridge Physicists (1820-1850): A Case Study in the Nineteenth-Century Mechanical Philosophy" (1968), David Wilson carefully examines the reception of the undulatory theory by Cambridge physicists, claiming that the history of optics during the 1820s was mainly a story of the conflicts between the two rival theories of light, and that the victory of the undulatory theory in the early 1830s was due to its superior explanatory power and greater simplicity in contrast to the emission theory. Also in an unpublished Ph.D dissertation, "Natural Philosophy, Hypotheses, And Impiety: Sir David Brewster Confronts the Undulatory Theory of Light" (1972), Edgar Morse describes how Brewster kept challenging the undulatory theory of light throughout his life and attributes Brewster's persistent rejection of the undulatory theory to his epistemology, especially his conception of truth, and to his religious beliefs. Wilson's and Morse's researches shed light on the study of the emission-undulatory controversy, but, unfortunately, these two authors still focus their attention on the period of the 1820s and the early 1830s, just as their predecessors did.

The emission-undulatory controversy in nineteenth-century Britain has also drawn attention from philosophers of science. In his paper, entitled "Thomas Young and the 'Refutation' of Newtonian Optics" (1976), John Worrall gives a reconstruction of the replacement of the emission theory by the undulatory theory based upon Lakatos' philosophical model of science. He tries to show why the undulatory theory replaced its rival around 1830 rather than in the early 1800s when its major principles had already been established by Young. He argues that neither the emission theory nor the undulatory theory could make empirical progress, in Lakatos' technical sense, during the first two decades of the nineteenth century, and therefore no replacement could occur according to Lakatos' philosophical model. This situation did not change until Fresnel's analyses of diffraction and polarization made the undulatory theory empirically progressive. Hence, the replacement around 1830 was caused by the progressive status of the undulatory theory and the degenerating status of the emission theory, and was rational according to the Lakatos's philosophy of science.[4]

The most recent and systematic study of the history of optics in Britain is provided by George Cantor in his book, *Optics after Newton: Theories of Light in Britain and Ireland, 1704-1840* (1983). Beginning with a critique of Whewell's historiography, Cantor devoted a large portion of his book to the improvement of optics achieved by emissionists in the eighteenth century, which was overlooked by Whewell and most previous studies. Turning to the nineteenth century, Cantor examines the emission-

[4] I have criticized Worrall's reconstruction in an earlier paper (Chen 1988), arguing that Worrall overestimates the empirical progress made by Fresnel, and that the conceptual problems of the emission theory might be one of the causes of the replacement.

undulatory controversy both at the level of scientific theory and at the level of "scientific style," which includes the concerns of a discipline's domain, methods, problems, training patterns, audience, and its connections with other disciplines (1983, p.17). Cantor's conclusion is that the emission-undulatory controversy reflected a fundamental conflict at the level of scientific style: The rise of the undulatory theory represented the victory of a new style in physics and the decline of the emission theory was the result of the defeat of an old research style.

Cantor points to 1840 as the date of the final downfall of the emission theory, or, using his terms, the final stage of the optical revolution (*Ibid.*, p.186). Although he notes that the controversy between the two rival camps lingered on well into the 1850s, he implies that it is not worth devoting time to these later debates. His book covers only the period to 1840, and takes only two pages briefly to describe the state of optics and the emission-undulatory controversy after 1840. The way that Cantor handles the episode undoubtedly reinforces an illusion that has existed for a long time in the history of optics, namely, that nothing important or significant happened after the undulatory theory of light became dominant in the mid-1830s.

2. The Impact of Whewell's Legacy

Although some historians like Cantor have tried to escape Whewell's historiographic legacy, most previous historical studies of optics in early nineteenth century Britain are still deeply influenced by it. Whewell's historical judgment of the status of the two rival theories of light around 1830 was widely accepted by most previous studies. By labelling the period of Young and Fresnel "the inductive epoch", Whewell claimed that the undulatory theory had reached its peek in the hand of Young and Fresnel. This point was totally endorsed by Ronchi. He wrote that Fresnel's undulatory theory was as good as the one today, because Fresnel "had a complete grasp of the wave theory" and "use[d] this theory with a skill and finesse equal to that of the most expert in optics of our times" (1970, p.254 & 252).

Whewell's historical judgment on the status of the optical theories led to two widespread views among the historians of optics. First, since the fundamentals of the undulatory theory of light had been formulated in the 1820s, the emission theory of light would automatically die off after the undulatory theory became dominant, hence nothing in the optical controversy was important or significant after the mid-1830s. Consequently, there is a tendency to overlook the emission-undulatory controversy after the mid-1830s, a neglect shown by most previous historical studies of optics in early nineteenth century Britain. Second, since the undulatory theory had been proved to be true, every challenge to it after the mid-1830s was irrational and unscientific. Many previous historical studies on early nineteenth century optics adopt this Whiggish tone not only by looking down upon the works done by emissionists after the mid-1830s, but also by denouncing their resistance to the dominant theory. Even in the recent work of Cantor, who has paid great attention to the problems of historiography, we can easily detect a hostile attitude toward emissionists' resistance after 1840. According to Cantor, although the emission-undulatory controversy lasted well into the 1850s, it had nothing to do with the development of the undulatory theory and made no contribution to optical knowledge. "The controversy's long life span was due to the longevity of three of the wave theory's major opponents - Brewster, Brougham and Richard Potter - who, perhaps by Divine Providence, lived well into their eighties" (Cantor 1983, p.173). As for the fierce attacks launched by emissionists in the 1840s and 1850s, Cantor regards them only as "sabre-rattling," which should be "interpreted psychologically as an indication of defeat" (*Ibid.*, p.187). Cantor also denounces such emissionists as Brewster, who refused to accept the undulatory theory throughout his life, calling him "an embittered reactionary unwilling and unable to adapt to new, progressive intellectual currents"

(1981, p.67).[5] Hence, Cantor only gets rid of a part of Whewell's historiographic legacy, namely, the overlooking of the development of optics before the nineteenth century. And yet he still accepts the other part, namely, the neglect of the importance and significance of the emission-undulatory controversy after the mid-1830s.

In addition to Whewell's historical judgment, his philosophy of science also had a deep impact on previous studies of the history of optics in early nineteenth century Britain. The most important feature of Whewell's philosophy of science was his promotion of an hypothetico-deductive method. According to Whewell, "the hypotheses which we accept ought to explain phenomena we have observed", and "ought to foretell phenomena which have not yet been observed" (Whewell 1847, Vol,1, p.62). Because of Whewell's promotion of it, as well as other scientists' and philosophers' endorsements, the hypothetico-deductive method replaced the traditional inductive method between the 1830s and the 1840s and became the philosophical orthodoxy (Laudan 1981, pp.181-91). However, the rejection of inductivist methodology and the popularity of the hypothetico-deductive methodology led to a neglect of experiment (Nickles 1989; Gooding 1989). According to this methodology, experiment is always a form of empirical check on theories. Experiment is portrayed as the handmaiden of theory, with the sole function of enabling scientists to choose rationally between rival theories. When a selection between rival theories is needed, experiment will become significant. The results of the relevant experiments will be closely examined but the processes that produce these results will be taken as non-problematic. After the selection is done, experiment will be much less interesting. This was exactly the attitude taken in previous studies of optics in early nineteenth century Britain. Since the undulatory theory of light

[5] A similar opinion can be found in Worrall's recent work. Worrall claims that Brewster was irrational and unscientific when he continued to believe in the viability of the emission theory and resisted the undulatory theory after 1840 (Worrall 1990, p.350).

had been repeatedly tested between the 1820s and the early 1830s, those later experimental researches in optics, which constituted the main topic of the emission-undulatory controversy after the mid 1830s, become uninteresting.

The historiography of nineteenth century optics that primarily stems from Whewell's historical judgment and his philosophy of science is largely unfounded. The undulatory theory of light was first systematically introduced into Britain by Herschel in his comprehensive essay on light in 1827. According to Buchwald's recent study, however, Herschel at that time actually failed to appreciate the deep conceptual change caused by Fresnel's theory. The undulatory theory he understood and presented to his British fellows was in fact an incoherent one: a number of concepts from emission theory were still retained (Buchwald 1989, pp.291-6). In the early 1830s, the received undulatory theory of light in Britain was not Fresnel's. Even after Fresnel's profound conceptions of the undulatory theory had been comprehended by British physicists, we still cannot equate Fresnel's undulatory theory of light and the one we receive today. Fresnel's undulatory theory had fundamental differences from the current theory, now called the wave theory of light. To pick only one glaring example, the undulatory theory in the early nineteenth century was formulated within the framework of Newtonian mechanics, defining light as mechanical waves, rather than electromagnetic waves as we do at the present. Therefore, the differences between the nineteenth century undulatory theory and the one received today are fundamental. Underestimating these differences will be doomed to produce a misunderstanding of the later development of the undulatory theory, overlooking not only the significant attacks from the emission camp but also the important improvements made by the later undulationists.

The substantial differences among Fresnel's undulatory theory, its British version in the early nineteenth century, and today's wave theory of light, suggest that the long-lived span of the emissionists' resistance was not irrational. Even after the undulatory theory became dominant in the mid-1830s, it still had difficulties in explaining several important optical phenomena such as the dispersion and the absorption of light. The immature status of mathematical analysis further handicapped the explanatory ability of the undulatory theory in such crucial field as the diffraction of light. These problems and difficulties of the undulatory theory gave emissionists reasons for an extended rear-guard action and a hope of reviving their own theory. Moreover, the immature status of the undulatory theory during the 1830s and the 1840s suggests that the extended emission-undulatory controversy after the mid-1830 was not negligible. By employing their experimental skills, emissionists after the mid-1830s were able to make substantial contributions to experimental knowledge in optics, and stimulated the revision and extension of the undulatory theory. Without acknowledging the constructive role of these long-term interactions between undulationists and emissionists after the dominance of the undulatory theory, the progress of optical knowledge in this period would become incomprehensible. Lastly, the problems of the undulatory theory in the 1830s and the 1840s also indicate that its initial victory in the mid-1830s cannot be interpreted solely in terms of its explanatory successes. In addition to successful predictions, undulationists' control of the British Association and their monopoly of the teaching positions of natural philosophy in the major universities were also essential for their victory. Hence, without considering the roles of both cognitive and social factors, no one could provide a complete and accurate account of the optical revolution.

3. Overview

Unlike most previous studies on nineteenth-century optics in Britain that merely cover a short period between the 1820s and the early 1830s, I discuss a longer historical period that began in 1827 and ended in 1859. The beginning of this period was the date when Herschel published the first essay in Britain that systematically reviewed the emission theory and the undulatory theory and prepared a ground for the coming battles between the two rivals. The end of this period fell on the date when the last public attack of the undulatory theory from the emission camp was launched by Richard Potter, one of the most stubborn emissionists. In this dissertation, I also adopt a different historiographic or philosophical approach when I explore the underlying causes of the optical revolution. Most previous studies limit their attention to such cognitive factors as explanatory and predictive successes when they explain the optical revolution. Unlike these previous studies, in this dissertation I pay equal attention to both cognitive and social factors, taking not only theories' explanatory power but also actors' historical, social, and political characters into account.

The first part of this dissertation, from chapter two to chapter four, covers the first stage of the emission-undulatory controversy. This is a stage in which the undulatory theory was introduced into Britain and gradually became dominate.

Through examining the works of Herschel and Brewster, in chapter two I review the contemporary attitudes toward the two optical theories during the late 1820s. In this period, Herschel, an advocate of the new undulatory theory, and Brewster, a defender of the old emission theory, displayed a considerable consensus in their assessments of the two theories of light. Both of them agreed that the undulatory theory was superior in explaining facts, and yet it was still reasonable to continue optical research in the emission tradition. This consensus indicates that logical inconsistencies between rival theories may not necessarily lead to debates between rival scientists. Rather, controversies in science are embodied in scientists' practices, produced and controlled by the relevant actors.

In Chapter three I document the first-round confrontations between emissionists and undulationists between 1830 and 1833. These confrontations included the debate between John Barton and Powell on diffraction, the debate between Potter and Airy on interference, and the debate between Brewster and Herschel on absorption. In these debates, emissionists attempted to use experimental findings to challenge the undulatory theory. But not all of their experimental works actually became bases for theory testing. Whether an experimental findings could be used to test a theory in fact also depended upon a series of contextual factors, including the social characters of relevant actors.

In chapter four I examine the processes through which undulationists controlled the major British scientific and educational institutions in the mid-1830s. Many previous studies attribute the causes of the undulatory theory's victory in the mid-1830s to its successful predictions based on Newton's rings and on conical refraction. But historical actors did not interpret the undulatory victory in this way. In fact, two battles, one in the British Association and the other in Edinburgh University, were crucial for the undulatory victory. These battles were primarily political operations, including the selection of Lloyd to make the report of optics at the 1834 British Association meeting, and the defeat of Brewster by James Forbes in the competition for the teaching position in Edinburgh University. These two battles demonstrate that experimental successes *per se* could not produce the undulatory dominance, and that only when contextual factors are considered can the undulatory victory in the mid-1830s become comprehensible.

The second part of this dissertation, from chapter five to chapter seven, covers the second stage of the emission-undulatory controversy. This is a stage in which the emissionists fought a long-lived defense and gradually died out.

The undulatory dominance did not immediately stop emissionists' counter-actions. In chapter five I report a number of events between the late 1830s and the early 1840s in which emissionists attempted to overthrow the undulatory dominance through deliberate observations and experiments. Emissionists in this period chose the topics of refractive indices, the intensity of light in diffraction, and a newly discovered phenomenon called "polarity of light" as their targets, showing the explanatory incompleteness of the undulatory theory. These attacks were successful: undulationists had to publicly defend their theory, sometimes even openly admitting its defects. The results of these debates do not coincide with modern cognitive judgments of the two rival theories of light; indeed, the results show that the relationship between the rival scientific theories was not solely determined by their cognitive content, but mediated by human actors.

Facing the problems exposed by emissionists, some undulationists in the mid 1840s began to doubt their own theory. In chapter six I document the doubt, suspicion, and skepticism, among three undulationists: James MacCullagh, Herschel, and Robert Moon. All of these doubts among undulationists represented a setback in the development of the undulatory theory, and indicate that a cognitively superior theory, judged by modern standards, may not necessarily win the competition with its rival all the time. The results of the competition between rival scientific theories also depend upon the involvement of relevant human actors.

Because of undulationists' doubts, emissionists in the late 1840s were still hopeful about the future of their own theory. In chapter seven I describe how emissionists attempted to revive their own theory in the 1850s and how this attempt finally failed. Emissionists in this period mapped out a final campaign to give the undulatory theory a vital punch. They recruited new allies who had both scientific credentials and political power, explored such new fields as photography and photochemistry to look for new evidence, and endeavored to develop a new emission theory that was able to explain more phenomena than predecessors. In this final campaign, emissionists adopted a deliberate tactic: they did not directly attack undulationists who were powerful in controlling scientific institutions, but only presented experimental findings that could not be explained by either theory. In planning this tactic, they were concerned more about who had power than about how the experiment turned out, another example showing that the relationship between theory and experiment was mediated by human actors.

The last parts of this dissertation, chapter eight and chapter nine, are theoretical accounts of the extended emission-undulatory controversy, drawing in methods and concepts from history, philosophy and sociology of science. The multi-disciplinary account of the episode supplies a critique of some simplistic theories of scientific change in the received philosophy of science and the sociology of science. My theoretical interpretation of the historical episode demonstrates that the replacement of the emission theory by the undulatory theory of light in nineteenth-century Britain was such a complicated historical process that any single disciplinary account is unlikely to do justice to it.

In chapter eight I provide a theoretical discussion on the underlying mechanisms of the extended emission-undulatory controversy. Several "why" questions are asked. First, why did the emission-undulatory controversy last such a long period after the fundamentals of the undulatory theory had been formulated in the 1820s? Second, why did the controversy come to its closure at the end of the 1850s? And third, why did the controversy end in such a peculiar way, namely, when one side voluntarily resigned and the other was reluctant to launch any further attack? Two historical events can give us hints to answer these questions. These were the popularity of Humboldtian science in the early nineteenth-century Britain and the emergence of a new generation of undulationists at mid-century, which were responsible both for the longevity of the controversy as well as for the timing and the features of its closure. But these events do not fit the cognitive-social dichotomy accepted by both philosophers and sociologists of science. These factors suggest that an alternative historiography is needed for understanding the emission-undulatory controversy.

Chapter nine is a philosophical analysis of appraisal in the emission-undulatory controversy, including the evaluations of theory and of experiment. In this chapter I select several cases in the controversy to illustrate the patterns and the characteristics of both theory appraisal and experiment appraisal. These cases exemplify the complexities of appraisal practice, most of which are incomprehensible in terms of received philosophies of science. To understand these historical complexities, an alternative theoretical perspective is needed. This new theoretical perspective defines appraisal as a practice and emphasizes its procedural and informal aspects. Second, this new perspective highlights the role of individual actors in determining the goals and the tactics in their appraisal practices. Lastly this perspective demonstrates how human actors mediate the relationship between theory and experiment, as well as the relationship between rival theory, how social factors inevitably enter the process of appraisal, and how the results of appraisals depend both on the interactions between actors and the natural world, as well as the interactions among actors.

Chapter 2

The Prelude to the Controversy

Since Newton's endorsement in the late seventeenth century, the emission theory of light had dominated the field of optics in Britain for more than a hundred years. Throughout the eighteenth century, optical researches in Britain were conducted within the emission framework, and considerable progress was made during this period.[1] This emission influence existed well up to the first quarter of the nineteenth century. The emission dominance, however, became shaky when John Herschel in 1827 published an essay systematically reviewing the two rival theories of light and preparing a ground for the coming battles. The publication of this essay signified the beginning of the showdown between the emission theory and the undulatory theory in Britain.

In this chapter I reveal the attitudes of the major members of the optical community toward the two rival theories of light immediately after the undulatory theory was introduced into Britain. I narrow my analysis to two important actors in this period: one is John Herschel, who introduced the undulatory theory into Britain and represented the advocates of the new theory, and the other is David Brewster, who faithfully accepted the emission theory and represented the defenders of the old tradition.

Since Herschel and Brewster represented different traditions and had conflicting interests in their theory appraisals, we may expect that confrontations between them were inevitable. However, we soon find that there was no direct confrontation between the representatives of the rival theories during this period. Instead, Herschel and Brewster

[1] For detailed discussion of the emission influence and dominance in the eighteenth century, see Steffens (1977), Canton (1983), and Pav (1964).

shared many positions in their evaluations of the two rival theories of light. This raised an interesting question: why the conflicts between the emission theory and the undulatory theory, which were logically inconsistent in many aspects, did not inevitably produce confrontations between their advocate.

1. The Introduction of the Undulatory Theory to Britain

In the first quarter of the nineteenth century, the emission theory of light dominated the field of optics in Britain. Partially because of its explanatory successes developed in the seventeenth and the eighteenth century, and partially because of the recommendation of such high an authority as Newton, the emission theory was easily received and became very popular. When Thomas Young published a series of papers to advocate the undulatory theory of light between 1799 and 1803, he was immediately in an awkward predicament. Henry Brougham launched fierce attacks against Young in three reviews published in the *Edinburgh Review*. In these reviews, Brougham accused Young of using an hypothesis that "is a work of fancy, useless in science" (1803a, p.451). He objected to Young's attempt to introduce the idea of ether by quoting Newton's name (*Ibid.*, p.455). He also challenged Young's experimental skill, claiming that Young did not provide any new observations and experiments (1803b, p.457). Brougham's attacks were disastrous to Young. Although Young later wrote a reply to Brougham, this response had to be published in the form of a pamphlet and had very little circulation.[2] According to William Whewell, Brougham's attacks of Young had the effect of successfully stopping the spread of Young's undulatory theory (Whewell

[2] Young, (1804b). According to Young's own statement, only one copy of this pamphlet was sold. See Peacock (1855b, p.215).

1967, Vol.2, pp.347-8).[3]

During the same period, the undulatory theory of light developed rapidly on the other side of the channel. Due to the works of Dominique-Francois Arago and Augustin Fresnel, the undulatory theory became widely accepted in France in the 1820s (Buchwald, 1989). When this theory was re-introduced into Britain in the late 1820s, a long controversy on the nature of light began.

It was John Herschel (1792-1871) who introduced the undulatory theory of light to Britain from France. As Sir William Herschel's only child, John Herschel's career choice was strongly influenced by his father. John devoted most of his life to astronomical study, but he took up this discipline out of a sense of "filial devotion": to continue his father's work. In fact, optics was a more attractive subject than astronomy to Herschel, at least when he was young. Herschel began his optical research as early as 1808, when he was only 16 year old. He later recalled that "light was my first love" (Buttmann 1970, p.27).

Under the dominance of the emission theory, it was not a surprise that Herschel's early optical research was done within the emission framework. Herschel was particularly influenced by Jean-Baptiste Biot's version of emission theory. In order to explain the phenomena of polarization, Biot in 1812 proposed a "theory of oscillations," or a theory of "mobile polarization" as he later labeled it. This theory accounted for the polarization of light in terms of "a succession of oscillations that the luminous molecules experience about their center of gravity, in virtue of attractive and repulsive forces that act on them" (Buchwald 1989, p.99). Herschel first learned the outline of Biot's theory of mobile polarization in 1818, and later committed himself fully to this theory after he visited Biot in 1819.

[3] A similar opinion can be found in Peacock (1855a, p.182). For a different opinion of the effect of Brougham's attack, see Worrall (1976, pp.107-10).

Herschel began in 1819 to study the phenomena of double refraction and polarization in biaxial crystals, and published an extended paper on this subject in 1820, titled "On the action of crystallized bodies on homogenous light". In the paper, Herschel suggested a new method to observe double refraction and polarization in biaxial crystals. Instead of directly measuring the angular deviation of the extraordinary rays, Herschel noted that the graduated colors at the surfaces of the crystals were a more sensible and more precise indicator for determining the intensity of the responsible optical force. Using this method, he reported several experiments in which biaxial crystals, when exposed to polarized light, developed graduated colors differing from those observed and reported by Newton. These new phenomena, Herschel claimed, could not be directly explained by any existing theory, including Biot's. His solution was a revision of Biot's mobile theory of polarization. Instead of assuming a single set of axes for all the colors, Herschel proposed that the depolarizing axes in some crystals were different for different colors. By assigning different sets of axes to each color and considering the effects of optical forces exerted from these axes, Herschel claimed that the deviations of the graduated colors could be explained (1820, pp.62-73). Although Herschel attempted to use neutral language to describe his results and even claimed that his analytical expression for the graduated colors at the surfaces of biaxial crystals was compatible with both the emission and the undulatory theory, this paper was certainly not a neutral experimental report. By appealing to the concept of optical forces and the assumption of the oscillations of luminous molecules, Herschel in this paper clearly demonstrated his commitment to the emission theory.

However, Herschel was not a dogmatic follower of the emission theory. Instead, he kept his eyes open to any new development in the field of optics. The explanatory successes of Fresnel's undulatory theory in the early 1820s made a deep impression on Herschel, and around 1824, he decided to write an essay systematically to review the two theories of light. In his diary entry on October 27, 1824, Herschel wrote that he "[b]egan an essay on physical optics" (Buttmann 1970, p.43). This note marked the beginning of an important essay that later drew great attention from the optical community in Britain.

In fact, Herschel may probably be the only person in Britain at the time who was able to give a thoughtful account of both the emission theory and the undulatory theory of light.[4] Undoubtedly, his long-term commitment to the emission theory made him particularly familiar with its doctrines. On the other hand, with an excellent education in mathematics, he did not have too many difficulties in understanding and representing the highly mathematical undulatory theory. Before he completed his essay, Herschel even found several problems about double refraction that Fresnel had not fully explained. He then wrote to Fresnel, asking details on the laws of double refraction in unpolarized and polarized light, and the intensity of the partial reflected light on a crystalline or noncrystalline surface.[5] Fresnel later cleared up these questions in his second memoir on double refraction published until 1827.

Herschel finished his essay on December 12, 1827 (Schweber 1981, p.147). It filled 245 quarto pages. Although the essay was not published until 1845 in the *Encyclopaedia Metropolitana*, it was privately circulated in the optical community immediately after it was completed. From the spring of 1828, Herschel sent copies of his essay, titled "Light", to a number of people, including William Whewell, Thomas Young, David Brewster, George Airy, William Hamilton, and William Fox Talbot

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[4] According to Brewster, Herschel was the only person in *Europe* who was able to do so. See Brewster to Herschel, (December 6, 1828), Royal Society Library, Herschel Paper, HS. 4.261.

[5] Herschel's questions appear in Fresnel's reply. See Fresnel (1965, Vol.2, pp.647-60). A translation of these questions is provided by Buchwald in Buchwald (1989, p.291).

(Cantor 1983, p.162). Herschel did not expect that his essay would bring about any strong reaction, and was a little surprised when he received an immediate response. In writing to his aunt, Herschel said that his essay "has excited a much greater sensation than I expected it would" (Buttmann 1970, p.61). Neither did Herschel anticipate that his essay would provide a ground for a long-lived controversy on the nature of light that lasted more than three decades in Britain.

2. Herschel on the Optical Theories

By the time when Herschel completed his essay on "Light", he had become more and more favorable to the undulatory theory of light. At the same time, however, he did not completely reject the emission theory, even though it contradicted the undulatory theory in many ways. In this section, I give a detailed analysis of Herschel's attitude toward the two rival theories of light. I first examine Herschel's methodology, which deeply shaped his opinion in theory appraisal. Then I describe his comparison and judgment on the explanatory powers and the status of the two rival theories.

2.1 Herschel's Methodology

Herschel's attitude toward the emission theory and the undulatory theory was in a large degree shaped by his methodology, which followed the basic tenets of the Scottish philosophy, or, as some people called it, the Common Sense methodology (Olson 1975, pp.252-70). In his methodological manual, *Preliminary Discourse on the Study of Natural Philosophy*, published in 1830 for the *Cabinet Cyclopedia*, Herschel clearly expressed his idea on what a good hypothesis or a good theory should be.

According to Herschel, an hypothesis is "a most real and important accession to our knowledge" because "it serves to group together in one comprehensive point of view a mass of facts almost infinite in number and variety, to reason from one to another, and to establish analogies and relations between them" (1830, p.262). Through providing explanations for different kinds of phenomena, an hypothesis could function as a guide to understand "the mutual connection . . . of two classes of individuals" (*Ibid.*, p.101).

Herschel also pointed out that science needs "the knowledge of the hidden processes of nature in their production" (*Ibid.*, p.191). But to obtain this knowledge involves the discovery of the actual structures and mechanisms of the universe and its parts. Detection of these structures and mechanisms may go beyond our ability, because they are, for the most part, "either on too large or too small a scale to be immediately cognizable by our senses" (*Ibid.*). We might formulate hypotheses about these hidden structures and mechanisms, but only in few cases are we able to know if our hypotheses truly represent all the facts.

Nevertheless, Herschel was still confident about the positive functions of hypothesis. He argued that, although an hypothesis could not tell us about the true state of hidden causes, it was able to supply us with valuable suggestions. The major function of an hypothesis is to "serve as a scaffold for the erection of general laws." Hence, Herschel reminded his readers:

> Regarded in this light, hypotheses have often an eminent use: and a faculty in framing them, if attended with an equal facility in laying them aside when they have served their turn, is one of the most valuable qualities a philosopher can process; while, on the other hand, a bigoted adherence to them, or indeed to peculiar view of any kind, in opposition to the tenor of facts as they arise, is the bane of all philosophy. (*Ibid.*, p.204).

It is very clear that, for Herschel, the primary values of hypotheses are their explanatory powers and their fruitfulness or suggestiveness rather than their certainty or truth. Based on such a methodology, Herschel concentrated his attention on the aspects of explanation when he evaluated the emission theory and the undulatory theory.

2.2 The Comparison of Explanatory Power

Herschel did not doubt the explanatory power of the emission theory of light. He said:

> This [emission] hypothesis, which was discussed and reasoned by Newton in a manner worthy of himself, affords, by the application of the same dynamical laws which he had applied with so much success to the explanation of the planetary motions, not merely a plausible, but a perfectly reasonable and fair explanation of all the *usual* phenomena of light known in his time. (*Ibid.*, pp.250-1, original emphasis)

In his "Light," Herschel carefully examined all the phenomena of light that the emission theory could explain. These included a group of phenomena already known in Newton's time, such as reflection and refraction, total reflection, double refraction, colors in thin plates, colors in thick plates, colors of the sky, and colors of natural bodies. Herschel also noted that the emission theory at the beginning of the nineteenth century could continue to improve its explanatory power to fit the newly discovered facts. For example, by using the principle of least action, Laplace could supply an emission account of double refraction that strictly and quantitatively explained the phenomenon.

But the emission theory of light had difficulties in explaining a very important group of optical phenomena -- those involving the diffraction of light. Herschel used a simple experiment to illustrate the problems of the emission theory. It was an experiment in which diffraction was produced by a small opaque body. When the distance between the opaque body and the light source changed, the diffractive fringes expanded considerably. Herschel noted that this fact was evidently incompatible with the emission account. The emission theory attributed the cause of diffraction to the deflecting force emanating from the opaque body, and implied that the change of the distance between the opaque body and the light source would not have any impact on the pattern of the diffractive fringes (Herschel 1827, p.481).

The emission theory also had troubles in explaining a series of optical phenomena just discovered at the beginning of the nineteenth century, especially those in the field of polarization. In his essay "Light," Herschel drew the attention of his readers to the newly discovered phenomena related to polarization. Herschel's essay is divided into four parts. The first and the second part are about optical instruments and the theory of colors, the third part introduces the two main theories of light and explores all unpolarized phenomena, and the last part focuses on the phenomena of polarization. By devoting a relatively large portion of his essay to polarization, Herschel apparently tried to convince his readers that polarization was the most important developing frontier in the field of physical optics. The emission theory, however, was particularly weak in this subject. For most of the polarization effects, the emission theory could not provide any satisfactory account. Although some emissionists could explain a few polarization effects by adding *ad hoc* hypotheses to their systems, like Biot's assumption that luminous molecules rotated about their axes, Herschel noted that these emission accounts were obtained "with a great sacrifice of clearness of conception" (*Ibid.*, p.529).

The undulatory theory, however, exhibited excellent explanatory power for the phenomena that presented the greatest difficulties to the emission theory, such as those in the field of diffraction and polarization. For diffraction, the undulatory theory could perfectly explain every detail of the diffractive fringes, including the distances of the fringes from the geometrical shadow and from each other. The ability to account for the alternations of diffractive fringes when the distance between the diffracting body and light source changes, according to Herschel, was "the strongest fact in favor of the undulatory doctrine" (*Ibid.*, p.483). The undulatory advantages in the field of polarization were even more evident, Herschel argued. Throughout the last part of his essay on light, Herschel used undulatory language to describe and explain all kinds of polarization effects, including polarization by reflection and refraction, polarization by double refraction, colors of polarized light, interference of polarized light, and circular polarization. The undulatory explanations of these phenomena, Herschel claimed, were "really the most natural," adapting themselves "with the least violence and obscurity to the facts" (*Ibid.*, p.529).

But the undulatory theory was not perfect. It still had several difficulties in explaining optical phenomena, especially the dispersion of light. According to the undulatory doctrines, Herschel pointed out, the velocity of propagation of the light wave depended solely on the elasticity of the medium, bearing no relation to the original disturbance. Thus, the undulatory theory asserted that light of every color travels with one and the same velocity in a homogeneous medium. In the phenomenon of dispersion, however, the deviation of light by refraction is evidently a consequence of the difference of the velocities of light within the refracting medium. "Now here arises, *in limine*, a great difficulty; and it must not be dissembled, that it is impossible to look on it in any other light than as a most formidable objection to the undulatory doctrine" (*Ibid.*, pp.449-50).

Neither the emission theory nor the undulatory theory could, therefore, "furnish that complete and satisfactory explanation of *all* the phenomena of light which is desirable" (*Ibid.*, p.450, original emphasis). But Herschel did not regard these two theories of light as having equal explanatory power because not all the fields in optics were equivalent. For him, the phenomena of polarization were certainly more important and more valuable than those of unpolarization, because polarization effects were newly discovered and apparently represented the developing frontier of the discipline. Herschel thus gave more weight to the undulatory successes in the field of polarization. And despite the apparent equivalence of the explanatory powers of the two rival theories in some fields, Herschel had his own way to make a comparison:

> When two theories run parallel to each other, and each explains a great many facts in common with the other, any experiment which affords a crucial instance to decide between them, or by which one or other must fall, is of great importance. (1830, p.206).

One such crucial experiment involved the colors of thin plates. The arrangement of this experiment was similar to the one for producing Newton's rings, except that a prism rather than a convex plate of glass was placed on a flat plate of glass. A series of alternate dark and bright stripes could be observed between the prism and the flat plate. Both the emission theory and the undulatory theory were able to explain the generation of these stripes, but the former predicted that the intervals between the bright stripes ought to be half bright, and the latter, absolutely black. The observational results of the experiment were in favor of the undulatory theory, and provided more evidence to demonstrate the explanatory superiority of the undulatory theory (Herschel 1827, p.473; 1830, p.207).

2.3 The Judgment on the Two Rival Theories of Light

Herschel certainly had a preference for the undulatory theory over the emission theory by 1827, because of the former's superiority in explaining a variety of optical phenomena. Herschel, however, did not regard the undulatory theory as really representing the nature of reality. He reminded his audience:

> We shall adopt, therefore, in the remainder of this essay, the undulatory system, not as being at all satisfied of its reality as a *physical fact*, but regarding it as by far the simplest means yet devised of grouping together, and representing not only all the phenomena explicable by Newton's doctrine, but a vast variety of other classes of facts to which that doctrine can hardly be applied without great violence, and much additional hypothesis of a very gratuitous kind. (1827, p.475, original emphasis)

Thus, Herschel was reluctant to commit himself fully to the undulatory theory. The value of this theory consisted only in its successes in explaining a variety of groups of phenomena, but not in representing real physical facts. Clearly, this judgment of the undulatory theory was consistent with Herschel's own methodological principle, which emphasized the explanatory function of hypotheses rather than their certainty or truth.

The obstacle that preventing Herschel from fully committing himself to the undulatory theory was the problem of the ether. Herschel did not separate the undulatory theory from ether mechanics. His primary reason was the need to explain the production of our sensations. He argued that "to put in motion the molecules of the nerves of our retina with sufficient efficacy, it is necessary that the almost infinitely minute impulse of the adjacent ethereal molecules should be often and regularly repeated, so as to multiply, and, as it were, concentrate their effect" (*Ibid.*, p.450). Hence, discussing the problems of ether, including its properties and its relationships with light vibrations and with sense organs, were the legitimate questions for the undulatory theory.

But, Herschel admitted, it was just impossible to obtain certain knowledge of the ether, because the particles of the ether were so tiny that they lay beyond the limit of direct observation. Every existing model of the ether involved some kind of defect, more or less contradicting the existing mechanical knowledge. The reason to keep a model of the ether consisted only in its function in providing uniform explanations of a variety of optical phenomena. The existence of ethereal particles was not a demonstrated fact, but only a kind of *locum teneus*. In this sense, the undulatory theory did not correctly describe or represent the underlying mechanisms and the hidden interactions, and fell short of the truth.

Herschel, however, did not regard representing the hidden physical facts as the necessary condition for a good, or a preferable, hypothesis. In his opinion, the most important thing was the ability to group different kinds of phenomena together through explanations. Since the undulatory theory was good at this, Herschel would not reject it simply because of its troublesome assumption of ether. Herschel claimed:

> [I]f the phenomena can be thereby accounted for, i.e. reduced to uniform and general principles, we see no reason why that [the assumption of ether], or any still wilder doctrine, should not be admitted, not indeed to all the privileges of a demonstrated fact, but to those of its representative, or *locum tenens*, till the real truth shall be discovered. (*Ibid.*, p.535)

The undulatory theory consequently should not be condemned because of its ether assumption. The problems associated with ether could be shelved as long as the ether assumption was able to help the generalizations and explanations of optical phenomena. Herschel even went a step further, attempting to persuade his readers to tolerate some explanatory failures of the undulatory theory. After describing the problem of dispersion for the undulatory theory, for example, Herschel asked his reader to "suspend his condemnation of the doctrine for what it *apparently* will not explain" (*Ibid.*, p.450, original emphasis). Here Herschel's reasoning was clear: Since representation of physical facts was not a requirement for a good or preferable hypothesis, and since the undulatory theory already had enough successes in accounting for a variety of phenomena, its occasional failures in explanation should not be the deciding factor in our judgment of the theory.

In comparison to the undulatory theory, the emission theory was certainly inferior in explanatory power. Based on this consideration, Herschel had given up his preference for the emission theory by the time he wrote his "Light." But during a rather long period after he established his preference for the undulatory theory, Herschel did not think that the emission theory should be totally rejected because of its inferior explanatory power. This judgment of the emission theory was consistent with his methodology. For Herschel, hypotheses were a kind of intellectual tool, or a "scaffold", for approaching the general laws of the nature, but they did not represent the laws directly. Consequently, although the scaffold provided by the emission theory was not so good as the one provided by the undulatory theory, keeping it for a while might still be beneficial, and would not bring about any harm.

Instead of rejecting the emission theory immediately, Herschel suggested, even after he had realized the superior explanatory power of the undulatory theory, that the emission theory could be improved. Herschel claimed:

> [I]t is by no means impossible that the Newtonian theory of light, if cultivated with equal diligence with the Huyghenian, might lead to an equally plausible explanation of phenomena now regarded as beyond its reach. (1830, p.262)

Those who did not accept Herschel's methodology found this opinion hard to understand. Whewell later criticized Herschel, charging that this point "was certainly untenable after the fair trial of the two theories in the case of diffraction, and extravagant after Fresnel's beautiful explanation of double refraction and polarization" (1967, Vol.2, p.349). But Herschel had insisted on this point for a long time, at least up to 1845.[6]

Herschel not only suggested improving the emission theory, he also devoted himself to the project of constructing a new emission theory of light. In a letter sent to Richard Potter in 1832, Herschel described in detail his work on improving the emission theory.[7] In this letter, Herschel told Potter that he had developed the emission theory further than he ever detailed in print. The main idea of Herschel's new emission theory was developed from Biot's theory of mobile polarization, assuming a rotatory motion of the particles of light about their axes. Herschel made the following five postulates the major assumptions of the new emission theory:

1) One rotation of a particle is completed as it travels the distance between particles;

2) Rays differ in the nature of the particles and the distance between them;

3) The rate of rotation and the inter-particle distance change in the same ratio at the boundary of a new medium;

4) The rotatory velocity changes during reflection and refraction;

5) The axes of rotation may be oriented in any way to the direction of the ray.

Using this new model with rotating particles, Herschel claimed he could explain many optical phenomena that were difficult for the Newtonian version of the emission

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[6] A detailed discussion on Herschel's opinion on this topic in 1845 will be given in chapter 6.

[7] Herschel to Potter, (April 20, 1832), Texas University, Herschel collection, UT. L0315. I am in debt to Gregory Good for the discovery of this important source. See Good (1982).

theory. As an example, he outlined an explanation of the interference of polarized light. He proposed that a ray of light is composed of a series of rotating spherical particles at equal intervals and all in the same phase. He further assumed that each particle of light has two poles of opposite "qualities." If two of these particles are side by side, they either reinforce or "neutralize" each other, depending on the cosine square of half the angle between their axes of rotation. The interference effect of polarized light thus could be explained in terms of the interactions of the particles of light. In the letter, Herschel reminded Potter that he had said, in his *Preliminary Discourse on the Study of Natural Philosophy*, that the phenomena of interference "would be readily enough explained" by the emission theory without the admission of ether (1830, p.263). Herschel proclaimed that this was not said hastily, and that the problem had been solved by his "perfectly distinct and definite" idea of a new emission theory.

After tracing Herschel's evaluations and judgments of the emission theory and the undulatory theory in detail, it is evident that Herschel in this period did not fully commit himself to either theory of light. Herschel clearly expressed his strong preference for the undulatory theory, because of its superior explanatory power, but he did not regard it as true, due to its assumption of the ether. On the other hand, he realized the inferior status of the emission theory in giving explanations, but did not want to reject it immediately. Instead, he attempted to improve the emission theory, hoping it could become a complement of the undulatory theory. In his letter to Potter, Herschel summarized his position on the two theories of light in a clear and definite way, claiming that, although he declared that the undulatory theory was superior in explanatory power, "I should be sorry to have expressed myself in the language of a partisan, a character in my opinion incompatible with that of a philosopher."[8]

[8] Herschel to Potter, *op. cit.*, note 5.

Herschel's judgment of the two rival theories of light was not a hasty conclusion. Instead, it was a logical consequence of his methodology, which emphasized the explanatory rather than the representative function of hypotheses. In the later long-term controversy between the emission theory and the undulatory theory, Herschel did not change his basic position on the two rival theories of light. Consequently, he could criticize the emission account at one time and on a particular topic, when he found that the undulatory theory could offer a better option. And based on the same rationale, he could prefer the emission account at a different time and on a different topic. These different preferences were not contradictory. Herschel was just choosing different intellectual "scaffolds" at different times in order to approach the real physical facts.

3. Brewster on Optical Theories

Brewster was a faithful defender of the emission theory of light. But at the same time, he openly admitted its explanatory inferiority, and hoped that he could improve its explanatory power by incorporating the interference principle into the emission theory. In this section, I first briefly sketch Brewster's early optical research, which not only shaped his theoretical opinion, but also earned him prestige in the field of experimental optics. Then I summarize Brewster's evaluations of the two rival theories of light, which had many similarities with those of Herschel.

3.1 Brewster's Early Optical Research

David Brewster (1781-1868), the son of a grammar school rector, entered the University of Edinburgh at a very early age, and pursued his studies under John Robinson, John Playfair, and Dugald Stewart. He attended Stewart's moral philosophy class, and read intensively from the Common Sense philosophers, including Stewart and Thomas Reid (Olson 1982, p.178). Brewster was thus thoroughly acquainted with the

Scottish tradition, and his methodology of science, which determined his positions in the later emission-undulatory controversy, was deeply shaped by the principal doctrines of Common Sense philosophy.

Brewster began his optical experiments about 1799 when he was still at the University, probably due to the influence of his classmate Henry Brougham (Forbes 1858, p.113). In 1813, he published his first book, *A Treatise of New Philosophical Instruments*, in which he described many new or improved optical instruments, and reported his measurements of the refractive powers and dispersive powers of a great number of substances (Levene, 1966). Just before he completed his book, Brewster learned about Malus' discovery of polarization by reflection, and quickly devoted himself to this new field. Through a series of experiments, Brewster found that light was also polarized by oblique refraction. By the end of 1813 he had determined the law of polarization in the case of successive refraction by a pile of thin glass plates (Brewster 1814, p.221). In 1814, Brewster investigated the law of polarization by reflection, finding the so-called Brewster law, namely, that the angle of polarization by reflection is related to the refractive index of the reflecting material (Brewster 1815a, p.126). In addition to experimental explorations, Brewster also supplied a theoretical explanation of the polarizations by reflection and refraction, attributing their causes to the "polarizing forces" that "rotated" the particles of light (*Ibid.*, p.149). The optical community soon recognized Brewster's discoveries. The Royal Society of London in 1815 awarded Brewster the Copley Medal for his studies of polarization, and elected him Fellow of the Society.[9]

[9] Brewster also received one-half of the prize of three thousand frances from the France Institute in 1816 for his work on polarization, which was praised as one of the two most important discoveries in physical science made in europe between 1814 to 1815. See *The Encyclopedia Britannica* (13th Edition, 1926), Vol.4, p.513.

Brewster also conducted experiments in such fields as metallic reflection, optical mineralogy, and absorptive spectroscopy. In 1819, he received the Rumford medal from the Royal Society for his study of the interference pattern produced by polarized light through crystals. In 1830, he won another medal from the Royal Society for his discoveries of the laws of polarization by refraction and by pressure. Through these successes, Brewster established his prestige in optics, especially in optical experiment. For example, James Forbes complimented Brewster's original discoveries in physical optics, claiming that "few people have made with their own eyes so vast a number of independent observations more faithfully" than Brewster did (Forbes 1858, p.118). And William Whewell, although he disliked Brewster's theoretical viewpoint, still admitted that Brewster was "the father of modern experimental optics" (Whewell 1967, Vol.2, p.373).

Due to his high prestige, Brewster's works on optics were very influential both in the optical community and among general scientific audience. During this period, he published two comprehensive works on optics, mainly presenting his experimental results within the emission framework. The first one is an essay titled "Optics" for the *Edinburgh Encyclopedia* (Brewster 1822). Another is a book, *A Treatise on Optics*, written for the *Cabinet Cyclopedia* (Brewster 1831a). This book was very successful: over four thousand copies were sold within the first year of publication.[10]

3.2 Brewster on the Emission Theory

In an 1822 essay, Brewster claimed that the true "nature of light is absolutely unknown, and probably will remain among the arcana of science" (1822, p.499). In his 1831 book on optics, he was still reluctant to answer the question whether light is particles or undulations (1831, pp.1-2). But Brewster clearly committed himself to the

[10] See The House of Longman, (1978), *Archives of the House of Longman, 1794-1914*, D8, Cambridge.

emission theory in his early optical researches. Following the emission tradition, he attempted to define every optical phenomenon as the result of the interaction between light and matter, and interpret it in terms of particles and forces. He explained the phenomena of polarization, as already noted, in terms of "polarizing forces" that rotated the particles of light and produced an ordered arrangement. For the phenomena of double refraction, Brewster attributed the cause to the attractive forces that emanated from the axes and acted differentially on the ordinary and extraordinary rays (Brewster 1822, p.747). Also following the emission tradition, Brewster accepted Newton's theory of fits. He praised the value of this theory as "a general method of expressing all the various facts which he [Newton] had observed" (1815, p.436), and conceived it as a descriptive law that was equal to the law of interference formulated by Young. He later even tried to provide a theoretical basis for Newton's theory of fits, assuming that the phenomena of fits were produced by the rotations of particles with two opposite poles (Brewster 1831b, p.79).

Although Brewster accepted the basic emission doctrines, he was not a blind believer of the theory. Instead, one of his early studies on optics was a critique of Newton's theory of inflection. In 1802, Brewster wrote an article "Observation on the inflexion of light," which was never published. In the article, Brewster rejected repulsive forces as the cause of inflection, and argued that, according to his experimental results, inflection did not depend on the density of the diffractive material (Cantor 1984, p.68). A few years later, Brewster restated his criticism in a paper read to the Royal Society of Edinburgh, claiming:

> From the experiments on inflection, it follows that the deviation which the rays experience, in passing by the edges of bodies, is not produced by any force

inherent in the bodies themselves, but that it is a property of the light itself, ...[11]

Thus, Brewster had already departed significantly from the Newtonian version of the emission theory.

Brewster openly expressed his dissatisfaction with Newton's accounts in a series of other optical phenomena. He regarded Newton's explanation of double refraction as "absolutely incompatible with observation" (Brewster 1821, p.129). He attacked Newton's theory of the solar spectrum, claiming that his experiments contradicted Newton's supposition (Brewster 1823, p.442). He also stated that Newton's theory of the colors of natural bodies was "no longer admissible as a general truth" (1831a, p.72), and proposed a new theory to replace Newton's. Thus, at the very beginning of his optical studies, Brewster had already realized the defects of the emission theory, and attempted to correct these problems in his own way.

3.3 Brewster on the Undulatory Theory

Brewster was one of the few people in Britain who paid attention to the undulatory theory in the early 1820s. As early as 1820, he published an anonymous paper, introducing Fresnel's discoveries on the inflection of light (Brewster 1820). Brewster was also the first British "man of science" who recognized the value and significance of Young's principle of interference.[12] After an extended correspondence with Young, Brewster was convinced that the principle was confirmed by experiments, and began to accept it as a descriptive law (Cantor 1984, p.70). In his 1822 essay,

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[11] An abstract of this paper was published in 1816 in *The Quarterly Journal of Literature, Science and the Arts* 2 (1816):207. See Morse (1972), p.82.

[12] In the early and mid nineteenth century, the label "men of science" was used by the contemporaries to describe those who studied or "cultivated" science, including both professionals and amateurs. The label "scientists", first appeared in the 1830s, referred only to those who had full-time devotion to scientific work. See Heyck (1982, p.24, 62).

Brewster favorably outlined Young's principle of interference. After examining the applications of the interference principle in a number cases, he wrote:

> [I]t is satisfactory to state, that the general law of interference which he [Young] has discovered, has found an extensive application in a variety of phenomena discovered since the time of its publication; that he most happily applied it to explain the colours of crystallized plates. (1822, p.613)

Brewster was particularly impressed by the ability of the interference principle in explaining the phenomena of inflection. In his treatise on optics, Brewster directed his audience's attention to a series of experimental results, including the fact that diffractive fringes were independent of the density of the diffracting material but dependent of the distance between the source and the diffractive body, and that the locus of each fringe, in respect to distance from the diffractive body, was hypobulic rather than straight line (Brewster 1831a, pp.96-7). All of these facts contradicted the Newtonian account but comforted with the interference principle. In this way, Brewster clearly expressed his preference for the interference principle in dealing with the phenomena of inflection.

In his treatise on optics published in 1831, Brewster also compared the explanatory ability of the emission theory and undulatory theory in handling other optical phenomena. He concluded:

> Each of these two theories of light is beset with difficulties peculiar to itself; but the theory of undulations has made great progress in modern times, and derives such powerful support from an extensive class of phenomena, that it has been received by many of our most distinguished philosophers. (*Ibid.*, p.135)

The superiority of the undulatory theory's explanatory power was evident, but it did not mean that it should replace the emission theory. One of the major obstacles for the acceptance of the undulatory theory was a set of problems associated with the chemical properties of light. When Brewster began his optical research, he firmly believed that the alliance of chemistry with optics was essential because the forces of affinity were likely to be responsible for the refraction, inflection and polarization of incident light

(Brewster 1815, pp.285-302). Brewster's later studies further confirmed his opinion that the undulatory theory failed to explain the phenomena associated with the chemical properties of light. He argued that the rays of solar light possess several remarkable physical properties: they promote chemical combinations, they affect chemical decompositions, they alter the colors of bodies, and they are necessary to the development of plants and flowers. "It is impossible to admit for a moment that these varied effects are produced by a mere mechanical action, or that they arise from the agitation of the particles of bodies by the vibrations of the ether which is considered to be the cause of light" (Brewster 1831b, p.90).

Brewster attributed the successes of the undulatory theory primarily to the application of the interference principle, which he conceived as a descriptive law in principle compatible with both the undulatory theory and the emission theory. Although the interference principle apparently was a natural conclusion of the undulatory theory, Brewster hoped that he could also incorporate the interference principle into the emission framework and improve the explanatory power of the emission theory.

Brewster's first attempt can be found in his 1822 essay, in which he proposed a mechanism for reconciling the emission theory with the interference principle (Brewster 1822, p.685). It was a psychological interpretation of the interference principle, locating the vibration effect in the retina rather than in light. If the particles of light excited sensation by means of the vibrations of the fibres of the retina and of the nerves, then such vibrations could be annihilated when the particles arrived at the retina at half-interval durations, thus setting up mutually destructive vibrations in the fibres. And such vibrations could be enhanced when the particles arrive at complete-interval durations.

Brewster continued to apply the interference principle neutrally in his later works. In his 1831 treatise on optics, he specifically tried to reconcile the interference principle with Newton's theory of fits. Brewster noted that the phenomena of interference were dependent upon a quantity of distance d: If the path difference of two intersecting rays is d, $2d$, $3d$, etc., they mutually reinforce, while, if the path difference is $\frac{1}{2}d$, $1\frac{1}{2}d$, $2\frac{1}{2}d$, etc., they destroy each other. But Brewster argued that the physical meaning of this crucial quantity could be interpreted in two equally sound ways. It could be interpreted as the breadth of an undulation or wave of light, as the undulatory theory did. And it could also be interpreted as "double the interval of the fits of easy reflexion and transmission" (Brewster 1831, p.134). Thus, the principle of interference was also compatible with the emission theory.

Because Brewster was confident that the interference principle could be employed equally by both theories, he decided not to abandon the emission theory. Instead, he continued to devote his time and energy to improving the explanatory power of this theory. In 1831 he proposed a new emission account of inflection:

> That the particles of light, like those of heat, are endowed with a repulsive force which prevents then from accumulating when in a state of condensation, or when they are detained by the absorptive action of opaque bodies, will be readily admitted. By this power a beam of light radiating from a luminous point has in every azimuth the same degree of intensity at the same distance from its center of divergence: but if we intercept a portion of such a beam by an opaque body, the repulsive force of the light which formerly occupied its shadow is withdrawn, and consequently the rays which pass near the body will be repelled into the shadow, and will form, by their interference with those similarly repelled on the other side, the interior fringes, . . . The rays which pass at a greater distance will in the like manner be bent toward the body, . . . and, interfering with those rays which retain their primitive direction, . . . they will form the exterior fringes. (Brewster 1831b, pp.105-6)

This new theory of inflection, Brewster claimed, could immediately eliminate the problems met by the traditional emission theory. Since inflection in this new account arises from a property of light, it will be independent of the density of the diffractive body. And since the greater proximity of the rays produces a greater repulsive force when the diffractive body is near the source, the diffractive fringes should be affected by the distance with the source. By developing a new emission account of inflection

Brewster had accomplished a crucial improvement of the emission theory through eliminating one of its major obstacles. Although he did not explore this theory of inflection further, probably due to his lack of mathematical ability, Brewster continued to proclaim its correctness at least up to the late 1840s.[13]

Conclusion: After examining Herschel's and Brewster's attitudes toward the two rival theories of light, we may be surprised to find that there are many points of agreement between them, although one was the pioneer of the undulatory theory and another was the stubborn supporter of the emission theory. We have found that both Herschel and Brewster accepted the principle of interference and admired the superior explanatory power of the undulatory theory. There was no significant discrepancy between their opinions on the status of the undulatory theory. We have also found that they did not have any great disagreement in their judgment of the emission theory. Because Herschel defined hypotheses as a kind of tool rather than a representation of the real world, he did not demand an immediate rejection of the emission theory simply due to its inferior status in explanation. Instead, he allowed it to remain alive. Since Brewster believed that the principle of interference could be incorporated into the emission theory, he did not give up his endeavor to revise and develop the theory. Both Herschel and Brewster tended to agree that there was no need to give up the emission theory immediately.

It is evident that there was no severe confrontation between undulationists and emissionists in the period when the undulatory theory was introduced into Britain. Although these two theories implied many logical contradictions, the major actors in the

[13] Brewster to Brougham, (August 29, 1848), Library of University College, Brougham Collection, 26,634.

field did not feel the need to make a clear-cut theory choice by accepting one, rejecting another, and initiating an argument with their rival. The Common Sense methodology shared by Herschel and Brewster, though in different degrees, provided a ground for the temporary agreement between the actors on the status of the two rival theories of light.

The consensus between Herschel's and Brewster's judgments on the status of the two rival theories of light raises a very interesting historiographical or philosophical question. Historians of optics traditionally assumed that conflict between emissionists and undulationists was inevitable, because the theories they accepted were logically contradictory. They consequently conceptualized the history of optics as a sequence of confrontations between the emission theory and the undulatory theory. Recent philosophical accounts of science, which emphasize the crucial role of competition between rival theories in the development of science, further reinforce this historiography. But the tolerance shown by Herschel and Brewster suggests that emissionists and undulationists were not always in conflict. In fact, confrontations in science cannot be conceptualized merely as collisions of different abstract ideas. Rather, scientific controversies are embodied in scientists' practice, produced and controlled by the relevant actors. It is people, rather than ideas, who decide when and how to initiate or avoid a confrontation. Hence, we cannot tell whether there was a confrontation through merely analyzing the logical or cognitive relationships between two theories. It is evident that the logical or cognitive relationships between the emission theory and the undulatory theory are not the whole story of the history of optics.

Chapter 3

The Emissionists' First Reaction

The publication of Herschel's "Light" spread the influence of the undulatory theory in Britain. Within a few years after Herschel's "Light" appeared, a group of British "men of science", most of them Cambridge-trained physicists, adopted the undulatory theory. Beginning in 1830, these newly committed undulationists started to conduct their own researches under the guidance of the theory. Among their eventual publications, George Airy's book was most important and influential. As Lucasian Professor at Cambridge, Airy had published a mathematical tract on astronomy as a textbook for Cambridge students in 1826 (Airy 1826). In 1830, after he repeated most of the experiments described by Fresnel, Airy decided to write a new edition of his book, including a new section on the undulatory theory of light.[1] He finished his revision in 1831, adding 150 pages on optics, and published it with a new title, *Mathematical Tracts on the Lunar and Planetary Theories, the Figure of the Earth, Precession and Nutation, the Calculus of Variations, and the Undulatory Theory of Optics* (Airy 1831). Unlike Herschel, Airy in his *Tracts* exhibited a high degree of confidence in the undulatory theory. He devoted no time to the emission theory. Instead, he presented the undulatory theory to his reader "as having the same claims to his attention as the theory of gravitation: namely that it is certainly true" (Airy 1831, pp.iii-iv). Around the time Airy published his *Tracts*, other undulationists produced a number of articles, applying the undulatory doctrines to such fields as reflection and refraction (Challis 1832), dispersion (Challis 1830), interference (Powell 1832), polarization (MacCullagh 1831a), and double

[1] Airy to Herschel, (July 20, 1830), Royal Society Library, Herschel Collection, HS1.43.

refraction (MacCullagh 1831b).

The rapidly increasing number of publications claiming the victory of the undulatory theory stimulated strong reactions from emissionists. The supporters of the emission theory initiated a series of debates, attempting to stop the spread of the undulatory theory. In these debates, emissionists mainly presented a series of observational or experimental findings that the undulatory theory purportedly could not explain. Sometimes, they also tried to expose internal inconsistencies in the undulatory theory. The challenges from emissionists forced undulationists to improve their theory, both expanding its explanatory power and eliminating its internal or conceptual difficulties. On some occasions they also attempted to deny the reliability and validity of emissionists' empirical findings. The debates between emissionists and undulationists in this period thus centered on both the evaluation of the undulatory theory as well as the appraisal of a series of observational and experimental results.

In this chapter I document three debates between emissionists and undulationists in 1833 -- between Barton and Powell on inflection, between Potter and Airy on prismatic interference, and between Brewster and Herschel on absorption. The debates on inflection and on prismatic interference were about experiment appraisal. These two cases are peculiar because they were indecisive: historical actors could not even agree on what actually happened in the relevant optical experiments. The debate on absorption, on the other hand, was about theory appraisal, focusing on the conceptual coherence of the undulatory theory. When I present these debates, I pay special attention to the practices of theory appraisal and experiment appraisal. I uncover and discuss a series of contingent and contextual factors that deeply affected the results of the appraisal practices in these debates. These contingent and contextual factors show that appraisal practice cannot be understood in terms of the purely cognitive models provided by existing philosophical accounts of science.

1. The Debate on the Inflection of Light

The debate on inflection between John Barton and Baden Powell occurred in 1833. At the beginning, this debate was about whether the undulatory theory was able to explain several experiments on inflection. But later the focus of the debate became what really happened in an experiment. Because of historical contingencies, this debate finally was undecided.

In this section, I first introduce Barton's theory of inflection, in which we can see how he developed his theoretical viewpoint on the nature of light. Then I describe the disputes between Barton and Powell about the undulatory account of inflection and about two basic inflection experiments.

1.1 Barton's Theory of Inflection

We know very little about the biography and personal life of John Barton. According to Grodzinski (1947-49, p.79), Sir John Barton (1771-1834) was Deputy Comptroller of the Royal Mint. In 1806 Barton invented a measuring instrument that, with a differential screw, could measure to 0.00001 inch. In 1822 Barton obtained a patent for another invention: his delicately engraved buttons. These buttons, later called Barton's buttons, were ruled with a large number of fine lines and could produce beautiful interference patterns.

Although Barton engraved his buttons merely for an ornamental purpose, the interference patterns produced by the buttons sparked his interest in optics, especially in the field of interference and diffraction. In 1831 he read a paper, "On the Inflexion of Light," before the Royal Society, in which he wished to "carry on the investigation of the phenomena of the inflexion of light from the point at which it was left by Newton"

(Barton 1831, p.72).[2] The paper included two parts. The first was a critique of Newton's theory of inflection, in which Barton listed several observations inconsistent with the Newtonian account. In the second part, he presented a new theory of inflection, which explained the phenomena of diffraction in terms of a power of mutual repulsion within light particles. Barton's critiques of Newton and the early version of his theory of inflection were very similar to those Brewster presented in his *Life of Newton* (1831b). Barton probably encountered and eventually accepted the emission theory through reading Brewster's works.

Barton later developed his theory of inflection in some detail, and distinguished it significantly from Brewster's version. He supposed "light to consist of material particles, endued with a mutually repulsive force, and so constituting an elastic fluid of great tenuity" (1833b, p.177). When such a fluid was passing through an aperture, it would be pressed against the opposing surfaces, and be thrown into a series of undulatory movements, which finally produced the diffractive fringes. Here, Barton clearly attempted to reconcile the doctrines of undulation with the emission theory.

Barton claimed that his theory of inflection could overcome a fundamental problem involved in Fresnel's theory. He agreed that Fresnel's theory of diffraction was apparently able to account for the phenomena of inflection, by assuming the formation of a secondary wavefront. But Fresnel's theory could not explain how the secondary wavefront was generated, and particularly, why every part in the secondary wavefront was in the same status. Barton proposed that these questions could be answered by his theory of inflection. In his theory, these undulations were generated by a single cause: the interactions between the aperture and the fluid. Barton thus claimed that his theory of inflection was superior to Fresnel's although it was not fully established. It seems that

[2] The paper was not published. Only an abstract of it appeared in *Abstract of the Papers in Philosophical Transactions* 3 (1831):72-3.

Barton had a plan to complete his theory of inflection, but was prevented by his death in 1834.

In his 1831 paper, Barton did not criticize the undulatory account of diffraction. Later by repeating several experiments that Newton and Biot had conducted, he found some evidence that he believed was decisive against the undulatory theory. Barton published his discoveries in 1833 (Barton 1833a, 1833b). The most interesting arguments he presented in these papers were about two experiments, one in which diffractive fringes were produced by a single slit between two straight edges, and the other in which a single slit between two convex edges was used.

1.2 The Diffraction Experiments with Straight Edges

Barton noted that Fresnel's theory of diffraction agreed pretty well with the results of his own experiments. But there were great discrepancies between Fresnel's theory and the experiments conducted by such people as Newton and Biot, who had the reputation of being accurate experimenters. Newton's work mentioned here by Barton was a diffraction experiment. In this experiment a beam of light fell on a single slit between two straight knives, at the distance of 8 feet 5 inches from the light source, and produced diffractive fringes on a screen behind the slit. Varying the width of the slit, Newton measured the distance of the screen from the slit at which the center of the screen first became dark (Newton 1952, p.332). Using Newton's measurements, which included the distance between the source and the slit, the distance between the slit and the screen, and the width of the slit, Barton calculated the values of the wavelengths by the formula provided by Fresnel. The results indicated that the wavelengths derived from Newton's experimental parameters, for all the different widths of the slit, were far smaller than those assigned by Fresnel. The longest value of wavelength derived from Newton's experiment was no more than 0.00002075 inch, whereas, according to Fresnel, the wavelength of red light is 0.00002512 inch, 20% larger than Barton's result. Because the discrepancies between the values derived from experiment and by theory were so great, Barton claimed that they could not be reduced to errors of observation, and were bound to "destroy all confidence in the [undulatory] theory" (Barton 1833a, p.266).

Biot's experiment was almost identical to Newton's, except that he used red light rather than sunlight and he did not mention the distance between the source and the slit. Using Biot's measurements and the wavelength of red light given by Fresnel, Barton used Fresnel's formula to calculate the distances between the source and the slit for various widths of the slit. He found that in some cases the calculated values of the distance between the source and the slit could be negative quantities, namely, without any physical meaning. These results clearly indicated, Barton believed, the inconsistency between Fresnel's theory of diffraction and Biot's experiment.

Barton's critiques of Fresnel stimulated an immediate response from undulationists. The reaction came from Baden Powell, Savilian Professor of geometry at Oxford. Powell was one of the most active advocates of the undulatory theory in the early 1830s. With the help of Airy, he published a reply to Barton only two months after Barton's paper appeared.[3]

Powell's first counter-attack was to challenge the accuracy of Newton's and Biot's experiments. Powell admitted that Newton's observations on diffraction were indeed remarkably accurate. However, armed with superior instruments, modern researchers should have produced more precise and more reliable experimental determinations than their predecessors did. Consequently, Newton's experimental results done at an earlier date could not be compared with new undulatory theory developed in the present. Clearly, "all that Mr. Barton alleges is grounded on a comparison of the theory with

[3] When Powell prepared his reply, he asked Airy's help. Airy repeated several relevant experiments, and wrote Powell a letter telling him the results. Airy's letter was published in *Philosophical Magazine* 2 (1833):433-4 as a postscript of Powell's paper.

older, and therefore probably far less accurate experiments" (Powell 1833a, p.426). In the view of Powell, Fresnel's experiments on diffraction were conducted with "a delicacy and a precision" far exceeding those of Newton's and Biot's work. The agreements between Fresnel's theory and his own experiments on diffraction, which even Barton admitted, were "irresistible" evidence of the truth of Fresnel's theory.

Powell's argument, however, did not move Barton. Barton knew the details of both Newton's and Fresnel's experimental designs. In his reply to Powell's critique, published in the same journal and in the same year, Barton stated that "I cannot think that the accuracy of different series of observations is to be estimated solely by their relative antiquity" (1833b, p.174). Barton noted that the main advantage of Fresnel's experimental method was the way he observed the diffractive fringes. Instead of using a piece of paper as a screen to receive the interference pattern like Newton, Fresnel observed the interference pattern directly through a lens. Fresnel then was able to use a micrometer to measure the fringe widths, considerably improving the accuracy of measurement (Buchwald 1989, pp.125-6). According to Barton, however, Fresnel's method of measuring had no advantage in the related case where the most important parameter was the width of the slit, rather than the widths of fringes. Concerning the measurement of the width of a slit, Barton insisted that Newton's method was superior to Fresnel's. Since Fresnel in his memoir did not give any account of how he determined the width of a slit, Barton believed that Fresnel must have used a method of direct measurement. Unlike Fresnel, Newton used a more accurate method of indirect measurement. He placed the knife-edges facing each other, and had them meeting at a given small angle. He then computed the interval between the edges at any distance from that angle by solving similar triangles. Barton argued that "this method appears susceptible of greater accuracy than any process of direct measurement" (*Ibid.*, p.175).[4]

Powell's second criticism was about Barton's way of using Fresnel's formula. Barton had deduced the following formula for inflection with a single slit from Fresnel's theory:

$$1.875=\frac{c}{2}\sqrt{\frac{2(a+b)}{ab\lambda}}$$

Here a is the distance between the source and the slit, b the distance between the slit and the screen, c the width of the slit, and λ the wave length. Barton calculated the value λ by using the measurements of a, b, and c provided by Newton, and claimed the values of λ derived from Newton's experimental parameters were significantly smaller, about 20%, than those assigned by Fresnel's theory. But Powell claimed that Barton failed to consider the influence of error in his calculation. Powell noted that the measurements of a, b, and c were in the range of inches or hundredth inches, but the value of wave length was in the range of millionths of an inch. Since measuring errors were unavoidable in the values of a, b, and c, these errors, no matter how small they were, would greatly distort the calculated value of wave length. In Barton's paper, however, "no consideration seems to be made of the amount to which a small uncertainty here might influence the almost infinitesimal values which are to be deduced" (Powell 1833a, p.427).

The appropriate way to employ Fresnel's formula, according to Powell, was the other way around, namely, to determine the values of a, b, or c by using wave lengths as known parameters. To demonstrate the differences, Powell calculated the values of

[4] This argument did not convince Powell. He later pointed out that, since what Newton used were *inclined* knife-edges, Newton's experiment could not be used to test Fresnel's theory about *parallel* edges (Powell 1833b, p.415).

c, the width of the slit, using the measurements of a and b provided by Newton and the values of wave length provided by Fresnel. Table 3.1 gives the results of his calculations. These results show that the differences between the calculated values of c and its observed values in Newton's experiment range merely from .001 inch to .011 inch, which, in Powell's own words, "are fairly within the limits of error" (*Ibid.*, p.428). Hence, Fresnel's theory of diffraction also could cover Newton's experiment, should an appropriate method of calculation be employed and the influences of measuring errors be considered.

In Table 3.1, Powell chose only to show the absolute errors between the calculated values and the observed values, which are all very small. Powell however did not tell his audience the relative errors of his calculations, which range from 1.5% to 11%.[5] To make matters worse, the calculated values Powell provided are not accurate. Through re-calculation, it becomes apparent that all the calculated values Powell gave are smaller than they should be, and the actual relative errors range from 8% to 25% (Table 3.2). It is difficult to explain why Powell made such mistakes. But, clearly, accepting these mistaken results would make Powell's argument more convincing. Curiously, none of his contemporaries seemed to notice.

1.3 The Diffraction Experiments with Convex Edges

When Newton observed the diffractive fringes produced by two inclined edges, he found that the narrower part of the slit projected a dark shadow into the screen. He said that "when the distance of the edges was about the four hundredth part of an inch, the stream of light parted in the middle, and let a shadow between the two parts" (Newton 1952, p.327). Barton repeated this experiment, and found that the most

[5] An absolute error is the discrepancy between the observed value Y_{in} and the true value Y_{tr}: Absolute error = e = Y_{in} - Y_{tr}. A relative error is defined as: Relative error = E = e/Y_{in}. See Warshawsky (1990, p.22).

Table 3.1 The Results of Powell's Calculation
[From Powell 1833a, p.428.]

Newton's value of b	Value of c caculated nearly	Newtron's measurement of c	Difference
1.5	.013	.012	.001 inch
3.3	.023	.020	.003
8.6	.036	.034	.002
32.0	.058	.057	.001
96.0	.091	.081	.010
131.0	.098	.087	.011

Table 3.2 A Re-Examination of Powell's Calculation

Newton's value of b	Powell's calculated value of c	Re-calculated value of c (X)	Newton's measurement (X')	Absolute error (X-X')	Relative error (X-X')/X
1.5 inch	.013 inch	.016 inch	.012 inch	.004 inch	25 %
3.3	.023	.024	.020	.004	17
8.6	.036	.037	.034	.003	8
32.0	.058	.065	.057	.008	12
96.0	.091	.093	.081	.012	13
131.0	.098	.099	.087	.012	12

satisfactory way of performing it was to employ two razor-blades, whose edges were slightly convex. He showed the diffractive fringes in a diagram (Figure 3.1), in which the greatest divergence of the dark space and the colored bands corresponded to the point where the razor-blades were closest (Barton 1833a, p.268). This experimental result, however, contradicted Fresnel's theory. Barton demonstrated that Fresnel's theory always predicted an intensity maximum at the center, where a dark space actually appeared in his experiment. This conflict between the experiment and Fresnel's theory of diffraction, Barton believed, was sufficient to overthrow the undulatory theory.

In his counter-attack, however, Powell cast doubt on the reliability of Barton's experiment. Powell replicated Barton's experiment under various conditions, including using edges with different convexities and changing the distance between the edges and the light source. When the curved edges approached each other very closely, he reported the appearance of fringes similar to those in Barton's diagram, but with important differences. Barton's diagram suggested that the central shadow was dark and isolated, with bright fringes on each side of it. Powell's central shadow was not absolutely dark. It appeared to join continuously with the dark shadow on each side, and the bright fringes were bent in an hyperbolic form that did not meet at the sides. When the distance between the edges was increased, a faint light began to appear at the center, and the center became brighter and brighter with the increase of the distance between the edges (Powell 1833a, pp.430-2).

Powell concluded that Fresnel's theory was quite consistent with the results produced by the experiment with convex edges. When the slit was wide enough to allow a sensible portion of light to pass, the relatively bright center agreed "perfectly" with the predictions of Fresnel's theory. If the two convex edges touched, a dark shadow would appear at the center according to the Fresnel's theory, because no significant amount of light could pass. And if the two convex edges were very close but not in contact, the

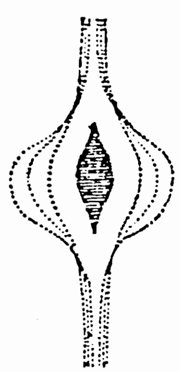

Figure 3.1 The Diffractive Fringes Produced by Two Convex Edges
[From Barton 1833a, p.268]

shadows of the edges would coalesce due to diffractive effects; the undulatory theory predicted that shadows were enlarged beyond their geometrical boundaries. In this case, the central portion of the diffractive fringes would be extremely dull, or almost totally dark, but not absolutely black (*Ibid.*).

In addition to challenging the reliability of Barton's experiment, Powell had also provided a "defensive" argument to resolve the difficulties that Barton created. The basic tactic of this "defensive" argument was to demonstrate that the convex-edges experiment conducted by Barton was irrelevant to Fresnel's theory. Since the experimental results went beyond the theory's domain, they could not and should not be used as a test of the theory. As part of this tactic, Powell first established that the light sources in Newton's and Barton's experiments were holes a quarter of an inch in diameter. Since one of the essential assumptions of the undulatory theory required that the light source should be a single point, Newton's and Barton's experiments with convex edges were not legitimate tests for Fresnel's theory (Powell 1833a, p.430).

In a letter, Airy had reminded Powell to avoid the discussion of curved edges, because it introduced "insurmountable difficulties into the mathematical investigation" (Powell 1833a, p.433). Prompted by Airy, Powell argued that Fresnel's theory of diffraction should only be tested by experiments with rectilinear edges. The experiments with curved edges were not appropriate because they were "immensely complicated". With curved edges, for example, there might be interactions between the lights from different parts of the slit, in a degree dependent on the curvature of the edges. The standard form of Fresnel's formula on diffraction could not and should not be applied to the case of curved edges (*Ibid.*, p.431; Powell 1833b, p.413). Hence, no matter what Barton found in his convex edges experiments, he could not use his results to challenge Fresnel's theory.

In his reply to Powell's critiques, Barton first pointed out that Powell's explanation of the dark center in terms of the enlargement of the shadows was invalid. In modern terms, that is, Powell had appealed to an *ad hoc* assumption after he had decided that the phenomenon could not be explained by Fresnel's theory. Barton insisted that "it cannot be allowable to employ this enlargement as an after correction to the result of calculation" (1833b, p.173).

Barton also endeavored to demonstrate the reliability of his experimental findings. He insisted that Powell's observations were essentially similar to his. The existence of hyperbolic lines in Powell's experiments implied that there was a dark portion between them, giving a result just as Barton had described. Moreover, "those [convex edges] used by him [Powell] in repeating my experiment had, if I do not mistake, so inconsiderable a radius of curvature as to reduce the dimensions of the dark space within a limit too small to be conveniently observed" (*Ibid.*, p.172).

Up to this point, the key question in the dispute between Barton and Powell on the diffraction experiment with convex edges became around the experimental results *per se*: whether there really was a dark space in the center of the diffractive fringes. To settle this dispute, replicating the experiment and verifying the experimental result in question were necessary. The replicating and verifying process could only be complete when both sides provided details of their experimental settings. When such data as the convexity of the edges and the distance between the light source and the edges in each experiment became available, Barton or Powell or any others could replicate the experiment and verify its results. Unfortunately, Barton's death in 1834 ended the dispute without settling it.

2. The Debate on Prismatic Interference

The debate on prismatic interference in 1833 involved Richard Potter, William Hamilton, and George Airy. Similar to the debate on inflection, the debate on prismatic interference at the beginning focused on the tests of the undulatory prediction about the velocity of light, but finally centered on experimental facts *per se*. This debate was also an indecisive one, where contextual factors played a very important role.

In this section I first describe Potter's early works on photometry. These works not only deeply shaped Potter's attitude toward the undulatory theory, but also determined his research approach in the next few decades. Then I portray the disputes among Potter, Hamilton and Airy on prismatic interference. Finally I discuss why actors in this debate could not agree on a simple experimental fact and how social and historical factors affected the results of this debate.

2.1 Potter's Early Works on Photometry

Richard Potter (1799-1886) was a son of a brewer at Manchester. After he graduated from grammar school, Potter worked as a merchant for many years. At the same time, he devoted his leisure time to scientific research, particularity the study of optics and chemistry. In these subjects, he had John Dalton as his tutor.[6]

Potter's earliest optical research concerned instruments. He had spent many years in grinding and polishing lenses and mirrors, and eventually attained some proficiency in this difficult art. In early 1829, because Potter was confident in the quality of his polished metallic mirrors, he decided to make a comparison of his mirrors with those made by other well-known "men of science". He selected the mirrors made by William Herschel, who in 1800 had given measurements of the quantities of light reflected by the plane mirrors he used in his telescope.

The main measure of a mirror's quality was its ability to reflect light. In order to measure the reflective power of his mirrors, Potter made a photometer, following the

[6] *Dictionary of National Biography*, Vol.16, p.219.

design of Rumford with some modifications (Figure 3.2).[7] The photometer consisted of an upright screen *ab*, and a lateral piece *bc*. In the middle breadth of the lateral piece an upright pasteboard was fixed. Two lamps were on the lateral piece, one on each side of the pasteboard, which separated the light from each side. There was an aperture in the screen, covered over with thin paper. When the lamps were lighted, each half of this paper was illuminated by its own lamp. These lamps were placed on slides that could be drawn nearer to, or pushed farther from the screen. Figure 3.3 shows the details of one of the slides, in which a mirror is fixed to an arm turning around the pivot *f* as a center, and two upright pieces *ab*, *bc* are used to separate the direct rays and the reflected rays (Potter 1830, pp.284-5).

To measure the reflective power of a mirror, one of the lamps in the photometer must remain stationary for the purpose of comparison. The other lamp, the one with the mirror, is moved until the reflected light from the mirror gives the same illumination on the thin paper in the screen as the comparison lamp does. The quantity of light reflected by the mirror can be found by comparing the square of the distances of the two lamps.

Potter first used this photometer to measure the intensity of reflected light from a metallic mirror, composed of 14.5 parts of tin to 32 of copper. He found results that were not consistent with the received view on the subject. Newton had reported that metallic mirrors, in common with all other substances, reflected light "most copiously, when incident most obliquely". Newton's opinion was widely adopted in the field of optics. But Potter found more light to be reflected by the mirror when the incidence was perpendicular than that when the incidence was oblique. Repeating his measurements with different metallic mirrors, Potter obtained the same results, namely, that the amount of reflected light decreased as the angle of incidence increased. After conducting several

[7] Count Rumford (Benjamin Thompson) described his photometer in a paper published in 1794. For a photo picture of Rumford's photometer, see Hawkes (1981, pp.124-5).

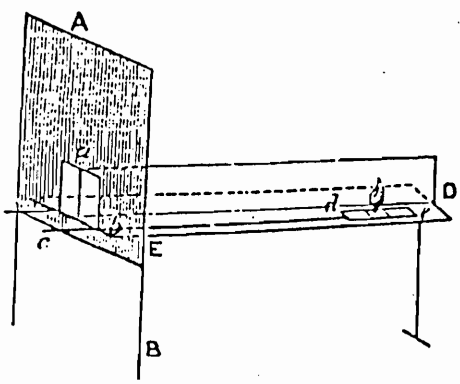

Figure 3.2 Potter's Photometer
[From Potter 1856, p.111]

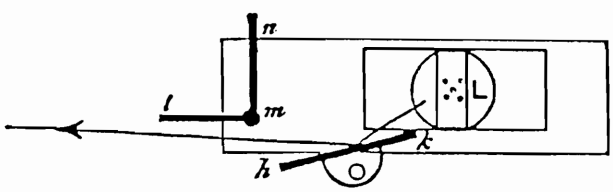

Figure 3.3 Details of Potter's Photometer
[From Potter 1856, p.112]

experiments, Potter was finally convinced that his measurements of the reflective power of metallic mirrors were just the contrary to the general opinion adopted by many great natural philosophers, including Newton.

Potter did not retreat because of the conflict with authority. Even when just beginning his optical research, he had a critical attitude toward the Newtonian theory of light. He said that "at present we know so little respecting the forces concerned in producing the phenomena of reflection, that every theory must be mere hypothesis" (*Ibid.*, p.284).

Immediately after he published his experimental results about metallic mirrors, Potter conducted further experiments to measure the reflective power of transparent bodies. Using the same photometer, Potter measured the amount of light reflected from the surfaces of crown glass, plate glass, and flint glass with different incident angles. This time his findings were consistent with the received opinion, namely, that the amount of reflected light increased as the angle of incidence increased. From his experimental findings, Potter generated the following empirical law on the quantity of light reflected by transparent materials:

$$y=a+\frac{c}{r+b-x}$$

Here y is the quantity of the reflected light, x is the sine of incidence, r is the quantity of the incident light, and a, b, and c are constants, having different values for different transparent materials (Potter 1831, p.63).

Using this empirical law, Potter later discovered a way to measure the relative intensities of the dark and bright rings produced by thin plates. The phenomenon of the colored pattern of thin plates, also called Newton's rings, drew Potter's attention in 1832. After observing the great differences in intensity between the dark and the bright rings in the interference patterns produced by thin plates, Potter concluded that this was a phenomenon that neither current theory of light could explain. In order to prove this point, Potter designed a new photometer to measure the relative intensity of light reflected by transparent materials.

This photometer (Figure 3.4) consisted of a flat board. On the board a rectangular piece of pasteboard was fixed along a semi-cycle. At the center of the circular arc there was a pin, as at *a*, upon which turned the two perpendicular arms *ab* and *ac*. Attached to each of these arms was a piece of crown glass, ground flat and polished. In order to obtain a uniform illumination in the semicircular pasteboard, the measurement should be conducted in day time when it was either misty or cloudy. By using a tube through which to view the glasses at *d*, these glasses would reflect images of some portions of the surface of the pasteboard. Since the pasteboard was equally illuminated, the brightness of the reflection in the glasses would depend only on their inclinations to the visual rays, and could be adjusted by rotating the arms. Knowing the angles of incidence upon the glasses, the intensities of the reflected lights could be calculated by Potter's empirical law (Potter 1832, pp.175-6).

To determine the relative intensities of the dark and bright rings in thin plates, Potter covered the glasses with blackened paper, and left only a narrow stripe in each glass. He attached a thin plate that produced Newton's rings close to the photometer, so that he could view the interference pattern alternately without any delay. He also colored the pasteboard in the photometer to the same tint as the homogeneous light used to produce the Newton's rings. Then he moved the arms carrying the glasses until the relative intensities of the two reflected beams of light from the glasses were sensibly the same as the relative intensities between the dark and the bright rings in the Newton's rings. Finally he measured the incidence angles and used his empirical law to calculate the relative intensities.

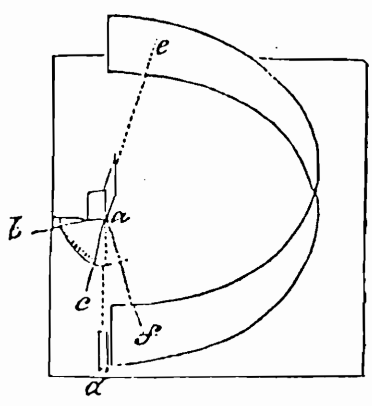

Figure 3.4 Potter's Comparative Photometer
[From Potter 1832, p.175]

Following this procedure, Potter was able to determine the relative intensities of the dark and the bright rings in the interference pattern produced by thin plates. He conducted his measurements under different conditions, first using homogeneous green light and then homogeneous red light. He found that the ratios of the relative intensities of the dark rings and the bright rings ranged from 2.36 to 2.48 when homogeneous green light was used, and ranged from 3.21 to 3.81 when red light was used. These results, Potter noted, were significantly larger than the predictions made by the undulatory theory.

According to Potter, the colored patterns produced by thin plates were among the most important phenomena in the field of optics. If an optical theory really represented "the true law" in the field, it ought to be able to explain this group of phenomena. However, the Newtonian theory of light, or the doctrine of fits of easy reflection and transmission, could not provide any satisfactory account for this phenomenon. Thus this theory did not represent "the true law" in the field. Unlike the Newtonian theory, the undulatory theory was able to give a quantitative account of the phenomenon. But, Potter pointed out, its predictions were not accurate. According to Herschel's calculation based upon a formula developed from Fresnel's theory, the ratio of the relative intensities of the dark and the bright rings in Newton's rings should be 1 to 1.1538 (Herschel 1827, pp.469-73). Herschel's theoretical calculation was significantly smaller than Potter's measurements. Because of this discrepancy, Potter concluded that the undulatory theory also must be wrong. He claimed:

> The great difference in intensity between the dark and the bright rings which we here find, is certainly not to be accounted for on any principles of interference yet proposed; and it furnishes a very strong argument against the undulatory theory, in which the effects of interference are supposed to be perfectly determinate when we know the circumstances of the interfering pencils (*Ibid.*, p.178).

It is evident that Potter in his early optics research held a critical attitude toward both the emission theory and the undulatory theory of light. Without fully committing himself to either theoretical tradition in the field of optics, Potter attempted to criticize every theoretical account when it conflicted with his experimental discoveries. He continued this attitude when he examined the phenomenon of interference.

2.2 Potter on the Problems of Prismatic Interference

In 1832, Powell published an article in the *Philosophical Magazine* on several experiments about diffraction and interference (Powell 1832). One of the experiments Powell described in detail was originally proposed by Fresnel. This was the experiment using two plane glasses inclined at a very large angle to demonstrate the phenomenon of interference by reflection. Powell repeated this experiment with some modifications. In addition to having two plane glasses inclined at a very large angle as Fresnel did, Powell placed a prism of glass in front of the glasses, in the position where the two reflected rays were supposed to intercept (Figure 3.5). Using sunlight as the light source, he found that, after being refracted by the prism, the two reflected rays continued to interfere. He also found that the pattern and the position of the interference fringes did not change after interception by the prism. Powell believed that the results of this prismatic interference were entirely consistent with the undulatory theory.

After reading Powell's article, Potter replicated the prismatic interference experiment. By using a homogeneous light produced by a colored solution as the light source, Potter observed that some portions of the reflected rays, which would have interfered without the prism, did not interfere after refraction. On the other hand, he found that interference took place between other portions of the reflected rays. As shown in Figure 3.5, Powell reported that the interference fringes produced after refraction were unchanged, and the central band of the interference fringes was still on the line $m'n'$. However, Potter reported that different portions of the reflected rays were involved in the interference, and that the central band of the interference fringes was on a new line

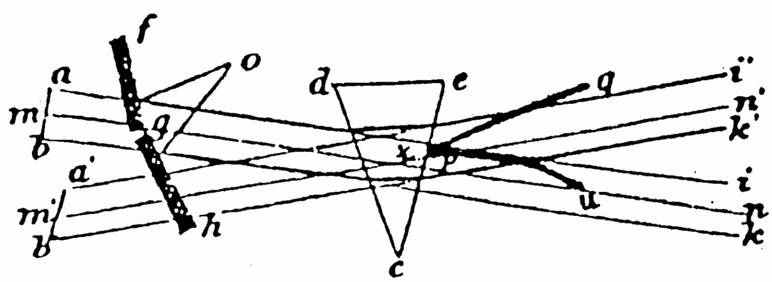

Figure 3.5 Powell's Experiment on Prismatic Interference
[From Potter 1833a, p.488]

pq (Potter 1833a, p.82).

Potter also found that this experiment on prismatic interference could be used to determine the velocity of light in refractive media. The positions of the interference fringes in this experiment were determined by the path differences of the intersecting rays. These path differences were affected by the prism because rays of light changed their velocities in refractive media. Hence, the velocity of light in the prism could be calculated based upon the position of the central band of the interference fringes. Since the two rival theories of light had different predictions on the velocity of light in refractive media, a test of these theories could be made using the calculated results.

The emission theory assumed that light moved with an increased velocity when passing through refractive media, in the direct ratio of their refractive indices. Following this assumption, Potter found, the central band of the interference fringes in his experiment ought to be seen along the line *tu* in Figure 3.5, which was "far from the truth" shown by the experiment. On the other hand, the undulatory theory assumed that light traveled with a decreased velocity in refractive media, in the inverse ratio of their refractive indices. According to this assumption, Potter demonstrated, the central band of the interference fringes in this experiment should coincide with the intermediate line *m'n'*, which was still "far from the truth" although better than the emission theory's prediction. Therefore, neither the emission theory nor the undulatory theory of light gave a correct determination of the velocity of light in refractive media.

According to the experimental results, Potter believed that the velocities of light in different refractive media were actually smaller than the values proposed by the undulatory theory. These experimental results, Potter claimed, constituted a fatal objection to the undulatory theory. At the end of his paper, Potter concluded:

> We see that the experiment with the prism draws a clear line of distinction; but from what I have observed, I believe the velocity will not eventually be found extensively different from that according to the said theory [the undulatory

theory]. The slight difference is, however, of fatal consequence; for the ratio ought, according to common consent, to depend rigorously on the refractive index, which is one of the fundamental principles of the theory. (*Ibid.*, p.94)

2.3 Airy's and Hamilton's Reactions

Potter's attack stimulated strong reactions from the undulatory camp. Immediately after Potter's paper appeared, William Hamilton published three papers in the *Philosophical Magazine*, attempting to explain the prismatic experiment in terms of the undulatory theory. William Hamilton was the Irish Astronomer Royal at Dunsink Observatory and Andrew professor of astronomy at the University of Dublin. He was one of the few non-cambridge trained physicists who firmly adopted the undulatory theory at the beginning of the 1830s. According to Hamilton, Potter's experiments and his analysis on prismatic interference deserved attention, because "if correct, they would furnish a formidable and, perhaps, fatal objection against the undulatory theory of light" (Hamilton 1833a, p.191). In these papers Hamilton claimed that Potter had committed a mistake in his mathematical reasoning. According to Hamilton's analysis, when light passed through a prism, it would create an effect called prismatic aberration, which was greater than the aberration of a lens. Because of this prismatic aberration, the section of an emergent wave would not be circular any more, and, consequently, the paths traveled by the rays in the prism required corrections. After giving a formula for the path correction, Hamilton calculated the locus of the central point of the interference fringes, and found that it would move toward the thick end of the prism as Potter described. He therefore concluded that the phenomenon observed by Potter was explicable by the undulatory theory.

The undulatory account of the prismatic interference given by Hamilton did not satisfy Potter. He published a reply to Hamilton in the same journal and the same year. In his reply Potter carefully calculated the displacement of the center of the interference fringes toward the thick end of the prism, using the formula given by Hamilton. He found that the calculated values of the displacement were too small, only at the range of millionths of an inch. And yet the observed displacements were at least several thousand times the amount of the calculated values (Potter 1833b, p.281). Thus, Potter concluded that Hamilton's account could not solve the problem even if the effect of prismatic aberration was adopted.[8]

Another reaction toward Potter's experiments came from George Airy. Airy also published two remarks on Potter's experiments in the *Philosophical Magazine*, just one month after the appearance of Potter's paper. In these remarks, Airy first cast doubt on one of the most important experimental conditions in Potter's work -- the light source. Airy insisted that Potter must have not used homogeneous light as the source in his experiments. Airy listed two reasons to support his allegation. First, interference by reflection required a light source with very high intensity, but so far all homogeneous sources could only produce very faint light. Second, if homogeneous light had been used in Potter's experiment, it would have produced a series of bright and dark bars, "unlimited in number as far as the mixture of light from the two pencils extends, and undistinguishable in quality". Under this circumstance, no one would be able to determine where the center of the fringes had been (Airy 1833a, p.164,162).

If the light source was not homogeneous but heterogeneous, Airy argued, then the center of the fringes was not necessarily the point where the two rays of light had equal paths. The position of the center of the fringes "is determined by considerations of a perfectly different kind. And this is the radical error into which Mr. Potter has fallen" (*Ibid.*, p.162). If a heterogenous light source was used, each homogeneous ray composing the reflected heterogeneous light would produce its own group of bars. Due

[8] Hamilton later admitted that prismatic aberration might not "energetic enough to account for the whole, or even the greatest part of the observed effect" (Hamilton 1833c, p.371).

to the impact of the prism, the bars produced by each color would have different breadths and move slightly toward the thick end of the prism. When these different groups of bars coincided with each other, according to Airy, they would produce the observed fringes with a center moved far toward the thick end of the prism, just as Potter described. Airy thus concluded that "it appears then that, according to the theory of undulations, we ought to have precisely the phenomenon which Mr. Potter had observed, supposing the light heterogenous" (*Ibid.*, p.164).

Potter published a reply in the 1833 *Philosophical Magazine*, in which he complained that Airy's analysis of his experiment completely missed the point. First, Potter provided details about the light source he used in the experiment. It was the red light produced by a solution of iodine in hidrotic acid, which gave a much purer light than red glass. Even according to the standard adopted by undulationists, the light source in his experiments was satisfactorily homogeneous. Potter emphasized that the method he used was considered by Fresnel as sufficiently homogeneous for any "delicate experiment". Second, Potter pointed out that Airy did not represent his experimental results correctly. According to Airy's description, it sounded as if the prism only caused a shift of the center of the fringes, but the position of the whole group of fringes did not change. Potter claimed that this was a misrepresentation of his experiment, in which he observed a movement of the whole group of the fringes as their distance from the prism increased. Thus, Potter insisted that Airy's analysis of how heterogeneous light might cause the shift of the fringes' center could not solve the problem (Potter 1833b, pp.276-7).

The dispute between Potter and Airy finally centered on a very basic question: What actually happened in the experiments? Potter's complaint forced Airy to replicate the experiment. In his replication, Airy used a new observation method to determine the displacements of the fringes. His new idea consisted of an eye-piece with a wire fixed in the focus of the lens, both attached to a slide on a bar. By proper adjustment of the bar's direction, Airy was able to keep the wire steady upon one of the fringes when he looked through the eye-piece, even though the distance between the eye-piece and the prism varied. Using this method, Airy was sure that the whole group of fringes remained stationary and only the center of the fringes shifted when the distance for the prism increased (Airy 1833c, p.451). After reading Airy's report of the replication, Potter immediately pointed out that Airy did not give enough information for others to verify his observation, since the most important parameter, the angle between the bar and the incident pencils was unknown. Potter also indicated that Airy's observation method was not very reliable, because it relied on many parameters such as the angle between the bar and the incident pencils. A simpler and better way to observe the change of the interference pattern was his method, which used the position of a fringe caused by the edge of one of the mirrors as the standard (Potter 1833d, p.333). All these differences implied that Airy's replication could not be accepted. Thus, after several rounds of exchanges, Potter and Airy still could not obtain a consensus on what really happened in the experiment. More specifically, they simply did not agree with each other on whether the position of the whole group of fringes moved when their distance from the prism changed.

The debate on prismatic interference, which produced more than ten publications from Powell, Potter, Hamilton, and Airy between 1832 and 1833, did not yield any clear outcome. On the one hand, the undulationists in the debate were confident that, through their work, the problem had been successfully solved by the undulatory theory. Airy even predicted that, if Potter continued to study this problem, he "will very soon become an undulationist" (Airy 1833b, p.167). On the other hand, Potter found that it became harder and harder for him to accept the undulatory theory based on the experimental results. In his reply to Airy's prediction about his future acceptance of the undulatory theory, Potter said:

> I believe the probability of my becoming an undulationist becomes daily less and less; as, from the time of my having merely an opinion upon the general theory, from having read Dr. Young's Bakerian Lecture, I am now gradually come to see many serious and weighty objections against it, of which several have the greater influence with me from having arisen in my own experimental inquiries. (1833b, p.277)

These completely opposite judgments of the intellectual status of the debate stemmed from Airy's and Potter's different observations of what really happened in the experiments. The discrepancy on experimental results could have been resolved through a process of evidence verification, or experiment appraisal. But in a letter to Hamilton on April 1833, Airy expressed reluctance to continue the debate with Potter or to verify the experimental finding in question.[9] One reason suggested for Airy's retreat was that the debate had become too personal.[10] However, a more plausible reason was that Airy just did not have an interest continuously to interact with Potter. There were great differences between Airy and Potter in terms of social and intellectual status. In the early 1830s Airy had been one of the most successful and prestigious "men of science" in Britain. Potter, on the other hand, was only an unknown amateur who had no formal training in science. All these differences could become a barrier for the evidence verification process, which required further communication between Airy and Potter about the details of their experimental settings.

2.4 The Disagreements on Experimental Outcomes

There were many similarities between the debate on inflection and the one on prismatic interference. The central issue in these two debates was certainly the

[9] Hamilton to Adare, (April 22, 1833), in Graves (1882, Vol.2, p.44).

[10] This was the viewpoint of Hamilton. In the same letter to Adare, Hamilton wrote that "Airy is right, I think, to stop, for it was in danger of becoming too personal a matter".

evaluation of the undulatory theory. The basic tactic of emissionists in these debates was to present all kinds of observational and experimental findings that their rivals supposedly could not explain. In their defenses, undulationists not only endeavored to improve the explanatory power of their own theory but also, if possible, tried to cast doubt on the reliability of their rivals' experimental findings. An affair of theory appraisal then became an affair of experiment appraisal.

The experiment appraisals in these two debates, however, were unsettled. The actors in these debates did not achieve consensus in their experiment appraisals. More importantly, the disagreements on experiment appraisal in these debates did not stem from different interpretations of an experimental result, but from different descriptions, or different "observations", of the experimental results *per se*. In the dispute on the diffraction experiment with convex edges, for instance, neither Barton nor Powell could agree on whether there was a dark space in the center of the diffractive fringes. Also, in the dispute on the prismatic interference experiment, neither Potter nor Airy could agree on whether the whole group of interference fringes had moved in the experiment.

The peculiar inconclusiveness of experiment appraisal in these debates might result from the current style of reporting and representing experiment. In early nineteenth century Britain, there was no standard format for reporting optical experiments. Most experimental reports on optics published in academic journals were relatively simple, usually lacking detailed descriptions of experimental results, experimental procedures, and experimental instruments. This was particularly true for those appearing in the *Philosophical Magazine*. Unlike the *Philosophical Transaction*, the *Philosophical Magazine* provided only a very limited space for publication. Within a length of three to five pages, it was really difficult to portray the relevant experimental results, procedures, and instruments in detail, or to provide the necessary information for experiment replication.

Moreover, optical experimenters in the early nineteenth century lacked adequate techniques to reproduce the optical images that appeared in their experiments in their experimental reports or publications. The available illustrative techniques in this period were sketches and engraved diagrams. But these could not accurately represent the details of optical images, especially the variation of the intensity of light. In the debate on inflection, for example, Powell had complained about the quality of the diagram Barton used to represent his experimental result (Figure 3.1). According to Powell, Barton's diagram was sketched or engraved poorly, and failed to represent the variation of the intensity of light (Powell 1833a, p.432).

All these limitations made experiment replication extremely difficult, if one only had the information from published experimental reports. To complete the replication process for experiment appraisal, intensive communication, especially informal exchanges, between experimenters was necessary. In terms of their functions in experiment appraisal, there were significant differences between formal communication -- published replies or comments in our cases, and informal communication -- private conversations and correspondence. In the debates discussed above, formal communication might be able to convince those who did not have direct experience of the experiment in question. However, it was not effective to persuade those who had been directly involved in the debates, because the published replies and comments did not supply the detailed information necessary for experiment replication. Only informal communication that aimed at information exchange between relevant experimenters could complete the process of replication. These informal exchanges, however, depended upon a series of contingent and contextual factors. In the debate on inflection, for example, a historical contingency - the death of Barton - made continuous exchanges between the actors impossible. Also, the differences in intellectual and social status between Potter and Airy prevented them from private communication, even though Airy was willing publicly to reply to Potter.

All these points imply an inconsistency with the existing philosophical accounts of science, which regard experimental tests of scientific theories as purely cognitive affairs. In the debates discussed above, a series of contingent and contextual factors on different occasions made the informal communication necessary for experiment replication and prevented participants from reaching agreements in their experiment appraisals. This implies that not every experiment can be used to test theory, although epistemologically every theory should be tested in the light of experimental evidence. Hence, whether and how an experiment can be used to test scientific theories is no longer a purely cognitive question, but also depends upon historical and social conditions.

3. The Debate on Absorption

The major actors involved in the 1833 debate on absorption were David Brewster and John Herschel. Unlike the other two debates that happened in this period that centered on experimental tests of theories, the debate on absorption focused on the other aspect of theory appraisal - the conceptual evaluation of scientific theories. In this section I first describe Brewster's 1832 report on optics, where he raised the question of absorption. Then I document the disputes between Brewster and Herschel, especially paying attention to the conceptual analysis that the actors employed in their theory appraisal.

3.1 Brewster's 1832 Report

The establishment of the British Association for the Advancement of Science in 1831 provided a new platform for the controversy between the emission theory and the undulatory theory of light. Vernon Harcourt was one of the founders of the British Association. When he first sketched out his ideas about the organization, he suggested that it should be able to "look over the map of science and to say 'here is a shore of which the soundings should be more accurately taken, there a line of coast along which a voyage of discovery should be made'".[11] Supported by Herschel and Whewell, Harcourt's suggestion was soon developed into a course of action: commissioned reports. These reports were supposed to be written by well qualified experts and to review the recent conditions and progress of different subjects of science.

At the 1831 British Association meeting, a sub-committee for mathematics and physical science was founded. Brewster, Hamilton, Powell, and Whewell were the members of this committee.[12] One of the responsibilities of this sub-committee was to decide the topics and authors of the proposed reports. At this meeting, the committee requested six reports for the next session: on physical astronomy, tides, meteorology, heat, thermo-electricity, and optics. For the topic of optics, the number of the candidates able to make the report was limited. According to Whewell, only three persons, Airy, Herschel and Brewster, were qualified.[13] Since Airy had been assigned the report on physical astronomy and Herschel did not attend the meeting, Brewster became the only qualified and appropriate candidate. Hence, the committee requested Brewster "to prepare for the next meeting a report on the progress of optical science".[14]

In his "Report on the recent progress of optics", which was presented to the section of mathematics and physical science at the 1832 British Association meeting, Brewster provided a survey of the field of optics. He first listed a series of important

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[11] Harcourt to Whewell, (August 27, 1831), in Morrell & Thackray (1981, p.474).

[12] The committee had total eight members. The other four were Thomas Brisbane, William Pearson, William Scoresby, and R. Willis. See *Report of the British Association* 1 (1831):46. Whewell was not present at the meeting although he was elected as a member of the committee. For his reason, see Whewell to Forbes, (July 14, 1831), in Morrell & Thackray (1984, p.42).

[13] Whewell to Harcourt, (September 1, 1831), in Morrell & Thackray (1984, p.53).

[14] *Report of the British Association* 1 (1831):52.

discoveries in the field during the initial three decades of the century. In the view of Brewster, these important discoveries included Brougham's and Young's works on inflection, Laplace's study on double refraction, Malus's discovery of polarization in reflected light, and of course Fresnel's researches. Regarding recent discoveries that were relatively unknown, Brewster pointed to the discoveries of Airy, which included his works on elliptical polarization and Newton's rings.[15]

After describing the recent progress in the field of optics, Brewster reported a number of unsolved problems. As a response to the increasing claims of the undulatory theory, Brewster reminded his audience that "even the theory of undulations, with all its power and all its beauty, is still burthened with difficulties, and cannot claim our implicit assent" (*Ibid.*, p.318) These difficulties, according to Brewster, included elliptical polarization, "from the rectilinear polarization of transparent bodies, to the almost circular polarization of pure silver," and the relationship between polarization and double refraction (*ibid.*). And yet the most formidable challenge to the undulatory theory, Brewster said, lay in the domain of absorption, a phenomenon newly studied and explored by him in a series of experiments. Brewster thus devoted the last part of his report to this subject.

3.2 Brewster on the Problems of Absorption

Brewster began his studies on the phenomena of absorption as early as 1822. In that year he published a paper describing his experiments on the action of colored media on the solar spectrum (Brewster 1822). Brewster's early studies on the subject of

[15] For details of Airy's work on Newton's rings, see chapter four. Brewster also regarded Cauchy's recent work on dispersion as important, because if Cauchy's work was successful it could remove one of formidable difficulties of the undulatory theory. But Brewster admitted that he himself was unable to give any satisfactory account of Cauchy's work, probably because of the mathematics it involved (Brewster 1832, p.317). Hence he only briefly sketched Cauchy's work on dispersion.

absorption were closely related to the question of the colors of natural bodies. In Brewster's view, these colors were caused by absorption rather than by reflection as Newton suggested (Brewster 1831a, pp.280-6).

In his early experiments on absorption, Brewster observed the actions of different kinds of materials on light. The absorbing materials he used included thin films of metals, colored glasses, rock crystals, and transparent fluids. He found that these materials could selectively absorb the colors in the spectrum. For example, a thick piece of blue glass absorbed parts of the red, yellow and green colors but left the rest of the spectrum untouched. To explain these phenomena Brewster assumed that "the light is actually *stopped* by the particles of the body, and remains within it in the form of imponderable matter" (*Ibid.*, p.138, original emphasis).

Brewster made his most important discovery in absorption when he turned his attention to the absorptive spectrum of "nitrous acid gas" (nitrogen dioxide) in February or March of 1832.[16] The experimental findings were very surprising. Directing the light of a lamp through a small thickness of the "nitrous acid gas", Brewster found hundreds of dark lines and bands in the absorptive spectrum, sharp at the violet end but faint at the red end. When the thickness of the gas was increased, the lines became more and more distinct in the yellow and red region of the spectrum. When the temperature of the gas was raised, distinct lines even appeared at the red end of the spectrum. Finally, Brewster was able to use the gas to produce more than a thousand dark lines in the spectrum of ordinary flames (Brewster 1834, pp.521-2).

Brewster immediately realized the theoretical implications of these experimental

[16] See Talbot to Herschel (March 27, 1833), in which he remarked that it was "a twelvemonth" since Brewster had told him about the experiments and the discoveries. In his 1832 report Brewster did not provide details of this experiment. He gave more information about it later in a paper read to the Royal Society of Edinburgh in April 1833. The paper was published in the *Transactions of the Royal Society of Edinburgh* in 1834.

results. He pointed out that his experiment had very strong bearings on the rival theories of light, because these two theories did not have equal abilities to explain this phenomenon. In Brewster's view, the Newtonian emission theory could easily produce an explanation for the phenomenon he discovered. According to this theory, when a beam of light was transmitted through a certain thickness of a particular gas, some portions of the beam would be stopped by a special action of the material atoms in the gas. "Such a special affinity between definite atoms and definite rays, though we do not understand its nature, is yet perfectly conceivable" (Brewster 1832, p.321). Brewster even suggested that the action of the atoms in the gas could be easily understood by assuming that the particles of light were identical with the molecules of the gas. These similar particles would unite when they were brought within the spheres of their mutual attraction.

In the language of the undulatory theory, however, it was difficult to account for the same phenomenon. The experiment showed that more than a thousand waves of light with different wavelengths were incapable of propagating through the ether of a transparent gas. But at the same time, all other waves with intermediate wavelengths were freely transmitted through the same medium. For example, waves of red light with wavelength of 250-millionths and 252-millionths of an inch were able to pass freely through the gas, but another red light with wavelength of 251-millionths of an inch was entirely stopped.

Although these absorption phenomena were peculiar, they did not constitute empirical evidence against the undulatory theory -- there was no undulatory account of absorption yet. For Brewster, the main obstacle to an undulatory account of the phenomena of absorption was that "there is no fact analogous to this in the phenomena of sound" (*Ibid.*). Brewster indicated that, according to the undulatory theory, there should not be fundamental difference between the phenomena of light and those of sound

- both light and sound consisted in the undulations of an elastic medium. Hence, absorption would also appear in the field of sound, if the undulatory theory were right. But, Brewster said:

> Among the various phenomena of sound no such analogous fact exists, and we can scarcely conceive an elastic medium so singularly constituted as to exhibit such extraordinary effects. We might readily understand how a medium could transmit sounds of a high pinch, and refuse to transmit sounds of a low pinch; but it is incomprehensible how any medium could transmit two sounds of nearly adjacent pitches, and yet obstruct a sound of an intermediate pitch. (1833b, p.363)

The fact that there was not an analogous phenomenon of absorption in the field of sound exposed a potential incoherence within the undulatory theory. If the undulatory assumption of the analogy between light and sound was right, then the absorption of light ought to be excluded from the domain of the undulatory theory because there was no absorption in sound. On the other hand, if an undulatory account of absorption was possible, then the analogy between light and sound had to be given up. Hence, Brewster implied that an undulatory explanation of absorption was inconsistent with the undulatory assumption of the analogy between light and sound. This internal incoherence constituted a non-empirical or a conceptual problem for the undulatory theory, which was distinguishable from those empirical difficulties created by the conflicts between the theory and experimental results.[17] Because of this conceptual problem, Brewster concluded in his report that absorption in gaseous media presented a formidable objection to the undulatory theory.

Brewster's conviction regarding the undulatory theory's problem in absorption was further confirmed when he continued his experiments. When he later condensed the "nitrous acid gas" into a liquid state, all absorption lines disappeared. This fact was

[17] For more philosophical discussion of non-empirical or conceptual problems, see Buchdahl (1970, 1980), and Laudan (1977, pp.454-69).

entirely at odds with the undulatory theory, Brewster claimed. "The aether in the liquid undulates readily to all their rays, while the aether in the gas, in which we should expect it to exist in a much free state, has not the power of transmitting the undulations of two thousand portions of white light" (Brewster 1833b, p.363).

Because of his works in absorption, Brewster dramatically changed his attitude toward the undulatory theory. Until his discoveries of the absorptive power of "nitrous acid gas", Brewster did not openly challenge the undulatory theory. Although he did not accept the theory, he never publicly rejected it. Now, in a paper published in 1833, titled "Observations on the absorption of specific rays, in reference to the undulatory theory," Brewster expressed his first public disavowal of the undulatory theory. At beginning of the paper, Brewster stated:

> I have long been an admirer of the singular power of this [undulatory] theory to explain some of the most perplexing phenomena of optics; . . . The power of a theory, however, to explain and predict facts, is by no means a test of its truth; . . . Twenty theories, indeed, may all enjoy the merit of accounting for a certain class of facts, provided they have all contrived to interweave some common principle to which these facts are actually related. (1833b, pp.360-1)

Here Brewster raised his main methodological objection to the undulatory theory. He insisted that great explanatory power was only a *necessary* condition but not a *sufficient* one for a theory correctly to represent the phenomena. Although the undulatory theory could explain a great number of phenomena related to polarization, double refraction, and diffraction, it did not touch the problems about the nature of ponderable matter and about the interactions between matter and light, such as dispersion and absorption. Citing these reasons, Brewster asserted that "the undulatory theory is defective as a *physical* representation of the phenomena of light" (*Ibid.*, original emphasis).

3.3 Airy's and Herschel's Reactions

Brewster's attack initiated strong reactions from the undulatory camp. On May 7 immediately after reading Brewster's paper in the May issue of the *Philosophical*

Magazine, Airy sent a letter to the journal, seeking to minimize the damage done by Brewster.[18] In this letter, Airy sought to defend the undulatory theory by denying any vital importance to its failure in explaining absorption. One of the major arguments Airy made was that the discussion of absorption did not necessarily enter into a theory of light. According to Airy, reflection, refraction, interference, double refraction, and polarization were the legitimate fields of optics. Absorption, however, required "a supplementary theory," and "either theory (emission or undulatory) seems likely to admit of such a supplement, and I do not see that one will admit of it more easily than the other." Absorption thus should be left as a subject for further investigations. Airy also remarked that, if Newton had included the effects of magnetism and capillarity in his general theory of attraction, the theory of gravitation would never have been formed (Airy 1833b, p.423). Airy's conclusion was clear: the phenomenon of absorption was not an appropriate test for the two rival theories of light, and the failure of the undulatory theory in this subject should not be exaggerated.

Another response to Brewster came from Herschel. In a paper read to the 1833 British Association meeting, which was held in July at Cambridge, Herschel tried to demonstrate the potential of the undulatory theory in explaining absorption.[19] In the paper, Herschel first criticized the emission account of absorption presented by Brewster. According to Herschel, the emission account of absorption was unacceptable because it "appeals to our ignorance" (Herschel 1833, p.402). The emission theory assumed that light consisted of material particles, which were supposed to be permanently existing

[18] The letter was later published in the June issue of the *Philosophical Magazine*, entitled "Remarks on Sir David Brewster's paper 'On the absorption of specific rays, &c.'", (Airy 1833b).

[19] A short version of the paper was published in *Report of the British Association* 3 (1833):373-4. The complete version of it appeared in the same year in *Philosophical Magazine* 3 (1833):401-12.

bodies. In order to explain the extinction of light, the emission account had to consider light being transformed into other materials such as imponderable bodies, heat, or electricity. Although there were hypotheses concerning the processes of transformation of light into other materials, all of them involved serious difficulties and none of them could really make any sense. Hence, appealing to these uncertain hypotheses would do nothing to improve our understanding of absorption.

Unlike the emission account, Herschel argued, the undulatory account of absorption "appeals to our knowledge", specifically, our knowledge of dynamical principles (*Ibid.*). The only difficulty for the undulatory account, as pointed out by Brewster, was the internal inconsistency between an undulatory account and the undulatory assumption of the analogy between light and sound. One way to eliminate this inconsistency was to argue that absorption of sound was also possible. More specifically, this was to argue that a medium like air or the ether could be constituted so as to transmit one wavelength but not another differing only slightly in frequency. Herschel's solution to this problem was a conceptual model. He assumed that the medium transmitting sound consisted of a series of special chambers. Inside each chamber, there were two separate pipes, which had the same starting and ending point but different lengths (Figure 3.6). The length of one pipe was shorter than the other by half the wavelength of a particular note. If a note with this particular wavelength were sounded at the entrance of the chamber, its vibrations would first divide at *Bb* to go along the two separate pipes, and would then meet again at the exit *E* and cancel each other out because of their different phases. If several such chambers were arranged in succession, Herschel claimed, it could easily be imagined that a series of notes would be absorbed by the medium, just like the absorption of light (*Ibid.*, pp.405-6). Through this conceptual model, Herschel had shown that the phenomena of absorption could also exist in the field of acoustics and, consequently, that absorption of light was in principle

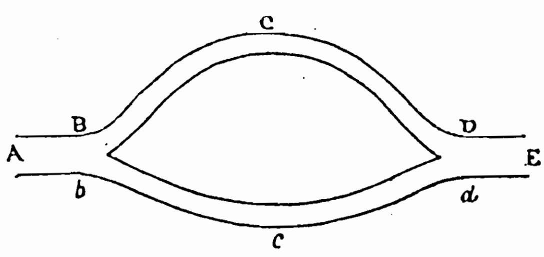

Figure 3.6 Herschel's Acoustic Chamber
[From Herschel 1833, p.405]

explicable by the undulatory theory.

Herschel's conceptual argument was very convincing, at least to those, including Brewster, who attended the section of Mathematics and Physical Science at the 1833 British Association meeting. The *Literary Gazette* reported:

> [Herschel's paper] called forth from Sir David Brewster an important acknowledgement, that the emission theory cannot be supported; and that the observation just made went far to remove his difficulties with regard to the undulating hypothesis.[20]

If this report is accurate, it gives us a clear indication that Brewster and Herschel finally achieved some consensus on the possibility of an undulatory account of absorption. The major objection Brewster had raised in this debate was a conceptual one, an internal inconsistency involved in the undulatory account of absorption. Hence, unless Brewster could identify problems in Herschel's conceptual analysis, he lost his grounds for objection. Herschel had seemingly eliminated the inconsistency by showing the possibility of absorption in sound.

However, what Brewster said at the meeting may just have been a spontaneous statement in the heat of the moment. Moreover, although Brewster admitted that the undulatory theory could in principle explain absorption of light, it did not mean that he had to accept every particular undulatory explanation. In fact he did not surrender himself to any undulatory account of absorption. Later he continued to insist that the available undulatory accounts of absorption were not satisfactory. In the 1837 British Association meeting, Brewster reopened the debate on absorption. At this meeting Brewster criticized the theory of absorption provided by Baron von Wrede, which was the first complete explanation of the subject based upon the undulatory theory. In the view of Brewster, Wrede's theory of absorption was "entirely inadmissible". Brewster's

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[20] *Literary Gazette* 861 (1833):453.

attack on the undulatory account of absorption was quite successful: undulationists like Hamilton were forced openly to concede that Wrede's theory of absorption needed further examination before it could be adopted.[21]

Conclusion: The three debates between emissionists and undulationists in 1833 clearly had different themes and different outcomes. The debate on inflection and the one on prismatic interference belonged to one group, distinguished from the debate on absorption in many ways. In the debates on inflection and prismatic interference, the main theme was the empirical testing of the undulatory theory. Emissionists in these debates used experimental findings to attack their rivals, and undulationists on many occasions challenged the reliability of rivals' experimental reports. These debates ultimately centered on experiment appraisal. But the replication processes that might have settled the disputes in these cases were not completed. Hence, these two debates were indecisive: neither side could even agree on the experimental outcomes *per sa*. The debate on absorption, however, focused on evaluating the conceptual soundness of the undulatory theory. In this debate, Brewster's objection to the undulatory theory involved an inconsistency within the theory, and Herschel's response was to eliminate this inconsistency by hypothetical argument. Since only conceptual analysis was involved, there were fewer communication barriers in this debate than in the other two. This produced a decisive outcome: both sides finally could come to some kind of consensus, though only temporarily.

There were also different types of social interaction between disputants involved

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[21] See *Literary Gazette* 1079 (1837):601, and *Athenaeum* 510 (1837):690. F. James (1983) holds that, after hearing Herschel's paper, Brewster accepted the undulatory account on absorption and avoided reopening debate on this subject. But as these materials show, this obviously was not the case.

in these debates. In the debate on inflection and the one on prismatic interference, the disputes mainly occurred between two different social groups. On the one hand, there were scientific amateurs like Barton and Potter, who had neither formal scientific training nor professional positions. On the other hand, there were scientific professionals like Powell and Airy, who had college education and occupied professorships in first rank universities. Differing social and intellectual status between the disputants made communication, especially informal exchange, extremely difficult. This certainly increased the probability of landing in an impasse in experiment appraisal. The debate on absorption, however, occurred mainly between two scientific professionals: Brewster and Herschel. Their similar intellectual experience and social status made informal communication between them possible and easy. Consequently, Brewster and Herschel in this debate shared many opinions about the subject in question. All these certainly increased their chances of achieving agreement.

Another interesting question is about the tactics that the disputants employed in these debates. In these three debates, both emissionists and undulationists had their own unique and consistent tactics. These tactics reflected actors' judgments of the status of the two rival theories in this particular historical period -- the eve of a fundamental change in the field of optics. Emissionists in this period had lost their faith in the Newtonian emission theory, and did not even have a strong intention to defend their own theory. They openly admitted the problems of their theory, like Potter in the debate on prismatic interference, and easily gave up emission accounts when they encountered rival's criticism, like Brewster in the debate of absorption. Their tactic in these debates was only to attack the undulatory theory, but they did not compare it with their own theory. This was the basic strategy emissionists adopted in the whole extended emission-undulatory controversy.

Although the emission theory was increasingly suspect, the undulatory theory in this period did not become dominant. In the first two British Association meetings, in 1831 and 1832, the undulatory theory did not gain a clear superiority over its rival. Undulationists did not present any papers on optics in these two meetings. Instead, emissionists monopolized all the optical presentations, including the official report. Being aware of their subservient status, undulationists in this period were very sensitive to their rivals' critiques. One tactic they adopted was to give immediate responses to every attack from the rival, because they were afraid that these attacks might damage the image of their theory. Sometimes they even openly admitted the importance and seriousness of a rival's critique, as Hamilton did in his reply to Potter's attack. The purpose of this tactic was to attract the attention of other undulationists, mobilizing more allies into the battle. All these tactics, however, appeared mainly in this period of uncertainty. Undulationists soon gave up these tactics after they established their dominance in the key British scientific societies.

Chapter 4
The Undulationists' First Victory

The 1833 British Association meeting was held at Cambridge, the stronghold of the undulatory theory. William Whewell, one of the most committed undulationists of the time, was the elected president of the meeting. In his presidential address, Whewell spoke on the controversy between the emission theory and the undulatory theory of light. According to Whewell, undulationists's researches in the past few years clearly showed that the theory had reached a similar status to the gravitational law. The undulatory theory was able not only to explain those facts that it was originally intended to account for, but also to present a coherent picture covering entirely different classes of optical phenomena. The emission theory, on the contrary, had nothing corresponding to these successes. Whewell thus openly criticized Herschel's opinion that if the emission theory had been cultivated as much as its rival it might have had a similar brilliant future. Facing the great successes of the undulatory theory, Whewell claimed, no one could now still have hope for the emission theory. According to Whewell, the stage of final showdown in the field of optics had come, or, in his own words, "the prominent point of interest [in optics] is the selection of the general theory" (1833, p.422).

Whewell's address at the 1833 British Association meeting indicated that the rivalry between the emission theory and the undulatory theory had reached a turning point. After competing with the emissionists for several years, undulationists in Britain were now confident enough to proclaim the replacement of the emission theory and the establishment of the undulatory control in the major scientific institutions in Britain.

In this chapter, I discuss a series of cognitive, social, and political factors that brought about the undulatory control in the major scientific institutions in Britain. First, I examine two important experiments conducted by undulationists, which confirmed novel predictions and provided powerful evidence for the undulatory theory. Furthermore, I describe two battles, one in the British Association and the other in Edinburgh University, which were crucial for the undulatory victory. I show how undulationists established their control in this scientific organization and their monopoly in this university by manipulating their political power and the cognitive successes of their theory. These two battles demonstrated that experimental successes *per se* could not produce the undulatory victory, and that only when appropriate contextual conditions were present could cognitive factors in this episode become effective in supporting the undulatory theory.

1. The Intellectual Successes of the Undulatory Theory

Previous historical studies on the emission-undulatory controversy usually attribute the undulatory victory in the mid 1830s to its empirical successes, in particular the experimental confirmations of some novel predictions of the theory. Some historians pinpoint Airy's experiments on Newton's rings (Whittaker 1960, p.126) and Lloyd's experiments on conical refraction (Cantor 1983, pp.195-6) as the crucial experiments that verified the undulatory theory. In this section, I will examine these two important experiments, and their roles in the establishment of the undulatory control, especially from the actor's point of view.

1.1 Airy on Newton's Rings

If a thin plate with one convex surface is placed on a plane plate, and a beam of unpolarized light falls on these two plates, a set of colored rings with a black spot in the center will appear. These colored patterns were called the colors of thin plates, or Newton's rings, because Newton was the first to note the phenomenon. In the early 1830s, advocates of both the emission theory and the undulatory theory claimed that they were able to explain these colored patterns. On the Newtonian theory of light, these rings were produced by the light that reflected from the surface of the plane plate, due to the "fits" of the light particles. According to the undulatory theory, however, the generation of these rings depended on the interference of the light reflected from the plane plate with that reflected from the convex plate. Hence, if the light reflected from the surface of the convex plate was stopped, the rings ought to vanish according to the undulatory theory, but to remain according to the Newtonian account.

Airy conducted a series of experiments on the colors of thin plates, and hoped that he could produce conclusive evidence in favor of the undulatory theory.[1] As in the ordinary experiments on thin plates, Airy put a thin plate with a convex surface over a plane plate. Instead of using unpolarized light as the light source, however, he used polarized light with the plane of polarization perpendicular to the plane of reflection. By setting the angle of incidence of the polarized light equal to the polarizing angle, Airy found that the whole set of the colored rings disappeared. This phenomenon could easily be explained in terms of the interference principle, Airy noted. When the angle of the incident light was equal to the polarizing angle, according to the Brewster Law, no ray of light could be reflected from the surface of the convex plate. The only rays that reached the eye were those reflected from the surface of the plane plate. Consequently, no interference and no colored rings occurred.

In order to further demonstrate that the vanishing of the Newton's rings could only be explained in terms of the undulatory theory, Airy designed another experiment, in which he replaced the bottom plane plate with a polished plate of metal. Since polished metals could reflect larger amounts of light than glasses, the new experimental

[1] Airy conducted these experiments in 1831, and later published his results in the *Transactions of Cambridge Philosophical Society* (1833d), entitled "On a remarkable modification of Newton's rings".

arrangement should generate more distinct colored rings according to the Newtonian theory of light. However, as in the first experiment, the rings disappeared when the angle of incidence was equal to the polarizing angle. Airy then concluded that "this simple fact (the disappearance of the rings while abundance of light is reflected from the metal) seems to be satisfactory evidence, if any were wanted, to shew that the rings are produced by interference only", "and the fact appears to be perfectly inexplicable on any theory of emissions" (Airy 1833d, p.281, 279).

Airy's experimental results, however, did not convince Brewster. Brewster admitted that the Newtonian hypothesis of the "fit" could not explain Airy's experiments, and that the colors of thin plates were produced by the interactions of the two reflected pencils of light. But he immediately pointed out, since the principle of interference was reconcilable with the doctrine of emission, at least in his own version, "the disappearance of the rings is not necessary inexplicable on any theory of emission" (1832, p.317). Hence, in Brewster's view, Airy's experiments on Newton's rings were not yet conclusive in determining the fate of the two rival theories of light.

1.2 Conical Refraction

The phenomenon of double refraction in biaxial crystals like Iceland spar and quartz was discovered in the seventeenth century. Huygens explained the phenomenon by assigning different forms to the two waves in the crystals. He described the form of the ordinary wave as a sphere, while the extraordinary wave was a spheroid. The majority of the scientific community at the time, however, did not accept Huygens' explanation of double refraction. Fresnel studied the phenomenon of double refraction in the early 1820s. He first assumed that the elasticities of the etherial medium within biaxial crystals were unequal in three perpendicular axes. He then demonstrated that the surface of the wave in biaxial crystals was neither a sphere nor a spheroid. The wave surface had a form consisting of two sheets whose points of contact with the tangent planes determined the directions of the two refracted rays in the crystals (Figure 4.1). From this new construction of wave surface, Fresnel concluded that neither of the rays obeyed the law of Huygens, but that they were both refracted according to a new and more complicated law (Lloyd 1833a, p.112). However, Fresnel only briefly sketched the method he used in deriving the equation of the wave surface and did not fully explore every implication entailed by his new equation.

William Hamilton studied the phenomenon of double refraction in 1832. Between the late 1820s and the early 1830s, Hamilton developed a series of new concepts in his studies of geometrical optics. Among them the most important one was the notion of "characteristic function". According to Hamilton, a "characteristic function" represented the geometrical length of a ray regardless of whether the ray was considered as particles or as waves (Hamilton 1830, 1837). When applying this new concept to the study of Fresnel's wave surface in biaxial crystals, Hamilton discovered two remarkable features of double refraction that had been overlooked by Fresnel.

Figure 4.2 is a three-dimension representation of Fresnel's wave surface in biaxial crystal, where the circle and the ellipse represent the sheets of the wave. There are four singularities at the points where the two sheets of the wave meet. Fresnel held that, at each of the four singular points on the wave surface, one could draw tangent planes that determined the directions of the refracted rays. He also supposed that only two such tangent planes, one to each sheet of the wave surface, could be drawn at each of the four points. Following this supposition, Fresnel's work implied that, a single ray, when entering into a biaxial crystal and refracted in the direction of the optical axis, would necessarily be divided into two, and only two, rays.

Hamilton's discovery was that the four singular points in Figure 4.2 were in fact conoidal cusps on the wave surface at the points of the intersection of the circle and the ellipse. Consequently, at the end of each cusp, not only two but an infinite number of

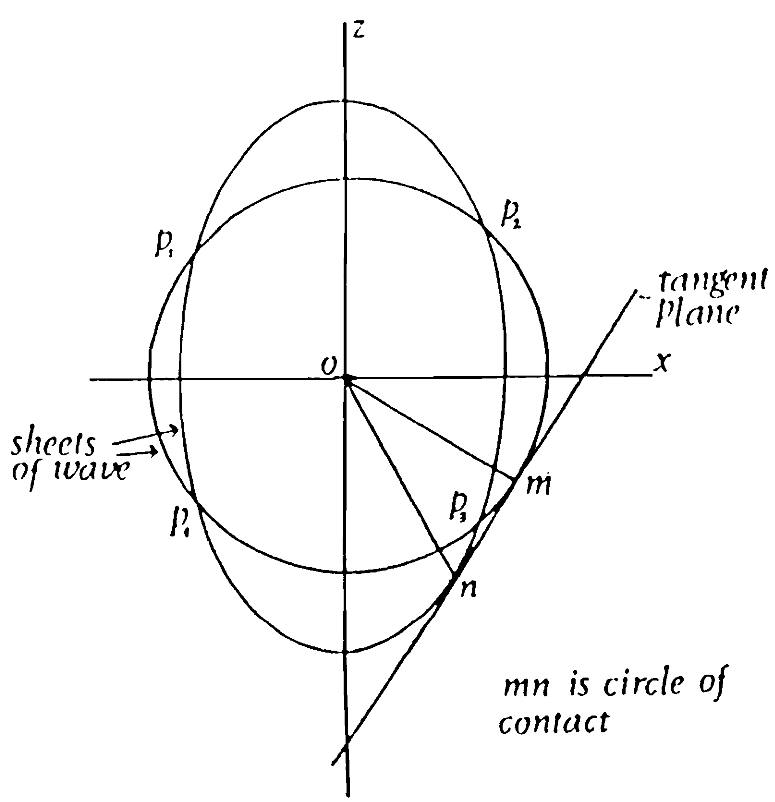

Figure 4.1 Section of Fresnel's Wave Surface
[From O'Hara 1982, p.234]

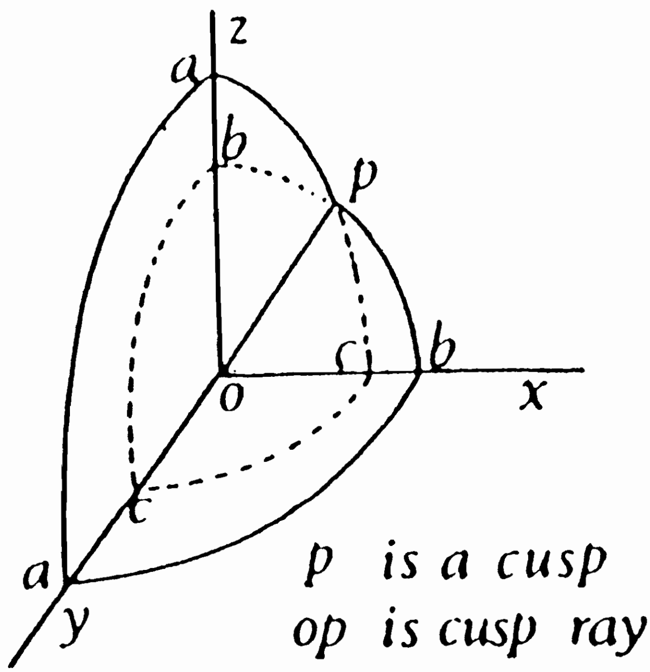

Figure 4.2 3-dimension Representation of Fresnel's Wave Surface
[From O'Hara 1982, p.235]

tangent planes could be drawn to form a tangent cone. This was a novel discovery of the features of the wave surface in biaxial crystals and an important improvement for Fresnel's theory of double refraction. According to Hamilton, Fresnel did not "appear himself to have suspected the existence of these circles of contact", nor did he "appear to have been aware of the existence of these tangent cones to his wave" (Hamilton 1837, p.134, 133).

On the basis of this new understanding of the wave surface in biaxial crystals, Hamilton predicted two hitherto unobserved optical phenomena. According to this prediction, a pencil of light should be refracted as a cone on entering and leaving a biaxial crystal. In the first case, a pencil of unpolarized light, when entering a biaxial crystal and refracted in the direction of the optical axis, would be divided into an infinite number of rays, constituting a conical surface within the crystal. Hamilton called this "internal conical refraction". In the second case, a pencil of light, when passing through a biaxial crystal along a particular direction, would be divided after emerging at the crystal surface into an infinite number of rays, forming a cone of the fourth order. Hamilton called this "external conical refraction" (*Ibid.*, p.136). Hamilton announced his theoretical discoveries at the Royal Irish Academy on October 22, 1832. On the same day, he asked his colleague and friend Humphrey Lloyd to conduct the necessary experiments for confirming his predictions.

Humphrey Lloyd was professor of natural and experimental science at Trinity College, Dublin. He had developed a strong interest in optics in the late 1820s. In 1831, he published a book, titled *A Treatise on Light and Vision*, which mainly covered issues in geometrical optics and physiological optics. It was evident that, when Lloyd wrote this book, he did not prefer either of the two rival theories of light.

Lloyd immediately undertook the task of confirming Hamilton's predictions. He decided first to demonstrate the existence of external conical refraction. The crystal he used in his experiments was a specimen of arragonite. He selected this particular crystal because its three elasticities had been determined. Figure 4.3 shows Lloyd's experimental arrangement for external conical refraction. He placed two plates of thin metal, each having a small aperture, on the two surfaces of the crystal. The line connecting the positions of the apertures was in the direction of the optical axis. He then used a lens of short focus to converge a beam of unpolarized light to the aperture at the upper surface. The incident light was refracted as a single ray within the crystal. Lloyd used a ground glass screen to observe the emergence of the cone directly. After several trials, he successfully produced external conical refraction on December 14, 1832. With sun light as the source, Lloyd was able to show a hollow cone of the refracted rays emerging from the aperture of the second surface. He reported that the section of the cone on the screen was as large as two inches in diameter (Lloyd 1833a, pp.115-6).

Although Lloyd established the existence of external conical refraction, his initial measurements did not conform very well to Hamilton's theoretical predictions. According to Hamilton's calculation, the angle of the cone in external conical refraction should be about 3°. In Lloyd's experiments, however, the measured value of the angle was 6°14', doubling the theoretical result. It took Lloyd a while to explain this discrepancy between the observational result and the theoretical prediction. In a paper published in the February issue of the *Philosophical Magazine*, Lloyd reported his experimental discoveries of external conical refraction, and attributed the discrepancy between his observation and Hamilton's prediction to the effect of diffraction. Lloyd noted that, after the effect of diffraction was taken into account, the real angle of the cone should be half the observed value. He then concluded that the observed angle of the cone should have a mean value of 3°47', which nearly corresponded to Hamilton's prediction (*Ibid.*, pp.118-20).

Lloyd continued his experimental exploration to verify the existence of internal

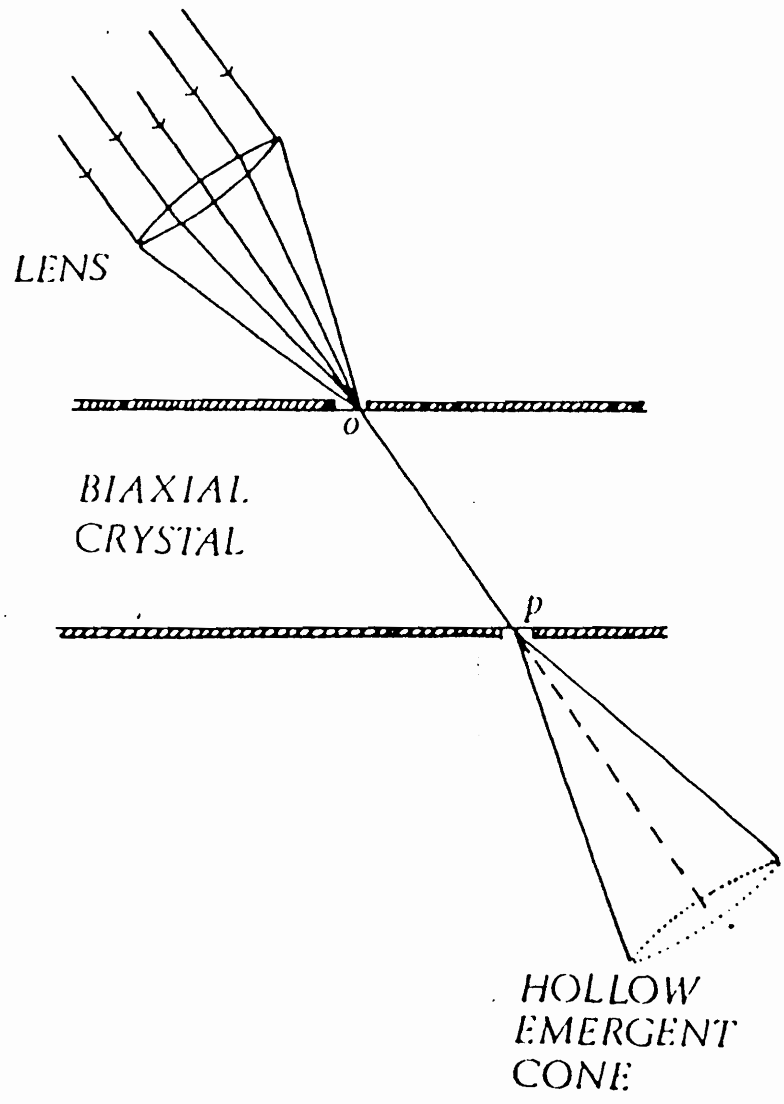

Figure 4.3 Lloyd's Experiment on External Conical Refraction
[From O'Hara 1982, p.235]

conical refraction. In the January of 1833, he succeeded in producing the phenomenon of internal conical refraction. Figure 4.4 shows Lloyd's experimental arrangement. Lloyd first employed a lamp placed at some distance from the crystal as the light source. In order to obtain an incident ray as small as possible, he made the light to pass through two small apertures. One of the apertures was in a screen near to the lamp and the other in a thin plate of metal on the upper surface of the crystal. After observing that the incident ray was generally divided into two beams within the crystal, Lloyd tried to alter the direction of the incidence by turning the crystal slowly. After several trials, he obtained an incidence at which the two beams were seen to spread into a continuous circle, which emerged as a hollow cylinder at the second surface of the crystal. By carefully measuring the angle of the cylinder, Lloyd reported that it was about 1°50', only differing by 5' from the value assigned by Hamilton's prediction (Lloyd 1833b, pp.209-210).

Lloyd's verifications of Hamilton's novel predictions caused a great deal of excitement among undulationists. Some of them like Lloyd used these successes as evidence to prove the superiority of the undulatory theory over the emission theory. In his paper published in the 1833 *Philosophical Magazine*, Lloyd announced that Hamilton's successful predictions of internal and external conical refraction had constituted a crucial experiment for the truth status of the undulatory theory. He wrote:

> Here then are two singular and unexpected consequences of the undulatory theory, not only unsupported by any phenomena hitherto noticed, but even opposed to all the analogies derived from experience. If confirmed by experiment, they would furnish a new and almost convincing proof of the truth of that theory; and if disproved, on the other hand, it was evident that the theory must be abandoned or modified. (1833a, p.114)

Lloyd was definitely right in indicating that the discoveries of conical refraction provided new evidence in supporting the undulatory theory. Lloyd himself must have finally adopted the undulatory theory because of these confirmations. It was evident that Lloyd

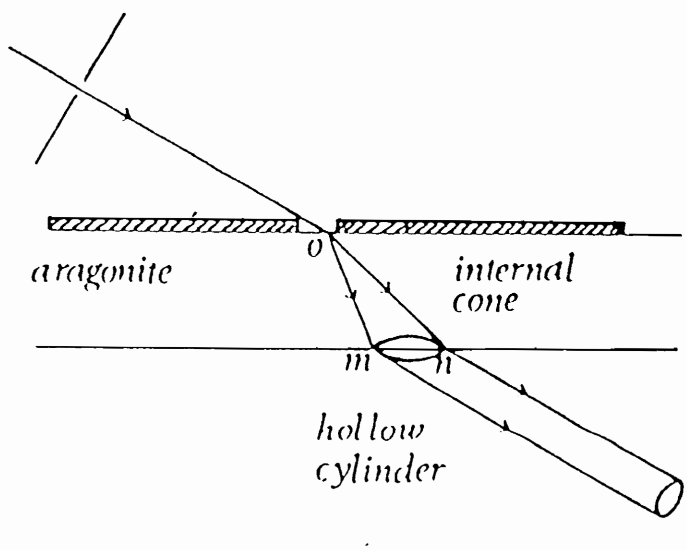

Figure 4.4 Lloyd's Experiment on Internal Conical Refraction
[From O'Hara 1982, p.236]

had not adopted the undulatory theory in 1831 when he published his treatise on light. Between 1831 and 1832, Lloyd did not conduct any optical research except the experiments on conical refraction. Hence, it was the successes in confirming Hamilton's predictions that made Lloyd an undulationist in the early 1833.

Lloyd also presented his experiments on conical refraction as a crucial test for the truth of the undulatory theory. He made his 1833 statement after he had discovered external conical refraction and was confident the he would find internal conical refraction soon. However, Lloyd dramatized the situation by claiming that if Hamilton's predictions were disproved, the theory "must be abandoned", even though he knew that such a scenario would never happen. This was a rhetorical trick, which exaggerated the importance of the experiments on conical refraction by deliberately inventing a scenario of a disconfirmation of the undulatory theory.

Cognitively, Lloyd's argument on the importance of his experiment on conical refraction was unfounded. Hamilton's predictions of conical refraction, based upon his theory of characteristic function and a new understanding of the structure of the wave surface within biaxial crystals, were directly in contrast with Fresnel's theory of double refraction, rather than with any emission account. The successes of Hamilton's predictions proved the superiority of Hamilton's version of double refraction theory over Fresnel's. Since Hamilton's double refraction theory was built upon the undulatory doctrines, the predictive successes could also provide a limited support for the undulatory theory, in the sense of improving its explanatory ability but not proving its truth. However, if the predictions had failed, Hamilton's special double refraction theory would have borne the blame and Fresnel's double refraction theory as well as the general undulatory doctrines would have remained unhurt. From a cognitive viewpoint, Hamilton's predictions of conical refraction can hardly be regarded as a crucial test for the undulatory theory.

In fact not every undulationist claimed that the discovery of conical refraction could play a conclusive role in the emission-undulatory controversy. Most undulationists expressed their cautious welcome to Hamilton's and Lloyd's discoveries, but did not regard them as a conclusive triumph of the undulatory theory. Even Hamilton himself did not interpret the verification of his own predictions in the way Lloyd did. In a letter written to John Herschel in 1833, Hamilton denied that the verification of his predictions could be used to test the two rival theories of light. He told Herschel that "you are aware that the fundamental principle of my optical methods does not essentially require the adoption of either of the two great theories of light in preference to other".[2] For Hamilton, his theory of the characteristic function could be applied equally well to both accounts of light, either emission or undulatory. Hamilton regarded his research on conical refraction as only an application of his optical methods to Fresnel's theory of biaxial crystals. The discoveries of conical refraction indeed could provide another bit of evidence for waves, but Hamilton never expected that it could become a life or death test for the undulatory theory.

From an actor's viewpoint, thus, neither Airy's experiments on Newton's rings nor Lloyd's experiments on conical refraction could guarantee the dominance of the undulatory theory. To understand why the undulatory theory could be so successful in the mid 1830s, we need to shift our attention from cognitive to social factors.

2. The Undulatory Control in the British Association

Within a few years after the British Association was founded, it became an extremely important institution for the emission-undulatory controversy. The annual meetings of the Association and its publications provided a platform for the debates.

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[2] Hamilton to Herschel, (December 18, 1832), in Grave (1882, Vol.1, p.627).

More importantly, its official report on the recent conditions and progress of different scientific subjects became a powerful means to spread a reporter's personal view, with the impression of the endorsement by the Association. Dominating the British Association, in particular controlling its official reports, was one of the crucial steps for the victory of the undulatory theory. In this section, I first document how undulationists controlled the second official report on optics by carefully selecting Lloyd as the reporter. I also give an analysis of the content of Lloyd's report, and a discussion of its role in the emission-undulatory controversy.

2.1 Selection of the Reporter for the Second Optical Report

The first report on optics at the British Association was made by Brewster during the 1832 meeting held at Oxford. In this report, Brewster provided a brief survey of the history and the current development of the field -- a survey entirely based upon his emission viewpoint. After listing all kinds of difficulties that the undulatory theory faced, from dispersion to absorption, Brewster gave the impression that the undulatory theory was far from an acceptable theory of light.

Without surprise, Brewster's report caused strong discontent among undulationists. Due to the successful recruitment conducted by Vernon Harcourt, one of the founders of the British Association, Cambridge academics already constituted the largest geographical group at the 1832 meeting.[3] In the section of mathematical and physical science, the number from the Cambridge school was even more impressive. Among the nine members on the sub-committee of the section, four were Cambridge physicists or admirers. They were Whewell, Airy, Forbes, and Hamilton.[4] For these Cambridge physicists and their followers, the conclusion of Brewster's report was totally

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[3] See Morrell & Thackray (1981, p.126).

[4] See *Report of the British Association* 2 (1832):112.

unacceptable. They could not tolerate Brewster's conclusion on the status of the undulatory theory, neither the spread of the confusion created by Brewster. There therefore requested another report on optics to eliminate Brewster's influence. Considering the high prestige of Brewster, however, these Cambridge physicists and their admirers did not openly criticize Brewster's report. Instead, they simply requested another report at a future meeting "on the phenomena considered as opposed to the undulatory theory".[5] This was a very vague description of the forthcoming report. However, since Cambridge physicists and their admirers occupied important positions in the Association, they were confident that they could ensure that the new report would be written in the way they wanted. Here the key was to select an appropriate reporter, and, through this person, to solve all the problems raised by Brewster in his 1832 report.

The decision on whom the reporter should be was made at the 1833 British Association meeting held at Cambridge. With Whewell as the local secretary, Airy the vice president, and Adam Sedgwick as the president, Cambridge physicists were even more powerful than before. Their perfect candidate for the reporter was a person who was not only a committed advocate of the undulatory theory but also a qualified practitioner of optics, both in theoretical analysis and in experimental operation. Perhaps not by coincidence, a person who perfectly fit these criteria emerged at the meeting: Humphrey Lloyd.

It was the first time for Lloyd to attend the British Association meeting in 1833. But he, together with other Irish "men of science" like Hamilton, gave brilliant performances at the meeting and drew great attention from the scientific community. At the meeting, Hamilton reported his novel theoretical predictions of conical refraction, and Lloyd described how he verified Hamilton's predictions by experiment. In Hamilton's

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[5] *Ibid.*, p.116.

own words, through these presentations, "we Irish formed a strong party this time, and were well received".[6] Lloyd's presentation on his experimental confirmation of conical refraction was particularly successful. In his presentation, Lloyd demonstrated both his theoretical accomplishment in understanding Hamilton's extremely abstract theory and his experimental skills in designing and conducting delicate experiments. More importantly, Lloyd had already demonstrated his commitment to the undulatory theory in his experimental reports of conical refraction. Not surprisingly, Lloyd was selected as the reporter for the new survey of optics in this meeting. He was requested to draw up for the next British Association meeting a report on the recent progress of physical optics. It is interesting to notice that, although the discovery of conical refraction did not constitute a conclusive test for the undulatory theory, it played a crucial role in the emission-undulatory controversy in another way. The confirmation of conical refraction helped to bring about Lloyd's selection as the reporter of the second report on optics. Then it was Lloyd's report that provided further supports to the undulatory theory and secured the undulatory control in the British Association.

2.2 Lloyd's Report on Optics

Lloyd's "Report on the progress and present state of physical optics" was published in the 1834 issue of the Association journal, the *Report of the British Association*.[7] In terms of both length and style, Lloyd's report was very different from the one presented by Brewster. Lloyd's report was 118 pages long, compared to Brewster's fourteen pages. Brewster's report was primarily one-sided, revealing all the problems of the undulatory theory and saying very little about the emission theory.

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[6] Hamilton to De Vere, (July 13, 1833), in Graves (1885, Vol.2, p.52).

[7] It is important to note that the 1834 issue of the *British Association Report* was printed in 1835.

Lloyd's report, however, was comparative, summarizing all the purported facts in the field and examining every theoretical account of them provided by the two rival theories.

In the first paragraph of his report, Lloyd made a clear statement of his goal and method. He wrote:

> The method which I have thought it expedient to adopt in this review has been to take, in the first instance, a rapid survey of the several leading classes of optical phenomena, which the labours of experimental philosophers have wrought out in such rich profusion, and afterwards to examine how far that are reducible to one or other of the two rival theories which have alone advanced any claim to our consideration. (Lloyd 1834, p.9)

Lloyd, like Whewell and Airy, believed that the explanatory power of a theory was a necessary and sufficient condition for its truth.[8] He insisted that, if a theory could make mathematical expressions and these mathematical consequences could be numerically compared with established facts, then the truth of the theory could be "fully and finally ascertained". This was exactly the point that the undulatory theory had reached. Due to the fact that the undulatory theory was able to explain a large group of optical phenomena quantitatively, Lloyd asserted that it was almost, if not entirely, "as advanced as" the theory of universal gravitation. Using the same criterion to assess the emission theory, Lloyd noted that the theory explained a large number of optical phenomena only through adding unconnected supplementary hypotheses. The complexities of the emission accounts, according to Lloyd, "furnished a presumption against its truth; for the higher we are permitted to ascend in scale of physical induction, the more we perceive of that harmony, and unity, and order, which must reign in the works of One Superior Author".

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[8] In his report, Lloyd gave at least two different descriptions of the condition for a true theory. One was the requirement of explanatory success, which came from the Common Sense methodology. The other was a consideration of a theory's internal coherence or harmony. It is interesting to note that Lloyd mainly used the criterion of explanatory success to advocate the undulatory theory, and employed the requirement of internal coherence to attack the emission theory. For more discussion of the different tactics used by Lloyd and other undulationists in the controversy, see chapter nine.

Worst than all, Lloyd added, "in almost every instance in which it [the emission theory] had been developed, its consequences are *at variance* with facts". Hence, the emission theory had exhibited all "symptoms of unsoundness" (*Ibid.*, p.11, original emphasis).

Lloyd's report was massive in scope. He divided the report into two parts: the first treated those phenomena associated with unpolarized light, and the second polarized light. In the first part, he discussed the propagation of light, interference, aberration, reflection, refraction, dispersion, absorption, phosphorescence, diffraction, and colors of thin and thick plates. In the second part, he covered polarization, the principle of transverse vibration, the reflection and refraction of polarized light, double refraction, and the colors of crystalline plates. Surveying these sub-fields one by one, Lloyd presented an enormous number of analyses and arguments that repeated the same theme, namely, that the undulatory theory should be accepted and the emission theory should be rejected.

Lloyd began with the propagation of light. He first examined the Newtonian assumption of the nature of light. When Newton speculated on the physical theory of light, he distinctly stated that the vibrations of an ethereal medium were necessary, although he denied that light consisted of these ethereal vibrations. For Lloyd, Newton here had in fact admitted all the apparatus required in the undulatory theory. "It would appear, then, that Newton assumed *too much*, and that he erred against his own valuable rule -- *Causas rerum naturalium non plures admitti debere*" (p.23, original emphasis). Without appealing to any observation, Lloyd insisted that Newton's assumption of the nature of light was unacceptable simply because it violated the methodological rule set be Newton himself.

For reflection and refraction, the emission accounts required an additional and awkward hypothesis of "fits" of easy reflection and transmission. Worst of all, according to Lloyd, the emission theory also involved an inconsistency in its assumption on the velocity of light in refractive medium. The emission doctrine required that the velocity of light should be greater in the denser medium. However, in Newton's explanations of the colors in thin plates, he assumed that the intervals of the fits, which were proportional to the velocity of light, diminished in the denser medium. It followed that the velocity of light was slower in the denser medium, contradicting the general assumption of the emission theory.

Dispersion and absorption were the two phenomena cited most frequently by emissionists as formidable difficulties for the undulatory theory. However, Lloyd argued that, although the undulatory accounts of these two phenomena were not completely satisfactory, the emission accounts were even worse. The emission account of dispersion attributed the cause to a special kind of attraction analogous to chemical affinity. This supposition, Lloyd said, "is but veiling our inability to assign a mechanical cause for the phenomenon" (p.40). Similarly, the emission account of absorption required an assumption that, in effect, the attractive force between light particles and medium particles varies with the color of the light. This assumption clearly conflicted with the Newtonian account of refraction, which assumed the attractive force did not vary with the colors of the refracted light (p.45).

Unlike dispersion and absorption, diffraction and the colors in thin plates favored the undulatory theory. In order to explain diffraction, Newton supposed that the attractive and repulsive forces succeed one another alternately. However, Lloyd claimed that this kind of explanation was too vague to be accepted (p.52). In a quite similar manner, the emission account of the colors of thin plates required some very strange properties of the fits, "inconsistent with every physical account which has been given of them [the fits]" (p.70). By contrast, the undulatory theory was quite successful in these two areas. The undulatory theory was able not only to provide numerical explanations of diffraction and the colors in thin plates, but also, more impressively, to make new predictions, such as the case of diffraction by an opaque circular disc (pp.57-9).

In those cases related to polarized light, Lloyd indicated that the hypothesis of transverse vibration adopted by the undulatory theory was impressively successful. He claimed:

> [T]hough we were unable to render any account of this hypothesis, or even to show that it is consistent with mechanical principles; yet the numerous classes of phenomena which it has explained, and the striking and the exact manner in which its predictions have been verified on trial, compel us to admit, that if the law to which we have thus reduced so various and such complicated facts be not itself a law of nature, it at least coordinate with it, in such a sense that we may take it as the representative of actual existence, and reason from it as we would from an established physical law. (p.81)

The emission theory, however, was full of troubles in the field of polarized light. For example, Biot tried to explain the reflection and refraction of polarized light in terms of an assumed polarizing force that, unfortunately, was not connected to any known mechanical law. He therefore was not able to compare his explanation with observation numerically (p.92). For double refraction, Lloyd pointed out that all emission accounts, including the works of Biot, Laplace, and Brewster, were incomplete, in the sense that they did not account for the attendant phenomena of polarization (pp.110-115). Although Biot had developed a beautiful theory of moveable polarization for the colors of crystalline plates, his accounts were unfortunately inconsistent with the decisive experiments conducted by Fresnel (p.132).

After case-by-case comparisons of the two rival theories of light, Lloyd observed that "any well-imagined theory may be accommodated to phenomena, and seem to explain them, if only we increase the number of its *postulates*, so as still to embrace each new class of phenomena as it arises. In a certain sense, and to a certain extent, such a theory may be said to be true". "Such appears to be the present state of the theory of emission" (p.78). However, Lloyd claimed that the emission theory was no longer a physical theory, "whose very essence it is to connect these laws together, and to demonstrate their dependence on some higher principle" (*Ibid.*). In other words, an acceptable physical theory required something more: it should be able to establish the connection of the phenomena that it could explain. He rejected the emission theory of light simply because it did not satisfy this more important criterion for an acceptable physical theory, although it was true "in a certain sense and to certain extent".

Unlike the emission theory, Lloyd pointed out, the undulatory theory of light explained optical phenomena in a totally different way. The undulatory theory not only explained the individual laws in the field, but related the classes of phenomena. "There is thus established that connection and harmony in its parts which is the never-failing attribute of truth" (p.79). The undulatory theory also explained the phenomena not in a vague and general manner, "but in the precise language of analysis, and with an accuracy which the refinements of modern observation have not been able to impugn" (*Ibid.*). These two features, harmony and accuracy, Lloyd thought, constituted the primary and methodological reasons why the undulatory theory was superior to the emission theory.

2.3 The Impact of Lloyd's Report

Lloyd's report on the progress and the present state of physical optics reinforced the undulatory control of the British Association by providing overwhelming evidence and compelling arguments for the undulatory theory. Reading Lloyd's report, those undulationists who controlled the British Association were convinced that the emission-undulatory controversy had been settled and no further report on optics was needed. Indeed, after Lloyd's report, the British Association did not request any further report on optics in the next two decades. The two other reports about optics in the nineteenth century were presented by George Stokes on double refraction in 1862 and by Glazebrook on optical theories in 1885. In neither of these reports was the emission-undulatory controversy raised as an issue.

Undoubtedly, Lloyd's report was an important event in the emission-undulatory controversy. But it may be an exaggeration to claim that Lloyd's report represented a turning point in the controversy, or, that it made the undulationism the orthodox theory of light in the British Association in 1834 (Morrell & Thackray 1981, p.469). One very important fact has been ignored by previous studies: Lloyd did not present his report to the 1834 British Association meeting, and his report only appeared in the journal of the 1834 meeting that was printed in 1835. By the time of the 1834 British Association meeting, Lloyd had not yet finished his writing of the report. He only completed and sent out a portion of his report in early 1835. In a letter to Harcourt, Lloyd explained his long delay, saying that "[h]ad I been aware at the outset of its probable length, or of the labour and time which it would require I should certainly have attempted only to prepare *one part* for the present volume but such an arrangement became impossible from the accident of my *commencing with the second*".[9] The fact that Lloyd's report was not published until 1835 indicated that, if his report had any impact on the emission-undulatory controversy, its effect would not surface in 1834. Furthermore, without an oral presentation at the meeting that had more than a thousand in attendance, the influence of Lloyd's report might not have been as great as historians previously expected.

The immediate impact of Lloyd's report on the scientific community was, in fact, limited. The target audience of this report included those who had adopted the emission theory and those who were theoretically neutral, with the aim of converting them into undulationists. However, Lloyd's report did not change the theoretical position of any known emissionist. Neither Brewster nor Potter decreased their hostility toward the undulatory theory following to Lloyd's report. Moreover, there is no evidence to suggest

[9] Lloyd to Harcourt, (February 8, 1835), in Morrell & Thackray (1984, p.200); original emphasis.

that it produced any new undulationists in the 1830s. All the undulationists in Britain had adopted the theory long before Lloyd's report. These undulationists included Airy, James Challis, James Forbes, Hamilton, Herschel, James MacCullagh, Powell, William Fox Talbot, and Whewell, who had published or presented optical papers in scientific journals or scientific societies.

From a Cognitive point of view, Lloyd's report was not able to make the undulationism the orthodox theory of light in the British Association. Indeed, Lloyd's report in many ways looks like a cognitive judgment of the rival theories of light, employing the cognitive criteria defined by modern philosophy of science, such as Imre Lakatos' model, in which the ability to make successful explanations and predictions is defined as *the* criterion for an acceptable theory. But in fact the persuasive power of Lloyd's report mainly came from its rhetorical rather than its cognitive components.[10]

At the time when Lloyd prepared his report, undulationists in Britain had not gained any decisive evidence to support that their theory had reached the status as advanced as the theory of universal gravitation. The undulatory theory did exhibit explanatory superiority in comparison to its rival. But even undulationists themselves admitted that successes in explanation, like the confirmation of conical refraction, could not be used as a "life or death trial" for the emission-undulatory controversy. Furthermore, the undulatory theory could not explain every important category of observational and experimental results in optics. The main obstacles were the phenomena of dispersion and absorption, the two categories for which undulationists did not have reasonable accounts in the mid 1830s. In addition to these empirical

[10] Here, Cognitive components mainly refer to theory-evidence relationship and theory's internal coherence. Rhetorical components refer to the persuasive or argumentative use of literary devices. For more discussion of the varieties of rhetoric in science, see Finocchiaro (1990).

difficulties, the undulatory theory also had severe conceptual problems, like the vagueness and inconsistencies created by the concept of the ether. Even undulationists themselves realized these problems and admitted the uncertainties they might produce. Airy, for instance, conceded that only the "geometrical part" of the undulatory theory was "certainly true", but "the mechanical part of the theory, as the suppositions relative to the constitution of the ether, the computations of the intensity of reflected and refracted light, &c, though generally probable, I conceive to be far from certain" (1831, p.x).

Lloyd in his report just did not have a strong cognitive ground to claim that the undulatory theory had gained a cognitive status like the theory of universal gravitation. Many arguments for the undulatory theory in Lloyd's report were rhetorical rather than cognitive. One example of Lloyd's rhetorical tricks was his careful selection of the emphasis of his report. Lloyd deliberately shifted his analytic focus to the problems of the emission theory, rather than the successes of the undulatory theory. He claimed that "the proof of its [the emission theory's] insufficiency seems even stronger than the positive evidence in favour of the rival theory" (1834, p.21). In his case-by-case comparisons, Lloyd focused more on analyzing the emission accounts than on advocating the undulatory theory. In his analyses of the phenomena of dispersion and absorption, for instance, Lloyd did not develop any new accounts for these phenomena, and he even admitted the defects of the existing undulatory accounts. But Lloyd argued that the emission accounts were even worse because of their internal inconsistencies. This argument had some persuasive power in supporting the undulatory theory. Since there were only two rival theories of light, every setback of the emission theory could be rhetorically interpreted as a victory of its rival. Using this tactic, Lloyd was able to supply persuasive arguments for the undulatory theory with little discussion of its cognitive merits.

3. The Undulatory Dominance in Universities

In addition to scientific societies, universities and colleges also became an sensitive institution for the emission-undulatory controversy. The control of professorships and university curriculums were vital for both rival theories of light because universities were the major place for training and recruiting new allies. In the following, I first describe how undulationists easily controlled Cambridge University because of some historical factors, and then I document how undulationists defeated Brewster in the competition for a professorship at the University of Edinburgh.

3.1 Cambridge University

Because of its intellectual tradition, Cambridge had the atmosphere most in favor of the undulatory theory. As early as 1812, George Peacock, John Herschel, and Charles Babbage established the Analytical Society at Cambridge to promote the Continental differential calculus. These Cambridge mathematicians vigorously propounded the educational value of mathematics. For them mathematics was a valuable agent of every mental discipline, and particularly, of the physical sciences because of its rigor and certainty. They proposed that mathematical techniques ought to be taught to advanced students as an instrument of physical research. Consequently, the undulatory theory that required heavy mathematical analysis was soon adopted by the Cambridge mathematicians and physicists, probably because they could better employ their mathematical skills in this particular theory.[11]

It was Airy who first introduced the undulatory theory of light into Cambridge. In 1827, shortly after he was appointed to the Lucasian chair, Airy began to give lectures on experimental philosophy. A large portion of his lectures treated optics. In the early

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[11] The interest theory of the Strong Program provides an explanation for this historical phenomenon. See Pickering (1981).

years, Airy's lectures on optics were primarily experimental. He told Herschel that in his lectures he had "repeated most of Fresnel's experiments and they do well".[12] These experiments covered the phenomena of interference, diffraction, and polarization (Shairp 1873, pp.71-3). In his lectures, Airy also compared the two rival theories of light. He claimed that, although some experiments could be explained by either theory, a number of diffraction and interference experiments were "explained perfectly by the theory of undulation, and imperfectly by the theory of emission" (Cantor 1983, p.163). Airy had advocated the undulatory theory in his early lectures, but, according to Challis' recollection, he did not maintain this view exclusively in his experimental class (Challis 1875, p.22).

In 1831, Airy published the second edition of *Mathematical Tracts*, in which he added a 150-page section on optics, and unreservedly adopted the undulatory theory. Airy's *Tracts* was intended as a textbook for students to use in the mathematical Tripos. Cambridge established a system of honors examinations in 1747, which included the Mathematical Tripos and the Smith's Prize examination. Since a high place in the honors list was a main path to fellowships and to good careers, students devoted their university lives to cramming for the examinations, and the content of the examinations in a great degree shaped students' thought. Beginning in the 1830s, optics, both geometrical and physical, constituted an important portion of the Tripos. During this period, the Mathematical Tripos examination contained a considerable number of questions on geometrical optics, and a few, about four to five, on physical optics. The questions on physical optics covered many aspects of the undulatory theory, including interference, diffraction, double refraction and polarization (Wilson 1985, p.16, 18). The correct answers to these questions, undoubtedly, came from Airy's *Tracts*. In this way, the

[12] Airy to Herschel, (July 20, 1830), Royal Society Library, Herschel Paper, Ms.1.43.

undulatory theory, or, more specifically, Airy's version of the theory became the norm at Cambridge.

3.2 The University of Edinburgh

The fate of the undulatory theory in Scotland was slightly different from that in England, where its dominance was established without controversy. In the early nineteenth century, Glasgow University and the University of Edinburgh were the two largest universities in Scotland. At Glasgow, the professorship of natural philosophy was occupied by William Meikleham between 1803 and 1839. Meikleham did not have a strong preference for either of the two rival theories of light. In his lectures, he on the one hand cited Newton's emission theory as the "received opinion", and on the other hand, introduced Airy's and Herschel's works as recent discoveries in the field (Wilson 1985, pp.28-9). Neither the emission theory nor the undulatory theory dominated the curriculum at Glasgow through most of the 1830s. This situation did not change until William Thompson took over the professorship in 1839.

At Edinburgh, the professor of natural philosophy, John Leslie, died in 1832. The vacancy of the chair of natural philosophy immediately triggered a competition between an emissionist, David Brewster, and an undulationist, James Forbes.

For Brewster, the primary motive to apply for the chair was financial. For a long time Brewster had been complaining that "there is no profession so incompatible with original inquiry as a Scotch Professorship, where one's income depends on the number of pupils".[13] In 1816 he declined the offer by the Lord Provost to teach John Playfair's natural philosophy class at Edinburgh, saying that he did not have time to prepare for the lecture adequately. Then in 1828 he expressed his interest in the sinecure Edinburgh chair of practical astronomy, but withdrew when he learned that the salary of this non-

[13] Brewster to Forbes (February 11, 1830), in Shairp (1873, p.59).

teaching position was only £120 a year. But after living without financial security for more than a decade as a scientific journal editor, and suffering a series of setbacks in his editorial career, Brewster changed his attitude toward a professorship. He began to regard a university post as a career "in which I could find ample leisure to pursue my scientific research".[14] So in 1832 he decided to apply for the chair at Edinburgh.

James Forbes (1809-1868) was the youngest son of William Forbes, the leading Edinburgh banker. He entered the University of Edinburgh in 1825 and became a distinguished student in the natural philosophy course offered by Leslie. Forbes was also an informal student of Brewster, who recommended Forbes when the latter became a member of the Royal Society of Edinburgh in 1829. With letters of introduction from Brewster, Forbes made a tour of England in 1831, visiting London, Cambridge, Oxford, and Manchester. At Cambridge, after attending Airy's lectures on the undulatory theory and having intensive conversations with Airy and Whewell, Forbes was deeply attracted by the Cambridge physicists.

The trip to Cambridge was crucial for Forbes in many ways. First, it made him a committed undulationist. Forbes did not have any preference for either theory of light before he visited Cambridge, but he clearly committed himself to the undulatory theory after he came back. Moreover, because of the trip to Cambridge, and in particular the influence of Whewell, Forbes began to be interested in meteorology. In 1832 he presented a report on meteorology to the British Association, proposing the application of mathematical reasoning in the field (Forbes 1832). Finally, the trip helped Forbes make up his mind in choosing his career. In early 1830 when Forbes began to consider his future career, he had two options: he could become either a lawyer or a college professor. His family and friends strongly objected to the option of a professorship.

[14] Brewster to Brougham, (November 6, 1832), University College Library, Brougham Collection, 15,744.

Even Brewster advised him not to give up law as a profession (Shairp 1873, p.59). But with the intellectual and social support from Cambridge, Forbes became confident enough to pursue an academic career and decided to seek the vacant chair at Edinburgh. Hence, Forbes' competition with Brewster for the chair of natural philosophy reflected the confrontation between the rival schools on the nature of light, although the actors of this competition did not publicly identify these partisan connections.

In his competition for the chair, Forbes successfully obtained 59 testimonials in favor from Whewell, Airy, Herschel, Hamilton, Vernon Harcourt, George Peacock, and many more by the end of 1832 (Morrell & Thackray 1981, p.432; Shairp 1873, p.84). Some of these testimonials praised Forbes' scientific achievements, in particular his report on meteorology presented to the 1832 British Association meeting. Other testimonials emphasized Forbes scientific promise and brilliant future. Herschel, for example, wrote that Forbes was "marked by nature for scientific distinction, if he should continue to aim at its attainment". Thus, Herschel claimed that "it would be the height of absurdity to think of raising any objection on the score of standing to one who had already brilliantly distinguished himself, and whose talents and application can only be rendered more precious by the vigour of age - youth he means - to which they are attached" (quoted in Shairp 1873, p.85).

Brewster also endeavored to get support from the members of the scientific community, and he even asked for testimonials from such undulationists as Whewell and Airy. Both Whewell and Airy had written testimonials in favor of Forbes, but, because of Brewster's reputation, they could not simply turn down Brewster's request. They then presented very delicate arguments against Brewster in their testimonials, which were supposed to recommend Brewster. On the one hand, neither Whewell nor Airy openly denied any achievement of Brewster in the field of optics. Whewell, for instance, said that "in the department of optical science he [Brewster] had brought to light more new facts and new principles than any other person in any country, and he might almost say than all other observers together" (*Ibid.*, p.84). On the other hand, however, these undulationists in their testimonials implied that Brewster might not be the best choice despite his prestige. Airy for example expressed his conviction that "there is no person of the present day to whom his [Brewster's] country owes so much for its scientific character as himself". Hence, if the choice was to be determined by an estimation of past successes, it must fall on Brewster. But Airy believed that past success should not be the only consideration for the selection. At the end of the testimonial that was supposed to endorse Brewster, Airy concluded that "whatever the decision of the electors between Brewster and Mr. Forbes may be, it cannot be unfavorable to the university" (*Ibid.*).

Although Brewster knew that he won a lot of credit from his scientific achievements, he was not sure whether he could win this competition. As a Whig, Brewster decided to get help from his partisan allies. He appealed to his old friend Henry Brougham, the Lord Chancellor of the Whig government at the time. Knowing the provost of the university was a Whig, Brewster sent two letters to Brougham, asking him to exert his personal and even governmental influence on the provost.[15] Brewster's lobbying was successful. By the end of 1832, the provost of the university declared himself in Brewster's favor.[16]

The final decision to select the new professor was made by the Edinburgh Town Council. In the minds of the Town Council, scientific credentials were not the only

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[15] Brewster to Brougham, (November 12, 1832), (December 11, 1832), University College Library, Brougham Collection, 15,745, 15,746. The Lord Provost of Edinburgh was John Learmonth (1789-1858).

[16] Brewster to Brougham, (January 3, 1833), University College Library, Brougham Collection, 15,747.

consideration. As stated in an influential Edinburgh journal, "it is clear that other qualifications besides mere celebrity in any particular department of science may be sought for".[17]

These "other qualifications" began with teaching ability. It was well known that Brewster was extremely nervous when he spoke before a large audience. This extreme nervousness even plagued him when he said grace at dinner parties. On one occasion, his daughter recalled, "he began, but as he went on the words chocked in his mouth, and he sat in a faint" (Gordon 1869, p.57). In the minds of the Town Council, this peculiar handicap cast doubt whether Brewster would be a successful lecturer.

The Town Council's consideration in fact went far beyond teaching ability. Forbes's family was wealthy and influential, owning the principal banking house at Edinburgh. The majority of the Town Council were shopkeepers "who", as Brewster later put it, "either had cash-accounts in the above bank, or who received pecuniary accommodations or who were employed by the Forbes, or who were promised employment by them".[18] In addition to this enormous personal and family influence, party politics also played an important role in this episode. The Edinburgh Town Council was essentially a Tory corporation, and the Forbes family was the nucleus of the Tory party in Edinburgh. On the other hand, Brewster was a Whig, recently taking an active role against a Tory candidate in the election at Roxburghshire. Late 1832 was the day of the Reform Bill, and "the spirit of party was then running very high in Edinburgh and elsewhere" (Shairp 1873, p.86). In this situation, whether the candidate was a Tory or a Whig became an issue affecting the Town Council's decision. Moreover, the Edinburgh Town Council was about to be reformed by the Burgh Reform Bill. For most

[17] "Chair of natural philosophy", *Edinburgh Weekly Journal*, (December 26, 1832).

[18] Brewster to Babbage, (February 3, 1833), in Morrell & Thackray (1984, p.159).

council members, the election of the chair would be the last chance to exercise their power. All these factors made the majority of the Council support Forbes, the Tory candidate, in the election.

The final vote took place on January 30, 1833, and the result did not surprise either side. Of the 39 council members, 27 voted for Forbes, 3 abstained, and only 9 for Brewster. Forbes was then appointed as Leslie's successor to the chair of natural philosophy at the University of Edinburgh.

The appointment of Forbes substantially changed the natural philosophy courses at the Edinburgh. When Forbes prepared his first-year lectures in 1833, he found himself in need of scientific counsellors. He turned to his friends at Cambridge, mainly Airy and Whewell. After spending almost a year studying the undulatory theory, Forbes was finally ready to teach his optical course in the winter of 1834. Forbes's first course on optics included 22 lectures, and represented one of the largest topics in his natural philosophy courses (Wilson 1985, p.22). The contents of these lectures on optics were drawn from two sources, Herschel's "Light" and Airy's *Tracts*. Because of the mathematical analyses involved, most students at Edinburgh in the early 1830s did not have the background to understand the subject. Forbes then had to limit his lectures on optics to a small group of ten or twelve students. Nevertheless, Forbes was quite happy with his lectures on optics, because he had been "able to introduce the undulatory theory for the first time in Scotland".[19]

Conclusion: By 1834 the undulatory theory of light had been very successful in dominating both the scientific societies and universities in Britain. Several factors definitely contributed to the establishment of the undulatory control. These factors

[19] Forbes to Whewell, (February 22, 1835), in Shairp (1873, p.117).

included the experimental successes in conical refraction, the selection of Lloyd to make the persuasive report on optics to the British Association, and the monopoly of the teaching positions of natural philosophy in the major universities.

It is interesting to note that the role of experimental confirmations in establishing the undulatory control was more complicated than we usually assume. On the one hand, the discovery of conical refraction undoubtedly produced new evidence to support the undulatory theory. These experimental successes probably persuaded Lloyd finally to endorse the undulatory theory, and definitely generated excitement among other undulationists. On the other hand, undulationists at the time did not believe that these experimental successes alone could prove the truth of the undulatory theory.

In fact the discoveries of conical refraction affected the emission-undulatory controversy in an indirect way. The success in confirming Hamilton's predictions of conical refraction helped Lloyd establish his prestige in the field of optics, thus giving him the opportunity to make the second report on optics in the British Association. By choosing an effective rhetorical format, Lloyd was able to produce a persuasive report. This report finally reinforced the undulatory control in the British Association, which had been set up by the Cambridge physicists who had occupied the administrative posts of the Association.

In addition to experimental confirmation, social and political factors also had a very important role in the establishment of the undulatory success. Clearly, the undulatory theory's success in the field of education was crucial for its dominance. However, the competition for the chair of natural philosophy at Edinburgh was mainly a political game. The primary resources Forbes used to win this game were definitely not his intellectual credibility. Instead, his most important resources were the support of Whewell and Airy, who had intellectual and social power both in natural philosophy and, particularly, in the field of optics. Moreover, Forbes's resources came from the local politics. It was the local political situation that guaranteed his victory. Due to all these factors, Forbes finally was able to defeat Brewster in the competition for the teaching position at Edinburgh, and secure the undulatory monopoly in British universities. Only by this time had the success of the undulatory theory of light become inevitable and perhaps irreversible.

Chapter 5

The Emissionists' Counter-Action

Although undulationists had successfully controlled the major scientific institutions in the mid 1830s, emissionists in Britain did not immediately surrender to the new orthodoxy. Instead, they continued to resist the undulatory theory. During the late 1830s and the entire 1840s, emissionists in Britain kept throwing up all kinds of observational and experimental data that the undulatory theory at that time did not or could not explain, thus hoping to demonstrate its explanatory incompetence and to challenge its dominance. During the same period, undulationists also fought hard to minimize the damage created by emissionists' attacks, maintaining their orthodox status.

In this chapter I document three debates between the two rival camps from the late 1830s to the early 1840s. The first covered a newly discovered phenomenon called the "polarity of light", the second involved the measurements of refractive indices, and the third dealt with the intensities of light in diffraction. In these debates, undulationists did not always get the upper hand. Instead, emissionists on some occasions successfully forced their rivals to concede the inadequacy of their own theory. Moreover, emissionists' experimental findings in this period, originally presented as counter-examples, not only stimulated the improvement of the undulatory theory, but also generated new knowledge eventually absorbed by undulationists. Hence, the debates between the two rival theories of light in the late 1830s and the early 1840s significantly shaped the development of physical optics. In the following sections, I play particular attention to how emissionists initiated these debates, how undulationists responded to these challenges, and the impact these debates had on the development of physical optics. All my analyses support three conclusions: first, the controversy between rival theories could extend long after the cognitive superiority of one of them, according a contemporary standard, became obvious; second, the result of a scientific debate did not necessarily coincide with the cognitive judgment of the corresponding scientific theories; and third, scientists who accepted a cognitively inferior theory could continue to make contributions to the development of science.

1. The Debate on the "Polarity of Light"

The debate on the "polarity of light" was probably the most interesting episode in the emission-undulatory controversy due to its scale and duration. This debate involved almost every major actor in the field of optics in Britain and lasted more than a decade. In this section, I give a detailed analysis of this extended debate. I first examine how Brewster initiated the debate and how he won a temporary victory in the first-round dispute. I also describe the impact of Brewster's challenge on the undulatory theory and how undulationists finally solved the problem.

1.1 The Discovery of the "Polarity of Light"

Fox Talbot in 1837 discovered an interesting phenomenon in prismatic spectra. When he viewed a spectrum formed by a prism through a circular aperture that was half covered by a thin plate of glass (Figure 5.1), Talbot saw a group of parallel dark bands crossing the spectrum. Talbot believed that these bands were due to the interference of the two halves of the prismatic spectrum, one of which was retarded by the plate (1837, p.364).

Talbot's discovery immediately drew Brewster's attention. He repeated Talbot's experiment, in a more delicate manner. Instead of using the naked eye, Brewster examined the spectrum formed in the focus of an achromatic telescope, a method that could produce a distinct and sharp image of the spectrum. Brewster placed a thin plate of glass in front of one half of the pupil of his eye, so that the plate could retard one half

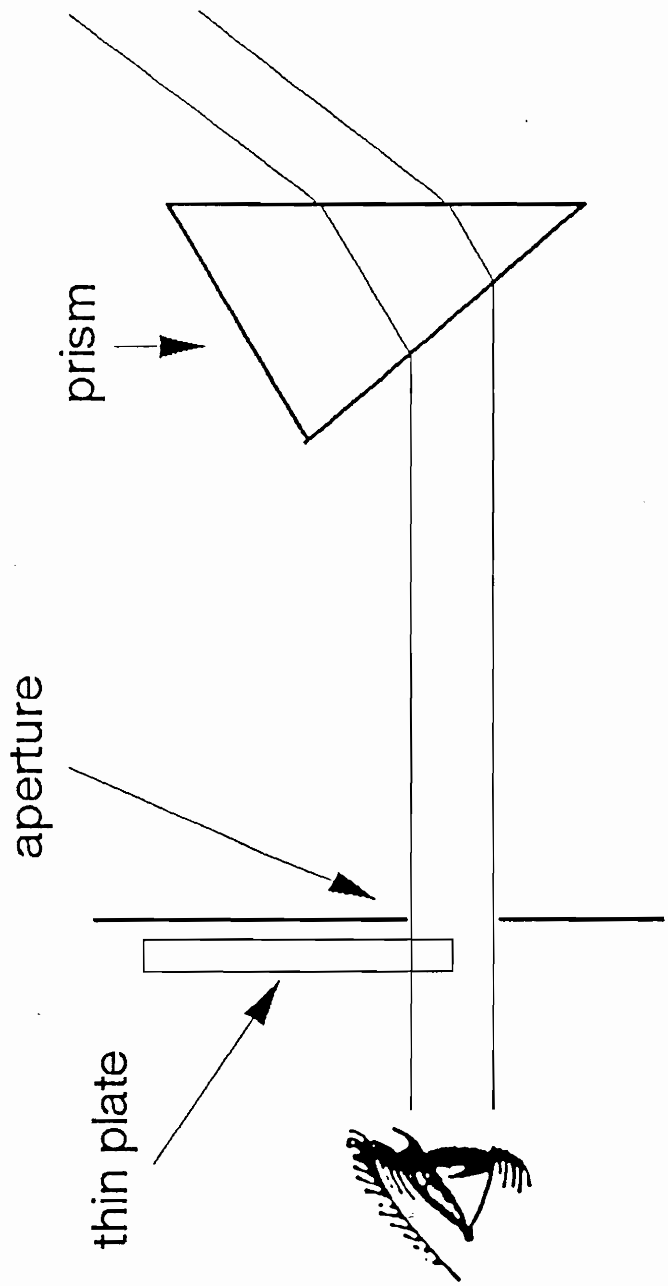

Figure 5.1 Talbot's Experiment on the "Polarity of Light"

of the pencil of light (Figure 5.2). He was then surprised to find a new phenomenon not reported by Talbot. When he held the plate of glass over the violet end of the spectrum, intensely dark bands appeared just as Talbot described. However, when he held the plate of glass over the red end of the spectrum, all of the dark bands disappeared. When the plate was in an intermediate position, the dark bands appeared more or less distinct, according to whether the plate was closer to the violet or to the red (Brewster 1837, p.12).

Apparently, what Brewster originally found was not very exciting to the members of the optical community, because his discovery could easily be regarded as a failure in repeating a newly conducted experiment. However, Brewster in this period was very sensitive to every observational and experimental result that was inconsistent with, or just could not be explained by the undulatory theory. Clearly, Brewster's purpose was to collect all possible evidence to support his rear-guard action against the undulatory theory. Brewster immediately realized that the undulatory theory might not be able to explain why the dark bands disappeared when the plate was in a particular position. According to the current understanding of the undulatory theory, the interference of the two halves of the light should occur no matter where the plate was, and so should the dark bands. Brewster did not want to miss this opportunity to embarrass the advocates of the undulatory theory. He reported his discoveries to the 1837 British Association meeting, hoping to attract people's attention by calling the peculiar phenomenon he found "a very curious and entirely inexplicable property of light."

Brewster's report did not initiate the kind of reactions from the audience that he expected. Whewell and Lloyd were present when Brewster read his report. Both undulationists tried to minimize the impact of Brewster's discovery. Whewell simply denied that there was any new property of light involved in Brewster's discovery, although he was not quite sure about the details of Brewster's experiment. Lloyd

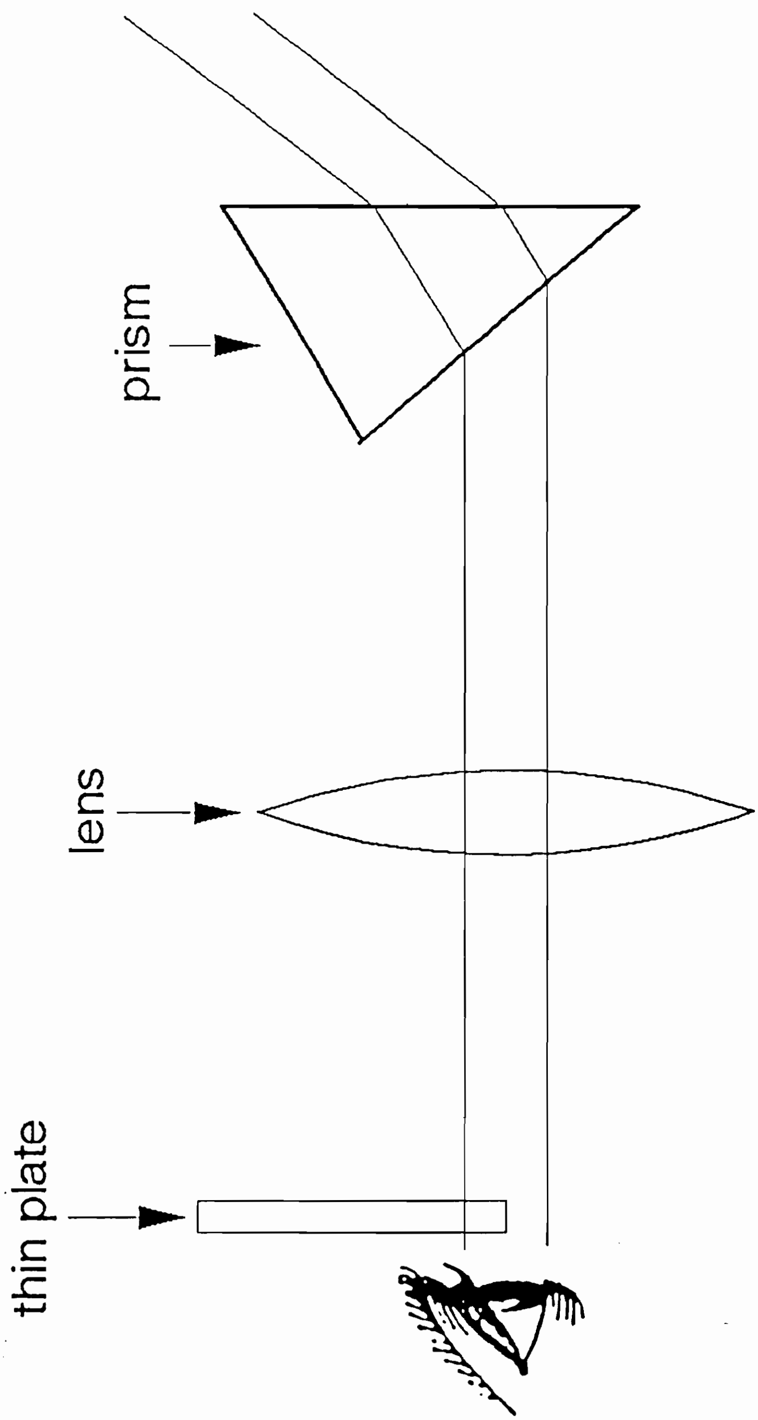

Figure 5.2 Brewster's Experiment on the "Polarity of Light"

admitted that he could not at the present imagine any probable way of explaining the fact, but he insisted that this inexplicable property described by Brewster should not compel people to adopt the conclusion that "the time of an undulation of light could, under certain circumstance, be altered".[1] In effect, these undulationists tried to argue that, although Brewster's discovery might be inexplicable, it was trivial because it did not affect the cognitive superiority of the undulatory theory. After the undulatory theory became dominant, this was a common tactic adopted by undulationists when they faced the anomalies.

Brewster did not give up his endeavor despite the disappointing response at the 1837 British Association meeting. He continued to conduct experiments, and brought up the topic again at the 1838 British Association meeting. At this time, he organized his argument in a more deliberate way, and obtained the response he expected from the audience.

Brewster first told his audience about a new discovery. Instead of using one plate, he had let one half of a light beam pass through a series of plates, each the same thickness but different widths, piled up so that different parts of the light beam should suffer different degrees of retardation. Brewster described in detail the difficulties he encountered in combining such a series of thin plates. Finally, in a demonstration of his great experimental skill, he used laminated crystals and obtained the desired combination of plates. By looking through this combination of plates, Brewster said that he observed another surprising phenomenon. A splendid series of bands and lines crossed the whole spectrum, and the spectrum exhibited the bands "as if it had been acted upon by absorbing media, so that we have here dark lines and the effects of local absorptions produced by the interference of an unretarded pencil with different other pencils,

[1] *Athenaeum*, 518 (1837):719.

proceeding in the same path with different degrees of retardation" (Brewster 1838, p.13). Brewster here clearly wanted to connect his new discovery with the phenomenon of absorption, because people from both camps at that time agreed that absorption was one of the few unsolved problems for the undulatory theory.[2] In this way, Brewster hoped to convince his audience that the undulatory theory could not explain the phenomenon he reported, and the phenomenon might constitute a formidable difficulty for the theory.

In his second report on the new, peculiar phenomenon, Brewster not only described the facts in detail, he also supplied a theoretical interpretation of the phenomenon. He said:

> Here then we have certain phenomena of interference, and also of absorption, distinctly exhibited when the least refrangible side of the retarded ray is toward . . . the most refrangible side of the unretarded ray, while the same phenomena disappear altogether when the most refrangible side of the retarded ray is towards the least refrangible side of the unretarded ray; . . . Hence I conclude, that the different sides of the rays of homogeneous light have different properties when they are separated by prismatic refraction or by the diffraction of grooved surfaces or gratings, - that is, these rays have polarity (1838, p.14).[3]

By introducing the concept of "polarity of light," and by attributing different properties to the different sides of the light beam, Brewster interpreted this newly discovered phenomenon within the framework of the emission theory. Although he did not mention the emission theory at all, he had smuggled the emission doctrine into his theoretical interpretation. He implicitly assumed that the interactions between the light beam and

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[2] For details of the debate about the impact of absorption on the undulatory theory, see chapter 3.

[3] Here that the least (or, the most) refrangible side of the retarded ray is towards to the most (or, the least) refrangible side of the unretarded ray refers to the condition where the retarding plate covers the violet (or, the red) end of the spectrum.

the prism generated the different properties at different sides of the beam.[4]

The responses from the audience were more positive than at the last meeting. Both Whewell and Lloyd were again present when Brewster made his report. This time, however, these two undulationists focused their attention on Brewster's experimental details, and did not raise any substantial objection. John Herschel also listened to Brewster's report, and his reaction was quite positive. After admitting that the undulatory theory was not able to explain the phenomenon described by Brewster, Herschel praised the "indefatigable zeal and industry" of Brewster in these experiments, and asserted that Brewster's works "opened an entirely new field of optical discovery."[5] The compliment from Herschel gave Brewster confidence. So when William Hamilton said that he did not doubt that the undulatory theory would soon be able to give an explanation of the phenomenon, Brewster strongly asserted that he saw no way whatever of accounting for these new properties of light by the undulatory theory.[6]

1.2 The Undulatory Accounts of the "Polarity of Light"

After the undulatory theory of light became dominant in the major British scientific institutions, undulationists were no longer afraid of the new observational and experimental results described as inexplicable by their rivals. Their tactics in handling rivals' attacks changed substantially in comparison with those they adopted in the early 1830s, when they vigorously and rapidly responded to every challenge of their rivals. The new tactic undulationists employed, after becoming dominant, was to play down the

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[4] Brewster's concept of "polarity of light" should not be confused with the concept of polarization. The emission theory interpreted polarization as a property of collection of rays, each of which has an inherent asymmetry. If all rays in a given beam have the same asymmetry, then the beam is polarized. But Brewster's "polarity of light" was a property of a whole beam, without considering the individual rays that consist of the beam.

[5] *Athenaeum*, 566 (1838):625.

[6] *Ibid.*.

value or significance of these experimental works, sometimes just by keeping silent about them. One example is Brewster's experience at the same 1838 British Association meeting. In addition to addressing the problem of the "polarity of light," Brewster also presented three other papers to the meeting on several new phenomena about diffraction that Fresnel's theory could not explain, but the undulationists at the meeting did not say a word about these works. Their silence even embarrassed Herschel, who was sympathetic to Brewster.[7]

The problem of the "polarity of light," however, was different. It was not negligible even in the view of the most committed undulationists. The urgency of solving this problem stemmed not only from the fact that Brewster had brought the problem up twice at British Association meetings, but also from the way that he presented and interpreted it. It was Talbot, an undulationist, who first discovered that dark bands crossed the whole spectrum when a thin plate retarded one half of the light beam. What Brewster found was an extension of Talbot's discovery, namely that the dark bands would disappear if the retarding plate was in a different position. Brewster's discovery then had great persuasive power. It exhibited a phenomenon contradicting Talbot's discovery under almost the same conditions. In the experiment, Brewster only changed the position of the retarding plate, which, according to the undulatory theory, could not create such a dramatic difference. Brewster's results then implied either that the undulatory theory could not explain this new discoveries or that the undulatory account of Talbot's experiment was wrong. To make the thing worse, Brewster interpreted his findings within the emission framework, and introduced a highly theory-contaminated concept -- "the polarity of light" -- to describe the phenomenon. Brewster's presentation and interpretation of his experimental discovery alarmed

[7] *Athenaeum*, 566 (1838):675.

undulationists, and forced them to respond seriously.

It was Airy who gave the first complete undulatory account for the phenomenon of the "polarity of light". In June 1840, Airy presented a paper to the Royal Society, titled "On the Theoretical Explanation of An Apparent New Polarity of Light." He later published this paper in the *Philosophical Transactions*, as the Bakerian lecture for the year. In order to spread his undulatory account among the audience familiar with Brewster's work, Airy also read a simplified version of this paper to the 1840 British Association meeting, although he knew that oral presentation was not an appropriate way to present his mathematical analysis.

The key of Airy's explanation was the refractive effect created by the interior structure of the eye, which functions as a convex lens. Airy's explanation assumed that both Talbot and Brewster in their experiments viewed the prismatic spectra when they were out of focus. When a spectrum was out of focus, a luminous point in the spectrum became a small circular image in the pupil, because of the refractive effect of the eye. When the retarding plate was interposed, some dark bands appeared in the circular image. Since the spectrum consisted of a large number of luminous points, each luminous point produced a group of dark bands, which may be coincident or separate. By a very complicated mathematical derivation based on the undulatory theory, Airy was able to prove that, when the retarding plate covered the violet end of the spectrum, the bands produced by the luminous points would coincide and generate strong dark bands crossing the whole spectrum. When the retarding plate covered the red end of the spectrum, these bands would move further from coincidence so that no dark band was visible (Airy 1840, pp.225-39).

When Airy read his paper to the 1840 British Association meeting, Brewster was present. He might not have fully understood Airy's argument, because it involved a very complicated mathematical analysis, and Brewster himself was particularly weak in this respect.[8] Brewster however seized on a problem in Airy's explanation, namely, that the prismatic spectrum was assumed to be out of focus. This assumption implied that the dark bands existed only when the spectrum was out of focus. But Brewster insisted that, because the Fraunhofer lines in his experiments were sharp and distinct, the dark bands apparently were in focus. Airy's assumption clearly conflicted with his experiments, Brewster claimed.[9]

Brewster's critique forced Airy to consider the situation in which the spectrum was in focus. After revising the assumption, Airy found that the mathematical derivation became much simpler when he assumed that the spectrum was in focus. Following the same computational procedure, Airy obtained the same result, showing why dark bands sometimes existed and sometimes did not. He also deduced a formula that indicated that the intervals of the bands were inversely proportional to the diameter of the pupil. After these successes, Airy was confident that the investigation of the apparent polarity of light "may now be considered as sufficiently complete, and (I conceive) as perfectly satisfactory" (1841, p.9).

Powell also supplied an undulatory account of the apparent "polarity of light". He gave his attention to the subject in the summer of 1839. After conducting several experiments, Powell reported his investigations briefly to the 1839 British Association meeting (1839, p.1). For unknown reasons, Powell delayed publishing the details of his research. However, when he learned that Airy had pursued the research and come up with new conclusions, Powell was anxious to put his own work on record and sent a paper to the *Philosophical Magazine* (1840, pp.81-5).

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[8] For Brewster's own confession, see Brewster to Brougham, (August 25, 1849), University College London, Brougham Collection, 26.643.

[9] *Athenaeum*, 679 (1840):870.

In his account of the phenomenon, Powell attributed the cause of the apparent "polarity of light" to the retarding effect of the prism. In Brewster's experiment, Powell noted, a pencil of light from the spectrum suffered not only the retardation of the plate, but also that of the prism. According to the undulatory theory, the half of pencil that contained the violet rays underwent more retardation in the prism than the other half that contained the red rays. Therefore, if the retarding plate covered the violet end, the difference in the degree of retardation between the two halves of a pencil would be reinforced and dark bands would appear because of interference. However, if the retarding plate covered the red end, the difference in retardation would become minimal, and the dark bands would disappear.

1.3 The Temporary Victory of Brewster

Neither Airy's nor Powell's account satisfied Brewster. Powell's account was wrong according to Brewster, simply because it did not have any physical foundation. Brewster noted that Powell's treatment assumed that a whole half of the pencil suffered the same degree of retardation from the prism. This assumption was physically erroneous. Brewster maintained that "every elementary part of the spectrum consists of rays which have passed through all the different thicknesses of that portion of the prism which receives that incident beam of white light" (1839, p.781). Hence, different parts of the spectrum suffered different degrees of retardation, and Powell's account could only explain the vanishing of some dark bands but not the disappearance of the whole set. Moreover, the "polarity of light" did not merely exist in the spectrum produced by a prism. In 1839 Brewster used a number of parallel grooves cut on a polished steel surface to produce an interference spectrum, and found the same "polarity of light" in this spectrum. This experimental finding indicated that the "polarity of light" resulted from something other than the retardation of a prism.[10] Brewster thus asserted that Powell's account did not reveal the real cause of the phenomenon.

Airy's account of the "polarity of light" was also problematic. Brewster addressed the problem in two papers presented to the British Association, one at the 1842 meeting and another at the 1845 meeting (Brewster 1842; 1845). Brewster's papers stirred up heated debates in the British Association. Although in these debates Brewster alone faced almost all of the first-rank undulationists in Britain, including Airy, Powell, Herschel, Hamilton, MacCullagh, Challis, and Lloyd, he successfully made his point and finally convinced some of his rivals.

According to Brewster, Airy's account of the "polarity of light" involved two major difficulties. The first one was the assumption that a single point of a spectrum formed a circular image in the pupil. Airy introduced this assumption when he assumed that Brewster viewed the spectrum when it was out of focus. Under these circumstances, this assumption was obviously valid. Later when Airy considered the case in which a spectrum was in focus, he only altered one parameter in his mathematical derivation and did not further justify this assumption. With the assumption that a spectrum was in focus, it is obvious that Airy needed further analysis to explain why a single point in a spectrum was able to produce a circular image in the pupil. Thus Brewster asserted that Airy's assumption was "quite untenable," and "cannot for a moment be admitted" (1845, p.8).

A more formidable difficulty in Airy's account, Brewster indicated, was its formula for the relationship between the intervals of the bands and the size of the pupil. Airy had obtained an inverse relationship between the intervals of the bands and the size of the pupil, or the radius of the object-glass if a telescope was used. Brewster

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[10] Later Powell claimed that his account could "apply equally well" to the case of interference spectrum, but he did not give any detail of how it worked. See Powell (1839, p.795).

conducted several experiments to test the inverse relationship that Airy proposed, with negative results. After carefully repeating the experiments, Brewster confidently stated that the dark bands did not vary with the diameter of the pupil or of the object-glass. The intervals between the bands remained the same, whether he looked through a pin-hole or with the pupil in its fullest expansion, and they were constant when the aperture of the object-glass varied from a quarter of an inch to four inches. Hence, Brewster concluded:

> The system of bands to which Mr. Airy's theory is applicable has no existence in nature; ... the phenomena which I discovered are still unexplained by the undulatory theory, and may be regarded as indicative of a new species of polarity, till they are brought under the dominion of some general principle. (*Ibid.*)

Base on his new experimental findings, Brewster intensified his objection to the undulatory theory. The "polarity of light" was now no longer an inexplicable phenomenon to the undulatory theory, but a contradictory one. Undulationists had tried their best to solve the problem, but in the view of Brewster they had failed.

The successes in exposing the difficulties and problems in Powell's and Airy's accounts on the "polarity of light" encouraged Brewster to launch a strong attack against the undulatory theory. At the 1845 British Association meeting, Brewster even proposed a discussion of the general merit of the undulatory theory after it had dominated the field for more than ten years. Brewster insisted that the undulatory theory still failed to explain whole classes of well-observed and distinctly marked phenomena. One of these classes of unexplained phenomena was the recently discovered "polarity of light," which remained inexplicable despite the efforts of Powell and Airy. The second class of unexplained phenomena was the one he had discovered more than a decade ago: the phenomena of transverse fringes that crossed the fringes produced by grooved surface (Brewster 1829, pp.301-6). In these phenomena, a polished metallic surface with equal and equidistant grooves failed to reflect a single ray of homogeneous light at various angles of incidence, while it reflected all the rays freely at intermediate angles. The explanation of these "extraordinary facts", Brewster said, was beyond the power of the undulatory theory. Its failures in explaining these classes of phenomena gave Brewster a sufficient reason to reject the undulatory theory:

> Notwithstanding the great power of the undulatory theory in explaining phenomena, and its occasional success in predicting them, I have never been able to consider it as a representation of that interesting assemblage of facts which constitute physical optics. (Brewster 1845, p.7)

Considering the time when Brewster made this complaint, he could expect nothing but fierce counter-attack from the undulatory camp, because he was openly challenging the general merit of the undulatory theory. Surprisingly, Brewster got a sympathetic response from one of his rivals. This was from James MacCullagh, professor of mathematics at Trinity College, Dublin. After listening to Brewster's complaints against the undulatory theory at the 1842 British Association meeting, MacCullagh expressed his sympathy for Brewster's position. MacCullagh noted that some undulationists tended to exaggerate the explanatory power of the undulatory theory. They asserted that the undulatory theory had explained some phenomena that it actually did not. In fact, MacCullagh said, the undulatory theory was still very imperfect. Undulationists still "knew so little of the undulatory theory, that it would be premature to pronounce, that it either could or could not explain every thing." The major problem, according to MacCullagh, consisted in the physical foundation of the theory. The undulatory theory could provide very beautiful mathematical explanations of some optical phenomena, but it said very little about the constitution of the ether and the interactions between the ether and the particles of bodies.[11] MacCullagh thought that perhaps the research style of the

[11] For detail of MacCullagh's analysis of the undulatory theory's problems in its physical foundation, see chapter 6.

theory, namely, the employment of a purely mathematical approach, was responsible for the neglect of physical inquiry.[12]

Agreeing with MacCullagh, Brewster expressed his strong discontent with the current tendency in the field to overlook the importance and value of experimental inquiry. The undulatory theory just explained some of the "grosser phenomena," but some undulationists supported it in a way that held back optical science by discouraging all experimental researches. People who knew very little of the subject had praised the theory as perfect, and even ventured to place it on the same level as the theory of universal gravitation.[13] These people held up those facts explained by the theory as great discoveries, while they ignored other far more interesting and valuable facts simply because they were either hostile to or unexplained by the theory.[14]

To support his criticism that undulationists discouraged experimental researches, Brewster complained to the audience about one of his recent experiences. In 1841 he had submitted a paper containing mainly experimental results on polarization to the Royal Society, but the Council of the Society rejected its publication. As one of the oldest members of the society, and author of more than thirty papers in the *Transactions*, Brewster felt humiliated. He believed that the Council rejected the paper solely because it was experimental and contained results and views hostile to the undulatory theory. This rejection was a clear indicator that the process of discouraging experimental research had spread to such an extent that "even learned societies were so completely

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[12] *Athenaeum*, 769 (1842):662; *Literary Gazette*, 1332 (1842):534.

[13] Brewster here referred to Airy and Whewell.

[14] *Literary Gazette*, 1332 (1842):534.

under the incubus of the undulatory theory."[15]

Among the undulationists who listened to Brewster's attack, MacCullagh was the only one who was sympathetic. Most could accept neither Brewster's critiques nor the doubt caste on the theory. Nevertheless, they admitted, as Brewster had claimed, that neither Airy nor Powell could explain the "polarity of light," and that the phenomenon was still a problem for the undulatory theory. What they did was to minimize the damage, dismissing any implication of this local failure. For example, Herschel asked the audience to suspend their judgment, not to put the theory on trial for life or death based just upon Brewster's extraordinary discovery. Similarly, Hamilton reminded the audience that, although undulationists considered Brewster's discovery as inexplicable, "it would not be supposed that the wave men were wavering, or that the undulatory theory was at all undulatory in their minds." Hamilton stated that at least the "wave men" from Dublin, probably except MacCullagh, retained as strong a conviction as ever of "the substantial truth" of the undulatory theory.[16]

Even the most stubborn undulationists such as Airy and Powell also realized the compelling power of Brewster's critique. Although they did not accept the charge made by Brewster and insisted that they had explained away the phenomenon, they knew that it was not a good idea to continue to confront Brewster on this subject. Thus, after Brewster presented his critique at the 1845 British Association meeting, Airy was clearly not willing to carry on the debate. He complained that he was not aware of Brewster's plan to discuss the subject until he saw the announcement about half an hour before the meeting began, and he said that his memory on the subject was so imperfect that he did

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[15] Brewster believed that Airy, who acting as referee of the *Philosophical Transactions*, was responsible for the rejection and had done this entirely from his personal feelings. See Brewster to Brougham, (December 4, 1841), University College London, Brougham Collection, 26,624.

[16] *Athenaeum*, 769 (1842):662; *Literary Gazette*, 1332 (1842):534.

not even remember the details of his account. So Airy declared that under this circumstance he was totally unprepared to debate the matter, and refused to have any substantial discussion with Brewster.[17] It seems likely that Airy here had realized that the best tactic in handling Brewster's challenge was just to keep silent.

1.4 The Solution of Stokes

The temporary victory of Brewster at the 1842 and the 1845 British Association meetings made the problem of the "polarity of light" a burning issue of the day, and forced undulationists to continue their researches on the matter. Of the two available undulatory explanations of the phenomenon, Airy's account was more sophisticated and more promising. However, nobody could accept this account due to Brewster's very compelling critique. Airy predicted that the intervals between the dark bands were in an inverse relation to the diameter of the pupil, but Brewster's experiments showed that the intervals had nothing to do with the size of the pupil. In 1846 Powell repeated Brewster's experiments, and found the same results (1846, p.4). This criticism, which now came from a friend rather than a rival, forced Airy to reconsider the problem. Later in 1846 he sent a paper to the *Philosophical Magazine*, claiming that both Brewster and Powell had misunderstood his mathematical analysis and drawn an invalid implication from it (Airy 1846, pp.337-41). Airy argued that his inverse relation between the intervals of the bands and the size of the pupil was subject to a limitation, which required "a relation between the aperture of the eye or telescope on the one hand, and the extent of the spectrum and the thickness of the retarding plate on the other hand." He had not discussed the expression for the intervals of the bands when the size of the pupil was varied without varying other parts of the experimental arrangement. Airy insisted that he had stated this limitation clearly in his 1841 paper, but neither Brewster nor Powell

[17] *Athenaeum*, 924 (1845):699.

had paid attention to it. By conducting several experiments, Airy showed that the intervals between the bands really varied with the size of the pupil, if the limitation on other parameters was satisfied. Airy thus concluded that his account of the "polarity of light" was successful, and gave one of the strongest confirmations of the undulatory theory.

Powell in this period also continued to focus his attention to the problem of the "polarity of light." With the help of George Stokes, Powell in 1847 developed a new experimental apparatus that could reproduce Brewster's phenomenon of "polarity". Powell sent a paper to the Royal Society to report this discovery. At the beginning of this paper, Powell admitted that, given the advanced state of theory of light, his topic -- a case of interference of unpolarized light -- could hardly be deemed of sufficient importance to form the subject of a paper for the Society. However, Powell claimed that the matter he was going to discuss was a case "which by no means stands isolated, but offers analogies with other classes of phenomena which have excited considerable interest and discussion, especially with regard to what has been termed, perhaps improperly, a 'polarity' in the prismatic rays" (1848, p.213).

The new experimental apparatus Powell introduced was a hollow glass prism containing highly refractive liquid and a transparent plate (Figure 5.3). When light was transmitted through this apparatus, a number of dark bands appeared crossing the spectrum, just as Talbot and Brewster had found. However, with some combinations of plate and liquid, such as glass with water, or glass with oil of turpentine, the dark bands disappeared entirely, just as Brewster had discovered when the plate covered the red end of the spectrum.

Powell's mainly experimental paper did not give any strict or detailed explanation of his experiments, especially the "polarity of light." But Powell's experiments drew the attention of Stokes. In his correspondence with Powell between 1837 and 1838, Stokes

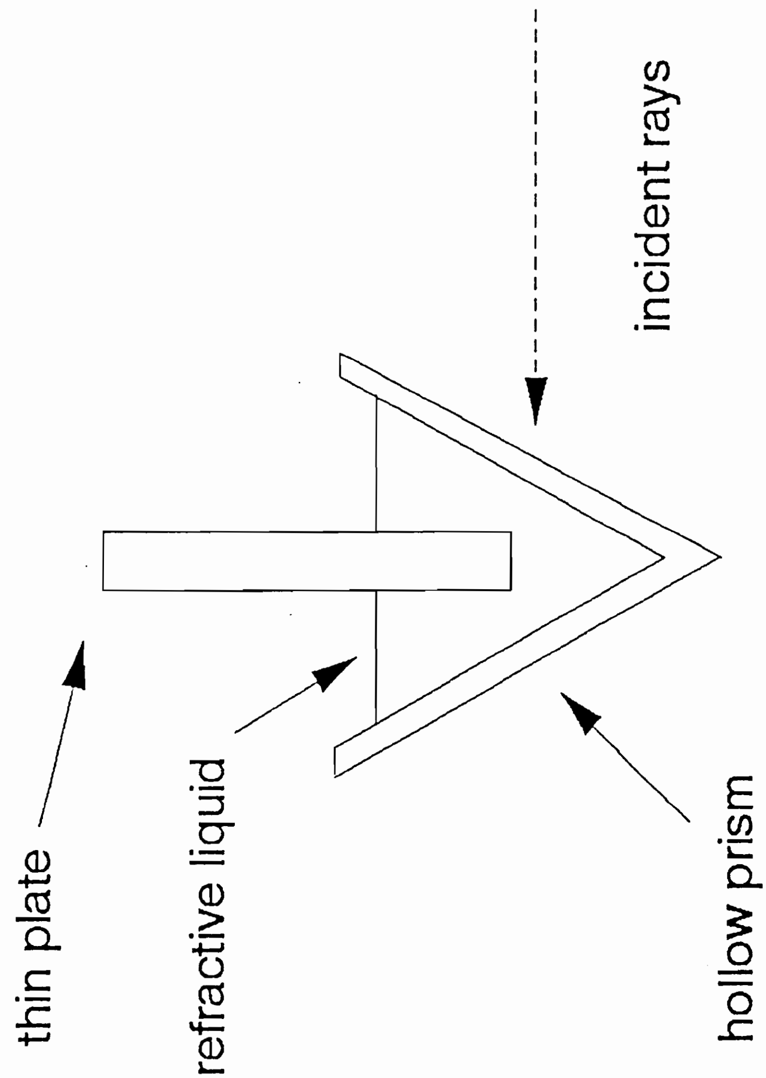

Figure 5.3 Powell's Experiment on the "Polarity of Light"
[From Powell 1848, p.214]

provided Powell with a detailed mathematical analysis of the phenomenon. Stokes' analysis was so fertile that Powell later complained of an "embarras de richesses" and suggested that Stokes write a separate paper (Larmor 1907, p.115).

Accepting Powell's suggestion, Stokes sent a paper to the Royal Society (Stokes 1848, pp.227-42). His explanation of the phenomenon of the "polarity of light" did not fundamentally differ from Airy's, but he explicated his account in a sophisticated way so that he could overcome all the difficulties and problems that Airy had met. In his account, Stokes justified the assumption that a single point of light in the spectrum would produce a small, but slightly diffused image even when the spectrum was in focus, and attributed the cause of these diffused images to the effect of diffraction. This resolved one of the objections that Brewster posed. With his mathematical analysis, Stokes was able to simplify the mathematical derivation and eliminate a couple of mistakes Airy had made. The new account no longer implied any direct relation between the band intervals and the pupil size, resolving another objection raised by Brewster. Moreover, he corrected a mistake in Airy's treatment, which implied the appearance of dark bands when the retarding plate covered the red end of the spectrum, contradicting the experimental results. This was a problem that even Brewster had not seen. At the end, Stokes was able to derive a formula for the intensity of light in the spectrum. This formula specified that dark bands would appear when the retarding plate covered the violet end of the spectrum, and these dark bands would entirely disappear when the retarding plate covered the red end, in agreement with Brewster's experiments.

Stokes's account of the phenomenon of the "polarity of light" was very convincing, because it completely overcame the objections Brewster raised. Even Brewster could not deny its success. When Brewster once again mentioned the matter at the 1852 British Association meeting, he acknowledged that the phenomenon of the "polarity of light" was "merely the internal diffraction fringes" and the undulatory theory had fully explained it (Brewster 1852, pp.24-5). After that he never raised this issue again. Thus, the debate on the polarity of light, which had lasted for more than ten years and drawn the attentions of almost every major actor in the field of physical optics, finally ended.

2. The Debate on Refractive Indices

The major actors in the debate on refractive indices were Brewster and Powell. The importance of this debate lay not in its impact on the measurement of refractive indices, but in its influence on the problem of dispersion, the remaining obstacle for the undulatory theory. In this section, I first document how Powell raised the question of determining refractive indices in his attempt to develop an undulatory account of dispersion. Then I describe Brewster's criticism of both the methods and the results of Powell's measures.

2.1 Refractive Indices and Dispersion

In the early 1830s, it was widely recognized that the undulatory theory of light failed to give any adequate explanation for the phenomenon of dispersion, namely that light with different wave-lengths suffered different degrees of refraction. This failure certainly stemmed from the complexity of the phenomenon. Even at the empirical level, the phenomenon of dispersion was very irregular. Close examination showed that degree of dispersion did not maintain a simple proportion to refractive index, and that there was no regular ratio between rays at different positions in the dispersive spectrum (Powell 1835b, p.249). Up to the mid 1830s, undulationists were incapable of deriving any empirical law to describe the phenomenon, not to mention a physical explanation of it. The phenomenon of dispersion thus persisted as a formidable difficulty for the undulatory theory, and attracted more and more attention from undulationists.

Beginning in 1835, Baden Powell published a series of papers, in which he developed an undulatory account of dispersion. The key to Powell's explanation was to derive, from the general equation of the propagation of light, a relation between the length of an undulation and the velocity of its propagation. If he obtained such a relation and proved its validity, the phenomenon of dispersion could become explicable. In order to obtain such a relation, Powell appealed to Cauchy's undulatory theory. Following Cauchy's assumption that the molecules of refractive media were so disposed that the intervals between them always bore a ratio to the wavelength, Powell was able to derive a formula connecting the wavelength and the velocity of the wave's propagation. Powell realized that, before he applied this formula in the case of dispersion, he had first to verify it empirically. Since measuring the velocity of light was extremely difficult at the time, Powell decided to replace the velocity of light in the formula with the reciprocal of refractive index, and expressed the formula as follows:

$$\frac{1}{\mu} = H \frac{\sin\left(\frac{\pi r n}{\lambda}\right)}{\left(\frac{\pi r n}{\lambda}\right)}$$

Here μ is refractive index, λ is the length of an undulation, H, r, and n are quantities dependent of the nature of the medium, and r is a quantity dependent of λ (Powell 1835b, p.250). To test this formula, Powell first needed to deduce the values of the refractive indices corresponding to different wave-lengths, and then compare these theoretical values with experimental measures. But the job of theoretical derivation turned out to be difficult, because all the constants in the formula were unknown. Powell first adopted an indirect and approximate method. He used the observed values of the refractive indices corresponding to two given wavelengths to determine the constants, and then extrapolated other values of the refractive indices by assuming that the change of the index was proportional to the change of wave length (Powell 1835b, p.251). Later, on Hamilton's advice, Powell employed an indirect method that calculated the value of a refractive index corresponding to a given wavelength by using the observed values of three other refractive indices (Powell 1836d, pp.204-10).

After determining a way to derive the values of refractive indices corresponding to different wavelengths from the formula, Powell's next step was to compare these derived results with observations. Fraunhofer had made the most accurate measurement of refractive indices in the early nineteenth century. In 1814 Fraunhofer had conducted a series of experiments in which he allowed sunlight to pass through a narrow slit into a dark room and through a prism placed on a theodolite. He found that a variety of dark lines or bands appeared crossing the whole spectrum produced by the prism. Fraunhofer specifically labeled seven of these lines with letters B, C, D, . . . up to H. With the theodolite, he was able to measure accurately the refractive indices of the seven lines and the corresponding wavelengths in ten different media (Ames 1898, p.4). Powell used Fraunhofer's measurements to test his formula. By calculating all the refractive indices corresponding to the seven lines in the ten media, Powell reported that his calculations were very close to Fraunhofer's measurements. He insisted that, because of the coincidences between the theoretical derivations and observations, the formula he deduced from Cauchy's undulatory theory should be regarded as "a very close representation of the law of nature". Consequently, he concluded, the verification of the formula indicated that the undulatory theory of light was able to supply "at once both the law and the explanation of the phenomena of dispersion" (Powell 1835b, p.254).

Powell did not limit his test to the comparison with Fraunhofer's measurements. He also compared his formula with the observations made by Rudberg, who measured the refractive indices of the seven definite lines in another ten different media. The results of these comparisons, according to Powell, were also satisfactory (Powell 1836a, pp.18-9).

In 1836, Powell even decided to make his own measurements of the refractive indices of the seven definite lines in a variety of media, so that he could further confirm his undulatory account of dispersion. Powell recognized that he did not have the skill to handle the delicate apparatus that Fraunhofer used, so he decided to use a much simpler apparatus. The essential part of this apparatus was a prism located in the center of a ten-inch graduated circle. An achromatic telescope with cross wires at its focus was fixed in an arm moveable around the center. Incident light fell into the prism from a slit placed about 12 feet away. The dispersion spectrum was observed through the telescope, and the refractive indices were then measured. The prism in this apparatus was actually hollow in order to measure the refractive indices of liquid media, and a thermometer was inserted into the prism to note the temperature of the media (Figure 5.4). With this simple experimental apparatus, Powell was able to determine the refractive indices of the seven definite lines in twenty-eight liquid media. In May, 1836, Powell reported his discoveries to the Ashmolean Society at Oxford in a paper titled "Observations for determining the refractive indices for the standard rays of the solar spectrum in various media" (Powell 1836, pp.1-24). Later Powell presented his work to the 1836 British Association meeting, in which his work drew the attention of Brewster and subsequently initiated a debate on the measurement of refractive indices.

2.2 Brewster on the Methods of Measuring Refractive Indices

In Brewster's view, measuring the refractive indices of all kinds of media was one of the most important tasks in the field of optics. In his 1832 report on optics presented to the British Association, Brewster had listed the determination of "the refractive and dispersive powers of ordinary solid and fluid bodies" as the problem to which "we would call the attention of young and active observers" (Brewster 1832, p.319). Following Brewster's suggestion, the British Association in 1833 put the determination of refractive

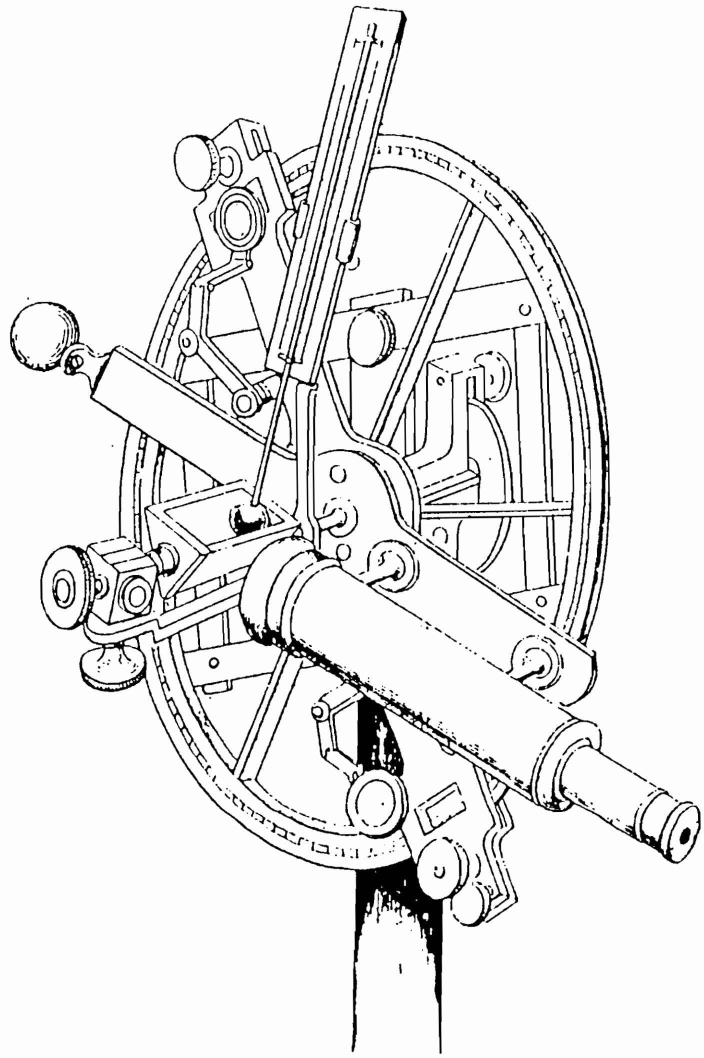

Figure 5.4 Powell's Instrument for Determining Refractive Indices
[From Powell 1839, Plate I]

and dispersive indices into its list of recommendations for optical studies.[18] When Powell decided to pick up this research topic, the Association in 1835 provided him a small grant to carry on the experiments (Powell 1839, p.2). Probably regarding Powell's effort on refractive indices as an attempt to follow his suggestion, Brewster's initial reaction toward Powell's work was very positive.

At the end of his presentation at the 1836 British Association meeting, Powell admitted that he had difficulties in measuring the refractive indices of several crystals with high refractive power. These crystals were always imperfect and very difficult to reduce to the proper shape for measurement. Hearing Powell's difficulties, Brewster made a short comment at the meeting, giving Powell some tips to overcome these problems. Brewster told Powell that one method to fix crystals was to cover up their imperfection with China ink or, sometime, by interposing silk. A more effective method was to use the "triple oxalate of chromium and lead" as the interposing substance. In this way, Brewster said, he could always succeed in making the lines in the spectrum distinct and visible. Brewster also remarked that the angle between the incident light and the edge of the prism was crucial for the success of the measurement. He suggested Powell pay attention to this key parameter, and accurately determine this angle in his next measurement.[19]

Powell, however, did not adopt Brewster's methods to fix imperfect crystals. Instead, he continued to look for substances that had both high refractive power and a perfect shape for prismatic examination. Finally, he came up with a prism of chromate of lead that met his requirements. In his experiment, however, this prism did not produce any distinguishable lines for measurement. Powell reported the failure of his

[18] *Report of the British Association* 3 (1833):473.

[19] *Athenaeum* 461 (1836):608.

experiment to the 1838 British Association meeting. According to Powell, the spectrum this prism produced was confused and distorted, none of the seven definite lines were visible, and the whole of the blue and violet rays was absorbed (1838a, p.6).

Brewster was clearly disappointed when he found that Powell did not follow his suggestion. After hearing Powell's description of the difficulties and problems with the prism of chromate of lead, Brewster immediately pointed out to Powell that he had explained a method of solving these problems two years ago, at the 1836 British Association meeting. For such substance as chromate of lead, Brewster said, interposing a thin plate of mica could overcome the difficulties. In this way, although Fraunhofer's lines in the spectrum could not be seen directly, the interposing plate could produce a group of parallel lines crossing the whole spectrum. By counting the number of these parallel lines, Brewster said, he could determine the refractive indices of the substance as accurate as by the aid of Fraunhofer's lines.[20] Several days later, at the same British Association meeting, Brewster once again mentioned Powell's problems with the prism of chromate of lead. Brewster indicated that he had no difficulty in obtaining distinct lines in the blue and violet section of the spectrum produced by the lead chromate prism, because he had remedied the problems by using a small refractive angle. Brewster also remarked that the suggestions he had made to remove Powell's difficulties, including interposing a plate and carefully selecting the refractive angle, were the only methods by which approximate measurements could be made in the case of lead chromate. Without following his suggestion, Brewster implied, Powell was doomed to fail in measuring the refractive indices of crystals with high refractive power.[21]

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[20] *Athenaeum*, 566 (1838):622.

[21] *Athenaeum*, 566 (1838):626.

2.3 The Positions of the Lines G and H

Brewster not only caste doubt on Powell's skills in handling prismatic experiments, he also claimed that Powell's measurements were not accurate. The major problems came from the measurements of the lines G and H. When Powell compared the derived refractive indices with the observation results, he always found discrepancies at the line G. In several ordinary media, the derived refractive indices at line G were always significantly in excess of the observation results provided by Fraunhofer. Powell insisted, however, that these discrepancies resulted from the ambiguity in the experimental data, and not from the theory nor from his derivation. According to Powell, the position marked G by Fraunhofer in the spectrum actually consisted of a number of small lines, which, in media with high refractive power, spread over a considerable space (1838a, p.6). Where was the exact position of the line G? According to Powell, the answer was straightforward: "it appeared to me the only fair and reasonable method, to take the *mean* of the *expanded* set of lines as corresponding to the value of the wave-length, given for the *condensed* line" (1838c, p.841, original emphasis). Following this principle, Powell repeated some of his former measurements, and found that he could reduce the discrepancies at the line G into an acceptable range. A similar problem also occurred at the line H, the definite ray closest to the violet end. When Powell measured the refractive indices of such liquid media as anise-seed oil, carbon disulfide, and creosote, he found that the line H marked by Fraunhofer actually consisted of two separate lines. Powell used the same method to handle this problem, namely, to take the mean of these two lines as the precise position of the line H (Powell 1836b, pp.14-7).

According to Brewster, however, Powell's observations of the lines G and H were completely mistaken. At the same British Association meeting (1838), Brewster pointed out that both lines were remarkably distinct and easily recognized, as he knew from almost daily observation. He even brought a map of a prismatic spectrum to the meeting, on a scale of about five feet, to illustrate how the lines G and H were distinguishable in the spectrum.[22] With respect to Powell's report that there were several undistinguishable lines at the positions of G and H, Brewster believed that this confusion stemmed from a defect of Powell's experimental apparatus, probably the relatively low magnifying power of the telescope. Brewster insisted that, since Fraunhofer had obtained the most accurate measurement of these lines with very delicate apparatus, anyone who reported measures different from Fraunhofer's should first use an equally delicate and effective apparatus. Obviously, Powell's apparatus was very coarse, even Powell himself admitted this. Therefore, Brewster claimed that Powell's measures of the lines G and H, which were inconsistent with Fraunhofer's discoveries, were not reliable.

Moreover, Brewster pointed out, Powell's mistakes in measuring the refractive indices at the lines G and H made the undulatory account of dispersion unacceptable. In the case of the line G, Brewster remarked that Powell had taken the mean of a number of lines, which spread over a considerable space, as the precise value of the line. However, this considerable space actually embraced two groups of lines, and Fraunhofer's line G was located in the middle of the group closer to the red end of the spectrum. In the case of the line H, Powell had taken the mean of two separate lines as the precise value of the line. But in Fraunhofer's original map, the line H was the least refrangible one of the two lines that Powell referred to. Therefore, Brewster claimed that all of Powell's measurements of the refractive indices for the lines G and H were "too great". Since the values of refractive indices derived from Powell's formula were always even larger than his observational results, the measurements that Powell had

[22] *Athenaeum*, 566 (1838):625-6.

obtained misleadingly would "offer fewer discrepancies with the undulatory theory" (Brewster 1838c, p.826). Worse than that, Brewster noted, Powell had used the measure of the refractive index at the line H to extrapolate the refractive indices at other definite lines when he applied Hamilton's method to calculate the theoretical values of refractive indices. Hence, the calculated values of the refractive indices at the line C, D, E, and G were affected by the error in the measure of the refractive index at line H (Brewster 1838d, p.876). Since both the measures and the theoretical calculations of refractive indices were problematic, the coincidence between them became insignificant, and Powell's formula that specified a relation between the length and the velocity of a wave also became unfounded. Brewster thus concluded that Powell's undulatory account of dispersion was unsuccessful.

Brewster's critique forced Powell to defend his measures of the refractive indices. In 1839 Powell presented a report on refractive indices to the British Association. In this report, Powell challenged the accuracy of Brewster's representation of Fraunhofer's map of prismatic spectrum. Powell first praised the superiority of Fraunhofer's map of the spectrum (Figure 5.5) "in delicacy of representation, conveying by shading ... an idea of the relative intensity of the different parts of the spectrum" (Powell 1839, p.4). However, Brewster's map of the spectrum (Figure 5.6), which was first printed in 1822 in the *Edinburgh Encyclopedia* and recently presented to the 1838 British Association meeting, lost the "delicacy" in Fraunhofer's original picture. Brewster's picture, Powell said, was very coarse, and blurred several crucial features of the spectrum, especially at lines G and H. For example, in Fraunhofer's original map, "the appearance of the numerous lines about G is beautifully given", but in Brewster's map this detail had been lost and G had become a distinct line. Also in Fraunhofer's map, "two small groups of lines ... at H are made so conspicuous ...", but in Brewster's map, "H is distinctly marked at the point midway between the two bands, instead of being opposite the lower"

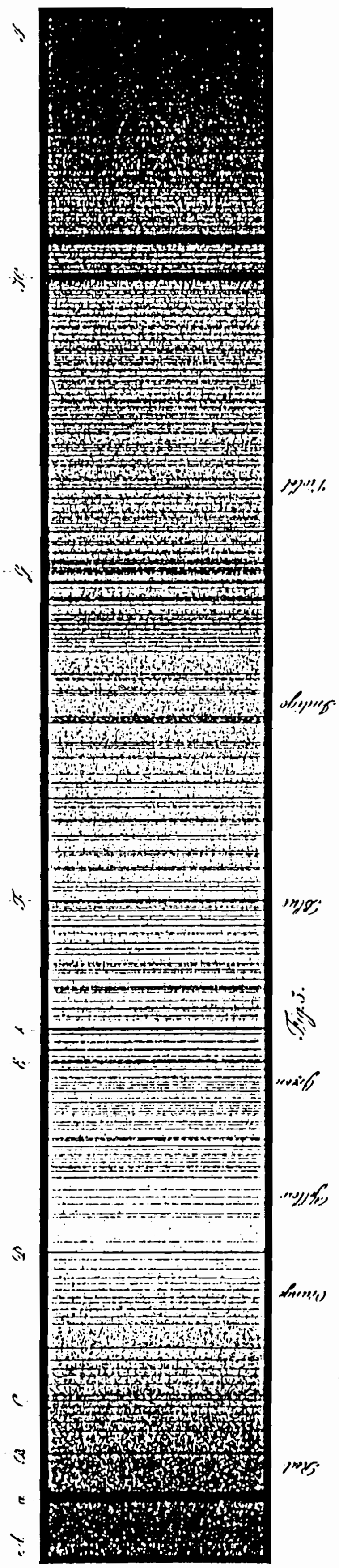

Figure 5.5 Fraunhofer's Map of Solar Spectrum
[From Ames 1898, Figure 6]

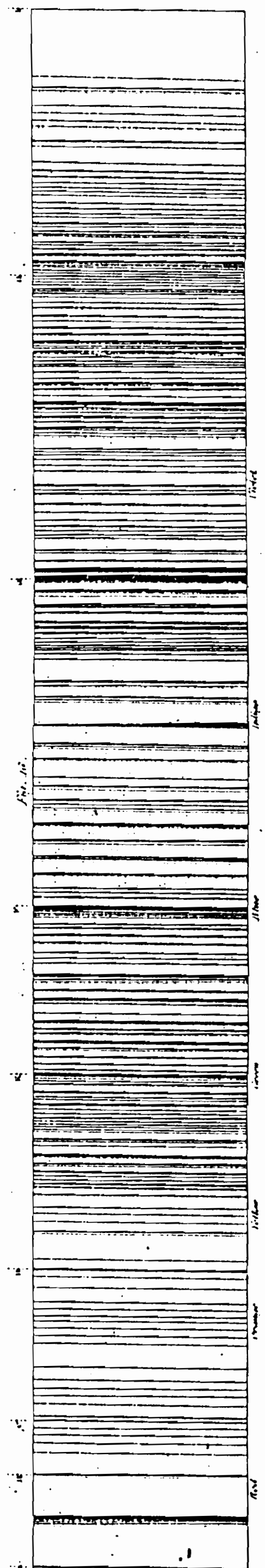

Figure 5.6 Brewster's Map of Solar Spectrum
[From Brewster 1822, Figure 16]

(*Ibid.*). To further support his allegation, Powell presented a map of a prismatic spectrum based upon his own observation (Figure 5.7). In terms of accuracy, Powell's map was drawn very poorly. Fraunhofer's original map contained 574 lines, while Brewster's copy in the *Edinburgh Encyclopedia* contained about 300 lines. Powell's map had fewer than 60 lines. But this map clearly showed that lines G and H were not distinct but fuzzy.

Now the debate between Powell and Brewster reduced to the following simple but crucial questions: Where were the exact positions of the line G and the line H in Fraunhofer's original map? Were they distinct lines or groups of several small lines? Unfortunately, Fraunhofer gave no answer to these questions before he died in 1826. The only way to find the answers to these questions, therefore, was to repeat Fraunhofer's measurements and to redetermine the refractive indices and the corresponding wavelengths from line B to line H in a spectrum. These would be very complicated experimental measurements that required delicate apparatus and sophisticated skills. Although Brewster had conducted experiments to measure the refractive indices of a large number of media, his experimental setting was significantly different from Fraunhofer's (Levene 1966). Brewster did not have the required apparatus and skills for replicating Fraunhofer's measurements, and he at that time was apparently unwilling to repeat such complicated experiments. When Brewster responded to Powell's counterattack in the 1840 British Association meeting, he only repeated all the arguments he had made before, including presenting to the meeting two large diagrams of the detailed structures of lines G and H (Brewster 1840, p.5). Without repeating Fraunhofer's experiments, however, Brewster's arguments did not become a real threat against Powell's works on refractive indices and dispersion. In 1841 Powell presented a paper on the theoretical calculation of refractive indices to the British Association meeting. In this paper, he simply ignored Brewster's criticisms and used his

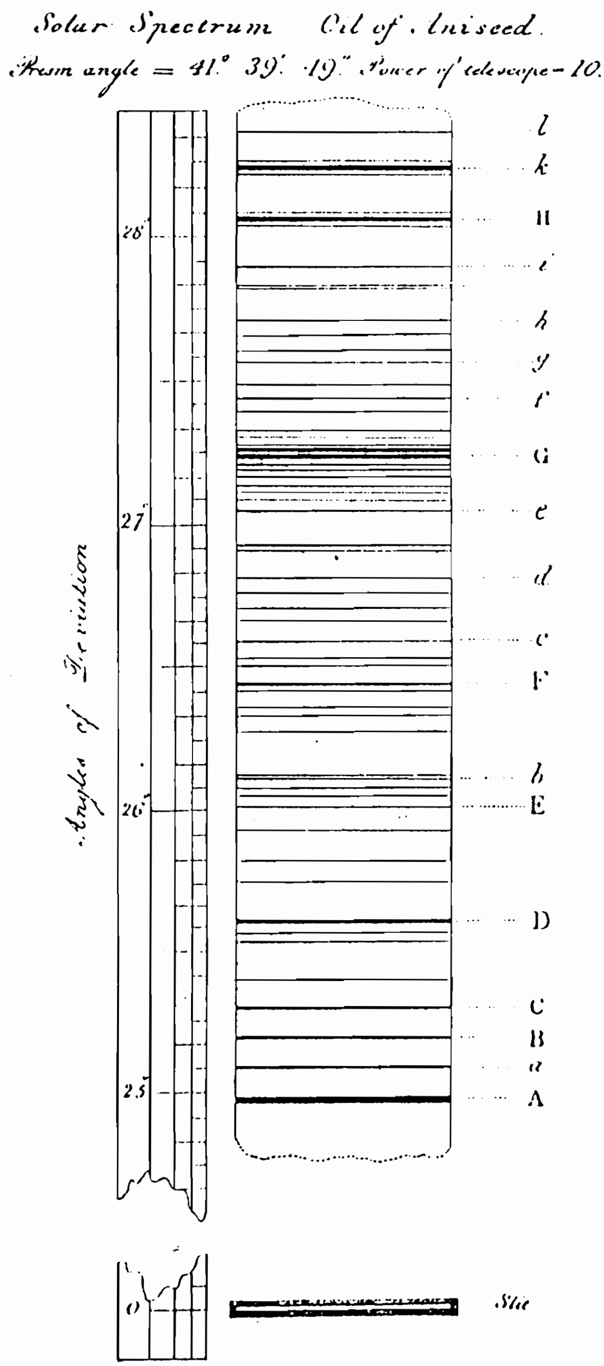

Figure 5.7 Powell's Map of Solar Spectrum
[From Powell 1839, Plate II]

measurements of refractive indices as an unproblematic basis to test his theory of dispersion (Powell 1841, p.25).

Unlike the problem of the "polarity of light", the problem of refractive indices did not become a serious difficulty for the undulatory theory. Indeed, Brewster had make every effort to turn the issue of refractive indices into a test of the undulatory theory. He persistently raised the questions about Powell's measurements in the British Association meetings, and deliberately adopted certain rhetorical tactics in his arguments, like presenting large scale diagrams of the spectrum to his audience. However, he did not conduct the required experiments to determine the refractive indices in question: his criticism of Powell's measurements of the refractive indices of the line G and H depended solely upon conceptual and methodological analyses. Without a surprise, Brewster's argument was easily dismissed by his rival, because it lacked experimental components. In the debate on the "polarity of light", we have seen that, without the relevant contextual factors that included actors' reputation and their rhetorical tactics, an experimental finding could not become the epistemic foundation for a scientific theory. Now, in the debate on refractive indices, we find the other side of the same coin: without substantial experimental findings, contextual factors alone could not bring about a legitimate test of a scientific theory. Thus, the debate on the "polarity of light" and the one on refractive indices together show that theory appraisal involves both cognitive factors (mainly theory-evidence relationship) and contextual factors (personal, social, and contingent relations). Neither cognitive nor contextual factors alone can guarantee a valid appraisal of scientific theories.

3. The Debate on the Intensity of Light

Potter was the major voice in the debate on the intensity of light in the early 1840s, in which he raised objections to the undulatory theory based on his photometric experiments and thus triggered strong reactions from undulationists. This debate was peculiar because it centered on the phenomenon of diffraction. Since the undulatory theory was generally very successful in the field of diffraction, it was unusual for someone such as Potter to attack this stronghold. In this section, I pay special attention to Potter's research approach -- his photometric method -- and why he dared to challenge the undulatory account of diffraction in the early 1840s.

3.1 The Reliability of Potter's Photometric Method

In 1832 Potter had used a comparative photometer to determine the relative intensities of light in the bright and dark rings of interference patterns. He concluded that both the Newtonian and the undulatory theory of light were wrong in predicting the intensity of light.[23] Potter's critiques of the undulatory theory, especially his use of the photometric method, initiated strong reactions from undulationists. One of the reactions came from Forbes. In a paper presented to the Royal Society of Edinburgh in 1838, Forbes caste doubt on Potter's photometric measurements. Forbes' arguments were based upon his experiments on reflected heat, assumed to be analogous to the case of light. Having measured the amount of heat reflected at transparent and metallic surfaces, Forbes found that his experiments had verified Potter's remark that reflection from metallic surfaces was less intense when the incident angle increased. However, Forbes also reported that the amounts of heat reflected from both metallic and transparent surfaces were significantly larger than those specified by Potter's empirical law. "The quantity of heat reflected by the metals is so much greater than Mr. Potter's estimate for light, as to lead me to suspect that his photometric ratios are all too small, which would nearly account for their deviation from Fresnel's law" (Forbes 1839, p.480). Hence, Forbes insisted that Potter's measures based upon photometric method should not be used

[23] For details of Potter's analysis, see chapter 3.

to test the undulatory theory. Later, Forbes admitted that his findings from experiments on heat were only analogical. But he claimed that this analogical method was acceptable "whilst photometric methods are so very imperfect as I still consider them to be, however dexterously employed" (1840, p.103).

The critiques from undulationists did not scare Potter away. Instead, in 1840 he published a paper in the *Philosophical Magazine*, titled "On photometry in connexion with physical optics", continuing to argue the legitimacy of photometric methods. Under attacks by several first-rank undulationists, Potter knew that it would be very difficult for him, a sixth wrangler just graduated from Cambridge in 1838, to prove the legitimacy and the reliability of his photometric experiments. He thus in this paper mentioned the photometric studies conducted by such first-rank researchers as William Herschel and Michael Faraday, possibly hoping that the prestige of these people could help him to establish the legitimacy of photometric methods. He particularly gave details of Faraday's work on photometry as presented in Faraday's 1830 Bakerian lecture on the manufacture of optical glass. Potter first listed Faraday's results on the intensities of reflected light at the surfaces of crown, plate, and flint glasses, which were measured at a 45 degree reflective angle. He indicated that Faraday's results were very close to his own measures with the same glasses but at slightly different reflective angles. Potter also compared Faraday's results with the values derived from Fresnel's theory, showing Faraday's measures were significantly smaller than the values derived from Fresnel's theory, the same conclusion as he drew from his own photometric measures (Potter 1840, pp.19-20). By emphasizing the similarity of results between Faraday's and his photometric measurements, Potter implied that the photometric methods he used were legitimate and reliable.

In the same paper, Potter also decried the critiques of his photometric methods as a sign of a trend in optics that blindly admired Fresnel's theory and overlooked the value of experiment. He complained:

> The fashion of pinning their faith on Fresnel's sleeve having become general amongst the influential in learned societies, and amongst the most eminent in mathematical attainments... Under such circumstance, ... my objections to Fresnel's formula for the intensity of light reflected and transmitted by transparent bodies, although founded on laborious and careful experimental researches, have been treated as though other men's guesses were more worth than my experiments" (*Ibid.*, p.17).

With a desire to find out "the truth according to the principles laid down by Lord Bacon", Potter regarded photometry as an experimental foundation for physical optics. Thus he decided to stick to his photometric approach, and to use this method to expose the undulatory theory's problems with respect to the intensity of light.

3.2 The Intensity of the Central Spot of A Circular Disc

In 1840 Potter published two papers in the *Philosophical Magazine*, challenging the fundamental principles of the undulatory theory. The first paper, titled "On the method of performing the simple experiment of interference with two mirrors slightly inclined, so as to afford an experimentum crucis as to the nature of light", attacked the principle of interference. By examining the interference pattern produced by two slightly inclined mirrors, Potter claimed that he had observed a dark band at the center of the interference pattern, contradicting the undulatory requirement that the central band should be white. Based upon this discovery, Potter suggested that the undulatory version of the interference principle should be amended (Potter 1940a, pp.382-4).

The second paper Potter published in 1840 was titled "On the application of Huygens's principle in physical optics". This paper examined the consequences of Huygens' principle, especially those associated with the intensity of light. As one of the fundamental principles of the undulatory theory, Huygens' principle stated that each point at the front of a wave might be considered as a small source of wave motions. In terms of Huygens' principle and the principle of interference, undulationists had successfully explained a large number of optical phenomena, including the linear propagation of light, which had been a formidable difficulty to the theory before Fresnel. However, Potter in this paper argued that Huygens' principle was flawed because he was able to derive several obviously absurd consequences from the principle.

Potter's analysis was relatively simple. He examined two cases untouched by the undulatory theory: the distributions of the intensity of light in the diffractive patterns produced by a circular disc and by a circular aperture. Starting from the general equation of wave propagation accepted by every undulationist, Potter first derived a general formula regarding the intensity of light in the diffractive pattern behind a circular aperture, which may be a complete circle or a portion of a circle. According to this formula, Potter found that, no matter what the shape of a portion of the circular aperture was, either a parallelogram or a whole sector, the distribution of the intensity of light was always the same. This result was obviously contradicted one of the well-known facts in optics - the linear propagation of light. Potter claimed:

> The result of [Huygens's] principle is therefore that light ought to bend into the shadows of bodies to an indefinite extent, as sound is known to pass through all aperture, and bend round all obstacles. It proves that the result Mr. Airy (*Tract*, page 270.) has obtained by an approximate method is not to be depended upon, and that the objection to the undulatory theory which was believed to have been removed remains in full force. (Potter 1840, p.246)

For the case of a circular disc, Potter also derived a formula regarding the intensity of light in the center of the shadow of the disc, following a similar procedure. This formula also entailed an absurd consequence. Instead of predicting a slowly diminishing intensity along the central line behind the disc, the formula assigned a series of maximum and minimum values of the intensity, regardless of how far from the disc (*Ibid.*, p.248). This consequence also contradicted the known facts about diffraction with a circular disc.

Potter's challenge to the undulatory theory initiated a strong response. The first reaction came from undulationist John Tovey. Just two months after Potter published his paper, Tovey sent a short remark to the *Philosophical Magazine*, claiming that Potter's analysis of the Huygens' principle was completely wrong. According to Tovey, when Potter derived his formulas of the intensity of light in the diffractive patterns produced by a circular aperture and a circular disc, he only considered a luminous line. Potter had not taken other lines in these diffractive patterns into account. But, Tovey claimed, a luminous line was merely a geometrical conception, from which no interference could happen, nor any diffractive pattern. Thus Tovey concluded that "it appears then that Mr. Potter has mistaken a luminous *line* for a luminous *space*; and consequently, that his conclusions have, in reality, no foundation" (1840, p.432).

Another reaction toward Potter's challenge of the Huygens' principle came from Airy. In 1841 Airy published a paper in the *Philosophical Magazine* titled "On the diffraction of an annular aperture". At the beginning of this paper, Airy stated:

> I had no wish to make a communication to you which should assume the form of a discussion with Mr. Potter, and I proposed, therefore, in adverting to the subject to which Mr. Potter has alluded in your number of October last, rather to add something to the investigation of a point which has perhaps been passed over too lightly by writers on the undulatory theory, than to employ myself specially in indicating what I consider to be failing steps in Mr. Potter's reasoning. (Airy 1841, p.1)

Following this tactic, Airy in his paper did not analyze where Potter's reasoning went wrong. Rather, he devoted the paper to developing a comprehensive theory on the diffraction of a circular aperture or a circular disc. In this way, however, Airy could also effectively eliminate the confusions Potter created.

Unlike Potter who only considered the distribution of the intensity of light along one geometrical line, Airy in his analyses took the whole space into account. Starting from the equation of the propagation of light, through a very complicated mathematical analysis, Airy obtained a general formula on the intensity of light in the diffractive pattern produced by a circular aperture or by a circular disc. According to this formula, in the case of a circular aperture, there would be a bright spot at the center of the diffractive pattern, surrounded by a series of dark and bright rings. The intensity of this bright spot was double the source. In the case of a circular disc, there would be a bright spot at the center of the shadow, with an intensity equal to that of the uninterrupted light. The surrounding rings were much feebler, their intensities decreasing rapidly until they became insensible (*Ibid.*, p.9). All of these predictions of the diffraction of a circular aperture or disc were in agreement with observations. Thus, Airy concluded that, despite the objection Potter had made, both the undulatory theory in general and the Huygens' principle in particular stood "as firmly as they did before", and "perhaps even more firmly" (*Ibid.*, p.10).

3.3 Potter's Photometric Experiment

Although Airy's comprehensive theory of the diffractions of circular apertures and circular discs was very convincing and persuasive, it did not silence Potter. In June of 1941, Potter published a paper in the *Philosophical Magazine*, titled "On the phenomena of diffraction in the center of the shadow of a circular disc, placed before a luminous point, as exhibited by experiment". In this paper, Potter narrowed his focus to the diffraction of a circular disc, reporting several experimental results that were inconsistent with Airy's theoretical explanation.

Potter's experimental setting was simple. Using a lathe, he prepared several circular discs of brass with the diameter of 1/20, 1/10, 2/10, 3/10, 4/10, and 7/10 inch. The luminous point in his experiment was the sun's image formed by a lens located at 60 inches from the disc. He put an eye-lens 60 inches behind a circular disc to observe the diffraction pattern. When the whole apparatus was adjusted, Potter saw a bright spot at the center of the shadow cast by the disc, surrounded by a number of colored rings. When using a disc with 1/20 inch diameter, Potter reported that the central bright spot in the shadow of the disc was large, and so bright that "at the first view it would have been taken to be equally bright with the light which had passed uninterruptedly" (Potter 1841, p.154). On the basis of his experience in photometric experiments, however, Potter soon realized that the spot was not so bright as it looked. Since the bright spot was surrounded by a dark ring, the effect of contrast would heighten its apparent brightness.

Potter invented a new technique to eliminate the effect of contrast. He perforated a thin sheet of brass with the point of a needle, making a number of small circular holes of different sizes. He placed this brass sheet in the focus of the eye-lens, making one of these holes transmit the central part of the bright spot, and then comparing it with the uninterrupted light passing through another hole with equal diameter. Potter immediately found that the brightness of the central bright spot in the shadow was very much less intense than that of the uninterrupted light. After repeating the observation in different cases where the diameter of the circular disc was changed, Potter also found that, the larger the disc was, the greater the difference between the intensity of the central spot and that of the uninterrupted light.

In order to estimate the relative intensity of the central spot for the disc of 1/20 inch diameter, Potter used a very simple technique. He cut a number of small plates of mica out of the same sheet, and then paced them before the uninterrupted light to see how many of them could reduce its intensity to equal the central spot. After many trials, Potter concluded that four plates of mica produced the most accurate correspondence. By a simple experiment, he was able to figure out that four such plates of mica would only transmit one-third of the incident light. In other words, in the experiment with a disc of 1/20 inch diameter, the intensity of the central spot in the shadow was only one-third that of the uninterrupted light.

According to Airy's theory on the diffraction of a circular disc, the brightness of the central spot should be equal to that of the uninterrupted light for all sizes of discs.

Thus, Potter remarked that he had just found a result that was at variance with the undulatory theory. Because of the discovery of this anomaly, "the capability of the undulatory theory to explain the phenomena is thus completely set at rest" (*Ibid.*, p.154). Although his photometric methods were severely challenged by his rivals, Potter was quite confident in the reliability of his experimental results, and believed that his experimental results could play a decisive role in affecting the current emission-undulatory controversy. At the end of his paper, he claimed:

> I must be allowed to state, that I consider the controversy, as to the undulatory theory being the physical theory of light, to be nearly terminated; and that the experiments necessary for completing the basis of a physical theory are those now most desirable to be undertaken. (*Ibid.*, p.155).

Despite Potter's assertion that his experimental findings "falsified" the undulatory theory, no undulationist took his charge seriously. This time they just kept silent: no response at all was made toward Potter's measurements of the intensity of the central spot in the shadow of a circular disc.

In fact, undulationists had their reasons for silence. In the undulationists' view, the photometric method Potter used in his experiment was not reliable. In 1838 Powell, for example, presented a paper to the British Association meeting, challenging the legitimacy of photometric methods generally. His target was one crucial procedure in photometric experiments, namely, placing two luminous surfaces in juxtaposition and then using the eye to judge their equalization. Powell asked his audience to imagine a very simple experiment: a candle as the source, a piece of white paper as the screen, and a clear glass plate between the source and the screen to intercept a part of the light from the source. A very important result of this experiment was that "the eye cannot recognized the least difference in the illumination of the part covered by the glass" (Powell 1838a, p.7). However, reflections did take place at the first and the second surface of the glass plate. According to both emission and undulatory theory, no less than one half of the rays were reflected at the first surface alone. If the photometric procedure to be examined was reliable, it ought to exhibit the nearly two to one difference in the intensities of light. Thus, Powell concluded that the principal procedure in photometric method was unreliable, and photometric experiments were totally illegitimate in physical optics.

Conclusion: The debates during the late 1830s and the 1840s clearly indicated that the emission-undulatory controversy did not vanish immediately after the undulatory theory controlled the key scientific institutions in Britain. In a rather long period from the late 1830s up to the end of the 1840s, emissionists kept throwing up all kinds of observational and experimental data that their rivals could not explain, hoping to reopen the debate on the status of the undulatory theory. At the same time, undulationists still felt the threat from their rivals, and fought hard to minimize the damages inflicted by emissionists' attacks. These confrontations between the men of science who belonged to two different theoretical traditions extended long after the intellectual superiority of the undulatory theory, according to the modern standard, had become obvious.

These extended debates between emissionists and undulationists were definitely not trivial. Most of the important practitioners in the field of optics were involved in these debates. On the emission side, there were Brewster and Potter, the most stubborn emissionists in Britain. On the other side, the whole undulatory camp turned out in full strength. Almost every first-rank undulationist in Britain, including Airy, Powell, Tovey, Whewell, Herschel, Lloyd, Hamilton, and MacCullagh, in one way or another took part in the fight. The participation of these great names increased the publicity of these debates. During this period, the major confrontations occurred on the platform of the British Association, the most influential scientific society in Britain. The argumentative papers from both sides appeared in such journals as *Philosophical*

Transactions, *Philosophical Magazine*, and *Report of the British Association*, which had wide audiences.

Undoubtedly, both sides had their own reasons to join the fight. For emissionists, using inexplicable experimental results to initiate debates on the completeness of the undulatory theory was the best way, or perhaps the only way, to rock the theory's dominance. For the undulationists, immediately responding to rivals' challenge was necessary in order to maintain the dominant status of their theory. During this period, emissionists were still able to generate persuasive arguments against the undulatory theory. In some single cases, like the debate on the "polarity of light", they could even get an upper hand in the battle, forcing undulationists to admit their theory's incompetence, and whipping up discontent with the theory from undulationists. The wavering within the undulatory camp certainly irritated those undulationists who were firmly committed to the theory. Therefore, in the viewpoint of the contemporary participants, these debates were absolutely vital, because they might have changed the status of the dominant theory.

Retrospectively, these debates were also highly significant for the development of optics. For example, the phenomenon of the "polarity of light," generated by the interference of unpolarized light, actually did not belong to the research frontier in the 1840s. If Brewster had not addressed the phenomenon of "polarity" again and again at British Association meetings, and if he had not used it to attack the undulatory theory, the problem of "polarity of light" would probably have been shelved. Similarly, if Potter had not attacked Huygens's principle, the comprehensive theory of the diffraction of circular apertures and circular discs would have been born much later. Facing the challenges from emissionists, however, undulationists had no choice but to keep working on the subjects until they could solve the problems. In this way, the undulatory theory considerably increased its explanatory power.

In addition to functioning as stimuli for the revision and extension of the undulatory theory, emissionists in this period also generated new experimental knowledge, which was directly absorbed by undulationists. For example, when Brewster first addressed the phenomenon of the "polarity of light", he indicated that "polarity" could be used to determine the dispersive or refractive indices of materials, by measuring the distance between the dark bands (1837, p.12). Later Stokes accepted Brewster's idea, developing several techniques to determine the refractive indices of the retarding plates by counting the number of the dark bands (Stokes 1849, p.795). In his 1849 paper "On the Dynamical Theory of Diffraction," which constituted a substantial step in the development of the undulatory theory, Stokes repeatedly applied the technique he learned from Brewster, using it in his experiments for measuring the direction of the plane of polarization in diffracted light (Stokes 1849, p.296, 303). Also, in the case of refractive indices, Brewster had developed several techniques for measuring faint lines in the spectrum, such as interposing a thin plate of mica between the prism and the screen. Powell later used this technique to measure the refractive indices of several media that produced very faint lines. By interposing a plate of colored glass, Powell was able to estimate roughly the refractive indices of these media, although their spectral lines were almost invisible (Powell 1839, p.5). The cognitive connection between Brewster and undulationists indicated that emissionists continued to play an active role in the development of physical optics during the late 1830s and the early 1840s.

The debates discussed in this chapter also illustrate the important role of contextual factors in the processes of theory appraisal and experiment appraisal. Brewster's temporary victory in the debate on the "polarity of light", for example, was very unusual, considering that the undulatory theory had become the orthodoxy in optics for almost a decade. Apparently, this debate was a cognitive event -- a test of the undulatory theory in terms of experimental facts. However, since the mid 1830s undulationists had developed a tactic to handle the attacks from their rivals. They tried to play down the value of the experimental work presented by emissionists, and sometimes even kept silent about them. These were exactly what happened when Brewster first raised questions about the "polarity of light". In fact, without Brewster's persistence and his reputation as the "father of experimental optics", the "polarity of light" would not have become an issue in the emission-undulatory controversy, leading to Brewster's temporary victory. Thus, observational and experimental facts did not automatically become objective evidence for testing a scientific theory. Certain contextual factors, like actors' personal characters and their tactics, both cognitive and rhetorical, were very important in determining the status of observational and experimental facts in scientific debates in the case of the "polarity of light".

We see here again that the debates initiated by emissionists' rear-guard actions during the late 1830s and the early 1840s demonstrated that the emission-undulatory controversy in nineteenth-century Britain cannot be interpreted solely in terms of cognitive factors. If we believe that the development of science is the result of the inevitable march of theories with cognitive superiority, then a number of features of the debates we documented in this chapter can hardly be understood. For example, it would be difficult to accept that the controversy between rival theories could extend long after the cognitive superiority of one of them became obvious according to the modern standard, that scientists who accepted a cognitively superior theory might not necessarily win in a scientific debate, and that scientists who accepted a cognitively inferior theory could continue to make contribution to the development of science. Without considering these overlooked details of the real history, however, the development of optics, especially the advancement of the undulatory theory in Britain around the mid-century would become incomprehensible.

Chapter 6

The Undulationists' Setback

The persistent criticism from emissionists between the late 1830s and the early 1840s had a great impact on the undulatory theory. Undulationists had to accept rivals' challenges, working hard to solve the problems exposed by emissionists. They soon found that some of the problems were solvable, such as the one about the so-called "polarity of light". In solving these problems, undulationists revised their theory, and significantly improved its explanatory power. But they also found that some of the problems exposed by the rivals were not immediately solvable, especially those related to the physical foundation of the theory, namely, the constitution of the ether. Facing these intractable problems, the majority of undulationists believed that their failures in handling them were temporary, and that they would soon come up with the solutions. But some undulationists conceded that some of the problems exposed by the rivals were in principle unsolvable within the framework of the undulatory theory. These skeptics generated considerable doubts about the undulatory theory. They began to question the fundamental doctrines, the methodological principles, and the research style associated with the theory, attempting to find what was responsible for the theory's failures. All of this doubt, suspicion, and skepticism among undulationists represented a setback in the development of the undulatory theory.

In this chapter I document the doubts, suspicions, and skepticism of three undulationists regarding their theory during the period of the 1840s. The first wavering undulationist I discuss was James MacCullagh, who publicly expressed his skepticism about the physical foundation of the undulatory theory at the 1842 British Association meeting. The second one was John Herschel, who cast doubt on the orthodox status of the undulatory theory at the 1845 British Association meeting. The last one was Robert Moon, who published a series of articles and books in the late 1840s, openly criticizing Fresnel's undulatory theory. In the following sections, I examine the factors that generated these doubts. In particular, I concentrate on the question how both cognitive and contextual, including historical and social, factors were responsible for the wavering of these undulationists.

1. MacCullagh on the Problems of the Undulatory Theory

MacCullagh's doubts on the undulatory theory resulted both from emissionists' criticisms and from his own exploration of the physical foundation of the theory. In this section, I first review the debate between Brewster and MacCullagh on crystalline reflection, showing how Brewster compelled MacCullagh to admit the explanatory failures of the undulatory theory. I also describe MacCullagh's own research on the physical foundation of the undulatory theory, and how his lack of success generated his crisis of confidence in the theory.

1.1 Reflection from Crystallized Surfaces

MacCullagh (1809-1847) was born in County Tyrone, Ireland. He entered Trinity College, Dublin at the age of 15, became a Fellow of the college in 1831, and chaired the professorship of Natural Philosophy in 1835. In the early nineteenth century, mathematical education in Trinity College was deeply shaped by the French analytical tradition. With an education from Trinity, MacCullagh completely endorsed Frensel's undulatory theory in his early optical research. His earliest optical works were two papers published in 1831, one on polarization (1831a) and the other on double refraction (1831b). In these papers, he made an contribution to the undulatory theory by developing an improved method for constructing the wave surface. However, MacCullagh's faith on the undulatory theory became wavering when he faced Brewster's challenge.

In a series of experiments conducted in 1819 on the actions of crystalline surfaces upon light, Brewster made a surprising finding. On the reflecting surface of Iceland spar he dropped a little cassia oil, a fluid with a refractive index nearly equal to the ordinary refractive index of the spar. When a beam of unpolarized light, incident at about 45°, was reflected at the surface separating the oil and the spar, Brewster observed that the polarizing angle of the reflected light remained the same when the reflection plane was turned half around relative to the crystal, from 0° to 180°. This discovery was extraordinary because in the case of reflection from the surface of Iceland spar the polarizing angles of the reflected light would be different when the reflection plane was turned half around. Brewster reported his discovery in a paper published in the *Philosophical Transactions* (Brewster 1819, pp.152-5). But Brewster's finding did not draw anyone's attention until more then two decades later when MacCullagh took up the subject of reflection from crystalline surfaces.

When MacCullagh in 1835 reviewed Brewster's finding, he at first was disappointed, because it was inconsistent with his anticipation drawn from the undulatory theory. Soon, however, MacCullagh found that Brewster's finding could be explained if he made an essential change in Fresnel's account of double refraction. In his theory of double refraction, Fresnel assumed that the vibrations of the wave were perpendicular to its plane of polarization. But MacCullagh found that, in order to explain Brewster's result, it was necessary to assume that the vibrations of the wave were parallel rather than perpendicular to its plane of polarization, an idea exactly opposite to Fresnel's assumption. Starting from this new assumption, MacCullagh adopted a revised version of the wave equation developed by Cauchy in his first theory, in which Cauchy assumed that the vibrations of the wave were parallel to the plane of polarization. After a complicated derivation from the wave equation, MacCullagh was able to prove that the formula for the polarizing angle contained only even powers of the sine or cosine of the azimuth of the reflection plane, and therefore a change of 180° in the azimuth would not affect the polarizing angle. So MacCullagh confidently announced that "[Brewster's] observation is now a result of [the undulatory] theory." (MacCullagh 1835, pp.76-81).

MacCullagh's successful explanation made Brewster anxious to extend his experiments on reflection from crystalline surfaces. Brewster at first wanted to use several fine specimens of natural crystals at the British Museum, including specimens of arragonite and quartz. But later he found that there was an act of parliament against even "the dust of a crystal" leaving the Museum. As a last resort, Brewster decided to use crystals with artificially polished surfaces. The experimental design was similar to that in the 1819 experiment, namely, that a beam of unpolarized light was reflected at the surface separating a crystal and some kind of fluid. In the course of the experiment, Brewster tried several different fluids. On one occasion, he used a mixture of olive oil and oil of cloves, dropping it on the surface of a crystal. Brewster was surprised to observe that the surface separating the crystal and the fluid did not affect the reflected light, namely, that a beam of unpolarized light did not become polarized after reflection from the surface. This finding was obviously extraordinary, contradicting all the known facts with respect to reflection from crystalline surfaces. Brewster reported his new discovery to the 1836 British Association meeting, and claimed that his new discovery "was inexplicable upon any theory of light whatever" (Brewster 1836).

After hearing Brewster's discovery, the first reaction from Hamilton was that MacCullagh's theory of crystalline reflection should be able to handle this anomaly. Hamilton thus urged MacCullagh to give an explanation of the extraordinary phenomenon by his "strengthful and fruitful theory".[1] Contrary to Hamilton's anticipation, however,

[1] *Athemaeum*, 462 (1836):630.

MacCullagh soon found that Brewster's new discovery was really inexplicable. In a paper published in 1837, titled "On the laws of crystalline reflection and refraction", MacCullagh explained why neither his own theory of crystalline reflection nor any current version of the undulatory theory was able to account for Brewster's new finding. According to MacCullagh, Brewster's new finding in 1836 actually belonged to an entirely different class of phenomena, because he had used artificially polished surfaces. "The process of artificial polishing must necessarily occasion small inequalities, by exposing little elementary rhombs with their faces inclined to the general surfaces; and the action of these faces may produce the unsymmetrical effects which Brewster notices as so extraordinary" (MacCullagh 1837, p.135). Since the physical properties of artificially polished surfaces were essentially different from those of natural surfaces, MacCullagh asserted that Brewster's new findings "cannot be so explained [by his theory], nor ought we to expect that they should" (*Ibid.*).

Brewster was not satisfied with MacCullagh's explanation, because he wanted to claim that the undulatory theory was incapable of explaining not only the cases with artificially polished surfaces, but also those with natural surfaces. It took six years for Brewster to develop the experimental evidence that enabled him to make this point openly. At the 1842 British Association meeting, Brewster presented a paper, titled "On crystalline reflection", in which he provided more evidence to prove the incompetence of the undulatory theory in cases of crystalline reflection. According to Brewster, MacCullagh's appeal to the differences between natural surfaces and artificially polished surfaces was unfounded. Brewster reported that "I have obtained exactly the same results in using natural faces, and in artificial ones, and especially on planes perpendicular to the axis of the crystal, where I have found the same results with the natural faces of the *Chaux carbonatée basée* of Hauy, and with those produced by artificial grinding" (1842b, p.14). Furthermore, Brewster noted that, by mere friction of his finger, he could produce elementary rhombs on the natural surfaces, a feature that was identical with the one in artificially polished surfaces. But the reflections from these surfaces with rhombs were no different from those at natural surfaces without rhombs. Thus, MacCullagh's view on the differences between natural and artificial polished surfaces was mistaken. Based on these new discoveries, Brewster believed that he held the trump finally to defeat MacCullagh. He claimed:

> I have no doubt, that Prof. MacCullagh will concur in the accuracy of these views, and, with that candour which distinguishes him, will acknowledge, as he has almost done already in the preceding note, that the undulatory theory is, generally speaking, incapable of explaining the phenomena of crystalline reflection. (*Ibid.*)

In fact, Brewster's argument was not very compelling. He did not provide any details of his experimental arrangements, nor the peculiarities of the crystals he actually used. For one who was competent in experimental optics, there were many ways to challenge Brewster's conclusion by questioning the reliability of his experimental results. However, MacCullagh did not do so, perhaps because he was not an experimental physicist and perhaps because Brewster was the "father of experimental optics". Brewster's high prestige in optical experiments might have made it difficult for MacCullagh to question the reliability of his experimental findings.

Facing Brewster's criticism, MacCullagh decided to admit the failure of the undulatory theory in explaining crystalline reflection. He openly stated that he had changed his opinion on the subject and adopted "pretty similar views to those entertained by Sir D. Brewster".[2] MacCullagh admitted that the undulatory theory failed to explain crystalline reflection with any mathematical precision. In addition to the problems that Brewster had raised, MacCullagh remarked that when reflection took place on the separate surface between a crystal and air, whose refractive indices were very different,

[2] *Athenaeum*, 770 (1842):687.

the results were also at odds with the undulatory theory. Thus, MacCullagh agreed with Brewster that the undulatory theory was unable to deal with the phenomena of crystalline reflection. Moreover, since "the theory of crystalline reflection laid down comprehended all the phenomena," MacCullagh even claimed that "the undulatory theory [was] unable to grasp more than the grossest phenomena of light."[3] Now the problem was not only the undulatory theory's ability in explaining crystalline reflection, but its general merit in the whole field of optics. Indeed, MacCullagh was under no compulsion to raise general questions about the undulatory theory, because Brewster's experimental findings did not cast doubt on the general merit of the theory. MacCullagh's statement clearly indicated that his faith on the undulatory theory was really wavering at the moment.[4]

1.2 The Physical Foundation of the Undulatory Theory

At the very beginning of his study of crystalline reflection, MacCullagh had realized that some fundamental hypotheses of the undulatory theory had to be changed in order to explain reflection from crystalline surfaces. In his theory of double refraction, Fresnel assumed that, in the reflection of ordinary rays, 1) the vibrations of the wave were perpendicular to the plane of polarization; 2) the density of the ether was a function of the refractive index of the medium; 3) the *vis viva* of the wave was preserved; and 4) only the vibrations parallel to the reflective surface were continuous. In his 1837 paper on the laws of crystalline reflection, however, MacCullagh presented a different group of hypotheses about the physical properties of the ether, which were necessary in order to explain crystalline reflection. These assumptions were 1) the

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[3] *Literary Gazette*, 1332 (1842):538.

[4] Brewster was satisfied with MacCullagh's reply and believed that MacCullagh would continue the study on crystalline reflection with an "unbiased" attitude. So later Brewster gave up his inquiry on crystalline reflection and left the subject in MacCullagh's hands. See Brewster to Stokes, (March 8, 1861), in Larmor (1971, pp.153-4).

vibrations of the wave (or the ethereal motion) were parallel to the plane of polarization; 2) the density of the ether was the same in all bodies as in vacuum; 3) the *vis viva* of the wave was preserved at interfaces; and 4) the vibrations were continuous across interfaces (MacCullagh 1837, p.91). These hypotheses, except the third about the preservation of *vis viva*, contradicted those proposed by Fresnel.

MacCullagh's new hypotheses were initially successful, because he could explain Brewster's 1819 experimental finding by means of these new assumptions about the physical properties of the ether. However, these hypotheses were highly problematic, because they opposed the received notions that the ethereal molecules were strongly attracted or repelled by the particles of bodies, and that the density of the ether varied from one medium to another. MacCullagh thus conceded:

> We are obliged to confess that, with the exception of the law of *vis viva*, the hypotheses are nothing more than fortunate conjectures. These conjectures are very probably right, since they have led to elegant laws which are fully borne out by experiments; but this is all that we can assert respecting them. (1837, p.129)

Here MacCullagh clearly believed that the legitimacy of the hypotheses should not be merely built upon explanatory successes. To be deduced from some kind of first principles was a more direct and more powerful evidence. But it was impossible to deduce these hypotheses from first principles, because, in the field of optics, such principles were still lacking. Within the framework of the undulatory theory, such questions as the constitution of the ether and the connections of the ether with the particles of bodies, in the opinion of MacCullagh, were totally unknown. As a proof of this ignorance, MacCullagh listed a series of the simplest and most familiar phenomena that the theory had never been able to explain rigorously, including dispersion, ordinary refraction, and double refraction. MacCullagh concluded that the knowledge provided by the undulatory theory was "confined to the laws of phenomena: scarcely any approach has been made to a mechanical theory of those laws" (*Ibid.*, p.130).

The discontent with the physical foundation of the undulatory theory constituted a motive for MacCullagh to develop a dynamical theory for reflection and refraction, which he published in a long paper in the 1839 *Transactions of the Royal Irish Academy*.[5] MacCullagh's purpose in this paper was to look for the dynamical principles that could mechanically explain crystalline reflection and refraction. His strategies were first to derive a potential function for the ether from a mechanical structure, and then to derive a wave equation by means of the rules of analytical mechanics given by Lagrange (MacCullagh 1839, p.148). The second step, namely, moving from the potential function to the wave equation, was very complicated but possible. The first step, that is, the determination of the potential function, obviously depended upon assumptions about the nature of the ether and involved formidable difficulties. After a complicated analysis, MacCullagh finally realized that it was impossible to derive the potential function from a complete mechanical structure without any speculation. He conceded:

> In this theory, everything depends on the form of the [potential] function V; and we have seen that, when the form is properly assigned, the laws by which crystals act upon light are included in the general equation of dynamics. . . . But the reasoning which has been used to account for the form of the function is indirect, and cannot be regarded as sufficient, in a mechanical point of view. It is, however, the only kind of reasoning which we are able to employ, as the constitution of the luminiferous medium is entirely unknown. (1839, p.184)

Several years later MacCullagh stated the same point, and further asserted that not only his approach but all other versions of the undulatory theory were doomed to fail in searching for a mechanical theory. In a letter to Herschel in 1846, MacCullagh wrote:

> I have thought a good deal (as you may suppose) on the subject - but have not succeeded in acquiring any *definite mechanical* conception - i.e. such a conception as would lead directly to the form of my function V, and would of course include the actual laws of the phenomena. One thing only I am persuaded of, that the constitution of the ether if it ever should be discovered, will be found

[5] The title of the paper is "An essay toward a dynamical theory of crystalline reflection and refraction".

to be quite different from any thing that we are in the habit of conceiving, though at the same time very simple and very beautiful. An elastic medium composed of points acting on each other in the way supposed by Poisson and others will not answer.[6]

The failures to establish a physical basis for explaining crystalline reflection within the undulatory framework initiated a crisis of MacCullagh's confidence, casting doubt on the validity of the undulatory theory. MacCullagh publicly expressed his skepticism about the physical foundation of the undulatory theory at the 1842 British Association meeting. After listening to Brewster's remark on the "polarity of light", MacCullagh stated that the incapability of the theory to explain a number of optical phenomena stemmed from its imperfect physical foundation. According to MacCullagh, the undulatory theory was not based upon any physical law.[7] For this reason, he said that he lacked faith in Fresnel's theory of refraction, which was "discovered by some kind of mathematical deduction, and then explained by principles invented for the purpose". With respect to Cauchy's undulatory theory, MacCullagh indicated that its physical assumption, the symmetrically arranged ether, contradicted the well-known fact of elliptical polarization.[8] Thus, MacCullagh concluded that, "as we knew nothing absolutely of the undulatory theory (though . . . it had explained many things in a very beautiful way), yet, being entirely without physical foundation, we could know it but

[6] MacCullagh to Herschel, (October 2, 1846), Royal Society Library, Herschel Papers, HS. 12.11.

[7] There was a disagreement between MacCullagh and Lloyd on whether the principle of transverse vibration was a physical law. MacCullagh insisted that this was rather a mathematical principle.

[8] According to MacCullagh, if we assumed, as Cauchy did, a perfect optical symmetry all round the axes of a crystal, it would be hard to conceive that the vibrations of a wave in the crystal were in the form of an ellipse, because there was nothing to determine the position and the ratio of the axes of the ellipse (MacCullagh 1841, p.196).

very imperfectly."[9]

MacCullagh continued this critical attitude toward the undulatory theory in the last few years of his life. In 1843, at the British Association meeting, he once again openly expressed his discontent with the physical foundation of the undulatory theory. In a comment on Powell's paper about elliptic polarization in metallic reflection, MacCullagh pointed out that the phenomenon of metallic reflection was still incomprehensible within the undulatory framework. The failure of the undulatory theory in this field also lay in its physical assumptions: "the cause of the peculiar action of metallic bodies upon light seemed to be involved in mystery".[10]

2. Herschel on the Fate of the Emission Theory

Herschel's doubts on the orthodox status of the undulatory theory were mainly the results of his own scientific researches. In this section, I first review Herschel's discoveries in photography and photochemistry between the late 1830s and the early 1840s, showing how he found failures of the undulatory theory in these fields. I then portray a dramatic event at the 1845 British Association meeting, in which Herschel questioned the orthodoxy of the undulatory theory by openly praising the emission theory.

2.1 Photographic Pictures of the Solar Spectrum

After John Herschel presented his work on absorption in the summer of 1833, he left the field of optics for a while. In November of 1833, he moved to Cape Town, where he could extend his astronomical survey to the southern hemisphere. Herschel stayed in Cape Town for almost five years, and concentrated on astronomical research.

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[9] *Literary Gazette*, 1332 (1842):534.

[10] *Athenaeum*, 826 (1843):773.

After he came back Britain in March 1838, however, he quickly resumed his optical interest. This time, his focus was not on physical optics, but on a new-born field - photography.

As early as 1819, Herschel had made a discovery that later proved to be very important for photography. In that year, he found that sodium thiosulphate had the property of dissolving silver salts; he soon published this finding in a scientific journal (Herschel 1819). But Herschel did not realize the possible application of sodium thiosulphate as a fixing agent in photographic processes, so that his discovery did not draw attention from those people who were conducting photographic experiments.[11]

In January 1839, a Frenchman, Louis Daguerre, announced a new method of producing photographic pictures. He was able to produce sharp images on iodized silver plates, developed by mercury vapor and fixed by solutions of common salt. In Britain, Talbot in the same month also presented to the Royal Society a new process of making photographic pictures on silver-chloride paper. Daguerre's and Talbot's discoveries immediately drew Herschel's attention. He quickly realized the significance of his own finding of the peculiar property of sodium thiosulphate, which could be used as a more effective fixing agent in both Daguerre's and Talbot's photographic processes. Herschel then began to conduct his own photographic experiments, in which he coated sensitized paper with silver-carbonate solution, employed an achromatic lens to produce sharp images, and used sodium thiosulphate as a fixing agent. He presented his experimental

[11] In the 1810s Humphry Davy conducted a series of experiments to produce photographic pictures on paper coated with silver-nitrate solution. He succeeded in producing photographic pictures, but failed to fix them for a long period. Davy knew nothing about Herschel's discovery although they had close contact. See Buttmann (1970, pp.131-2).

results, 23 photographic pictures, to the Royal Society in March 1839.[12]

After spending some time studying the practical aspects of photography, Herschel turned his attention to theory, focusing on the cause rather than the effect of photographic processes. Among Herschel's theoretical works on photography, the most interesting one was his study of the chemical reactions induced by the rays of light from various sections of the solar spectrum.

In the summer of 1839, Herschel succeeded in producing a photographic picture of the solar spectrum, not in black and white, but in its natural colors. He immediately reported his discovery to the British Association, describing his picture of the solar spectrum in detail. The first extraordinary feature of this picture of the spectrum was its length, which was almost double of the length of the luminous spectrum (Figure 6.1). Clearly, the chemically active portion of the solar spectrum extended far beyond both the visible red and violet end (Herschel 1839b, p.10).

Another extraordinary feature, Herschel noted, was a very special function of the red rays in the spectrum. In Figure 6.1, the portion $\alpha\gamma$ is white, its middle corresponding to the red end of the luminous spectrum; $\gamma\delta$ is red; $\delta\epsilon$ green; $\epsilon\zeta$ blue; and $\zeta\eta$ black. According to Herschel, the peculiar thing in this picture was that those portions where the red rays had fallen were white, although the whole picture had a faint brownish background because of scattered sunlight falling on it during the exposure. Thus, the red rays had induced a very peculiar chemical reaction on the sensitized paper. Instead of blackening the paper like other rays, the red rays whitened it. Herschel

[12] The paper was entitled "Note on the art of photography or the application of the chemical rays of light to the purposes of pictorial representation". On April 25, 1839, Herschel withdrew the paper from publication. An abstract of this paper was printed in the *Proceedings of the Royal Society*, see Herschel (1839a). A complete version of this paper is transcribed by Schaaf and appears as an appendix in his 1979 paper. Schaaf also provides an interpretation of the reason for Herschel to withdraw this paper, see Schaaf (1979).

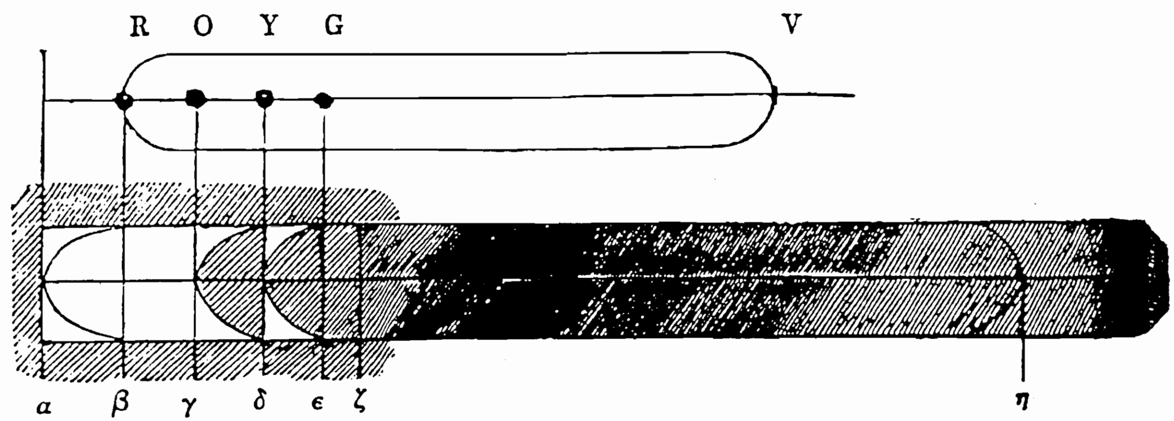

Figure 6.1 Herschel's Photographic Picture of Solar Spectrum
[From Herschel 1839, p.10]

described this special function of the red rays as follows:

> [T]he extreme red rays, . . . not only have no tendency to darken the prepared paper, but usually exert a contrary influence, and *preserve* the whiteness of paper on which they are received when exposed at the same time to the action of a dispersed light sufficient of itself to produce a considerable impression. (*Ibid.* p.9; original emphasis)

Herschel also investigated the chemical reaction induced by the combination of the red rays and other rays. Using a special apparatus, Herschel could superpose the less refrangible rays of the spectrum, from the green to the red, on the image of the more refrangible rays, from the blue to the violet. He surprisingly found that "the blackening power of the more refrangible rays seemed to be suspended over all that portion on which the less refrangible fell" (1840, p.24). The whitening function of the red rays was so powerful that it could weaken the effects created by other rays. This special function of the red rays later came to be known as the "Herschel effect".

From this peculiar effect Herschel drew a conclusion at odds with the received undulatory theory of light. According to the undulatory theory, when two rays with different refrangibilities, and therefore different wave-lengths, interacted with each other, they should produce an effect that neither of them acting separately was able to produce, because of the effect of interference. But in the "Herschel effect", the combined effect was almost the same as the one produced by the red rays alone. This fact, according to Herschel, was "so totally at variance with all our ideas", and that it "seems difficult to deal with on any theory of light" (Herschel 1840, p.17). Herschel concluded that the degree of photographic or chemical effectiveness of any given ray from the solar spectrum was by no means merely a function of the refrangibility or the wavelength of the ray. Rather, Herschel insisted that the photographic effectiveness of any ray depended on other physical properties of the ray, such as a "deoxidizing power" in the red rays that was responsible for its powerful whitening function (Herschel 1839b, p.11).

2.2 The Physical Causes of Photographic Processes

At the 1842 British Association meeting, a new discovery in photography made by Ludwig Moser, professor of physics at Konigsberg, drew the attention of the participants. In his experiments Moser placed a black plate, either of horn or of agate, on a well-polished surface of silver for ten minutes. Then he found that the surface of silver received an impression of the figure of the black plate, which could become visible by exposing the silver plates to vapor either of water or of mercury, or even by a breath. Moser then used iodized silver plates in his experiments, the sensitized material that Daguerre used, and found the same results. He also reported that these experiments succeeded just as well in complete darkness, or during the night, as under the influence of light (Lerebours 1843, pp.171-2). From these experiments, Moser concluded that all materials radiated light, but, that without appropriate conditions, the light would be latent. When a surface of silver, or other materials, was touched by an object, the touched parts of the surface acquired the property of condensing the vapors of the substances. Then, the condensed vapor, in a liquid state, liberated the latent light and made it visible (*Ibid.*, pp.177-8).

Moser's discovery of "latent light" initiated a lively discussion in section A of the 1842 British Association meeting. Brewster responded enthusiastically to Moser's discovery, because he thought he could use it as new evidence to criticize the undulatory theory. According to Brewster, if latent light was an actual physical fact, then it might imply that light took its residence permanently in a body, and, in the state of latency, it stood still and had no impact on the ether in any way. This implication was entirely incompatible with the undulatory theory, which assumed that light was some kind of motion. If light was really a wave motion, Brewster asked, did the motion extinguish in the state of latency? and, if so, how could it return to motion again? Brewster said that he could not conceive how the phenomenon of latent light could possibly be explained in terms of the undulatory theory. On the contrary, the same phenomenon would be easily understood in terms of the emission theory. According to Brewster, the idea of latent light corresponded with Newton's opinion that the particles of light might be caught by the force of attraction and retained when they entered into bodies. Brewster's opinion on a possible emission account of latent light was supported by MacCullagh in the discussion.[13]

Herschel was also enthusiastic about the discovery of latent light. He exhibited to the British Association meeting another way of demonstrating the existence of latent light. He used a specially prepared paper, which, when first examined had no picture on it. After he held the paper in strong light a red picture became visible. This demonstration showed, Herschel believed, that the impression of some colors could turn from latent to permanent by the action of light.[14] Herschel also agreed that the phenomenon of latent light was so extraordinary that no current theory could easily explain it. But he disagreed with Brewster on the use of this phenomenon as a test of the rival theories of light. He thought it "a pity to encumber this new and extensive field of discovery . . . by any speculations connected with the theory, either of undulations or emissions".[15] Instead of appealing to any current theory, Herschel put forward a bold suggestion, that latent light might actually be some kind of thermal effect.[16]

Herschel explored his notion of thermal effect in two later papers published in 1843. According to Herschel, there were actually three different kinds of rays involved in photographic processes. They were luminous rays, chemical rays, and one, just

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[13] *Athenaeum*, 770 (1842):687; *Literary Gazette*, 1332 (1842):539.

[14] *Athenaeum*, 770 (1842):687.

[15] *Athenaeum*, 770 (1842):687.

[16] *Literary Gazette*, 1331 (1842):464.

identified by him but still unknown by most people, called parathermic rays. These parathermic rays, Herschel said, were invisible, distinguishing them from luminous rays. They were "the rays which radiated from molecule to molecule in the interior of bodies" (Herschel 1843a, p.5). Moreover, these rays resided in the less refrangible portion, the red end, of the solar spectrum, but possessed chemical properties that purely calorific rays did not have (Herschel 1843b, p.132). These parathermic rays, Herschel believed, were responsible for most extraordinary phenomena in photographic processes. For example, in Moser's experiments on latent light, it was parathermic rays that were "active in producing those singular molecular affections which determine the precipitation of vapors" (Herschel 1843a, p.5).

The introduction of the concept of parathermic rays and the emphasis on the differences between parathermic rays and luminous rays clearly indicated that Herschel had separated heat from light and regarded them as distinguished entities. This position was incompatible with the undulatory tradition. According to the undulatory theory, heat was also a kind of undulation, which differed from light only in the frequency of its vibrations. The identity of heat and light, first advocated by Young and then further supported by Forbes and Powell, was one of the important discoveries of the undulatory theory. But Herschel here entirely gave up the identity of heat and light, and regarded heat as an independent entity, which, in effect, was the position adopted by his father William Herschel, a committed emissionist (Lovell, 1968). Thus, although Herschel did not agree in using the discovery of latent light to question the undulatory theory or to support the emission theory, his interpretation of latent light had become distant from the undulatory tradition and close to the emission framework.

2.3 The Fate of the Emission Theory

The assumption of an independent existence for heat only represented Herschel's implicit wavering in his faith in the undulatory theory. Two years later, however, Herschel publicly expressed his doubt on the orthodox status of the undulatory theory at the 1845 British Association meeting. This dramatic event happened immediately after Powell read a paper on the elliptic polarization of light by reflection from metallic surfaces (Powell 1845). In this paper, Powell reported a series of observations on changes of the polarizing plane in reflections with different angles of incidence and from various metals. Powell found that his experiments were consistent with MacCullagh's empirical formula about metallic reflection, but the change of polarizing plane could not yet be explained by the undulatory theory.

Herschel was obviously discontent with Powell's work, and even with the whole undulatory theory, in handling the problems of elliptic polarization. Immediately after Power's report, Herschel threw out an opinion, which he had advocated 15 years ago in his *Preliminary Discourse*, that, "if the same amount of analytic skill had been expended upon the corpuscular [emission] theory, perhaps more could be done with it than was at present believed". Herschel said that, as in the case of the undulatory theory, physicists might also be able to put forward a few assumptions about the physical conditions of the particles of light within the emission framework, and then to develop equations and explanations about optical phenomena. For example, as the undulatory theory assumed different elasticities of media, the emission theory could assume different attractions between particles. Or, the emission theory could assume that the individual particles of light might have poles, and be thrown off from the luminous body with revolving as well as progressive motions. While these motions were not interfered with, the original arrangement of the poles of the particles would remain unchanged. But if the particles interacted with media, which could change the arrangement of the poles, a new physical property, such as polarization or elliptic polarization, might appear in the beam of light. Herschel admitted that he had not worked out any details of an emission account as complete as those given by the undulatory theory, but he insisted that, by appealing to the polarity, attraction, repulsion, and rotation of the particles of light, the emission theory might be able to explain more optical phenomena than it did at present.[17]

Herschel's remark on the fate of the emission theory resulted in a strong reaction from Airy, one of the most faithful undulationists. Airy clearly disagreed with Herschel, but, probably because of Herschel's high prestige, he did not simply denounce Herschel's opinion as false. Instead, Airy politely asked Herschel to give a complete account of his emission theory, which he knew Herschel was unwilling and unable to do. Airy then restated his faith in the undulatory theory, claiming that the theory was superior to its rival both in explanatory power and in simplicity. Airy concluded that the undulatory theory, as a geometrical theory, was perfect, although it was imperfect as a mechanical theory. Airy's conclusion however was criticized by Challis, who insisted that any explanation or any theory, if it was only geometrical or mathematical and without a mechanical foundation, was bound to be imperfect.[18]

The most interest reaction to Herschel's remark on the emission theory came from the *Literary Gazette*. Writing in one of the few weekly journals that gave detailed reports of the British Association meetings, the reporter from the *Literary Gazette* described the disagreement between Herschel and Airy as follows:

> The principal topics brought forward in Section A were magnetism, light, and rain. The discussion upon the corpuscular and undulatory theories of light, the former upheld by Sir John Herschel, and the latter by the Astronomer Royal [Airy], formed the chief interest.[19]

In the eyes of the reporter, Herschel was representing the emission theory and had become an opponent of the undulatory theory in this episode. But this opinion did not

[17] *Athenaeum*, 922 (1845):640; *Literary Gazette*, 1484 (1845):416.

[18] *Literary Gazette*, 1484 (1845):416.

[19] *Literary Gazette*, 1483 (1845):389.

correctly reflect Herschel's point. In fact, Herschel did not convert to emissionism, neither did he regard himself as representing the emission theory. What he proposed was a possible emission account for some optical phenomena that the undulatory theory was not able to deal with. As I have described in chapter two, Herschel adopted an "instrumentalist" position on hypotheses, conceptualizing them as "scaffolds" for approaching the general laws of the nature. Thus, he would not insist on one particular theory or hypothesis when he found it incompetent. After he realized the problems that the undulatory theory faced in photography and in elliptic polarization, it was natural for him to look for a new intellectual tool that could provide reasonable accounts for these phenomena. At the same time, he did not deny the explanatory successes of the undulatory theory in other phenomena, nor did he convert to emissionism. The *Literary Gazette* report exaggerated the wavering of Herschel's faith in the undulatory theory, and gave its readers, especially those emissionists who were not present at the meeting, a distorted impression of the status of the emission-undulatory controversy. Some emissionists might even have developed an illusion from this report that their own theory was still promising.

3. The Revolt of Moon

Unlike MacCullagh and Herschel, Moon did not conduct any empirical study in the field of optics. As a Cambridge-trained physicist, Moon was a competent mathematical analyst. His criticism of the undulatory theory was based upon purely mathematical analysis. In this section, I first review Moon's analysis of double refraction, showing how he identified the problems in Fresnel's account. I then examine Moon's opinion of the general merits of the undulatory theory and his critical remarks on its research style - the emphasis on mathematical analysis.

3.1 Moon on Double Refraction

Robert Moon (1817-1889) was the second son of Richard Moon, a Liverpool merchant. At the age of 17 he entered Queens College, Cambridge, and graduated as eighth wrangler in 1838. Moon continued his scholarly career in Cambridge after his graduation. He was selected as a Fellow in 1839 and awarded a M.A. degree in 1841 (Venn 1951, p.448). Probably due to the influence of his classmate Richard Potter, who graduated from the same college in the same year as sixth wrangler, Moon became interested in physical optics and developed a very peculiar opinion on the undulatory theory that was rare among Cambridge physicists.

Moon never had doubts on the basic doctrine of the undulatory theory, that light was vibrations rather than particles. Neither did he had any doubt on the undulatory accounts of a series of important optical phenomena. He clearly stated:

> I still admit and profess my belief in the truth of the undulatory theory of reflexion and refraction, of the explanations of the interesting experiments of the two mirrors and the prism of a small refracting angle, and of that admirable portion of the theory which relates to the phenomena observed in the shadows of thin fibres and the colours of Newton's rings. (Moon 1849, p.8)

Although Moon always regarded himself as an undulationist and did not give up the fundamental doctrines of the undulatory theory, he adopted a very critical attitude toward Fresnel's works. Beginning in 1844, Moon published a series of papers in the *Philosophical Magazine*, including one on diffraction and two on double refraction, openly criticizing Fresnel's version of the undulatory theory. Moon's papers initiated strong reactions from the undulatory camp, condemning his revolt. At the same time Moon earned sympathy and supports from emissionists, especially his classmate Potter.[20] Probably due to both his eccentric ideas and his argumentative style, the

[20] According to Moon, Potter rendered to him many valuable assistances when he was preparing the pamphlet, see Moon (1849, p.xvi).

Philosophical Magazine soon refused to continue publishing Moon's papers. In 1849 he published them in a separate pamphlet, titled *Fresnel and His Followers: A Criticism.* In this pamphlet Moon collected all of his papers on optics, in which he not only attacked Fresnel but also criticized most contemporary undulationists, including Airy, Powell, Stokes, Kelland, Green, MacCullagh, O'Brien, and Challis. By challenging almost every undulationist in Britain, Moon was simply declaring a war against the received undulatory theory.

Among the topics Moon discussed in the pamphlet, double refraction was the one that initiated a heated dispute. According to Fresnel, the phenomena of double refraction could be explained if a number of assumptions about the physical properties of the medium were adopted. Among these assumptions, one asserted the existence of the axes of elasticity in three directions at right angles to each other, along which the forces of restitution would act in the same direction of the displacement when a particle of the medium was displaced. Another necessary assumption for Fresnel's explanation of double refraction was the one about the effects of the restitution force in the direction perpendicular to the front of a wave. The force of restitution in any crystal could always be decomposed into two forces, one parallel and the other perpendicular to the wavefront. Fresnel assumed that the one perpendicular to the wavefront had no influence on the propagation of the vibrations and thus could be neglected. In Moon's opinion, however, these two assumptions in Fresnel's theory of double refraction were problematic and unacceptable.

For the assumption of the axes of elasticity, Moon pointed out that the force of restitution in crystals actually did not act in the same direction of displaced particles. He found that in their analyses of the subject both Fresnel and his followers such as Smith only considered the motion of one displaced particle and implicitly assumed other particles of the medium to be at rest. This was entirely fallacious, according to Moon, because any motion of one particle would necessarily cause the motions of the others. If the motions of other particles were taken into account, Moon proved that the direction of the restitution force at a given particle was relevant to the displacements of other particles. Even along the axes of elasticity, it was impossible that the restitution force could act in the same direction as the displacement of the particle (Moon 1845).

Moreover, Moon claimed that, since Fresnel and his followers assumed that the motions of surrounding particles had no effect upon a displaced particle, it followed that the properties of the displaced particle were entirely independent of the surrounding medium. Moon said:

> Thus, without some special interposition of Providence directed towards this particular particle, it will never move at all; and, once set in motion, it will vibrate for ever, . . . as might have been anticipated, as the other particles exercise no influence upon it, conversely it exerts none upon them: or, in a word, *so far from Fresnel's law of propagation being true, no motion whatever will be propagated.* (1849, p.41; original emphasis)

Fresnel's assumption concerning the axes of elasticity therefore implied that the propagation of waves in crystals was impossible.

For the assumption about the restitution force perpendicular to the wavefront, Moon indicated that "though for certain purposes it may be allowable to neglect the force perpendicular to the front, it must not be forgotten that it still *exists*" (*Ibid.*, p.43; original emphasis). The existence of such a perpendicular force, according to Moon, would cause a displaced particle to move away from the normal of the front at a given time and then to locate in a plane perpendicular to the front. If the axes of elasticity existed, Moon was able to prove by mathematical analysis that the perpendicular force could not keep the particle stably at the plane perpendicular to the front. "If at any moment the particle were actually moving in a plane perpendicular to the front, the next it would cease to do so" (*Ibid.*, p.45). This derivation directly contradicted a well-known fact that polarization occurred after double refraction, because polarization required such perpendicular vibrations. Thus Moon noted, Fresnel's assumptions on the perpendicular restitution force implied that "the polarization of the rays spoken of by Fresnel is entirely visionary" (*Ibid.*). From this unacceptable implication, plus the impossibility of propagation, Moon concluded that Fresnel's analysis of double refraction was "erroneous from beginning to end" and "his whole theory falls to the ground" (1845, p.538; 1849, P.45).

3.2 Moon on the General Merits of the Undulatory Theory

Moon's paper on double refraction drew quick responses from undulationists. In 1846, Archibald Smith sent a remark to the *Philosophical Magazine*, noting that what Moon criticized was not Fresnel's original idea, but an opinion that he added in his 1839 paper in order to fill a gap in Fresnel's original reasoning. Smith said that he had immediately corrected the mistake in the second edition of the journal that contained the paper (Smith 1846, p.48). Therefore, Smith implied that Moon was just attacking a straw man.

Another response was from an anonymous writer, who called himself "Jesuiticus". This anonymous writer published a paper in the *Philosophical Magazine* in 1846, scolding Moon for his skepticism of Fresnel's theory of double refraction. In this paper, Jesuiticus did not focus his attention on Moon's concrete arguments. Instead, he presented a general argument about how to evaluate the undulatory theory. According to Jesuiticus, any theory of light must be, to a considerable extent, imaginative. In order to appraisal these rival imaginings, the only legitimate criterion was the explanatory power of the theory. The "theory which can explain the greatest number of facts ought to claim the attention of the philosophers more than any other" (Jesuiticus 1846). In the field of double refraction, the explanatory power of the undulatory theory was extraordinary. It was able to explain a great number of phenomena both in biaxial and uniaxial crystals, not to mention the novel predictions of conical refraction made by

Hamilton. Because of these explanatory and predictive successes, Jesuiticus said, the undulatory theory had earned "great celebrity", and "ought to be regarded as a stupendous monument of human ingenuity" (*Ibid.*). In this argument, Jesuiticus clearly implied that, although Moon had identified some problems in the assumptions of Fresnel's theory of double refraction, this theory was still the best one as long as it could make successful explanations and predictions. In other words, Moon's critiques were inadequate and did not rock the undulatory account of double refraction.

Jesuiticus' attack forced Moon to a general discussion of the merits of the undulatory theory. In his reply to Jesuiticus, Moon first pointed out that Fresnel's theory of double refraction did not have the explanatory power Jesuiticus attributed to it. In the view of Moon, Fresnel's theory entirely failed to explain the separation of the ordinary and extraordinary rays, one of the fundamental features in double refraction. With respect to the predictions of conical refraction, Moon claimed that Jesuiticus was not fully aware of the recent discovery about the subject, which had shown that Hamilton's predictions of conical refraction were actually at odds with experiment. Here Moon referred to Potter's experimental study of conical refraction in 1841.

Probably at Moon's request, Potter sent a short note to the *Philosophical Magazine*, helping Moon to discredit Jesuiticus' critique. In this note Potter reminded the readers that he had published a paper in the same journal in 1841, titled "An examination of the phenomena of conical refraction in biaxial crystals". In this paper, Potter said, he had shown by accurate experiments that the peculiar refraction near the optic axes of biaxial crystals was at odd with the analytical predictions that Hamilton drew from Fresnel's wave equation in biaxial crystals. Then Potter wrote:

> The anonymous correspondent "Jesuiticus", in the last No., refers to those analytical researches triumphantly in favour of the undulatory theory of light. I do not write to disturb the philosophical opinions of "Jesuiticus," but to remind the readers of the Magazine where they will find the discussion of the points referred to. (1846, p.39)

It is understandable why Potter did not want to be involved in the debate between Moon and Jesuiticus, because it was one between undulationists. But it was clear that he definitely wanted Moon to be the winner, a situation that would benefit emissionists.

In addition to arguing that Fresnel's theory lacked great explanatory power with respect to the phenomena of double refraction, Moon also challenged the methodological norm Jesuiticus proposed, namely, that explanatory power was the central criterion for evaluating a theory. Moon held that high explanatory power could certainly give credibility to a theory, but some portion of its credibility also depended upon its "antecedent probability", because "it may happen that a theory may be contrived so fantastical as to require just as much explanation as the phenomena it was intended to elucidate" (Moon 1849, p.31). The "antecedent probability" here clearly referred to the degree of the conceptual clarity or explicitness of a theory. Containing vague or unclear concepts would decrease the credibility of a theory, no matter how many successful explanations or predictions it could make. Fresnel's undulatory theory, in Moon's view, had fallen into this predicament, namely, that it suffered a low antecedent probability although it had a relatively high explanatory power.

For one salient symptom of a low antecedent probability for the undulatory theory, Moon pinpointed the ether problem. He asked his readers to look at the different theories on the constitution of the ether, including those proposed by Fresnel, Cauchy, Kelland, Tovey, O'Brien, Challis, Green, MacCullagh, and Stokes. The proponents of these ether theories believed that they had uncovered the physical causes of optical phenomena, but the ether theories they invented were so inconsistent with each other that no one knew the real causes of optical phenomena. Moon made the following comment:

> [I]f I say we look at these several theories, the reflection cannot but suggest itself that those must be very odd principles of investigation which from the same results lead to such opposite causes. From the powerful machinery they have brought to bear upon the subject, one might suppose it was the object of these gentlemen to invent light rather than to discover the means already devised and

> put in action for the accomplishment of that great phenomenon: nor can we be surprised if with such an aim the production of each optical Frankenstein should be some frightful monster. (1849, pp.ix-x).

Hence, although these ether theories could explain some optical phenomena, according to Moon, the inconsistencies among them might require more explanations than those they intended to provide. The low antecedent probability stemming from the ether problem discredited the undulatory theory, no matter how many explanations or predictions it could make.

Moon's critiques of the undulatory theory, and especially its mechanical basis, finally resulted in a reaction from Airy, one of the most prestigious undulationists at the time. In a paper published in the *Philosophical Magazine* in 1846, Airy counterattacked Moon's critiques of the ether problem. In the paper, Airy insisted that, although there were many different theories of ether, they all had the same fundamental idea, namely, that waves propagated in accordance with mechanical laws applying to the attractive or repulsive interactions of the ether. In terms of this basic idea, the undulatory theory had been able to explain such optical phenomena as dispersion and double refraction mechanically. Airy admitted that none of the current ether theories was perfect and he did not prefer any one of them, but he claimed that "the investigation and publication of these mechanical theories have been advantageous to the science by showing that mechanical laws *may be able* to explain effects never before ascribed to mechanical laws" (1846, p.469; original emphasis).

Airy's defence did not satisfy Moon. In a reply to Airy that was published in *Philosophical Magazine* in 1847, Moon seized on Airy's confession of the imperfections of the current ether theories and caste doubt on the undulatory theory's ability to solve the ether problem. Moon noted that Airy had recognized the ether problem as early as 15 years ago in his second edition of the *Tracts*. But after 15 years of continuous endeavour, Moon said, undulationists had achieved no substantial progress toward "a just appreciation of Fresnel's theory": Airy's judgment of the status of the ether theories in 1846 was almost exactly the same as the one he made 15 years ago (Moon 1849, p.98). Thus, Moon concluded that Airy's arguments defending ether theories were only rhetorical, and the progress that Airy attributed to the undulatory theory in solving the ether problem was only apparent.

In addition to questioning the mechanical or physical foundation of the undulatory theory, Moon further questioned the methodology associated with the theory. He believed that the methods employed by undulationists entirely violated the legitimate methodology, which he took to be the Baconian philosophy. "[I]t would be difficult to conceive a more thorough repudiation of the principles of Bacon than is exhibited by the disciples of the Fresnelian Theory of Optics" (Moon 1849, p.x). The major problem in the methods of the undulatory theory was its divorcing mathematical analysis from physical study. Moon held that the advancements of science in 1830s and 1840s Britain were achieved in the hands of physicists rather than mathematicians. But the Fresnelian undulationists ignored this fact and limited their attention to purely mathematical analysis. This approach was probably responsible for the imperfection of the theory in handling the ether problem. Thus, Moon thought that it was time to reexamine the methodology for optical theory, and to introduce fundamental changes that could eliminate the separation between physics and mathematics. He even suggested that the first step should be taken by such universities as Cambridge, which had been the "bulwark" of mathematical knowledge, to introduce an "interdiction" of that branch of study in the future (Moon 1949, p.xv).

Conclusion: The undulationists' doubts about their own theory in the 1840s resulted from several factors. The failures of the undulatory theory in explaining crystalline reflection and some photographic phenomena certainly rocked both MacCullagh's and Herschel's confidence. Moreover, defects in the theory's physical foundation, and the problems associated with the notion of the ether, definitely caused the wavering of MacCullagh, Herschel, and Moon. In addition to these cognitive factors, several contextual factors were also responsible for these undulationists' doubts. Emissionists' persistent attacks certainly made a significant contribution. In the case of MacCullagh, the interactions with Brewster were crucial. It was Brewster who forced MacCullagh to admit the failures of the undulatory theory by presenting experimental findings. More importantly, only Brewster, with his prestige in experimental optics, was able to convince MacCullagh of the reliability of his experimental findings without a process of witnessing or verification. In addition, some undulationists's attitude toward the undulatory theory was also responsible. Both MacCullagh and Moon indicated that there was a "blind admiration" of the undulatory theory, especially of Fresnel's work, in the undulatory camp. According to MacCullagh, this "blind admiration" reflected a tendency to exaggerate the explanatory power of the undulatory theory: some undulationists tended to claim victories for the undulatory theory in fields where it actually failed.[21] In the words of Moon, the symptom of this "blind admiration" was an attitude that "refused to admit the defects of the machinery employed [by the theory]" (Moon 1849, p.xi). Both MacCullagh and Moon clearly stated their discontent with this "blind admiration", and their criticisms of the undulatory theory in some degree reflected their intention to correct this erroneous attitude.

The undulationists' doubts about their own theory in the 1840s had a significant impact on the emission-undulatory controversy. Obviously, those wavering undulationists were only a minority. In addition, MacCullagh died in 1847, Herschel did not conduct

[21] *Literary Gazette*, 1332 (1842):534.

any optical research after 1845, and Moon was merely a second-rank participant in physical optics. These waverers within the undulatory camp did not threaten the dominant status of the theory. For the majority of undulationists, especially those first-rank participants who had power in controlling scientific societies, scientific journals, and university education, like Airy, Lloyd, Forbes, and Whewell, their faith in the theory remained unchanged. However, we cannot thereby entirely dismiss the significance of this crisis of confidence in these three undulationists. In fact, this crisis of confidence meant a great deal to emissionists. All of MacCullagh's, Herschel's and Moon's critiques of the undulatory theory helped expose the defects of the theory, especially those with respect to its physical foundation. Emissionists may have particularly appreciated undulationists' discussions of the ether problem, because the analysis of this problem required considerable mathematics, in which some emissionists such as Brewster were not proficient. Undulationists' discussions of the ether problem provided their rivals with evidence that emissionists would not have been able to obtain themselves. Furthermore, in their critiques of the undulatory theory, the wavering undulationists also expressed, directly or indirectly, their sympathy to the emission theory. Herschel gave the most direct support to the emission theory, praising its potential ability in explaining a series of optical phenomena. Both MacCullagh and Moon expressed their discontent with the blind admiration of the undulatory theory held by some of its supporters. They also showed their concerns over the research style of the undulatory theory and called for more experimental investigations on the physical foundation of the theory. These critiques of the undulatory theory were similar to the complaints made by emissionists, who felt that the emission theory was unfairly treated in an atmosphere of blind admiration for Fresnel and neglect of experimental researches.

Although the doubts in some undulationists about their own theory did not affect the faith of the majority of undulationists, they had an impact on emissionists and provided them with some encouragement. The crisis of confidence in a few undulationists gave some emissionists an illusion that the undulatory theory could be defeated and their own theory could be revived. Encouraged by this illusion, emissionists continued their attacks of the undulatory theory in the late 1840s and even the early 1850s. The life of the emission-undulatory controversy was thus significantly lengthened.

Chapter 7

The Emissionists' Final Campaigns

The doubt, suspicion, and skepticism among some undulationists about their own theory gave great encouragement to emissionists. From the setback of some undulationists, emissionists got a wrong impression that the end of the undulatory theory was imminent. For example, Brewster in 1843 boldly predicted that the undulatory theory's "doom, as a physical theory, is sealed, and when it has lingered for another century as a mathematical hypothesis, the true cause of the phenomena of light will reward the diligence and genius of those who, in the spirit of genuine induction, have advanced in the straight and narrow way that leads to the temple of truth" (1843, p.306). With the hope of winning the competition with the undulatory theory, emissionists in the late 1840s mapped out a final campaign to give the undulatory theory a knockout punch. They recruited new allies who had both scientific credentials and political power, explored such new fields as photography and photochemistry to look for fresh evidence, and endeavoured to develop a new emission theory that could explain more optical phenomena and replace the undulatory theory.

The emissionists' final campaigns, however, occurred in a very unfavorable situation. By the late 1840s, the power of undulationists in British scientific institutions, including scientific societies, scientific journals, and universities, had reached it zenith. The power of their rivals forced emissionists to adopt a deliberate strategy in their final campaigns. They avoided directly attacking the undulatory theory, and limited their work only to presenting experimental findings that could not be explained by *either* theory. Adopting this strategy, emissionists hoped they could first create a relatively independent and autonomous domain of experimental discourse, and then gradually realize their ultimate goal - the downfall of the undulatory theory.

In this chapter I give a detailed description of these final campaigns launched by the emissionists. I examine the debate on diffraction initiated by Brewster and Brougham in the early 1850s, and Brewster's and Potter's attempts to develop new emission theories of light in the late 1850s. I particularly analyze the strategies that emissionists adopted in these final campaigns, discussing such questions as how emissionists' strategies were shaped by their considerations of cognitive appraisal and power, and how the closure of the emission-undulatory controversy was affected by their strategies.

1. The Debate on Diffraction

The major actors in the debate on diffraction in the early 1850s were Brewster and Brougham. In this section, I examine how Brewster initiated the debate by convincing Brougham to take up optical research again, the deliberate strategy he designed, and why he later withdrew his support from Brougham. I also examine how Brougham conducted optical experiments, the discoveries he claimed, and what kinds of responses he received from the supporters of the emission and the undulatory theory.

1.1 The Recruitment of Brougham

Henry Brougham (1778-1868) was one of the most extraordinary figures in the history of nineteenth-century Britain. Rising from humble origins, he made his reputation as a lawyer in the southern circuit of Scotland. Brougham was elected to the House of Commons as early as in 1810. At the same time, he became an advisor of the Princess of Wales. After the princess became queen, she appointed Brougham as her attorney-general. When, in 1820, a bill was introduced at the House of Lords seeking to depose the queen and dissolve her marriage to George IV, Brougham led the defense, securing her victory. He reached the peak of his political career in 1830, when he became Lord Chancellor and entered the House of Lords as First Baron Brougham and Vaux. Brougham lost the Chancellorship in 1834 when Tories took office, but he remained a prominent figure in British Society (Hawes 1957).

With an education from Edinburgh, Brougham attended Playfair's mathematics class and Stewart's moral philosophy class, and was involved in the business of science, especially the controversies between the emission theory and the undulatory theory of light. In 1794 when he was only fifteen years old, Brougham had already conducted a series of optical experiments on the diffraction of light. He presented his findings in two papers to the Royal Society during 1796 and 1797, later published in the *Philosophical Transactions* (Brougham 1796; 1797). Through these publications Brougham established his scientific reputation and became a Fellow of the Royal Society in 1803. In these papers Brougham had clearly committed himself to the emission theory of light. When Young presented his undulatory theory of light in a series of papers published around the turn of the century, Brougham launched fierce attacks on Young in the *Edinburgh Review* (Brougham 1803a; 1803b; 1804). These attacks, according to contemporaries, retarded the acceptance of Young's theory (Peacock 1855, p.182; Whewell 1967, Vol.2, pp.347-8).[1]

From the second decade of the nineteenth century Brougham heavily immersed himself in political affairs. He did not conduct any research in optics for more than thirty years. But he came back to the field of optics in the late 1840s and was involved in a heated debate between the two rival theories of light in the early 1850s.[2]

In fact, Brougham did not volunteer to attack the undulatory theory, he was

[1] For a different opinion of the effect of Brougham's attack, see Worrall (1976, pp.107-10).

[2] Unlike the Brougham-Young debate in the early nineteenth century, Brougham's involvement in the emission-undulatory controversy in the early 1850s has seldom been mentioned in histories of optics. As an exception, see Cantor (1983, p.177).

recruited. It was a letter from David Brewster that renewed Brougham's interest in the controversy between the two rival theories of light. On September 26, 1847, Brewster sent a letter to Brougham, in which he wrote:

> I wish much that your Lordship would take up the subject of the Emission versus Undulatory theory of light. In this cause I have struggled single handed for the last 30 years, and I think I see in the division of sentiment which exists among the Undulationists, and the results of recent experimental inquiries, the downfall of that presumptuous theory.[3]

As we have seen in chapter five and chapter six, Brewster threw up new experimental evidence against the undulatory theory beginning from the late 1830s. His direct attacks on the hegemony of the undulatory theory created confusions in the undulatory camp. Many undulationists, including Lloyd, John Herschel, and James MacCullagh, openly admitted that the new facts presented by Brewster could not be explained by the undulatory theory. These initial successes encouraged Brewster to hope that he could finally restore the dominant status of the emission theory by exposing the explanatory incompleteness of its rival. But Brewster also realized that he was the lone emissionist in the major scientific societies. In the British Association, for example, Brewster was the only opponent of the undulatory theory after John Barton died and Potter left.[4] Brewster looked around for new allies, and set out to enlist Brougham, who shared many opinions from politics to natural philosophy. In his attacks against Young, Brougham had championed the emission theory. Although his scientific credentials were dated, Brougham's political reputation and social status made him an attractive ally.

Brougham accepted Brewster's invitation to rejoin the fight against the undulatory

[3] Brewster to Brougham (September 26, 1847), Library of University College, Brougham Collection, 26,632.

[4] John Barton and Richard Potter supported Brewster at the first three British Association meetings in the early 1830s. But Barton died in 1834 and Potter did not present any work to the British Association meeting after 1834 when he entered Cambridge as an undergraduate.

theory. But since he had left the field of optics for so many years, he lacked the background knowledge required to start his research. Brougham was not familiar with the new developments in the field, especially the new and powerful undulatory accounts of diffraction developed from the work of Fresnel. Brewster therefore provided a reading list for him, which included Lloyd's "Report on the progress and present state of physical optics" presented to the British Association in 1834 and Lloyd's *Lectures on the Wave-Theory of Light*. Brewster assured Brougham that he would find "a very excellent account of optical discussion related to diffraction" in these readings.[5]

It is interesting that Brewster recommended the works of Lloyd, who was one of the most active advocates of the undulatory theory. Possibly Brewster regarded Lloyd's presentation of the undulatory theory not only as more readable -- it had less mathematical analysis -- but also as more informative than other contemporary versions. In the discussion of diffraction in his 1834 report, Lloyd not only presented arguments for the undulatory explanations, but also indicated the unsolved problems in the undulatory accounts, which included the intensity of light in fringes and the *ad hoc* assumption proposed by Fresnel to explain the direction of secondary waves (Lloyd 1834, pp.334-6). By contrast, Airy, in his book of the undulatory theory, praised the advantages of the undulatory accounts of diffraction but said nothing about its problems (Airy 1831). Brewster's recommendation to read Lloyd would acquaint Brougham with the state of undulatory theory, as well as its defects and limits.

For Brougham, who was almost seventy, it was not an easy task to learn a new theoretical framework. It took Brewster a while to explain to Brougham how the

[5] Brewster to Brougham, (September 26, 1847), Library of University College, Brougham Collection, 26,632.

interference principle worked in the undulatory accounts of diffraction.[6] But with Brewster's help, Brougham finally became somewhat proficient in the undulatory account and the function of the interference principle. In late 1848, he was ready to conduct his own experiments.

In his correspondence with Brougham, Brewster also admonished him not to challenge the interference principle, which was applied extensively by undulationists in their accounts of diffraction. According to Brewster, Fresnel had applied the interference principle to provide successful explanations for the phenomenon of diffraction produced by small opaque bodies, by apertures, and by straight edges. By virtue of this principle, he wrote, "the undulatory theory completely explains all these phenomena of diffraction".[7] But Brewster believed that the interference principle was an empirical law, independent of the undulatory theory and even of the nature of light. Although the undulatory theory happened to be coherent with this empirical law, the emission theory was also compatible with it. In fact, he had proposed several arguments reconciling the emission theory and the interference principle (Brewster 1822, p.685; 1831a, p.134). Thus, the interference principle was not the right target for Brougham to attack.

Brewster recommended that Brougham attempt to discover experimental facts not covered by the undulatory accounts, like those he had presented in the previous ten years at the British Association meetings. Brewster said, "as one of the important class of *Rienistes*, I place greatest value on the results of observation, and on experimental

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[6] Brewster to Brougham (September 25, 1848), Library of University College, Brougham Collection, 26,636.

[7] Brewster to Brougham (December 16, 1848), Library of University College, Brougham Collection, 26,637.

laws".[8] Since Brewster had clearly stated in his recruiting letter that the final goal of this campaign was the "downfall" of the undulatory theory, his claim to be a *Rieniste* should only be understood as a tactic. Under the circumstances that the undulatory theory was dominant, direct attacks against it would not be accepted by its supporters. Presenting apparently neutral experimental results that could not be explained by both rival theories, however, could weaken the dominance of the undulatory theory, provided nobody denied the value of experiments. Thus, Brewster suggested a deliberate strategy: in the first stage, to present new facts that the undulatory theory did not cover and hence to challenge its completeness, but not to challenge its existing successes. After these facts were widely accepted, a second campaign for the downfall of the undulatory theory could be launched.

1.2 Brougham's Discovery of A "New Property of Light"

When Brougham started his optical researches, he first repeated several "classical" experiments on diffraction, such as using a single slit to produce diffractive fringes.[9] But soon he began to design new experiments. On December 9 and 10, 1848, Brougham sent two letters to Brewster, describing the results of such an experiment. It was a double-edge experiment, in which he put one straight edge behind another at point A', A'', or A''' from the light source R (Figure 7.1). An undulationist would expect diffraction at both edges, so that inserting the second edge should change the pattern created by the first. Brougham found that when he introduced the second edge to the position behind the first edge, the fringes produced by diffraction at the first edge did not

[8] Brewster to Brougham (February 21, 1849), Library of University College, Brougham Collection, 26,638. Here "*Rienistes*", literally "nothingists", are those who adopt neither theory as true.

[9] See Brewster to Brougham (September 25, 1848), Library of University College, Brougham Collection, 26,636.

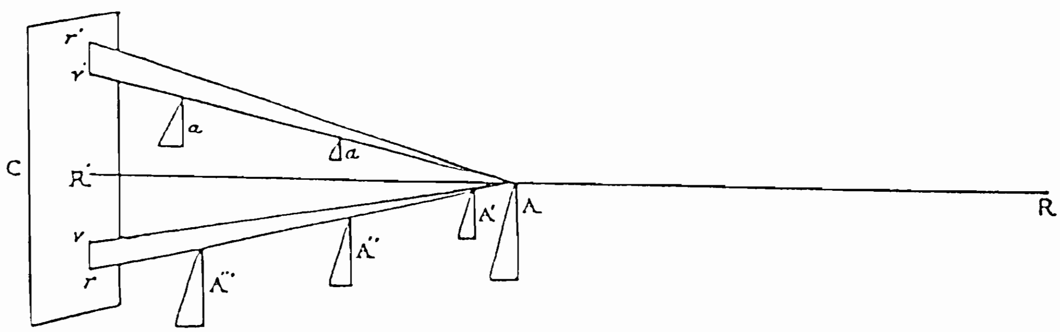

Figure 7.1 Brougham's First Double-Edge Experiment on Diffraction
[From Brougham 1850a, Figure 5]

change at all.[10]

Brewster's response to Brougham's experiment was very positive. Brewster praised the importance of this discovery, which raised the possibility of a new experimental phenomenon that the existing undulatory accounts of diffraction did not cover, and might be interpreted as a new kind of "polarity of light", the notion he had explored since the late 1830s. However, Brewster also regretted to report that he had tried in vain to obtain the same experimental result. In all his experiments, the introduction of the second edge changed the previous pattern of diffractive fringes. Brewster suggested that this failure might result from a difference in the light source; he used artificial light but Brougham used natural light. To repeat the experiment, Brewster asked Brougham to make sure that light actually fell upon the second edge, and that the fringes were really the joint effect of the two edges.[11]

Following Brewster's suggestion, Brougham designed his next experiment to make sure that light actually fell on the second edge. No later than the summer of 1849, Brougham conducted another double-edge experiment, in which he put the second edge B in from the other side, opposite edge A, but at a different distance from the light source R (Figure 7.2). He obtained an entirely different result. Instead of having no effect, edge B enhanced the diffractive effects produced by edge A. Introducing edge B in this way, Brougham found that the new fringes $r''v''$ became wider and moved further from the direct rays RR'.

As a person with a long-term commitment to the emission theory of light, Brougham understood light as a beam of particles. By combining the new experimental

[10] See Brewster to Brougham (December 16, 1848), Library of University College, Brougham Collection, 26,637.

[11] *Ibid.*

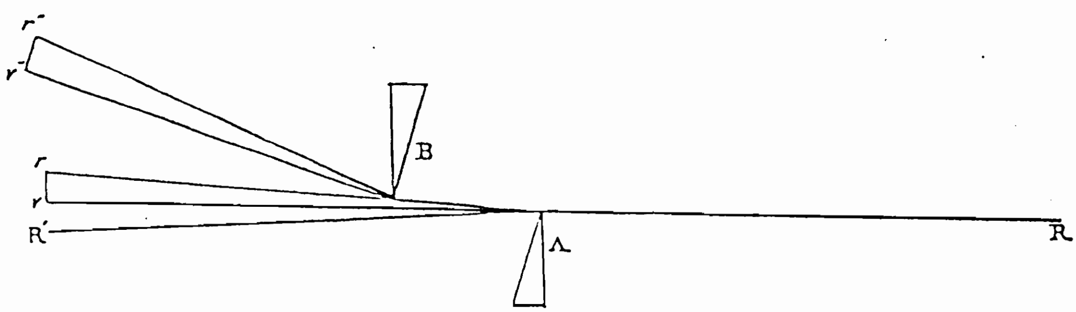

Figure 7.2 Brougham's Second Double-Edge Experiment on Diffraction
[From Brougham 1850a, Figure 7]

result and the earlier one, Brougham believed that he had demonstrated a new property of light that differed on opposite sides of a pencil of light rays. He concluded that after a pencil of light had been inflected by an edge it exhibited different properties on different sides when submitted to a second edge. On the side close to the first edge, the pencil of light became difficult or even impossible to inflect again, but on the side opposite to the first edge, the pencil of light became easier to inflect (Brougham 1849, pp.7-8).

Brewster responded enthusiastically to Brougham's new discovery. In August 1849, he asked Brougham both to represent more clearly in a diagram "the two distinct actions of the oppositely placed diffracting edges", and to allow him to give an account of these experiments at the British Association meeting, about to take place at Birmingham on September 12, so that these results could be publicized without delay.[12] Brewster had long complained that the purely mathematical discussions of undulationists were "advantageously published" while experimental works from emissionists were ignored and unfairly treated.[13] Using Brougham's experimental study, revealing unknown phenomena, Brewster hoped to challenge the dominance of mathematical discussion in the British Association.

On September 18, 1849, Brewster reported Brougham's experimental works to the Section of Mathematical and Physical Science (Section A) at the British Association meeting. In a letter sent to Brougham, Brewster reported interest in Brougham's new

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[12] Brewster to Brougham (August 25, 1849), Library of University College, Brougham Collection, 26,643.

[13] In 1841 Brewster submitted a mainly experimental paper on polarization to the *Philosophical Transactions*, but it was rejected by Airy who acting as referee. Brewster regarded this rejection as an evidence that experimental works were overlooked by undulatory theorists. See Brewster to Brougham (December 14, 1841), Library of University London, Brougham Collection, 26,624. Brewster's paper was later published in another journal. See Brewster, (1843).

experimental work by those who studied physical optics.[14]

With Brewster's encouragement, Brougham continued to pursue what he now considered to be the distinct properties of opposite sides in a pencil of light rays. Around September or October of 1849, Brougham designed a new experiment, in which he used three successive edges: he placed the first edge and the third edge on the same side but the second edge on the opposite side. Brougham carefully examined the change of fringes when he introduced the third edge. At first he could not determine the effect of the third edge because the change it produced was too subtle. Brougham then designed a precise instrument, and asked Soleil, a well-known maker of optical apparatus in France,[15] to make it for him.

The new instrument (Figure 7.3) consisted of a grooved beam with three uprights (*H*, *I*, and *K*) that could move along the groove by rack and pinion. On each upright was a broad sharp-edged plate (*A*, *B*, and *C*, respectively), moved up and down by a rack and pinion in the upright. The instrument enabled Brougham to easily change the position of each edge and to obtain accurate measurements of both vertical and horizontal distances among the three edges. With the help of this instrument, Brougham said that he could perform a "crucial experiment" to determine the effect of the third edge. He consistently found that the third edge *G*, when placed on the opposite side of the second edge *F* but on the same side as the first edge *E*, enhanced the diffractive effect produced by the first and the second edges; the fringes *p* produced by the three successive edges was broader than the original fringes *o'* produced by edge *E* and *F*, and further removed from the direct rays *RR'* (Figure 7.4). This experimental result, Brougham believed,

[14] Brewster to Brougham (September 20, 1849), Library of University College, Brougham Collection, 26,648.

[15] For biographical information on Soleil, see Turner (1975).

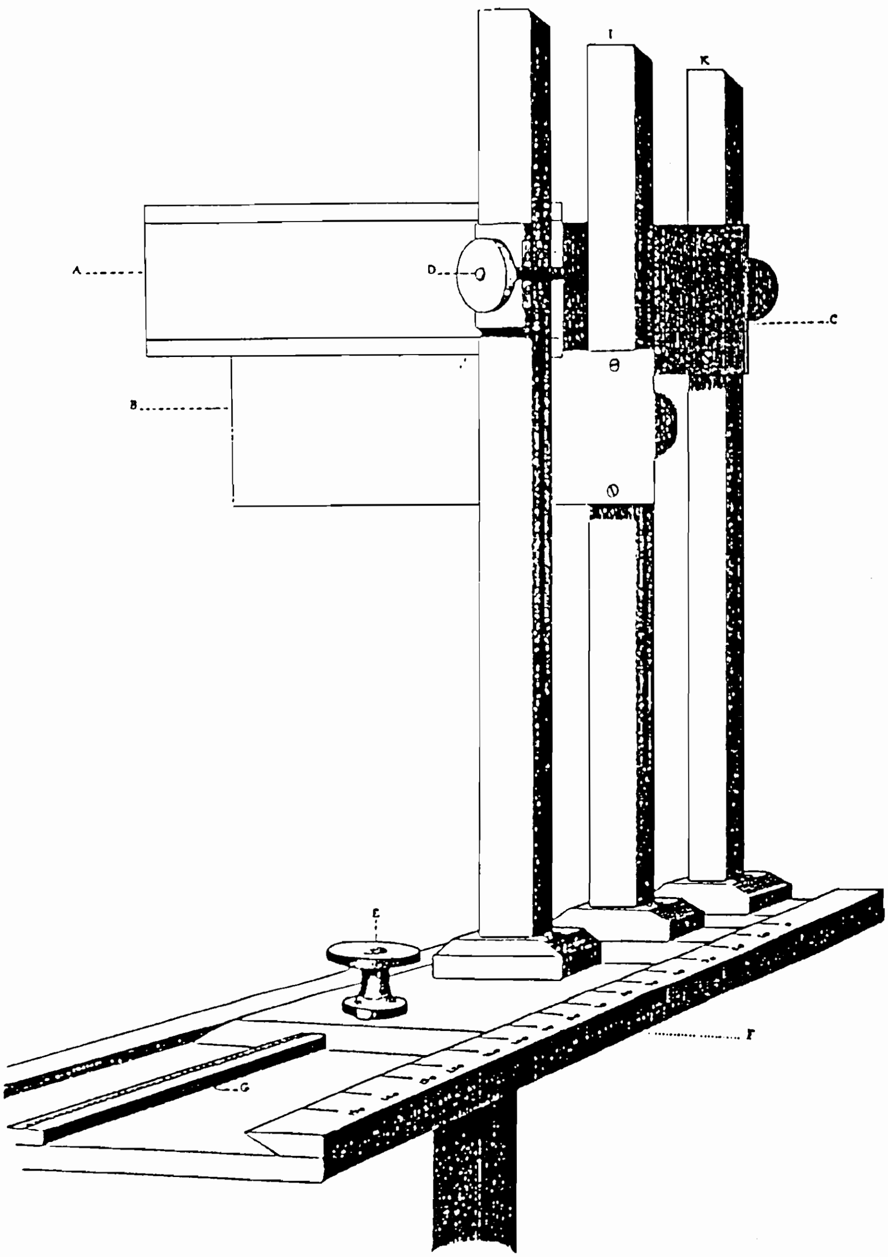

Figure 7.3 **Instrument in Brougham's Triple-Edge Experiment**
[From Brougham 1850a, Plate XIV]

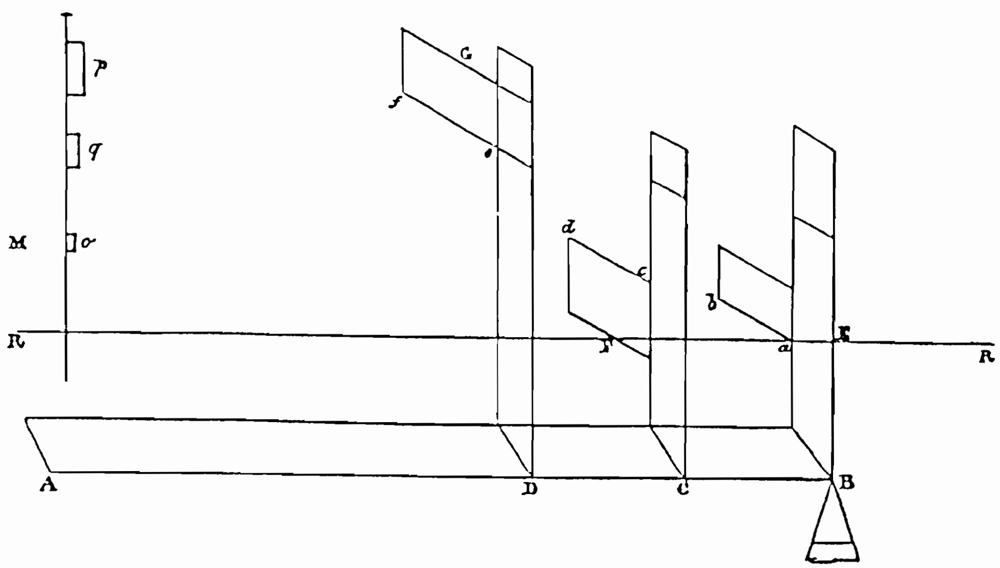

Figure 7.4 Brougham's Triple-Edge Experiment on Diffraction
[From Brougham 1850a, p.242]

could further demonstrate the different properties existing on the different sides of the pencil of light (Brougham 1850a, pp.242-3).

Brougham did not hesitate to publicize his new discoveries. On November 1, 1849 he completed a paper summarizing all the new phenomena he had found. He read this paper at the Royal Society on January 10, 1850, and at the French *Académie des sciences* in the same year. Encouraged by Brewster, Brougham submitted the paper to the *Philosophical Transactions*. According to Brewster, since the paper contained "positive experiments and observations", he did not see how the editors and referees of the journal could refuse publication.[16] Brewster's anticipation was correct: the journal accepted Brougham's paper and published it in the same year. Brougham also successfully had an outline of this paper published in a French journal (Brougham 1850b).

At the beginning of the paper, Brougham claimed that he did not intend to involve himself in the controversy between the mission and the undulatory theory of light. He promised:

> I purposely avoid all arguments and suggestions upon the two rival theories - the Newtonian or atomic, and the undulatory. The conclusions at which I have arrived are wholly independent, as it appears to me, of the controversy. I cautiously avoid giving any opinion upon it; and instead of belonging to the sect of undulationists or anti-undulationists, I incline to agree with my learned and eminent colleague M. Biot, who considers himself as a "*Rieniste*," and neither "*ondulationiste*" or "*anti-ondulationiste*" (1850a, pp.235-6).

In fact, he had not obtained theory-free descriptions of experiments. For Brougham, his representations of the experimental results were heavily contaminated by his emission framework, especially in his descriptions of light as a pencil of particles and his discussions of the cause of inflection and deflection. But this opening statement clearly

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[16] Brewster to Brougham (February 15, 1850), Library of University College, Brougham Collection, 26,656.

indicates that Brougham had accepted Brewster's suggestion, namely, he intended to avoid challenging the existing explanations provided by the undulatory theory, but merely to present new experimental results that could not be explained by both theories.

The major content of Brougham's paper was a summary of a variety of experiments concerning the different properties exhibited in the different sides of a pencil of light. He first demonstrated the existence of such different properties of light by virtues of the experiment with two successive edges on different sides and the "crucial" experiment with three successive edges. He then went further to reveal a series of characteristics of the fringes produced by two successive edges. He reported that the property of being easily deflected or easily inflected decreases as the distance between two edges increases, and that the effects of being inflected by the first edge and then deflected by the second were unsymmetrical to the effects of being deflected by the first edge and then inflected by the second.

Finally, Brougham tried to prove that his discoveries were not covered by the undulatory theory, in particular by the principle of interference. He claimed that "[t]he phenomena described in the foregoing propositions are wholly unconnected with interference, and incapable of being referred to it" (*Ibid.*, p.251). He presented several arguments to support this claim.

In one of these arguments, Brougham claimed that, according to the undulatory theory, interference between two rays could happen only when the path difference of the two rays was a very small magnitude, such as several wavelengths of light. But in his experiment with two successive edges on different sides, Brougham said, fringes continued to form even when he placed the second edge a foot and a half away from the first. In such cases the path difference between the rays responsible for the production of the fringes was much larger than the magnitude required by the interference principle. Brougham concluded, therefore, the production of this fringes had nothing to do with interference (*Ibid.*).

Another interesting argument concerned the relationship between the breadth of the fringes and the path difference of the rays responsible for the fringes. Brougham indicated that, both in the case of ordinary apertures or in the case of his two-successive-edge experiment, the breadth of the fringes decreased when the distance of the fringes to the direct rays increased, namely, the first fringe was broader than the second, and the second broader than the third. According to the principle of interference, in the diffraction at ordinary apertures, this pattern of diffractive fringes was due to a fact that the path difference of the relevant rays increased more rapidly when their point of intersection moved further from the direct rays, and hence the effects of enhancement and cancellation among the rays happened more frequently. So the principle of interference implied that, in diffraction at ordinary apertures, the breadth of the diffractive fringes was inversely proportional to the path difference between the intersecting rays. However, Brougham pointed out, the path difference between intersecting rays *decreases* rather than *increases* in his experiment with two successive edges, violating the inverse proportion required by the interference principle. Brougham consequently asserted that the diffraction pattern in his two-successive-edge experiment was entirely different from those in regular apertures and was not covered by the principle of interference (*Ibid.*, pp.251-2).

In these two arguments, Brougham clearly interpreted the principle of interference in his own way. In his analyses of diffraction in the two-successive-edge experiment, Brougham only considered the intersection of two pencils of rays: one inflected from the first edge and another deflected from the second. Similarly, in his analyses of diffraction at an ordinary aperture, he only counted two pencils of rays, each inflected or deflected from one edge of the aperture. By contrast, undulationists after Fresnel would consider in these cases all of the rays coming from every point of the secondary wavefront.

Brougham's binary rays interpretation, however, was similar to that of Young, who also only considered the intersections of two rays. Thus, Brougham's analyses of diffraction in the two-successive-edge experiment and in the ordinary aperture were comprehensible for most British undulationists who could still remember Young's original theory of interference.

1.3 The Rival Responses

The publication of Brougham's discoveries triggered strong reactions from both emissionists and undulationists. Not surprisingly, Brougham received high praise from Brewster. But Brewster did not approve of Brougham's work primarily for its substantial conclusions. Brewster never expected that Brougham's experimental results would immediately overturn any existing account provided by the undulatory theory, although he later agreed with Brougham on the importance of distinguishing diffraction from interference by showing the cases of diffraction that were not connected to the interference principle.[17] Brewster appreciated Brougham's courage in challenging the dominance of the undulatory theory and the symbolic significance of Brougham's challenge. After learning that Brougham had given a report of his discoveries at the French *Académie des sciences*, Brewster sent congratulations to Brougham, saying:

> I was greatly delighted with the account ... of your Lordship's lecture before the Academy of Science - the headquarter of Undulationism; For though Biot is an emissionist he has never, in so far as I know, ever ventured to impugn the undulatory theory, either by supplying his own view, or adducing facts which that theory cannot explain.[18]

Brewster had long been complaining that he was the only individual who had the courage

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[17] Brewster to Brougham (October 2, 1849), Library of University College, Brougham Collection, 26,649.

[18] Brewster to Brougham (February 7, 1850), Library of University College, Brougham Collection, 26,655.

to attack the undulatory theory. Now he confidently believed that Brougham's experiments would "encourage others to throw off the yoke, and convince them that the true theory of light, embracing the phenomena of inflexion, double refraction and polarization remains to be discovered".[19]

The first reaction to Brougham's discoveries from the undulatory camp was cautious and moderate. Airy, the most influential figure among the undulationists in nineteenth-century Britain, was the president of the British Association meeting in 1851. Although he seldom conducted optical research after the mid 1830s, his book on optics written in 1831 was assigned as the textbook for the optical section of the Mathematics Tripos at Cambridge (Wilson 1987, p.27). He was also one of the most dogmatic advocates of the theory and had been known to counterattack fiercely any challenge from the emission camp. But in his presidential address at the 1851 meeting, Airy felt obliged to mention Brougham's work. When he turned to the field of optics in the presidential address, Airy said that "in optics, two or three investigations, of [a] rather important character, have, since the last meeting of the Association, attracted public attention".[20] The first discovery that Airy regarded as important was Foucault's 1850 measurement of the velocities of light in air and in water. The second important discovery was Stokes's experiment proving that the vibrations of polarized light were perpendicular to the plane of polarization, as Fresnel had predicted. The last discovery, the importance of which Airy still thought uncertain, was Brougham's experimental work on diffraction. Airy said:

> A curious series of experiments on diffraction has been published by Lord Brougham, but they have at present no bearing on theory, as the theoretical

[19] Brewster to Brougham (February 7, 1850), Library of University College, Brougham Collection, 26,655.

[20] *Literary Gazette*, 1798 (1851):464.

> calculations with which they must be confronted appear to be too difficult or too complicated for the present state of pure mathematics.[21]

Here was a public statement from one of the most stubborn undulationists, admitting that the undulatory theory was not yet able to provide a theoretical treatment of Brougham's experimental discoveries. At the same time, Airy also tried to minimize the damage due to Brougham's publication. He seized upon the statement in the beginning of Brougham's paper, claiming that the experimental results did not bear on any theory and thereby could not harm the dominant status of the undulatory theory. But by accepting the apparently harmless experimental results, which eventually could undermine the dominant status of the undulatory theory, Airy had perfectly conformed to Brewster's expectation of his opponents' reaction.

The next response to Brougham's experiments from the undulatory camp was more challenging. At the same British Association meeting, Powell read a comment on Brougham's 1850 paper in the section of Mathematical and Physical Science. Although he was a professor of geometry, Powell was the most active advocate of the undulatory theory of light. From the 1820s to the 1850s, Powell published more than 70 scientific papers on physical optics and involved himself in almost every debate between the emission and the undulatory theory of light.[22] In his comment on Brougham's paper, Powell described Brougham's intention as "offering new facts at variance with the principle of interference, hitherto so successfully applied to all phenomena of this class" (1851, p.11) But in Brougham's attack on the interference principle, Powell said, "some misconception" of the principle was involved. One of such misconception consisted in

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[21] *Literary Gazette*, 1798 (1851):464.

[22] Among physicists in nineteenth-century Britain, Powell was second in the number of published optical papers. Brewster was first, with more than one hundred published papers on optics, and Stokes was third with less than 50. No any other physicist published more than 30 papers on optics. See Royal Society of London, (1867).

Brougham's comprehension of the relationship between the breadth of the fringes and the path difference of the rays. Powell argued that the breadth of the fringes had no dependence on the length of the rays' path, but rather depended on the angle at which they intersected. Since in Brougham's two-successive-edge experiment the intersecting angle between the rays increases when the intersecting point moves further from the direct rays, Powell claimed that the inverse proportion implied by the interference principle still held.[23]

Referring to the two-successive-edge experiment, Powell noted that Brougham's arrangement would be equivalent to a wide aperture placed obliquely to the direction of the rays. And he also indicated that Fresnel had provided a general and qualitative discussion of this particular case of oblique aperture (Fresnel 1826, p.452). But the quantitative treatment of this phenomenon, Powell admitted, was well beyond the ability of the theory. He said:

> Though the undulatory theory has been successfully applied to the general subject of these fringes, yet it is well known that the application of the formulas to any but the simplest cases of edges and apertures is defective, owing to the great complexity of the resulting expressions, and the impossibility of integrating them except under very restricted conditions. Thus the integration has not been extended to the action of a second or third edge *at different distances*; ... In an attempt to deduce these expressions at length, it has been found that the expressions become extremely complicated, though it seems difficult to say whether they may not still yield to proper treatment (1851, pp.11-2; original emphasis).

The diffraction of light was the most difficult phenomenon in the field of optics because the theoretical treatments for this sort of phenomena involved integral equations. The undulatory theory had been able to supply quantitative solutions to a few simple cases of diffraction by 1850. But Powell argued that such shortcomings would not trouble the

[23] Powell's argument was problematic, because it cannot be applied to ordinary apertures, in which the intersecting angle *decreases* when the intersecting point moves further from the direct rays, and hence the inverse proportion implied by the principle does not hold.

undulatory theory at all. It remained a question, Powell claimed, whether it was worth spending time on these problems, since the value of their solutions would not repay the expenditure of time (Powell 1851, p.12).

Brewster was present when Powell read his comment. Immediately after Powell finished his remark, Brewster stood up to defend Brougham. Brewster first explained to the audience that Brougham did not intend to overthrow or to establish any theoretical principle by his experiments. What Brougham endeavoured to show was just a new property of light. In his defense of Brougham, Brewster agreed with Powell that Brougham's experiments did not militate against the undulatory theory, but he claimed that Powell had overlooked the significance of Brougham's experimental discoveries.[24] For a long time Brewster had been complaining that the undulationists only paid attention to the experimental results that could be inferred from their theory. In a letter sent to Brougham in 1849, Brewster wrote:

> Theorists value these [experimental results] only when they are deducible from theory, and the undulationists of this country are such fanatics that they have no faith in physical truths beyond their pale.[25]

In Brewster's eyes, Powell's comment on Brougham was a typical example of such disregard of experimental work by undulationists.

Brewster's defence forced Powell to pay attention to Brougham's experimental works. In July 1852, Powell published a paper in the *Philosophical Magazine*, in which he challenged Brougham's experimental works directly, by carefully replicating the two-successive-edge and the three-successive-edge experiments.

Except for a picture of the instrument used in the "crucial experiment", Brougham

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[24] *Athenaeum*, 1237 (1851):746; *Literary Gazette*, 1802 (1851):532.

[25] Brewster to Brougham (February 21, 1849), Library of University College, Brougham Collection, 26,638.

had not provided any details about his experimental design, such as what kind of light source he used and what the distances between the source, the edges, and the screen were. Consequently, Powell had to replicate Brougham's experiments from the brief verbal description in Brougham's paper.

In his description of the "crucial" experiment, Brougham had used the term "fringe", in the singular, to refer to the whole diffractive pattern produced by two or three successive edges. According to the context, the term "fringe" here referred to both the light spot at the geometrical center of the pattern and the stripes on both sides of the center. But it seems that Powell understood "fringe" in another way. In his paper, he always used this term in its plural form, and used it to refer only to the stripes on one side of the geometrical center. The singular usage of the term in Brougham's text might have had Powell believe that what Brougham described was only a group of stripes on one side of the geometrical center, since Brougham spoke of "only *one* set of fringes" (Powell 1852, p.5; original emphasis).

In order to learn how such experimental results could be obtained, namely, the appearance of only one group of stripes on one side of the geometrical center, Powell first conducted a series of double-edge experiments. Powell noted that, when two edges were at the same distance from the source and formed a narrow aperture, they produced two symmetrical groups of stripes on both sides of the center. When two edges were at different distances from the source, they still produced two groups of stripes but one of them was dilated and became faint. When the distance between the edges went beyond a certain limit, one group of the stripes disappeared and only the fringes on the other side remained. On the basis of these preliminary studies, Powell believed that he had successfully determined one of the important experimental parameters in Brougham's experiments. The distances between edges in the two-successive-edge and the three-successive-edge experiment must have been relatively large, and beyond certain limits.

After arriving at his own understanding of Brougham's experimental parameters, Powell replicated the three-successive-edge experiment. He reported that he came up with a completely different result. Brougham noted that, when he introduced the third edge, it would enhance the diffractive fringes made by the first and the second edge. The new "fringe" became broader and moved further from the direct rays but remained on the same side as the original one. But Powell said that he observed the new "fringes" moving over to the other side of the direct rays when he introduced the third edge. He claimed that "in repeating the experiment a great number of times at very different distances, and under varied conditions, I have never been able to obtain any other result: indeed it would clearly be inconsistent with the former experiments that it should be otherwise" (*Ibid.*, pp.5-6). Powell flatly contradicted Brougham's findings, casting doubt on the reliability of Brougham's "crucial experiment".

In spite of Powell's attack, Brougham was still confident in his discoveries, and continued to conduct research along the same lines. He wrote a new paper in 1852, and read it at the Royal Society on April 30, 1852.[26] In the first part of the paper, Brougham reported an experiment in which he obtained diffractive fringes by combining direct rays and reflected rays from a mirror. Brougham intended to use this experiment to explore the relationship between inflection and reflection. The second part of the paper consists of criticisms of the undulatory theory. On the one hand, Brougham continued to argue that the undulatory theory could not cover the experiments he presented in his 1850 paper; on the other hand, he began to attack some existing explanations provided by the undulatory theory. Brougham first challenged the undulatory account of the breadth of fringes in ordinary apertures, claiming the undulatory calculations were wrong. He also cast doubt on the undulatory explanation

[26] For an outline of this unpublished paper, see Brougham (1853a).

of the internal fringes of small objects in terms of interference, asking for more careful examinations of the phenomenon.

The response from Brewster toward this paper was mixed. On the one hand, Brewster continued to praise the experimental work presented in the first part, because it was "a pure experimental determination of the principal phenomena of inflexion, though it is not unlikely that they may say that the undulatory theory give the same results."[27] On the other hand, however, Brewster disagreed with Brougham's remarks in the second part of his paper. Brewster told Brougham:

> I do not feel myself able to give an opinion of the second part in which you shew that the phenomena of exterior and interior fringes cannot be explained by the doctrine of interference; and I am sure that the Society will stand aghast at the result at which you have arrived, as the undulatory theory is driven from its chief stronghold if it fails in representing the phenomena of the interior and exterior fringes.[28]

Brewster objected to Brougham departing from his strategy against the undulatory theory. From the outset he had emphasized that the interference principle should not be the target, and that direct attacks against any existing undulatory explanation were not appropriate while the undulatory theory remained dominant. Brewster rightly anticipated the fate of Brougham's paper: it was rejected by the *Philosophical Transactions*.[29]

Brougham continued his optical studies in 1853. In this year, he read a paper, titled "Further experiments and observations on the properties of light", at the Royal Society on June 9.[30] In this paper Brougham claimed that both the Newtonian theory

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[27] Brewster to Brougham (May 8, 1853), Library of University College, Brougham Collection, 26,670.

[28] Brewster to Brougham (May 8, 1853), Library of University College, Brougham Collection, 26,670.

[29] An outline of this paper was published in French. See Brougham, (1852).

[30] For an outline of this unpublished paper, see Brougham (1853b).

and the undulatory theory of light failed to account for the breadth of fringes formed by small objects. In consequence, he introduced a new concept, the "property of different flexibility", claiming that he could use this notion to solve the problems that both the Newtonian and undulatory theory met. In this paper Brougham had once again departed from Brewster's advice on strategy and directly challenged the undulatory theory.

Recognizing that Brougham could no longer be relied upon to follow his directions, which had now been ignored in two instances, Brewster withdrew his support. Although Brewster later continued to cite Brougham's work of 1850 to show the undulatory theory's incompleteness, he never mentioned Brougham's works of 1852 and 1853 (Brewster 1855, pp.209-10). The response from the undulatory camp was also negative. In his referee's report on the paper, Airy wrote that "it is written in entire ignorance both of the general principles of the great Undulatory Theory and the algebraical and numerical results which have been deduced from it. The paper is more than twenty years behind the actual state of science".[31] Airy thus rejected its publication in the *Philosophical Transactions*.[32] After the second rejection of his paper, Brougham gave up trying to publishing his optical papers in British journals. Although he continued to circulate his optical papers among sympathizers such as Potter,[33] his studies on optics no longer appeared in public.

[31] Airy's Referee's Report on Brougham's paper, Royal Society Library, RR. 2.36.

[32] An outline of this paper was published in French, see Brougham, (1853c). A complete version of this paper was also published in French, see Brougham, (1854).

[33] See Potter to Brougham (March 19, 1855), Library of University College, Brougham Collection, 26,199.

2. Brewster in the 1850s

In addition to recruiting new allies, Brewster himself in the 1850s was also deeply involved in the emission-undulatory controversy. In this section, I first review Brewster's reaction to Fizeau's and Foucault's experiments on the velocity of light, two of the most important supports for the undulatory theory. I also examine Brewster's attempt to develop a universal emission theory, showing how his theoretical exploration finally failed.

2.1 Fizeau's and Foucault's Experiments

Despite achieving enormously successful explanations and predictions, until the end of the 1840s undulationists still had not verified one of the key predictions of their theory, that the velocity of light in dense media was smaller than that in rare media. In 1838 Arago had suggested a technique of using a rotating mirror for comparing the velocities of light in different media. However, his attempt to carry out the experiment was unsuccessful, and his falling eyesight finally forced him to give it up. Later in 1850 Fizeau and Foucault picked up Arago's idea, overcame the obstacles that had perplexed Arago, and successfully measured the relative velocities of light in air and in water. Although Fizeau and Foucault conducted their experiments separately, their experimental arrangements were almost identical (Figure 7.5). A beam of light from the source a was reflected by a rapidly rotating mirror m to a fixed mirror M placed at a considerable distance. The light beam reflected from M came back to m along the same path, but, since the rotating mirror had a small angular displacement, it was deflected by m and formed a reflected image a'. In addition, a beam of light was reflected by m to another fixed mirror M', going through a water-filled tube and forming another reflected image a''. The relative velocities of light in air and in water could then be determined by measuring the displacements of the reflected images a' and a'' from the source a. Since both Fizeau and Foucault in their experiments found that the reflected image a'' was

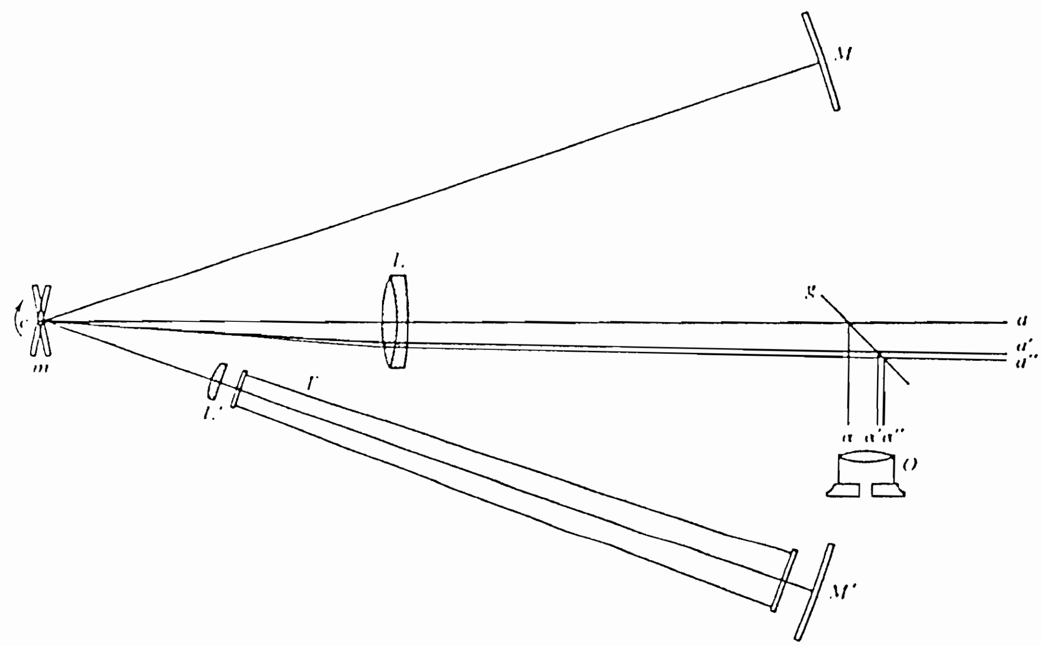

Figure 7.5 Foucault's Experiment on the Velocities of Light
[From Burstyn 1975, p.85]

deflected more than the image a', they concluded that light must travel slower in water than in air (Burstyn, 1975; Gough, 1975). Based upon these experimental findings, Foucault further asserted that he had "experimentally demonstrated the truth of the undulatory theory, and the falsehood of the Newtonian" (Brewster 1854, p.262).

Brewster had witnessed both Fizeau's and Foucault's experiment.[34] He did not have any question about the concrete experimental findings, namely that light traveled faster in air than in water, but he strongly disagreed with Foucault's use of this experiment as a crucial test of the two rival theories of light. Brewster conceded that Fizeau's and Foucault's experiments could really support the undulatory theory, which had predicted a faster velocity of light in air than in water while the emission theory predicted the opposite. Nevertheless, Brewster insisted that there should be an essential difference between confirming a theory and demonstrating the truth of a theory. He argued:

> Arago himself never even asserted that the undulatory theory was *demonstrated* even by his own early experiments and those of Fresnel. He for twelve years looked forward to the experiment of Foucault as a *conformation* of his view; and were he alive, he would tell us, with his usual candour, that something more is wanting to make the prevailing theory of light a theory of universal and necessary truth. (1854, p.262; original emphasis)

Brewster had long been claiming that to explain and predict facts was by no means a test of the truth of a theory. According to his philosophical standpoint, a hypothesis, no matter how successful and useful, was essentially different from the laws of nature. In order to prove the truth of a hypothesis, it was necessary to show that the hypothesis had revealed a true cause of nature, and this was exactly what the undulatory

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[34] Brewster said that he had seen Fizeau's experiment in 1850 in Paris. See Brewster to Brougham, (August 28, 1856), University College Library, Brougham Collection, 26,695. Brewster also stated that he had seen Foucault's experiment, but he did not indicate the time and the place. See Brewster (1854, p.262).

theory failed to achieve. Brewster insisted that, although the undulatory theory could explain a considerably large number of optical phenomena and even could predict some new facts, the theory did not correctly reveal the true causes of optical phenomena. He said:

> Now, though the undulatory theory does assume an *ether*, invisible, intangible, imponderable, inseparable from all bodies, and extending from our own eye to the remotest verge of the starry heavens; yet, as the expounder of phenomena the most complex, and otherwise inexplicable; and as the predictor of highly important facts, it must contain among its assumptions (though as a physical theory, it may still be false) some principle which is inherent in, and inseparable from, the real producing cause of the phenomena of light; and to this extent it is worthy of our adoption as a valuable instrument of discovery, and of our admiration as an ingenious and fertile philosophical conception. (Brewster 1838, pp.306-7; original emphasis)

Thus, Fizeau's and Foucault's experiments could improve the explanatory and predictive power of the undulatory theory, but not its ability to reveal the true causes of optical phenomena. The undulatory theory of light was still far from a true theory.

Moreover, Brewster indicated that it was possible to explain Fizeau's and Foucault's experimental findings in terms of an early version of Newton's optical theory. Newton in his second paper on color and light, read to the Royal Society in 1675, proposed a theory of light in which some principles of the emission and the undulatory theory were combined. Newton assumed the existence of both the particles of light and the ether, and explained a series of optical phenomena by the supposition that "light and ether mutually act upon one another, ether in refracting light, and light in warming ether" (Newton 1675, p.255). Brewster believed that "with that theory, which we do not adopt, the great experiment of Foucault is not at variance" (1854, p.267). Therefore, the undulatory theory was not the only theory that could explain Fizeau's and Foucault's experiments, and then these experiments could not be used as a crucial test to either justify the truth of the undulatory theory or to prove the falsehood of the emission theory.

2.2 Brewster's Universal Emission Theory

From the early 1840s, the newly discovered photographic and photochemical phenomena began to draw Brewster's attention. His enthusiasm for the nascent photographic and photochemical research came from his hope that he could find in these fields new evidence to attack the undulatory theory. Brewster believed that "the curious chemical actions of light which are displayed in the processes of the Daguerreotype and Talbotype cannot possibly be the results of mere vibratory motions".[35] After he learned about Moser's discovery of "latent light" at the 1842 British Association meeting (see above, chapter six), Brewster had intuitively realized that this phenomenon could not possibly be explained in terms of the undulatory theory, but seemed compatible with the emission theory.

Later, discoveries similar to Moser's "latent light" made by chemists in the mid 1840s further reinforced Brewster's speculation. In 1844, Hunt reported that he could copy engravings on metallic plates by the effect of heat. Hunt placed a silver medal on a well-polished copper plate and warmed the plate with a lamp. He then cooled the plate and exposed it to the vapor of mercury. He found that the silver medal left a distinct impression on the plate. In this way Hunt copied printed pages and engravings on iodized paper by mere contact and exposure to heat. He also found that such copying process could be done even when the engravings and the iodized papers were kept at considerable distance (Hunt 1844, pp.228-30). Draper, Knorr, and Fizeau also made similar discoveries during the first half of the 1840s (Brewster 1847, pp.264-7).

According to Brewster, these phenomena could become comprehensible through acceptance of a universal emission hypothesis, namely that not only luminous bodies but

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[35] Brewster to Brougham, (September 26, 1847), University College Library, Brougham Collection, 26,632.

all materials radiated particles. He proposed:

> [A]ll bodies throw off emanations in greater or less abundance, in particles of greater or less size, and with greater or less velocities - that these particles enter more or less into the pores of solid and fluid bodies, sometimes resting near their surface, sometimes effecting a deeper entrance, and sometimes permeating them altogether. (Brewster 1847, p.267)

The emanations of these particles, Brewster speculated, were generated by every cause such as heat, vibratory action, fiction, and even electricity. These causes affected the forces of aggregation, by which the particles of bodies were held together. When these emanations of particles were feeble, they showed themselves in the phenomena discovered by Draper, Hunt, Moser, Fizeau, and Knorr. When these emanations were stronger, they could cause certain chemical changes, and when they were still stronger, they could affect the olfactory nerves, generating smell. When the emanating particles were thrown off most copiously and rapidly, they could affect the sense of touch and produce the effect of heat, separate or combine the elements of matter in photographic processes, and excite the retina to produce vision. Since the only difference between the particles of light and other emanating particles was their velocities of projection, Brewster's universal emission theory not only assumed all materials radiated particles, but implied that the particles of light contained all the elements of materials. Thus, the emanations of particles from luminous bodies -- the fundamental doctrine of the emission theory of light -- were just a special case in a spectrum of material emanations.

Brewster had reasons to believe that the universal emission theory he proposed was not just a speculation. He knew that Professor Zantedeschi of Venice in 1847 conducted a series of experiments, which "prove that even metallic substances pass into the radiant state; and that they are reflected and refracted like light or heat, and return into a concrete state in virtue of their chemical affinities".[36] Although Brewster still had some doubts on the reliability of Zantedeschi's experiments, he was convinced that these findings, if reliable, could support his universal emission theory. In particular, Zantedeschi's experiments could be used to demonstrate that "light contains, or consists of, all the elements of matter, and that the similar atoms of light and of matter may be united again when they are brought within the sphere of their mutual attraction".[37]

The universal emission theory, if justified, could certainly give the emission theory of light some kind of theoretical support, showing the similarity between optical phenomena and those in other related fields. But the universal emission theory could not help Brewster solve any particular problems, offering neither new explanations nor new predictions of optical phenomena. For a long time Brewster had admired the explanatory power of the interference principle and implicitly recognized the existence of periodicity in optical phenomena. As early as in the 1820s, Brewster had endeavoured to reconcile the emission theory and the interference principle, but without success. Up to the mid 1850s, he still failed to come up with any new idea that could reconcile the emission theory and the interference principle. Finally in the late 1850s Brewster developed a new idea, in which he attempted to combine his universal emission theory with some features of the undulatory theory.

Brewster's new idea was directly inspired by Niepce Victor's photographic work in 1858. In a series of experiments Victor reported a new phenomenon of latent light, very similar to those found by Moser and Hunt in the 1840s. Stimulated by Victor's discoveries, Brewster threw out a very bold hypothesis. In a letter sent to Brougham,

[36] Brewster to Brougham, (September 26, 1847), University College Library, Brougham Collection, 26,632.

[37] *Ibid.*.

he proposed:

> The existence of material particles in an elastic medium, if such a medium exists, seems to be proved by these experiments. I once threw out the idea, suggested by the chemical action of light, that it consisted of particles of all the elementary bodies. Newton, as you know, in his letter to Boyle, combined particles with the ether in explaining refraction and inflexion.[38]

Clearly, Brewster's new hypothesis of the nature of light was not coherent. On the one hand, based upon those discoveries in the field of photography, Brewster believed that light consisted of particles, even of all elements of matter. On the other hand, in order to explain a variety of optical phenomena, he also needed the principle of interference, and consequently had to introduce the concept of elastic medium -- the physical base of vibrations and interference. Although Brewster conjectured that this elastic medium was not the ether but particles of all elements, he did not work out any detail about the interactions between the particles of light and the particles of the medium. By citing Newton in this letter, however, Brewster wanted to at least convince Brougham that such a combination of the particles of light and the particles of the medium was in principle possible.

Unfortunately, Brewster was never able to clarify his new hypothesis. In 1861, also in a letter to Brougham, Brewster stated:

> I have long entertained the rather extravagant notion that light, if corpuscular, contains all the elements of matter; and if the undulations of a medium, that this medium is composed of these elements of matter. The chemical actions of light upon bodies, and the effect of the combination of its elements with the elements of material bodies receive some explanation from such an hypothesis.[39]

Here Brewster became even more confused, because he could no longer decide whether

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[38] Brewster to Brougham, (March 10, 1858), University College Library, Brougham Collection, 26,703.

[39] Brewster to Brougham, (September 25, 1861), University College Library, Brougham Collection, 26,764.

light was particles or light was undulations. He attempted to develop a new emission theory of light that could absorb some advantages of the undulatory theory, but the hypothesis he came up with was so incoherent that even he himself was uncertain about its implication. At this point, Brewster's last attempt to revive the emission theory finally failed.

This predicament partly explains why Brewster almost completely withdrew from the field of physical optics and from the emission-undulatory controversy in the 1860s. In the last decade of his life, Brewster continued to immerse himself in optical research. From 1860 to 1868, he published about 40 papers in the field of optics. The majority of these papers, however, was on photography and physiological optics, while only four of them covered the topics of physical optics.[40] In this period, Brewster focused his attention to the "sanatory" aspect of light, studying the influence of light on life, particularly on human beings (Brewster 1866). In public he did not propose any version of the emission theory, nor did he raise any critique against the undulatory theory. He now seemed uninterested in the emission-undulatory controversy. The bravest warrior of anti-undulationism, who had been fighting against the undulatory theory for more than four decades, finally put down his spear, not because of his fear of the rivals' counter-attacks, but because of his own confusion and wavering.

3. Potter's New Emission Theory of Light

Besides Brewster, Potter was probably the only other person who continued working on the emission tradition in the late 1850s. In this section, I describe how Potter challenged the undulatory theory of interference, how he developed a new emission theory of light, and what kind of responses his new emission theory received.

[40] See Morrison-Low and Christie, (1984, pp.132-6).

3.1 The Principle of Interference

For a long time, Fresnel's double mirror experiment had been regarded by undulationists as an *experimentum crucis* to demonstrate that light was a kind of vibration. In this experiment, a beam of sunlight passing through a common lens and a small aperture fell on two slightly inclined mirrors, and the reflected rays of light from the mirrors produced interference fringes with a bright band on the center (Figure 7.6). However, Potter in 1840 reported that he obtained an entirely different result from a similar double mirror experiment. Instead of using a combination of a common lens and a small aperture to form a luminous point as Fresnel did, Potter in his experiment used only a spherical and convex lens to form a luminous point. This new arrangement, Potter claimed, could avoid all distortions caused by a common lens. With this slight change in experimental arrangement, Potter observed a dark rather than a bright band on the center of the interference fringes when the sky was unclouded. This finding even surprised Potter himself. He carefully reexamined his experimental arrangement, eliminating all elements that might resulted in distortions. He then asked friends who had a reputation as accurate observers to examine the appearances of the interference fringes, and they came to the same observational result. With these precautions, Potter finally announced that "I find, when the sun is perfectly unclouded, and near the meridian, *high above the horizon*, that the central band is black. When there are clouds before the sun disc, however slight, the central band is more difficult to fix upon, and generally either white or doubtful" (Potter 1840, p.384).

The existence of a central dark band in the interference fringes of the double mirror experiment, according to Potter, was certainly inconsistent with the undulatory theory, which predicted that the central band must be white. He later recalled that it was this experimental finding that shocked his confidence in the undulatory theory (Potter 1856, p.50). But Potter also stated that, although his experimental finding was "a fatal

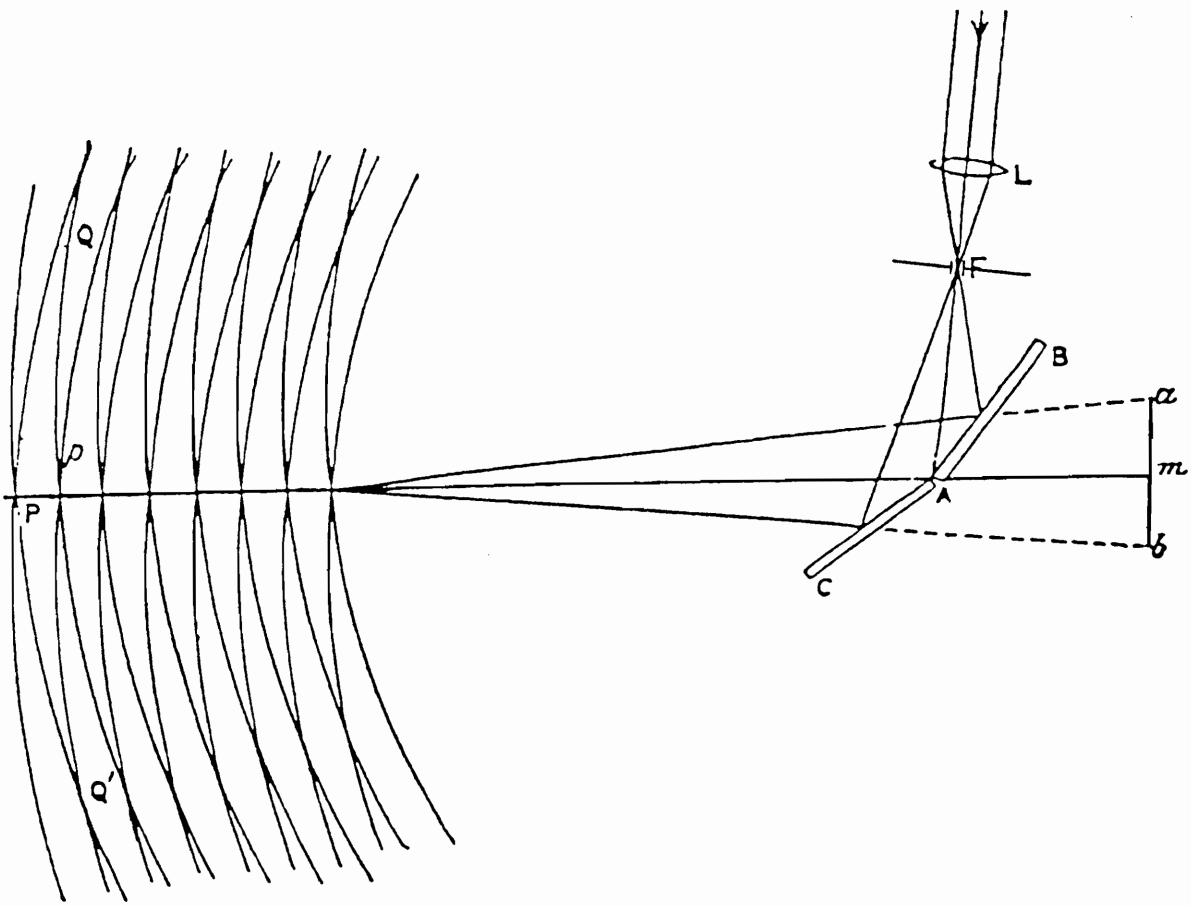

Figure 7.6 Fresnel's Experiment on Interference
[From Potter 1856, p.48]

objection" to the undulatory theory, "it would not weaken the demonstration of the theory of interference" (Potter 1840, p.381). The finding of his experiment, namely that the central band of the interference fringes was dark rather than bright, did not falsify the interference principle, because it could become consistent with the principle by introducing the following new assumption:

> When light in a state of interference is made to interfere again, the result is of an opposite character to what is would have been if the light had been in the first instance in the ordinary state. (Potter 1840, p.384)

This assumption, according to Potter, could eliminate all the anomalies. In the case of Fresnel's double mirror experiment, the light passing through a small aperture was thrown into a state of interference by diffraction at the edges of the aperture, and hence the central band was white, opposite to its ordinary character. Again, when sun light passed through thin clouds, or through the vapors of the atmosphere near the horizon, it was thrown more or less into a state of interference by diffraction at the edges of the particles of the vapors, and gave results that were either doubtful or with a white center.

Potter's assumption about interference was *ad hoc*. Although he insisted that this assumption "is in itself reasonable", Potter did not give any argument to support it. However, Potter's confidence in this assumption was further enhanced in his later experiment about interference near a caustic.[41] In this experiment (Figure 7.7), Potter placed a globule of mercury *d* about one inch in front of a concave spherical mirror *A*. Then sunlight came in the direction *Sd*, and formed an image of the sun in the globule, from which the reflected light would diverge and fall on the mirror *A*. The reflected light from mirror *A* formed a caustic at *f*, which had branches *fa* and *fb*, and interference fringes near the points *n* and *p*. With an eye lens, Potter observed that the outer ring of

[41] When a spherical surface is used as a mirror, the reflected rays do not all focus to a point; instead, their intersections form a luminous and curved surface called caustic.

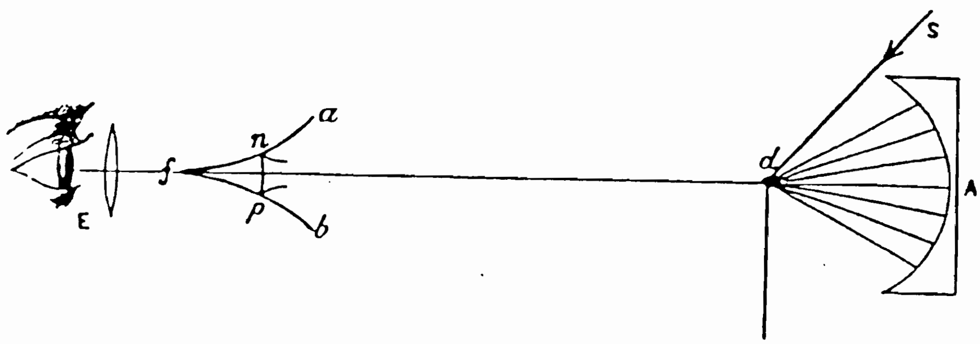

Figure 7.7 Potter's Experiment on Interference near A Caustic
[From Potter 1856, p.80]

the interference fringes shaded away gradually. This observation was also inconsistent with the undulatory theory, which predicted a brightest ring at the outer edge, since the rays there had travelled over equal spaces. Potter then concluded:

> I had fallen upon a confirmation of what I had before found in experiments with the two mirrors slightly inclined, namely, that the central band of interference, where the rays have travelled over equal distances, was in normal circumstances a *black* band and not a *bright* one, as it had been asserted to be by the late M. Arago and Professor Airy, in accordance with the undulatory theory of light. (Potter 1855, p.321; original emphasis)

Potter further confirmed this conclusion in his observation of the natural phenomenon of rainbows. He found that in the phenomenon of spurious rainbows, or called supernumerary rainbows, the positions where should be bright according to the undulatory theory turned out to be dark, the same result as in the interference experiments he conducted (*Ibid.*, pp.322-3).

On the basis of these experiments and observations, Potter announced that although the undulatory theory had led to the discoveries of some of the most important properties of light, it could not be the true theory. Potter realized that the explanatory successes of the undulatory theory had made it very popular. "The inertia of the previous century came now into effect again in favour of the undulatory theory of light, and the investigations which professed to confirm or advance it were extolled, whilst those which militated against it were rejected or met by assertion that at the utmost some subsidiary hypothesis might be needed" (Potter 1859, p.iv). Nevertheless, Potter was still confident about developing a new emission theory to replace the undulatory one. He believed that he would not commit the same mistakes as undulationists did, because of his nearly thirty-year experience in the field of physical optics. He even trusted that, by developing a new emission theory of light, physical optics could end its "wanderings" from the undulatory tradition, and then "by-and-by take a straight course of progress" (*Ibid.*, p.vi).

3.2 A New Emission Theory of Light

In 1856, Potter published the first volume of his *Physical Optics*, which, according to his own characterization, was "a descriptive, experimental, and popular treatise on physical optics" (Potter 1856, p.i). Three years later, he published the second volume of his *Physical Optics*, with a subtitle "The Corpuscular Theory of Light: Discussed Mathematically". In these two books, Potter endeavoured to develop a new emission theory of light that he hoped could replace the dominant undulatory theory.

The key feature of Potter's new emission theory of light was that, while it defined light as material particles, it also admitted that periodicity was an essential and inherent property of light. Potter stated that "we must recur to a corpuscular theory, with the principle of periodicity as an essential property of light" (Potter 1859, p.1). He developed such an idea from his experiments on interference, which he believed had falsified the undulatory theory but demonstrated the truth of periodicity. In order to combine the particle hypothesis and the property of periodicity, Potter had to put a series of new features into the particles of light. He remarked:

> [T]he luminiferous corpuscles must be considered as flying off in *surfaces*, *sheets*, or *shells* from luminous points, with intervals μ which are constant for the same colour of the solar spectrum, but which vary from colour to colour, ... (*Ibid.*; original emphasis)

By assuming that luminous sources emitted sheets of particles at regular intervals, which were identical with the concept of wavelength in the undulatory theory, Potter believed that he was able to incorporate the property of periodicity and then the principle of interference into his new emission theory.

But Potter soon found out that, in order to explain other optical phenomena in addition to those associated with the property of periodicity, he needed to assign more specific features to the particles of light. For example, to explain the phenomena of polarization, the particles of light had to have "poles, axes, and equators, similar to what are recognized in the particles of ponderable matter, and a beam is called a polarized beam when the axes of the luminiferous corpuscles are all parallel" (Potter 1859, p.2). Again, to explain the chemical and heat-making properties of light, the particle of light also had to be considered as "compounded of atoms of various subtle matter" (*Ibid.* p.26).

Since the particles of light were emitted at regular intervals, they might interfere with each other when they met in some special manner. Also, since the particles of light contained all elements of matter, the interferences between them probably would be of a different character to the mechanical interference of waves. Potter noted:

> If asked what would be the character of such interference, we may say it will be chemical, that is, it will involve higher and more varied mechanical principles than those supposed for vibratory and undulatory motions in an elastic medium. (1856, pp.45-6)

Based upon these new assumptions of the nature of light, Potter worked out some mathematical explanations of optical phenomena. Among them, his explanation of the interference pattern in double mirror experiments was most interesting.

The key of Potter's explanation was to account for how a dark rather than bright band at the center of the interference fringes could be produced in his double mirror experiment while a central bright band was produced in Fresnel's original double mirror experiment. In order to solve this problem, Potter distinguished a normal state and an anatropic state of interference. In the normal state of interference, the axes and the poles of the particles of light in two interfering beams were in parallel. In the anatropic state, however, the axes of the particles of light in two interfering beams were parallel, but their poles were opposed (Potter 1859, p.2). To calculate the intensity of the interference fringes, Potter used the traditional method of the parallelogram of forces. In Figure 7.8, *OA* and *OB* represent the intensities of the interfering lights, *OC* represents the intensity of the interfering light in the normal state, and *OD* the intensity

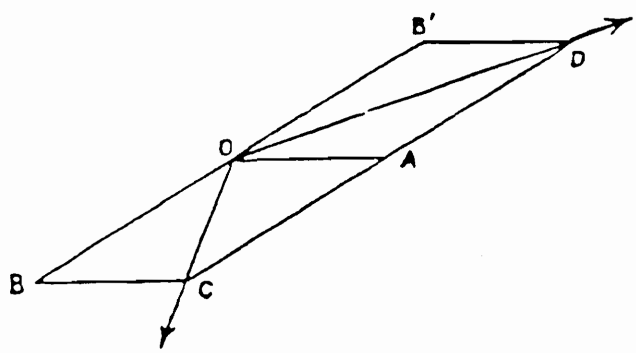

Figure 7.8 Potter's Method of Parallelogram
[From Potter 1856, p.74]

of the interfering light in the anatropic state. The angle between *OA* and *OB*, and the one between *OA* and *OB'*, were determined by the path difference δ of the two interfering light beams. But in order to explain why two light beams travelling over the same distance could produce a *dark* band, which was the normal state of interference by definition, Potter had to insert a constant of $\lambda/2$ into the formula for determining the angle between two interfering lights in the normal state of interference (Potter 1856, pp.74-5). He defined the angle θ between OA and OB as:

$$\theta=2\pi\frac{(\frac{\lambda}{2}\pm\delta)}{\lambda}$$

For the angle θ' between *OA* and *OB'*, namely, the angle between the interfering light beams in the anatropic state, Potter used the following formula:

$$\theta'=\frac{2\pi\delta}{\lambda}$$

Using these two formulas, Potter deduced the equations for the intensity of interference both in the normal state and in the anatropic state. In the normal state, the intensity of interference was:

$$I=I_1^2+I_2^2-2I_1I_2\cos\frac{2\pi\delta}{\lambda}$$

In the anatropic state, however, the intensity of interference was:

$$I'=I_1^2+I_2^2+2I_1I_2\cos\frac{2\pi\delta}{\lambda}$$

Here I_1 and I_2 were the intensities of the interfering light beams. Using these two equations, Potter finally obtained the results that, in his double mirror experiment that was in a normal state, the interfering intensity I was equal to 0 when $\delta=0$, and hence the central band was dark, while in Fresnel's double mirror experiment that was in an anatropic state, the interfering intensity I was maximal when $\delta=0$, and hence the central band was bright (Potter 1859, pp.39-51).

Potter's explanations of the double mirror experiments were obviously problematic. One of the maneuvers Potter tried in his explanations was to introduce a constant of $\lambda/2$ into the formula (1). Using the terminology of the undulatory theory, Potter here assumed a loss of half an undulation. But he did not give any analysis to show why the insertion of the constant of $\lambda/2$ was reasonable, nor did he establish any connection between his hypothesis of the nature of light and his *ad hoc* assumption. Furthermore, the loss of $\lambda/2$, if any, was more likely to occur in the anatropic state than in the normal state, because, according to Potter's own definition, in the former the poles of the particles of the interfering beams were opposite while in the latter they were parallel. Potter's explanations seemed inconsistent with his own hypotheses of the features of the particles of light.

Thus, from a modern viewpoint, Potter's new emission theory was a failure. Although he had endeavored to incorporate the principle of interference into his emission theory, hoping to explain more optical phenomena within the emission framework with the help of this powerful law, he failed to supply any new explanation beyond the domain of the undulatory theory. He could not even give a satisfactory explanation for double mirror experiments, the experiments that he used as a crucial test for any optical theory. It was not surprising that Potter's unsuccessful endeavor got very apathetic responses from the optical community. Even Brewster was strongly suspicious of Potter's work, although he praised Potter's courage in attacking the undulatory theory. In a letter to Brougham in 1860, Brewster expressed his doubt at Potter's ability to confront the undulatory theory, saying that "I do not understand how he [Potter] is to get rid of

Fizeau's crucial experiment in favour of the undulatory theory".[42]

In the eyes of undulationists, Potter's unsophisticated and even inconsistent work might have not create any threat. They apparently did not see the need to comment on Potter's work. In fact, Potter's book passed unnoticed in major scientific journals. Probably because of this embarrassing silence and probably because of the realization of his own failures, Potter was also disappointed and gave up his plan of reviving the emission theory of light. After the publication of his two volume *Physical Optics*, Potter never again conducted optical research. With the withdrawal of Potter, a stubborn emissionist who kept fighting against the undulatory theory for three decades, the long-term confrontation between the emission theory and the undulatory theory of light finally closed at the end of the 1850s.

Conclusion: The final campaigns of the emissionists in the 1850s were deeply shaped by the balance of power between the two rival theories of light. The powerful status of the undulatory theory forced emissionists to adopt a deliberate strategy in the campaigns. Brewster's recruitment of Brougham was an important part of this strategy. If Brewster had regarded cognitive consideration as overriding, the selection of Brougham as a new ally would have not made sense. In effect, Brewster might have realized that another ally, say, one fresh from university, would need to establish his credentials -- a difficult task for an emissionist when undulationists held much of the power. Even Potter, who was not a newly trained person and had a certain degree of scientific credentials, still failed to make his work heard. Brougham, however, was already a member of the Royal Society, and his social standing ensured a hearing and serious consideration.

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[42] Brewster to Brougham, (April 25, 1860), University College Library, Brougham Collection, 26,744.

Having selected Brougham, Brewster mapped the tactics for the debate he intended to create. Brougham was to demonstrate new experimental findings outside the province of the undulatory theory, and not to attack the theory directly. Brewster's tactics was to create a domain of experimental discourse in which undulationism was relatively weak, by elaborating new experimental findings that could not be explained by the undulatory doctrine. After the experimental findings were firmly established as respectable matters for scientific attention, the question of the adequacy of the undulatory theory could then be raised. Brewster here was clearly concerned that without this preliminary consolidation of the emissionists' position, each individual effort to undermine the undulatory theory could easily be defeated. In planing his tactics, Brewster subordinated his cognitive goals to social concerns -- who had power was more important than how the experiments turned out.

Although a good deal of the emissionists' strategy during the 1850s can be understood in terms of social concerns, two points prevent the use of social factors alone to account for the episode. First, the exercise of power in this episode was closely related to and even depended upon the results of cognitive appraisals.[43] For Brewster, the goal of suppressing cognitive concerns in the first stage of his tactic was eventually to establish an atmosphere in which the fate of the undulatory theory could finally be judged in terms of experimental findings. Also, it should be clear that the strategies of both sides were informed by their cognitive appraisal of the consequences of experimental findings for the undulatory theory, should it be allowed to stand. To claim that there was no more involved in the episode than an exercise in power, or to regard the exercise of power and the cognitive appraisals in this episode as completely incompatible, ignores features upon which historical actors would have agreed.

[43] It was contra Latour and Woolgar's proposal of a moratorium on the cognitive interpretation of science. See Latour & Woolgar (1986, p.280).

Although they adopted a deliberate strategy, emissionists failed to overthrow the dominance of the undulatory theory in their final campaigns. Accompanying these failures, the emission-undulatory controversy also came to its closure. It was clear that the closure of the emission-undulatory controversy was not caused by the counterattacks of undulationists, but by the resignation of emissionists.

In the case of Brougham, his work declined to the status of a non-issue with Brewster's withdrawal of support. Brewster's withdrawal was the result not of undulationists' counterattack but of emissionists' mistakes -- Brougham's divergence from Brewster's tactic for the campaign. On the second occasion of Brougham's disobedience Brewster correctly predicted the fate of Brougham's work, and withdrew from public prosecution of the case. Brougham was henceforth dismissed, and his disappearance from public view marked the end of the debate on his experiment on diffraction.

In the case of Brewster, he only mentioned his universal emission theory in private conversation, not because there was any objection to it from the rivals, but because the strategy he had designed for the final campaign prohibited the publication of such speculation. Later, when Brewster attempted to incorporate some undulatory doctrines into the emission framework, he encountered a conceptual predicament on the nature of light, which finally made him voluntarily withdrew from the emission-undulation controversy.

Similarly, in the case Potter, his withdrawal from the controversy did not result from rivals' criticism. In fact, undulationists did not make any comment on Potter's work. It seems that Potter's withdrawal mainly resulted from his failures in developing coherent and sophisticated accounts for the key optical experiments. Thus, it was emissionists who voluntarily resigned from the controversy. With their own hands, emissionists finally ended the life of their theory as one of the competitors in the field of physical optics.

Chapter 8
The Closure of the Controversy

In the previous chapters, I have provided the details of the long-term controversy between emissionists and undulationists in nineteenth-century Britain. Some of the characteristics of the emission-undulatory controversy are worth emphasizing. First, when the undulatory theory was first introduced into Britain in the late 1820s, there was no immediate confrontation between emissionists and undulationists, and both sides at the beginning had many points of agreement in their evaluations of the rival theories; the first confrontation between the two camps did not occur until the early 1830s. Second, the undulatory theory established its dominance in Britain in the mid-1830s, not only through its successes in explanation and prediction, but, more importantly, through its supporters' control of the major British scientific societies and universities. Third, emissionists did not immediately surrender in the late 1830s after the undulatory theory became dominant; by throwing up all kinds of experimental results that the undulatory theory could not explain, emissionists launched a series of attacks, which successfully brought about considerable confusion in the undulatory camp. Fourth, the long-term controversy finally came to closure at the end of the 1850s, not by the victory of the undulationists, but by the voluntary resignation of the emissionists.

After spending several chapters in describing how the emission-undulatory controversy began in Britain in the early 1830s, how it continued for three decades, and how it came to closure at the end of 1850s, it is time for a theoretical discussion of the underlying mechanisms of this controversy. Several "why" questions can be raised from this historical episode, which will interest not only historians but also philosophers and sociologists of science. First, why did the emission-undulatory controversy last such a long period after the fundamentals of the undulatory theory had been formulated in the 1820s? Second, why did the controversy come to closure at the end of the 1850s? And third, why did the controversy end in such a peculiar way, namely, the leading emissionists voluntarily resigned from the controversy and ended the life of the emission theory of light with their own hands.

The main purpose of this chapter is to look for the answers to these questions, providing a theoretical interpretation of the emission-undulatory controversy in early nineteenth- century Britain. In effect, the emission-undulatory controversy was an episode in which the undulatory theory replaced the emission theory, and the closure of this controversy amounted to the most complete kind of scientific change, or scientific revolution. In the following sections, I first review several theories of scientific change, both from the philosophy of science and from the sociology of science, to see if they can shed light on our historical episode. After examining the problems of previous theoretical interpretations, I provide an alternative analysis of the underlying mechanisms in the emission-undulatory controversy. I particularly concentrate on two historical processes: the influence of Humboldtian science and the emergence of a new generation of physicists. These two factors were responsible both for the longevity of the emission-undulatory controversy as well as for the timing and the features of its closure, but they cannot be put into the categories of cognitive or social factors. The traditional dichotomy accepted by both philosophers and sociologists of science is not appropriate here. These conclusions suggest that a new historiography is needed for understanding the emission-undulatory controversy.

1. Theoretical Interpretations of Closure

Both the philosophy of science and the sociology of science provide a large number of theoretical models for scientific change as well as for the closure of scientific debate. In this section, I try to apply these models to the episode of the emission-undulatory controversy, to see whether they provide a meaningful interpretation of historical events. Specifically, I focus on whether these theoretical models can explain the longevity of the controversy, as well as the timing and the features of its closure.

1.1 Interpretations from the Philosophy of Science

Philosophers of science have spilled much ink in explaining scientific change and scientific controversy. Despite the differences in the details of their models, all philosophical interpretations of scientific change and scientific controversy attribute the causes of the closure of a scientific controversy, or, the completeness of a scientific change, to some cognitive or intellectual features of scientific theories. In Lakatos' model, for example, a scientific change is complete and the corresponding controversy ends if and only if one of the research programs in the controversy becomes progressive while its rival degenerates. According to Lakatos' criteria, whether a research program is progressive or degenerating is entirely determined by its ability to make successful explanations and predictions (Lakatos 1978, p.32). Thus, the closure of a scientific controversy or the completeness of a scientific change is determined entirely by the features of the relevant theories and their relationships with empirical evidence, which Lakatos classifies as the "internal factors" of science. In other words, the closure of a controversy or a scientific change seems to have nothing to do with the activities of scientists who are involved in the controversy or the transition process, except to the extent that they generate theories and empirical evidence. We do not even need to ask whether the scientists who take part in the controversy and the transition process really follow the purported criteria before we declare that the scientific change or the closure of the controversy is "rational".

A similar rationale also appears in Laudan's model of scientific change. In Laudan's early view, a scientific change should come to its closure when one of the rival theories, or "scientific traditions" in his terms, exhibits a higher degree of problem-solving ability (Laudan 1977, p.109). Unlike Lakatos, Laudan does not limit himself to a theory's ability to make successful explanations and predictions, and expands the notion of problem-solving ability to cover a theory's ability to reduce conceptual problems. However, as Laudan explicitly states, the purpose of enlarging the list of a theory's features is to ensure that scientific change is always determined by the "rational" or "internal" factors of science (*Ibid.*, pp.3-5).

Such an internalist rationale is further emphasized in Shapere's model of scientific change. Shapere holds that scientific change or scientific progress is a process of internalization, which means that a theory has shown itself as successful and free of doubt without appeal to "external" considerations (Shapere 1984, p.651; 1987, p.15). We then should expect that a scientific controversy or a scientific change comes to an end when one of the rival theories clearly demonstrates its ability to get rid of all external elements in resolving problems.

When we apply these philosophical models of scientific change into our historical episode, it appears that the victory of the undulatory theory immediately becomes comprehensible. It seems that we can attribute the causes of this scientific change to the cognitive superiority of the undulatory theory, that is, either its ability to make successful explanations and predictions, or its internal coherence. Furthermore, in the light of these philosophical models, the longevity of the emission-undulatory controversy also becomes understandable and predictable. In Lakatos' model, for example, a degenerating research program does not disappear immediately and may revive at any time through a burst of "heuristic power". The stubborn defenders of a degenerating research program may continue to resist and dream of a comeback for a long time. Therefore, there is no "instant rationality" in scientific change (Lakatos 1978, p.31, 71-2). Similarly, Laudan also believes that a scientific change cannot be completed instantly or simultaneously.

By incorporating theories, methods, and aims into a reticulated model, Laudan argues that a scientific change is completed in a piecemeal fashion (Laudan 1984, pp.73-9). The replacement of an old tradition or paradigm by a new one is realized through a temporal sequence of changes, say, first a replacement in theory, then in method, and finally in aim. Consequently, we should expect that a scientific change consists of a sequence of gradual shifts and the corresponding controversy may last a considerable period.

If we only ask very general questions about the emission-undulatory controversy, such as why the undulatory theory could replace its rival, all the above philosophical accounts of scientific change can give us satisfactory interpretations. However, if we get into the historical details, the explanatory power of these philosophical models immediately disappears. According to Lakatos' model, for example, all a degenerating research program needs for a comeback is to produce a new "content-increasing" theory, some of the novel predictions of which are verified. If we accept this model, we would expect that the stubborn defenders of the degenerating emission theory would draw new predictions from their own theory and that the focus of the controversy would be the verification of these predictions. But this was not the case in the emission-undulatory controversy. Here the focus of the emissionists' defending actions, especially Brewster's, Potter's, and Brougham's works from the late 1830s to the 1850s, was to present observational and experimental findings that their rivals could not explain, rather than to verify new predictions drawn from their own theory.

For Laudan's model, the problem is his expectation that the focus of a scientific controversy would always be the comparison of the rival theories' problem-solving abilities. This expectation is confirmed by some of the arguments the historical actors presented in the early phase of the controversy, such as in Brewster's and in Lloyd's reports on the status of optics in the early 1830s. But as I have described in chapter five and seven, the focus of the controversy gradually changed after the mid-1830s, from theory appraisal to experiment appraisal, namely, from comparing rival theories' problem-solving abilities to examining the reliability and validity of the experimental results that were employed in theory testing.

Again, for Shapere's model, the problem is his requirement of internalization, which obviously cannot cover those "external" or contextual factors that played very important roles in determining the results of the emission-undulatory controversy. For example, it is extremely difficult for Shapere's model to explain the undulatory victory in the mid 1830s, which, as I described in chapter four, involved considerable "external" factors.

1.2 Interpretations from Sociology of Science

Recently sociologists of science also became interested in the problems of scientific controversy and scientific change, and supply a variety of theoretical models to explore their causes. Unlike philosophers, sociologists of science tend to attribute the closure of a scientific controversy or the completeness of a scientific change to social or contextual factors rather than any internal features of theories. According to the Edinburgh School, for example, a scientist or a scientific community accepts one theory and rejects its rival simple because the former is able to "intersect" the interests of the scientist or of the scientific community. For example, one can speak of the scientist or the scientific community having an expertise relevant to the articulation of some phenomena as having an "investment" in the expertise, or, as having an "interest" in the deployment of their expertise in articulating the phenomena. Since scientists have diverse expertise, they always have their distinct "interests" in developing different theories, in which their expertise can be deployed as much as possible. Thus, a scientific controversy ends or a scientific change is complete because one of the rival theories can "intersect" with the interests of the majority of a scientific community and then is accepted, while its rival fails to do so and then is rejected (Pickering 1981, pp.120-43).

In Edinburgh School's model, scientific theories can still have a role in affecting the closure of a scientific controversy or a scientific change by intersecting with scientists' interests, but in the relativist program, the closure is entirely determined by non-scientific factors. The basic assumption of the relativist program is that what counts as the truth can vary from place to place and from time to time, so that empirical data have no meaning outside an interpretative context. This interpretative flexibility is bound to produce endless debates on the meaning of all empirical findings. If the limitless debates are to end, they must be brought to a closure by some mechanisms not usually regarded as strictly scientific, which include various rhetorical and institutional devices (Collins 1983, p.95,99).

For the actor-network theory, the closure of scientific controversy is determined not by general non-scientific elements, but, specifically, by political factors. Latour argues that in a scientific controversy actors will first employ a series of rhetorical techniques. If the rivals continue to resist, the controversy will become more "technical" and more "social" in the sense that the actors appeal to established technical artifacts and mobilize allies. In this way, the participants in the controversy build up their actor-networks, which contain entities we are accustomed to call "objects" and "people". In the eyes of Latour, these non-human objects, as well as people, are actually political beings used as allies to win a war. The controversy finally ends when one side is successful in building its actor-network while the other side fails to do so (Latour 1987).

These sociological interpretations of scientific controversy and scientific change surely shed light on our case of the emission-undulatory controversy. A variety of social, political and rhetorical factors in the controversy become understandable in the light of these sociological models. Moreover, the longevity of the controversy is also predictable in terms of the sociological theories. For example, Collins argues that knowledge changes emerge as products of negotiations among key actors. Consequently, a scientific change cannot come to its closure before a long process of negotiation, in which scientists argue with each other on topics from the interpretation of experimental findings to methodological standards (Collins 1981). Similarly, having used many expressions with military connotations (like "struggle", "strategy", "power", and "ally"), Latour argues that science, or "technoscience" as he puts it, "is part of a war machine" (Latour 1987, p.172). In this way, he interprets successful scientific theories as manifestations of the triumph of the winners' wills. Thus, long-term persistence and fierce counter-attacks from the losing side are expectable and it is unlikely that any scientific controversy will come to a quick end.

However, by interpreting science as a process of negotiation or even as a part of a war machine, Collins' and Latour's sociological accounts of science imply that scientific controversy itself always is inevitable. This point is not consistent with our historical episode, in which controversy is a mode of scientific practice that actors may choose to employ or avoid. Also, if we ask a specific historical question about the emission-undulatory controversy, say, why did the controversy crease *at the end of the 1850s*?, the sociological interpretations immediately get into trouble. For example, if we accept Pickering's model and use the intersections with scientists' interests to explain the episode, the emission-undulatory controversy should have ended thirty years earlier than it actually did, because the intersection between the undulatory theory and the interests of the Cambridge School occurred as early as the end of the 1820s. Also, if we follow Latour and employ the concept of actor-networks to interpret the case, the controversy should have come to its closure two decades earlier than it actually did, because the undulationists had successfully established their actor-network by the mid 1830s. For Collins' model, the problem is how to explain the most direct factor that brought about the closure, the voluntary resignation of emissionists at the end of the 1850s. Collins' model may be able to interpret the undulationists' silence toward Potter's books as a rhetorical device. But it just makes no sense at all in this theoretical model for emissionists to put down their weapons voluntarily.

The difficulties of both the philosophical accounts and the sociological accounts in explaining the emission-undulatory controversy indicate that we cannot limit ourselves merely to either internal factors or contextual factor. Instead, we should play attention to the more general historical background upon which the scientific change and the scientific controversy in question proceeded. In this aspect, there were two historical processes that we cannot afford to ignore: the popularity of Humboldtian science in the field of physics in the early nineteenth century Britain, and the emergence of a new generation of physicists at mid century.

2. Optics and Humboldtian Science

Cannon in his book *Science in Culture* first identified the existence and popularity of a "Humboldtian spirit", or Humboldtian science, in Britain during the first half of the nineteenth century (Cannon 1978, pp.73-110). In this section, I examine the impact of Humboldtian science on optics and, in particular, on the emission-undulatory controversy. After briefly introducing the key features of Humboldtian science, I concentrate on two important phenomena closely associated with the popularity of Humboldtian science in early nineteenth-century Britain: the shortage of scientific manpower and the focus on interrelation. I argue that these two phenomena were responsible for the longevity of the emission-undulatory controversy.

2.1 Alexander von Humboldt and Humboldtian Science

Alexander von Humboldt (1786-1859), a German naturalist and scientific traveller, was a very important figure in the development of physical geography and biogeography. He developed strong interests in botany, mineralogy, and geology when he received his college education. Collecting plant and mineral specimens became his hobby. But the countryside of Germany did not give him much stimulus, and he soon dreamed of journeys to more exotic lands. In 1798, he obtained permission from the Spanish government to make an expedition to the Spanish colonies in Central and South America. On June 5, 1799, he was bound for South America, beginning a five-year scientific expedition.

Between 1799 and 1804, Humboldt traveled a wide expanse of the Central and South American continent, from Venezuela to Colombia, then Ecuador and Mexico. Covering more than 6,000 miles on foot, on horseback, and in canoes, Humboldt had the unique opportunity to study a new world in detail. Unlike earlier explorers, Humboldt in his expedition not only recorded novel phenomena but also conducted very accurate measurements. To make these measurements, he carried a most impressive list of instruments, including two chronometers, two telescopes, two microscopes, two sextants, one theodolite, one graphometer, one magnetometer, four barometers, several thermometers, two hygrometers, two eletrometers, one cyanometer, and many more.[1] Armed with the latest instruments, Humboldt was able to measure accurately a large range of natural phenomena that either were unknown or had been inaccurately reported by earlier explorers. When he returned, he brought back an enormous amount of information. In addition to a huge collection of new plants, there were determinations of longitudes and latitudes, measurements of the earth's geomagnetic field, observations of temperatures and barometric pressure, and statistical data on the social and economic conditions of Mexico (Kellner 1963, pp.3-65).

Humboldt's American expedition set up an example of his research style. According to Cannon's summary, Humboldt's style, or Humboldtian science, had the following features:

[1] For a complete list of the instruments that Humboldt used in his expedition, see Humboldt (1814-29, vol.1, pp.32-9).

1. A new insistence on accuracy, not for just a few fixed instruments, but for all instruments and all observations. 2. A new mental sophistication, expressed as contempt for the easy theories of the past, or as taking lightly the theoretical mechanisms and entities of the past. 3. A new set of conceptual tools: isomaps, graphs, theory of errors. 4. An application of these not to laboratory isolates but to the immense variety of real phenomena, so as to produce laws dealing with the very complex interrelationships of the physical, the biological, and even the human. (Cannon 1978, p.104)

After finishing his American expedition, Humboldt in 1814 published a three-volume book in French to describe his discoveries. This book was expanded to seven volumes in the English translation, which was published from 1814 to 1829 under the title *Personal Narrative of Travels to the Equinoxial Regions of the New Continent during the Years 1799 - 1804*. British readers were immediately impressed by the tremendous number of detailed and accurate descriptions Humboldt had given in his book. One reviewer in the *Quarterly Review* wrote:

> [Humboldt's] great merit, however, is that of seeing every thing, and leaving nothing unsaid of what he sees; - not a rock nor a thicket, a pool or a rivulet, - say, not a plant nor an insect, from the lofty palm and the ferocious alligator, to the humble lichen and half-animated polypus, escapes his scrutinizing eye, and they all find a place in his book. (Anon. 1817-8, p.136)

Beginning in the late 1820s in Britain, the admiration for Humboldt's research style caused an explosion of interest in the studies of the earth and its environment. These studies followed Humboldt by accurately and systematically measuring data and producing quantitative relations often in the forms of charts and graphs. The subjects of these studies included tidology, meteorology, terrestrial magnetism, physical geology, fossil zoology, marine biology, and the works of astronomical reduction,[2] all of which can be categorized under the title of "Humboldtian science" (Morrell & Thackray 1981,

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[2] The aim of astronomical reductions was to systematize the computations involved in reducing the apparent places of the fixed stars to their mean places, and to produce tables showing the corrections to be made for the aberration of light, for precession, and for nutation (Morrell and Thackray 1981, p.510)

pp.512-3).

In addition to his American expedition, Humboldt's work in organizing scientific societies and scientific conferences also deeply impressed British "men of science". In 1828, Humboldt in Berlin organized an international scientific conference, the meeting of the Society of Naturalists and Natural Philosophers. The meeting was very successful, with more than six hundred people present. At the opening session Humboldt as the president of the meeting gave an address. He stated:

> The main purpose of this society is the personal contact of men who work in the same field, an oral and thus more stimulating exchange of ideas, they may be facts, opinions or doubts; the forming of friendly relations which light up the sciences, give charm of life and gentleness and tolerance to intercourse . . . (Kellner 1963, p.119)

Among Humboldt's audience was Charles Babbage, Lucasian professor of mathematics at Cambridge. Babbage was impressed by the success of the conference, and particularly inspired by Humboldt's speech. He immediately translated Humboldt's speech into English, and published it with a report of the conference in the *Edinburgh Journal of Science* in 1829 (Babbage 1829). Among some British "men of science", like Babbage and Brewster, Humboldt's views on science and his success in organizing a scientific society induced a strong interest in establishing a scientific institution like the one in Germany. Humboldt's example became one of the main factors leading to the founding of the British Association for the Advancement of Science.[3] Thus, it is not surprising that the first meeting of the British Association in 1831 was modeled on the Humboldt's conference.

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[3] There are several different opinions on how the British Association was founded, but all of them agree that the successful exemplar set up by Humboldt was one of the important factors. See Foote (1951), Williams (1961), Morrell (1971), Orange (1971), and Cannon (1978, pp.181-96).

2.2 The Shortage of Scientific Manpower in Optics

The influence of Humboldt and Humboldtian science significantly shaped the research pattern in the British Association when it was founded in 1831. In fact, in the Mathematics and Physical Sciences Section of the Association, researches that belonged to Humboldtian science constituted the main stream for a long period. The most vivid example of Humboldt's influence on research in the British Association was the "magnetism crusade", a project aiming to establish a global network of geomagnetic observatories. Inspired by Humboldt's observations and measurements of terrestrial magnetism in the first two decades of the century, Lloyd, Herschel, and Edward Sabine advocated the importance of large scale geomagnetic measurements, typifying the Humboldtian passion for data collecting and accurate measuring. In the mid-1830s, the advocates of the geomagnetic research project began to lobby the British government, asking it to supply financial support for building stationary observatories in the southern polar region. Among the supporters of the magnetic lobby, we find Lloyd, Herschel, Whewell, Airy, and Forbes, the principal members of the undulatory camp. The first attempt of the magnetic lobby failed, primarily because the Royal Society at the time did not endorse the project. Upon the request of Sabine, in 1836 Humboldt wrote a letter to the Royal Society, asking the Society's and the British government's support in establishing magnetic observatories in the colonies. Because of the prestige of Humboldt, the Royal Society changed its position on the project of geomagnetic measurements, and, together with the British Association, successfully persuaded the government to supply the financial support (Cawood 1979, pp.494-505).

In addition to the project of geomagnetic measurement, the Humboldtian science pursued by the members of the British Association also included tidology, meteorology, physical geology, and astronomical reductions. The works on tides, mainly done by Whewell and John Lubbock, consisted of the observations of the variations of tides in different locations, the effects of the moon's declination and parallax on tides, and the effects of solar parallax and barometric pressure on tides. The researches on meteorology, pursued by Forbes and William Harris, included projects on atmospheric waves, on meteorological observations, and on improving the instruments of meteorological measurements. And the astronomical reductions conducted by Airy and Francis Baily produced star and planet catalogues that provided corrected positions of a large number of fixed stars and planets (Morrell & Thackray 1981, pp.509-17). All of these works requiring large scale observations, tremendous data collections, and laborious calculations, typified the research style practiced by Humboldt.

Because of the scale of work demanded by Humboldtian science, the British Association in its the first fifteen years spent a large amount of its financial resources in the form of research grants to support those projects inspired by the Humboldtian style. The distribution of research grants in the British Association was very disproportionate. Although the membership ran into thousands, 14 people received over half of the research money, £6,681 out of £11,784 during the period from 1833 to 1844 (Morrell and Thackray 1981, p.551). Among these 14 research projects supported by the Association, eight were for Humboldtian works, including astronomical reductions, tidology, meteorology, natural history, and marine biology. In terms of money, these eight Humboldtian projects consisted of over 70 percent of the grants that these 14 major grantees had drawn. Referring to the grant distribution by Section, the largest fraction (70.8%) of these 14 major grants went to the Mathematical and Physical Sciences Section (Section A). Similarly, Humboldtian projects counted for over 70 percent of the research money drawn by the major grantees in the Section A (Table 8.1).

The investment in Humboldtian science induced a large number of research papers in these fields. In the Section A of the British Association, papers on Humboldtian science, which included tidology, meteorology, terrestrial magnetism, astronomical

Table 8.1 Major Research Grants in the British Association, 1833-44
[Source: Morrell & Thackray 1981, p.551]

14 major grantees	Section	Subject	Amount(£)	Percent
Humboldtian Science:				
Francis Baily	A	astronomy reductions	1298	19.4
William Whewell	A	tidology	987	14.8
William Harris	A	meteorology	624	9.3
Richard Owen	D	natural history	618	9.3
John Lubbock	A	tidology	577	8.6
Jean Agassiz	D	natural history	520	7.8
John Herschel	A	meteorology	115	1.7
Edward Forbes	D	marine biology	100	1.5
Subtotal:			4839	72.4
Non-Humboltian Sciences:				
John Russell	A	waves and the contours of ships	1132	16.9
Eaton Hodgkinson	G	mechanical properties of iron	360	5.4
William Harcourt		fabrication of glass	176	2.6
Richard Taylor		printing of the annual reports	174	2.6
Subtotal:			1842	27.6
Total			**6681**	**100.0**

reductions, and physical geology, dramatically increased after the mid 1830s. Between 1837 and 1849, papers on Humboldtian science actually outnumbered those on the traditional physical sciences, including dynamics, optics, heat, electricity, and magnetism (Table 8.2). During this period, works on Humboldtian science became the major focus of the Section -- papers on Humboldtian science constituted about a half of the works presented in the Section, while papers on physical science dropped to about 30% (Figure 8.1). This high density of publication in Humboldtian science would have required that the members of the Association spend a substantial amount of time in these fields. Humboldtian science usually required laborious measuring, counting, reducing, tabulating, and graphing of data. The expenditure of manpower in these fields would have been correspondingly great. Thus, Humboldtian science not only drained a large portion of the financial resources of the British Association, but, more importantly, a considerable amount of the manpower of the scientific community.

In the first half of the nineteenth century, the opportunities for scientific research in Britain were very limited. Without support from the government, positions for scientific research were only available at universities and in industry. However, professorships in natural sciences at British universities were few, and openings for scientists in industry were rare, probably excepting the chemical industry. For many people with scientific training, moving to the Dominions and South American countries was more attractive than remaining in Britain, and this brought about a "brain-drain" of scientific manpower (Roderick & Stephens 1974, p.58). Under these circumstances, a shortage of scientific manpower appeared in the field of physics (Cannon 1978, p.126). Very few people were capable of doing physical investigations. Those who were qualified had diverse interests, covering fields not only within but outside the domain of physics. Through further diversifying the interests of the researchers, the popularity of Humboldtian science made the shortage of scientific manpower in physics even worse.

Table 8.2 Papers presented in Section A, 1831-55
[Source: *Report of the British Association*, 1831-55]

	Mathematics	Astronomy	Physics	Humboldtian	Others	Total
1831	-	-	10	4	5	19
1832	2	1	13	7	-	24
1833	1	-	17	2	3	23
1834	2	2	12	8	-	22
1835	3	-	15	9	2	29
1836	5	-	17	10	4	36
1837	3	3	10	16	3	35
1838	2	3	8	17	4	34
1839	1	1	13	18	2	35
1840	1	2	15	23	5	47
1841	4	-	8	15	2	29
1842	3	2	13	14	3	35
1843	2	2	10	19	3	36
1844	5	4	12	17	3	41
1845	3	7	10	18	-	38
1846	1	2	14	18	2	37
1847	4	9	22	25	7	67
1848	-	3	17	19	-	39
1849	-	3	18	28	4	53
1850	1	3	21	11	1	37
1851	-	4	18	6	-	28
1852	-	3	20	14	4	41
1853	1	6	13	10	-	30
1854	3	6	20	14	3	46
1855	3	4	24	14	2	47

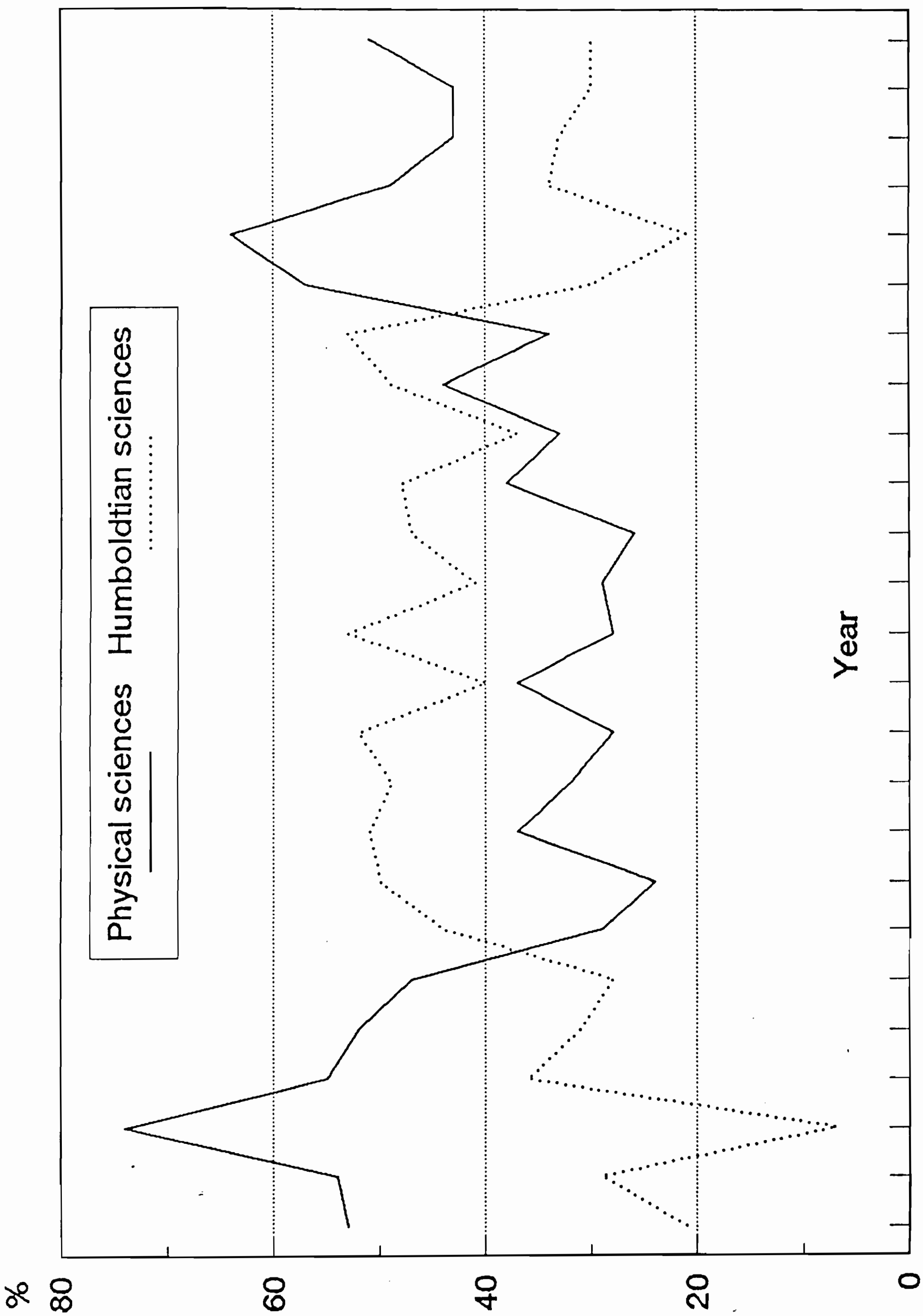

Figure 8.1 Distribution of Papers Presented to Section A, 1831-55

In the field of optics, for example, only about one dozen people were involved when the emission-undulatory controversy began, three on the emission side and about ten on the undulatory side. With the exception of Brewster and Powell, most participants only spent a slight fraction of their time in optics. Consequently, the number of the publications on optics was not very high, with an average of about ten papers each year in the 1830s. When Humboldtian science became the new fashion, the time and manpower spent in optics inevitably decreased. This produced a great impact on the emission-undulatory controversy, especially on the undulatory camp. After 1835, most undulationists were no longer interested in optical phenomena. Except in responding to rival attacks, they seldom conducted original optical researches, and the number of their publications on optics also dropped significantly (Table 8.3). After the mid 1830s, the most influential undulationists who played critical roles for the establishment of the undulatory dominance in Britain, such as Herschel, Airy, and Lloyd, turned their attention to the new Humboldtian science. In the period between 1835 and 1849, for instance, Lloyd published 33 papers on Humboldtian science, but only four about optics. For Airy, he maintained his interests in astronomy, but, nevertheless, his papers on Humboldtian science in the 1840s still outnumbered those on optics -- most of them (six out of seven) were merely reactions to Brewster's and Potter's attacks. Similarly, Herschel published nine papers on Humboldtian science in contrast to two on optics in the whole 1840s (Table 8.4). In fact, after 1835, only two undulationists, Powell and MacCullagh, maintained their focus on optics, and the latter, unfortunately, died young in 1847.

Despite the undulatory theory having a relatively large number of supporters in Britain, after 1835 the actual scientific manpower spent on its development was very limited. Thus, the undulatory theory was relatively stagnant, in comparison to its achievements in the first half of the 1830s, not to mention those achieved by Fresnel in

Table 8.3 Optical Papers by Older-Generation Undulationists, 1830-1859
[Source: Royal Society of London 1867]

	1830-34	1835-39	1840-44	1845-49	1850-54	1855-59	Total
George Airy	10	1	4	3	-	-	18
James Challis	3	-	-	8	1	6	18
James Forbes	-	4	-	1	1	-	6
Willaim Hamilton	6	2	5	-	-	1	14
John Herschel	1	-	-	2	-	-	3
Humphrey Lloyd	2	2	2	-	-	1	7
James MacCullagh	4	9	7	1	-	-	21
Baden Powell	4	23	18	8	2	2	57
Willaim Talbot	5	5	-	1	-	-	11
William Whewell	1	-	-	-	1	-	1
Total	**36**	**46**	**36**	**24**	**5**	**10**	**155**

Table 8.4 Papers on Humboldtian Sciences by Older-Generation Undulationists, 1830-59
[Sources: Royal Society of London 1867]

	1830-34	1835-39	1840-44	1845-49	1850-54	1855-59	Total
George Airy	-	1	4	5	1	2	13
James Challis	-	1	-	-	-	-	1
James Forbes	9	3	17	16	5	5	55
Willaim Hamilton	-	-	1	-	-	-	1
John Herschel	2	2	8	1	-	3	16
Humphrey Lloyd	-	13	14	6	8	5	46
James MacCullagh	-	-	-	-	-	-	-
Baden Powell	-	-	1	3	7	5	16
Willaim Talbot	-	-	-	-	-	-	-
William Whewell	2	17	4	5	2	-	30
Total	**13**	**37**	**49**	**36**	**23**	**20**	**178**

the 1820s. Up to the late 1840s, for example, the undulatory theory still had difficulties in explaining a variety of optical phenomena, especially those deliberately uncovered by emissionists, such as the "polarity of light" discovered by Brewster and the experiments on diffraction conducted by Brougham. Consequently, the stagnancy of the undulatory theory not only gave emissionists chances to continue their counter-actions, but also stirred up discontent among some undulationists. As we saw in chapter six, these undulationists' doubts about their own theory further gave emissionists an illusion that the undulatory theory could be defeated and the emission theory could be revived. In this way, the life of the emission-undulatory controversy was significantly lengthened. Thus, the popularity of Humboldtian science and the subsequent shortage of scientific manpower in optics produced one of the cognitive conditions that made a long-term emission-undulatory controversy possible.

2.2 The Focus on Interrelation

In addition to systematic and accurate measurements, another very important characteristic of Humboldt's research style was his interest in complex inter-relationships. According to Humboldt, one could not gain the laws of complex subjects by direct deductions from established theories. Science "can only progress by individual study, and by the bringing together of all the phenomena and creations which the surface of the earth has to offer. In this great sequence of cause and effect, nothing can be considered in isolation. . . . [T]he study of nature, . . . , requires the bringing together of all the forms of knowledge which deal with modification of matter" (Humboldt 1807, pp.42-3).[4] A good example of these complex subjects was the phenomenon of tides. Although this was a subject in which the Newtonian laws could be applied, the complex conditions, such as depth of water, width of channels, and shape of land, made direct deductions

[4] Translated and cited in Nicolson (1987, pp.176-7)

from the Newtonian laws impossible. To study these complex subjects, Humboldt insisted, the most important thing should be an insight into the interrelation of phenomena, which could not be obtained by appeal to any single branch in existing sciences. No complex subject was comprehensible merely by physics or by chemistry or by biology. Humboldt's new approach was to apply all the appropriate theories or laws drawn from not one but several existing disciplines. An example of his multi-disciplinary study was his "geography of plants". According to Humboldt's own definition, "plant geography traces the connections and relations by which all plants are bound together among themselves, designates in what lands they are found, in what atmospheric conditions they live, and tells of the destruction of rocks and stones by what primitive forms of the most powerful algae, by what roots of trees, and describes the surface of the earth in which humus is prepared".[5] Thus, such a "geography of plants" incorporated theories and laws drawn from the fields of classification, vegetable physiology, climatology, mineralogy, etc.

When the Humboldtian style became a new fashion in Britain, Humboldt's emphasis upon complex interrelations was also appreciated by some British "men of science" such as Brewster. Brewster's publication pattern (Table 8.5) indicates that he did not limit his optical interests to the narrow field of traditional physical optics, which included such topics as reflection, refraction, dispersion, diffraction, double refraction, and polarization. Instead, Brewster's optical interests spread widely into a series of complex optical phenomena, which were not covered by traditional physical optics. The comprehension of these complex optical phenomena required knowledge drawn from both traditional physical optics and from other disciplines, ranging from spectroscopy and mineralogy to astronomy, photochemistry, and physiology. In fact, physical optics

[5] Cited in Nicolson (1987, pp.174-5).

Table 8.5 Optical Papers by Brewster, 1830-59
[Source: Morrison-Low and Christie 1984]

Subjects	1830-34 No.	1830-34 %	1835-39 No.	1835-39 %	1840-44 No.	1840-44 %	1845-49 No.	1845-49 %	1850-54 No.	1850-54 %	1855-59 No.	1855-59 %	Total No.
physical optics	13	37%	7	35%	11	28%	8	28%	2	11%	2	11%	43
optics & spectroscopy	2	6%	-		5	12%	2	7%	-		2	10%	11
optics & mineralogy	8	23%	7	35%	10	25%	7	24%	9	56%	4	19%	4
optics & astronomy	-		-		3	15%	2	7%	1	5%	1	5%	7
optics & photochem.	-		-		3	15%	2	7%	1	5%	3	14%	9
optics & physiology	12	34%	6	30%	8	20%	8	28%	5	28%	9	43%	48
Total	**35**		**20**		**40**		**29**		**18**		**21**		**163**

creased to be Brewster's major research topic after 1830. Between 1830 and 1859 the time Brewster spent on physical optics continuously decreased and complex optical phenomena constituted his main focus. Among these complex phenomena, Brewster was particularly interested in optical mineralogy and vision physiology, studying the effect of the structures of materials such as crystals on light, and the optical structures and properties of human and animal eyes. Thus, Brewster's research pattern was significantly different from that of undulationists. Because most undulationists believed that optics was simply an application of mathematics, their interests in optical phenomena were limited to those relatively simple phenomena where mathematical analyses could be applied, almost all of which fell into the domain of traditional physical optics. Undulationists obviously disliked Brewster's wide-spread and mainly descriptive style, but they realized that this style was justified by the popular Humboldtian fashion. They had to tolerate Brewster's approach and did not raise any criticism against his research style.

Focusing on complex optical phenomena, however, Brewster was able to make a series of new discoveries that the purely mathematical analysis employed by undulationists could not reach. For example, Brewster successfully forced MacCullagh to admit the explanatory failures of the undulatory theory by studying the action of crystallized surfaces on light, a research topic that required knowledge from both physical optics and mineralogy. Brewster's discovery of absorption, which constituted one of the formidable difficulties for the undulatory theory, reflected the intersection between physical optics and spectroscopy. Again, Brewster's works on atmospheric polarization, which were published around 1847 as evidence against the undulatory theory, was a combination of physical optics and meteorology (Brewster 1847). Brewster's critiques developed from an interdisciplinary approach could have been easily dismissed by undulationists as irrelevant to physical optics, if the Humboldtian style of scientific research had not been appreciated by most British "men of science" in early nineteenth-century Britain. However, the popularity of Humboldtian science legitimated Brewster's approach. Undulationists had to admit the methodological validity of Brewster's empirical discoveries, and tried different ways to counter-attack Brewster's works. Consequently, the Humboldtian style of scientific research fueled the emission-undulatory controversy by legitimating Brewster's research approach from which important evidence used to against the undulatory theory was drawn.

3. The Emergence of A New-Generation of Physicists

As the number of the research papers on Humboldtian science presented to the Section A of the British association substantially decreased at the beginning of the 1850s (Table 8.2), the enthusiasm for this research approach among British physicists finally died out. At the same time, a new historical process began: the emergence of a new generation of physicists. In this section, I review the key features of these new-generation physicists, especially their attitude toward specialization, experimentation, and the Newtonian tradition. I also examine how the emergence of these new-generation physicists was related to the emission-undulatory controversy, and how their emergence leaded to the closure of the extended controversy.

3.1 The Values of Specialization

In the field of optics, a fundamental transition occurred around mid century. Those undulationists who were actively involved in the emission-undulatory controversy in the 1830s and the 1840s gradually disengaged. The time they spent in optical research and the papers they published on optical problems significantly decreased after 1845 (Table 8.3). These undulationists, who received their college education before the undulatory theory became dominant in Britain and thereby personally experienced the conversion from emissionism to undulationism, constituted an older generation in optics.

During the same period, however, a group of new undulationists gradually emerged and replaced the older generation. The main figures in this new generation included Harvey Goodwin, Samuel Haughton, Philip Kelland, Matthew O'Brien, William Rankine, Archibald Smith, George Stokes, and William Swan. Except Kelland who was trained in the late 1820s at Cambridge, already a stronghold of the undulationism at that time, all of these new undulationists received their college education after the undulatory theory controlled university curricula in Britain from the mid-1830s. These new undulationists were thus directly trained by older-generation undulationists, and accepted the theory from the very beginning of their intellectual career, without the experience of a conversion from an old theoretical framework to a new one. From the late 1830s, these new undulationists began their optical researches. The time they spent in optical research and the papers they published on optical problems rapidly increased in the following years. Beginning from the second half of the 1840s, the optical papers from the new generation outnumbered those from the older generation, and these new undulationists replaced their predecessors to become the principal force of the undulatory camp (Table 8.6).

One of the fundamental differences between these two generations of undulationists was their attitude toward Humboldtian science. Unlike the older generation, undulationists from the new generation did not have strong interests in Humboldtian science and had very few publications in these fields (Table 8.7). Undulationists from the older generation had very diverse interests and seldom specialized in a narrow field. Some of them even opposed the process of specialization. Up to 1835, for instance, Herschel still regarded specialization as regrettable, complaining that "[s]uch is science now-a-day. No man can now hope to know more

Table 8.6 Optical Papers by New-Generation Undulationists, 1835-59
[Source: Royal Society of London 1867]

	1835-39	1840-44	1845-49	1850-54	1855-59	Total
Harvey Goodwin	-	-	3	-	-	3
Samuel Haughton	-	-	1	4	-	5
Philip Kelland	3	5	-	-	-	8
Matthew O'Brien	-	3	4	-	-	7
William Rankine	-	-	-	3	-	3
Archibald Smith	2	2	-	-	-	4
George Stokes	-	-	14	15	6	34
Willaim Swan	-	1	2	1	-	4
Total	5	14	25	24	6	74

Table 8.7 Scientific Papers by New-Generation Undulationists, 1835-59
[Source: Royal Society of London 1867]

	Mathematics	Astronomy	Physics	Humboldtian	Total
Harvey Goodwin	9	-	5	-	14
Samuel Haughton	1	-	12	17	30
Philip Kelland	4	-	22	-	26
Matthew O'Brein	3	3	11	-	17
William Rankine	1	1	39	4	45
Archibald Smith	15	2	9	1	27
George Stokes	5	-	57	-	62
William Swan	-	3	5	3	12
Total	**43**	**9**	**173**	**25**	**250**

than one part of one science".[6] This attitude partly explained why older-generation undulationists were attracted by Humboldtian science. Compared with their predecessors, the intellectual interests of the new generation were relatively narrow and highly specialized. Most of them were only interested in a very narrow list of subjects, mainly mathematics and those physical investigations to which mathematical analysis could be directly applied, and never said a word about such complex subjects as tidology, meteorology, or terrestrial magnetism. The emergence of this new generation indicated that the process of specialization had greatly advanced and that physics had begun to become an independent discipline around the mid-nineteenth century.

The emergence of these new-generation undulationists brought about fundamental changes in the emission-undulatory controversy. The most direct impact was the improvement of the undulatory theory's explanatory power achieved by these new undulationists. With better mathematical training, these new undulationists were able to handle those complicated optical phenomena that their predecessors could not. The major contributions from this new generation to the theory's explanatory power included the explanation of dispersion in terms of a molecular theory provided by Kelland and O'Brien between 1839 and 1842 (Buchwald 1981, pp.230-1), and the explanations of the "polarity of light" (Talbot bands) and of fluorescence by Stokes in the late 1840s and early 1850s (Stokes 1848, 1852). In addition to the improvement of explanatory power, these new undulationists also significantly improved the theoretical structure of the undulatory theory. An incomplete list of their theoretical contributions included Stokes's explication of the concept of a solid ether that eliminated some of the confusions about the nature of ether among undulationists (Wilson 1987, p.135), Haughton's work on the general equations of an elastic solid ether (Haughton 1855), Stokes' dynamic theory of

[6] Herschel to Whewell, (2/7/1835), WP a.2071.

diffraction that eliminated an error implied in Fresnel's account of diffraction (Meyer 1934, pp.264-70), and Smith's method for determining the equation of the wave-surface in a medium of given coefficients of elasticity (Smith 1846). All of these achievements, both in explanatory power and in theoretical coherence, significantly reinforced the undulatory theory's cognitive position in its rivalry with the emission theory.

3.2 A New Spirit of Experimentation

Accompanying the emergence of these new-generation physicists, the general attitude of the physics community toward experiment also fundamentally changed around mid-century. The image that was popular in the first half of the nineteenth century pictured physical sciences in a spectrum - ranging from mathematical dynamics at the one end to experimental chemistry at the other. According to Herschel, since dynamics displayed the closest alliance with mathematics, it was thus "placed at the head of all the sciences" (Herschel 1830, p.96). Chemistry, on the other hand, "is, of all the sciences, perhaps, the most completely an experimental one; . . . [its theories] demand no intense concentration of thought, and lead to no profound mathematical researches" (*Ibid.*, p.299). Thus, Herschel put chemistry at the bottom of the spectrum because of its similarity to natural philosophy. Between dynamics and chemistry, there were other physical sciences - astronomy, optics, heat, electricity, and magnetism - ranged between the two ends according to their degrees of mathematicization. The closer to dynamics, or the higher degree of mathematicization, the more advanced the discipline. In this picture of physical sciences, clearly, the degree of experimental skills and experimental sophistication played no role in the advancement of science. This tendency to ignore experiment was further reinforced when Humboldtian science became popular in Britain, though for an entirely different reason. One of the methodological features of Humboldtian science was its emphasis upon the accurate and systematic measurements of real and complex phenomena. Compared to this, studying nature in simplified or idealized situations through experiments in a laboratory became old-fashioned (Cannon 1978, p.105).

All of these factors explained the underdevelopment of physics experiments and physics laboratories in Britain during the first half of the century. The first physics laboratory was developed from a chemistry laboratory when Faraday changed his personal interests from chemical phenomena to electricity in the 1830s. However, the underdevelopment of physics laboratories began to change in the 1840s and the early 1850s, when several private physics laboratories appeared in major universities like Cambridge, Edinburgh, and University of Glasgow. In Cambridge, Stokes established the first private physics laboratory in 1849, using it not only for his own research but for teaching demonstrations (Sviedrys 1976, p.409).

Accompanying the development of physics laboratories, the system of academic examinations in Cambridge also underwent an important transformation. In the first half of the century, the academic examinations in Cambridge were the Mathematical Tripos and the Smith Examination, which emphasized mathematical analysis and excluded several physical subjects as well as experimental works. The content of these examinations exactly coincided with the tendency to ignore experiment popular at that time. This situation also changed at mid-century. In 1851, a new academic examination, the Natural Science Tripos, was introduced to Cambridge. The content of the Natural Science Tripos was mainly experimental physics, covering the subjects of heat, electricity, magnetism, and the connected chemical phenomena (Wilson 1982, p.341). The introduction of the Natural Science Tripos to Cambridge, together with the emergence of physics laboratories in major universities, were symptoms of the changing attitude toward experimentation.

The changing attitude toward experimentation was also evident in the research style exhibited by those new-generation undulationists. Stokes' works, for example, exhibited an excellent integration of mathematical analysis and experimental exploration. In his research on optical phenomena, Stokes conducted a large number of very skillful experiments, including those on diffraction (Stokes 1849b), on the Haidinger's brushes (Stokes 1850b), on phase differences in steams of polarized light reflected from metallic surfaces (Stokes 1850a), and on the colors of think plates (Stokes 1851). Stokes even invented and constructed his own instruments in his optical experiments, as in 1851 he made an instrument for analyzing elliptically polarized light (Parkinson, 1975, p.77).

This changing attitude toward experimentation had a very profound impact on the emission-undulatory controversy. The reasons why Brewster for a rather long period had been respected as "the father of optical experiments" lay not only in his own personal achievements, but also in the fact that his contemporaries were reluctant to spend more time in optical experiments. Brewster's prestige in optical experiments was crucial for the longevity of the controversy. This prestige not only forced undulationists to listen seriously to every critique that Brewster raised, but also gave Brewster self-confidence to continue his lonely fight against the majority of the optical community. However, when the new-generation undulationists emerged with equal or even superior skills in optical experiments, the hard currency in Brewster's and other emissionists's hands suddenly vanished, and the emissionists realized that their chance of winning the battle had become infinitesimal. This change partly explains why emissionists in the late 1850s voluntarily withdrew from the emission-undulatory controversy.

3.3 The Break with Newtonian Tradition

Essentially all of the older-generation undulationists were Newtonian. For example, Young's optical investigations were overwhelmingly an extension rather than a refutation of Newton's optical works. Later strong supporters of the undulatory theory in Britain like Airy and Herschel were also strong advocates of Newtonianism. For a rather long period, one of the most embarrassing problems for undulationists was the nature of the ether, but it was troublesome only within the Newtonian framework that required transverse waves translate in an elastic solid.

The break with the Newtonian tradition in the field of optics began with the new undulationists. The best example of such a break can be found in Stokes' work on fluorescence.

Herschel in 1845 discovered a very remarkable phenomenon in his experimental studies about the impact of light on liquid. He found that a solution of sulphate of quinine, or a fluorspar crystal, though it appeared to be perfectly transparent, could exhibit a celestial blue color when viewed by transmitted light at certain incidence. He also found that the blue color appeared only in a stratum of the fluid or the crystal next to the surface by which the light entered, and that, after passing through this stratum, the incident light lost the power to produce the same effect. Herschel introduced a term "epipolic dispersion" to describe the occurrence of the blue color, and called the incident light epipolized after it had passed a stratum of fluid or solid, it was incapable of producing further epipolic dispersion (Herschel 1845a, 1845b). Having heard of Herschel's experimental results, Brewster in 1848 noticed that he had come up with similar experimental results more than ten years before both in solution of sulphate of quinine and in fluorspar. Instead of calling them epipolic dispersion, however, Brewster labeled these phenomena "internal dispersion" (Brewster 1849). Both Herschel and Brewster had certainly discovered some unknown phenomena, but neither of them was able to understand what they had found. They just made up new words to label the new phenomena and said nothing about the possible causes of them.

Herschel's discoveries later drew Stokes' attention. In 1852 Stokes wrote a letter to Herschel, in which he said:

> [Y]our 'epipolic' dispersion has given me the clue to a most extensive field of research, which has occupied me during the last year when sunlight permitted. I have now nearly finished a paper on the subject, which I hope soon to present

to the Royal Society.[7]

This paper was Stokes' famous work on fluorescence, published in the *Philosophical Transactions* in 1852. The significance of this paper was not in continuing Herschel's work, but, as reflected in its title "On the Change of Refrangibility of Light", in changing a fundamental principle in Newtonian mechanics.

Based upon the experimental results reported by Herschel and Brewster, Stokes assumed that there was a difference in the nature of light rays before and after they produced the epipolic or internal dispersion. According to the undulatory theory, the nature of light was defined by two things, its period of vibration and its state of polarization. So the experimental results suggested that either the period of vibration or the state of polarization of the rays had changed during the epipolic dispersion. Since it was a basic principle in Newtonian mechanics that a vibration in a mechanical system always retains its periodicity through all the modifications which it may undergo, Stokes at first did not think about the possibility of changing the vibration period in this case. Instead, he tried the alternative of changing the state of polarization, but soon he found it could not work. He then quickly came to a dilemma, either giving up a fundamental doctrine of the undulatory theory, namely that the nature of light was defined by its period of vibration and its state of polarization, or violating a principle of Newtonian mechanics that a vibration would not change its periodicity within a mechanical system. Facing this critical choice, Stokes clearly put more weight on the undulatory theory than Newtonian mechanics, because he said that "it seems to me less improbable that the refrangibility should have changed, than that the undulatory theory should have been found at fault" (Stokes 1852, p.466). To explain the phenomena of fluorescence, Stokes thus adopted a bold assumption, the changing refrangibility of light, a decision that

[7] Stokes to Herschel, (April 6, 1852), cited in Cannon (1978, p.120).

clearly indicated a lessening in his commitment to the Newtonian tradition compared to his predecessors.

The new critical attitude toward the Newtonian tradition created a conceptual obstacle in the communication between the two generations of physicists. The older generation experienced more and more difficulties in understanding the methods and the conceptions used by those who belonged to the new generation. Herschel, for instance, had great difficulty in the late 1850s in comprehending the principle of the conservation of energy entertained by the younger generation (James 1985, pp.33-4). Not surprisingly, the conceptual obstacles between old-fashioned emissionists and new-generation undulationists were greater. Because he was not proficient in mathematics, Brewster had difficulties in following the sophisticated mathematical analyses presented by new-generation undulationists, not to mention their physical conceptions. On the other hand, since new-generation undulationists embraced the undulatory theory from the very beginning of their careers, they were not familiar with the emission tradition, and did not have the interests to understand it. An increasing incommensurability between the emissionists and the new-generation undulationists can give us another hint to why the intensity of the emission-undulatory controversy died down in the 1850s, why the emissionists voluntarily resigned from the controversy at the end of the 1850s, and why undulationists did not make any response to Potter's book.

Conclusion: The closure of the emission-undulatory controversy was a very complicated historical event. As I sketched above, both the popularity of Humboldtian science in early nineteenth-century Britain and the emergence of new-generation undulationists around mid-century were responsible for the longevity of the emission-undulatory controversy and the timing and the features of its closure. However, we should also note that these two historical events can hardly be put into a dichotomy of internal versus external factors, or, cognitive versus social factors. Neither the popularity of Humboldtian science nor the emergence of new-generation undulationists were purely cognitive (or internal) or purely social (or external) factors. In terms of the relationship between different disciplines, the popularity of Humboldtian science functions like a cognitive factor. But it would not have had any impact on the emission-undulatory controversy if there had been no shortage of scientific manpower, an external or social factor. Similarly, because it involved a relationship between different scientific communities, the emergence of new-generation undulationists may be pictured as a typical external factor. However, it was irrelevant to the controversy without the involvement of the cognitive factors that came with the emergence of these new undulationists, such as the improvement of the explanatory power of the undulatory theory, the new positive attitude toward experimentation, and the incommensurability between the two different generations.

Our discussion of the closure of the emission-undulatory controversy also indicates that neither the popularity of Humboldtian science nor the emergence of new-generation undulationists really *caused* the closure at the end of the 1850s. Rather, all the events discussed were only inducing factors, which produced a series of options for the historical actors to make their choices. In effect, both the philosophical accounts and sociological accounts of science discussed in the first section of this chapter attempt to identify some causal factors that can explain the development and the closure of scientific controversies or scientific changes. But they all commit two mistakes in their cause hunting. First, they confuse necessary conditions with sufficient conditions in historical events. There is no doubt that, general speaking, both cognitive and social factors are necessary for the development and the closure of scientific controversies or scientific changes, but neither of them can be sufficient. Second, they confuse inducing or optional factors with causal or compelling factors in historical events. In real history, neither cognitive nor social factors force actors to do anything. Most of the time, the relevant cognitive and social factors are even inconsistent. These cognitive and social factors merely produce options or chances for individual actors. It is individual actors' final decisions to pick up a particular option that constitutes historical events. For example, the popularity of Humboldtian science did not directly *cause* the stagnancy of the undulatory theory, but only provided some new options or alternatives for undulationists to satisfy their scientific curiosity. It was those undulationists who made their own decisions to give up their optical researches and consequently brought about the stagnancy of the theory.

To express this point another way around, the issue here is the role of individuals in historical events. Traditional philosophical theories of science give no place for individuals in their accounts of science. Sociological theories, on the other hand, give places to individuals in their accounts, but assign no freedom to them. In these sociological accounts, individuals are bound or compelled by social and political forces, or by their own contextual conditions. But the emission-undulatory controversy indicates that individual scientists were able to make free choices when they faced the options produced by the relevant cognitive and social factors. In this particular case, individuals' decisions were even more important for the development and ending of the controversy than those cognitive and social factors. For example, it was Brewster who decided to resist the undulatory theory although he had fully comprehended the explanatory advantages of the theory, and who chose to withdraw from the controversy in the late 1850s although at that time he did not face any major challenge. Thus, it is a serious mistake to reduce the complexity of a historical event to a string or a network of causal factors. The organism of history will not and should not be pictured as an inorganic machine governed merely by a bunch of cause-effect links.

Chapter 9

The Main Theme of the Controversy

The main theme of the emission-undulatory controversy is the issue of appraisal, beginning with evaluations of the two rival theories of light and concluding with evaluations of the experimental researches conducted by the two rival groups of "men of science". The primary purpose of this chapter is to provide theoretical analyses of appraisal practices in the emission-undulatory controversy. In the following sections, I select several cases in the emission-undulatory controversy to illustrate the patterns and the characteristics of both theory appraisal and experiment appraisal. These cases exemplify the complexities of appraisal practice, most of which are incomprehensible within the frameworks provided by existing philosophical accounts of science.

Based upon analyses of the patterns and characteristics of theory appraisal and experiment appraisal in the emission-undulatory controversy, I suggest a new theoretical perspective to understand appraisal practice in a context of scientific controversy, or, a context of scientific change. This new theoretical perspective differs from the existing philosophical accounts of science in many ways. First, instead of conceptualizing appraisal merely as a logical affair involving, for example, comparing degrees of confirmation or probability, I propose that we understand appraisal as a practice and emphasize its procedural and informal aspects. Second, instead of defining appraisal as an agent-free process, I highlight the role of individual actors who have freedom in determining goals and selecting tactics in their appraisal practices. Third, instead of excluding social considerations from the process of appraisal, I endeavor to show how social factors inevitably enter the process of appraisal, and how the results of appraisals depend on the interactions between actors and the natural world, as well as the interactions among actors.

1. The Patterns of Theory Appraisal

When they discuss the pattern of theory appraisal in the emission-undulatory controversy, most previous scholars focus on "the reasons given for the superiority of the wave theory, reasons which were to a greater or lesser extent *endorsed by protagonists on both sides* of the controversy" (Cantor 1983, p.192; my emphasis). Not surprisingly, they identify explanatory power as a reason endorsed by both sides in the controversy and it becomes the criterion for measuring the superiority of the undulatory theory. Consequently, theory appraisal in the emission-undulatory controversy becomes a purely logical affair, which amounts to comparisons between theories and natural phenomena, as well as comparisons between the explanatory abilities of the two rival theories.

In this section, I first provide detailed analyses of the tactics employed by emissionists and undulationists in their theory appraisals. Historical details show that theory appraisal in the emission-undulatory controversy was definitely not a purely logical affair. To better understand the nature of theory appraisal, I suggest a new theoretical viewpoint, which conceptualizes theory appraisal as a goal-oriented practice conducted by individual actors.

1.1 The Tactics of Emissionists

One of the most salient characteristics of emissionists in their theory appraisal was that they seldom openly advocated their own doctrines. Although most emissionists kept their faith in the emission theory during the whole controversy, they pretended to be *Reinistes* [nothingists] in public, as Brewster and Brougham did in the 1840s and the early 1850s. Their advocacy and support for the emission theory of light could only be found in private conversations, for instance, in the correspondence between Brewster and

Brougham. The major reason for these emissionists to keep a low profile was their own judgment of the intellectual or cognitive status of their theory. Even the most stubborn emissionists recognized that the emission theory was unable to provide the kind of coherent and accurate explanations for optical phenomena that the undulatory theory did. Although they could employ the emission doctrine to explain some particular optical phenomena, for example Brewster's account of diffraction in the 1850s, these explanations were so coarse that they could only air them in private. In addition to cognitive considerations, the recognition of the power of undulationists in the major British scientific institutions also forced emissionists to keep a low profile in the controversy. After the mid 1830s, undulationists successfully controlled the most important scientific institutions in Britain. In this circumstance, the attempt to advocate openly the emission theory could easily be defeated by calling it "behind the [current] state of science". Undulationists' control within the optical community left emissionists little choice but to adopt a circuitous or outflanking tactic in their theory appraisal.

The emissionists' tactic was to challenge the undulatory theory not by pinpointing its errors but by exposing its empirical incompleteness. Instead of directly attacking the undulatory theory, emissionists endeavored to explore a series of new phenomena that neither the emission nor the undulatory theory was able to explain. Examples of this tactic included the debates about the absorption of light, the polarity of light, and the chemical properties of light initiated by emissionists during the 1830s and the 1840s. In these debates, emissionists made every effort to prove that the undulatory theory could not explain some newly found phenomena, while they also tried to convince the audience that they did not mean to overthrow the undulatory theory. Perhaps the most explicit example of this delicate tactic is in Brewster's correspondence to Brougham when he recruited and trained the latter to rejoin the fight with the undulatory theory. In these letters, Brewster clearly stated the tactic he preferred for the coming battle. According to Brewster, Brougham in the coming battle was to demonstrate new experimental effects outside the province of the undulatory theory, and not to attack the theory directly. From these letters, it is quite clear that Brewster's ultimate goal was the "downfall" of the "presumptuous" undulatory theory, but this was to be achieved in two stages. First, Brewster, aided by Brougham, was to present new experimental facts that could not be explained directly by undulatory principles, and to create a domain of experimental discourse in which the undulatory theory had no applicability. Second, only after these new experimental facts were firmly established as respectable matters and the domain of experimental discourse became autonomous, could doubts be cast on the adequacy of the undulatory theory.[1]

The reason for adopting such a circuitous or outflanking tactic came from certain social considerations. Although it was clear that *ultimately* the fate of the undulatory theory would be decided on experimental grounds, the emissionists' first concern was to accumulate a critical mass of experimental evidence, and, more importantly, to safeguard this evidence by establishing it as a relatively independent and autonomous domain of discourse. Emissionists had realized that, without this preliminary consolidation of their position, each individual effort to undermine the undulation theory could be defeated on a case by case basis, motivated by the undulationists' cognitive position and enabled by their social power within the scientific community. In adopting the outflanking tactic, therefore, emissionists subordinated their cognitive goals to their social concerns. In their strategic planning, they gave greater weight to the control of scientific journals and professional societies than the cognitive evidence provided by experiments.

1.2 The Tactics of Undulationists

Theory appraisal is not merely a matter of personal evaluation. In the emission-

[1] For more discussion of this tactic, see chapter 7, as well as Chen & Barker (1992).

undulatory controversy, when undulationists were involved in theory appraisal, their goal was not to convince themselves of the merits of their own theory or the defects of the rival theory - they had already developed specific attitudes toward these rival theories before they began their theory appraisal. Their goal in theory appraisal was to advocate the undulatory theory and to oppose the emission theory. In effect they intended to convince the rest in the optical community, making others agree with their personal judgments of the rival optical theories.

When undulationists advocated their own theory, they, without exception, advertised its great explanatory power. They repeatedly claimed that the undulatory theory was able to explain not only a large number but also a variety of classes of optical phenomena while the emission theory could not. In order to convince their audience, undulationists adopted a series of tactics to emphasize or even exaggerate the explanatory successes of their own theory.

One of these tactics was to emphasize or exaggerate the number of cases that the theory could explain by repeatedly citing favorable examples from a narrow field. During the 1830s and the 1840s, most of undulatory theory's new successes came from the fields of diffraction and polarization. Not surprisingly, undulationists in this period repeatedly referred to examples in these two fields. This was exactly what Powell did when he compared the explanatory abilities of the two rival theories. While in dispute with Barton in 1833 Powell drew up a table of 23 optical phenomena, noting whether each was explained by the undulatory or the emission theory (Figure 9.1). According to Powell, this table indicated the superiority of the undulatory theory, because it "perfectly" explained 18 cited phenomena while the emission theory accounted for only five. However, of the 23 optical phenomena Powell cited, eight belonged to polarization, five to diffraction, and four to colors in thin plates. In total, 17 out of 23 -- over 70 percent -- came from a relatively narrow branch of optics in which the

Phænomena.	*Corpuscular Explanation.*	*Undulatory Explanation.*
Reflection	..... Perfect	...... Perfect.
Ditto at boundary of transparent medium	...... Imperfect.....	...... Perfect.
Refraction (light homogeneous)	...... Perfect	...... Perfect.
Dispersion	...... Imperfect ...	...... Imperfect. (? Cauchy.)
Absorption	...... Imperfect	...... Imperfect.
Colours of thin plates (in general).........	...... Perfect .... (with subsidiary theory of fits)...	...... Perfect.
Central spot............	...... None	...... Perfect. (Imperfect according to Mr. Potter.)
Airy's modification ...	...... None	...... Perfect.
Thick plates............	...... Perfect	...... Perfect.
Coloured fringes of apertures and shadows in simple cases	...... Imperfect .... (with subsidiary theory of inflection)	...... Perfect. (Imperfect according to Mr. Barton.)
—— in more complex cases	...... None	...... None.
Stripes in mixed light	...... None	...... Perfect.
Shifting by interposed plate..................	...... None	...... Perfect. (Imperfect according to Mr. Potter.)
Colours of gratings...	...... None	...... Perfect.
Double refraction	...... Perfect........	...... Perfect.
Polarization............	...... Imperfect .... (with subsidiary theory of polarity)	...... Perfect. (with subsidiary theory of transverse vibrations.)
Connexion with double refraction........	...... None	...... Perfect.
Law of tangents	...... None	...... Perfect.
Interferences of polarized light.........	...... None	...... Perfect.
Polarized rings........	...... Imperfect..... (with subsidiary theory of moveable polarization)	...... Perfect.
Circular and elliptic polarization: at internal reflection	...... None	...... Imperfect.
at metallic surfaces	...... None .... (? Sir D. Brewster)	...... None.
Conical refraction	...... None..........	...... Perfect.

Oxford, Nov. 1, 1833.

Figure 9.1 Powell's Assessment of the Theories of Light
[From Powell 1833b, pp.416-7]

undulatory theory enjoyed explanatory successes. At the same time, only few cases came from the categories of reflection, refraction, dispersion, absorption, and double refraction, with some of which the undulatory theory still had difficulties. It is clear that the items in the table were deliberately selected by Powell for the purpose of achieving the best result in convincing his audience. And the table itself is a vivid example of the undulationists' tactic in advertising the explanatory successes of the theory.

Another tactic that undulationists used was to emphasize the scope of the theory's explanatory successes by changing the classification system of optical phenomena. The question of the scope of a theory's explanatory successes, or, how many different classes of phenomena it can explain, is directly related to how the corresponding field is classified. Before 1830, the available classification systems for optical phenomena were unfavorable to of the undulatory theory. For example, Young in 1807 proposed a classification system for optical phenomena with the following twelve categories (Young 1807):

1. The propagation of light;
2. Diffraction;
3. The transmission of light in a medium;
4. The uniform velocity of light;
5. Reflection and refraction;
6. Partial reflection;
7. Total reflection;
8. Solar phosphorus;
9. Aberration;
10. Double refraction;
11. Dispersion;
12. The production of colors.

Another classification system available during this period was the one proposed by Brewster in 1822 (Brewster 1822), which included the following seven categories:

1. Reflection;
2. Refraction;
3. Dispersion;
4. Diffraction;
5. Colors of thin, thick, and mixed plates;
6. Double refraction;
7. Polarization.

Under both Young's and Brewster's classifications, the merits of the undulatory theory were significantly reduced. Its explanatory successes were limited to a few categories, only three out of seven in Brewster's system (diffraction, polarization, and colors of plates), and even fewer in Young's. Thus, undulationists had to develop a new classification system in which their theory's explanatory successes could become evident.

Such a new classification system first emerged in Herschel's "Light", in which he tried to divide all optical phenomena into two sorts: unpolarized and polarized. Herschel's classification was innovative but immature and even slightly inconsistent. Following Herschel's idea, however, Lloyd later in his 1834 report on physical optics developed the following coherent classification system:

1. Unpolarized light
   a. The propagation of light;
   b. Reflection and refraction;
   c. Diffraction;
   d. Colors in thin plates.
2. Polarized light
   a. Polarization and transversal vibrations;

b. Reflection and refraction of polarized light;

c. Double refraction;

d. Colors of crystalline plates.

Under this new system, the undulatory theory was able to control one half of the major categories firmly - those related to polarized light, in which the emission theory had no currency at all. In the other half, the undulatory theory could also demonstrate its superiority in the subcategories of diffraction and colors of thin plates. On the other hand, the troublesome cases of dispersion and absorption now became sub-subcategories under reflection and refraction of unpolarized light. Even though the undulatory theory might still have difficulties in dealing with these phenomena, these failures now became trivial in comparison to the theory's successes in varied classes of optical phenomena. In this way, the merits of the undulatory theory were emphasized to a maximum extent and its defects were reduced to a minimum. This was one of the most effective tactics to advocate the undulatory theory and to persuade people to accept the theory.

In addition to advocating their own theory, the other goal for undulationists in the controversy was to oppose the rival, namely, to convince other members of the optical community to give up or to reject the emission theory. To achieve this goal, undulationists also paid tremendous attention to selecting the appropriate tactic to carry out the attack. Such considerations of the effective ways to oppose the emission theory clearly are reflected in Lloyd's 1834 report on physical optics. In the report, Lloyd, on the one hand, advocated the undulatory theory by advertising its explanatory power, but, on the other hand, he attacked the emission theory not simply by pinpointing its low explanatory ability. Lloyd realized that, as a "well-imagined" theory, the emission theory of light might be able to accommodate those unexplained phenomena by increasing the number of its postulates. Thus, pinpointing its explanatory failures was not an effective way to oppose the theory. A better tactic in many cases, according to Lloyd, was to expose the emission theory's failures in obeying the principle of "harmony, and unity, and order, which must reign in the works of One Supreme Author" (Lloyd 1834, p.296). In other words, Lloyd believed that under many circumstances a more effective tactic to attack the emission theory was to uncover its conceptual problems, including its internal inconsistencies and its conflicts with the received knowledge background.[2]

The effectiveness of conceptual analysis first appeared in the cases in which the emission theory was able to accommodate the phenomena only by adding new hypotheses. For example, in terms of explanation, the emission and the undulatory theory were evenly matched in the field of reflection and refraction. However, Lloyd pointed out that the emission theory's explanatory successes in this particular field were achieved at the price of giving up its conceptual coherence. The general principle underlying the emission account of reflection and refraction was the repulsion of the ether at the surface between two different media. According to Newton, the ether was rarer in a dense medium but denser in a rare medium. Newton could easily explain the refractions both from dense medium to rare medium, and other way around, in terms of the repulsion of the ether from the side with denser ether (rarer medium). Using the same principle, Newton could also explain the total reflection that happens when light transmits from a dense medium to a rare medium. However, Newton could not employ this principle to explain the ordinary reflection that happens when light transmits from a rare medium to a dense medium -- the repulsion of the ether from the side with a rare medium (dense ether) ought to make the ray pass through the surface, and hence no reflection should happen. Newton's solution was to appeal to an *ad hoc* assumption: the ordinary reflection was possible because some of the incident rays were in "fits" of reflection (Newton 1952, p.281). According to Lloyd, however, this *ad hoc* assumption

[2] For more discussion of Lloyd's view on the role of conceptual considerations in theory appraisal, see Chen (1990).

was totally unrelated to and incoherent with other principles underlying the emission account of reflection and refraction. Thus, this assumption was entirely "gratuitous" (Lloyd 1834, p.33), and the introduction of it caste doubt on the legitimacy of the emission accounts.

The effectiveness of the tactic of conceptual analysis was also reflected in those cases in which experiments were not ready to test the relevant theoretical claims. One example of such cases was the question of the velocities of light in different media. The emission theory of light predicted that light would be transmitted faster in a denser medium while the undulatory theory claimed the opposite. Throughout the 1830s, however, there was no experiment that could accurately measure the velocities of light in various media. In fact, the extant experimental reports were contradictory: Arago reported that his experiment confirmed the undulatory prediction but Potter claimed that his supported the emission theory. Under this circumstance, Lloyd found that conceptual analysis was the only tactic left for him to critique the emission theory on this particular topic. Lloyd drew his audience's attention to an internal inconsistency within the emission framework with respect to the velocities of light in different media. Emissionists like Newton in general assumed that light transmitted faster in denser media. However, Lloyd indicated, Newton in his explanation of the colors in thin plates also assumed that the intervals of the "fits" diminished in denser media because the colored rings contracted when a drop of water was introduced between the glasses (Newton 1952, p.207, 284). Since in Newton's hypothesis of the "fits" the number of the intervals of the "fits" in a given color of light did not alter, the diminished intervals implied a decrease of the velocity of light, an implication contradicted by a general hypothesis of the emission theory (Lloyd 1834, p.37). This internal inconsistency, Lloyd believed, cast enough doubt on the legitimacy of the emission theory with respect to the question of the velocity of light.

Even when experimental testing was available, Lloyd showed that the tactic of conceptual analysis might still be a better choice, because it might be more effective or more efficient than direct tests by experiments. For example, Newton's explanations of the colors of thin plates in terms of the hypothesis of "fits of easy reflection and transmission" had been empirically tested and falsified by Airy through a number of carefully designed experiments, which were expensive both in terms of labor and in terms of money. However, Lloyd in his report demonstrated a different way to attack Newton's explanation that was simpler than, but at least as effective as, experimental testing. Lloyd once again drew his audience's attention to a conceptual incoherence of the Newtonian theory. In order to explain the contraction of the colored rings with increasing obliquity of the incident light, Newton had to assume that the intervals of the "fits" increased with the increase of the angle of incidence (Newton 1952, p.203, 283). But according to elastic mechanics, the periods or intervals of all mechanical vibrations, including the "fits" produced by ethereal vibrations, did not vary within the same medium, and hence should not change despite the increase of the incidence angle. Thus, Lloyd concluded that Newton's explanation was "at entire variance with the physical theory", a conceptual problem resulting from the conflict with the well-grounded and widely received background knowledge (Lloyd 1834, p.70). This charge was powerful enough to cast doubts on the emission account but cost much less than conducting a number of experiments.

1.3 A New Look of Theory Appraisal

In their studies of theory appraisal, most existing philosophical accounts of science search for a universal criterion for evaluating theories. This universal criterion, after it is justified, is supposed to be applicable to every historical case, and should be acceptable to every scientist who is doing theory appraisal. However, as I sketched above, the theory appraisal practice in the emission-undulatory controversy cannot be interpreted as an application of any universal criterion provided by the existing philosophical accounts of science. The most impressive feature of this theory appraisal practice is the diversity of the evaluation criteria and tactics that the historical actors actual applied. We can summarize these criteria and tactics in the following diagram (Table 9.1). As shown in this diagram, emissionists and undulationists in the controversy actually employed different criteria and tactics, and each side even had different sets of criteria and tactics for different tasks. This diversity is a clear indication of the creative role that actors can play in the process of theory appraisal. The adoptions of different criteria and different tactics by the different sides in the emission-undulatory controversy were the results of actors' judgments of the intellectual status of the rival theories as well as the social status of the rival camps. The adjustments of evaluation criteria and tactics in this case clearly reflected actors's considerations of their effectiveness in achieving their goal. In this case, theory appraisal is definitely not a purely logic matter or a purely intellectual business as the existing philosophies of science prescribe.

In order to understand fully the nature and the characteristics of theory appraisal, we first must take the active role of actors or agents into account. Theory appraisal is not the self-execution of a universal criterion as explored and defined by philosophers of science. Instead, theory appraisal is executed and completed by historical actors with the purpose of directing or ending debates within a particular scientific community. Consequently, theory appraisal, in a context of scientific controversy or scientific change, is not even a personal matter. Since they have already adopted specific positions, individual agents in theory appraisals aim to persuade others to accept their own judgment on some particular theories.[3] The interactions between human agents are

[3] It is interesting to ask how individual agents opted for a specific theory to begin with. In our historical episode, there were two different ways: by training and by initial evaluation. All emissionists and the new-generation undulationists adopted their theoretical positions because they

inevitable in every process of theory appraisal.

Since theory appraisal is practiced by human agents, the next thing we have to take into account is the notion of goal. Without exception, the ultimate goal for theory appraisal in a context of scientific change is to persuade other members of a scientific community to accept a certain judgment on a particular theory. Agents always have very specific goals when they start the process of theory appraisal. Usually, these goals included defending, advocating, or attacking a particular theory. In practice, formulating a specific goal, to defend or to advocate or to attack a particular theory, is a precondition for the appraisal. For an individual agent, the formulation of his or her goal is in a great degree constrained by his or her intellectual history and social context.

The consideration of goals leads to the consideration of means, or the most effective tactic for achieving the goal. The selection of appropriate tactics is first constrained by the goal of an agent, because different tactics serve different goals. For instance, the goal of advocating a theory and the goal of attacking a theory required different tactics to achieve the best results in the emission-undulatory controversy. Furthermore, the selection of tactics is also constrained by the cognitive status of the theories involved and the social status of the actors involved. Emissionists in the controversy who selected a low-profile, two-stage tactic clearly exemplified this point.

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were trained in that way. However, the older-generation undulationists established their theoretical positions by initial evaluation. Like the practice of theory appraisal in a context of scientific change, the process of initial evaluation is not a purely logical matter either. A good example is Herschel's acceptance of the undulatory theory in the mid-1820s. Herschel had developed a preference to the undulatory theory by 1824 when he began to write his "Light". His adoption of the undulatory theory, however, cannot have been the result of purely logical or cognitive considerations: Fresnel's second memoir on double refraction, one of the most important achievement of the undulatory theory, did not appear until 1827. Thus, Herschel's initial evaluation was also a goal-oriented activity. He adopted the undulatory theory first, and then developed arguments during his writing of "Light" to convince his audience that he had made a right decision in adopting undulationism.

Table 9.1 The Patterns of Theory Appraisal in the Emission-Undulatory Controversy

		Agent	
		Emissionists	Undulationists
Theory	Emission Theory	- Goal: defending; - Tactic: pretending to be "*Rientistes*".	- Goal: attacking; - Tactic: exposing rival's conceptual problems.
	Undulatory Theory	- Goal: attacking; - Tactic: exposing rival's empirical incompleteness.	- Goal: advocating; - Tactic: emphasizing its explanatory power.

Unlike most philosophers of science who focus on the criterion of theory appraisal, I emphasize the importance and significance of tactics that agents employ in the process of appraisal. An abstract criterion could not have any impact on theory appraisal without the application and execution of human agents. To apply a criterion in theory appraisal first requires the employment of certain tactics by human agents. For emissionists, like Brewster and Brougham in the early 1850s, their ultimate criterion for judging the undulatory theory could not be applied before the execution of a deliberate two-stage tactic. Moreover, to make an appraisal criterion applicable and appropriate for a particular context also requires further tactics. In our episode, the undulationists would have not been able to apply the criterion of explanatory power without appeal to a series of tactics like, for example, changing the classification system.

The interpretation of theory appraisal as a practical process in which agents employ a variety of tactics for achieving particular goals first implies that theory appraisal is a process of social interaction -- agents endeavor to convince or persuade the rest of the scientific community. However, in addition to the interactions within the members of the scientific community, theory appraisal also includes interactions between human agents and the natural world. All the tactics employed in theory appraisal aim to create general agreement that a particular theory reflects or distorts, or, is applicable or inapplicable to the natural world experienced by all members of the scientific community. The natural world still provides a powerful constraint to the result of theory appraisal.

2. The Patterns of Experiment Appraisal

Unlike the topic of theory appraisal, the questions of how experimental evidence is evaluated and justified drew far less attention from the students of science. In traditional philosophy of science, there is no independent process of experiment appraisal. Some philosophers like Popper claim that experimental results, stated as basic statements, are accepted by convention, and other suggest that the examinations and justifications of experimental evidence are completed in the process of theory appraisal. Since experimental instruments, procedures, and techniques are usually described as auxiliary assumptions that enter into the hypothetico-deductive model of theory testing, experimental evidence is evaluated when theories are tested, and is justified in terms of the successful predictions of theories.

However, recent empirical studies of science reveal that experimental evidence is not treated by scientists as non-problematic. Most experimental instruments, procedures, and techniques now widely accepted as reliable, or as "black boxes" in Latour's sense (Latour 1987), all experienced an early period in which their legitimacy was controversial. Even after these experimental instruments, procedures, and techniques are sealed as "black boxes", they may be reopened later in scientific debates, just as in our episode of the emission-undulatory controversy. All of these findings suggest that experiment appraisal is a crucial practice in science, and, more importantly, is frequently independent of the process of theory testing.

The differences between theory appraisal and experiment appraisal have begun to draw the attention of philosophers of science. In a recent study of the development of two experimental instruments in biology, Bechtel suggests a set of criteria for evaluating experiment findings that are significantly different from those for theory appraisal. According to Bechtel, the criteria for experiment appraisal include:

> (1) whether the [experimental] technique yielded what appeared to be well-defined evidence (e.g., clear images in micrographs, sharp separation of fractions), (2) whether others were able to employ the technique with similar outcomes, (3) whether the evidence generated by the new procedures was consistent with evidence gathered using other techniques and (4) whether this evidence fit into plausible theories of the operation [of the subject] (Bechtel 1990, p.561).

Bechtel's suggestion reveals some significant differences between theory appraisal and experiment appraisal, such as the unique criteria about well-defined evidence and about the replicability in evaluating experiment. By merely outlining the criteria of evaluation, Bechtel's suggestion, like the traditional accounts of theory appraisal, implicitly interprets experiment appraisal as an agent-free process that amounts to the self-executions of the criteria as conceptualized. However, as I have indicated in the last section, the tactics and the procedures actually employed by historical actors, rather than the criteria summarized and imposed by philosophers, are highly significant in reflecting the nature of appraisal practice.

In this section, I first provide detailed analyses of two cases of experiment appraisal in the emission-undulatory controversy: the debate between Brougham and Powell on diffraction and the debate between Brewster and Airy on spectral colors. These two cases illustrate the procedures and the complexities of experiment replication. Based upon the historical discussion, at the end of this section I examine the epistemological foundation of experiment replication, and suggest a new theoretical perspective for understanding experiment appraisal.

2.1 Experiment Replication

One of the most important and effective procedures or tactics for experiment appraisal is replication. According to some philosophers of science, the possibility of repeating experiments constitutes the essential basis for the successes of science: we can finally agree about the results of science because we have learned that experiments carried out under precisely the same conditions do actually lead to the same results. In the eyes of some sociologists of science, however, replication is simply an institutionalized requirement for experiments imposed by the system of social control (Mulkay & Gilbert 1986, p.21).

The key feature of experiment replication is to multiply witnesses. According to

Shapin and Schaffer (1985, pp.57-60), in practice this multiplication of witnesses can be achieved in at least three different ways. The first way is direct and face-to-face witnesses, perhaps performing experiments in a public space. Boyle, for example, routinely performed his air-pump experiment in the Royal Society's ordinary assembly rooms while aiming to prove the legitimacy of experimental study. Another way of multiplying witnesses to experiments is direct but distant. Experimenters always report their experiments in a way that facilitates their replication, to provide the readers with the necessary information to perform the experiments for themselves. The third way of witness multiplication is totally indirect. This is a special technique called virtual witnessing, which involves the production in a reader's mind of an image of an experiment and avoids either direct witnessing or replication by the reader. In this way, the multiplication of witnesses is not constrained by any material condition and can be, in principle, unlimited. Thus, if numbers are important, of the three techniques of multiplying witnesses, virtual witnessing is the most powerful and important one.

In the emission-undulatory controversy, the most vivid example of employing the tactic of virtual witnessing in experiment appraisal can be found in the debate about Brougham's diffraction experiment in the early 1850s.[4] The goal of the emissionists, Brewster and Brougham, in this debate was to re-establish the importance of experiment as an autonomous discourse, by presenting new experimental work that their rivals could not explain. But in order to count a new experimental finding as experimental knowledge upon which theories should be based, a process of witnessing, warranting the reliability and validity of the finding, was necessary. Brougham, for example, had to first multiply the witnesses to his experimental finding by convincing Brewster. He invited Brewster to his home and replicated experiments in Brewster's presence (Brougham 1850, p.235).

[4] For details of this debate, see chapter 7.

In his correspondence with Brewster, Brougham also gave Brewster detailed information of his experimental setting, facilitating his replication of the experiment. Brewster did not obtain the same effect in his replication, but he attributed the negative result to his different experimental arrangement and did not challenge the reliability of Brougham's experiment, because he liked Brougham's finding. Having persuaded Brewster, Brougham further multiplied the witnesses to other people, including undulationists and other audiences, convincing them of the reliability and legitimacy of his experiment. To achieve this goal, Brougham did not employ the tactic of direct witnessing as he did in persuading Brewster. Rather, Brougham tried to convince his rivals by a tactic that may be called virtual witnessing.

In his 1850 paper published in the *Philosophical Transactions*, Brougham first described the locations of his experiments. He reported that he performed the experiments in two places: first in Brougham Hall, an inherited estate near Penrith, England, and then in Provence, his summer home near Cannes, France. According to Brougham, the climate of Provence was "singularly adapted" to his experiments, because in Provence there was more sun than at Brougham Hall. In Provence, Brougham wrote: "I find, by my journal of 1848, that during forty-six days which I spent in those experiments, from 8 a.m. to 3 p.m., I scarcely ever was interrupted by a cloud, although in was November and December". But "of sixty-one days of summer at Brougham, I had but three or four of clear weather" (Brougham 1850, p.235). Brougham also reported that he performed most of his experiments in the winter of 1848. He said that: "In experiments at this place [Provence], in winter, I found one great advantage, namely, the more horizontal direction of the rays. In summer they are so near vertical, that a mirror must be used to obtain a long beam or pencil, which is often required in these experiments, and so the loss of light countervails the great strength of the summer sun's light" (*Ibid.*, p.236).

According to today's standard, these descriptions of the locations and the time of experiments seem trivial for the purpose of multiplying witnesses. However, they were significant and meaningful in the context of nineteenth century optics. Until the mid nineteenth century, sunlight was still the major light source for optical experiments. Although artificial lights were available at the time, their were not powerful enough for experiments about diffraction that required light sources with relatively high intensity.[5] Because of the dependence upon sunlight, the locations and times of optical experiments became significant factors for their appraisals, especially in the context of Britain where favorable climate for optical experiments was scarce. By drawing his readers' attention to the locations and times of his experiments at the beginning of his paper, Brougham wanted to convince his readers that the findings produced by these experiments were acceptable because the favorable climate allowed him to observe the clearest results, and to obtain reliable findings by enough repetitions and variations. Moreover, by emphasizing the location of his experiments, Brougham implicitly suggested that his readers could not directly replicate his experiments, unless they had a house in France as he did. Brougham's readers then had little choice but virtual witnessing.

Another virtual witnessing tactic Brougham used in his paper was the introduction of the three-successive-edge experiment. The major purpose of Brougham's paper was to demonstrate the different properties of light exhibited in the different sides of a pencil of light after diffraction. To achieve this purpose, his original experiments with two-successive-edges were logically and cognitively enough. However, Brougham argued that the three-successive-edge experiment was the *experimentum crucis*. In his 1850 paper, Brougham told his readers that the effect of the third edge in the three-successive-

[5] One way to compensate the defect of artificial light was to design rather complicated optical devices to enhance its intensity. For an example of such a device, see Brewster to Brougham, (8/29/1851), University College Library, 26,665.

edge experiment was very subtle, and that he could not confidently determine it until he had the benefit of a delicate instrument. This instrument was designed and made by Soleil, a well-known maker of optical apparatus whose "great ingenuity and profound knowledge of optical subjects can only be exceeded by his admirable workmanship" (*Ibid.*, p.235). To further impress his readers, Brougham provided rather detailed information about the instrument both by verbal descriptions and by visual representations. His visual representations included an analytic diagram of the experimental design and an expensive engraved picture that gave a detailed image of the instrument. With this engraved picture, Brougham repeatedly emphasized the accuracy of the instrument, telling his readers how the racks and pinions of the instrument made adjustments easy and how the metal construction gave solidity to the apparatus. Here his purpose was also to convince his readers that it would be impossible to replicate his experiments without such a delicate instrument, and, more importantly, that the experimental results produced by this instrument were acceptable and reliable.

For undulationists, their objective in this debate was not just to defend passively their theory by digesting all the anomalous evidence that the emissionists presented. They chose to defend their theory aggressively by attacking the legitimacy of their rival's experiments, and raising questions whether the emissionists' findings could be counted as experimental knowledge. One way to discredit a rival's experimental work was to challenge its reliability under replication, and this was exactly what Powell did. But replication attempts themselves also required witnessing. Here Powell also employed a technique that can be called "virtual witnessing". Like Brougham, Powell did not give details about his experimental parameters. He did not even mention what kind of experimental apparatus he actually used. Rather, Powell tried to draw readers' attention to his diligence in identifying one of the essential parameters in Brougham's work - the distance between the edges, attempting to convince his readers that he had really replicated the experiments exactly as described by Brougham, and failed to obtain Brougham's results. By describing a series of double-edge experiments, including one with a standard aperture, Powell appealed to existing experimental knowledge that would have been very familiar to the contemporary optical community, while Brougham's three-successive-edge experiments were not. Hence Powell's audience would associate his experiments, but not Brougham's, with experiments they had performed themselves. In effect, Powell was encouraging his audience to extend their *actual* witnessing of experiments they had performed themselves to the *virtual* witnessing of Powell's experiments, which contradicted Brougham's. Powell therefore hinted that his failure to replicate, based on such familiar experiments, was more reliable than Brougham's work, which did not connect with existing experimental knowledge.

In the debate about Brougham's experiments on diffraction, virtual witnessing became the major tactic adopted by the actors for multiplying witnesses to their experiments. The selection of this particular tactic clearly reflected the actors' deliberate considerations of how to convince a particular group of audience. For the emissionists, the audience at whom they aimed were not just the members of the optical community. The publication of Brougham's paper in the *Philosophical Transaction* indicated that Brougham's audience might include all the Fellows of the Royal Society, a large number of whom were amateurs until the early 1850s. However, appeal to the appreciation and support from these amateurs was still significant for the emissionists as the processes of specialization and professionalization in physics were not complete. Since the undulatory theory dominated the optical community and the Section of Mathematical and Physical Sciences in the British Association, these amateurs became the only possible candidates in the emissionists' campaign to recruit new allies. To convince these amateurs, however, the ordinary tactic of providing facilitating information for direct replication was clearly not appropriate. These amateurs, though they had high prestige as the

Fellows of the Society, may not have had the interest or even the ability to replicate Brougham's experiments. The only choice for Brougham, therefore, was to adopt the tactic of virtual witnessing, convincing them by triggering in their minds a vivid and detailed image of the experimental scene.

Similarly, the adoption of virtual witnessing in Powell's appraisal of Brougham's experiments also reflected the concern of how to prevent Brougham's experimental work spreading among his audience. When facing Brougham's experimental findings, no undulationists would expect that any committed supporter of their theory would give up his belief because of Brougham's experimental results. What they were concerned about was the influence of Brougham's work in a different audience - those amateurs in the Royal Society. Thus, their responses were calculated to eliminate the public attention from this group of audience toward Brougham's experimental results. This was particularly striking in the response from Powell.

In his experiment appraisal of Brougham's work, Powell made it clear that he was far from certain of the details of Brougham's experimental arrangements, in particularly the distances between the edges in his three-successive-edge experiment. This admission was potentially disastrous: Powell's most damaging charge against Brougham, that he was unable to replicate Brougham's experiments, was credible only if Powell exactly recreated Brougham's experimental set up. And yet all Powell had to do was to communicate with Brougham to request the dimensions of the apparatus, and then the dispute could be resolved by a series of actual replications. But Powell did not want the actual replication, because any more discussion with Brougham would certainly draw the public's attention and help spread Brougham's work among his audience. Instead, he was satisfied with the virtual witnessing he used in justifying his own experimental results, hoping his appeal to existing and familiar experimental knowledge could override Brougham's inducement to those amateur scientists. To proceed as Powell did seems inexplicable from a cognitive viewpoint, but it makes sense as a tactic for denying emissionists further opportunities to present their cases. Powell was, in effect, denying Brougham the opportunity to "lengthen his network", in Latour's sense.

When Airy dealt with the challenge from Brougham's experimental works, he, like Powell, also adopted a tactic that emphasized the denial of publicity to Brougham. Powell in fact first sent his 1852 paper on Brougham's experiments to the *Philosophical Transactions*, rather than the *Philosophical Magazine*. Airy at the time was one of the referees reviewing Powell's paper, and probably the most influential person in determining its fate. Surprisingly enough, Airy rejected the publication of Powell's paper in the *Transactions*, despite the fact that it was a critical piece against Brougham, and that he regarded it as "perfectly correct". In his Referee Report of Powell's paper, Airy explained the reason of his rejection as follows:

> It is known to the officers of the Royal Society that I objected strongly to the publication of Lord Brougham's paper . . . in the *Philosophical Transactions*. But, although I think that its errors ought not to have received permanence in our *Transactions*, and although I think it desirable that they should be criticized *somewhere*, I do not think the *Transactions* the proper place. It would be a bad application of our publication to make them subservient.[6]

Here is further evidence of the undulationists' deliberate calculation to divert the public attention from Brougham's work. Although publishing a critical remark on Brougham's work in the *Transactions* would be inevitable in a purely cognitive viewpoint, such a publication might also initiate more debate and give Brougham more publicity. Thus, according to Airy, not only were the actual replications of Brougham's experiments unnecessary, but also the publication of any remarks on Brougham's results in a first rank journal such as the *Philosophical Transactions* was inappropriate.

The debate between Brougham and Powell on diffraction shows that replications

[6] Airy's Referee Report on Powell, (4/5/1852), Royal Society Library, RR. 2,191.

could be performed by different tactics. First, actors could directly repeat an experiment, showing its procedures and results to others in a face-to-face context as Brougham did with Brewster. This direct and face-to-face replication, if successful, is rather convincing, but its multiplication of witnesses is limited - only those who are invited to witness can have the chance to believe.

Second, an experiment can be replicated in a direct but distant way - the experimenter provides the necessary information of his experiment and other people who are interested perform the replication. In our episode, Powell's replication of Brougham's experiment of diffraction belonged to this type. In terms of the power of multiplying witnesses, distance replication is stronger than face-to-face one because through this method witnesses can be achieved beyond the original experiment site. However, because of the involvement of skill or tacit knowledge in experiment, direct replication by others is difficult. The information given by the original experimenters is seldom enough - they cannot articulate some necessary skill or tacit knowledge in their experimental reports. A successful direct replication thus requires intensive communication, especially informal communication, between the original experimenters and repeaters. This informal communication, however, depends upon a series of contingent and contextual factors, like the personal relationships between original experimenters and repeaters.

Third, replication can also be achieved by virtual witnessing, convincing others without really repeating the experiment. This was exactly the tactic used by Brougham and Powell when they wanted to convince others of the legitimacy and reliability of their own experiments. In terms of the power of multiplying witnesses, virtual witnessing is the strongest method, because people can be convinced without really performing the experiment. But during a dispute, for instance, in the debate between emissionists and undulationists about Brougham's experiments, the power of virtual witnessing will be reduced dramatically. In these circumstances, virtual witnessing can easily convince an ally, but hardly persuade a rival. Therefore, the effectiveness of the tactics for experiment replication is highly context-dependent, and the selection of a particular tactic is determined by actors' judgments of the contextual conditions.

2.2 The Rule of Reproducibility

Based upon their studies upon scientists' practice of experiment replication, many philosophers of science regard the rule of reproducibility as the most important criterion for experiment appraisal. In fact philosophers of science have emphasized the value and the importance of reproducibility for a long time. According to Popper, for example, reproducibility is a demarcation criterion for objective observational and experimental knowledge. He argues that "[w]e do not take even our own observations quite seriously, or accept them as scientific observations, until we have repeated and tested them. Only by such repetitions can we convince ourselves that we are not dealing with a mere isolated 'coincidence', but with events which, on account of their regularity and reproducibility, are in principle inter-subjectively testable" (Popper 1968, p.45). Following Popper, many philosophers agree that reproducibility is the essential basis that guarantees the validity of experimental results. Some philosophers further suggest that the requirement of reproducibility should be understood as a mandatory rule for experimental practices. In particular, the rule of reproducibility can be expressed in the form of an hypothetical imperative: if one wishes to produce experimental knowledge, then one ought to conduct experiments whose results are reproducible (Hones 1990, p.586). This suggests that repetition is a necessary procedure for establishing the validity of experimental results, and that scientists will without exception apply the rule of reproducibility whenever they need to evaluate experimental findings.

However, the results of some recent studies cast doubt on the proposed normative or mandatory status of the reproducibility rule. Based on detailed analyses of experimental discoveries in contemporary physics, Alan Franklin summarizes a set of strategies that scientists use in their practices to achieve validity in their experimental findings (Franklin 1986, pp.166-84). Among this set of strategies, repetition is only one of the possible means that, according to Franklin, are neither necessary nor sufficient for the validation of experimental results (Franklin & Howson 1988, p.426). Also, based on interviews with a group of biochemists, Mulkay and Gilbert report that scientists only occasionally make efforts to repeat what somebody else did, and always have several conceptions of what a valuable or proper repetition should be (Mulkay & Gilbert 1986, p.22). These empirical studies suggest that the role of the reproducibility rule in experiment appraisal and the process of experiment repetition are a lot more complicated than philosophers expect.

In the following I illustrate the complexities in the application of the reproducibility rule through analyzing an historical case. This is the debate on the analysis of sunlight in the 1840s. The main themes of this debate were whether a particular experimental finding should be counted as experimental knowledge, and how the reproducibility rule should be properly applied to evaluate this experimental result. This historical episode vividly shows that the reproducibility rule was not applied mandatorily in experiment appraisal. The rule was only applicable under certain conditions, which were not defined logically by the rule itself but determined contextually by scientists. Scientists were able to decide whether or not they should apply the rule, and, more importantly, they could make the rule applicable by creating the conditions for its successful application.

By examining the solar spectrum produced by a prism, Newton in the seventeenth century concluded that there were seven primary colors in sunlight (Newton 1952, p.126). In the early nineteenth century, David Brewster challenged Newton's observations of the solar spectrum. In a paper presented to the Royal Society of

Edinburgh in 1831 and published in its *Transactions* in 1834, Brewster presented a series of new experimental findings inconsistent with Newton's observations. In his experiments Brewster examined the impact of absorbing materials on different rays of sunlight. His experimental apparatus included a prism and a plate of colored glass (Figure 9.2). A narrow beam of sunlight entered a dark room and passed through the prism; the spectrum emerged from the prism, passed through the plate of colored glass and directly into the eye of the observer. When Brewster interposed a plate of purplish blue glass, about one twentieth of an inch think, he found that all the orange and a large part of the green light in the spectrum disappeared. By varying the absorbing material, Brewster could eliminate the indigo and the violet light from the spectrum. According to Brewster, all of these results indicated that the orange, green, indigo, and violet colors in the spectrum were not primary colors. He concluded that the red, yellow, and blue rays were the only primary colors, and that all the others were compound colors, each of them consisting of red, yellow, and blue light in different proportions (Brewster 1834, pp.124-35).

Brewster's new analysis of sunlight relied entirely upon his experimental findings: interpositions of absorbing material caused changes of color in the spectrum. In 1833 George Airy repeated Brewster's experiment, attempting to examine these experimental findings directly. The experimental setting of Airy's repetition was in principle identical to that of Brewster's original experiment (Figure 9.3). However, Airy added a lens in front of the prism to obtain a better image of the spectrum. Instead of observing it directly with the eye, he used a piece of paper as a screen to receive the spectrum. Another significant difference was the position of the absorbing material. Airy placed the plate of colored glass between the light source and the prism, rather than between the prism and the observer as in Brewster's experiment. The results of this repetition, however, were apparently negative. Under most circumstances, Airy said that he did not

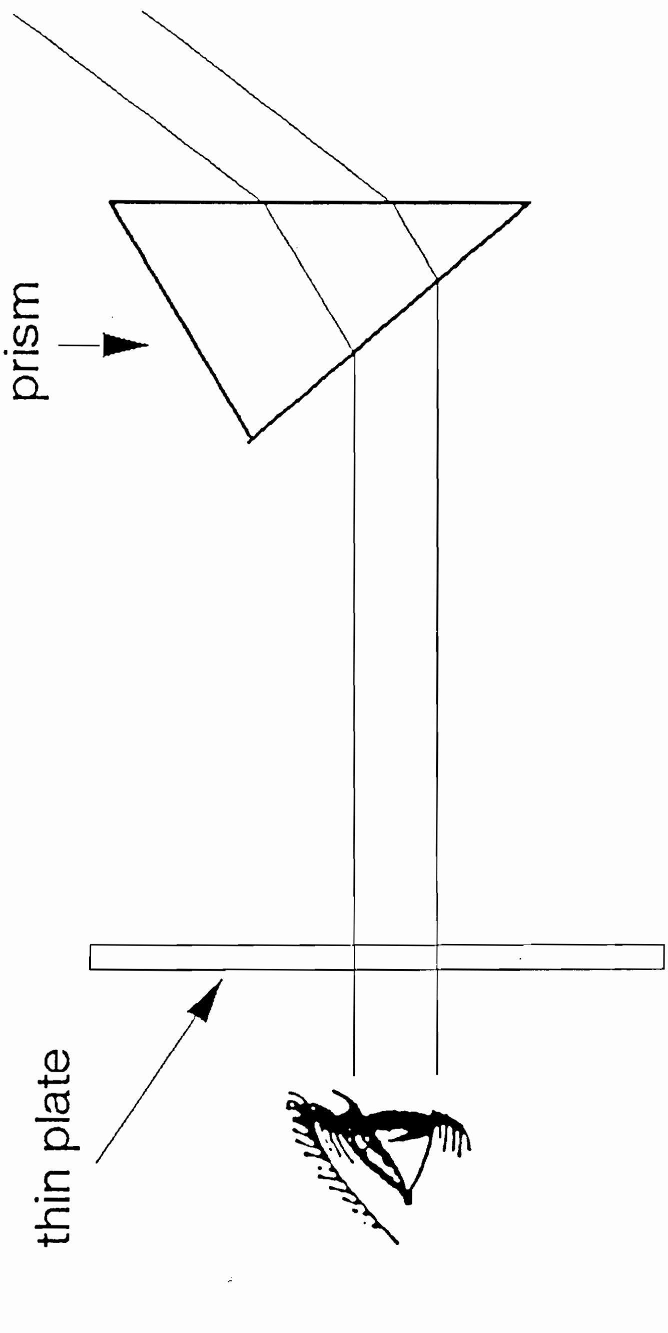

Figure 9.2 Brewster's Experiment on Solar Spectrum

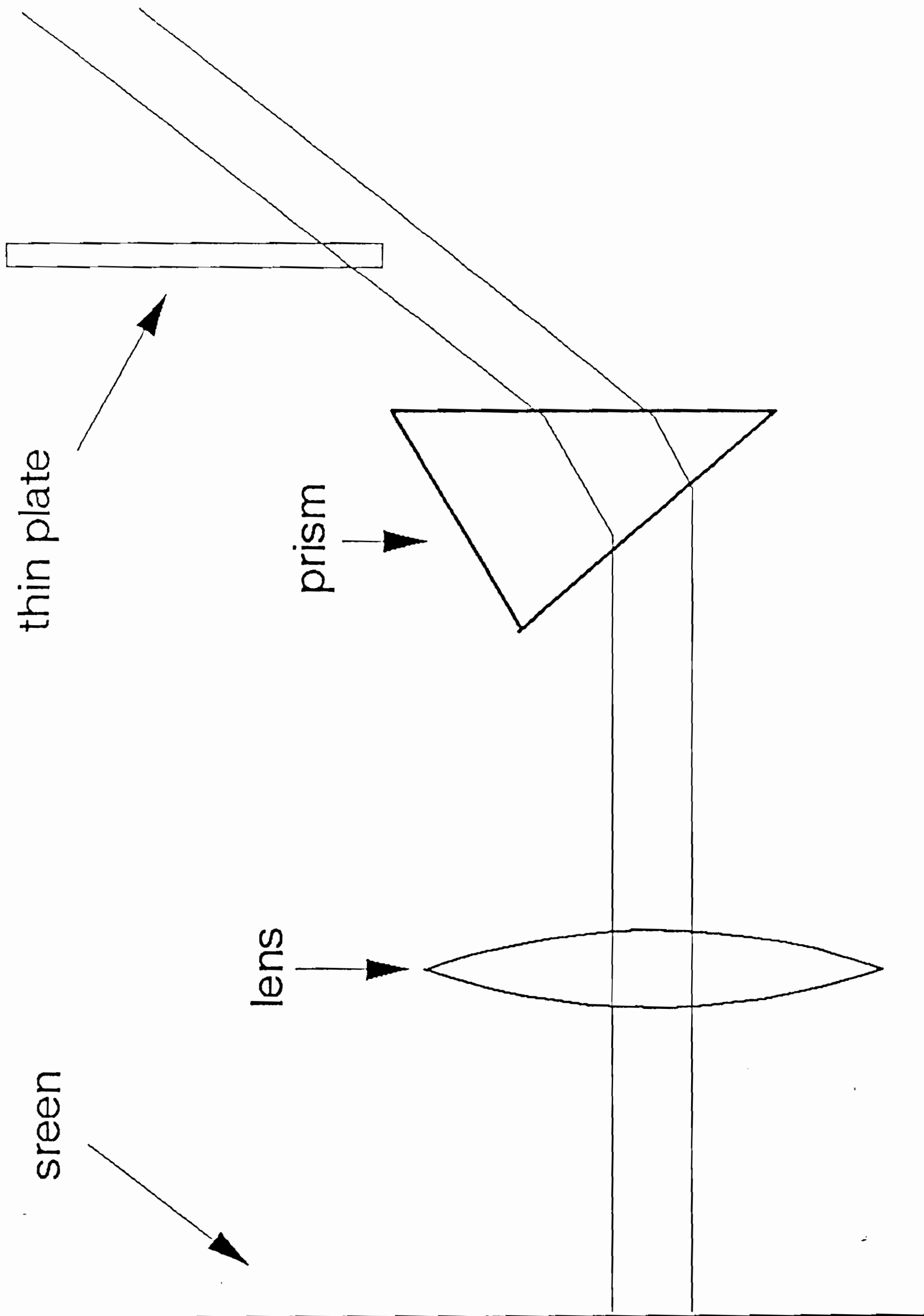

Figure 9.3 Airy's Experiment Replication

observe any change of color in the spectrum caused by absorbing materials. Although in two or three instances he found that the blue color was extended, these alterations disappeared when he prevented the screen from being illuminated by white light coming directly from the light source. Airy finally concluded that: "no change was produced in the qualities of the colours" (Airy 1847, p.75).

Although Airy obtained negative results in his experiment, he was reluctant to publish his findings. He only made an oral report of his experiments to the Cambridge Philosophical Society, in which he merely described his experimental findings, but did not connect them to Brewster's original experiments. Later he recalled:

> I never drew up in writing any orderly statement either of the experiments or of the reasoning connected with them. I had the pencil notes, written each at the instant of making an experiment; and from these notes, or rather from the strong recollection of every experiment detailed in the notes, I made my oral statement (as far as the mere experimental facts were concerned) to the Cambridge Philosophical Society. (*Ibid.*, p.73)

After the oral presentation, Airy did not preserve the pencil notes. "These notes I have mislaid", he said, and he later could not find them any more (*Ibid.*).

Airy's reluctance in publishing his results indicates, perhaps, that he had doubts about his own findings. It is interesting to ask what might have caused Airy's doubts. One possible answer was his concern about the qualifications of the experimenters. In the early 1830s Brewster was one of the most prestigious British scientists in optics, with an especially strong reputation in optical experiment. His contemporaries regarded him as "the father of experimental optics" (Whewell 1967, Vol.2, p.373). The impact of absorbing materials on light was one of the major interests of Brewster, who had studied it and had conducted experiments on it since the early 1820s. By contrast, Airy did not have prestige in experimental optics, although he was well-known as a theoretical analyst and mathematical calculator. Moreover, his attempted replication of Brewster's work was his first experiment about the phenomenon of absorption. Consequently, the members of the scientific community would be likely to interpret the negative results of Airy's repetition not as a challenge to Brewster's experiments, but as evidence of Airy's own failure. The fate of Airy's notes of the experiments indicates that Airy himself may have interpreted the results of his repetition in this way. Airy's doubts about his own experiment suggest, perhaps, that he included the qualifications of experimenters, both the original and the repeater, as one of the conditions for properly applying the reproducibility rule. Once he realized that his repetition did not satisfy this condition, he kept his experimental results private, and in effect gave up his attempt to test Brewster's experimental findings by the reproducibility rule.

The first open criticism of Brewster's experimental findings came from William Whewell in his *History of the Inductive Sciences* published in 1837. In a short footnote, Whewell mentioned that Brewster's experimental result, namely that absorbing materials could change some colors in the spectrum, "has, however, been denied by other experimenters" (*Ibid.*, p.287). Whewell here referred to Airy's experiment, which was unknown to Brewster. But for some reason, Whewell did not reveal Airy's name, nor any details of the experiment. In a review of Whewell's *History*, Brewster asked Whewell to give the details of the alleged experimental denial (Brewster 1837, p.72). The complaint from Brewster forced Whewell to release more information about Airy's work. In the second edition of his *History* published in 1847, Whewell wrote that "Mr. Airy repeated [Brewster'] experiments with about thirty different absorbing substances, and could not satisfy himself that in any case they changed the colour of a ray of given refractive power" (1967, Vol.2, p.288). At the same time, Whewell asked Airy to publicize his results. In response to Whewell's request, Airy in 1847 published a paper in the *Philosophical Magazine*, titled "On Sir David Brewster's New Analysis of Solar Light," providing details of his repetition of Brewster's experiment.

In his 1847 paper, Airy's confidence in his experiment, done more than a decade ago, had increased dramatically. He claimed that "I have no hesitation in saying that no form of experiment anterior to my own has been such as to place its conclusions beyond doubt" (1847, p.76). But when Airy had just completed his repetition, he clearly had doubts about the results of the experiment. Apart from the verbal report to the Cambridge Philosophical Society, he decided to keep his results private, and gave up his attempt to challenge Brewster by employing the reproducibility rule. But fourteen years later, he firmly believed that the results of his repetition were not open to doubt and that they could be used to challenge Brewster's experimental findings, despite the fact that he did not keep any written record of his experiment.

That Airy in 1847 became absolutely confident in his own experiment reflected a dramatic change in the field of optics. By the mid 1840s, the undulatory theory of light, the theory to which Airy was firmly committed, had convincingly demonstrated its explanatory superiority over its rival - the emission theory of light. Accompanying this change, Brewster's prestige had decreased, because of his persistent support of the emission theory. On the other hand, Airy's influence and prestige had considerably risen. For example, since his *Tract* on optics (1831) had become the textbook for the Mathematical Tripos at Cambridge, more and more Cambridge-trained physicists had accepted his version of the undulatory theory. Moreover, Airy in 1835 became the Astronomer Royal, one of the most prestigious scientific positions in nineteenth-century Britain. In this new situation, the obstacle that prevented Airy from employing the reproducibility rule to challenge Brewster in the early 1830s disappeared. In terms of experimenters' qualifications, Airy could now say that his competence was the same as, or even higher than, that of Brewster.

Although Airy's repetition now satisfied the condition concerning experimenters' qualifications, at the same time it encountered new troubles. After he made an oral presentation to the Cambridge Philosophical Society in 1833, Airy gave up his experimental research on the colors of sunlight. Since he had lost the only pencil notes, he was unable to remember even the names and combinations of the absorbing materials he used in his repetition. His 1847 paper was entirely based on his recollection of events that had happened more than a decade ago. What Airy presented in 1847 was not a real repetition but a reconstructed one. Airy openly admitted that this was a "partial imperfection". In other words, Airy realized that the proper conditions for applying the reproducibility rule still did not exist. But this time, he believed that he was able to create such conditions. Airy's tactic was to persuade his readers that his reconstructed repetition based upon personal recollection was as reliable as the real one. On this point, Airy did not give any substantial argument. Instead, he simply made a rhetorical statement, claiming that "upon the method, upon the results, and upon the reasonings, my recollection is as perfect as it was on the days on which the experiments were made". The only evidence Airy gave to support this statement was the order of presentation. He said that "I shall give my statement [in the 1847 paper] in the same order in which I gave it (in the year 1833, I believe) to the Cambridge Philosophical Society" (Airy 1847, p.73). Although the order of presentation was not essential here, Airy hoped that by emphasizing this apparent identity of the original repetition and the reconstructed one his readers would believe in the reliability of his recollection.

The other difficulty Airy had in 1847 concerned the experimental setting. His repetition differed from Brewster's original experiment in many ways, including the use of a paper screen and a lens, and a different position for the absorbing material. These differences might imply that Airy had not replicated the original experiment. Airy, in effect, admitted that he did not have the conditions for properly applying the reproducibility rule, but in his 1847 paper he tried to create these conditions. His key tactic was to show that, although his repetition differed from Brewster's original, these differences would not distort the experimental results but improve the experimental accuracy. For example, Airy argued that, by changing the position of the absorbing material, he could observe the possible change of the spectrum in the most favorable conditions. Airy had placed the plate of colored glass between the light source and the prism, in a position that covered only a portion of the light beam. This arrangement could simultaneously produce two spectra side by side on the screen, one modified by the colored glass and the other not. In Brewster's experiment, however, since the plate of colored glass was located between the prism and the observer, only the modified spectrum appeared. Brewster was not able to compare a modified with an unmodified spectrum simultaneously. According to Airy, this was a fatal defect of Brewster's experimental arrangement:

> Now I state unhesitatingly as my own belief, that the eye has no memory for colours; and I have no confidence in any evidence upon the change of colour, unless the colour which is not altered and the colour in which change is suspected are placed side by side. (*Ibid.*, p.74).

By a simultaneous comparison of the modified and unmodified spectra, Airy claimed that he could obtain a more accurate result than Brewster and that his repetition could be used to test Brewster's original experiment, though there were differences between them.

Airy's concerns in his 1847 paper suggested that he included not only the experimenters' qualifications, but also the nature of the repeated experiment, among the conditions for properly applying the reproducibility rule. He understood that to be identical with the original experiment was the most important requirement. He also realized that his work in 1847 did not satisfy this condition, because he was now merely reconstructing a repetition and because his apparatus had differed from Brewster's original experiment in many ways. However, Airy did not simply give up his attempt to attack Brewster by employing the reproducibility rule. Instead, he endeavored to retrieve the situation by arguing the reliability of his personal recollection and by demonstrating the advantages of his own experimental arrangement. In this case, the application of the reproducibility rule required skillful and creative effort.

Not surprisingly, Airy's paper initiated a strong reaction from Brewster. In the same year, 1847, Brewster published a reply to Airy in the *Philosophical Magazine*, in which he both attacked Airy's repetition and defended his original experiment.

According to Brewster, the most formidable problem in Airy's account was that he built everything upon his recollection. Brewster wrote that "no apology can be made for those who, with the means and the leisure for repeating their experiments, bring forward their recollections to discredit or to overturn the researches of others who have laboured patiently and successfully in the same field of scientific research" (1847a, p.157). Brewster continued that it was particularly wrong for Airy to base his experiment appraisal upon his own recollection, because Airy himself confessed that he had no memory of colors.

According to Brewster, his opponent was "bound to repeat the identical experiments which he challenges, with similar apparatus and similar materials". If discrepancies were found in the process of these identical repetitions, the challenger should "inquire into the causes by which such discrepancies have arisen", and "establish his own views by new and effective experiments". After the causes were identified, the challenger also needed to justify his claim publicly, "to publish his researches in vindication of his charge against a fellow-labourer in science" (*Ibid.*, p.155). The most important point here was the condition of identity. This was the central methodological principle concerning the nature of the repeated experiment, and equally central for properly applying the reproducibility rule. This methodological principle was in fact accepted by Airy, although he did not explicitly state it as Brewster did.

Following this methodological principle, Brewster decided not further to repeat Airy's experiment, because Airy in his 1847 paper only reconstructed his repetition. If Airy's work had been a real repetition, Brewster could have designed another repetition with an identical experimental setting. But facing a reconstructed repetition, Brewster was unable to figure out what exactly Airy's experimental arrangement was. Airy himself could not remember every detail, and even forgot what kind of absorbing materials he actually used. No matter what results Brewster came up with, Airy could easily deny Brewster's challenge by claiming that Brewster's repetition was not exactly identical with his own. A direct application of the reproducibility rule to Airy's experiment became impossible.

Instead of making new experiments, Brewster simply applied the methodological principle he summarized, which was also accepted by Airy, as a criterion to criticize Airy's work. Brewster particularly focused on the identity of his original experiment and Airy's repetition. One salient difference in Airy's repetition was the position of the absorbing material. Airy had argued that this different arrangement could make simultaneous comparison of the modified and unmodified spectrum, which was an essential improvement in observation accuracy because the eye had no memory of color. But Brewster pointed out that "it may be true that *the eye* has no memory of any kind, and therefore not for colours; but *I have a memory* for colours" (*Ibid.*, p.154, original emphasis). A simultaneous comparison was not necessarily an improvement and the different position of the absorbing material in Airy's repetition was not justified.

Brewster also pinpointed another different arrangement in Airy's repetition. Instead of observing the spectrum directly with the eye, Airy used a piece of paper as a screen to receive the spectrum. This technique, however, would create some distortions, according to Brewster. He believed that while Airy was viewing the paper screen "his retina was influenced by all the various colours which shone in his modified and unmodified spectrum" (*Ibid.*, p.158). Brewster's allegation was quite reasonable. In the mid 1840s, the technique of using a paper screen to receive images in optical experiments was replaced by direct observation of the image by the eye, thus eliminating distortions caused by reflection at the surface of paper. This technique of direct observation had been widely appreciated by first rank researchers in optics including Fresnel and was actually employed in Brewster's experiments. By exposing the problems associated with Airy's technique of observing the spectrum, Brewster implied that the use of a paper screen was responsible for Airy's negative results.

In addition to criticizing Airy's repetition, Brewster in his paper also provided further arguments to support the validity of his original experiment. Brewster tried to impress his readers with his qualifications as a first rank experimenter. He wanted to connect his qualifications with the quality of the experiment he performed, a link that had probably been accepted by Airy when he dealt with his 1832 experimental results. Brewster reminded his readers that the Royal Society had awarded him the Keith Biennial Prize in 1833 for his experiment on a new analysis of sunlight. He gave a full citation of the Royal Society's announcement of his award, and carefully explicated the intention of the Society in giving him this award as a recognition of "the accuracy of my experiments" and an official recognition of his qualifications as a first rank experimenter (*Ibid.* p.156). While emphasizing his qualifications as a first rank experimenter in the experiment in question, Brewster implied that Airy did not have the qualifications he did, and that the results of his repetition might not be reliable.

Airy did not respond to Brewster's criticisms. Later Brewster published several papers to justify his original experiment on the change of colors in the solar spectrum (Brewster 1847b, 1848), but Airy kept silent, without saying a word publicly. This suggests that Airy may have realized some of the problems in his repetition or have accepted some of Brewster's arguments. Since Airy did not object to Brewster's methodological principles concerning the appropriate conditions of repetition, it would be difficult for him to make substantial counterattacks unless he had new experimental findings. In the early 1850s the majority of the optical community agreed that the changes of colors in the solar spectrum caused by absorbing materials were physical facts. Helmholtz and later Maxwell, for instance, admitted that absorption could cause change of colors, although they had different interpretations of how these changes might happen (Brewster 1855, Vol.1, p.123-6; Larmor 1971, Vol.2, p.21). The debate between Brewster and Airy finally ended and a consensus among the majority of the optical community was achieved in Brewster's favor.

The debate between Brewster and Airy on experiment appraisal demonstrates that the process of experimental replication, or the application of the reproducibility rule, was much more complicated than what we might expect. First, contrary to some philosophical accounts of science, the reproducibility rule did not have a mandatory status in this instance of experiment appraisal. In the debate between Brewster and Airy, although both sides agreed that repetition was the essential method for testing experimental findings, neither of them felt compelled to obey the reproducibility rule. This was because the reproducibility rule itself did not specify the appropriate conditions for its application. Rather, it was the actors who decided whether the reproducibility rule was applicable in a particular case. A good example was Brewster's decision not to test Airy's work by a physical repetition. After the rule was known as in principle applicable, actors still needed to determine whether applying the rule to the particular case was appropriate. If not, they could terminate the application half way, just as Airy did in the early 1830s. Second, the successful application of the reproducibility rule always required actors' creative efforts. Like theory appraisal, experimental appraisal is not an agency-free process. Experiment appraisal is executed and completed by human agents through a series of practices including the formulation of goals, the selection of appraisal tactics, and the execution of the selected tactic. More importantly, although sometimes the rule was apparently inapplicable, actors were even able to create or produce the conditions needed for its proper application, by skillful and creative work.

An example was Airy's effort to prove the legitimacy of his repetition based on personal recollection. The active role that actors play in the process of repetition suggests that the result of an experiment appraisal may depend upon actors' personal efforts.

2.3 The Epistemic Bedrock for Experiment Appraisal

The historical complexities exhibited in the debate between Brougham and Powell, as well as the debate between Brewster and Airy lead to some serious questions. First, if the reproducibility rule is not mandatory and if its applications are not defined by the rule itself, what is the basis for its application? Second, if scientists are free to decide whether, when and how to conduct experiment replication, how can objective conclusions be achieved in their appraisals of experiment? These are questions about the epistemological foundation for experiment replication, or for applying the reproducibility rule.

One possible answer to these questions comes from the traditional philosophy of science. According to Popper, "just as chess might be defined by the rules proper to it, so empirical science may be defined by means of its methodological rules" (Popper 1968, p.53). Thus, if the reproducibility rule does not specify its application condition, philosophers can and should identify or construct some new methodological rules that provide the guidelines for its applications. Following this line, philosophers believe that they are able to construct a group of rules that specify the appropriate conditions for such applications, and expect that these newly constructed rules can become the bedrock for applying the reproducibility rule. The major problem for this attempt, however, is the "vicious regress" that it may create (Barker 1988, p.100). For the sake of argument, we can express the reproducibility rule as follows: "If one wishes to produce experimental knowledge, then one ought to conduct experiments whose results are reproducible". Now, if we want to introduce a new rule for the application of the reproducibility rule, the new rule should first specify what a reproducible result looks like. We may express this new rule in a specific form, say, the following conditional sentence:

> Rule 1: If an experimental result is reproducible, then the same result ought to be obtained through an appropriately repeated experiment.

Now, the question of applying the rule of reproducibility becomes the question of specifying the conditions for an appropriately repeated experiment. If we insist on using rules to solve this problem, we should expect a second rule to determine the appropriateness of a repeated experiment. We also can express this rule in a specific conditional sentence:

> Rule 2: If a repeated experiment is appropriate, then its experimental setting ought to be identical with the one in the original experiment.

However, the introduction of the second rule does not solve the problem. Before we can apply the second rule to any case, we need to know how we can judge the identity of two objects, or two actions, that happen in different times and different places. We need a third rule to specify the conditions of identity. No matter what the third rule says, we can expect that a new rule is needed for specifying the conditions of its application. This is a "vicious regress"; an infinite number of rules is needed for the application of the reproducibility rule and for any actual experiment appraisal. This, however, is an absolutely implausible picture for scientists' daily appraisal practices.

Another possible answer to the questions concerning the foundation of experiment replication appeals to conventions that scientists reach in a process of negotiation. Having realized the problem of vicious regress, some scholars turn to Wittgenstein's philosophy for help.[7] Inspired by Wittgenstein's ideas on rule-following (1958, $185-

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[7] Collins identifies the problem of vicious regress with a different analysis. According to Collins, because of the skill-like nature of experimentation, the appropriateness of a replication, including the competence of the experimenter and the integrity of the experiment, can only be ascertained by examining its results. But appropriate results in turn can only be recognized from competently performed replication. This is a vicious circle, which he labels the "experimenter's regress" (Collins 1985, p.130).

241), sociologists from the Strong Programme have developed a radically relativistic position on experiment appraisal. Harry Collins for example argues that even a simple rule of arithmetic like "add a 2 and then another 2 and then another and so on" can be interpreted in dramatically different ways (1985, p.13). From these different interpretations, a consensus can only be reached through negotiations, and through the social conventions that compel people to accept certain actions as right and others as wrong.[8] Therefore, David Bloor claims that "the application of formal principles is always a potential subject for informal negotiation" (1990, p.133). In his study of the replications of experiments about gravitational radiation, Collins shows that application of the reproducibility rule and the process of replication first occur without well-defined standards. Scientists subsequently negotiate the conditions that specify proper experiment replication. These negotiations among scientists break the vicious regress and guarantee the success of replications (Collins, 1981). Collins' solution to the vicious regress, however, leads to a relativist and subjectivist interpretation of experiment appraisal. According to this interpretation, on the one hand, the negotiations among scientists and the relevant social conventions determine the standards for appropriate replications and consequently the results of experiment appraisal, but, on the other hand, natural or objective phenomena have no role at all in the appraisal processes. This is also an absolutely implausible picture for scientists' daily appraisal practices.[9]

The underlying theme of Collins' and Bloor's relativism is the assumption that individual actions in rule-following are entirely determined by some kind of social mechanism. On the one hand, they assume that following or obeying a rule at its very

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[8] Here "social conventions" are used strictly in a sociological sense, referring to rules based upon general agreements among individuals.

[9] It is also very difficult for this subjectivist interpretation to explain the consensus in appraisals among individuals with different interests.

beginning is an purely individual action, and hence actors may have dramatically different interpretations of the rule. On the other hand, they claim that negotiations as well as social conventions are needed for settling these differences. But our historical case is a clear counter-example to this general assumption. In the debate between Brewster and Airy, we do not find negotiations between Brewster and Airy. A consensus was finally reached in this episode because actors' opinions on the conditions for properly applying the reproducibility rule were in principle coherent.

Collins' and Bloor's relativism also involves an inconsistency. Wittgenstein's view on rule-following, accepted by both Collins and Bloor, is set up to overthrow the quasi-causal picture of rule-following. According to Wittgenstein, following a rule is "a spontaneous decision" (Wittgenstein 1978, VI, $24). In Collins's and Bloor's relativist account, however, individuals' rule-following activities are explained by social conventions that are external to the individual practices. Further, according to the interest theory proposed by the Strong Programme, these external factors work as causal mechanisms affecting actors' decisions during the negotiations. Hence, using social conventions to account for the individual actions is to appeal to an external causal explanation. Since both Collins and Bloor regard Wittgenstein's account of rule-following as the starting point of their own analysis, their assumption and their following social explanations inevitably lead to an internal inconsistency.

If we take Wittgenstein's account of rule-following seriously, Collins' and Bloor's relativist position has to be abandoned. In his investigation of the nature of rules and their role in language, Wittgenstein makes it clear that following or obeying a rule is not based upon any interpretation, explication or negotiation of the appropriate conditions for applying the rule. According to Wittgenstein, "obeying a rule is a practice" (1958, $202). The simplest example to illustrate this point is the case of following a signpost. According to Wittgenstein, a signpost is just like a rule specifying which direction we should follow. In fact every signpost indicates two directions, from the post to the tip and from the tip to the post. But in daily life we do not need any new rules or any interpretation to tell us which direction we should follow. "[A] person goes by a sign-post only in so far as there exists a regular use of sign-post, a custom" (*Ibid*, $198). We obey a rule because there are regular practices or customs of following the rule in that way. We are trained to do so and through such training we firmly believe that what we do is simply the way it should be done.

Also in contrast to Collins and Bloor, Wittgenstein insists that individual actions cannot and should not be isolated from the regular practices or the customs of the community. For Wittgenstein, "it is not possible to obey a rule 'privately': otherwise thinking one was obeying a rule would be the same thing as obeying it" (*Ibid.*, $202). Individual actions, on the one hand, are bound by the customs of the community, and on the other hand, they constitute the community's customs. The connection between individual actions and the customs of the community provides a ground for producing consensus among the individuals who belong to the same community.

Like following a signpost, following or applying the reproducibility rule is also a practice, which is closely related to the regular practices or the customs existing in the relevant scientific community. In the debate between Brewster and Airy, the actors indeed applied the reproducibility rule in several different ways. Sometimes they were concerned about the qualifications of the experimenters, and sometimes they were concerned about the nature of the experiments *per se*. But the crucial point here is that these different concerns, or different ways of applying the rule, were in principle coherent. The concern about the qualifications of the experimenters did not generate conflict with the concern about the nature of the experiments. Neither Brewster nor Airy applied the rule in a way that caused a protest from his rival. Since there was no fundamental conflict between the ways that Brewster and Airy applied the rule, they did not need to negotiate in order to achieve a consensus. The final consensus between Brewster and Airy "is not agreement in opinion but in form of life" (*Ibid.*, $242). As members of the same scientific community, Brewster and Airy shared their training, their practice, and their form of life in many ways. Their understanding of the reproducibility rule, or the conditions for proper repetitions, was not simply their private judgment about the meaning of the rule, but reflected the training, the practice, and the form of life they shared. In short, although actors may apply the reproducibility rule in different ways, and they are able to decide whether, when and how to apply the rule, their applications will converge to a consensus on the ground of the shared practices in a community. In this way, the results of experiment replication are firmly built upon a non-subjective ground, rather than upon negotiations, personal persuasion or even coercion.

Conclusion: In the last section we have seen that there are three different approaches to answering questions about the epistemological foundation of experiment appraisal. The first attempt comes from the traditional philosophy of science, which believes that experiment appraisal can be defined by a group of methodological rules. But, as I have indicated, this attempt will inevitably lead to a vicious regress. The second attempt comes from sociology of science, which regards experiment appraisal as a process of negotiation. But this attempt is not acceptable either because it will lead to a subjectivist and relativist interpretation of science.

The third attempt appeals to Wittgenstein's account of rule-following and understands experiment appraisal as a practice. Wittgenstein's account of rule-following has drawn attention from many philosophers, but few of them have examined its implications to the practice of science.[10] In the last section I have argued that the

[10] For pioneer works on Wittgenstein's influence on the philosophy of science, see Barker (1986, 1988), and Dreyfus & Dreyfus (1986).

application of the reproducibility rule should be understood as a practice that is closely related to the regular practices or the customs of the relevant scientific community. Given this understanding of the epistemological ground for applying the reproducibility rule, the problem of vicious regress immediately disappears. The applications of the rule are now defined by the regular practices or the customs of a scientific community and no further rules for specifying applications are needed. Our understanding of the epistemological foundation of the reproducibility rule also eliminates the subjectivist and relativist interpretation of experiment appraisal proposed by sociologists. We can now build the consensus in experiment appraisal upon a basis of practice rather than negotiation. Unlike negotiation that only reflects the interactions among human agents, human practice inherently includes both the interactions among human beings and the interactions between human beings and the natural world. More precisely, human practices embrace human actions, objects, and sometimes language (Barker 1985, p.4). In this theoretical framework, the natural world is not separate from practice, but an intrinsic part of it. By building the application of the reproducibility rule upon the regular practices or the customs of the relevant scientific community, objective conclusions can be achieved in experiment appraisal.

This understanding of experiment appraisal should be extended to the issue of theory appraisal. Regarding their epistemological foundation, there is no essential difference between theory appraisal and experiment appraisal. In this chapter I have illustrated the complexities in theory appraisal and argued that theory appraisal is a process in which agents employ a variety of tactics for achieving particular goals. Only when we consider theory appraisal as a practice can the conceptions of agent, goal, and tactic be fully comprehensible.

Our understanding of the epistemological foundation of experiment appraisal can also shed light on a series of issues about the nature of scientific change or scientific revolution. Our historical study has indicated that the optical revolution was deeply shaped by a series of cognitive and social factors. Neither cognitive nor social factors alone can provide a satisfactory interpretation of the replacement of the emission theory by the undulatory theory in the early nineteenth century Britain. Our historical study has also showed the important role of individual actors in the process of scientific change. Individual actors not only were able to make their choices freely among the options produced by the relevant cognitive and social factors, but were also enabled to shape the logical relationship between rival theories and between theory and experiment. The key to understand this historical event is to adopt a theoretical perspective that, on the one hand, takes both cognitive and social factors into account, and, on the other hand, recognizes the role of individual actors in the process of scientific change. One way to achieve these goals is to understand science as a set of practices. The concept of practice generally embraces human actions, objects, and language. In the context of science, practices include individual scientists' actions, their interaction with the natural world, and their application of scientific theory. By understanding science as a set of practices, therefore, we are able to recognize the role of individual actors as well as cognitive and social factors, and have a solid ground for understanding scientific change.

If we understand science as a set of practices, then an immediate implication is an alternative approach to study scientific change. Traditional philosophy of science tends to describe scientific change as conceptual and theoretical evolutions, because it is only concerned about the end product of science - scientific theories. From a practical viewpoint, however, the processes that produce the end product of science are more interesting and important than scientific theories themselves. Thus, a scientific change not only involves evolutions in theoretical viewpoints, but, probably more important, evolutions in instruments, techniques, and skills.

The alternative theoretical perspective proposed here gives us a better understanding of the optical revolution. Our historical study has indicated that there was substantial development of experimental knowledge during the extended emission-undulatory controversy, contributed by both emissionists and undulationists. Our historical study has also shown that the change of the theoretical view on the nature of light, light as particles or as vibrations, was not the only issue in the optical revolution. A more fundamental change concerned skills, from a separation between experimental skills and mathematical skills, represented by emissionists and the older-generation undulationists, to an integration of these skills achieved by the new-generation undulationists. All these important features of the optical revolution have been ignored by previous historical studies, because they cannot be properly located within the old philosophical framework that limits its attention to the development of scientific theory. However, within a philosophical framework that conceptualizes science as a set of practices, issues about the development of experimental knowledge and the integration of experimental and mathematical skills draw immediate attention. Following this alternative approach to study the optical revolution, the continuity in the history of optics, especially the development of optical knowledge from the mid-1830s to the later 1850s, becomes apparent. More importantly, by appreciating the contributions of both emissionists and undulationists to optical theories as well as to instruments, techniques, and skills, the development of optical knowledge during this historical period finally becomes comprehensible.

References:

Airy to Herschel, (July 20, 1830), Royal Society Library, Herschel Paper, MS. 1.43.

Airy's Referee's Report on Powell's Paper, (April 5, 1852), Royal Society Library, RR. 2.191.

Airy's Referee's Report on Brougham's Paper, (June 17, 1852), Royal Society Library, RR. 2.36.

Airy, G., (1826), *Mathematical Tracts on Physical Astronomy, the Figure of the Earth, Precession and Nutation, the Calculus of Variations*. Cambridge: Deighton.

Airy, G., (1831), *Mathematical Tracts on the Lunar and Planetary Theories, the Figure of the Earth, Precession and Nutation, the Calculus of Variations, and the Undulatory Theory of Optics*. Cambridge: Deighton.

Airy, G. , (1833 a), "Remarks on Mr. Potter's Experiment on Interference", *Philosophical Magazine* 2: 161-167.

Airy, G., (1833 b), "Remarks on Sir D. Brewster's Paper 'On the Absorption of Specific Rays, &c.'", *Philosophical Magazine* 2: 419-424.

Airy, G., (1833 c), "Results of Repetition of Mr. Potter's Experiment of Interposing A Prism in the Path of Interfering Light", *Philosophical Magazine* 2: 451.

Airy, G., (1833 d), "On A Remarkable Modification of Newton's Rings", *Transactions of the Cambridge Philosophical Society* 4: 279-288 .

Airy, G., (1840 a), "On the Theoretical Explanation of An Apparently New Polarity in Light", *Philosophical Transactions* 130: 225-239.

Airy, G., (1840 b), "On A New Apparent Polarity of Light", *Report of the British Association* 10: 3-5.

Airy, G., (1841 a), "Supplement to A Paper "on the Theoretical Explanation of An Apparent New Polarity in Light"", *Philosophical Transactions* 131: 1-10.

Airy, G., (1841 b), "On the Diffraction of An Annular Aperture", *Philosophical Magazine* 18: 1-10.

Airy, G., (1846 a), "On the Bands Formed by the Partial Interception of the Prismatic Spectrum", *Philosophical Magazine* 29: 337-341.

Airy, G., (1846 b), "On the Equations Applying to Light under the Action of Magnetism", *Philosophical Magazine* 28: 469-477.

Airy, G., (1847), "On Sir David Brewster's New Analysis of Solar Light", *Philosophical Magazine* 30: 73-76.

Ames, J. (ed.), (1898), *Prismatic and Diffraction Spectra: Memoirs by Joseph Von Fraunhofer*. London: Harper & Brothers Publishers.

Anon., (1817-1818), "Review of Humboldt's *Relation Historique du Vogage*", *Quarterly Review* 18: 135-138.

Anon., (1910), "Brougham and Vaux, Henry Peter Brougham", in *Encyclopaedia Britannica*, 11th Edition, Vol.4. New York: The Encyclopaedia Britannica, Inc., pp.652-655.

Anon., (1917), "Richard Potter", in *Dictionary of National Biography*, Vol. 16. Oxford: Oxford University Press, p.219.

Anon., (1926), "Sir David Brewster", *Encyclopedia Britannica*, 13th Edition, Vol. 4. New York: The Encyclopaedia Britannica, Inc., pp.513-514.

Babbage, C., (1829), "Account of the Great Congress of Philosophers at Berlin on the 18th September 1828", *Edinburgh Journal of Science* 10: 231-232.

Barker, P., (1985), "Wittgenstein as Philosopher of Science", Unpublished Manuscript..

Barker, P., (1986), "Wittgenstein and Authority of Science", in W. Leinfellen and F. Wuketits (eds.), *The Task of Contemporary Philosophy*. Vienna: Holder-Pichler-Tempsky, pp. 265-267.

Barker, P., (1988), "The Reflexivity Problem in the Psychology of Science", in B. Gholson, *et al.* (eds.), *Psychology of Science*. Cambridge: Cambridge University Press, pp. 92-114.

Barton, J., (1831), "On the Inflexion of Light", *Abstract of the Papers in Philosophical Transactions* 3: 72-73.

Barton, J., (1833 a), "On the Inflexion of Light", *Philosophical Magazine* 2: 263-269.

Barton, J., (1833 b), "On the Inflexion of Light, in Reply to Professor Powell", *Philosophical Magazine* 3: 172-178.

Bechtel, W., (1990), "Scientific Evidence: Creating and Evaluating Experimental Instruments and Research Techniques", in A. Fine, M. Forbes, and L. Wessels (eds.), *PSA 1990*. East Lansing, MI: Philosophy of Science Association, pp.559-572.

Bloor, D., (1991), *Knowledge and Social Imagery*. Chicago: The University of Chicago Press.

Botting, D., (1973), *Humboldt and the Cosmos*. New York: Harper & Row.

Brewster to Brougham, (November 6, 1832), Library of University College, Brougham Collection, 15,744.

Brewster to Brougham, (November 12, 1832), Library of University College, Brougham Collection, 15,745.

Brewster to Brougham, (December 11, 1832), Library of University College, Brougham Collection, 15,746.

Brewster to Brougham, (January 3, 1833), Library of University College, Brougham Collection, 15,747.

Brewster to Brougham, (December 14, 1841), University College Library, Brougham Collection, 26,624.

Brewster to Brougham, (September 26, 1847), University College Library, Brougham Collection, 26,632.

Brewster to Brougham, (August 29, 1848), Library of University College, Brougham Collection, 26,634.

Brewster to Brougham, (September 9, 1848), University College Library, Brougham Collection, 26,635.

Brewster to Brougham, (September 25, 1848), University College Library, Brougham Collection, 26,636.

Brewster to Brougham, (December 16, 1848), University College Library, Brougham Collection, 26,637.

Brewster to Brougham, (February 21, 1849), University College Library, Brougham Collection, 26,638.

Brewster to Brougham, (August 25, 1849), University College Library, Brougham Collection, 26,643.

Brewster to Brougham, (September 20, 1849), University College Library, Brougham Collection, 26,648.

Brewster to Brougham, (October 2, 1849), University College Library, Brougham Collection, 26,649.

Brewster to Brougham, (February 7, 1850), University College Library, Brougham Collection, 26,655.

Brewster to Brougham, (February 15, 1850), University College Library, Brougham Collection, 26,656.

Brewster to Brougham, (August 29, 1851), University College Library, Brougham Collection, 26,665.

Brewster to Brougham, (May 8, 1853), University College Library, Brougham Collection, 26,670.

Brewster to Brougham, (August 28, 1856), University College Library, Brougham Collection, 26,695.

Brewster to Brougham, (March 10, 1858), University College Library, Brougham Collection, 26,703.

Brewster to Brougham, (April 25, 1860), University College Library, Brougham Collection, 26,744.

Brewster to Brougham, (September 25, 1861), University College Library, Brougham Collection, 26,764.

Brewster to Herschel, (December 6, 1828), Royal Society Library, Herschel Collection, HS. 4.261.

Brewster to Stokes, (March 8, 1861), in J. Larmor (ed.), *Memoir and Scientific Correspondence of the Late Sir George Stokes*.Cambridge: Cambridge University Press, pp.153-154.

Brewster, D., (1813), *A Treatise on New Philosophical Instruments, for Various Purposes in the Arts and Sciences, with Experiments on Light and Colours*. Edinburgh: John Murray.

Brewster, D. , (1814), "On the Polarization of Light by Oblique Transmission through All Bodies, Whether Crystallized or Uncrystallized", *Philosophical Transactions* 104: 219-230.

Brewster, D., (1815 a), "On the Laws Which Regulate the Polarization of Light Be Reflection from Transparent Bodies", *Philosophical Transactions* 105: 125-159.

Brewster, D., (1815 b), "On A New Species of Coloured Fringes Produced by the Reflection of Light Between Two Plates of Glass of Equal Thickness", *Transactions of the Royal Society of Edinburgh* 7: 435.

Brewster, D., (1815 c), "On the Optical Properties of Sulphuret of Carbon, Carbonate of Barytes and Nitrate of Potash, with Inferences Respecting the Structure of Doubly Refracting Crystals", *Transactions of the Royal Society of Edinburgh* 7: 285-302.

Brewster, D., (1819), "On the Action of Crystallized Surfaces upon Light", *Philosophical Transactions* 109: 145-160.

Brewster, D. , (1820), "Account of Fresnel's Discoveries Respecting in the Inflexion of

Light", *The Edinburgh Philosophical Journal* 2: 150.

Brewster, D., (1821), "Historical Account of Discoveries Respecting the Double Refraction and Polarization of Light, ... Period III - Containing the Investigations of Newton, Beccaria, Martin, Hauy, Wollaston, and Laplace", *Edinburgh Philosophical Journal* 4: 124-152.

Brewster, D., (1822), "Optics", in *Whiting & Watson's American Edition of the New Edinburgh Encyclopedia*, Vol. 15. New York: Samuel Whiting, pp.460-798.

Brewster, D., (1823), "Description of A Monochromatic Lamp for Microscopical Purpose, etc, with Remarks on the Absorption of the Prismatic Rays by Coloured Media", *Transactions of the Royal Society of Edinburgh* 9: 442.

Brewster, D., (1829), "On A New Series of Periodical Colours Produced by Grooved Surfaces of Metallic and Transparent Bodies", *Philosophical Transactions* 119: 301-316.

Brewster, D., (1831 a), *Treatise on Optics*. London: Longman.

Brewster, D., (1831 b), *The Life of Sir Isaac Newton*. London: John Murray.

Brewster, D. , (1832), "Report on the Recent Progress of Optics", *Report of the British Association* 2: 308-322.

Brewster, D., (1833 a), "Observations of the Lines of the Solar Spectrum, and on Those Produced by the Earth's Atmosphere, and by the Action of Nitrous Acid Gas", *Transactions of Royal Society of Edinburgh* 12: 519-530.

Brewster, D., (1833 b), "Observations on the Absorption of Specific Rays, in Reference to the Undulatory Theory of Light", *Philosophical Magazine* 2: 360-363.

Brewster, D., (1834), "On A New Analysis of Solar Light, Indicating Three Primary Colours, Forming Coincident Spectra of Equal Length", *Transactions of the Royal Society of Edinburgh* 12: 123-137.

Brewster, D., (1836), "On the Action of Crystallized Surfaces upon Common and Polarized Light", *Report of the British Association* 6: 13-16.

Brewster, D., (1837 a), "On A New Property of Light", *Report of the British Association* 7: 12-13.

Brewster, D., (1837 b), "Review of Whewell's *History of the Inductive Sciences*", *Edinburgh Review* 66: 58-79.

Brewster, D., (1838 a), "Review of A. Comet's *Cours de Philosophie Positive*", *Edinburgh Review* 67: 143-163.

Brewster, D., (1838 b), "On A New Kind of Polarity in Homogeneous Light", *Report of the British Association* 8: 13-14.

Brewster, D., (1838 c), "To the Editor of the *Athenaeum*", *Athenaeum* 577: 826.

Brewster, D., (1838 d), "Reply to Prof. Powell", *Athenaeum* 580: 876.

Brewster, D., (1839), "Observations on Prof. Powell's Explanation of Some Optical Phenomena Observed by Sir David Brewster", *Athenaeum* 624: 781.

Brewster, D., (1840), "On Prof. Powell's Measures of the Indices of Refraction for the Lines G and H in the Spectrum", *Report of the British Association* 10: 5.

Brewster, D., (1842 a), "On A New Property of the Rays of the Spectrum, with Observations on the Explanation of It Given by the Astronomer Royal, on the Principles of the Undulatory Theory", *Report of the British Association* 12: 12.

Brewster, D., (1842 b), "On Crystalline Reflection", *Report of the British Association* 12: 13-14.

Brewster, D., (1842 c), "Review of Whewell's *The Philosophy of the Inductive Sciences*", *Edinburgh Review* 74: 265-306.

Brewster, D., (1843), "On the Compensations of Polarized Light, with A Description of A Polarimeter for Measuring Degree of Polarization", *Transactions of the Royal Irish Academy* 19: 377-392.

Brewster, D., (1845), "On A New Polarity of Light, with An Examination of Mr. Airy's Explanation of It on the Undulatory Theory", *Report of the British Association* 15: 7-8.

Brewster, D., (1847 a), "Photography", *North British Review* 7: 248-269.

Brewster, D., (1847 b), "On the Polarization of the Atmosphere", *Philosophical Magazine* 31: 444-454.

Brewster, D., (1847 c), "Reply to the Astronomer Royal on the New Analysis of Solar Light", *Philosophical Magazine* 30: 153-158.

Brewster, D., (1849), "On the Decomposition and Dispersion of Light Within Solid and Fluid Bodies", *Transactions of the Royal Society of Edinburgh* 16: 111-121.

Brewster, D., (1852), "On Some New Phenomena of Diffraction", *Report of the British Association* 22: 24-25.

Brewster, D., (1854), "Review of F. Arago, His Life and Discoveries", *North British Review* 20: 246-269.

Brewster, D., (1855), *Memoirs of the Life, Writings and Discoveries of Sir Isaac Newton*. Edinburgh: Thomas Constable and Co.

Brewster, D., (1858), "Research on Light", *North British Review* 29: 96-115.

Brewster, D., (1866), "Presidential Address to the Royal Society of Edinburgh", *Proceedings of the Royal Society of Edinburgh* 6: 2-15.

Brougham, H., (1803 a), "Review of Young's the Barkerian Lecture on the Theory of Light and Colours", *Edinburgh Review* 1: 450-456.

Brougham, H., (1803 b), "Review of Young's An Account of Some Cases of the Production of Colours, Not Hitherto Described", *Edinburgh Review* 1: 457-460.

Brougham, H., (1804), "Review of Young's Barkerian Lecture, Experiments and Calculations Relative to Physical Optics", *Edinburgh Review* 5: 97-103.

Brougham, H., (1849), "Experiments on the Inflection of Light", *Report of the British Association* 19: 7-8.

Brougham, H., (1850 a), "Experiments and Observations upon the Properties of Light", *Philosophical Transactions* 140: 235-259.

Brougham, H., (1850 b), "Recherches Expérimentales et Analytiques Sur la Lumière", *Comptes Rendus de l'Académie* 30: 43-45.

Brougham, H., (1852), "Sur Divers Phénomènes de Diffraction ou d'inflexion", *Comptes Rendus de l'Académie* 34: 127-129.

Brougham, H., (1853 a), "Further Experiments on Light", *Proceedings of the Royal Society of London* 6: 172-174.

Brougham, H., (1853 b), "Further Experiments and Observations on the Properties of Light", *Proceedings of the Royal Society of London* 6: 312-319.

Brougham, H., (1853 c), "Recherches Expérimentales et Analytique Sur la Lumière", *Comptes Rendus de l'Académie* 36: 691-694.

Brougham, H., (1854), "Recherches Expérimentales et Analytique Sur la Lumière", *Mémoires De l'Institut* 27: 123-152.

Buchdahl, J., (1970), "History of Science and Criteria of Choice", in R. Steuwer (ed.), *Historical and Philosophical Perspectives of Science*. Minneapolis: University of Minnesota Press, pp.204-245.

Buchdahl, G., (1980), "Neo-Transcendental Approaches toward Scientific Theory Appraisal", in D. Mellor (ed.), *Science Belief and Behavior*. Cambridge: Cambridge University Press, p.1-21.

Buchwald, J., (1981), "The Quantitative Ether in the First Half of the Nineteenth Century", in G. Cantor and M. Hodge (eds.), *Conceptions of Ether: Studies in the History of Ether Theories 1740-1900*. Cambridge: Cambridge University Press, pp.215-38.

Buchwald, J., (1988), *The Rise of the Wave Theory of Light: Optical Theory and Experiment in the Early Nineteenth Century*. Chicago: The University of Chicago Press.

Burstyn, H., (1975), "Foucault, Jean Bernard Leon", in *Dictionary of Scientific Biography* Vol. 5 New York: Charles Scribner Sons, pp.84-87.

Buttmann, G., (1970), *The Shadow of the Telescope: A Biography of John Herschel*. New York: Scribner.

Cannon, S., (1978), *Science in Culture: the Early Victorian Period*. New York: Science History Publication.

Cantor, G., (1975), "The Reception of the Wave Theory of Light in Britain: A Case Study Illustrating the Role of Methodology in Scientific Debate", *Historical Studies in the Physical Sciences* 6: 109-132.

Cantor, G., (1978), "The Historiography of 'Georgian' Optics", *History of Science* 16: 1-21.

Cantor, G., (1983), *Optics after Newton: Theories of Light in Britain and Ireland, 1704-1840*. Manchester: Manchester University Press.

Cantor, G., (1984), "Brewster on the Nature of Light", in J. Christie and A. Morrison-Low (eds.), *David Brewster: Martyr of Science*. Edinburgh: Royal Scottish Museum Studies, pp.67-78.

Cawood, J., (1979), "The Magnetic Crusade: Science and Politics in Early Victorian Britain", *Isis* 70: 492-518.

Challis, J., (1830), "An Attempt to Explain Theoretically the Different Refrangibility of the Rays of Light, According to the Hypothesis of Undulations", *Philosophical Magazine* 8: 169.

Challis, J., (1832), "Theory of the Transmission of Light through Medium, and Od Its Reflexion At Their Surfaces, According to the Hypothesis of Undulations", *Philosophical Magazine* 11: 161.

Challis, J., (1875), *Remarks on the Cambridge Mathematical Studies*. Cambridge: Deighton, Bell and Co.

Chen, X., (1988), "Reconstruction of the Optical Revolution: Lakatos vs. Laudan", in A. Fine and J. Leplin (eds.), *PSA 1988* Vol. 1. East Lansing, MI: Philosophy of

Science Association, pp.103-109.

Chen, X., (1990), "Young and Lloyd on the Particle Theory of Light: A Response to Achinstein," *Studies in the History and Philosophy of Science* 21: 665-676.

Chen, X. and P. Barker., (1992), "Cognitive Appraisal and Power: David Brewster, Henry Brougham, and the Tactics of the Emission-Undulatory Controversy during the Early 1850s," *Studies in the History and Philosophy of Science* 23: 75-101.

Collins, H., (1981), "An Empirical Relativist Programme in the Sociology of Scientific Knowledge", in K. Knorr-Cetina and M. Mulkay (eds.), *Science Observed: Perspectives on the Social Study of Science*. London: Sage Publications, pp.85-114.

Collins, H., (1985), *Changing Order: Replication and Induction in Scientific Practice*. London: Sage Publications.

Dreyfus, H. and Dreyfus, S., (1986), *Mind over Machine: the Power of Human Intuition and Expertise in the Era of the Computer*. New York: Free Press.

Faraday, M., (1830), "On the Manufacture of the Glass for Optical Purpose", *Philosophical Transactions* 120: 1-57.

Finocchiaro, M., (1990), "Varieties of Rhetoric in Science", *History of the Human Sciences* 3: 177-193.

Foote, G., (1951), "The Place of Science in the British Reform Movement, 1830-1850", *Isis* 42: 192-208.

Forbes, J., (1832), "Report upon the Recent Progress and Present State of Meteorology", *Report of British Association* 2: 196-285.

Forbes, J., (1839), "Memorandum on the Intensity of Reflected Light and Heat", *Philosophical Magazine* 15: 479-481.

Forbes, J., (1840), "Letter to Richard Taylor, Esq., with Reference to Two Papers in the *Philosophical Magazine* for January, 1840", *Philosophical Magazine* 16: 102-104.

Forbes, J., (1858), *A Review of the Progress of Mathematical and Physical Science Between the Years 1775 and 1850*. Edinburgh: Adam & Charles Black.

Franklin, A., (1986), *The Neglect of Experiment*. Cambridge: Cambridge University Press.

Franklin, A. & Howson, C., (1988), "It Probably Is A Valid Experimental Result: A Bayesian Approach to the Epistemology of Experiment", *Study in the History and Philosophy of Science* 19: 419-427.

Fresnel, A., (1826), "Memoir Sur la Diffraction", *Memoires de l'Institut* 5: 452.

Fresnel, A., (1965), *Oeuvres*. New York: Johnson Reprint Corp.

Good, G., (1982), "J. F. W. Herschel's Optical Researches: A Study in Method", Unpublished Ph. D Dissertation, University of Toronto.

Gooding, D., (1989), "History in the Laboratory: Can We Tell What Really Went On?", in F. James (ed.) *The Development of Laboratory: Essays on the Place of Experiment in Industrial Civilization*. New York: American Institute of Physics, pp. 63-82.

Gooding, D., (1990), *Experiment and the Making of Meaning: Human Agency in Scientific Observation and Experiment*. Dordrecht: Kluwer Academic Publishers.

Gordon, M., (1870), *The Home Life of Sir David Brewster*. Edinburgh: Edmonston & Douglas.

Gough, J., (1975), "Fizeau, Armand-Hippolyte-Louis", in *Dictionary of Scientific Biography*, Vol. 5. New York: Charles Scribner Sons, pp.18-21.

Graves, R., (1882), *The Life of Sir William Rowan Hamilton*. Dublin: Hodges, Figgis, & Co.

Grodzinski, P., (1947), "A Ruling Engine Used by Sir John Barton and Its Products", *Transactions of Newcomen Society* 26: 79-88.

Hamilton, W., (1830), "Theory of Systems of Rays", *Transactions of the Irish Academy* 16: 4-62.

Hamilton, W., (1833 a), "On the Effect of Aberration in Prismatic Interference", *Philosophical Magazine* 2: 191-194.

Hamilton, W., (1833 b), "On the Undulatory Time of Passage of Light through A Prism", *Philosophical Magazine* 2: 284-287.

Hamilton, W., (1833 c), "Note on Mr. Potter's Reply", *Philosophical Magazine* 2: 371.

Hamilton, W., (1837), "Third Supplement to An Essay on the Theory of Systems of Rays", *Transactions of Irish Academy* 17: 1-144.

Haughton, S., (1855), "On A Classification of Elastic Media, and the Law of Plane Waves Propagated through Them", *Transactions of the Irish Royal Academy* 22: 97-138.

Hawes, F., (1957), *Henry Brougham*. London: Jonathan Cape.

Hawkes, N., (1981), *Early Scientific Instruments*. New York: Abbeville Press.

Herschel to Potter, (April 20, 1832), Texas University, Herschel Collection, UT. L0315.

Herschel to Whewell, (February 7, 1835), Trinity College, Cambridge, A.2071.

Herschel, J., (1819), "On the Hyposulhurous Acid and Its Compounds", *Edinburgh Philosophical Journal* 1: 8-29.

Herschel, J., (1820), "On the Action of Crystallized Bodies on Homogeneous Light, and on the Causes of the Deviation from Newton's Scale in the Tints Which Many of Them Develop on Exposure to A Polarised Ray", *Philosophical Transactions* 110: 45-100.

Herschel, J., (1827), "Light", in P. Barlow (ed.), (1854), *The Encyclopaedia of Mechanical Philosophy*. London: Richard Griffin and Co, pp.341-586.

Herschel, J., (1830), *Preliminary Discourse on the Study of Natural Philosophy*. London: Longman.

Herschel, J., (1833), "On the Absorption of Light by Coloured Medium, Viewed in Connexion with the Undulating Theory", *Philosophical Magazine* 3: 401-412.

Herschel, J., (1839 a), "Note on the Art of Photography or the Application of the Chemical Rays of Light to the Purposes of Pictorial Representation", *Proceedings of the Royal Society* 4: 131-133.

Herschel, J., (1839 b), "Letter to the Rev. William Whewell, President of the Section on the Chemical Action of the Solar Rays", *Report of the British Association* 9: 9-11.

Herschel, J., (1840), "On the Chemical Action of the Rays of the Solar Spectrum on Preparations of Silver and Other Substances, Both Metallic and Non-metallic, and on Some Photographic Processes", *Philosophical Transactions* 130: 1-59.

Herschel, J., (1843 a), "On Certain Improvements on Photographic Processes Described in A Former Communication, and on the Parathermic Rays of the Solar Spectrum", *Philosophical Transactions* 133: 1-6.

Herschel, J., (1843 b), "On the Action of the Rays of the Solar Spectrum on the Daguerreotype Plate", *Philosophical Magazine* 22: 120-132.

Herschel, J., (1845 a), "On A Case of Superficial Colour Presented by A Homogeneous Liquid Internally Colourless", *Philosophical Transactions* 135: 143-145.

Herschel, J., (1845 b), "On the Epipolic Dispersion of Light", *Philosophical Transactions* 135: 147-153.

Heyck, T., (1982), *The Transformation of Intellectual Life in Victorian England*.New York: St. Martin's Press.

Hones, M., (1990), "Reproducibility as A Methodological Imperative in Experimental Research", in A. Fine, M. Forbes, and L. Wessels (eds.), *PSA 1990*. East Lansing, MI: Philosophy of Science Association, pp.585-599.

Humboldt, A., (1814-29), *Personal Narrative of Travels to the Equinoctial Regions of the New Continent During the Years 1799-1804*. London: George Routledge and Sons.

Hunt, R., (1844), *Research on Light: An Examination of All the Phenomena Connected with the Chemical and Molecular Change Produced by the Influence of the Solar Rays' Embracing All the Known Photographic Processes*. London: Longman.

James, F., (1983), "The Debate on the Nature of the Absorption of Light, 1830-1835: A Core-set Analysis", *History of Science* 21: 335-367.

James, F., (1985), "Between Two Scientific Generations: John Herschel's Rejection of the Conservation of Energy in His 1864 Correspondence with William Thomson", *Notes and Records of the Royal Society of London* 40: 53-62.

Jellett, J. and S. Haughton (eds.), (1880), *The Collected Works of James MacCullagh*. Dublin: Hodges, Figgis & Co.

Jesuiticus, (1846), "Remarks on A Paper by Mr. Moon on Fresnel's Theory of Double Refraction", in R. Moon, (1849), *Fresnel and His Followers: A Criticism*. Cambridge: MacMillan, p.26-28.

Kellner, L., (1963), *Alexander von Humboldt*. London: Oxford University Press.

Lakatos, I., (1978), *The Methodology of Scientific Research Programmes*. Cambridge: Cambridge University Press.

Larmor, J. (ed.), (1971), *Memoir and Scientific Correspondence of the Late Sir George G. Stokes*. Cambridge: Cambridge University Press.

Latour, B., (1987), *Science in Action: How to Follow Scientists and Engineers through Society*. Cambridge, MA: Harvard University Press.

Laudan, L, (1977), *Progress and Its Problems: Towards A Theory of Scientific Growth*. Berkeley, CA: University of California Press.

Laudan, L., (1984), *Science and Values: the Aims of Science and Their Role in Scientific Debates*. Berkeley, CA: University of California Press.

Laudan, L., (1981), *Science and Hypothesis*. Dordrecht: D. Reidel.

Lerebours, N., (1843), *Treatise on Photography*. London: Longman.

Levene, J., (1966), "Sir D. Brewster and the Measurement of Refractive Power", *Proceedings of the Royal Microscopic Society* 1: 71-74.

Lloyd, H., (1831), *A Treatise on Light and Vision*. London: Longman.

Lloyd, H. , (1833 a), "On the Phenomena Presented by Light in Its Passage along the Axes of Biaxal Crystals", *Philosophical Magazine* 2: 112-120.

Lloyd, H. , (1833 b), "Further Experiment on the Phenomena Presented by Light in Its Passage along the Axes of Biaxal Crystals", *Philosophical Magazine* 2: 201-210.

Lloyd, H. , (1834), "Report on the Progress and Present State of Physical Optics", *Report of the British Association* 4: 295-413.

Lovell, D., (1968), "Herschel's Dilemma in the Interpretation of Thermal Radiation", *Isis* 59: 46-60.

Maccullagh, J., (1831 a), "On the Intensity of Light When the Vibration Is Elliptical", *The Edinburgh Journal of Science* 5: 86.

Maccullagh, J., (1831 b), "On the Double Refraction of Light in A Crystallized Medium, According to the Principle of Fresnel", *Irish Academy Transactions* 16: 65.

MacCullagh, J., (1836), "On the Laws of Reflexion Form Crystallized Surfaces", *Philosophical Magazine* 8: 103-108.

MacCullagh, J., (1838), "On the Laws of Crystalline Reflection and Refraction", *Transactions of the Royal Irish Academy* 28: 31-74.

MacCullagh, J., (1840), "An Essay toward A Dynamical Theory of Crystalline Reflection and Refraction", in J. Jellett and S. Haughton (eds.), (1880), *The Collected Works of James MacCullagh*. Dublin: Hodges, Figgis & Co, pp.145-184.

MacCullagh, J., (1841), "Notes on Some Points in the Theory of Light", in J. Jellett and S. Haughton (eds.), (1880), *The Collected Works of James MacCullagh*. Dublin: Hodges, Figgis & Co, pp.187-217.

Mach, E., (1926), *The Principles of Physical Optics: An Historical and Philosophical Treatment*. New York: Dutton and Co.

Meyer, C., (1934), *The Diffraction of Light, X-Rays, and Material Particles*. Chicago: The University of Chicago Press.

Moon, R., (1845), "On Fresnel's Theory of Double Refraction", *Philosophical Magazine* 27: 553-559.

Moon, R., (1846 a), "Reply to Jesuiticus", in R. Moon, (1849), *Fresnel and His Followers: A Criticism.*Cambridge: MacMillan, pp.29-38.

Moon, R., (1846 b), "On Double Refraction", *Philosophical Magazine* 28: 134-136.

Moon, R., (1847), "Reply to Some Remarks of Mr. Airy", in R. Moon, (1849), *Fresnel and His Followers: A Criticism*. Cambridge: MacMillan, pp.97-102.

Moon, R., (1849), *Fresnel and His Followers: A Criticism*. Cambridge: MacMillan.

Morrell, J., (1974), "Reflections on the History of Scottish Science", *History of Science* 12: 81-94.

Morrell, J. & Thackray, A., (1981), *Gentlemen of Science*. Oxford: Clarendon Press.

Morrell, J. & Thackray, A., (1984), *Gentlemen of Science: Early Correspondence of British Association for the Advancement of Science*. London: Offices of the Royal Historical Society.

Morrison-Law, A., (1984), *Martyr of Science: Sir David Brewster*. Edinburgh: Royal Scottish Museum Studies.

Morse, E., (1972), "Natural Philosophy, Hypotheses, and Impiety: Sir David Brewster Confronts the Undulatory Theory of Light", Unpublished Ph. D Dissertation,University of California.

Mulkay, M. and Gilbert, C., (1986), "Replication and Mere Replication", *Philosophy of Social Science* 16: 21-37.

Newton, I., (1675), "On Colour and Light", in T. Birch, *The History of the Royal Society of London*, Vol.3. London: A. Millar, pp.247-305.

Newton, I., (1952), *Opticks, of A Treatise of the Reflections, Refraction, Inflection & Colours of Light*. New York: Dover Publication, Inc.

Nickles, T., (1989), "Justification and Experiment", in D. Gooding, T. Pinch, and S. Schaffer (eds.), *The Uses of Experiment: Studies in the Natural Sciences*. Cambridge: Cambridge University Press, pp.299-333.

Nicolson, M., (1987), "Alexader Von Humboldt, Humboldtian Science and the Origins of the Study of Vegetation", *History of Science* 25: 165-194.

O'Hara, J., (1982), "The Prediction and Discovery of Conical Refraction by William Rowan Hamilton and Humphrey Lloyd (1832-1833)", *Proceedings of the Royal Irish Academy* 82a: 231-257.

Olson, R., (1975), *Scottish Philosophy and British Physics 1750-1880*. Princeton: Princeton University Press.

Orange, A., (1971), "The British Association for the Advancement of Science: the Provincial Background", *Science Studies* 1: 315-329.

Parkinson, E., (1975), "Stokes, George Gabriel", in *Dictionary of Scientific Biography* Vol.13. New York: Charles Scribner Sons, pp.70-79.

Pav, P., (1964), "Eighteenth Century Optics - the Age of Unenlightenment", Unpublished Ph. D. Dissertation, Indiana UniversityIndiana University.

Peacock, G., (1855 a), *The Life of Thomas Young*. London.

Peacock, G. (ed.), (1855 b), *Miscellaneous Works of the Late Thomas Young*. London: John Murray.

Pickering, A., (1981), "Interests and Analogies", in B. Barnes (ed.) *Science in Context*. Milton Keynes: Open University Press, pp.120-143.

Pickering, A., (1989), "Living in the Material World", in D. Gooding, T. Pinch, and S. Schaffer (eds.), *The Uses of Experiment: Studies in the Natural Sciences*. Cambridge: Cambridge University Press, pp.275-298.

Popper, K., (1968), *The Logic of Scientific Discovery*. London: Hutchinson.

Potter to Brougham, (March 19, 1855), University College Library, Brougham Collection, 26,199.

Potter, R., (1830), "An Account of Experiments to Determine the Quantity of Light Reflected by Plane Metallic Specula under Different Angles of Incidence", *The Edinburgh Journal of Science* 3: 278-288.

Potter, R., (1831), "An Account of Experiments to Determine the Reflective Powers of Crown, Plate, and Flint-glass, At Different Angles of Incidence; and An Investigation toward Determining the Law by Which the Reflective Power Varies in Transparent Bodies Possessing the Property of Single Refraction", *The Edinburgh Journal of Science* 4: 53-67.

Potter, R., (1832 a), "On the Modification of the Interference of Two Pencils of Homogeneous Light, Produced by Causing Them to Pass through A Prism of Glass; and on the Phenomena Which Then Take Place, with Reference to the Velocity of Light in Its Passage through Refracting Substance", *Report of the British Association* 2: 553-554.

Potter, R., (1832 b), "On An Instrument for Photometry by Comparison, and on Some Application of It to Optical Phenomena", *Report of the British Association* 2: 554-555.

Potter, R., (1832 c), "On An Instrument for Photometry by Comparison, and on Some Application of It to Important Optical Phenomena", *Philosophical Magazine* 1: 174-181.

Potter, R. , (1833 a), "On the Modification of the Interference of Two Pencils of Homogeneous Light Produced by Causing Them to Pass through A Prism of

Glass, and on the Importance of the Phenomena Which Then Take Place in Determining the Velocity with Which Light Traverses Refracting Substance", *Philosophical Magazine* 2: 81-95.

Potter, R., (1833 b), "A Reply to the Remarks of Professor Airy and Hamilton on the Paper upon the Interference of Light after Passing through A Prism of Glass", *Philosophical Magazine* 2: 276-281.

Potter, R., (1833 c), "On A Phenomenon in the Interference of Light Hitherto Undescribed", *Report of the British Association* 3: 378.

Potter, R., (1833 d), "Particulars of A Series of Experiments and Calculations Undertaken with A View to Determine the Velocity with Which Light Traverses Transparent Media", *Philosophical Magazine* 3: 333-342.

Potter, R., (1834), "On the Power of Glass of Antimony to Reflect Light", *Philosophical Magazine* 4: 6-9.

Potter, R., (1840 a), "On the Method of Performing the Simple Experiment of Interferences with Two Mirrors Slightly Inclined, so as to Afford An Experimentum Crucis as to the Nature of Light", *Philosophical Magazine* 16: 380-387.

Potter, R., (1840 b), "On the Application of Huygens's Principle in Physical Optics", *Philosophical Magazine* 17: 243-248.

Potter, R. , (1840 c), "On Photometry in Connexion with Physical Optics", *Philosophical Magazine* 16: 16-23.

Potter, R., (1841 a), "On the Phenomena of Diffraction in the Centre of the Shadow of A Circular Disc, Placed before A Luminous Point, as Exhibited Be Experiment", *Philosophical Magazine* 19: 151-155.

Potter, R., (1841 b), "Reply to Mr. Tovey's Remarks on A Paper on the Application of Huyghens's Principle in Physical Optics", *Philosophical Magazine* 18: 11-13.

Potter, R., (1841 c), "An Examination of the Phenomena of Conical Refraction in Biaxal Crystals", *Philosophical Magazine* 18: 343-353.

Potter, R., (1846), "A Reference to Former Contributions", in R. Moon, (1849), *Fresnel and His Followers: A Criticism*. Cambridge: MacMillan, p.39.

Potter, R., (1855), "On the Interference of Light Near A Caustic, and the Phenomena of the Rainbow", *Philosophical Magazine* 9: 321-326.

Potter, R., (1856), *Physical Optics, or the Nature and Properties of Light: A Descriptive and Experimental Treatise*. London: Walton and Maberly.

Potter, R., (1859), *Physical Optics, Part II. the Corpuscular Theory of Light: Discussed Mathematically*. Cambridge: Deighton, Bell, and Co.

Powell, B. , (1832 a), "On Experiments Relative to the Interference of Light", *Philosophical Magazine* 11: 1-7.

Powell, B., (1832 b), "Further Remarks on Experiments Relative to the Interference of Light", *Philosophical Magazine* 1: 433-438.

Powell, B., (1833 a), "Remarks on Mr. Barton's Paper 'On the Inflexion of Light'", *Philosophical Magazine* 2: 424-435.

Powell, B., (1833 b), "Remarks on Mr. Barton's Reply, Respecting the Inflection of Light", *Philosophical Magazine* 3: 412-417.

Powell, B., (1835 a), "An Abstract of the Essential Principles of Cauchy's View of the Undulatory Theory, Leading to An Explanation of the Dispersion of Light", *Philosophical Magazine* 6: 16-25, 107-113, 189-193, 262-267.

Powell, B., (1835 b), "Researches Towards Establishing A Theory of the Dispersion of Light", *Philosophical Transactions* 125: 249-254.

Powell, B., (1836 a), "Researches Towards Establishing A Theory of the Dispersion of Light. No. II", *Philosophical Transactions* 126: 17-19.

Powell, B. , (1836 b), "Observations for Determining the Refractive Indices for the Standard Rays of the Solar Spectrum in Various Media", *Transactions of the Ashmolean Society* 1: 1-24.

Powell, B. , (1836 d), "On the Formula for the Dispersion of Light Derived from M. Cauchy's Theory", *Philosophical Magazine* 8: 204-10.

Powell, B., (1838 a), "On Some Points Connected with the Theory of Light", *Reports of the British Association* 8: 6-7.

Powell, B. , (1838 b), "Observations for Determining the Refractive Indices for the Standard Rays of the Solar Spectrum in Various Media", *Transactions of the Ashmolean Society* 1: 8.

Powell, B., (1838 c), "Reply to Sir D. Brewster", *Athenaeum* 578: 841.

Powell, B., (1838 d), "Researches Towards Establishing A Theory of the Dispersion of Light. No. IV", *Philosophical Transactions* 128: 249-254.

Powell, B. , (1839 a), "Report on the Present State of Our Knowledge of Refractive Indices for the Standard Rays of the Solar Spectrum in Different Media", *Reports of the British Association* 9: 1-12.

Powell, B., (1839 b), "On the Explanation of Some Optical Phenomena Observed by Sir Brewster", *Report of the British Association* 9: 1-2.

Powell, B., (1839 c), "Reply to Sir David Brewster", *Athenaeum* 625: 795.

Powell, B., (1840), "On the Theory of the Dark Bands Formed in the Spectrum from Partial Interception by Transparent Plates", *Philosophical Magazine* 17: 81-85.

Powell, B., (1841), "On the Theoretical Computation of Refractive Indices", *Report of the British Association* 11: 24-25.

Powell, B., (1845), "On the Elliptic Polarization of Light, by Reflexion from Metallic Surfaces", *Report of the British Association* 15: 6-7.

Powell, B., (1846), "On the Bands Formed by Partial Interception of the Prismatic Spectrum", *Report of the British Association* 16: 4.

Powell, B., (1848), "On A New Case of the Interference of Light", *Philosophical Transactions* 138: 213-26.

Powell, B., (1851), "Remarks on Lord Brougham's 'Experiment on Light', &c. in the *Phil. Trans.* 1850, Part I", *Report of the British Association* 21: 11-12.

Powell, B., (1852), "Remarks on Lord Brougham's 'Experiments and Observations on the Properties of Light,' &c. Inserted in the *Phil. Trans.* 1850, Part. I", *Philosophical Magazine* 4: 1-8.

Ritchie, W., (1826), "On A New Photometer, Founded on the Principles of Bouguer", *Transactions of the Royal Society of Edinburgh* 10: 443-445.

Roderick, G. and Stephens, M., (1974), "Scientific Studies and Scientific Manpower in the English Civic Universities 1870-1914", *Science Studies* 4: 41-63.

Ronchi, V., (1970), *The Nature of Light: An Historical Survey*. Cambridge, MA: Harvard University Press.

Royal Society of London, (1867), *Catalogue of Scientific Papers, 1800-1863*. London: Royal Society of London.

Royal Society of London, (1908), *Catalogue of Scientific Papers, 1800-1900, Subject Index*, Vol.3. Cambridge: Cambridge University Press.

Rumford, C., (1794), "An Account of A Method of Measuring the Comparative Intensities of the Light Emitted by Luminous Bodies", *Philosophical Transactions* 84: 67-106.

Schaaf, L., (1979), "Sir John Herschel's 1839 Royal Society Paper on Photography", *History of Photography* 3: 47-60.

Schweber, S., (1981), *Aspects of the Life and Thought of Sir John Frederick Herschel*, Vol. 1. New York.

Shairp, J., (1873), *Life and Letters of James D. Forbes*. London: MacMillan.

Shapere, D., (1984), "Objectivity, Rationality, and Scientific Change", in P. Kitcher and P. Asquith (eds.), *PSA 1984*. East Lasting, MI: Philosophy of Science Association?, pp.637-663.

Shapere, D., (1987), "Method in the Philosophy of Science and Epistemology: How to Inquire About Inquiry and Knowledge", in N. Nersessian (ed.), *The Process of Science*. Dordrecht: Martinus Nijnoff Publishers, pp.1-39.

Shapin, S. and Schaffer, S., (1985), *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*. Princeton, NJ: Princeton University Press.

Smith, A., (1839), "Notes on the Undulatory Theory of Light", *Cambridge Mathematical Journal* 1: 3-11, 85-93.

Smith, A., (1846), "Remarks of Mr. Archibald Smith, Relating to Part of the Last Paper", *Philosophical Magazine* 28: 48.

Spearman, T., (1988), "James MacCullagh", in J. Nudds, N. McMillan, D. Weaire, and S. Lawlor (eds.), *Science in Ireland 1800 - 1930: Tradition and Reform*. Dublin: Trinity College.

Steffens, H., (1977), *The Development of Newtonian Optics in England*. New York: Science History Publication.

Stokes, G., (1848), "On the Theory of Certain Bands Seen in the Spectrum", *Philosophical Transactions* 138: 227-242.

Stokes, G., (1849 a), "Supplement to A Paper 'On the Theory of Certain Bands Seen in the Spectrum'", *Proceedings of the Royal Society* 5: 795-796.

Stokes, G., (1849 b), "On the Dynamical Theory of Diffraction", *Transactions of the Cambridge Philosophical Society* 9: 1-62.

Stokes, G., (1850 a), "On Metallic Reflexion", *Report of the British Association* 20: 19-20.

Stokes, G., (1850 b), "On Haidinger's Brushes", *Report of the British Association* 20: 20-21.

Stokes, G., (1851), "On the Colours of Thick Plates", *Philosophical Magazine* 2: 419-420.

Stokes, G., (1852), "On the Change of Refrangibility of Light", *Philosophical*

Transactions 142: 463-562.

Sutton, M., (1974), "Sir John Herschel and the Development of Spectroscopy in Britain", *British Journal for the History of Science* 7: 42-60.

Sviedrys, R., (1976), "The Rise of Physics Laboratories in Britain", *Historical Studies of Physical Sciences* 7: 405-436.

Talbot, W., (1837), "Experiment of Light", *Philosophical Magazine* 10: 364.

The House of Longman, (1978), *Archives of the House of Longman, 1794-1914.* Cambridge.

Tovey, J., (1840), "On Potter's Application of Huygens's Principle in Physical Optics", *Philosophical Magazine* 17: 431-433.

Turner, G., (1975), "Soleil, Jean-Baptiste-Francois", in *Dictionary of Scientific Biography*, Vol.12. New York: Charles Scribner Sons, p.519.

Venn, J., (1951), *Alumni Cantabrigienses: A Biographical List of All Known Students, Graduates and Holders of Office At the University of Cambridge, from the Earliest Time to 1900.* Cambridge: Cambridge University Press.

Walsh, J., (1958), *Photometry*. New York: Dover Publications.

Whewell, W., (1833), "Presidential Address to the 1833 British Association Meeting", *Literary Gazette* 859: 422.

Whewell, W., (1847), *The Philosophy of the Inductive Sciences*. London: John W. Parkers.

Whewell, W., (1967), *History of the Inductive Sciences*, 1st, 2nd and 3rd Edition. London: Frank Cass.

Whittaker, E., (1951), *A History of the Theories of Aether and Electricity*. New York: Happer & Brother.

Williams, L., (1961), "The Royal Society and the Founding of the British Association for the Advancement of Science", *Notes and Records of the Royal Society of London* 16: 221-233.

Wilson, D., (1968), "The Reception of the Wave Theory of Light by Cambridge Physicists (1820-1850): A Case Study in the Nineteenth-century Mechanical Philosophy", Unpublished Ph. D. Dissertation, the Johns Hopkins University.

Wilson, D., (1982), "Experimentalists Among the Mathematicians: Physics in the Cambridge Natural Science Tripos, 1851-1900", *Historical Studies of Physical Sciences* 12: 325-347.

Wilson, D., (1985), "The Educational Matrix: Physics Education At Early-Victorian Cambridge, Edinburgh and Glasgow Universities", in P. Harman (ed.), *Wranglers and Physicists. Studies on Cambridge Mathematical Physics in the Nineteenth Century*. Manchester: Menchester University Press, pp.12-48.

Wilson, D., (1987), *Kelvin and Stokes: A Comparative Study in Victorian Physics*. Bristol: Hilger.

Wittgenstein, L., (1958), *Philosophical Investigation*. Oxford: Basil Blackwell.

Wittgenstein, L., (1978), *Remarks on the Foundations of Mathematics*. Oxford: Basil Blackwell.

Worrall, J., (1976), "Thomas Young and the 'refutation' of Newtonian Optics: A Case Study in the Interaction of Philosophy of Science and History of Science", in C. Howson (ed.), *Method and Appraisal in the Physical Sciences*. Cambridge: Cambridge University Press, pp.107-179.

Worrall, J., (1990), "Scientific Revolutions and Scientific Rationality: the Case of the Elderly Holdout", in C. Savage (ed.), *Scientific Theories*. Minneapolis: University of Minnesota Press, pp.319-354.

Young, T., (1800), "Outlines of Experiments and Inquiries Respecting Sound and Light", *Philosophical Transactions* 90: 64-98.

Young, T., (1802 a), "On the Theory of Light and Colours", *Philosophical Transactions* 92: 12-48.

Young, T., (1802 b), "An Account of Some Cases of the Production of Colours Not Hitherto Described", *Philosophical Transactions* 92: 387-397.

Young, T., (1804 a), "Experiments and Calculations Relative to Physical Optics", *Philosophical Transactions* 94: 1-16.

Young, T., (1804 b), "Dr. Young's Reply to the Animadversions of the Edinburgh Reviewers", in G. Peacock (ed.), *Miscellaneous Works of the Late Thomas Young*. London: John Murray, pp.192-215.

Young, T., (1807), *A Course of Lectures on Natural Philosophy and the Mechanical Arts*. London: J. Johnson.

Vita

EDUCATION

Ph.D., Science and Technology Studies, April 1992.
Virginia Polytechnic Institute and State University.

M.S., Science and Technology Studies, October 1988.
Virginia Polytechnic Institute and State University.

M.A., Methodology and History of Science, January 1984.
Zhongshan (Sun Yatsen) University, Canton, China.

B.A., Philosophy, January 1982.
Zhongshan (Sun Yatsen) University, Canton, China.

PUBLICATIONS

Chen, Xiang & Peter Barker. 1992. "Cognitive Appraisal and Power: David Brewster, Henry Brougham, and the Tactics of the Emission-Undulatory Controversy during the Early 1850s," *Studies in the History and Philosophy of Science* 23: 75-101.

Chen, Xiang. 1990. "Young and Lloyd on the Particle Theory of Light: A Response to Achinstein," *Studies in the History and Philosophy of Science* 21: 665-676.

Chen, Xiang. 1990. "Local Incommensurability and Communicability," in A. Fine, M. Forbes & L. Wessels (eds.) *PSA 1990: Proceedings of the 1990 Biennial Meeting of the Philosophy of Science Association*, Vol.I, pp.67-76.

Barker, Peter & Xiang Chen. 1989. *A Critical Bibliography of Historical Orientation Philosophy of Science*, Monograph published by the Center for Study of Science in Society, Virginia Tech.

Chen, Xiang. 1988. "A Reconstruction of the Optical Revolution: Lakatos vs. Laudan," in A. Fine & L. Leplin (eds.), *PSA 1988: Proceedings of the 1988 Biennial Meeting of the Philosophy of Science Association*, Vol.I, pp.103-109.

Zhang, Huaxia & Xiang Chen. 1986. "The Structures of Explanations and Structural Explanations," *Journal of Natural Dialectics*, (1986, No.4), pp.23-31. (in Chinese.)

Chen, Xiang & Li Li. 1984. "The History of Understanding Structural-Functional Relations," *Exploration*, (1984, No.6), pp.52-57. (in Chinese.)

Chen, Xiang. 1984. "Correspondence Rules and the Processes of Cognition," *Journal of Social Sciences*, (1984, No.4), pp.14-20. (in Chinese.)

Chen, Xiang. 1984. "The Discovery of the Law of Free Fall," *Physical Bulletin*, (1984, No.5), pp.55-59. (in Chinese.)

Chen, Xiang. 1983. "Comments on the Problem of Induction," *Jianghai Academic Journal*, (1983, No.5), pp.25-30. (in Chinese.)

Chen, Xiang. 1983. "On the Method of Structural Explanations," *Natural Information*, (1983, No.4), pp.38-42. (in Chinese.)

Chen, Xiang. 1983. "On the Method of Reductive Explanations," *Graduate Students Journal of Zhongshan (Sun Yatsen) University*, (1983, No.2), pp.12-24. (in Chinese.)

AWARDS/HONORS

Graduate Research Development Project Grant, VPI & SU, 1990.
Graduate Student Assembly Travel Fund, VPI & SU, 1990.
Graduate Instruction Fee Scholarship, VPI & SU, 1991, 1989.
The Honor Society of Phi Kappa Phi, VPI & SU, 1991-present.
Honorary Fellow, University of Minnesota, Spring 1986.

EMPLOYMENT

Assistant professor of philosophy, California Lutheran University, beginning September 1992.